Migration Restrictions and the Migrant-Native Wage Gap: The Roles of Wage Setting and Sorting*

Naijia Guo† Li Zhang‡ Rongjie Zhang§ Ben Zou¶

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Abstract

We study the wage gap between internal migrants and native workers in China's urban labor market. Using employer-employee-linked administrative records from a large city, we estimate a two-way fixed effects model and decompose the average migrant-native wage gap. Compared to natives with the same skills, migrants receive lower pay from the same employers and are less likely to be employed by high-premium employers. These wage-setting and sorting effects contribute to an 8.7-log-point wage penalty for migrants. We then study the role of employer-sponsored *hukou* (household registration) quotas in determining the wage gap. Following a policy change that decreased the number of quotas by 40%, migrants' wages relative to natives increased by 5.3 log points. The decrease in quotas led to a reduction in workplace amenities for migrants, whose earnings increased relative to those of natives as predicted by the theory of compensating wage differentials. This rise in the wage-setting effect was especially notable among the young, the skilled, and those in the private sector. However, as quota reductions were more pronounced in the private sector, the policy change induced high-skilled migrants to shift towards the low-premium public sector, leading to a decline in the average migrant wage. Based on a wage-posting model, we estimate the willingness-to-pay for a quota to be between 2.2 and 3.7 times the average annual earnings.

Keyword: Migrant-Native Wage Gap, *Hukou* Quota, Wage Premium, Sorting

JEL codes: J31, J32, J42, J61, O15

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†The University of Hong Kong, njguo@hku.hk

‡University College London, zhang.l@ucl.ac.uk

§Tsinghua University, zhangrj19@mails.tsinghua.edu.cn

¶Purdue University, zou136@purdue.edu

1 Introduction

Economic opportunities are unevenly distributed across space, leading to substantial economic returns to migration. Global estimates suggest one in every seven individuals is a migrant, either internationally or internally (Bell and Charles-Edwards, 2013). Migrants are widely documented to earn less than natives (e.g., Borjas, 2018; Dorn and Zweimüller, 2021). Selection in observed and unobserved skills, cultural and language distances, and labor market networks have been proposed as potential explanations for the observed migrant-native wage gap.

The existing literature has paid less attention to restrictive migration policies. These policies, which impose barriers to migrants' employment and could affect their wages, are prevalent across the world. For example, the H-1B visa program in the United States requires foreign workers to acquire employer sponsorship. Many Gulf countries adopt guest worker programs where a migrant worker's right to work is tied to his employer. The number of work permits in these programs is often capped. Scarcity creates economic rent, allowing firms in possession of such permits to be selective in hiring migrants and to pay them lower wages (e.g., Kerr et al., 2015; Naidu et al., 2016).

This paper studies the wage gap between internal migrants and natives in China's urban labor market. China has experienced large internal migration in recent decades, with tens of millions of workers moving to the country's fast-growing cities. Migrants encounter various discriminatory policies in the destination labor market. The most prominent of such policies is the *hukou* (household registration) system, which grants exclusive access to many local goods and services, making a local *hukou* highly sought after by migrants. In most cities, formal employment is a major pathway for migrant workers to obtain a local *hukou*.

We focus on an anonymized large city in China. The studied city exemplifies China's urban labor market under migration restrictions. It is among the nation's most prosperous cities, and migrants account for about half of its workforce. However, it also enforces one of the most stringent *hukou* policies, which deprives migrants of access to public education, healthcare, and subsidized housing. Local *hukou* for migrants is allocated by the government through an employer-sponsored quota system, where the number of quotas falls far short of potential demand. Hence, employers that are allotted quotas have considerable discretion in deciding whom to hire and how much to pay them. All else equal,

employers that can sponsor *hukou* are deemed more desirable by migrants, especially those with a high willingness-to-pay.

Our main data are the employer-employee-linked administrative records that cover the entire formal sector of the studied city. To the best of our knowledge, this is the first such dataset in China, which provides a unique opportunity to examine firm behavior and employer-employee sorting. While the studied city has a sizable informal sector, employment in the formal sector is a prerequisite to qualify for a quota. Therefore, the data covers the relevant population to study the role of the quota policy on the migrant-native wage gap.

We first estimate a two-way fixed effects model to recover individual fixed effects (which we call "person effects" for the remainder of the paper) and employer wage premiums following Abowd et al. (1999) (henceforth AKM). We do this separately for native and migrant workers to account for the possibility that employers may offer different wage premiums to the two groups. We then employ the Oaxaca-Blinder method to decompose the average wage gap into differences in worker skills and employer wage premiums, following a similar approach as Gerard et al. (2021). We focus on two components. First, an employer may pay migrants a lower wage premium than natives with similar skills – this is the *wage-setting effect*. Second, among migrants and natives with similar skills, migrants may be less likely to be employed by high-premium employers – this is the *residual-sorting effect*. These two channels capture the wage gap resulting from employers' pay and hiring practices independent of worker skills, and may reflect worker and employer responses to restrictive migration policies.

Migrants in the formal sector earn, on average, 13% higher wages than natives. However, this difference is largely due to migrants possessing significantly higher skills. Positive sorting in the labor market implies that migrants are more likely to work for high-premium employers. However, after accounting for worker skills and skill-based sorting, migrants face a substantial wage penalty. The wage-setting effect reduces migrants' average wage relative to that of natives by 4.3 log points, while the residual-sorting effect reduces it by an additional 4.4 log points. Together, these two channels lower migrants' relative wages by 8.7 log points.

Does the employer-sponsored *hukou* quota affect the migrant-native wage gap? We leverage a policy change that reduced the number of quotas by 40%. To guide our empirical analyses, we build a wage-posting model with heterogeneous workers and em-

ployers in the spirit of Card et al. (2018). Quotas are a workplace amenity specific to migrants, who are willing to accept a lower wage in exchange for a pathway to a local *hukou*. Knowing that, employers that are allotted quotas offer lower wages to migrants than to similarly-skilled natives. This compensating wage differential effect contributes to the wage-setting channel. The model predicts that a reduction in the number of quotas would lead to a rise in the relative pay for migrants. In addition, heterogeneity in migrants' willingness to pay for the *hukou* and the allocation of quotas across employers with different wage premiums affect the sorting patterns.

We estimate and decompose the migrant-native wage gap separately for the period before and the period after the policy change. We find that after the policy change, the wage-setting effect increases migrants' relative wages by 4.3 log points, while the residual sorting effect decreases it by 2.3 log points. Back-of-the-envelope calculations suggest migrants' willingness to pay for a *hukou* is between 2.2 and 3.7 years of annual earnings.

Our model generates rich predictions on the heterogeneous impacts of the *hukou* quota tightening. While we do not observe quota allocation at the employer or worker level, policy documents reveal that quotas are disproportionately reserved for young migrants and those with high skills. These groups are more affected by the quota reduction, and we expect the wage-setting effect to be larger for them. In addition, quotas declined more precipitously in the private sector. We expect the increase in the relative migrant wage to be greater in the private sector than in the public sector.

To test these predictions, we employ a series of difference-in-differences models that utilize the variation before and after the policy shock, as well as the differences between migrants and natives. The wage-setting effects are isolated by restricting the comparison to workers employed by the same employer. We find that after the policy change, the relative migrant wage increases, as does the wage-setting effect. The increases are substantially larger for skilled migrants, young migrants, and those in the private sector. Furthermore, increases in the wage-setting effect are generally larger than the increase in the relative migrant wage, suggesting a worsening residual-sorting effect for migrants. Corresponding event studies show parallel pre-trends and an immediate and stable effect after the policy shock.

We further explore the impact of the policy change on the sorting of workers across employers. We find changes in the residual-sorting effect *within* each sector to be small. Following the policy change, the probability of skilled migrants being employed in the

public sector increased relative to those of less-skilled migrants. This is consistent with the increased concentration of quotas in the public sector and the higher willingness of skilled migrants to pay for a *hukou*. Because public-sector employers tend to offer lower wage premiums than private-sector employers, the changing sectoral choices of skilled migrants may have contributed to the observed worsening of the residual-sorting effect, and may imply misallocation of talent in the labor market.

Employer-sponsored quotas disproportionately impact young migrants' wages and placement. Studies have shown that initial setbacks at the start of one's career could have persistent negative impacts (Genda et al., 2010; Wachter, 2020). We investigate whether and the rate at which migrant wages assimilate to those of natives by tracking two cohorts of young workers, who appear in the sample before and after the quota tightening. We observe rapid wage catch-up for both cohorts. The skill-adjusted migrant-native wage gap for the pre-quota-tightening cohort was -20 log points in the first year but quickly narrowed to -5 log points after four years. The post-quota-tightening cohort started with a smaller gap of -5 log points, and the catch-up is nearly complete after three years. Skilled migrants and those initially employed in the public sector are more likely to obtain a quota at the cost of larger initial wage penalties. However, they also experience faster wage assimilation. About 15% of the catch-up can be attributed to migrants moving to higher-paying employers relative to natives.

Our study contributes to several strands of the literature. Internal migration in China in recent decades has been among the largest both globally and historically. There is a large literature on the relative pay of China's internal migrants, but it predominantly relies on traditional household surveys (Ma, 2018; Meng and Zhang, 2001; Zhang et al., 2016; Zhu, 2016). Using the first employer-employee-linked panel from China, we make three key contributions. First, the AKM model recovers person effects that capture unobserved worker skills. We show that the selection of unobserved skills plays an important role in the migrant-native wage gap but is typically not accounted for in existing studies. Second, we show that employers' pay practices and employer-employee sorting are crucial determinants of the migrant-native wage gap. Third, while some studies investigate how *hukou* reforms affect their wages, most existing work focuses on temporary, rural-to-urban migrants (recent studies include An et al., 2022; Fan, 2019; Gai et al., 2021; Meng, 2012; Ngai et al., 2019; Sieg et al., 2023; Zhao, 1999). While temporary migrants constitute the majority of the overall migrant population, about one-third of internal migrants have

acquired a local *hukou* and have taken up permanent residence in the destination cities. By looking at migrants in the formal sector, we focus on potential permanent migrants and analyze the wage penalties they endure to gain permanent residency.

Many factors influence the migrant-native wage gap. Studies have shown the roles of self-selection in both observed and unobserved skills, skills mismatch, search friction, and labor market networks (e.g., Bartolucci, 2014; Hirsch and Jahn, 2012; Pendakur and Woodcock, 2010; Picot and Piraino, 2013). This paper contributes to the growing literature on the effect of migration policies on labor market outcomes. The employer-sponsored quota policy in the studied city shares many similarities with foreign worker permit programs such as the H-1B visa in the United States and the guest worker programs in countries such as the UAE, Kuwait, and Singapore. Existing studies (e.g., Kerr et al., 2015; Naidu et al., 2016; Peri, 2016; Peri et al., 2015) show that such policies tend to negatively affect migrant workers' wage and employment. Using the worker-establishment panel data that covers the entire formal sector, we present direct evidence of employers' recruiting and wage-setting practices and show how such policies affect the allocation of workers in the labor market.

Several recent studies examine the wage gaps between different worker groups through the lens of the AKM model (Card et al., 2016; Dostie et al., 2023; Gerard et al., 2021). These studies show that group-specific firm wage premiums and the sorting of workers into high-premium firms contribute to the wage gap. However, the sources of these effects are typically not explored; instead, they are generally labeled as "discrimination." In this study, we show that the migration restriction policy can be a potential driver of the wage setting and sorting effects. As *hukou* quotas are a workplace amenity only for the migrants, non-discriminating firms will find it optimal to pay lower wages to migrant employees (Hall and Mueller, 2018; Lamadon et al., 2022; Le Barbanchon et al., 2020; Sorkin, 2018). Furthermore, uneven allocation of the *hukou* quotas could affect the sorting patterns and lead to worker misallocation (Eeckhout, 2018). We leverage a natural experiment and provide causal evidence within the AKM framework.

The rest of the paper is organized as follows. Section 2 introduces the data. In Section 3, we present the two-way fixed effects model of wage determination and decompose the average migrant-native wage gap. The *hukou* policy in the studied city is described in Section 4. Section 5 presents the model that conceptualizes how those policies affect the migrant-native wage gap via wage setting and sorting. In Section 6, we present empirical

tests of the model predictions using a policy change that tightens *hukou* quotas. Section 7 shows the dynamics of wage assimilation. We conclude in Section 8.

2 Data

2.1 Main Data

The main data used in this paper comes from the administrative records of the Housing Provident Fund (HPF) of a large Chinese city between 2006 and 2014. The HPF is part of China's employment-based social insurance system,[1] and is a defined-contribution, tax-free individual account that can be used towards housing-related expenditures. Employees contribute to their HPF accounts monthly through automatic, pre-tax deductions, with employers matching the contribution. Our data consists of employer-employee-linked monthly contribution records of all jobs covered by the HPF.

We can back out a worker's annual earnings because the contribution is a fixed percentage of the worker's yearly earnings during the sample period.[2] The data does not report hours of work, so we cannot calculate the hourly wage. However, in our case, a full-time contract is essentially a necessary condition for HPF coverage. For simplicity, we use wages and earnings interchangeably throughout the paper, acknowledging that the terms are not identical.

The city we study is anonymized in accordance with the data use agreement. This city is among the largest and the most prosperous in China and attracts millions of migrants despite its strict *hukou* policies. The data comprises approximately 330 million observations from 116,000 employers and 6.4 million workers. Compared to aggregate official statistics, we estimate our data covers about 30% of the city's total employment in 2006 and over 50% in 2014.[3] Jobs covered by the HPF are a selected sample. Employers in the data tend to be larger, pay higher wages, and have a higher likelihood of being in the

[1] The insurance system is colloquially known as the "five insurances, one fund," comprising retirement insurance, medical insurance, work-related injury insurance, unemployment insurance, maternity insurance, and the housing provident fund.

[2] HPF-eligible earnings include both the contractual salary and bonuses. The fixed percentage is either 8% or 10% in 2006, 8% or 12% in 2007, and 12% from 2008 to 2014. Earnings eligible for the HPF is capped at three times the social average wage in the city, defined as the average full-time formal-sector annual wage, which was approximately 55,000 yuan in 2010. In our sample, about 7% of earnings exceed the cap. In Appendix A.1, we explain how we back out the earnings and address the issue of top-coded earnings. In Appendix D.2, we restrict the sample to those with earnings below the upper limit of HPF contributions and repeat the empirical analyses. The conclusions hold.

[3] The HPF coverage was expanding during the sample period.

public sector. High-skilled workers are overrepresented in the data. Although the coverage is selective and far from universal, our data captures the relevant population for studying the impact of employment-based *hukou* policies, as paying social insurance is a prerequisite for obtaining a local *hukou*. Appendix A.2 shows that the summary statistics derived from our data align closely with the official records for the city's formal sector.

We observe the worker's birth year, gender, and the first four digits of his citizen's identification (ID) number. In China, residents are automatically assigned a citizen's ID when they turn 16, although one may apply for an ID at a younger age. The first four digits of the ID correspond to the prefecture of the individual's original *hukou* when the ID is first issued. While the individual's *hukou* may change later in life, the ID number is almost always permanent. We define a worker as a migrant if the first four digits of his ID correspond to a location other than the studied city. Therefore, our definition of a migrant is based on where the individual was raised. It is important to note that we do not observe whether an individual currently has a local *hukou*. We also do not observe workers' education levels.

Employers in the data are establishments that hire at least one formal-sector worker. These include firms, government agencies, and not-for-profit organizations such as schools, universities, and hospitals.[4] Employers in the employer-employee-linked panel data are anonymized. However, we have access to all HPF-backed mortgage applications during the sample period, which reveal the employer's identity and can be linked to the main data. An employer is identified as long as one of its employees applied for an HPF-backed mortgage during the sample period. About one-thrid of the employers are identified, collectively accounting for over 85% of the employer-employee observations. We use the business registration data to categorize the establishment's sector as public or private. Furthermore, since we observe all formal-sector workers in an establishment, we construct a rich set of employer-level characteristics based on worker composition and turnover.

2.2 Sample and Summary Statistics

We calculate annual earnings for each worker and create an employer-employee panel by retaining observations in November. In the few cases where a worker has multiple jobs in

[4]The baseline sample includes all employer types. Appendix D.4 shows that the results are robust to using only firms.

the same period, we keep the highest-paying job. We further restrict the sample to workers between the ages of 22 and 50, as college graduates typically enter the labor market at 22, while many female workers retire at 50. We include both public- and private-sector establishments in the sample. The public sector accounts for approximately half of the formal-sector jobs in the studied city and has better access to *hukou* quotas. Studying the heterogeneity in employers' pay and hiring practices between the public and private sectors is a key elements of the paper. The resulting sample includes 5.5 million workers, 115,179 employers, and 25 million worker-year observations.

Table 1: Summary Statistics (2010 Data)

Panel A: Workers	Migrants	Natives
Number of individuals	1,155,873	1,658,170
Percent of females	44.0	46.9
Age (years)	30.8	36.0
	(6.0)	(8.5)
Wage (CNY)	72,086	62,424
	(64,421)	(53,200)
Percent in public sector	36.4	66.7
	(48.1)	(47.1)
Percent having job switches in the past year	8.9	7.1
	(28.4)	(25.7)
Percent leaving the sample next year	9.6	6.9
	(29.5)	(25.4)
Panel B: Employers	**Public**	**Private**
Number of employers	11,086	17,997
Percent of firms	50.7	98.5
Employer size	117.1	59.6
	(343.1)	(181.9)
Average worker wage (CNY)	59,790	59,351
	(34,555)	(43,743)
Percent of female workers	51.4	46.1
	(21.8)	(23.5)
Percent of native workers	75.4	43.1
	(24.7)	(29.8)
Average worker age (years)	36.8	32.5
	(4.4)	(4.3)

Notes: In Panel A, each cell shows the average value or share for a specific worker group defined by migration status. In Panel B, each cell shows the average value or share for a specific employer group defined by sector. Standard deviations are shown in parentheses.

Table 1 shows the summary statistics of the sample using the year 2010 as an example. Panel A reports worker characteristics. Approximately 40% of the workers are migrants. Compared to natives, migrants tend to be much younger and slightly less likely to be female. Migrants earn substantially more than natives; their average annual earning is 15%

higher the natives'.[5] 69% of natives in the sample work in the public sector, compared to only 38% of migrants.

There is substantial job mobility. 8.9% of migrants and 7.1% of natives work for a different employer from a year ago. In addition, 9.6% of migrants and 6.9% of natives do not appear in the sample in the following year. Disappearing from the sample does not necessarily indicate that the worker has become unemployed: He could have moved to a job that is not covered by the HPF or migrated out of the city. Some may also reappear in the sample later. Appendix A.4 presents the patterns of sample attrition. There is substantial attrition for both migrants and natives, but the attrition of migrants does not appear to be more or less selected than that of natives.

Panel B of Table 1 reports employer characteristics separately for the public and private sectors. Relative to private-sector employers, public-sector employers are larger, and their employees are more likely to be female, older, and native. Employers in both sectors offer similar average wages.

3 Decomposition of the Migrant-Native Wage Gap

What contributes to the observed migrant-native wage gap? First, migrant and native workers may differ in skills, which are embedded in education, experiences, and unobserved abilities. Second, employers may influence wages through two channels: (1) the same employer may pay different wages to migrants and natives with the same skills, which we call "wage setting;" and (2) migrants and natives may sort into employers that offer different wage premiums, which we call "sorting."

In this section, we evaluate each component's contribution to the observed wage gap. We first recover unobserved worker skills and employer wage premiums by estimating a two-way fixed effects model of wage determination following Abowd et al. (1999) and Abowd et al. (2002) (henceforth AKM), where we allow employers to pay different wage premiums to migrants and natives. We then conduct an Oaxaca-Blinder decomposition that decomposes the average wage gap into differences in skills, wage setting, and the sorting of workers across employers, following the recent literature (Card et al., 2016;

[5]Most studies find that migrants in China earn lower wages than natives. The discrepancy comes from two sources. First, migrants in the formal sector are more positively selected than natives. Second, we define migrants by where they grew up, not by whether they currently have a local *hukou*. We show in Appendix A.3 that once we restrict the sample to those in the formal sector and apply our definition of migrants, the migrant-native wage gap in commonly used household surveys is also positive.

Dostie et al., 2023; Gerard et al., 2021).

Our sample period is between 2006 and 2014. There was a change in the *hukou* policy in 2011, which we leverage to evaluate the role of migration policies in the migrant-native wage gap. In this section, we use data from 2006 to 2010 (prior to the policy change) to describe the wage gap and its components. The full dataset from 2006 to 2014 is used when we estimate the causal effects of the policy change in Section 6.

3.1 The AKM Model

Consider the following wage determination equation:

$$\ln y_{git} = \alpha_{gi} + X'_{git}\beta_g + \psi^g_{j(g,i,t)} + \varepsilon_{git}, \tag{1}$$

where $\ln y_{git}$ is the log-transformed wage of worker i in group g in year t. We use $g \in \{M, N\}$ to indicate migrants and natives, respectively. The person effect, denoted by α_{gi}, represents the time-invariant component of individual i's earning potential and is fully portable across jobs. It encompasses the individual's innate ability as well as skills acquired through education.

Next, $\psi^g_{j(g,i,t)}$ is the wage premium paid by employer j to workers in group g; $j(g,i,t)$ indicates that worker i in group g works for employer j in year t. Importantly, employers pay different wage premiums to migrants and natives ($\psi^M_{j(g,i,t)} \neq \psi^N_{j(g,i,t)}$), which directly contributes to the wage gap. In the conceptual framework introduced in Section 5, we micro-found the different wage premiums and show how the differential is correlated with employer-sponsored *hukou* quotas, which we consider as a workplace amenity.

Finally, X_{git} is a vector of observed worker characteristics, which includes the polynomial of age, year dummies, and their interactions with gender. Following Card et al. (2018), we include the quadratic and cubic terms of the normalized age around 40 and omit the linear age term to identify the cohort effect and year effect. Note that $X'_{git}\beta_g$ flexibly captures the evolution of skills by experience, which may vary between migrants and natives. The error term is represented by ε_{it}. We estimate Equation (1) separately for migrants and natives.

The ordinary least squares (OLS) estimation of Equation (1) yields consistent estimates of the person effects and employer wage premiums under the "exogenous mobility" assumption, which states that job switches are uncorrelated with the time-varying

residual wage component ε_{git}. Following the literature (e.g., Card et al., 2013), we conduct several tests to evaluate the validity of the assumption. First, we group employers into quartiles according to their wage premiums and estimate a series of event studies where workers move between employers in different quartiles. If job switches are driven by unobserved matching between workers and employers, we would expect wages to increase after the job switch, regardless of the relative wage premiums offered by the origin and destination employers. Instead, we find that wages increase when workers move from a lower-premium employer to a higher-premium one, and decrease when they move in the opposite direction. Furthermore, the magnitudes of wage changes are similar, though with different signs. This is consistent with the AKM model, which predicts that the change in wages from a job switch is determined by the employer's wage premium. In the event studies, we do not find systematic pre-trends prior to job switches, which rules out correlations between job switches and "drifts" in worker effects or employer wage premiums (Ashenfelter, 1978).

The AKM model also assumes that worker effects (α_{gi}) and employer wage premiums ($\psi^{g}_{j(g,i,t)}$) are separable in the log wage equation. We present evidence generally supportive of this assumption. We obtain the residuals from the AKM model and group them into 100 cells, represented by the matrix of deciles of person effects and deciles of employer premiums. The magnitude of the mean residuals in each cell is generally close to zero, and there are no systematic patterns in the size of the mean residuals across cells. Furthermore, the AKM model accounts for over 90% of the wage variation. Allowing for the match-specific component by including employer-employee pair fixed effects in the AKM model adds little explanatory power. Further details on these tests are provided in Appendix B.1.

The separate identification of individual effects and employer effects in the AKM model is obtained in a "connected set" of workers and employers that are linked through job mobility (Abowd et al., 2002; Abowd et al., 1999). Following the literature, we consider the identification of person effects and employer effects within the largest connected set (Card et al., 2018; Card et al., 2016; Card et al., 2013). The largest connected set accounts for 83.5% of employers and 97.0% of worker-year observations despite the relatively short panel of five years.[6]

[6]The AKM model estimates person effects and employer effects, which can be used to decompose the wage variation (Card et al., 2013). Although the goal of this paper is to understand the average wage gap between migrants and natives and the extent to which this gap is influenced by restrictive *hukou* policies,

Our goal is to compare the skills of migrants and natives as well as the wage premiums offered by employers to each group of workers. However, Equation (1) is estimated separately for migrants and natives. The person fixed effects and employer fixed effects from each estimation are relative to a reference unit in the corresponding group but are not comparable across groups. Hence, normalization of the two sets of fixed effects is required. Normalization typically involves identifying a group of benchmark employers that offer zero wage premiums to their employees. The wages of these workers, after accounting for time-varying worker characteristics captured by $X'_{git}\beta_g$, are solely attributed to the person effects (Card et al., 2016; Gerard et al., 2021). The wage premiums of other employers and the person effects of their employees are then adjusted based on this normalization. This process yields adjusted person effects that are comparable across different groups.

Identifying employers that can serve as the benchmark requires making some assumptions. We assume that low-wage employers in a competitive labor market offer zero wage premiums.[7] We define benchmark employers as private-sector firms with average wages in the bottom 5th and 10th percentiles of all employers. We exclude firms in the bottom five percentiles to mitigate concerns of measurement error and misreporting.[8]

Specifically, person and employer effects from the two groups are normalized as follows. We first compute the average estimated employer effect of the benchmark firms. We subtract this average from all estimated employer effects in both estimations. This step shifts the distribution of employer effects such that the normalized employer effects of benchmark firms have a mean of zero. We then add the average employer effects of benchmark firms to all the estimated person effects to obtain the normalized person effects.

we present a decomposition of the wage variance in Appendix B.3. We address biases in the estimation of second-order moments due to limited mobility.

[7] Following similar assumptions, Card et al. (2016) and Gerard et al. (2021) use restaurants and other food service establishments as the benchmark group. We do not adopt their approach because most food service establishments in the studied city are in the informal sector, and our sample includes few such employers.

[8] Wages in public-sector establishments may be divorced from the establishment's profitability; therefore, we exclude them from the benchmark. In the data, we observe some firms offering exceptionally low wages. Concerns about misreporting lead us to exclude firms with the lowest average wages from the benchmark. Appendix B.2 provides more details and presents suggestive evidence that the choice of benchmark firms is appropriate. The results are robust to alternative definitions of benchmark firms. In Appendix D.5, we further restrict benchmark firms to those with a low within-firm migrant-native wage gap. In Appendix D.6, we define benchmark firms as private sector firms with an average wage in the bottom decile. Recent studies also validate the choice of benchmark firms by inspecting the relationship between the employer's profit margin and its wage premium (Card et al., 2016; Dostie et al., 2023). In Appendix D.7, we match a subset of employers in our sample to a database of global firms and define benchmark firms as those with the lowest revenues. The results remain robust.

3.2 Wage Gap Decomposition

According to Equation (1), the average wage difference between migrant and native workers can be expressed by the following equation.

$$
\begin{aligned}
E[\ln y_{Mit}] - E[\ln y_{Nit}] = & \underbrace{\alpha_M - \alpha_N}_{\text{1. gap in person effect}} + \underbrace{\bar{X}'_M\beta_M - \bar{X}'_N\beta_N}_{\text{2. gap in covariates}} + \underbrace{\sum_j \psi_j^M \pi_{Mj} - \sum_j \psi_j^N \pi_{Nj}}_{\text{3. gap in employer premium}} \\
= & \underbrace{\alpha_M - \alpha_N}_{\text{1. gap in person effect}} + \underbrace{\bar{X}'_M\beta_M - \bar{X}'_N\beta_N}_{\text{2. gap in covariates}} + \underbrace{\sum_j \left(\psi_j^M - \psi_j^N\right)\pi_{Mj}}_{\text{3.1 wage-setting}} + \underbrace{\sum_j \psi_j^N \left(\pi_{Mj} - \pi_{Nj}\right)}_{\text{3.2 sorting}},
\end{aligned}
\tag{2}
$$

where $g \in M, N$ represents migrants and natives, respectively. Let N_{gj} be the number of group g workers hired by employer j and $\pi_{gj} = N_{gj} / \sum_{j'} N_{gj'}$ be employer j's share of all group g workers employed in the labor market.

The average wage gap consists of three components. The first component is the mean difference in person effects, which are time-invariant and are priced the same across all employers. The second component is the difference attributable to employee's experiences. Migrants and natives may differ in this term not only because they have different levels of observable characteristics such as gender and age ($\bar{X}_M \neq \bar{X}_N$), but also because the returns to those characteristics could differ ($\beta_M \neq \beta_N$).[9]

The third component captures the average difference in the employer premium between migrants and natives, which can be further decomposed into two terms in the Oaxaca-Blinder style. The first term captures the "wage-setting effect" — the weighted average of the difference in the wage premium between migrant and native workers paid by the same employer, where the weight is each employer's share of all migrant workers in the labor market. This term represents the portion of the group wage gap that can be explained by employers' differential pay policies towards migrants and natives while holding the employment share constant.

The second term is the "sorting effect" — the wage gap due to the differential allocations of migrants and natives across employers while holding each employer's wage premium constant. It is the weighted average of the gap in the employment shares of mi-

[9]This term, typically along with differences in education levels and their returns, is the focus of the standard Oaxaca-Blinder decomposition based on individual-level data. This standard approach to studying the migrant-native wage gap in China has been employed in previous studies (e.g., Chen and Zhang, 2018; Lee, 2012). These studies find that migrant workers have both lower observed skills and lower returns to skills. This paper focuses on employers' hiring and wage policies, and does not further decompose the wage gap in observable worker skills.

grant and native workers across employers, where the weight assigned to each employer is the wage premium it pays native workers.

There are many reasons why migrants and natives may sort into employers offering different wage premiums. On the one hand, there is a positive sorting between worker skills and employer wage premiums. Because migrants are on average more skilled than natives, this skill-based component suggests that migrants are more likely to be hired by high-premium employers. On the other hand, migrants may have difficulty landing a job at a high-premium employer compared with otherwise identical native workers. While skill-based sorting underpins employers' production requirements, the second component, which we call "residual sorting," may reflect discriminatory or distortionary hiring practices. Following Gerard et al. (2021), we further decompose the sorting component in Equation (2) as follows:

$$\sum_j \psi_j^N \left(\pi_{Mj} - \pi_{Nj}\right) = \underbrace{\sum_j \psi_j^N \left(\pi_{Mj}^* - \pi_{Nj}^*\right)}_{\text{3.2.1 skill-based sorting}} + \underbrace{\sum_j \psi_j^N \left[\left(\pi_{Mj} - \pi_{Mj}^*\right) - \left(\pi_{Nj} - \pi_{Nj}^*\right)\right]}_{\text{3.2.2 residual sorting}}, \tag{3}$$

where π_{gj}^* is the counterfactual share of group g workers hired by employer j, assuming that j fulfills the same skill composition as its observed pool of workers, but does so by randomly drawing workers with the required skill levels from the local labor market, irrespective of their migration status.[10] The residual sorting is the difference between the sorting component and the skill-based sorting term. It captures the wage gap due to the disparity in the probability of identically-skilled migrant and native workers being hired by high-premium employers.

We are particularly interested in the wage setting and residual sorting components. They capture employers' differential pay practices toward migrants versus natives, as well as the distinct sorting patterns between employers and migrants versus employers and natives that cannot be explained by skill differences between migrants and natives. We estimate the sign and magnitude of these terms and investigate whether they are affected by migration policies.

[10]Note that $\pi_{gj}^* = \sum_z N_{zj}\pi_{gz} / \sum_{j'} N_{gj'}$, where N_{zj} is the number of skill z workers hired by employer j, $\pi_{gz} = N_{gz}/N_z$ is the share of skill z workers in group g. Hence, $\sum_z N_{zj}\pi_{gz}$ is the number of group g workers that employer j would have employed if workers were randomly selected from each skill group, regardless of their migration status. Skills (z) are divided into 120 groups representing six age groups, two gender groups, and ten deciles of estimated individual fixed effects.

The Oaxaca-Blinder decomposition can be alternatively expressed as:

$$E\left[\ln y_{Mit}\right] - E\left[\ln y_{Nit}\right] = \underbrace{\alpha_M - \alpha_N}_{\text{1. gap in person effect}} + \underbrace{\bar{X}'_M\beta_M - \bar{X}'_N\beta_N}_{\text{2. gap in covariates}} + \underbrace{\sum_j \left(\psi_j^M - \psi_j^N\right)\pi_{Nj}}_{\text{3.1 wage-setting}} + \underbrace{\sum_j \psi_j^M\left(\pi_{Mj} - \pi_{Nj}\right)}_{\text{3.2 sorting}} \tag{4}$$

where the sorting effect can be further decomposed as:

$$\sum_j \psi_j^M\left(\pi_{Mj} - \pi_{Nj}\right) = \underbrace{\sum_j \psi_j^M\left(\pi_{Mj}^* - \pi_{Nj}^*\right)}_{\text{3.2.1 skill-based sorting}} + \underbrace{\sum_j \psi_j^M\left[\left(\pi_{Mj} - \pi_{Mj}^*\right) - \left(\pi_{Nj} - \pi_{Nj}^*\right)\right]}_{\text{3.2.2 residual sorting}} \tag{5}$$

Compared with the decomposition described in Equations (2) and (3), the alternative decomposition in Equations (4) and (5) differ in the choice of weights. For the alternative decomposition, the weight in the wage-setting component is the employer's share in native worker employment, while the weight in the sorting component is the employer's wage premium for migrant workers.

Table 2: Decomposition of the Migrant-Native Wage Gap

	Log Wage Gap (Migrant-Native)	
	Decomposition (1)	Alternative Decomposition (2)
Log wage	0.134	0.134
	[0.001]	[0.001]
1. Person effect	0.271	0.271
	[0.024]	[0.024]
2. Covariates	-0.097	-0.097
	[0.002]	[0.002]
3. Employer effect	-0.040	-0.040
	[0.024]	[0.024]
3.1. Wage setting	-0.043	-0.076
	[0.023]	[0.024]
3.2. Sorting	0.003	0.035
	[0.003]	[0.006]
3.2.1. Skill-based sorting	0.047	0.060
	[0.002]	[0.003]
3.2.2. Residual sorting	-0.044	-0.025
	[0.003]	[0.003]

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010. The decomposition follows Equations (2) and (3). The alternative decomposition follows Equations (4) and (5). Bootstrap standard errors are in brackets.

The decompositions are conducted on the dual-connected sample, defined as the in-

tersection of the largest connected set for migrants and the largest connected set for natives. Standard errors are obtained by bootstrapping consecutive job spells.[11] Column 1 of Table 2 reports the results from the decomposition exercise based on Equations (2) and (3). The average wage of migrants is 13.4 log points higher than that of natives. This is primarily because migrants in the sample are more skilled than natives. Migrants' average person effect is 27.1 log points higher than that of natives.

Differences in time-varying individual characteristics and their returns shrink the migrant-native wage gap by 9.7 log points, largely because migrants are, on average, much younger than natives. Taking the first two components together, the migrant-native wage gap due to differences in individual characteristics amounts to 17.4 log points in migrants' favor.

Employer effects shrink the migrant-native wage gap by 4.0 log points. We further decompose the difference in employer effects into the wage-setting effect and the sorting effect. The wage-setting effect lowers migrants' average wage relative to natives' by 4.3 log points, implying that employers systematically pay lower wages to migrant workers than to native workers with the same skills.

Meanwhile, the sorting effect, although not statistically significant, is positive, accounting for 0.3 log points in migrants' favor. We further decompose the sorting effect into skill-based sorting and residual sorting.

The skill-based sorting effect, which assumes employers care only about skills and are blind to workers' migration status, implies that migrants' average wage would be 4.7 log points higher due to the general positive sorting between worker skills and employer premiums. Consequently, the residual sorting effect suggests that migrants are less likely to work for high-premium employers than natives with identical skills, lowering migrants' average wage by 4.4 log points. Collectively, these estimates indicate that migrants are slightly more likely than natives to work for high-premium employers, not because they are favored by these employers, but because they are more skilled.

In summary, employers' pay and hiring practices lower the average wage of migrants relative to natives by 8.7 log points — 4.3 log points due to the wage-setting effect and 4.4 log points due to the residual sorting effect. [12]

[11] Appendix D.3 shows that the results are robust in a sample consisting of job movers. Appendix B.5 details the bootstrapping procedure.

[12] Note that the person effects estimated from the AKM model include both observed and unobserved skills. In Appendix A.3, we show that controlling for observed skills, such as education, is insufficient to account for the overall skill difference between migrants and natives.

Column 2 reports the results from the alternative decomposition. The average migrant wage is depressed by 7.6 log points due to wage setting and by 2.5 log points due to residual sorting. Note that the employer premium for migrant workers — the weights in the sorting component in the alternative decomposition — is likely to be directly affected by the migration policy being studied. Hence, in the remainder of the paper, we focus on the decomposition described by Equations (2) and (3). Nevertheless, we highlight that both decompositions always generate similar estimates.[13]

The wage-setting and residual sorting effects reported in Table 2 are large. Decomposing the immigrant-native wage gap in Canada, Dostie et al. (2023) find that the wage-setting effect is a mere -0.2 log point. In Portugal, Card et al. (2016) find that the wage-setting effect contributes between 0.3 and 1.5 log points to the gender wage gap.[14] Our estimates are close to the racial wage gap in Brazil estimated by Gerard et al. (2021). They find that among college graduates, the wage-setting effect depresses the wages of non-white male workers by 2.3 log points, while the residual sorting effect further reduces their wages by 2.1 log points. These effects are larger among college-educated female workers, at 3.2 and 3.6 log points, respectively.

Why do employers hire workers based on migration status and pay migrants and natives differently? The decomposition exercise does not speak to the potential economic mechanisms. The differential treatments may indicate employer discrimination against migrants, but they may also be rational responses to the incentives created by labor market institutions and policies. While there may be many explanations, we are primarily interested in the effects of the *hukou* policy. In the following sections, we investigate how the city's employer-sponsored *hukou* quota system affects the wage gap and its components.

[13] One may be concerned that the substantial differences between migrants and natives in the distributions of demographic characteristics and the person effects could influence the gaps in wage setting and residual sorting. We test the sensitivity of the decomposition results by reweighting migrants and natives such that the distributions of worker characteristics between the two groups are the same. Appendix Table B.4 shows that results from the reweighted decompositions are quantitatively similar.

[14] Both studies find a substantial sorting effect of approximately 4 log points, but neither study decomposes the sorting effect into skill-based and non-skill-based components.

4 *Hukou* Policies in the Studied City

4.1 The Employer-sponsored *Hukou* Quota System

Internal migrants in China face discriminatory policies in the destination labor market, with the *hukou* (household registration) policy being the most prevalent and well-known. An individual's *hukou* is tied to his birthplace and cannot be easily changed. Migrants without a local *hukou* are typically denied access to essential local public goods and services, such as public education, health care, and subsidized housing. Private substitutes for these goods and services are limited, expensive, and typically of lower quality.

All cities provide pathways for migrants to acquire a local *hukou*. Employment-based pathways have been the most important channel in China's largest cities in recent years. During the sample period, the studied city operated an employer-sponsored quota system. Each year, the municipal government sets the number of new quotas and sets guidelines for their allocation. Employers submit applications for quotas, and the government decides which to approve. Priority is given to public-sector employers and positions requiring high-skilled workers. While a large share of the quotas is reserved for recent college graduates, other new hires and current employees also have access to quotas.

Whether the position comes with a *hukou* quota is typically a salient feature of a job posting and an important consideration for migrants. The quota attached to a position cannot be sold to another employer or transferred to a different position within the same employer. Upon being hired in a position with a quota, migrants typically need to wait between six months and three years before obtaining the local *hukou*. Some contracts also include clauses requiring a minimum service of several years. We can reasonably assume that employers with *hukou* quotas have some market power over their migrant hires for a short period of time. A local *hukou*, once obtained, is permanent and fully portable between jobs and employers.

The studied city is a popular destination for internal migration. According to the 2010 population census, one in every three residents in the city was a migrant without a local *hukou*.[15] However, the supply of quotas falls far short of demand. The number of employment-based quotas issued by the government in 2010 amounted to 11% of the migrant workers who entered the formal sector labor market for the first time that year.[16]

[15] Residents are defined as those who have lived in the city for more than six consecutive months, regardless of their *hukou* status.

[16] According to the Yearbook of the studied city, the government issued approximately 16,000 quotas for

4.2 *Hukou* Quota Tightening in 2011

A policy change in 2011 further reduced the *hukou* quotas. Figure 1 (left axis) shows the number of employer-sponsored quotas allocated to new college graduates throughout our sample period.[17] Prior to 2011, quotas hovered at approximately 16,000 annually. Following the policy change, quotas were reduced by nearly 40%, dropping to around 10,000 per year.

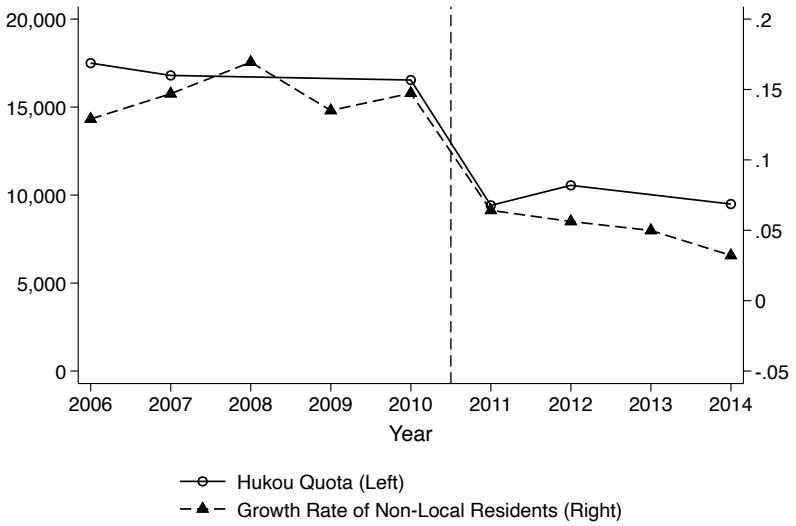


Figure 1: Total Hukou Quota, 2006 – 2014

Notes: Data on the quotas is from the Municipal Statistical Yearbooks of the studied city. The quotas for 2008 and 2009 are missing. The number of non-local residents is also from the Municipal Statistical Yearbooks. Non-local residents are defined as those who do not have a local *hukou* but have lived in the city for at least six months in the last calendar year.

The tightening of the *hukou* quotas was motivated by the city's faster-than-expected population growth. Between 2000 and 2010, the city's population grew by 44%, far exceeding the target set in earlier urban development plans. In 2011, the municipal government established a stricter population growth target for the coming decade. The reduction in *hukou* quotas was just one of several policies aimed at restricting migration. For example, the rules for non-*hukou* to buy local property became more stringent after 2011.

Despite the less welcoming policies, the city continued to be attractive to migrants. The labor supply of migrant workers did not decline either absolutely or relatively. Figure 1 (right axis) shows that although the growth rate of the non-local population de-

new graduates in 2010, whereas 141,000 migrants appeared in our sample for the first time that year.

[17]The tightening of the migration policy in 2011 affected all pathways to a local *hukou*. We focus on the quotas for new graduates because this statistic is consistently reported by the city's Statistics Bureau.

clined after 2011, it remained positive. Appendix Figure C.2 shows that both the migrants' share in the city's population and in the formal sector continued trending upward after 2011.

The policy change was unexpected and plausibly exogenous to individuals. While population pressure in China's largest cities was a prominent topic of public debate throughout the 2000s, that decade also coincided with a nationwide wave of loosening *hukou* restrictions (Fan, 2019). There was much uncertainty regarding the direction of *hukou* policies, not to mention the timing and the magnitude of any such policy changes. The higher-than-expected population counts from the 2010 decennial census came as a shock to policymakers in the city, who responded with a significant tightening of the *hukou* policy.

The reduction in *hukou* quotas was not even across all employers. According to the official documents, quota tightening was concentrated in the private sector, futher tilting the allocation of quotas toward the public sector. This has implications for sorting patterns because public-sector employers, on average, offer lower wage premiums than private-sector employers (see Appendix Table B.4). On the worker side, young migrants and those with high skills are more affected because they are the primary recipients of *hukou* quotas.[18]

Employer-sponsored *hukou* quotas have implications for the migrant-native wage gap and the sorting of workers across employers. First, quotas are a workplace amenity specific to migrants. According to the theory of compensating wage differentials, migrants are willing to accept a lower wage in exchange for this amenity, which could explain the wage-setting effect reported in Table 2. A reduction in the number of quotas induces employers to offer higher wages to migrants. Second, the allocation of quotas affects the sorting of workers across employers. All else equal, migrants prefer to work for employers with quotas, and those with a high willingness to pay for the quota are likely to outbid others by accepting a lower wage. In the next section, we present a wage-posting model with employer-sponsored *hukou* quota as an amenity specific to migrants. The model generates testable predictions about the policy's impacts on the migrant-native wage gap

[18]For the whole sample, we do not observe which employers or which jobs have quotas, or whether a migrant has a local *hukou*. In Appendix E, we focus on a small sample of workers for whom the *hukou* status is known. In this sample, 70% of migrants with a local *hukou* work in the public sector, while over 80% of those without a local *hukou* work in the private sector. This is strong evidence that the public sector is allocated more quotas than the private sector. In addition, we investigate a large sample of job postings in 2024 from a leading online platform. Positions posted by public-sector employers are 35% more likely to come with a quota.

and its components.

5 Conceptual Framework

In this section, we develop a wage posting model with heterogeneous firms and workers. We model *hukou* quotas as a workplace amenity that has positive value only for migrants. Following Card et al. (2018), workers have different efficient units of labor and different preferences for wages and amenities; heterogeneous firms with exogenous productivity and quotas endowment set optimal wages for migrants and natives. The model generates several empirically-testable predictions.

5.1 Workers' Labor Supply Problem

There are J firms and G types of workers, who differ in their migration status (migrants and natives), skills (high-skilled and low-skilled), and age (young and old). Each firm $j \in \{1, ..., J\}$ posts type-specific wages w_{gj} for workers of type g that are costlessly observable by all workers.

For worker i of type g, the indirect utility of working at firm j is:

$$u_{igj} = \beta_g \ln(w_{gj}) + \gamma_g a_{gj} + \epsilon_{igj}$$

where a_{gj} is the *hukou* quota offered by firm j. The coefficients β_g and γ_g capture the type-specific preferences for wage and for the *hukou* quota, respectively. We assume $\gamma_g = 0$ for natives. The error term ϵ_{igj} captures idiosyncratic preferences for individual i working at firm j, which are independent draws from a type-I extreme value distribution.

Given posted wages, workers choose the firm that gives them the highest utility. According to McFadden (1973), workers have logit choice probabilities as follows:

$$p_{gj} \equiv P(\arg\max_{k \in \{1,...,J\}} \{u_{igk}\} = j) = \frac{\exp(\beta_g \ln(w_{gj}) + \gamma_g a_{gj})}{\sum_{k=1}^{J} \exp(\beta_g \ln(w_{gk}) + \gamma_g a_{gk})}$$

To simplify the analysis and abstract from strategic interactions in wage setting, we assume that the number of firms J is very large, in which case the logit probabilities can be approximated by the exponential probabilities

$$p_{gj} = \lambda_g \exp(\beta_g \ln(w_{gj}) + \gamma_g a_{gj})$$

where λ_g is a constant common to all firms in the market. Thus, for a large J, the approximate firm-specific labor supply function is:

$$\ln N_{gj}(w_{gj}) = \ln(N_g \lambda_g) + \beta_g \ln(w_{gj}) + \gamma_g a_{gj} \tag{6}$$

where N_g is the number of type g workers in the labor market.

5.2 Firms' Optimization Problem

Firms have the linear production function

$$Y_j = T_j(\sum_g z_g N_{gj})$$

where T_j is the labor productivity of firm j, z_g represents the efficient units of labor of a type g worker, and N_{gj} is the labor supply of a type g worker in firm j.

We assume that the number of *hukou* quotas a_{gj} and its associated cost c are exogenous. The firm's problem is to post a set of type-specific wages that minimize the cost of labor:[19]

$$\min_{w_{gj}} \sum_g (w_{gj}(1 + a_{gj}c))N_{gj}(w_{gj}) \quad \text{such that} \quad T_j(\sum_g z_g N_{gj}) \geq Y$$

The first-order conditions can be written as:

$$w_{gj}(1 + a_{gj}c)\frac{1 + e_{gj}}{e_{gj}} = T_j z_g$$

where e_{gj} is the elasticity of the labor supply of type g workers. From Equation (6), the elasticity is:

$$e_{gj} = \beta_g$$

With these expressions, the firm's first-order condition can be rewritten as:

$$w_{gj} = \frac{\beta_g}{1 + \beta_g} \frac{z_g T_j}{(1 + a_{gj}c)}$$

[19] Firms cannot observe workers' preference shocks (ϵ_{igj}), which prevents them from perfectly price discriminating against workers according to their idiosyncratic reservation values.

Taking logarithms, we obtain:

$$\ln w_{gj} = \ln\left(\frac{\beta_g}{1+\beta_g}\right) + \ln(z_g) + \ln(T_j) - \ln(1 + a_{gj}c) \tag{7}$$

By using Equation (7), we can take the derivative of $\ln w_{gj}$ with respect to the *hukou* quota a_{gj}, resulting in:

$$\frac{\partial \ln w_{gj}}{\partial a_{gj}} = -\frac{c}{1 + a_{gj}c} < 0 \tag{8}$$

5.3 Predictions of the Model

Equation (8) generates two predictions on the cross-sectional wage gap between migrants and natives.

Prediction 1: Since firms provide *hukou* to migrants but not natives, migrants will be paid lower wages than natives in the same firm — a *negative wage-setting effect* (the wage premium for migrants is lower than that for natives).

Prediction 2: The negative wage-setting effect is particularly pronounced among workers who are *young* or *skilled*, as they are the primary beneficiaries of the *hukou* quotas, and among those employed in the *private sector*, where quotas were drastically reduced.

Equation (8) also generates a prediction related to the effect of the *hukou* quota tightening.

Prediction 3: The reduction in *hukou* quotas will *dampen the negative wage-setting effect*, particularly for workers who are young, skilled, or employed in the private sector.

When we plug Equation (7) into Equation (6), the labor supply of type g workers to firm j given the firm's optimal wage is:

$$\ln N_{gj}(w_{gj}) = \ln(N_g \lambda_g) + \beta_g \left(\ln\left(\frac{\beta_g}{1+\beta_g}\right) + \ln(z_g) + \ln(T_j) - \ln(1 + a_{gj}c)\right) + \gamma_g a_{gj}$$

We take the derivative of $\ln N_{gj}$ with respect to *hukou* quota a_{gj} to obtain:

$$\frac{\partial \ln N_{gj}}{\partial a_{gj}} = -\frac{\beta_g c}{1 + a_{gj}c} + \gamma_g \tag{9}$$

Therefore, $\frac{\partial \ln N_{gj}}{\partial a_{gj}} > 0$ if and only if $\frac{\gamma_g}{\beta_g} > \frac{c}{1+a_{gj}c} \equiv \bar{\gamma}_g$. A reduction in the number of quotas results in a decline in the labor supply of type g worker only if the worker's relative demand for *hukou* (γ_g/β_g) is greater than the threshold $\bar{\gamma}_g$.

Equation (9) generates a prediction on the cross-sectional sorting effect and a prediction regarding the change in the *hukou* quota.

Prediction 4: If migrants' relative demand for *hukou* is sufficiently large ($\gamma_g/\beta_g > \bar{\gamma}_g$) and if firms with higher wage premiums have fewer *hukou* quotas (e.g., private firms), migrants are more likely to sort into low-premium firms than natives — *a negative residual sorting effect*.

Prediction 5: If the reduction in the *hukou* quota primarily targets the private sector, which tends to offer higher wage premiums, *the residual sorting effect will decline*.

Prediction 5 suggests that a reduction in the *hukou* quota in the private sector is likely to induce migrants to move from the private sector to the public sector. Since migrants, on average, are higher-skilled than natives, and the private sector tends to offer higher wage premiums than the public sector, positive sorting will decline and the misallocation of migrant workers will worsen.

Our model generates a wage equation that is consistent with the AKM specification. We use this wage equation to conduct the wage decomposition. Consider the average wage gap between migrants ($g = M$) and natives ($g = N$):

$$\begin{aligned} E[\ln w_{Mj}] - E[\ln w_{Nj}] = & \underbrace{(E[\ln z_M] - E[\ln z_N])}_{\text{gap in workers' skills}} + \underbrace{\left(E[\ln(T_{Mj})] - E[\ln(T_{Nj})]\right)}_{\text{gap in firm productivity}} \\ & + \underbrace{\left(E\left[\ln\left(\frac{\beta_M}{1+\beta_M}\right)\right] - E\left[\ln\left(\frac{\beta_N}{1+\beta_N}\right)\right]\right)}_{\text{gap in workers' bargaining power}} - \underbrace{E[\ln(1+a_{Mj}c)]}_{\text{wage penalty for } \textit{hukou}} \end{aligned} \tag{10}$$

where $a_{Nj} = 0$ because firms do not need to provide *hukou* to natives. The first term in Equation (10) represents the gap in skills between migrants and natives. The subsequent terms reflect the gap in the employer premium, comprising the gap in employer productivity, the gap in workers' bargaining power (which is affected by workers' labor supply elasticity β_g), and the wage penalty for migrants as compensation for the *hukou* quota.

Following the policy change, quotas in the private sector are reduced. The wage-

setting effect suggests an increase in the migrant-native wage gap because the wage penalty for *hukou* ($E[\ln(1 + a_{Mj}c)]$) decreases (resulting in a positive wage-setting effect). The sorting effect suggests that the gap in firm productivity ($E[\ln(T_{Mj})] - E[\ln(T_{Nj})]$) shrinks because migrants, who tend to be higher skilled than natives, are now more likely to work in the less productive public sector (resulting in a negative sorting effect). Therefore, a reduction in *hukou* quotas has a positive effect on the migrant-native wage gap if the wage-setting effect dominates the sorting effect.

6 *Hukou* Quota Tightening and the Migrant-Native Wage Gap

This section presents empirical tests for the model predictions. We focus on establishing causal relationships by leveraging the 2011 reduction of the *hukou* quotas. Appendix C.1 presents some *cross-sectional* evidence consistent with the model predictions.

6.1 Wage Gap Decomposition Before and After the Policy Change

We first estimate the AKM model and conduct the same decomposition exercise described in Section 3 using data between 2011 and 2014, i.e., the period following the policy change. We limit the sample to employers who appear at least once in both the pre- and post-periods, but we do not impose restrictions on workers. Summary statistics are reported in Appendix C.[20] Table 3 Column 1 reports the average wage gap and its components in the period prior to the policy change (2006–2010) and duplicates Table 2 Column 1. Column 2 reports the decomposition results in the period following the policy change (2011–2014). All components are calculated as the difference between migrants and natives. Column 3 reports the change between the two periods. For simplicity, when we say a certain effect "increases" after the policy change, we mean an increase of migrants' average wage relative to that of natives.

The average wage gap between migrants and natives is 4.7 log points in the post-period. Migrants in the formal sector remain more positively selected than natives. The gap in the average person effects between migrants and natives is 15.6 log points in the post-period versus 27.1 log points in the pre-period. At the same time, the aver-

[20] Sample restrictions for the 2011–2014 period (post policy change) are the same as those for the 2006–2010 period (pre policy change). Appendix B.1 reports the results from the validity tests of the AKM assumptions. We conduct decompositions as described in Equations (2) and (3). The results of the alternative decompositions are reported in Appendix B.4.

Table 3: Wage Gap Decomposition Before and After the Policy Change

	Pre (1)	Post (2)	Change (3)
Log wage gap (Migrant-Native)	0.134	0.047	-0.086
	[0.001]	[0.001]	[0.002]
1. Person effect	0.271	0.156	-0.115
	[0.023]	[0.015]	[0.028]
2. Covariates	-0.097	-0.061	0.036
	[0.002]	[0.001]	[0.003]
3. Employer effect	-0.040	-0.048	-0.008
	[0.024]	[0.015]	[0.028]
3.1. Wage setting	-0.043	0.000	0.043
	[0.023]	[0.015]	[0.028]
3.2. Sorting	0.003	-0.048	-0.051
	[0.003]	[0.003]	[0.004]
3.2.1. Skill-based sorting	0.047	0.019	-0.028
	[0.002]	[0.001]	[0.002]
3.2.2. Residual sorting	-0.044	-0.067	-0.023
	[0.003]	[0.002]	[0.003]

Notes: Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3). Bootstrapped standard errors are in brackets.

age migrant-native gap in covariates narrows from -9.7 log points to -6.1 log points.[21] Overall, the migrant-native wage gap attributable to differences in worker characteristics narrows from 17.4 (27.1-9.7) to 9.5 (15.6-6.1) log points. Changes in worker skill compositions reflect secular trends. For example, the formal sector expanded during the sample period and gradually covered more lower-skilled migrants. We do not focus on changes in the wage gap due to changes in worker skill compositions.[22]

The migrant-native gap in average employer effects decreases by 0.8 log points after the policy change. The wage-setting effect increases by 4.3 log points in migrants' favor, while the sorting effect decreases by 5.1 log points. We further decompose the change in the sorting effect, and find that skill-based sorting reduces the average wage of migrants relative to that of natives by 2.8 log points, which corresponds to changes in the skill compositions. Meanwhile, the residual sorting effect declines by 2.3 log points.[23]

[21] This includes both the change in the gap of covariates and the change in the gap of their returns. Holding the returns to covariates constant in the post-period, the average migrant-native gap in covariates narrows to -7.5 log points, which accounts for 60% of the observed change.

[22] Changes in the distributions of worker characteristics are not driving the changes in the wage-setting and residual-sorting effects. Appendix Table B.4 reports the pre-period and post-period decompositions with workers reweighted to match the skill and demographic distributions of native workers in the pre-period. The changes in the wage setting and residual sorting effects between the two periods are similar to the changes using the unweighted samples.

[23] Appendix Table B.3 reports the decomposition of the group wage gaps in both periods using the alternative formula described in Equations (4) and (5). The wage-setting effect increases by 4.4 log points while the residual sorting effect decreases by 2.8 log points.

Using the model, we can deduce the value of a *hukou* from the estimated wage-setting effect. Recall that Equation (7) implies:

$$\ln w_{M1} - \ln w_{M0} = -\ln(1 + a_{M1}c) + \ln(1 + a_{M0}c)$$

Assuming that the *hukou* quota prior to the policy change was 0.11 (reflecting government-issued quotas equivalent to 11% of migrants entering the labor market in 2010) and the quota following the policy change is 0.066 (indicating a 40% reduction), the equation becomes:

$$-\ln(1 + 0.066c) + \ln(1 + 0.11c) = 0.043$$

Thus, $c = 1.069$. The value of a *hukou* is obtained by calculating $\ln(1 + c)$, which corresponds to 73% of annual earnings. Considering that positions offering a *hukou* quota typically require an average of three to five years of service, the total value of a quota amounts to 219% to 365% of annual earnings.

The observed increase in the wage-setting effect and decline in the residual sorting effect align with the predictions of the model. Employers, with reduced *hukou* quotas, respond by raising wages to compensate migrants. The worsening residual sorting effect reflects an increased probability of high-skilled migrants working in the public sector, which tends to offer lower premiums than the private sector. Of course, there could be mechanisms that are not captured by our model. For instance, fewer quotas create scarcity. While employers that face a reduction in quotas may find it necessary to raise migrant wages, those with access to the scarce quotas now wield more market power and can further depress migrant wages and be more selective in hiring. Therefore, the overall impacts on wages and sorting depend on the relative strengths of the two competing forces.

Private-sector employers experienced a larger reduction in quotas. As a result, we expect the increase in the wage-setting effect to be greater in the private sector. Changes in the residual sorting effect *within* each sector depends on how quotas are allocated across employers in each sector; unfortunately, we do not have that information.

Sector-specific changes in the wage gap and its components can be easily calculated by summing up the corresponding terms outlined in Equations (2) and (3) for each sector. Table 4 reports the results. The wage-setting effect increases in both sectors, but the magnitude is much larger in the private sector (5.7 log points) than in the public sector

(1.6 log points). Changes in the residual-sorting effect are minimal in both sectors. This suggests that the meaningful decline in the residual-sorting effect for the whole sample (-2.3 log points in Table 3 Column 3) is driven by migrant workers moving *across* sectors. We formally test this hypothesis in Section 6.2.4.

Table 4: Decomposition of the Gap in Employer Effect by Sector

	Public Sector			Private Sector		
	Pre	Post	Change	Pre	Post	Change
	(1)	(2)	(3)	(4)	(5)	(6)
Employer effect	-0.091	-0.089	0.002	-0.016	0.013	0.028
	[0.025]	[0.016]	[0.030]	[0.022]	[0.014]	[0.026]
1. Wage-setting	-0.077	-0.061	0.016	-0.015	0.042	0.057
	[0.026]	[0.017]	[0.031]	[0.022]	[0.013]	[0.026]
2. Sorting	-0.014	-0.027	-0.014	0.000	-0.029	-0.029
	[0.005]	[0.005]	[0.007]	[0.003]	[0.003]	[0.004]
2.1. Skill-based sorting	0.020	0.005	-0.015	0.066	0.041	-0.025
	[0.002]	[0.002]	[0.003]	[0.002]	[0.001]	[0.003]
2.2. Residual sorting	-0.034	-0.033	0.001	-0.067	-0.070	-0.004
	[0.004]	[0.003]	[0.005]	[0.003]	[0.002]	[0.004]

Notes: Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3), and are conducted separately for the public and private sectors. Bootstrapped standard errors are in brackets.

6.2 Identifying the Causal Impacts of the Policy Change

6.2.1 Regression Analyses

Our model generates predictions on the heterogeneous effects of the quota tightening on different worker subgroups. We postulate that young workers have a higher willingness to pay for a quota. Recall that we define migration status based on an individual's place of origin and that we do not observe his *current hukou* status. Older migrants in the sample may have obtained a local *hukou* or made arrangements that mitigate the inconveniences of not having one (e.g., by marrying a native resident).[24] In addition, high-skilled migrants are presumed to have a stronger preference for a *hukou* compared to low-skilled migrants, who tend to have less stable employment and are less likely to be permanent migrants. Moreover, the government allocates the majority of the quotas to young workers and individuals with college or postgraduate degrees. As quotas tighten, these individuals are more likely to be impacted. Our model unambiguously predicts the wage-setting effect to be stronger among workers who are young, skilled, or employed

[24] According to the 2010 census data, the proportion of migrants with a local *hukou* is increasing in age (see Appendix Figure A.2).

in the private sector. On the other hand, the effect of quota tightening on residual sorting by worker subgroups depends on their relative preferences for a *hukou* and the allocation of the remaining quotas between high- and low-premium employers, which is unknown to us.

In theory, our decomposition approach can be naturally generalized to include worker subgroups. Instead of assuming that employers have differential wage and hiring policies for migrants and natives, we can allow these policies to vary more flexibly by migration status, age, and skills. However, extending the AKM model to accommodate more detailed worker groups is not practically feasible in our setting. Specifically, allowing employers to pay different wage premiums based on migration status, age, and skills means that we need to estimate at least eight separate AKM models in each period. The estimated worker and employer effects from each of these equations are comparable only for a subset of workers and employers that are "connected" in all eight subgroups. To be included in the working sample, an employer needs to have at least one mover in *each* of the eight subgroups. Given our relatively short panel (five years prior to the policy change and four years after), the size of the resulting "octa-connected" sample is too small to be statistically meaningful.

Hence, we use regression analyses to exploit the heterogeneous impacts of the quota tightening on worker subgroups. We present evidence analogous to the decomposition analysis. Employer sector and workers' age are directly observed in the data, but workers' skills are not. We use the person effects estimated from Equation (1) as a measure of workers' skills. However, including the estimated parameters as regressors introduces error. Therefore, we also present evidence using specifications that do not rely on any estimates from the AKM model.

6.2.2 Event Studies

We first show that regression analyses produce causal impacts of the quota tightening on the overall migrant-native wage gap that are comparable to the decomposition approach (Table 3). Consider the following difference-in-differences model:

$$\ln y_{gijt} = \gamma \cdot Mig_i \times \text{Post}_t + \delta \cdot Mig_i + X'_{git}\beta_X + \hat{\alpha}_{gi}\beta_I(+\lambda_{jp}) + \tau_t + \varepsilon_{gijt} \tag{11}$$

where $\ln y_{gijt}$ is the log wage of worker i of group $g \in \{M, N\}$ employed by employer j in year t; Mig_i is an indicator for migrant; Post_t indicates the post-period between 2011 and 2014; X_{git} flexibly captures workers' experiences with a set of indicators for age interacted with gender; $\hat{\alpha}_{gi}$ is the person effect estimated from Equation (1), which enters non-parametrically as deciles, allowing us to control for differences in workers' skills and skill-based sorting; τ_t is the year fixed effects; and ε_{gijt} is the error term.

Note that λ_{jp} is the employer-by-period fixed effects, where period p is either the pre-period (2006–2010) or the post-period (2011–2014). By including λ_{jp}, we allow employer behavior to change flexibly between the two periods, although the results are almost identical to controlling for λ_j. The parameter of interest is γ, which captures the change in the migrant-native wage gap due to the quota tightening. We estimate Equation (11) with and without controlling for λ_{jp}. Without controlling for λ_{jp}, γ captures the overall change in the wage gap conditional on worker characteristics. Controlling for λ_{jp}, γ captures the change in the within-employer relative wage setting effect.

Column 1 of Table 5 shows that after the policy change, the average wage of migrants increases by 5.3 log points relative to that of natives. Column 2 indicates that the wage-setting effect increases by 7.8 log points in favor of migrants. The increase in the wage-setting effect is larger than the overall increase in the relative wage gap, which suggests that the residual sorting effect worsens for migrants. These findings are consistent with the before-and-after decomposition results reported in Table 3.

We estimate a version of the model where we replace person-effect deciles with a set of individual fixed effects, tracking workers who appear in the sample both before and after the policy change. This specification is completely independent of the parameters estimated from the AKM model.[25] But it comes with a cost. As described in Section 4, the quota system disproportionately affects recent college graduates. However, the college graduates who entered the labor market after the policy change are excluded in this specification.

Column 3 of Table 5 shows that following the policy change, the average wage of migrants increases by 7.4 log points relative to that of natives. Further conditional on staying with the same employer, Column 4 indicates that the relative wage increase for

[25] Therefore, this set of regressions is not subject to potential biases in the estimations of standard errors. Due to the huge computational cost of bootstrapping, we did not conduct the bootstrap analysis for this specification of the model.

migrants following the policy change is 6.0 log points.[26] For the remaining analyses, we always keep person effects from the AKM model, so our analyses depict a more complete picture of changes in the labor market. However, we note that the finding of a relative increase in migrant wage is robust to within-worker analyses.

Table 5: Quota Reduction and the Wage Gap: Difference-in-differences Estimates

	(1)	(2)	(3)	(4)
	Outcome variable: log income			
Migrant × Post	0.0525	0.0774	0.0741	0.0595
	(0.00824)***	(0.00400)***	(0.00532)***	(0.00196)***
	[0.0280]*	[0.0242]***		
Migrant	-0.0963	-0.0709		
	(0.00839)***	(0.00343)***		
	[0.0239]***	[0.0209]***		
Fixed Effects				
Year	X	X	X	X
Gender-Age-Period	X	X	X	X
PE decile-Period	X	X		
Individual			X	X
Employer-Period		X		X
Observations (mil.)	17.6	17.6	16.8	16.8
R-squared	0.691	0.905	0.898	0.923

Notes: The table reports the estimation results of Equation (11). Person Effects are estimated in Equation (1). Conventional standard errors in parentheses are clustered at the employer level. Bootstrapped standard errors are in brackets. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

One advantage of the difference-in-differences model is that it can be easily extended to the event study specification, which can test the parallel pre-trend hypothesis. Consider the following model:

$$\ln y_{gijt} = \sum_{\substack{m=-5 \\ m\neq -1}}^{3} \gamma_m \cdot Mig_i \times t_m + \delta \cdot Mig_i + X'_{it}\beta_X + \hat{\alpha}_i\beta_I(+\lambda_{jp}) + \tau_t + \varepsilon_{gijt}, \tag{12}$$

where t_m indicates the year relative to 2011. We set the base year as 2010, the year before the policy change. Our sample covers five years before the policy change and four years after.

Figure 2 plots $\hat{\gamma}_m$ from the estimation of Equation (12). The solid line is from the specification without controlling for λ_{jp}, which captures the overall change in the migrant-native wage gap. The dashed line is from the specification controlling for λ_{jp}, which captures changes in the wage-setting effect. In both specifications, there is a sharp increase

[26]The inclusion of employer fixed effects, which account for sorting, does not generate a larger increase in the migrant-native wage gap. This suggests that the decline in the residual sorting effect for migrants following the policy change is driven by new entrants. Section 7 provides supportive evidence for this observation.

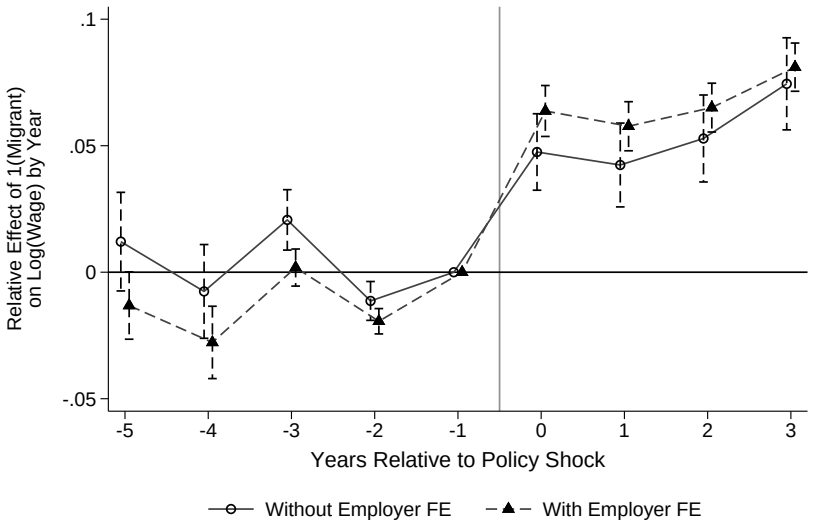


Figure 2: Quota Reduction and the Wage Gap: Event Study

Notes: The graph shows the results from estimating Equation (12). Each circle and triangle indicates the point estimates of the treatment effect (i.e., the coefficient of $Mig_i \times t_m$). Each vertical dashed line indicates the 95% confidence interval based on the conventional standard errors clustered at the employer level. Circles linked by the solid line represent the coefficients when employer-by-period fixed effects are not included in the model. Triangles linked by the dashed line represent the coefficients when employer-by-period fixed effects are included.

of $\hat{\gamma}_m$ at $m = 0$. Subsequently, $\hat{\gamma}_m$ remains statistically significant and stays relatively constant in magnitude. The increase in $\hat{\gamma}_m$ is greater in the specification with employer fixed effects, which is consistent with a negative residual sorting effect for migrants. Importantly, in both specifications, $\hat{\gamma}_m$ is small and generally statistically indistinguishable from zero for $m < 0$. This rules out differential pre-trends between migrants and natives and any other policy changes that do not fully coincide with the quota tightening.

6.2.3 Wage Effects on Worker Subgroups

The quota reductions disproportionately affect workers who are young, skilled, or employed in the private sector. Therefore, we expect the increase in the wage-setting effect to be particularly large for these workers. In addition, given the overall negative change in the residual sorting effect for the entire sample, we expect some subgroups to experience worsened residual sorting among migrants. We test our predictions by estimating

versions of the following model:

$$\begin{aligned}\ln y_{gijt} =& \gamma_1 \cdot Mig_i \times 1(T_{gijt} = 1) \times Post_t + \gamma_2 \cdot Mig_i \times 1(T_{gijt} = 0) \times Post_t \\ &+ \alpha_1 \cdot Mig_i \times 1(T_{gijt} = 1) + \alpha_2 \cdot Mig_i \times 1(T_{gijt} = 0) + \beta_1 \cdot 1(T_{gijt} = 1) \times Post_t \\ &+ \beta_2 \cdot 1(T_{gijt} = 0) \times Post_t + X'_{git}\beta_X + \hat{\alpha}_{gi}\beta_I(+\lambda_{jp}) + \tau_t + \varepsilon_{gijt}\end{aligned} \tag{13}$$

where T_{gijt} indicates the treatment status of worker i. We define treated groups along three dimensions: (i) young workers below the age of 30; (ii) skilled workers with an estimated person effect above the sample median; and (iii) workers employed in the private sector.

Table 6: Quota Reduction and the Wage Gap: Difference-in-differences Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcome variable: log income					
Migrant × Post × High-Skilled	0.0747	0.0866				
	(0.0109)***	(0.00467)***				
	[0.0308]**	[0.0247]***				
Migrant × Post × Low-Skilled	0.0281	0.0634				
	(0.00960)***	(0.00430)***				
	[0.0250]	[0.0234]***				
Migrant × Post × Young			0.106	0.117		
			(0.00974)***	(0.00386)***		
			[0.0288]***	[0.0250]***		
Migrant × Post × Old			-0.00192	0.0388		
			(0.00888)	(0.00456)***		
			[0.0274]	[0.0236]*		
Migrant × Post × Private					0.0900	0.0996
					(0.0107)***	(0.00377)***
					[0.0267]***	[0.0238]***
Migrant × Post × Public					0.0485	0.0479
					(0.0117)***	(0.00704)***
					[0.0302]	[0.0256]*
Two-way interactions	X	X	X	X	X	X
Fixed Effects						
Year	X	X	X	X	X	X
Gender-Age-Period	X	X	X	X	X	X
PE decile-Period	X	X	X	X	X	X
Employer-Period		X		X		X
Observations (mil.)	17.6	17.6	17.6	17.6	17.6	17.6
R-squared	0.692	0.906	0.692	0.906	0.693	0.906

Notes: The table reports the estimation results of Equation (13). Person effects are estimated in Equation (1). Conventional standard errors in parentheses are clustered at the employer level. Bootstrapped standard errors are in brackets. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table 6 reports the results from the estimation of Equation (13). Column 1 shows that the wages of both skilled and unskilled migrants increase relative to natives following the policy change. The increase is larger for skilled migrants (7.5 log points) than for un-

skilled migrants (2.8 log points). Column 2 reports that the wage-setting effect increases by 8.7 log points for skilled migrants and 6.3 log points for unskilled migrants. For each subgroup, the increase in the wage-setting effect is larger than the increase in the overall wage gap, implying a worsening residual sorting effect for migrants in both subgroups.

Columns 3 and 4 report the impacts on workers of different ages. The overall wage effect and the wage-setting effect are primarily driven by young migrants. For migrants in both age groups, the residual sorting effect is negative. The last two columns report the results by sector. The increase in the relative migrant wage is more pronounced in the private sector than the public sector. The coefficients in the two columns are quantitatively similar, suggesting that changes in the within-sector residual-sorting effect are small. These results are consistent with the estimates in Table 4.

To check the parallel trend assumption by subgroup, we also estimate Equation (12) separately for the treated group ($T_{gijt} = 1$) and the control group ($T_{gijt} = 0$). Appendix Figure C.3 reports the corresponding event studies, which show no discernible differential pre-trends between migrants and natives in any subgroup.

6.2.4 Sorting and Sectoral Choices

The reductions in the *hukou* quotas were uneven across employers. Given the heterogeneity in the willingness to pay for a quota among migrants, the skewed quota reduction could affect the sorting of migrants across employers. Although our model generates clear predictions about an employer's hiring practices based on its access to quotas, in practice, we do not observe which positions are associated with quotas.

However, both the wage gap decomposition (Table 3) and the regression analysis (for example, Table 5) suggest that the residual sorting effect worsened for migrants after the policy change. The worsening of the residual sorting effect does not seem to stem from changes in the *within*-sector sorting patterns (Table 4). Therefore, we hypothesize that it may stem from changes in sectoral choices. The allocation of quotas after the policy change further favored public-sector employers, which tend to offer lower wage premiums than private-sector employers (see Appendix Figure B.4). If skilled migrants, in pursuit of a quota, became more likely to work in the public sector after the policy change, residual sorting would have worsened.

We formally test the impacts of the quota tightening on the sectoral choices of workers

with different skills. Consider the following triple-difference specification:

$$\begin{aligned} Private_{it} = \gamma_1 Mig_i \cdot H_i \cdot Post_t + \gamma_2 Mig_i \cdot L_i \cdot Post_t + \alpha_1 Mig_i \cdot H_i + \alpha_2 Mig_i \cdot L_i \\ + \beta_1 \cdot H_i \cdot Post_t + \beta_2 \cdot L_i \cdot Post_t + X'_{giy}\beta_X + \hat{\alpha}_{gi}\beta_I + \Omega_{it} + \varepsilon_{it} \end{aligned} \tag{14}$$

where $Private_{it}$ is a binary outcome variable indicating whether worker i is in the private sector, and H_i and L_i are indicators for the high-skilled and low-skilled, respectively, defined by whether the estimated person effect is above or below the median. Throughout the sample period, the increase in the share of migrants working in the private sector is greater than that of natives (Appendix Figure C.4). We allow for different linear secular trends by migration status and skill level, which we denote succinctly as Ω_{it}. The remaining terms are the same as before. We estimate the equation using the full sample as well as the sample of young workers under the age of 30, who are more directly affected by the quota tightening.

Table 7: Sectoral Choices of Workers

	(1)	(2)
	Outcome variable: = 1 if in the Private Sector	
Migrant × Post × High-Skilled	0.00329	-0.0223
	(0.00752)	(0.00968)**
	[0.00462]	[0.00562]***
Migrant × Post × Low-Skilled	0.0291	0.0118
	(0.00667)***	(0.00828)
	[0.00740]***	[0.00728]
Fixed Effects		
Year	X	X
Gender-Age-Period	X	X
PE decile-Period	X	X
Linear Time Trend		
Migrant Specific	X	X
High-Skilled Migrant Specific	X	X
High-Skilled Specific	X	X
Sample	Full	Young
Observations (mil.)	17.6	7.2
R-squared	0.179	0.094

Notes: The table reports the estimation results of Equation (14). Person Effects are estimated in Equation (1). Conventional standard errors in parentheses are clustered at the employer level. Bootstrapped standard errors are in brackets. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table 7 Column 1 reports the estimation results for the whole sample. Following the policy change, the proportion of low-skilled migrants in the private sector increases relative to that of low-skilled natives. However, among high-skilled workers, migrants are no more likely to be in the private sector following the policy change. The difference in changes between the high-skilled and the low-skilled is statistically significant at the

1% level.

Column 2 reports the results for the sample of young workers. The policy change induced high-skilled young migrants to move from the private sector to the public sector. Following the policy change, high-skilled young migrants are 2.2 percentage points less likely to be in the private sector than high-skilled young natives, and the difference is statistically significant. On the other hand, low-skilled young migrants are slightly more likely to be in the private sector than their native counterparts (with a statistically insignificant coefficient of 0.0118). Appendix Figure C.5 presents the event studies and shows that the parallel trend assumption holds for both low-skilled and high-skilled workers. In sum, for both the overall sample and the sample of young workers, high-skilled migrants are less inclined to work in the private sector compared to their low-skilled counterparts following the policy shock. This indicates that the worsening of the residual sorting is primarily influenced by the sorting across sectors. Given that private firms typically exhibit higher productivity levels, this outcome suggests that the quota tightening exacerbated the misallocation of labor.

6.3 Discussion of Alternative Explanations

We attribute the rise in migrant workers' relative wage after 2011 to the reduction in the number of *hukou* quotas. The parallel trends between migrant and native wages prior to the quota tightening and the subsequent immediate impacts rule out pre-existing trends and confounding shocks that occurred at a different time. Our event study design cannot rule out the roles of other *contemporaneous* shocks. While it is infeasible to rule out all concurrent shocks, we conduct a thorough search of policy documents around 2011 and find that the quota tightening was the only noteworthy policy change that could directly impact the migrant-native wage gap in the formal sector. Furthermore, it is unlikely that any other concurrent shock can reproduce the same set of empirical patterns with the rich heterogeneity documented.

We interpret the rise in the relative migrant wage as a compensating wage differential in response to a reduction in quotas, which are workplace amenities for migrants but not natives. Here, we discuss several alternative mechanisms.

First, as mentioned in Section 4, the 2011 policy change also tightened restrictions on access to local goods and services for migrants without a local *hukou*, which increased the value of a *hukou*. Consequently, migrants should be willing to accept a lower wage to

obtain a quota. However, these predictions contradict our findings.

It is possible that the less welcoming migration policies could reduce the labor supply of migrant workers, causing their wages to rise relative to natives. This alternative interpretation is unlikely to hold for two reasons. First, we have shown that the number of migrant workers and their share in the formal sector continued increasing after the policy change (Appendix Figure C.2). Second, this reasoning is built on the assumption that migrant and native workers, conditional on their skills and other demographic characteristics, are not perfectly substitutable. The relative labor supply could affect the relative wage gap if firms demand *horizontally* differentiable skill types, while the skill type distributions differ among migrants and natives. We argue this is unlikely in the setting of China's internal migration. Both migrants and natives are trained in the same education system and share largely identical cultural backgrounds. Throughout the paper, we assume that skills are perfectly substitutable.[27]

Finally, the tightening of the *hukou* policy could affect the selection of migrants into the formal sector. Indeed, Table 3 reports that the migrant-native gap in person effect declined from 27.1 log points in the pre-period to 15.6 log points to the post-period. However, changes in selection do not affect our results. Both the wage-setting and residual sorting effects are conditional on skill compositions. A decomposition based on reweighted samples where migrants and natives have the same skill and demographic composition yield quantitatively similar results (Appendix Table B.4).

7 Wage Assimilation

A key question regarding the labor market outcomes of migrants is the extent and speed at which their wages converge to those of natives. Initial wage depression followed by subsequent catch-up are salient features of migrants' labor market outcomes. Evidence from the literature is mostly from cross-country migration. The speed and degree of assimilation depend on the religious, linguistic, and cultural distances between the source and destination countries (e.g., Abramitzky et al., 2014). Immigrants often do not fully close the earnings gap (e.g., Borjas, 1989). Internal migrants in China share the same language and culture as natives, but discriminatory migration policies impose unique barriers to wage assimilation.

[27] This assumption is also mandated by the AKM framework, which allows for vertically different skill *levels* but not horizontally differentiable skill *types*.

In this section, we study the dynamics of migrant wage assimilation by tracking cohorts of workers over time. We have shown that the wage penalties experienced by migrants stem from both wage setting and sorting. Young workers are disproportionately affected by the *hukou* policy. They are more likely to settle for a lower-premium employer and accept a lower wage than their native co-workers. Due to frictions in the labor market, initial wage penalties may persist. We focus on cohorts of young workers and investigate how migrants close the wage gap by renegotiating wages with their current employers or climbing up the job ladder.

7.1 Cohort Samples

We follow two cohorts of young workers. The first cohort comprises individuals born between 1980 and 1984. They appear in the sample in 2006, when they were between 22 and 26 years old. A large share of young workers in our sample have a college degree, so workers in this cohort were in the early stages of their careers. We track these workers for five years until 2010, the year before the policy change. The second cohort comprises individuals born between 1985 and 1989. We track them from 2011, when they were between 22 and 26 years old, to 2014, the last year of our sample. The pre-policy-change cohort has 242,317 individuals in the initial year, of which 40.7% are migrants. The post-policy-change cohort has 484,369 individuals in the initial year, of which 55.7% are migrants. There is sizable attrition in the cohort samples, but there is little evidence of differential selectivity in attrition between migrants and natives. Therefore, we focus on the balanced samples, which comprise workers who appear in the data every year. The balanced pre-cohort sample has 104,269 natives and 58,397 migrants while the balanced post-cohort sample has 138,962 natives and 121,847 migrants. For ease of comparison, we reweight workers such that the skill and demographic compositions are the same for migrants and natives in both cohorts.[28]

7.2 Dynamics of Wage Catch-up

To study the evolution of the migrant-native wage gap, we estimate the following model separately for each cohort in each year t:

$$Y_{git} = \gamma^t \cdot \text{Mig}_i + \rho_1^t \cdot \hat{\alpha}_{git} + X'_{git} \cdot \beta_g^t + \varepsilon_{git}. \tag{15}$$

[28] Appendix A.5 provides more details about the construction of the cohort samples.

where Y_{git} is the outcome for individual i in group $g \in \{M, N\}$ in year t. We consider two outcomes: log wage and the employer's wage premium for native workers, which is a proxy for the "job ladder." Meanwhile, Mig_i is an indicator for worker i being a migrant; γ^t captures the migrant-native gap after accounting for demographic (skill, gender, and age) differences and is the coefficient of interest; $\hat{\alpha}_{gi}$ is the person effect from the AKM, which enters the equation non-parametrically in deciles; and X_{git} flexibly controls for gender-specific age profiles.

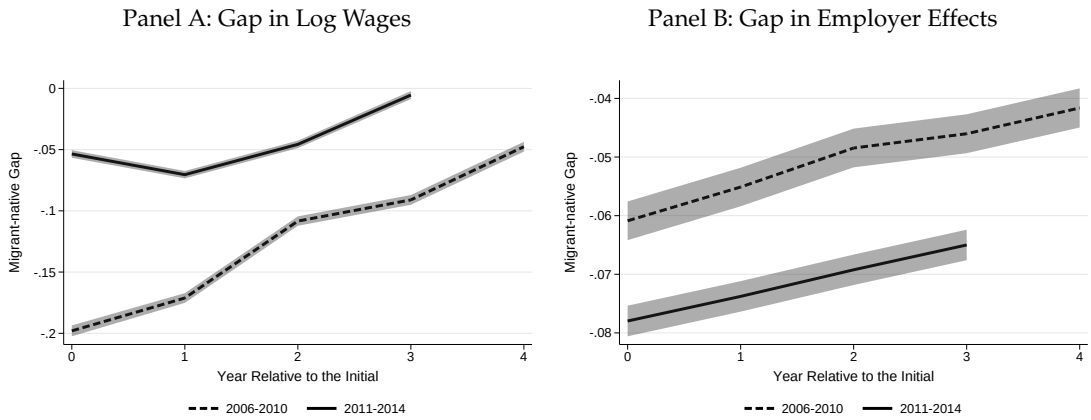


Figure 3: Wage Assimilation between Migrants and Natives

Notes: Each node of the line is from a separate estimation of Equation (15). In Panel A, the dependent variable is the log wage. In Panel B, the dependent variable is the estimated employer effect for natives from the AKM model. Standard errors are clustered at the employer level. Shaded bands indicate 95% confidence intervals.

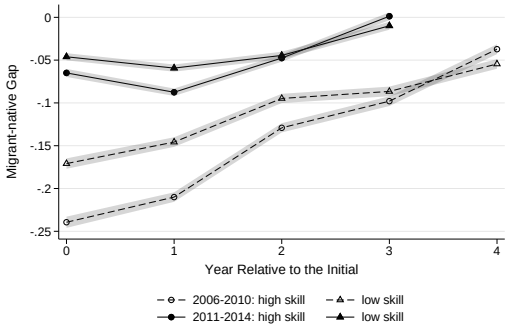
Figure 3 Panel A plots $\hat{\gamma}^t$ from the regressions where the log wage is the dependent variable. For the pre-cohort, migrant wages are 20 log points lower than native wages in the initial year (2006). Wage assimilation is rapid, with the wage gap narrowing at an annualized rate of four log points. After four years, the remaining wage gap is about five log points. The initial wage gap for the post-cohort is five log points, which is much smaller than the corresponding gap for the pre-cohort. This is consistent with our earlier finding that employers compensate migrants for the reduction in *hukou* quotas. Similar to the pre-cohort, wage assimilation for the post-cohort is also quick, with the catch-up nearly complete after three years.

Panel B plots the coefficients from the regressions where the employer effect for natives from the AKM model is the dependent variable. It shows whether migrants are converging with natives in terms of the employer type. Migrants are initially placed with employers that offer lower wage premiums. The initial gap in this measure of the

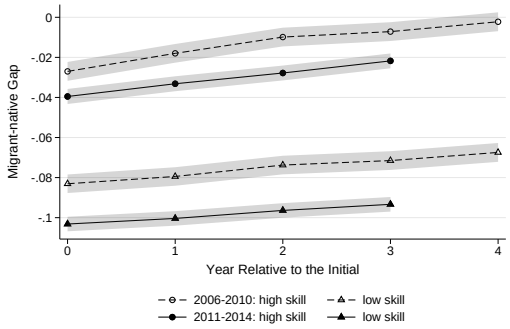
"job ladder" is 6.1 log points for the pre-cohort and 7.8 log points for the post-cohort. The larger gap in employer wage premiums for the post-cohort confirms our previous finding that the residual sorting effect worsened for migrants after the policy change. For both cohorts, migrants do not seem to be locked into low-premium employers. Instead, they progress up the job ladder more quickly than natives. The gap narrows to 4.2 log points for the pre-cohort in four years and 6.5 log points for the post-cohort in three years. The job ladder channel accounts for 13% of the observed wage assimilation for the pre-cohort and 27% for the post-cohort. The remaining reduction in the wage gap can be attributed to migrants negotiating higher wages with their current employers or gaining experience at an accelerated rate.

Panel A: By Skill Level

A1: Gap in Log Wages

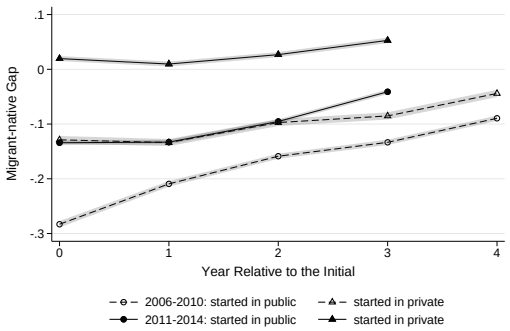


A2: Gap in Employer Effects



Panel B: By Initial Sector of Employment

B1: Gap in Log Wages



B2: Gap in Employer Effects

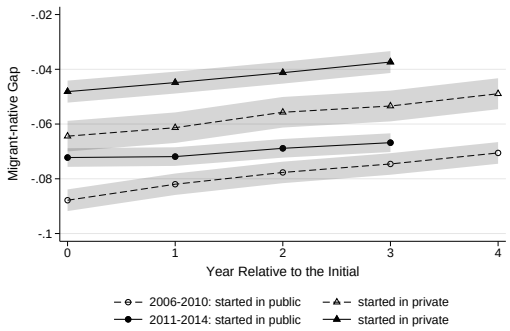


Figure 4: Wage Assimilation by Subgroups

Notes: Each node of the line is from a separate estimation of Equation (15). In the graphs on the left, the dependent variable is the log wage. In the graphs on the right, the dependent variable is the estimated employer effect for native workers. In Panel A, the sample is divided by workers' skill level. In Panel B, the sample is divided according to the sector of initial employment. Standard errors are clustered at the employer level. Shaded bands indicate 95% confidence intervals.

Figure 4 shows the dynamics of wage assimilation by subgroups. We divide workers along two dimensions: skill level and sector of employment in the initial year. Panel A shows that the initial migrant-native wage gap is larger for high-skilled workers than low-skilled workers. For example, Graph A1 shows that for the pre-cohort, the migrant-native wage gap in the initial year is 24 log points among high-skilled workers and 17 log points among low-skilled workers. The wage gap closes quickly for both subgroups, but especially so for high-skilled workers. After four years, the high-skilled migrant-native wage gap has converged to the low-skilled migrant-native wage gap of 5 log points. The migrant-native wage gaps of both the high-skilled and low-skilled are smaller in the initial year for the post-cohort than the pre-cohort, and they converge in approximately three years. Graph A2 shows that the narrowing of the employer premium gap is similar between the high skilled and low skilled. In both subgroups, migrants' faster advancement on the job ladder accounts for a substantial share of the overall wage assimilation.

Graph B1 shows that the initial migrant-native wage gaps are smaller for the post-cohort than the pre-cohort. For both cohorts, migrants in the public sector start with a larger wage gap than those in the private sector but catch up more quickly in subsequent years. Graph B2 shows that *within* each sector, migrant wages are lower than native wages due to a negative sorting effect. Conditional on the sector of initial employment, sorting improves slightly for the post-cohort, which again suggests that the overall decline in the sorting effect is driven by sectoral choice. Migrants advance up the job ladder at a faster rate for all subgroups.[29]

8 Conclusion

While differential treatments of migrants and natives in labor markets are well-documented, this paper is among the first to examine the role of employers in shaping the migrant-native wage gap. Using unique employer-employee-linked administrative records from a large Chinese city and a decomposition method based on the two-way fixed effects model, we show that conditional on workers' skills, migrant wages are lower because they are paid less than their native co-workers by the same employer and are less likely to work for high-premium employers. Collectively, these two channels account for 8.7

[29] Appendix Table F.1 reports the dynamics of the migrant-native wage gap and its components for both cohorts. Appendix Table F.2 reports the version without reweighting to balance worker composition. While reweighting affects the migrant-native wage gaps in the initial year, it does not change the conclusion regarding wage assimilation and its components.

log points of the migrant-native wage gap between 2006 and 2010.

We provide causal evidence that the *hukou* policy is a major source of the wage penalty endured by migrants. Following a substantial reduction in *hukou* quotas — a workplace amenity for migrants but not natives — migrants' wages rose relative to those of natives, which is consistent with the theory of compensating wage differentials. Increases in the wage-setting effect are stronger for young and skilled migrants, who are more affected by the quota tightening, and those in the private sector, where quotas declined more precipitously. On the other hand, as the remaining quotas became more concentrated in the relatively low-paying public sector, the residual sorting effect worsened for migrants. We present corroborative evidence that high-skilled young migrants are more likely to work in the public sector after the policy shock.

Our back-of-envelope calculation suggests that migrants are willing to sacrifice 2.2 to 3.7 years of earnings in exchange for a *hukou*. While young migrant workers initially face a substantial wage penalty, the wage gap closes within a few years. Wage assimilation is driven by migrants' faster advancement on the job ladder compared to natives.

A limitation of the data is that we do not observe which employers are allocated quotas and which workers obtain the quotas. The allocation of quotas across employers has profound implications for the sorting of migrants. Controlling for the skill level, migrants are less likely to be hired by high-premium employers, especially after the quota tightening. This suggests that the allocation of quotas in the studied city may lead to the misallocation of migrant workers. In recent years, the studied city has transitioned from the employer-sponsored quota system to a point-based mechanism, in which the qualification for a local *hukou* depends mostly on the individual's characteristics and less on whom they work for. The effects of the new *hukou* system on the migrant-native wage gap and labor market efficiency are fertile ground for future research.

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ONLINE APPENDIX, NOT FOR PUBLICATION

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A Data Appendix

A.1 Data Cleaning and Construction of Variables

Our original employer-employee-linked panel is at the monthly level. We form a worker-year panel by retaining observations in November of each year. The choice of November is arbitrary. We prefer using data close to the end of the year because any policy introduced during the year should have been implemented by then. Using observations from a different month is unlikely to affect our conclusions. This is especially true in our case because the Housing Provident Fund (HPF) contribution is calculated based on the *annual* earnings rather than the monthly wage. Moreover, November has one of the lowest job transition rates, making HPF contribution records in that month less prone to interruptions or misreporting due to job transitions.

In the sample period, the firm's HPF contribution is a fixed percentage of the worker's earnings. In 2006, the fixed percentage is either 8% or 10%. In 2007, the fixed percentage is either 8% or 12%. From 2008 to 2014, the rate is fixed at 12% for all employers. The contribution rate is fixed for an employer in a given year. We observe this information for each employer, which allows us to deduce workers' annual earnings. The HPF contribution is based on earnings that include contractual salaries and regular bonuses but not irregular bonuses or awards.

Earnings eligible for the HPF are capped at three times the city's social average wage. The social average wage is defined as the average annual earnings of full-time workers in all legal entities in the current year and was approximately 55,000 yuan in 2010. Around 7% of observations have a wage reaching the cap, and 9% of workers have reached the cap at least once during their time in the sample. The share of capped wage records is largely stable over time, ranging between 5.8% and 7.9%. Migrants have higher average wages and are more likely to hit the cap. Therefore, the presence of top coding would underestimate any shock that potentially increases the migrant relative wage.

To impute wages above the cap, we fit a log-normal function of observed wages below the cap for each year. The right tail of the wage distribution above the cap is assumed to follow the same fitted log-normal distribution. We impute the wage distribution separately for migrants and natives. Specifically, we randomly assign a wage in the right tail of the imputed distribution to an individual whose wage is capped. A worker with capped wages across multiple years is assigned the same rank in the imputed wage dis-

tribution each year. This approach prevents wage fluctuations across the years due to random assignment. Overall, this imputation method is largely assumption-free and imposes minimal restrictions on how wages are determined among different demographic groups.

A.2 Representativeness of the Data

The statistics on wages published by the studied city are limited. The city's statistical yearbooks report the average annual wage of all employees. Rows (1) – (2) of Table A.1 show that the annual earnings imputed from the HPF contributions are about 20% higher than the city's average wage. This is because employers are required to match an employee's HPF contributions, which is a substantial share of wages. Hence, jobs that contribute to the HPF are relatively high-paying.

We also compare the HPF sample in 2010 with the 0.1% individual sample from the 2010 population census. For the census, we focus on employed individuals in the studied city who are in the same age range as the HPF sample (22–50 years old). Since the census does not indicate whether a worker is employed in the formal sector, this comparison highlights the differences between jobs covered by the HPF and the broader labor market. Rows (3) – (6) of Table A.1 show that workers in our sample are slightly younger (average age of 33.9 versus 34.6), more likely to be female (46% versus 43%), and more likely to be natives (59% versus 45%). Additionally, workers in the HPF sample are much more skilled. 47% of those in the HPF sample for whom we can observe education level have a college degree (we discuss this small sample in Section E). In the census sample, only 27% are college graduates.

Table A.1 shows that workers covered by the HPF are a positively selected sample. As such, we do not expect the wage gap and its composition reported in our main analysis to be representative of the city's labor market as a whole. However, the HPF sample is a relevant population for studying the impact of the *hukou* quota policy on the migrant-native wage gap because essentially all employer-sponsored quotas are awarded to jobs covered by the HPF. We treat the differential selection into the HPF sample as given. The calculation of the migrant-native wage gap and its components are always conditional on group differences in skill and demographic (gender and age) compositions.

Table A.1: Comparison with Official Statistics

	HPF Data	Aggregate Data	Source
Earnings (2006–2014)	69,813	54,675	Statistical Yearbook (2007-2015)
Earnings (2010)	66,314	55,462	Statistical Yearbook (2011)
Share of female (2010)	0.46	0.43	Census 2010
Average Age (2010)	33.85	34.61	Census 2010
Share of native (2010)	0.59	0.45	Census 2010
Share with a college degree (2010)	0.47	0.27	Census 2010

Notes: For the census data, only individuals between the ages of 22 and 50 who are currently employed are included in the sample. The share with a college degree in the HPF sample is from a subsample of workers who applied for a mortgage using the HPF during the sample period. Appendix E provides more details about the mortgage sample.

A.3 Supplementary Analyses of the Migrant-Native Wage Gap

Anatomy of the Wage Gap in the HPF Sample. Table 1 shows that in the HPF sample, migrants earn a higher average wage than natives. Our decomposition analysis reveals that this is because migrants in the sample are, on average, much more skilled than natives. Conditional on skill, migrants earn substantially lower wages due to employers' hiring and pay practices.

The HPF data does not have measures of worker skills such as education. Instead, we recover worker skills from the AKM model. It is worth noting that the person effect from the AKM model captures skills that are both observed and unobserved in a typical dataset. Here, we conduct a series of linear regressions to demonstrate that education alone is insufficient to account for worker skills. Instead, the unobserved worker skills estimated by the AKM model are crucial to the analysis.

Consider the following model with the pooled HPF sample between 2006 and 2010:

$$\ln y_{it} = \beta_0 + \beta_1 Mig_i + \beta X_{it} + \gamma W_{j(i,t)t} + \lambda_t + \varepsilon_{it}. \tag{A.1}$$

where y_{it} is worker i's wage in year t; Mig_i is an indicator for a migrant; X_{it} is a set of time-varying individual characteristics; $W_{j(i,t)t}$ is a set of employer characteristics; and λ_t is the year fixed effects. The parameter of interest is β_1. We always control for λ_t while varying controls in X_{it} and $W_{j(i,t)t}$ across different specifications.

The first bar in Figure A.1 shows that the raw migrant-native wage gap is around 10 log points in favor of migrants. Because migrants are on average much younger than natives, the wage gap rises to around 16 log points once gender-specific age profiles are controlled for. Further controlling for industry and sector fixed effects reduces the

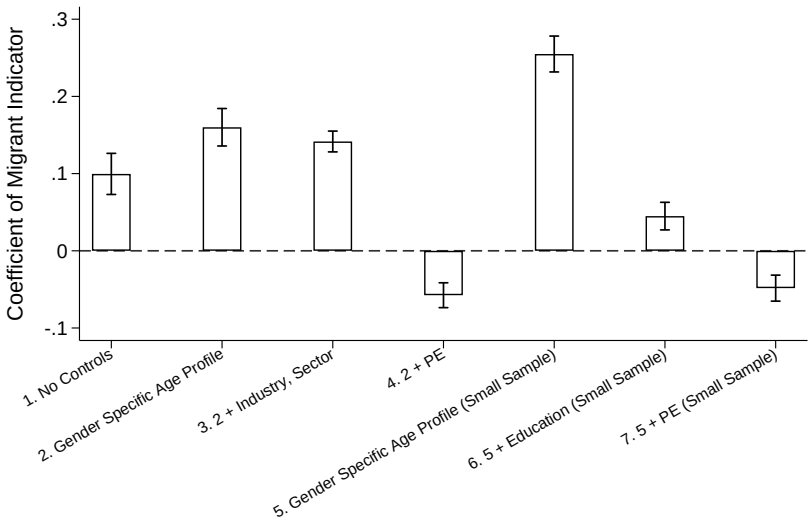


Figure A.1: Migrant-Native Wage Gap

Notes: Each bar shows β_1 from the estimation of Equation (A.1). The specification of each regression is summarized by the label on the horizontal axis. Capped vertical lines indicate the 95% confidence interval of the coefficient. The first four models use the baseline sample; the last three models use the subsample of mortgage applicants.

gap slightly to 14 log points, which suggests that although migrants are sorted into low-paying industries and sectors, this negative sorting is not a key driver of the migrant-native wage gap. The fourth bar shows that if we include the estimated person effect from the AKM model in the second specification, the migrant-native wage gap reverses to become -5.8 log points.

The main sample includes limited demographic information. The three bars on the right use the subsample of mortgage applicants (see Appendix E for a description of the sample), for whom we observe education levels. The migrant-native wage gap after accounting for gender-specific age profiles is about 25 log points (Bar 5). When we add education levels and education-specific age profiles as additional controls in this small sample, the migrant-native wage gap narrows substantially to about 4.5 log points but remains positive and statistically significant (Bar 6). Finally, we control for the estimated person effect instead of education-related variables. As shown in Bar 7, the migrant-native wage gap drops to a statistically significant -4.9 log points, similar to the corresponding specification in the baseline sample (Bar 4). The patterns in the last three models reveal two important points. First, a large share of the observed migrant-native wage gap can be accounted for by differences in observed skills (in this case, education levels and age distributions). Second, education alone is insufficient in accounting for skill

differences.

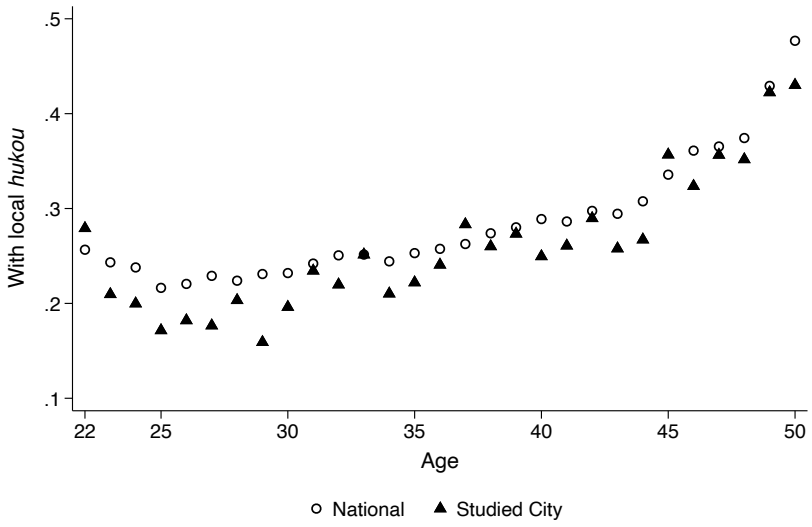


Figure A.2: Proportion of Migrants with Local *Hukou*

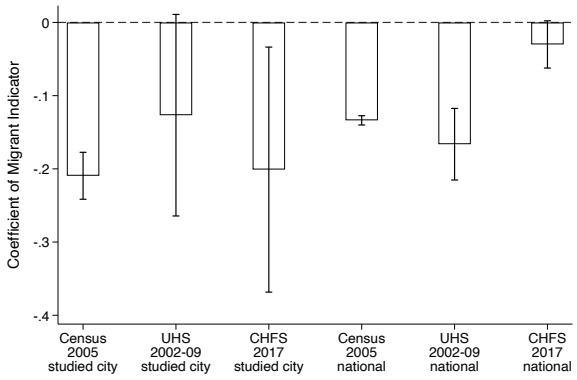
Note: The data is from the individual-level sample of the 2010 population census. The sample includes migrants in China's cities. Migrants are defined by whether their place of birth coincides with the city in which they currently reside. In addition, migrants whose city of *hukou* registration coincides with their city of residence are defined as holding a local *hukou*. Circles indicate the share of migrants with a local *hukou* in all cities. Triangles indicate the share of migrants with a local *hukou* in the studied city.

Comparisons with the Existing Literature. There is a large literature on the migrant-native wage gap in China (Chen and Zhang, 2018; Ma, 2018; Song, 2016; Zhang et al., 2016). These studies typically find a wage penalty for migrants after accounting for observed worker characteristics.

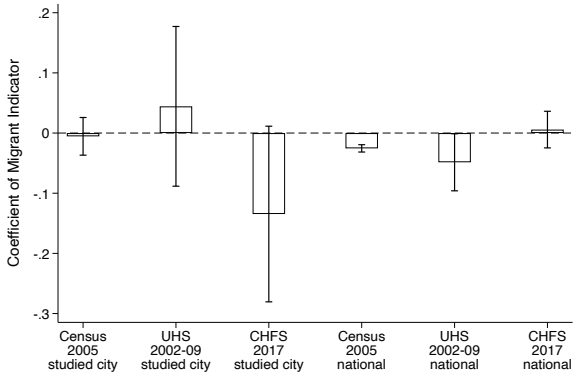
This paper departs from the previous literature in three important ways. First, most existing studies focus on rural-to-urban migration. Rural migrants typically have low skills and overwhelmingly work in the low-wage informal sector. Here, we investigate the migrant-native wage gap in the formal sector. Migrants in the formal sector are particularly positively selected, which potentially explains the higher migrant relative wage observed in the raw data.

Second, existing studies typically define migrants by their *hukou* status. In contrast, we define migrants by their location of origin. Some of the migrants in our sample may have acquired a local *hukou*. Indeed, the 2010 population census shows that about 25% of migrants in the studied city aged 22–50 have a local *hukou*. Figure A.2 further shows that the share of migrants with a local *hukou* increases with age, and this pattern is present for both the studied city (in triangles) and urban China as a whole (in circles). The pro-

Panel A: Traditional Definition of Migrants



Panel B: Traditional Definition of Migrants with Education Controls



Panel C: Definition of Migrants in This Study with Education Controls

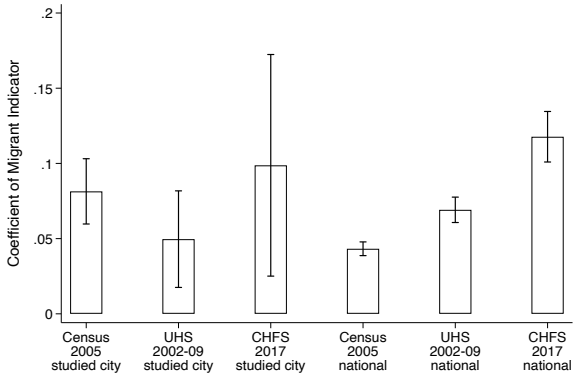


Figure A.3: Migrant-Native Wage Gap

Notes: The traditional definition of a migrant is whether the individual holds a non-local agricultural *hukou*. The definition of a migrant in this study is whether the individual is originally from outside the city, regardless of his/her current *hukou* status. Each bar reports β_1 from estimating a version of the following model: $\ln y_{it} = \beta_0 + \beta_1 Migrant_i + \beta X_{it} + \gamma W_{j(i,t)t} + \lambda_t + \varepsilon_{it}$ where X_{it} includes age, age squared, gender, and fixed effects of occupation, industry, and city. In Panels B and C, education is also included. The labels on the x-axis indicate the dataset, sample period, and geographic scope of the sample. We focus on employed workers aged 22–50. Capped vertical lines indicate the 95% confidence interval of the coefficient.

portion of migrants who obtain a local *hukou* is lower for young workers under 30 in the studied city than in other cities, consistent with the fact that the studied city has more restrictive *hukou* policies. Focusing on temporary migrants without a local *hukou* misses a substantial share of migrants who have successfully settled in the destination city.

Third, to shed light on the sources of the migrant-native wage gap, existing studies typically rely on observable worker characteristics such as education, gender, and age. We show that observable differences in worker characteristics are insufficient to account for the underlying skill differences. Using employer-employee-linked data and assuming a linearly additive wage determination process, we recover unobserved worker skills that play a crucial role in determining wages.

We reconcile our findings with the existing literature by illustrating the roles those differences play. We consider several nationally-representative datasets that are commonly used to investigate the migrant-native wage gap. The first dataset is the 1% population survey, which is conducted by China's National Bureau of Statistics (NBS) between two decennial census years, and hence known as the "mini census." These surveys are nationally representative and provide comprehensive information on individual characteristics, including age, gender, education, industry, location, employment status, and migration and registration status information. We use the 2005 survey, which reports the labor income of working individuals.

The second dataset is the Urban Household Survey (UHS), which is also conducted by the NBS. The UHS is an annual cross-sectional survey and is the most comprehensive dataset available on households in urban regions in the country. It covers 185 cities out of about 330 nationwide. We have access to the UHS between 2002 and 2009. The sampling method of the UHS is similar to that of the Current Population Surveys (CPS) in the U.S. The multi-stage probabilistic sample and stratified design ensure national representativeness. The data contains rich information on individual characteristics and income.

The third dataset is the Chinese Household Finance Survey (CHFS), which oversamples China's rich urban areas. We use the 2017 wave.

It is worth noting that all three datasets are conventional household surveys and contain little information about the employer. We form two samples from each dataset. One sample covers the studied city while the other sample covers all urban areas. We regress log wages on an indicator for migrants and investigate how the coefficient changes when worker characteristics are added as controls.

The regressions in Panel A of Figure A.3 use the traditional definition of migrants and control for basic worker characteristics such as gender-specific age profiles, industry, and occupation. The migrant-native wage gap is consistently negative across the different datasets for both the studied city and all urban areas. The wage gap ranges between -4 and -20 log points.

The regressions in Panel B include education levels as an additional control. The estimated migrant-native wage gap substantially narrows; some become positive, and many are not statistically significant. This suggests that under the traditional definition of migration, lower education levels among rural-to-urban temporary migrants are an important reason for the observed negative wage gap. However, the lack of a consistently negative wage penalty for the migrants after accounting for education does not mean that migrants are not negatively treated in China's urban labor market. On the contrary, it suggests the observed education levels are not perfect measures of skills. Observed education levels do not account for the *quality* of the education, which is likely lower for the migrants, who mostly receive their education in rural areas or smaller cities. Neither do they capture unobserved attributes, such as motivation, which may be positively selected among migrants.

In Panel C, we define migrants by their place of origin regardless of their current *hukou* status. As the conventional datasets do not indicate whether a worker is employed in the formal or informal sector, we include all workers. Across all datasets and samples, there is a consistent and statistically significant positive migrant-native wage gap. These findings reconcile with our results based on the HPF sample.

The differences across the three panels in Figure A.3 illustrate two important points. First, how we define migration matters. *Permanent* migrants make up an important share of the migrant population, and including them in the regression affects the results. Second, notice that the models in Panel C control for education, yet they still produce consistently *positive* migrant-native wage gaps. This suggests that migrants are positively selected in unobserved skills and that controlling for observed skills alone is insufficient. Recovering the unobserved skills from the AKM model is a crucial first step to understanding the migrant-native wage gap in China's urban labor market.

A.4 Attrition and Selection of Sample

Our sample covers just the formal sector of the studied city. Individuals endogenously choose whether to work in the formal sector, whether to stay in the labor market, and whether to stay in the studied city. These labor market transitions lead to sample attrition that may differ systematically between natives and migrants. In particular, low-skilled workers are more likely than high-skilled workers to leave the sample, and migrants are more likely than natives to leave the formal labor market. Consequently, migrants who remain in our sample may be more positively selected than their native counterparts, potentially biasing our results.

In this section, we discuss the attrition patterns of our sample. We show that these attrition patterns do not threaten our results and interpretations. We focus on two specific sub-samples: those observed in 2006 (the pre-sample) and those observed in 2011 (the post-sample).

There are substantial turnovers in the studied city's labor market. Consequently, the attrition rate in our sample is high. Figure A.4 Panel A shows the results. Among workers who were in the sample in 2006, 18.1% of the natives and 24.4% of the migrants had left the sample by 2010. Meanwhile, among workers who were in the sample in 2011, the three-year attrition rate is 21.7% among the natives and 37.2% among the migrants. Note that those who leave the sample because they exceed the age limit of 50 are not considered attrition.

The person effects estimated from the AKM model provide a natural measure of the degree of selectivity in attrition. Panel B plots each sample's evolution of the average person effect over time. Consistent with our prior belief, those who remain in the sample are increasingly positively selected. The average person effect is 10.34 among the natives observed in 2006 and 10.40 among the natives remaining in the sample in 2010 — an increase of 5.5 log points in four years. The selectivity in attrition is, surprisingly, weaker for migrants, whose average person effect increases by 3.1 log points during the same period.

For the sample observed in 2011, the three-year increase in the average person effect among those remaining in the sample is 3.7 log points for the natives and 6.0 log points for the migrants. That is, selectivity becomes more pronounced for migrants following the policy change, which is consistent with the quota tightening intended to curb migration in the studied city. The selection effect is strongest in the first two years of the sample;

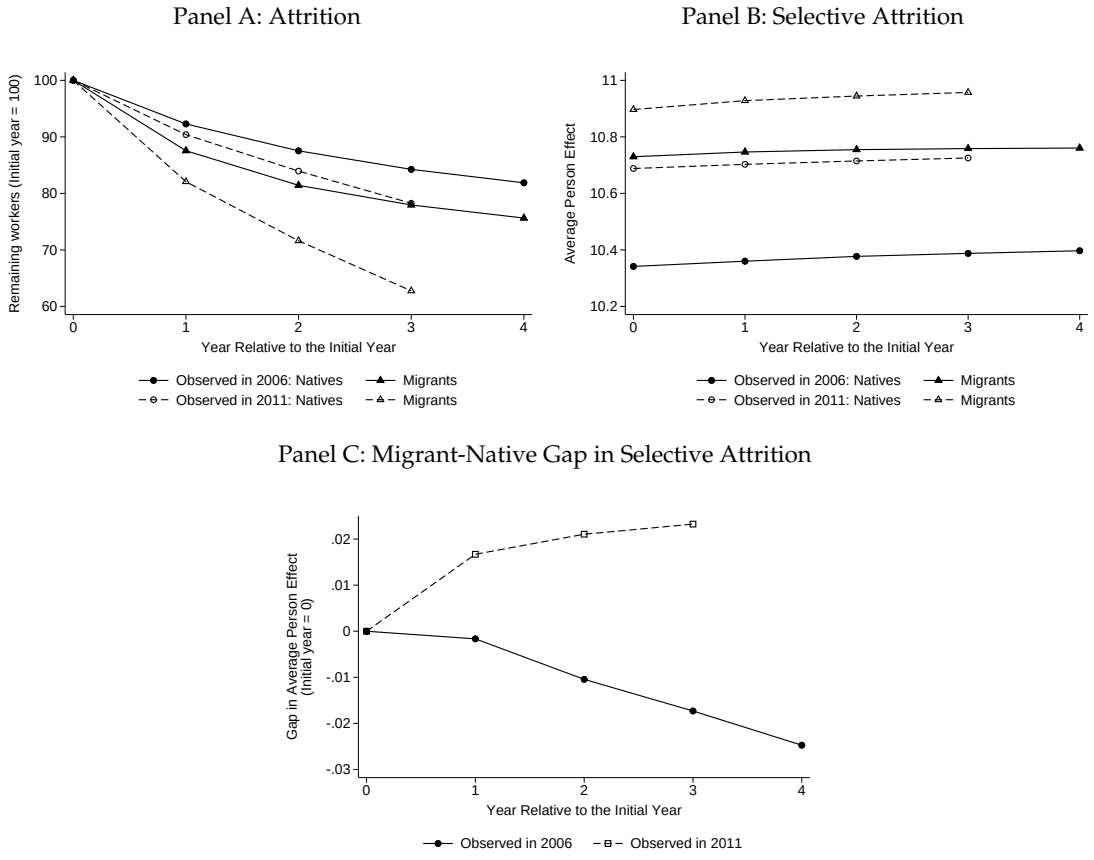


Figure A.4: Selective Attrition of the Sample

Notes: The graphs plot the rate and nature of attrition in the pre-sample and the post-sample. The pre-sample comprises workers observed in 2006; the post-sample comprises workers observed in 2011. Person effects are estimated from separate AKM models for the pre-policy-change period (2006–2010) and the post-policy-change period (2011–2014).

subsequently, the average person effects stabilize.

Panel C plots the changes in the migrant-native gap in the average person effects. The gap decreases by 2.5 log points in four years for the pre-cohort and by 2.3 log points in three years for the post-cohort. Although there is a discernible difference in selective attrition between migrants and natives, it is small relative to the initial migrant-native gap in the average person effects, which is around 30 log points for the pre-sample and 18 log points for the post-sample. The migrant-native difference in selective attrition does not affect our main findings. Appendix D.1 shows that the main results hold when the sample is restricted to workers who are observed in both periods.

A.5 Cohort Study Samples

Section 7 studies the speed and extent of wage assimilation by tracking two cohorts of young workers. The first cohort comprises individuals born between 1980 and 1984 who were in the sample in 2006 (aged 22–26 in 2006). We track these workers for four years until 2010, the year before the policy change. The second cohort comprises individuals born between 1985 and 1989 who were in our sample in 2011 (aged 22–26 in 2011). We track them for three years until 2014.

Following the method described in Appendix A.4, we investigate the sample attrition for these two young cohorts. Differential selectivity in attrition between migrants and natives could potentially confound wage assimilation. If low-skilled migrants are more likely than high-skilled migrants to drop out of the sample, we would mechanically observe wage assimilation.

Figure A.5 reports the patterns of attrition. Panel A shows that compared with the full sample, the attrition rate is higher among the young cohorts. Panel B indicates that the same positive selection pattern among stayers holds for the young cohorts. However, Panel C reveals that the difference in selective attrition between the two groups is small, especially when compared to the initial migrant-native gap in the average person effects or the magnitude of wage assimilation between the two groups (reported in Section 7). Based on these patterns, we do not discuss how selective attrition affects the dynamics of wage assimilation. Instead, we focus on the balanced panel of workers who are observed each year in the corresponding sample periods.

Similar to the whole sample, migrants in the cohort study samples are more skilled (higher average person effects) than natives. Migrants' higher innate skills mask the negative treatments they receive in the labor market. For example, in the initial year of the pre-cohort, the average wage of migrants is 9.8 log points higher than that of natives. To account for compositional differences, we reweight workers such that the skill-demographic compositions are the same across all groups. Specifically, we first divide workers into demographic cells by person effect deciles, gender, and birth years (rescaled to 0 for the earliest birth year in each cohort), and end up with 100 cells for each cohort. We set the distribution of the demographic cells of natives in the pre-cohort as the benchmark and reweight workers in the remaining three samples (pre-cohort migrants, post-cohort natives, and post-cohort migrants) such that the distribution of demographic cells is the same as the benchmark.

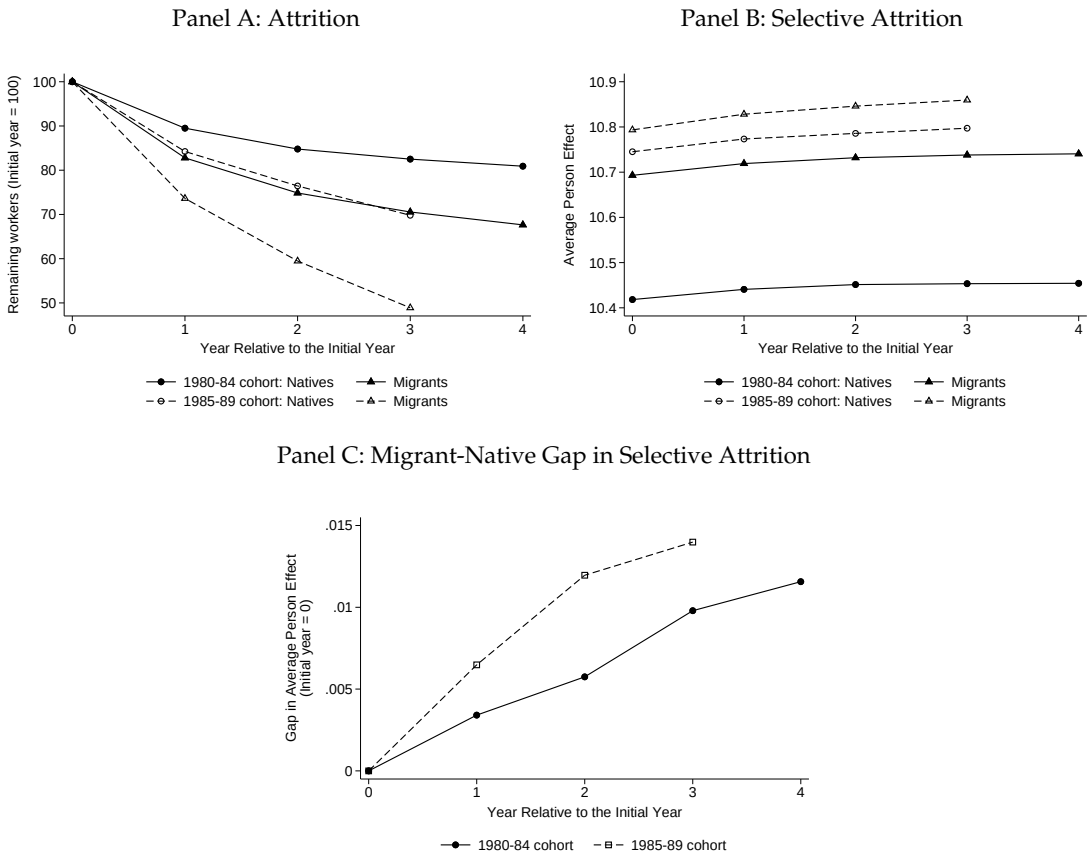


Figure A.5: Selective Attrition in Cohort Samples

Note: The graphs plot the rate and nature of attrition in the cohort study samples. Person effects are estimated separately for the pre-policy-change period for the 1980–1984 cohort and the post-policy-change period for the 1985–1989 cohort. The initial year is 2006 for the 1980–1984 cohort and 2011 for the 1985–1989 cohort. Workers are reweighted such that the compositions of workers in terms of person effects, gender, and age are the same across migration status and cohorts.

B Additional Details of the AKM Model

B.1 Tests of the AKM Assumptions

The consistent estimation of the AKM model in Equation (1) requires two key assumptions: separability and exogenous mobility. The separability assumption says that the log wage can be expressed as a linear sum of the person effect and the employer's wage premium; the unobserved matching quality between workers and their employers has a mean of zero and is uncorrelated with either the person effect or the employer effect. The exogenous mobility assumption says that the changes in the residual components of wages are not systematically correlated with the mobility patterns.

We follow Card et al. (2013) and conduct several tests for both assumptions. We first estimate a series of event studies around job switches. Specifically, we divide employers into four groups by the quartiles of the estimated employer premiums. There are 16 sets of event studies according to the quartile of the original employer and the quartile of the new employer. For each event study, we adjust the log wage by gender-specific age profiles and year effects, and take the average adjusted log wage of movers by year relative to the job switch.

In the main paper, we estimate four AKM models: migrants before the policy change, migrants after the policy change, natives before the policy change, and natives after the policy change. Figure B.1 shows four plots of event studies corresponding to each AKM model. Each plot reports eight event studies where the original employer's wage premium is in the first or fourth quartile.

Several features of these event study graphs support the AKM assumptions. First, wages are flat prior to the job switch, ruling out drifts in the person effects or employer effects. For example, if a worker or an employer experiences a negative productivity shock, we would expect wages to trend downward prior to the job switch.

Second, the relative wage premiums of the original and destination employers determine the magnitudes of the wage changes. For example, the average wage gain due to switching from an employer in the first quartile of the wage premium distribution to an employer in the fourth quartile is similar to the average wage loss due to job switches in the opposite direction. The symmetry in wage changes is consistent with the exogenous mobility assumption. Suppose job switches are driven by the idiosyncratic matching component between the worker and the employer, and workers only accept new jobs that

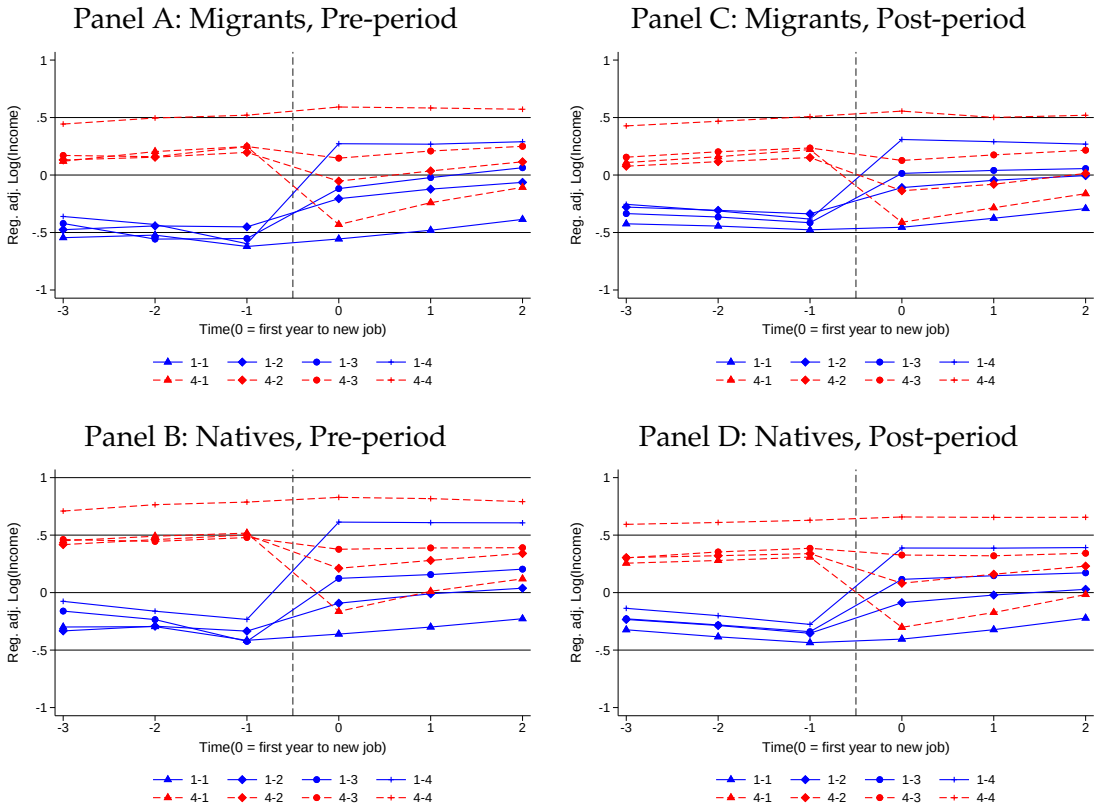


Figure B.1: Test for Parallel Trends in Job Switches

Notes: The red dashed lines show the changes in the average residualized wage of workers who leave quartile 4 employers. The blue solid lines show the changes in the average residualized wage of workers who leave quartile 1 employers. The legends show the direction of the job switches. Quartile 1 employers have the lowest estimated employer effects while quartile 4 employers have the highest.

are good matches. Then, job switches would systematically lead to wage increases, regardless of the relative wage premiums of the original and destination employers. However, Figure B.1 shows no evidence of that. Job switches between employers in the same wage premium quartiles do not lead to systemic wage changes. This also suggests that job switches are exogenous to unobserved components that also affect wages.[1]

If wage determination is truly separable and if idiosyncratic matching between workers and employers is not important, the AKM model should go a long way in explaining the wage variation. In particular, replacing the two-way fixed effects specification in the AKM model with a set of more saturated employer-by-worker fixed effects (which control for each job match) would add little to the model's explanatory power. Indeed, the adjusted R-squared of the AKM model is approximately 0.9. Accounting for the specific

[1]Note that the exogenous mobility assumption does not rule out the correlation between mobility and either person or employer effects. For example, it is possible that high-skilled workers are more likely to switch jobs, and are more likely to move from lower-premium employers to higher-premium ones.

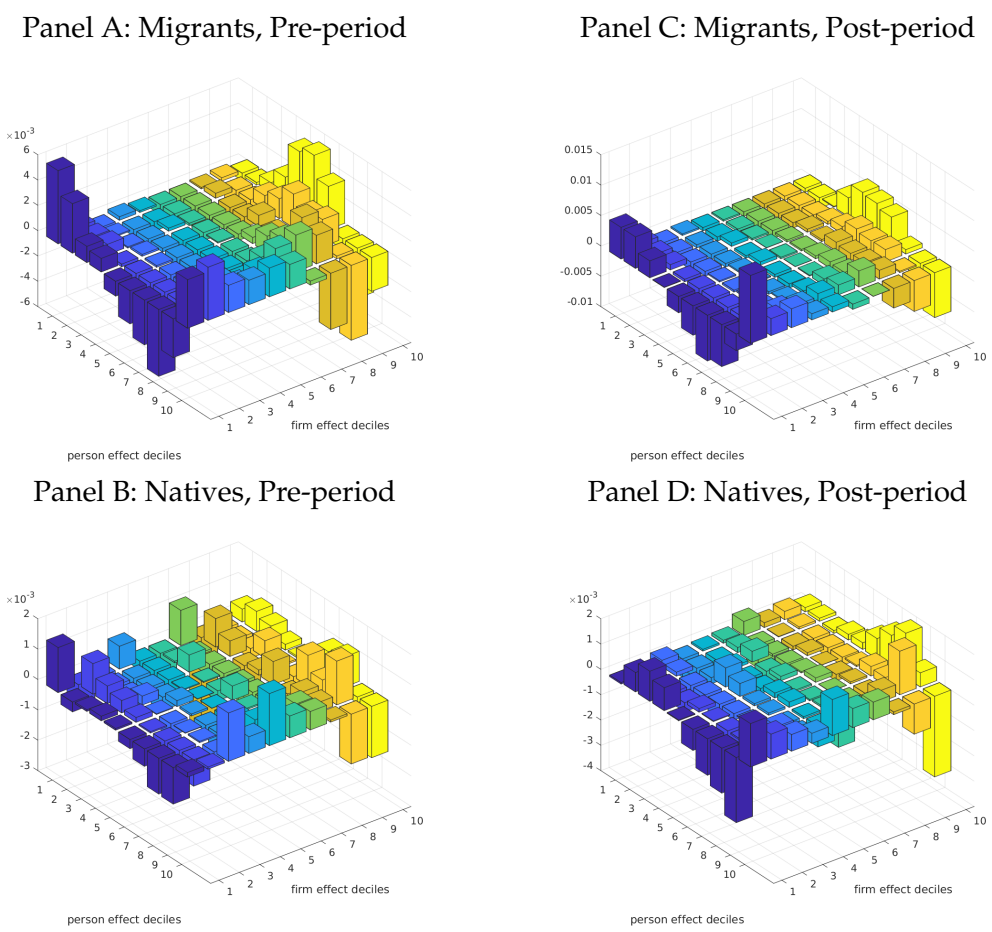


Figure B.2: Residuals from the AKM Regressions

Notes: The bars show the average estimated residuals in the AKM model in each person-by-employer decile cell. Decile 1 has the lowest estimated person and employer effect and decile 10 has the highest.

match effect merely increases the adjusted R-squared by 0.004 to 0.008 across the four models (see Table B.1 in Section B.3).

In Figure B.2, we take residuals from the AKM model and group them into 100 cells by deciles of person effects and deciles of employer effects. The figure shows that the average residual in each cell are overwhelmingly small, with magnitudes in the order of 10^{-3}. Importantly, the average residuals are expected to exhibit discernible patterns if there is systematic matching between workers and employers. However, Figure B.2 presents no such patterns.

B.2 Normalized Employer Premiums and Average Employer Wages

In the AKM model, the employer effect captures the wage premium offered by the employer. We estimate the AKM model separately for migrants and natives; the estimated

employer effects are relative values to the benchmark employers' effect in *each* model. To ensure the comparability of the person effects and the employer effects from the two models, we need to define a set of *common* benchmark employers. A natural choice of benchmark employers is those that offer zero wage premiums to their workers.

Because employer wage premium is fundamentally unobserved, the choice of benchmark employers is an essential assumption made by researchers. Some firm-level measures can corroborate the choice of the benchmark. For example, we can reasonably assume that firms with very low profit margins are unlikely to offer sizable wage premiums to their workers and are, therefore, natural candidates for benchmark employers. However, we do not have good information on employers' financial performance. We postulate that low-wage firms are likely also low-premium firms. In the baseline, firms with average wages between the 5th and 10th percentiles are selected as the benchmark. Appendix D shows that our results are robust to a long list of alternative benchmark definitions.

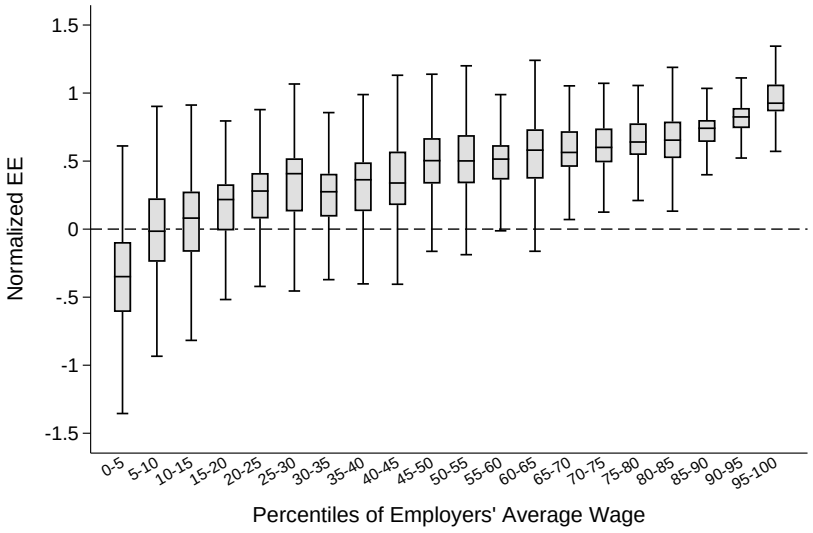


Figure B.3: Normalization of Employer Effects

Notes: Each box shows the average estimated (normalized) employer effect of employers by ventiles of average wage. The length of the bar indicates the interquartile range. The capped lines indicate the 95% confidence interval. In the baseline, employers with average wages between the 5th and 10th percentiles are selected as the benchmark. Therefore, employers with average wages in the second ventile has an average normalized employer effect of zero.

Here, we provide a simple sensibility test of the normalized employer wage premiums. Intuitively, if log wage is linearly additive between worker skills and employer premiums, and if there is generally positive sorting between worker skills and employer

premiums, we should expect a positive correlation between an employer's normalized premium and the average wage of its workers.

Using the pre-policy-change sample as an example, we divide employers into ventiles according to the average wage of their workers and plot the average normalized employer effects for each ventile. Figure B.3 shows that while there is some variation in the employer effects, the average normalized employer effect increases monotonically with the average wage ventile. It is worth noting that the first box on the left exhibits a particularly low average employer effect among those with the lowest average wage. In the baseline, we do not use these employers as benchmarks due to concerns of misreporting or measurement error, although Appendix D.6 shows that including them in the benchmark does not affect the results.

B.3 Decomposition of the Wage Variation

While this paper focuses on the average wage gap between migrants and natives, the results from the AKM model provide the necessary gradients to analyze the size and composition of the wage inequality. Wage variation can be decomposed into variations in person effects, employer effects, the time-varying components (e.g., age), their covariances, and the residual variance. This approach has been adopted by a growing body of studies in many countries and settings (Card et al., 2016; Card et al., 2013; Gerard et al., 2021; Kline et al., 2020; Song et al., 2019, among others).

China has experienced a large increase in income inequality since market reforms started in the 1980s. With rapid urbanization and economic growth, the wage inequality in the country's urban labor market is particularly interesting. The data in this paper is the first employer-employee-linked panel from China in the economic literature. Although the data covers only a selected sample in one city, it offers a unique perspective on wage inequality in China's urban labor market.

Following the literature, we estimate the shares of wage variation that are explained by observed worker characteristics, worker fixed effects, employer fixed effects, and their correlations.

Table B.1 reports the wage variation and its decomposition separately for natives and migrants, before and after the policy change. Log wages have a standard deviation between 0.71 and 0.78. The wage inequality in urban China is higher than those found among West German workers (0.5) (Card et al., 2013), among Portuguese men (0.55)

Table B.1: Decomposition of the Wage Variance (AKM)

	Native, pre-period (1)	Migrant, pre-period (2)	Native, post-period (3)	Migrant, post-period (4)
Largest connected set				
Standard deviation of log wages	0.735	0.774	0.709	0.781
Mean log wages	10.671	10.851	11.014	11.083
Variance decomposition				
SD of person effects	0.562	0.532	0.546	0.540
SD of employer effects	0.375	0.440	0.349	0.420
SD of covariates	0.218	0.286	0.247	0.264
Correlation of person/employer effects	0.053	0.013	0.055	0.045
Adjusted R^2 of model	0.908	0.892	0.938	0.927
Adjusted R^2 with match effects	0.912	0.900	0.942	0.933
Percentage of variance of log wages due to:				
Person effects	58.6	47.3	59.3	47.8
Employer effects	26.1	32.4	24.2	29.0
Covariance of person and employer effects	4.2	1.0	4.2	3.4
Emp. effects + covariance of person and emp. effects	30.2	33.3	28.4	32.3
Number of employers	31461	28630	29577	28369
Number of workers	1843272	1192447	1803724	1870394
Number of worker-year observations	6480198	3202168	5571545	4792155

Note: The results are from the estimation of Equation (1). Match effects models additionally include dummies for each worker-firm job match. Samples include only observations in the largest connected set.

(Card et al., 2016), among white men in Brazil's formal sector (0.67) (Gerard et al., 2021), but smaller than that among U.S. workers (0.96) (Song et al., 2019). Wage inequality among migrants is somewhat higher than that among natives.

Consistent with the findings in almost all other settings, we find that the person effect is the main driver of wage inequality. The variation in the person effects accounts for between 47 and 60 percent of the wage variation. There is positive sorting in the labor market. The correlation between the person effect and the employer effect is positive. Employers play an important role in driving wage variation. Employer effects and their correlations with person effects explain about one-third of the wage variance. The importance of employers in the wage variation is also consistent with the findings from other countries.

Kline et al. (2020) (henceforth KSS) shows that the limited mobility in the AKM models generates biased estimates of second-order moments. Importantly, they show that without accounting for the limited mobility, the correlation between worker and employer effects is underestimated. They propose a leave-one-out procedure that addresses this bias. This procedure reduces the sample size: approximately 26.9% (25.4%) of firms,

Table B.2: Decomposition of the Wage Variance (KSS)

	Native, pre-period (1)	Migrant, pre-period (2)	Native, post-period (3)	Migrant, post-period (4)
Leave-one-out connected set				
Standard deviation of log wages	0.693	0.721	0.660	0.704
Mean log wages	10.706	11.147	10.966	11.314
Variance decomposition (AKM)				
SD of person effects	0.523	0.439	0.492	0.461
SD of employer effects	0.345	0.465	0.333	0.425
Correlation of person/employer effects	0.122	0.141	0.150	0.156
Adjusted R^2 of model	0.908	0.896	0.922	0.917
Variance decomposition (KSS)				
SD of person effects	0.422	0.375	0.332	0.382
SD of employer effects	0.328	0.424	0.316	0.413
Correlation of person/employer effects	0.198	0.219	0.251	0.214
Adjusted R^2 of model	0.709	0.750	0.603	0.774
Percentage of variance of log wages due to: (AKM)				
Person effects	56.8	41.6	55.5	43.0
Employer effects	24.8	37.0	25.5	36.4
Covariance of person and employer effects	9.2	11.0	11.3	12.3
Emp. effects + covariance of person and emp. effects	34.0	48.0	36.7	48.8
Percentage of variance of log wages due to: (KSS)				
Person effects	37.0	27.0	25.3	29.4
Employer effects	22.4	34.5	22.9	34.4
Covariance of person and employer effects	11.4	13.4	12.1	13.6
Emp. effects + covariance of person and emp. effects	33.8	47.9	35.0	48.0
Number of employers	22344	19611	21431	20442
Number of movers	217462	168875	162813	205333
Number of worker-year observations	888448	615659	560747	670267

Note: The results are from the estimation of Equation (1) based on the correction method in Kline et al. (2020). Samples include only observations of job movers in the largest leave-one-out connected set.

61.4% (67.4%) of movers, and 50.9% (61.7%) of observations from the largest connected set are dropped in the pre-policy-change (post-policy-change) period. Table B.2 reports the corresponding results of the wage variation decomposition. Compared to the conventional AKM decomposition, the KSS procedure leads to lower estimates of the wage variation that is explained by fixed effects and larger estimates of the wage variation that is explained by the correlation between worker and employer effects. This is consistent with the theoretical prediction. Nonetheless, the combined explanatory power of employer effects and their correlation with person effects remains roughly the same.

B.4 Additional Results of the Wage Gap Decomposition

Alternative Oaxaca-Blinder Decomposition. In the baseline, we decompose the migrant-native wage gap according to Equations (2) and (3), where the employer's share of migrants is used as the weight in the wage-setting term and the employer's wage premium for natives is used as the weight in the sorting term. Alternatively, the decomposition can be conducted according to Equations (4) and (5), where the employer's share of natives and the employer's wage premium for migrants are used as weights instead. Table B.3 reports the results from the alternative decomposition. The results are quantitatively similar to those in Table 3. Importantly, changes in the wage-setting and residual sorting effects before and after the policy change are remarkably similar between the two decompositions.

Table B.3: Alternative Wage Gap Decomposition Before and After the Policy Change

	Pre (1)	Post (2)	Change (3)
Log wage (Migrant-Native)	0.134	0.047	-0.086
1. Person Effect	0.271	0.156	-0.115
2. Covariates	-0.097	-0.061	0.036
3. Employer Effect	-0.040	-0.048	-0.008
3.1. Wage-setting	-0.076	-0.032	0.044
3.2. Sorting	0.035	-0.016	-0.052
3.2.1. Skill-based sorting	0.060	0.036	-0.024
3.2.2. Residual sorting	-0.025	-0.052	-0.028

Note: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (4) and (5).

Wage Gap Decomposition with Worker Composition Reweighted. We test the sensitivity of the decomposition results to the differential selections of migrants and natives in the HPF sample. To do so, we reweight migrants and natives such that the distributions of worker characteristics between the two groups are the same. Specifically, we group workers into 120 cells by person effect deciles, gender, and six age groups. These demographic cells correspond to the z's defined in Equation (3). We reweight migrant workers such that the composition of migrants by demographic cell matches that of natives. We then decompose the reweighted migrant-native wage gap using Equation (2) (and alternatively, Equation (4)). Because the skill and demographic compositions of migrants and natives are the same after reweighting, the sorting effect in this decomposition is equivalent to the residual sorting, and no further decomposition of the sorting effect is conducted.

Table B.4 reports the results from the reweighted decompositions. We find that our main results are not driven by compositional differences between migrants and natives. Both the wage-setting effect and sorting effect are negative in the pre-period, with magnitudes similar to those reported in Table 2. Both the signs and the magnitudes of changes in wage setting and sorting also align with those in Table 3.

Table B.4: Migrant-Native Wage Gap Decomposition: Worker Composition Reweighted

	Decomposition			Alternative Decomposition		
	Pre	Post	Change	Pre	Post	Change
	(1)	(2)	(3)	(4)	(5)	(6)
Log wage (Migrant-Native)	-0.038	-0.057	-0.019	-0.038	-0.057	-0.019
1. Person effect	-0.004	-0.006	-0.002	-0.004	-0.006	-0.002
2. Covariates	0.038	0.023	-0.014	0.038	0.023	-0.014
3. Employer effect	-0.072	-0.075	-0.003	-0.072	-0.075	-0.003
3.1. Wage-setting	-0.027	-0.001	0.026	-0.076	-0.035	0.041
3.2. Sorting	-0.045	-0.073	-0.028	0.004	-0.040	-0.044

Note: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. Workers are reweighted such that the compositions of workers in terms of person effects, gender, and age are the same across migration status and periods. The decomposition follows Equation (2). The alternative decomposition follows Equation (4).

Employer Wage Premiums in the Public and Private Sectors. Employers in the public sector on average pay lower wage premiums than those in the private sector. For example, in the pre-policy-change period, the average wage premium for native workers is 0.42 among private-sector employers, compared with 0.37 among public-sector employers. We rank employers by the wage premium accorded to native workers. Figure B.4 plots the share of public employers by wage premium deciles. Employers in the public sector are concentrated in the middle of the wage premium distribution. There are relatively few public employers in the lowest decile, but they are also under-represented in employers that pay high premiums.

B.5 Bootstrapping in the Employer-Employee-Linked Data

While most existing studies do not report standard errors following the wage gap decomposition, we compute standard errors using bootstrapping. This section briefly describes the procedure.

The bootstrapping procedure follows Sorkin (2018). Note that the variation we use to identify the AKM model comes from job-to-job switches. To maintain the structure of the data, we use every two consecutive income records as the unit of randomization in con-

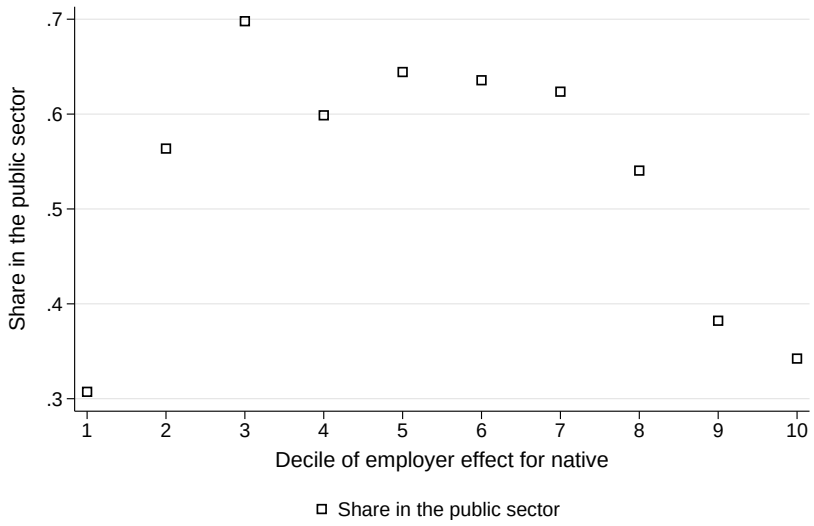


Figure B.4: Share in the Public Sector by Employer Wage Premium

Notes: Employers are categorized into deciles of employer effects for native workers. Each dot shows the proportion of public-sector employers in each decile.

structing our bootstrapped samples. Let $y_{i,t}$ indicate the wage of worker i in year t. Consider a worker with three consecutive observations $[y_{i,t-1}, y_{i,t}, y_{i,t+1}]$. We can construct two randomization units based on these observations: (i) $[y_{i,t-1}, y_{i,t}]$ and (ii) $[y_{i,t}, y_{i,t+1}]$. In constructing the bootstrapped samples, we randomly draw these randomization units with equal probability. Note that such a randomization process double-counts the interior income observations. For an interior observation that appears n times in a randomized sample, we include it $\left\lceil \frac{n}{2} \right\rceil$ times, where $\lceil \cdot \rceil$ is the ceiling operator.

As noted by Sorkin (2018), two issues arise in the bootstrap. First, the identified set of employers varies across different bootstrapped samples. Nonetheless, because we only focus on comparing the relative average person effects and employer wage premiums between migrants and natives, random changes in the composition of the identified set of employers should not significantly affect our results.

Second, the set of benchmark employers may change across bootstrapped samples. Moreover, due to random draws, employers may have different average wages from the original data. Therefore, we use the set of benchmark employers in the baseline sample to normalize the estimated fixed effects.

Due to the size of the dataset, the bootstrapping procedure is computationally demanding. We randomly draw 100 bootstrapped samples and use each sample to perform

the same empirical analysis applied to the baseline sample. Then, we use the standard deviations from the 100 estimations as the bootstrapped standard errors.

Table B.5 compares the average of the bootstrapped samples with the baseline sample. Both employer and employee characteristics are comparable across samples. Moreover, the bootstrapped standard deviations are small, suggesting that these characteristics are stable across different random draws. One notable difference is that employers in the bootstrapped samples tend to be larger, which is expected because some employers may not be included. This results in fewer employers and a larger average employer size in the bootstrapped samples. We report bootstrapped standard errors for the decomposition results in the main paper.

Table B.5: Summary Statistics Comparing Bootstrapped and Baseline Samples (2010)

Sample	Bootstrapped		Baseline	
	(1)	(2)	(3)	(4)
Panel A: Workers	Migrants	Natives	Migrants	Natives
Natives (%)	–	60.1	–	58.9
	–	[0.030]	–	–
Females (%)	43.5	47.1	44.0	46.9
	[0.045]	[0.049]	–	–
Average age	30.9	36.5	30.8	36.0
	[0.004]	[0.003]	(6.0)	(8.5)
Wage (CNY)	73,609	62,903	72,086	62,424
	[34.030]	[21.626]	(64,421)	(53,200)
Public sector (%)	36.6	66.9	36.4	66.7
	[0.001]	[0.000]	(48.1)	(47.1)
Panel B: Employers	Public	Private	Public	Private
Firms (%)	49.5	98.5	50.7	98.5
	[0.064]	[0.014]	–	–
Employer size	173.6	92.4	117.1	59.6
	[0.177]	[0.113]	(343.1)	(181.9)
Average wage of workers (CNY)	60,480	60,173	59,790	59,351
	[61.813]	[70.601]	(34,555)	(43,743)
Female workers (%)	51.5	45.7	51.4	46.1
	[0.031]	[0.051]	(21.8)	(23.5)
Native workers (%)	75.3	42.1	75.4	43.1
	[0.041]	[0.055]	(24.7)	(29.8)
Average age of workers	36.6	32.1	36.8	32.5
	[0.000]	[0.000]	(4.4)	(4.3)

Note: In Panel A, each cell shows the share or average for migrants and natives. In Panel B, each cell shows the share or average for public-sector employers and private-sector employers. Columns (1)–(2) show the summary statistics by averaging 100 bootstrapped resamples. Columns (3)–(4) show the summary statistics from the baseline sample, which are adopted from Table 1. Bootstrapped standard deviations are in brackets. Conventional standard deviations are in parentheses.

C Additional Tests of the Model Predictions

C.1 Cross-sectional Evidence

This section provides cross-sectional evidence supporting the model predictions on the impact of the *hukou* quotas on the wage gap by worker subgroups. Consider the following linear equation using the pooled pre-policy-change data between 2006 and 2010:

$$\ln y_{gzi} = \gamma_z \cdot \text{Mig}_i + \rho_{1z} \cdot \hat{\alpha}_{gzi} + X'_{gzi} \cdot \beta_{gz} + (\lambda_{j(i)}) + \varepsilon_{gzi} \tag{C.1}$$

where $\ln y_{gzi}$ is the log wage of person i in group $g \in \{M, N\}$ and subgroup z; Mig_i is an indicator for worker i being a migrant; $\hat{\alpha}_{gzi}$ is the estimated person effect from the AKM regressions in Equation (1), which enters the equation non-parametrically as deciles; X_{gzi} is a set of demographic characteristics, which enters flexibly as gender fully interacted with age; $\lambda_{j(i)}$ is the fixed effect of employer j that hires worker i. We estimate Equation (C.1) separately for each worker subgroup z. In all estimations, we cluster the standard error at the employer level.

The parameter of interest is γ_z, which captures the migrant-to-native wage gap conditional on workers' skills. We estimate two versions of this equation: one with $\lambda_{j(i)}$ and the other without. The skill-based sorting effect will be captured by the inclusion of $\hat{\alpha}_{gzi}$ and X_{gzi}. When $\lambda_{j(i)}$ is not included, γ_z (denote as γ^1) captures both the wage-setting and residual sorting effects. Including $\lambda_{j(i)}$ restricts the comparison to be within the same employer; hence, γ_z (denote as γ^2) captures the wage-setting effect.

The decomposition for the entire sample, reported in Table 2, suggests that both the wage-setting and residual sorting effects are negative for migrants. Therefore, we expect that in the corresponding regressions of Equation (C.1) using the entire sample, both γ^1 and γ^2 are negative, with γ^1 having a larger magnitude than γ^2. Figure C.1, row 1 shows that the estimates of γ^1 (in circles) and γ^2 (in triangles) are consistent with the prediction. Relative to natives of the same skills, migrants, on average, earn about 6.4 log points less, of which 4.8 log points can be attributed to the wage-setting effect. Although the specifications are not identical, the regression analyses can replicate the findings in the decomposition exercise, which, as reported in Table 2, finds a migrant-native wage gap of -8.7 log points and a wage-setting effect of -4.3 log points.

We then divide workers into young (less than 30 years old) versus old, high-skilled

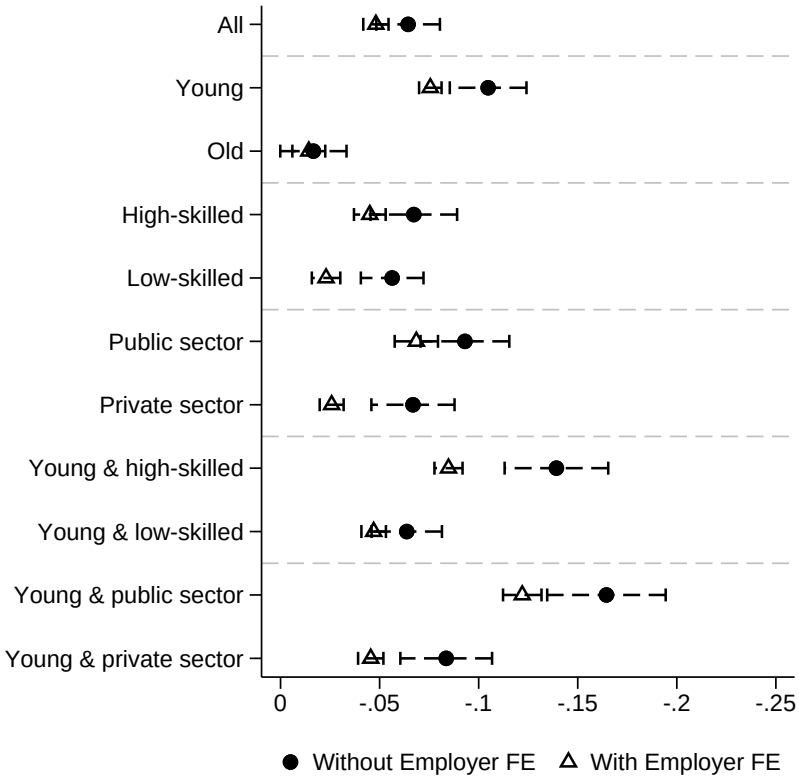


Figure C.1: Cross-sectional Evidence of the Wage Gap by Worker Subgroups

Notes: Each circle and triangle represents the coefficient associated with the migrant indicator from separate regressions. The circles are from estimating Appendix Equation (C.1) without controlling for employer fixed effects. The triangles are from estimating Appendix Equation (C.1) with employer fixed effects controlled for. The y-axis indicates the worker subgroups that are used for estimation. The capped dash lines indicate the 95% confidence intervals.

(with an estimated person effect above the median) versus low-skilled, and public-sector employees versus private-sector employees, and estimate Equation (C.1) for each subgroup. We hypothesize that: (i) both γ^1 and γ^2 are negative for all groups; (ii) γ^2, which captures the wage-setting effect, is negative and larger in magnitude for the young, the high-skilled, and those employed in the public sector; and (iii) γ^2 is smaller in magnitude than γ^1 for most subgroups, reflecting a negative residual sorting effect for migrants.

The results in the next three blocks of Figure C.1 are consistent with our hypotheses. Indeed, γ^1 is negative for all six worker subgroups, suggesting that migrant wages are lower across the board due to employer pay policies and sorting patterns. The wage gap is larger for the young, the skilled, and those employed in the public sector. Additionally, γ^2 is negative across the board, suggesting a negative wage-setting effect for all subgroups of migrants. The negative wage-setting effect is more pronounced for the young,

the skilled, and public-sector employees. Finally, γ^2 is uniformly smaller in magnitude than γ^1, suggesting negative residual sorting effects for all migrant subgroups.

Among the three dimensions of worker characteristics that we investigate, the migrant-native wage gap is particularly salient along the age dimension. The last two blocks of Figure C.1 report γ^1 and γ^2 by skill and sector among young workers. The migrant-native wage gap among high-skilled young workers is as large as -15 log points, with a wage-setting effect of about -9 log points. Among young workers in the public sector, the wage gap and the wage-setting effect are -17 and -12 log points, respectively. Low-skilled young workers and those in the private sector also suffer from negative wage-setting effects but to a much lesser degree.

In summary, Figure C.1 shows that most of the model predictions are supported by cross-sectional evidence. In the main paper, we estimate the impacts of the 2011 policy change to shed light on the potential *causal* effects of the quota system.

C.2 Additional Background of the 2011 Policy Change

The studied city is divided into more than a dozen districts. Each district sets up its own population control policies that collectively meet the municipality's overall target. We searched government documents for information on these policies and found population targets for ten districts, most of which are in the central city. Table C.1 shows that population targets come in different forms; they can be caps on the population, targets for the population growth rate, or caps on the number of migrants.

Table C.1: Population Control Targets

District	Target	District	Target
1	100k reduction in population	6	population <3.8 million
2	population <720k	7	annual population growth <2%
3	population <1.1 million	8	population <4 million
4	population <870k, migrants <240k	9	population <454k
5	population with *hukou* <450k, population <530k	10	population <1.45 million

Data Sources: the 12^{th} Five-Year Plan for the National Economic and Social Development of each district. Summarized by the authors.

Despite stricter population controls, migrants continued flowing to the studied city. Both the absolute number and the relative share of migrants kept rising in the post-policy-change period. Panel A Figure C.2 shows that the share of non-local residents (those without a local *hukou*), as reported in the city's Statistical Yearbooks, increased from 37% in 2010 to about 40% in 2014. Panel B reports that the share of migrants in the HPF sample

continues to increase in the post-period in both the public and private sectors.

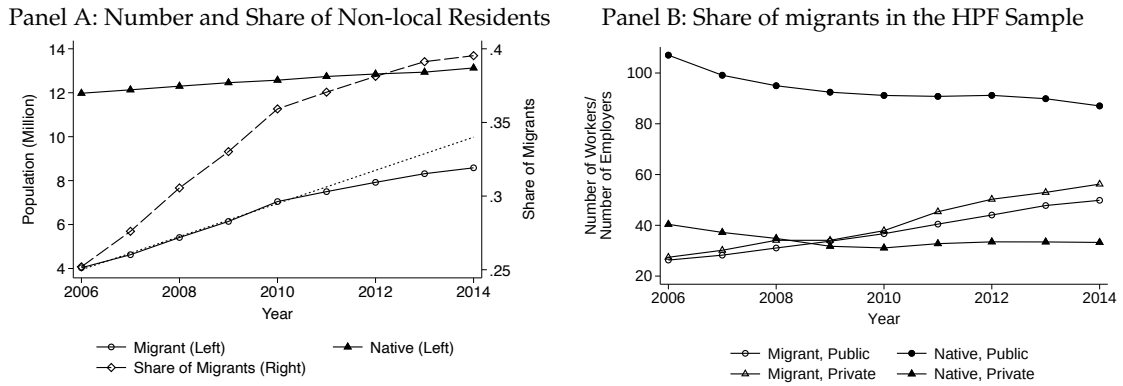


Figure C.2: Migrant Labor Supply before and after the 2011 Policy Change

Data Sources: Panel A – Municipal Statistical Yearbooks; migrants are defined as non-local residents. Panel B – the HPF panel data; migrants are defined by the first four digits of their citizen ID.

C.3 Additional Results on the Impacts of the 2011 Policy Change

Summary Statistics of the Post-Policy-Change Sample. In Section 2, we use the data in 2010, the year before the policy change, to provide an example of the summary statistics of the sample. To facilitate the decomposition of the post-policy-change period as shown in Table 3, we use the sample between 2011 and 2014. Here we provide the summary statistics of this post-policy-change sample to shed light on how the compositions of workers and employers change across periods.

Appendix Table C.2 reports the summary statistics of the post-policy-change sample, using the 2014 data as an example. Compared with those in the pre-period (reported in Table 1 Panel A), the share of migrants increases to 53% in the post-policy-change period, which is consistent with the increased migrant labor supply as shown in Appendix Figure C.2. The share of female workers remains roughly the same. Migrants are still younger than natives, on average, but to a smaller extent (age gap of 4.1 years in 2014 vs. 5.2 years in 2010). Migrant wages are 6.1% higher than that of natives. The private sector is expanding; fewer migrants and natives work in the public sector in 2014 compared to 2010. However, the majority of natives still work in the public sector (62%), compared to 33% of migrants. The HPF covers a larger share of the city's labor market in the post-period. Compared with the pre-policy-change period data, the post-policy-change period data covers more job-to-job transitions.

Panel B reports the employer-level summary statistics. There are 1.9 times as many

private-sector employers as public-sector ones, while the average public-sector employer is 1.7 times as large as the average private-sector one. Relative to the average private-sector employer, the average public-sector employer has a higher average worker wage (19% higher), a higher share of female workers (53% versus 48%), a much higher share of native workers (69% versus 37%), and older workers (average age of 37 versus 34). These patterns are similar to those in the pre-policy-change sample (Panel B of Table 1).

Table C.2: Summary Statistics (2014 Data)

Panel A: Workers	Migrants	Natives
Number of individuals	1,831,356	1,603,710
Females (%)	44.8	48.0
Average age (years)	31.6	35.7
	(6.4)	(8.1)
Wage (CNY)	88,597	83,498
	(80,144)	(69,327)
Public sector (%)	32.9	62.3
	(47.0)	(48.5)
Job switches in the past year (%)	12.7	9.4
	(33.3)	(29.1)
Panel B: Employers	**Public**	**Private**
Number of employers	10,980	20,669
Firms (%)	50.3	98.4
Employer size	123.9	73.0
	(355.7)	(249.6)
Average worker wage (CNY)	85,067	71,316
	(40,075)	(50,368)
Female workers (%)	53.1	47.8
	(21.8)	(24.1)
Native workers (%)	69.3	37.3
	(26.7)	(28.5)
Average age (years)	36.9	33.6
	(4.1)	(4.1)

Notes: In Panel A, each cell shows the average value or share for a specific worker group defined by migration status. In Panel B, each cell shows the average value or share for a specific employer group defined by sector. Standard deviations are shown in parentheses.

Event Studies of the Policy Change on Wages. Figure C.3 reports the event study estimates by worker subgroups, using the same specification as in Equation (12). The event studies by worker subgroups correspond to the triple-difference estimates reported in Table 6. Panel A divides workers by skill (above or below the median of the estimated person effects), Panel B by age (younger or older than 30), and Panel C by sector (private or public). For each dimension of worker characteristics, we estimate the event study model without controlling for employer fixed effects (graphs on the left) and controlling for those fixed effects (graphs on the right). The graphs on the left show the impact of the

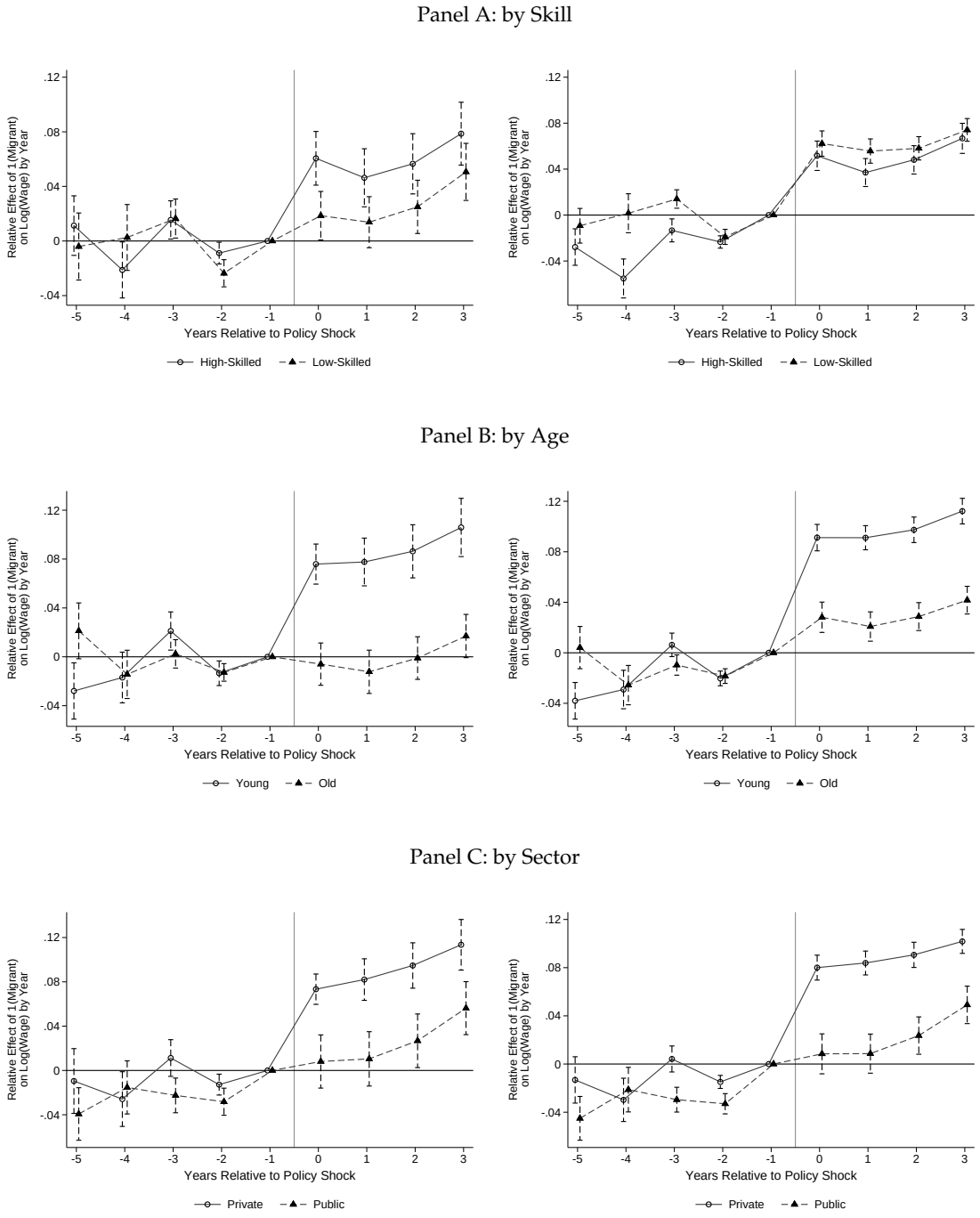


Figure C.3: Impacts of the Quota Reduction on Worker Subgroups: Event Studies

Notes: Each circle or triangle indicates the point estimate of the treatment effect in a given year (i.e., the coefficient of $Mig_i \times t_m$ in Equation (12)). Each vertical dashed line indicates the 95% confidence interval based on conventional standard errors clustered at the employer level. Circles linked by the solid line indicate the coefficient estimates of the model with the treatment group. Triangles linked by the dashed line indicate the coefficient estimates of the model with the control group. Panels on the left indicate the results without employer-by-period fixed effects. Panels on the right indicate the results with employer-by-period fixed effects.

quota tightening on the migrant-native wage gap conditional on workers' skills, while the graphs on the right depict the changes in the wage-setting effect.

Figure C.3 shows three salient patterns. First, there are parallel trends between migrants and natives for each subgroup. Second, for most subgroups, there is an immediate and substantial increase in the migrant relative wage following the policy change, with or without employer fixed effects. Third, the increase in the migrant relative wage is larger for the high-skilled, the young, and those in the private sector. These patterns are consistent with the findings in Table 6.

Secular Trends in Sectoral Choices. Migrants and natives experience different secular trends in employment sector. Those differential secular trends motivate us to include a group-specific linear trend to estimate the effect of the policy change on workers' sectoral choices. Here, we first show evidence of the differential secular trends. We estimate the following model using data between 2006 and 2010 (before the policy change):

$$Private_{gijy} = \sum_{y=2006, y\neq 2006}^{2010} [\gamma_y^M \cdot 1(\text{year} = y) \cdot 1(Mig_i = 1) + \gamma_y^N \cdot 1(\text{year} = y) \cdot 1(Mig_i = 0)] + Mig_i + X'_{giy}\beta_X + \hat{\alpha}_{gi}\beta_I + \varepsilon_{gijy}$$

where X_{giy} is the gender-by-age-by-year fixed effects and $\hat{\alpha}_{gi}$ is the person effect deciles. The reference year is set as 2006, with its coefficient restricted to be 0.

Figure C.4 plots $\hat{\gamma}_y^M$ and $\hat{\gamma}_y^N$. Both migrants and natives are increasingly likely to be employed in the private sector, and the increase is significantly faster for migrants. Between 2006 and 2010, the share of migrants in the private sector increased by 11 percentage points, compared to about 6 percentage points for natives. If we do not account for the differential secular trends, we are likely to underestimate the extent to which migrant workers are switching to the public sector following the policy change.

Event Studies of the Policy Change on Sectoral Choices. Table 7 reports the triple-difference estimates of the impacts of the quota tightening on sectoral choices. Figure C.5 reports the results from the corresponding event studies separately for high-skilled and low-skilled workers. After accounting for secular trends, migrants and natives are on parallel trends in terms of sectoral choices prior to the policy change. However, following the policy change, high-skilled migrants are increasingly less likely to be employed in the private sector. This pattern is stronger among young workers (Panel B). Furthermore, following the policy change, there is an abrupt decline in the share of high-skilled

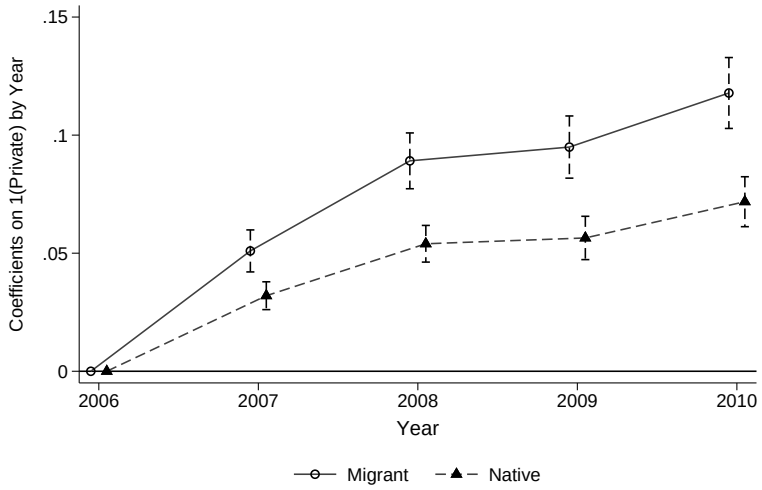


Figure C.4: Trends of Private-Sector Employment

Notes: The circles (triangles) are the coefficients of the regression of a private sector indicator on the indicator for migrant (native) interacted with year dummies. PE decile effects and gender-by-age fixed effects are controlled for.

young migrants in the private sector, while there is no significant change among their low-skilled counterparts.

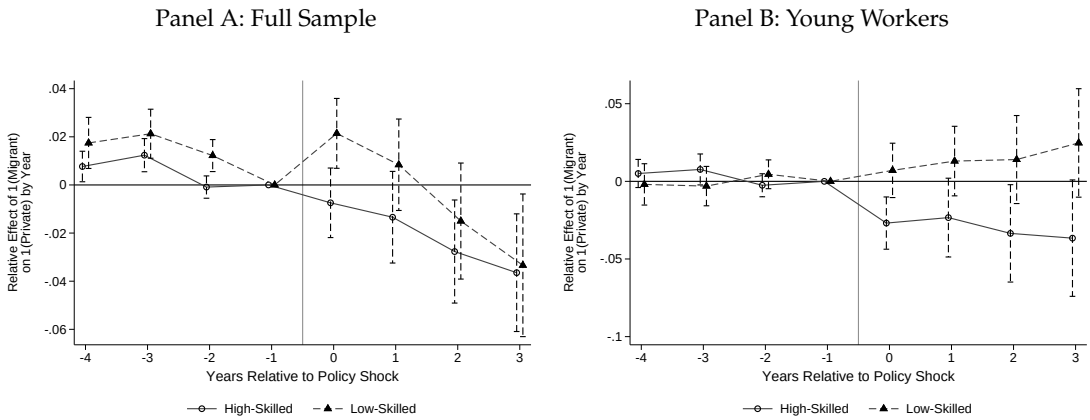


Figure C.5: Quota Tightening and Sectoral Choices: Event Study

Notes: The graph reports the results from the estimation of Equation (12), where the outcome variable is an indicator for working in the private sector. Each circle represents the point estimate of the treatment effect (i.e., the coefficient of $Mig_i \times t_m$). Each vertical dashed line represents the 95% confidence interval based on the conventional standard error clustered at the employer level. Circles linked by the solid line represent the coefficients of the estimation using a sample of high-skilled workers. Triangles linked by the dashed line represent the coefficients of the estimation using a sample of low-skilled workers.

D Robustness to Alternative Samples and Model Specifications

In this section, we conduct a host of robustness checks with alternative samples and model specifications. Here, we provide a brief overview of these robustness checks.

The first four sets of checks consider robustness to alternative samples. First, we consider restricting the sample to workers and employers who are observed at least once in both periods. This restriction is designed to address the concern that potential variations in sample compositions between the two periods may contribute to the changes in the migrant-native wage gap and its components. In our baseline analysis, we require employers to be observed in both periods, but not workers. Second, we consider dropping workers whose wages are censored by the HPF contribution cap. This tests whether results are sensitive to wage imputation and the inclusion of workers with very high earnings. Third, the AKM model is estimated by workers who switch employers. The sample in the baseline analysis includes the connected set, which contains workers who never switched employers and hence did not contribute to the identification. As a robustness check, we conduct analyses based on only job movers. Fourth, a large share of workers in the studied city works for not-for-profit employers. For such employers, wage determination may not adhere to conventional economic theories. We estimate a set of results by focusing on profit-maximizing firms and excluding government departments, educational institutions, and medical institutions.

The choice of benchmark employers plays a crucial role in the estimation of the migrant-native wage gap. The ideal candidates for benchmark employers are those that offer a zero-wage premium to both migrants and natives. In the baseline, we use private-sector firms with an average worker wage between the 5th and 10th percentiles as benchmark employers.

The next three sets of robustness checks explore different choices of benchmark employers. First, while the baseline choice of benchmark employers targets low-premium employers, it imposes no restrictions on whether they pay low premiums to both migrants and natives. Here, we further restrict benchmark employers to those whose average wage gap between their native and migrant employees is in the lowest quartile. Second, benchmark employers in the baseline exclude firms with the lowest average wages as we are concerned about misreporting or severe measurement error among a small number of employers that report particularly low wages. As a robustness check,

we include these firms and define benchmark employers as private-sector firms with an average worker wage in the bottom decile. Finally, we match firms in our sample to those covered by the Orbis data, and define benchmark firms as those with the lowest quartile of revenue.

We focus on four sets of results for each of the robustness checks: (i) the decomposition of the migrant-native wage gap before and after the policy change; (ii) the decomposition of the migrant-native wage gap for public-sector employers and for private-sector employers; (iii) the impacts of the quota tightening on the wage gap by skill, age, and sector of employment; (iv) the impacts of the quota tightening on sectoral choices. Our main results are robust to all the tests.

D.1 Workers and Employers Who Appear in the Sample in Both Periods

We restrict both employers and workers to those who are observed at least once in both the pre-policy-change and the post-policy-change periods; 2,235,190 (47.7%) workers and 152 (0.3%) employers are removed from the sample. This restriction mitigates the concern that secular changes in worker compositions could confound the impacts of the quota tightening.

Table D.1: Decomposition of the Migrant-Native Wage Gap – Restricted Sample

	Pre-period	Post-period	Change
Log wage (Migrant-Native)	0.143	0.182	0.039
1. Person Effect	0.266	0.272	0.006
2. Covariates	-0.077	-0.057	0.019
3. Employer Effect	-0.047	-0.033	0.014
3.1. Wage-setting	-0.048	-0.007	0.041
3.2. Sorting	0.001	-0.026	-0.027
3.2.1. Skill-based sorting	0.044	0.029	-0.015
3.2.2. Residual sorting	-0.043	-0.055	-0.013

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3).

Table D.2: Wage Gap Decomposition by Sector – Restricted Sample

	Public Sector			Private Sector		
	Pre	Post	Change	Pre	Post	Change
	(1)	(2)	(3)	(4)	(5)	(6)
Employer effect	-0.103	-0.090	0.013	-0.013	0.037	0.049
1. Wage-setting	-0.083	-0.065	0.018	-0.014	0.045	0.060
2. Sorting	-0.020	-0.025	-0.006	0.002	-0.009	-0.010
2.1. Skill-based sorting	0.014	0.002	-0.012	0.066	0.057	-0.009
2.2. Residual sorting	-0.034	-0.027	0.006	-0.065	-0.066	-0.001

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3), but are conducted separately for the public sector and the private sector.

Table D.3: DID and DDD Results of the *Hukou* Quota Tightening – Restricted Sample

VARIABLES	(1) Ln(Income)	(2) Ln(Income)	(3) Ln(Income)	(4) Ln(Income)	(5) Ln(Income)	(6) Ln(Income)	(7) Ln(Income)	(8) Ln(Income)
Migrant	-0.0939***	-0.0713***						
	(0.00864)	(0.00351)						
Migrant × Post	0.0264***	0.0456***						
	(0.00789)	(0.00421)						
Migrant × Post × High-Skilled			0.0569***	0.0548***				
			(0.00999)	(0.00489)				
Migrant × Post × Low-Skilled			-0.0106	0.0324***				
			(0.0104)	(0.00470)				
Migrant × Post × Young					0.0371***	0.0433***		
					(0.00902)	(0.00405)		
Migrant × Post × Old					-0.00196	0.0295***		
					(0.00876)	(0.00471)		
Migrant × Post × Private							0.0590***	0.0638***
							(0.00957)	(0.00399)
Migrant × Post × Public							0.0228**	0.0267***
							(0.0112)	(0.00722)
Observations (mil.)	13.3	13.3	13.3	13.3	13.3	13.3	13.3	13.3
R-squared	0.686	0.902	0.687	0.902	0.687	0.902	0.689	0.902
Gender-Age-Period FE	X	X	X	X	X	X	X	X
PE decile-Period FE	X	X	X	X	X	X	X	X
Employe-Period FE		X		X		X		X

Notes: All estimations are conducted using the largest connected set with employers and workers who appear in the data in both the pre-period and the post-period. The number of observations varies across columns because of missing values. Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table D.4: Sectoral Choices of Workers - Restricted Sample

	(1)	(2)
	Outcome variable: = 1 if in the Private Sector	
Migrant × Post × High-Skilled	-0.0172**	-0.00992
	(0.00735)	(0.00981)
Migrant × Post × Low-Skilled	0.0278***	0.0319***
	(0.00684)	(0.00863)
Fixed Effects		
Year	X	X
Gender-Age-Period	X	X
PE decile-Period	X	X
Linear Time Trend		
Migrant Specific	X	X
High-Skilled Migrant Specific	X	X
High-Skilled Specific	X	X
Sample	Full	Young
Observations (mil.)	13.3	4.7
R-squared	0.158	0.090

Notes: The table reports the estimation results of Equation (14) with the dependent variable being an indicator of working in the private sector. Person effect is estimated with Equation (1). Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

D.2 Uncensored Wages

In this set of robustness checks, we exclude all wage records that exceed the upper limit of the HPF contribution; 140,416 (3.0%) workers and 597 (1.1%) employers are removed from the sample. The baseline results hold, suggesting that the top-coding issue is not quantitatively important in driving our results.

Table D.5: Decomposition of the Migrant-Native Wage Gap – Uncensored Wages

	Pre-period	Post-period	Change
Log wage (Migrant-Native)	0.068	-0.012	-0.079
1. Person Effect	0.183	0.115	-0.068
2. Covariates	-0.043	-0.067	-0.024
3. Employer Effect	-0.072	-0.060	0.012
3.1. Wage-setting	-0.053	0.002	0.055
3.2. Sorting	-0.019	-0.062	-0.043
3.2.1. Skill-based sorting	0.023	0.005	-0.018
3.2.2. Residual sorting	-0.042	-0.067	-0.025

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3).

Table D.6: Wage Gap Decomposition by Sector – Uncensored Wages

	Public Sector			Private Sector		
	Pre	Post	Change	Pre	Post	Change
	(1)	(2)	(3)	(4)	(5)	(6)
Employer effect	-0.098	-0.081	0.017	-0.037	0.002	0.040
1. Wage-setting	-0.082	-0.055	0.027	-0.027	0.042	0.069
2. Sorting	-0.016	-0.026	-0.010	-0.011	-0.040	-0.030
2.1. Skill-based sorting	0.009	-0.002	-0.010	0.049	0.031	-0.018
2.2. Residual sorting	-0.025	-0.024	0.001	-0.060	-0.071	-0.011

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3), but are conducted separately for the public sector and the private sector.

Table D.7: DID and DDD Results of the *Hukou* Quota Tightening – Uncensored Wages

VARIABLES	(1) Ln(Income)	(2) Ln(Income)	(3) Ln(Income)	(4) Ln(Income)	(5) Ln(Income)	(6) Ln(Income)	(7) Ln(Income)	(8) Ln(Income)
Migrant	-0.0602***	-0.0322***						
	(0.00733)	(0.00361)						
Migrant × Post	0.0180**	0.0459***						
	(0.00761)	(0.00425)						
Migrant × Post × High-Skilled			0.0347***	0.0543***				
			(0.00900)	(0.00487)				
Migrant × Post × Low-Skilled			-0.000641	0.0335***				
			(0.0103)	(0.00456)				
Migrant × Post × Young					0.0455***	0.0671***		
					(0.00937)	(0.00402)		
Migrant × Post × Old					-0.0110	0.0239***		
					(0.00810)	(0.00507)		
Migrant × Post × Private							0.0436***	0.0609***
							(0.0111)	(0.00405)
Migrant × Post × Public							0.0150	0.0236***
							(0.0107)	(0.00728)
Observations (mil.)	15.9	15.9	15.9	15.9	15.9	15.9	15.9	15.9
R-squared	0.640	0.891	0.640	0.892	0.640	0.891	0.642	0.892
Gender-Age-Period FE	X	X	X	X	X	X	X	X
PE decile-Period FE	X	X	X	X	X	X	X	X
Employer-Period FE		X		X		X		X

Notes: All estimations are conducted using the largest connected set with employers who appear in the data in both the pre-period and the post-period. Normalization is based on an alternative definition of benchmark employers: those whose observed average wage ranks in the bottom 5 to 10 percentiles in the sample, and observed migrant-native wage gap ranks in the bottom quartile in the sample. The number of observations varies across columns because of missing values. Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table D.8: Sectoral Choices of Workers – Uncensored Wages

	(1)	(2)
	Outcome variable: = 1 if in the Private Sector	
Migrant × Post × High-Skilled	0.000210	-0.0265***
	(0.00730)	(0.00909)
Migrant × Post × Low-Skilled	0.0332***	0.0284***
	(0.00717)	(0.00843)
Fixed Effects		
Year	X	X
Gender-Age-Period	X	X
PE decile-Period	X	X
Linear Time Trend		
Migrant Specific	X	X
High-Skilled Migrant Specific	X	X
High-Skilled Specific	X	X
Sample	Full	Young
Observations (mil.)	15.9	6.9
R-squared	0.196	0.095

Notes: The table reports the estimation results of Equation (14) with the dependent variable being an indicator of working in the private sector. Person effect is estimated with Equation (1). Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

D.3 Job Movers Only

In this set of robustness checks, we focus on job movers in the largest connected set. Job movers are defined as those who have at least one job-to-job switch during the sample period; 3,363,144 (71.7%) workers and 9,810 (18.8%) employers are removed from the sample.

Table D.9: Decomposition of the Migrant-Native Wage Gap – Job Movers Only

	(1) Pre-period	(2) Post-period	(3) Change
Log wage (Migrant-Native)	0.194	0.205	0.011
1. Person Effect	0.319	0.272	-0.047
2. Covariates	-0.120	-0.069	0.051
3. Employer Effect	-0.005	0.002	0.007
3.1. Wage-setting	-0.019	0.016	0.035
3.2. Sorting	0.014	-0.014	-0.028
3.2.1. Skill-based sorting	0.063	0.049	-0.013
3.2.2. Residual sorting	-0.049	-0.063	-0.014

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3).

Table D.10: Wage Gap Decomposition by Sector – Job Movers Only

	Public Sector			Private Sector		
	Pre (1)	Post (2)	Change (3)	Pre (4)	Post (5)	Change (6)
Employer effect	-0.063	-0.070	-0.007	0.007	0.057	0.050
1. Wage setting	-0.063	-0.051	0.012	0.003	0.045	0.042
2. Sorting	0.000	-0.019	-0.019	0.004	0.013	0.008
2.1. Skill-based sorting	0.026	0.012	-0.014	0.079	0.076	-0.003
2.2. Residual sorting	-0.026	-0.031	-0.005	-0.075	-0.063	0.012

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3), but are conducted separately for the public sector and the private sector.

Table D.11: DID and DDD Results of the *Hukou* Quota Tightening – Job Movers Only

VARIABLES	Ln(Income)	Ln(Income)	Ln(Income)	Ln(Income)	Ln(Income)	Ln(Income)	Ln(Income)	Ln(Income)
Migrant	-0.115***	-0.0862***						
	(0.00842)	(0.00255)						
Migrant × Post	0.0614***	0.0743***						
	(0.00994)	(0.00299)						
Migrant × Post × High-Skilled			0.0990***	0.0884***				
			(0.0127)	(0.00323)				
Migrant × Post × Low-Skilled			0.0228**	0.0576***				
			(0.00985)	(0.00386)				
Migrant × Post × Young					0.0922***	0.0944***		
					(0.0109)	(0.00322)		
Migrant × Post × Old					0.00217	0.0318***		
					(0.0109)	(0.00337)		
Migrant × Post × Private							0.101***	0.0877***
							(0.00976)	(0.00319)
Migrant × Post × Public							0.0436***	0.0452***
							(0.0156)	(0.00583)
Observations (mil.)	5.5	5.5	5.5	5.5	5.5	5.5	5.5	5.5
R-squared	0.694	0.908	0.695	0.908	0.695	0.909	0.696	0.909
Gender-Age-Period FE	X	X	X	X	X	X	X	X
PE decile-Period FE	X	X	X	X	X	X	X	X
Employer-Period FE		X		X		X		X

Notes: All estimations are conducted using the largest connected set with employers who appear in the data in both the pre-period and the post-period. Only job movers are included. The number of observations varies across columns because of missing values. Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table D.12: Sectoral Choices of Workers – Job Movers Only

	(1)	(2)
	Outcome variable: = 1 if in the Private Sector	
Migrant × Post × High-Skilled	0.00172	-0.0272**
	(0.00991)	(0.0129)
Migrant × Post × Low-Skilled	0.0304***	0.0189**
	(0.00794)	(0.00924)
Fixed Effects		
Year	X	X
Gender-Age-Period	X	X
PE decile-Period	X	X
Linear Time Trend		
Migrant Specific	X	X
High-Skilled Migrant Specific	X	X
High-Skilled Specific	X	X
Sample	Full	Young
Observations (mil.)	5.5	2.6
R-squared	0.166	0.066

Notes: The table reports the estimation results of Equation (14) with the dependent variable being an indicator of working in the private sector. Person effect is estimated with Equation (1). Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

D.4 Profit-Maximizing Firms

In this set of robustness checks, we focus on profit-maximizing firms and exclude government departments, educational institutions, and medical institutions. It is worth noting that government departments, education institutions, and medical institutions account for only 6% of employers in the data, but 13% of workers and 20% of observations. This restriction disproportionately affects the composition of the public sector in the sample, reducing the share of the public sector from 56% to 43% at the worker-year level. Moreover, these public-sector employers' wage premiums for native workers are 12% lower than those of state-owned firms (0.35 vs. 0.39). The wage dispersion within the public sector widens as well, with the standard deviation of log-transformed annual income increasing from 0.7 to 0.8. The results related to the public (vs. private) sector should be interpreted with caution. Nonetheless, our qualitative conclusions hold. Moreover, the change in the wage-setting effect is larger here than in Table 3, which is consistent with the fact that non-firm public-sector employers are less affected by the quota tightening, and thus the increase in the wage-setting effect is concentrated among profit-maximizing firms.

Table D.13: Decomposition of the Migrant-Native Wage Gap – Profit-Maximizing Firms Only

	(1) Pre-period	(2) Post-period	(3) Change
Log wage (Migrant-Native)	0.194	0.099	-0.095
1. Person Effect	0.308	0.128	-0.180
2. Covariates	-0.099	-0.048	0.051
3. Employer Effect	-0.015	0.019	0.034
3.1. Wage-setting	-0.014	0.060	0.074
3.2. Sorting	-0.001	-0.040	-0.040
3.2.1. Skill-based sorting	0.056	0.029	-0.027
3.2.2. Residual sorting	-0.057	-0.070	-0.013

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3).

Table D.14: Wage Gap Decomposition by Sector – Profit-Maximizing Firms Only

	Public Sector			Private Sector		
	Pre	Post	Change	Pre	Post	Change
	(1)	(2)	(3)	(4)	(5)	(6)
Employer effect	-0.053	-0.007	0.046	-0.004	0.055	0.059
1. Wage-setting	-0.042	0.004	0.047	0.000	0.082	0.083
2. Sorting	-0.011	-0.011	-0.001	-0.004	-0.027	-0.023
2.1. Skill-based sorting	0.035	0.026	-0.009	0.065	0.039	-0.026
2.2. Residual sorting	-0.046	-0.038	0.009	-0.069	-0.067	0.002

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3), but are conducted separately for the public sector and the private sector.

Table D.15: DID and DDD Results of the *Hukou* Quota Tightening – Profit-Maximizing Firms Only

VARIABLES	(1) Ln(Income)	(2) Ln(Income)	(3) Ln(Income)	(4) Ln(Income)	(5) Ln(Income)	(6) Ln(Income)	(7) Ln(Income)	(8) Ln(Income)
Migrant	-0.0565***	-0.0218***						
	(0.00978)	(0.00361)						
Migrant × Post	0.100***	0.112***						
	(0.0103)	(0.00407)						
Migrant × Post × High-Skilled			0.108***	0.120***				
			(0.0142)	(0.00443)				
Migrant × Post × Low-Skilled			0.0923***	0.0998***				
			(0.0112)	(0.00484)				
Migrant × Post × Young					0.149***	0.148***		
					(0.0113)	(0.00416)		
Migrant × Post × Old					0.0421***	0.0712***		
					(0.0115)	(0.00443)		
Migrant × Post × Private							0.123***	0.124***
							(0.0106)	(0.00386)
Migrant × Post × Public							0.105***	0.0828***
							(0.0182)	(0.00912)
Observations (mil.)	13.4	13.4	13.4	13.4	13.4	13.4	13.4	13.4
R-squared	0.704	0.912	0.704	0.912	0.704	0.912	0.705	0.912
Gender-Age-Period FE	X	X	X	X	X	X	X	X
PE decile-Period FE	X	X	X	X	X	X	X	X
Employer-Period FE		X		X		X		X

Notes: All estimations are conducted using the largest connected set with firms that appear in the data in both the pre-period and the post-period. Non-firm public-sector employers comprising government departments, educational institutions, and medical institutions are excluded. The number of observations varies across columns because of missing values. Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table D.16: Sectoral Choices of Workers – Profit-Maximizing Firms

	(1)	(2)
	Outcome variable: = 1 if in the Private Sector	
Migrant × Post × High-Skilled	0.0114	-0.0259**
	(0.00851)	(0.0110)
Migrant × Post × Low-Skilled	0.0332***	0.00836
	(0.00795)	(0.00976)
Fixed Effects		
Year	X	X
Gender-Age-Period	X	X
PE decile-Period	X	X
Linear Time Trend		
Migrant Specific	X	X
High-Skilled Migrant Specific	X	X
High-Skilled Specific	X	X
Sample	Full	Young
Observations (mil.)	13.4	6.1
R-squared	0.148	0.058

Notes: The table reports the estimation results of Equation (14) with the dependent variable being an indicator of working in the private sector. Person Effect are estimated with Equation (1). Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

D.5 Alternative Normalization I

In this set of robustness checks, we define benchmark firms as those whose average wages are in the bottom 5 to 10 percentiles and the observed within-firm migrant-native wage gaps are in the bottom quartile.

Table D.17: Decomposition of the Migrant-Native Wage Gap – Alternative Normalization I

	Pre-period	Post-period	Change
Log wage (Migrant-Native)	0.134	0.047	-0.086
1. Person Effect	0.295	0.196	-0.099
2. Covariates	-0.097	-0.061	0.036
3. Employer Effect	-0.065	-0.088	-0.024
3.1. Wage-setting	-0.067	-0.040	0.027
3.2. Sorting	0.003	-0.048	-0.051
3.2.1. Skill-based sorting	0.052	0.022	-0.030
3.2.2. Residual sorting	-0.049	-0.070	-0.021

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3).

Table D.18: Wage Gap Decomposition by Sector – Alternative Normalization I

	Public Sector			Private Sector		
	Pre	Post	Change	Pre	Post	Change
	(1)	(2)	(3)	(4)	(5)	(6)
Employer effect	-0.115	-0.129	-0.014	-0.040	-0.027	0.013
1. Wage-setting	-0.101	-0.101	0.000	-0.039	0.002	0.041
2. Sorting	-0.014	-0.027	-0.014	0.000	-0.029	-0.029
2.1. Skill-based sorting	0.023	0.006	-0.017	0.075	0.045	-0.030
2.2. Residual sorting	-0.037	-0.033	0.004	-0.075	-0.074	0.001

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3), but are conducted separately for the public sector and the private sector.

Table D.19: DID and DDD Results of the *Hukou* Quota Tightening – Alternative Normalization I

VARIABLES	(1) Ln(Income)	(2) Ln(Income)	(3) Ln(Income)	(4) Ln(Income)	(5) Ln(Income)	(6) Ln(Income)	(7) Ln(Income)	(8) Ln(Income)
Migrant	-0.121***	-0.0929***						
	(0.00842)	(0.00344)						
Migrant × Post	0.0379***	0.0647***						
	(0.00825)	(0.00402)						
Migrant × Post × High-Skilled			0.0621***	0.0762***				
			(0.0110)	(0.00469)				
Migrant × Post × Low-Skilled			0.0105	0.0476***				
			(0.00943)	(0.00433)				
Migrant × Post × Young					0.0903***	0.103***		
					(0.00977)	(0.00388)		
Migrant × Post × Old					-0.0159*	0.0270***		
					(0.00886)	(0.00460)		
Migrant × Post × Private							0.0743***	0.0857***
							(0.0107)	(0.00379)
Migrant × Post × Public							0.0351***	0.0361***
							(0.0117)	(0.00713)
Observations (mil.)	17.6	17.6	17.6	17.6	17.6	17.6	17.6	17.6
R-squared	0.690	0.905	0.691	0.905	0.691	0.905	0.692	0.905
Gender-Age-Period FE	X	X	X	X	X	X	X	X
PE decile-Period FE	X	X	X	X	X	X	X	X
Employer-Period FE		X		X		X		X

Notes: All estimations are conducted using the largest connected set with employers who appear in the data in both the pre-period and the post-period. Normalization is based on an alternative definition of benchmark employers: those whose observed average wage ranks in the bottom 5 to 10 percentiles in the sample, and observed migrant-native wage gap ranks in the bottom quartile in the sample. The number of observations varies across columns because of missing values. Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table D.20: Sectoral Choices of Workers – Alternative Normalization I

	(1)	(2)
	Outcome variable: = 1 if in the Private Sector	
Migrant × Post × High-Skilled	0.00497	-0.0182*
	(0.00773)	(0.00980)
Migrant × Post × Low-Skilled	0.0338***	0.0166**
	(0.00668)	(0.00813)
Fixed Effects		
Year	X	X
Gender-Age-Period	X	X
PE decile-Period	X	X
Linear Time Trend		
Migrant Specific	X	X
High-Skilled Migrant Specific	X	X
High-Skilled Specific	X	X
Sample	Full	Young
Observations (mil.)	17.6	7.2
R-squared	0.179	0.094

Notes: The table reports the estimation results of Equation (14) with the dependent variable being an indicator of working in the private sector. Person effect is estimated with Equation (1). Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

D.6 Alternative Normalization II

In this set of robustness checks, we define benchmark employers as private-sector employers whose average wages are in the bottom decile.

Table D.21: Decomposition of the Migrant-Native Wage Gap – Alternative Normalization II

	Pre-period	Post-period	Change
Log wage (Migrant-Native)	0.134	0.047	-0.086
1. Person Effect	0.226	0.122	-0.104
2. Covariates	-0.097	-0.061	0.036
3. Employer Effect	0.004	-0.014	-0.018
3.1. Wage-setting	0.001	0.034	0.033
3.2. Sorting	0.003	-0.048	-0.051
3.2.1. Skill-based sorting	0.055	0.029	-0.027
3.2.2. Residual sorting	-0.052	-0.077	-0.024

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3).

Table D.22: Wage Gap Decomposition by Sector – Alternative Normalization II

	Public Sector			Private Sector		
	Pre	Post	Change	Pre	Post	Change
	(1)	(2)	(3)	(4)	(5)	(6)
Employer effect	-0.047	-0.055	-0.008	0.029	0.047	0.018
1. Wage-setting	-0.033	-0.027	0.005	0.029	0.076	0.047
2. Sorting	-0.014	-0.027	-0.014	0.000	-0.029	-0.029
2.1. Skill-based sorting	0.027	0.013	-0.014	0.081	0.058	-0.022
2.2. Residual sorting	-0.041	-0.040	0.001	-0.081	-0.087	-0.007

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3), but are conducted separately for the public sector and the private sector.

Table D.23: DID and DDD Results of the *Hukou* Quota Tightening – Alternative Normalization II

VARIABLES	(1) Ln(Income)	(2) Ln(Income)	(3) Ln(Income)	(4) Ln(Income)	(5) Ln(Income)	(6) Ln(Income)	(7) Ln(Income)	(8) Ln(Income)
Migrant	-0.0511***	-0.0310***						
	(0.00834)	(0.00340)						
Migrant × Post	0.0415***	0.0675***						
	(0.00818)	(0.00400)						
Migrant × Post × High-Skilled			0.0601***	0.0751***				
			(0.0106)	(0.00465)				
Migrant × Post × Low-Skilled			0.0221**	0.0561***				
			(0.00975)	(0.00429)				
Migrant × Post × Young					0.0949***	0.107***		
					(0.00971)	(0.00387)		
Migrant × Post × Old					-0.0124	0.0292***		
					(0.00881)	(0.00451)		
Migrant × Post × Private							0.0795***	0.0906***
							(0.0106)	(0.00377)
Migrant × Post × Public							0.0366***	0.0373***
							(0.0116)	(0.00698)
Observations (mil.)	17.6	17.6	17.6	17.6	17.6	17.6	17.6	17.6
R-squared	0.692	0.906	0.692	0.906	0.692	0.906	0.694	0.906
Gender-Age-Period FE	X	X	X	X	X	X	X	X
PE decile-Period FE	X	X	X	X	X	X	X	X
Employer-Period FE		X		X		X		X

Notes: All estimations are conducted using the largest connected set with employers who appear in the data in both the pre-period and the post-period. Normalization is based on an alternative definition of benchmark employers: those whose observed average wage ranks in the bottom 10 percentile in the sample. The number of observations varies across columns because of missing values. Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table D.24: Sectoral Choices of Workers – Alternative Normalization II

	(1)	(2)
	Outcome variable: = 1 if in the Private Sector	
Migrant × Post × High-Skilled	8.59e-05	-0.0253***
	(0.00718)	(0.00950)
Migrant × Post × Low-Skilled	0.0308***	0.0146*
	(0.00668)	(0.00827)
Fixed Effects		
Year	X	X
Gender-Age-Period	X	X
PE decile-Period	X	X
Linear Time Trend		
Migrant Specific	X	X
High-Skilled Migrant Specific	X	X
High-Skilled Specific	X	X
Sample	Full	Young
Observations (mil.)	17.6	7.2
R-squared	0.179	0.094

Notes: The table reports the estimation results of Equation (14) with the dependent variable being an indicator of working in the private sector. Person effect is estimated with Equation (1). Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

D.7 Alternative Normalization III

In this set of robustness checks, we define benchmark employers using information other than wages. Specifically, we link the HPF data with Bureau van Dijk's Orbis database, which is a global database covering information from more than 450 million companies in more than 200 countries. Using the Unified Social Credit Code, we are able to match 22,615 employers (20%) in our sample with the Orbis database, covering 60% of workers.

As Orbis' coverage has expanded in recent years, we focus on the 2014 wave, which has the richest information available. Even so, we do not observe any financial outcomes for many matched employers. Due to data limitations, we focus on revenue as the measure of firm performance, and we define benchmark employers as those with total revenue in the bottom quartile among employers for whom we observe revenue records.

Table D.25: Decomposition of the Migrant-Native Wage Gap – Alternative Normalization III

	Pre-period	Post-period	Change
Log wage (Migrant-Native)	0.134	0.047	-0.086
1. Person Effect	0.260	0.160	-0.099
2. Covariates	-0.097	-0.061	0.036
3. Employer Effect	-0.030	-0.052	-0.023
3.1. Wage-setting	-0.032	-0.004	0.028
3.2. Sorting	0.003	-0.048	-0.051
3.2.1. Skill-based sorting	0.036	0.013	-0.023
3.2.2. Residual sorting	-0.033	-0.061	-0.028

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3).

Table D.26: Wage Gap Decomposition by Sector – Alternative Normalization III

	Public Sector			Private Sector		
	Pre	Post	Change	Pre	Post	Change
	(1)	(2)	(3)	(4)	(5)	(6)
Employer effect	-0.080	-0.093	-0.013	-0.005	0.009	0.013
1. Wage-setting	-0.066	-0.065	0.000	-0.005	0.037	0.042
2. Sorting	-0.014	-0.027	-0.014	0.000	-0.029	-0.029
2.1. Skill-based sorting	0.013	0.001	-0.011	0.048	0.031	-0.017
2.2. Residual sorting	-0.027	-0.029	-0.002	-0.048	-0.060	-0.012

Notes: All components are the difference between migrants and natives. Calculations are based on the dual-connected sample between 2006 and 2010 for the pre-period and the dual-connected sample between 2011 and 2014 for the post-period. The decompositions follow Equations (2) and (3), but are conducted separately for the public sector and the private sector.

Table D.27: DID and DDD Results of the *Hukou* Quota Tightening – Alternative Normalization III

VARIABLES	(1) Ln(Income)	(2) Ln(Income)	(3) Ln(Income)	(4) Ln(Income)	(5) Ln(Income)	(6) Ln(Income)	(7) Ln(Income)	(8) Ln(Income)
Migrant	-0.0846***	-0.0604***						
	(0.00835)	(0.00340)						
Migrant × Post	0.0352***	0.0623***						
	(0.00821)	(0.00398)						
Migrant × Post × High-Skilled			0.0560***	0.0707***				
			(0.0107)	(0.00463)				
Migrant × Post × Low-Skilled			0.0122	0.0491***				
			(0.00973)	(0.00428)				
Migrant × Post × Young					0.0888***	0.102***		
					(0.00971)	(0.00385)		
Migrant × Post × Old					-0.0192**	0.0234***		
					(0.00886)	(0.00451)		
Migrant × Post × Private							0.0740***	0.0846***
							(0.0107)	(0.00378)
Migrant × Post × Public							0.0303***	0.0326***
							(0.0117)	(0.00698)
Observations (mil.)	17.6	17.6	17.6	17.6	17.6	17.6	17.6	17.6
R-squared	0.692	0.906	0.693	0.906	0.693	0.906	0.694	0.907
Gender-Age-Period FE	X	X	X	X	X	X	X	X
PE decile-Period FE	X	X	X	X	X	X	X	X
Employer-Period FE		X		X		X		X

Notes: All estimations are conducted using the largest connected set with employers who appear in the data in both the pre-period and the post-period. Normalization is based on an alternative definition of benchmark employers: those whose observed average wage ranks in the bottom decile in the sample. The number of observations varies across columns because of missing values. Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table D.28: Sectoral Choices of Workers – Alternative Normalization III

	(1)	(2)
	Outcome variable: = 1 if in the Private Sector	
Migrant × Post × High-Skilled	0.00179	-0.0192**
	(0.00736)	(0.00962)
Migrant × Post × Low-Skilled	0.0329***	0.0141*
	(0.00676)	(0.00828)
Fixed Effects		
Year	X	X
Gender-Age-Period	X	X
PE decile-Period	X	X
Linear Time Trend		
Migrant Specific	X	X
High-Skilled Migrant Specific	X	X
High-Skilled Specific	X	X
Sample	Full	Young
Observations (mil.)	17.6	7.2
R-squared	0.179	0.094

Notes: The table reports the estimation results of Equation (14) with the dependent variable being an indicator of working in the private sector. Person effect is estimated with Equation (1). Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

E Evidence from the Mortgage Sample

E.1 Description of the Mortgage Sample

We have access to additional information for a subsample of workers and employers through home mortgage applications via the Housing Provident Fund (HPF). To determine applicants' creditworthiness, HPF loan officers collect detailed information on applicants and their employers. Information on the applicant includes education and other qualifications, rank in the workplace, income, and the location of their current *hukou* while information on the employer includes sector (public or private), industry, and location. Only those who have contributed to the HPF are eligible to apply for a mortgage from the HPF. The application form also includes the anonymized identities of the applicant and the employer, which can be linked to the employer-employee-linked panel. Most mortgage applications have the main applicant's spouse listed as the co-applicant. The personal and employer characteristics of the co-applicant are also recorded. If the co-applicant contributed to the HPF during the sample period, he/she can also be matched to our main sample. An employer is matched as long as one of its workers applied for an HPF-backed mortgage during the sample period.

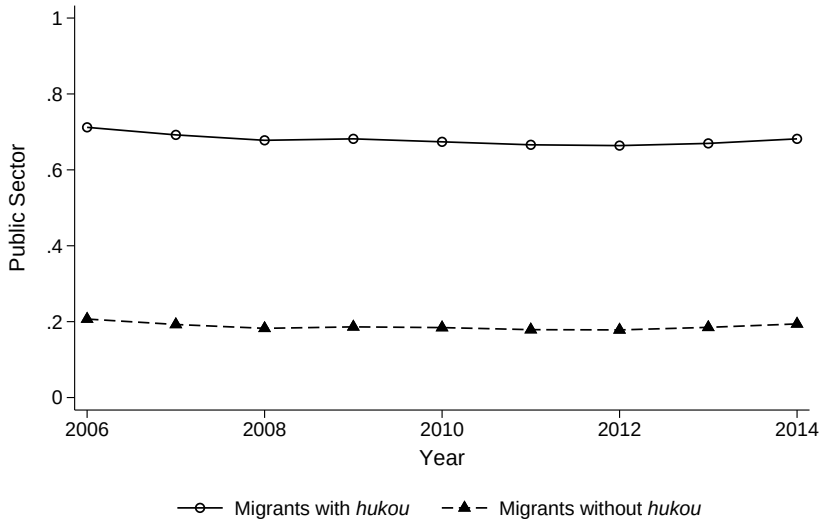
We are able to match 403,343 workers (about 7.6% of workers in our main sample) and 37,766 employers (about 33% of employers in our main sample). The matched workers are a selected sample. Those who apply for a mortgage in the studied city are more likely to be local, have higher earnings, and are more likely to be in a stable job (lower rates of job switches and attrition are observed). Nonetheless, information on the *hukou* status at the time of the mortgage application is a useful supplement to the main data. On the employer side, although matched employers account for only a third of all the employers in the sample, they tend to be larger employers, accounting for 88% of workers and 85% of worker-year observations. We use this small sample for a set of additional tests.

E.2 Evidence of the *Hukou* Quota Allocation

Figure E.1 shows that at the time of their mortgage application, 70% of migrants with a local *hukou* are in the public sector, and 80% of those without a local *hukou* are in the private sector. This sharp discrepancy shows that public-sector employees are much more likely to obtain a local *hukou* than their private-sector counterparts. However, we have no information on *when* a migrant acquires a local *hukou*. Although the trends appear

constant between 2006 and 2014, it does not necessarily mean that there was no change in the probability of obtaining a local *hukou* after the tightening of the quota in 2011.

Figure E.1: *Hukou* Status and Sector of Employment at the Time of Mortgage Application



Note: This graph shows the share of migrants who work in the public sector at the time of their mortgage application. The horizontal axis indicates the year of the mortgage application. Circles linked by the solid line indicate the share of migrants with a local *hukou* who are employed in the public sector. Triangles linked by the dashed line indicate the share of migrants without a local *hukou* who are employed in the public sector.

E.3 Effects of the 2011 Quota Tightening on Wages

For two groups of migrants, we can determine their *hukou* status in 2011, the year the quota was tightened: (i) those who applied for a mortgage after 2011 and reported not having a local *hukou* at that time; and (ii) those who applied for a mortgage before 2011 and reported having a local *hukou* at that time. We know that the first group did not have a local *hukou* in 2011 and the second group had a local *hukou* in 2011. Between these two groups, we expect the quota tightening to have a larger positive impact on the wages of the first group, those without a local *hukou* in 2011.

To test this prediction, we estimate the following equation:

$$\begin{aligned}\ln y_{gijt} =& \gamma_1 \cdot Mig_i \times 1(Hukou_{gijt} = 1) \times Post_t + \gamma_2 \cdot Mig_i \times 1(Hukou_{gijt} = 0) \times Post_t \\ &+ \alpha_1 \cdot Mig_i \times 1(Hukou_{gijt} = 1) + \alpha_2 \cdot Mig_i \times 1(Hukou_{gijt} = 0) \\ &+ \beta_1 \cdot 1(Hukou_{gijt} = 1) \times Post_t + \beta_2 \cdot 1(Hukou_{gijt} = 0) \times Post_t \\ &+ X'_{git}\beta_X + \hat{\alpha}_{gi}\beta_I(+\lambda_{jp}) + \tau_t + \varepsilon_{gijt}\end{aligned}$$

where $Hukou_{gijt}$ equals one when worker i with migration status g, working for employer j in year t, has a local *hukou* prior to the quota tightening, and zero when worker i does not have a local *hukou* after the quota tightening. We exclude migrants with ambiguous *hukou* status in the year of the quota tightening.

Table E.1 reports the estimation results, which support our prediction. The tightening of *hukou* quotas leads to a larger increase in the wages of migrants without a local *hukou* in 2011, compared with those with a local *hukou* in 2011, as shown in column (1). Column (2) further controls for employer-period fixed effects. The results suggest that the wage-setting effect is also stronger for those without a local *hukou* in 2011. These findings further evince the important role of the quota system on the migrant-native wage gap.

Table E.1: Effects of the Quota Tightening and *Hukou* Status in 2011

VARIABLES	(1) Ln(Income)	(2) Ln(Income)
Migrant × Post × No Hukou in 2011	0.0942***	0.0919***
	(0.0163)	(0.00709)
Migrant × Post × With Hukou in 2011	0.0239**	0.0478***
	(0.00937)	(0.00636)
Observations (mil.)	1.7	1.7
R-squared	0.691	0.903
Sample	Mortgage Applier	Mortgage Applier
Gender-Age-Period FE	X	X
PE decile-Period FE	X	X
Employer-Period FE		X

Notes: Two-way interaction terms are included. Only mortgage applicants are included in the sample. In addition, migrants with ambiguous *hukou* status in the year of the quota tightening are excluded from the sample. Conventional standard errors are clustered at the employer level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

F Additional Evidence of Wage Assimilation

Appendix Table F.1 provides detailed decompositions of the wage growth of two young cohorts. We report three-year wage catch-up by migrants and show how much of the wage assimilation is explained by migrants switching to high-premium employers at a faster pace than natives. In addition, we compare the assimilation patterns across different skill groups and workers with initial employment in different sectors. Workers are reweighted such that migrants and natives have the same skill, age (relative to the oldest in each cohort), and gender compositions in both cohorts.

Panel A focuses on the 1980–1984 birth cohort. In the first year, migrants make about 20 log points less than natives, as shown in row 1, column (2). After three years, the wage gap narrows by 10.7 log points (row 1, column (3)), with 14% of the catch-up, or 1.5 log points, explained by job ladders (row 1, column (5)). Migrants who are high skilled and those who start in the public sector suffer a larger initial wage penalty, but catch up more quickly than their counterparts, partly from climbing the job ladder. Panel B reports wage assimilation among the 1985–1989 birth cohort. The initial wage gap is smaller. After three years, migrants have almost fully closed the wage gap with natives.

Table F.1: Decomposition of the Three-Year Wage Assimilation

	Panel A: Birth Cohort 1980–1984						
Migrant-Native Gap in	PE	Log income		Native EE		Remaining	
		t_0	Δ	t_0	Δ	t_0	Δ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All	0	-0.198	0.107	-0.061	0.015	-0.137	0.092
High-skilled	0.004	-0.239	0.141	-0.027	0.02	-0.212	0.121
Low-skilled	-0.003	-0.171	0.084	-0.083	0.012	-0.088	0.072
Initially in public	0.001	-0.283	0.15	-0.088	0.013	-0.195	0.137
Initially in private	0	-0.129	0.044	-0.064	0.011	-0.065	0.033
	Panel B: Birth Cohort 1985–1989						
Migrant-Native Gap in	PE	Log income		Native EE		Remaining	
		t_0	Δ	t_0	Δ	t_0	Δ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All	-0.006	-0.054	0.049	-0.078	0.013	0.024	0.036
High-skilled	0	-0.065	0.066	-0.04	0.018	-0.025	0.048
Low-skilled	-0.01	-0.046	0.036	-0.103	0.01	0.057	0.026
Initially in public	-0.005	-0.134	0.093	-0.072	0.005	-0.062	0.088
Initially in private	-0.008	0.02	0.033	-0.048	0.011	0.068	0.022

Note: t_0 indicates the gap in the initial year. Δ indicates the three-year difference between t_3 and t_0. Workers are reweighted such that the compositions of workers in terms of person effects, gender, and age are the same across migration status and cohorts.

Reweighting makes it easier to plot and interpret the results. We show that the choice

of weights does not drive our main findings on wage assimilation. Table F.2 repeats the same analysis with unweighted samples. Column (1) shows a substantial initial gap in average person effects between migrants and natives. But the three-year catch-ups in wages and job ladders are similar to those found in Table F.1.

Table F.2: Decomposition of the Three-year Wage Assimilation: Unweighted

	Panel A: Birth Cohort 1980–1984						
Migrant-Native Gap in	PE	Log income		Native EE		Remaining	
		t_0	Δ	t_0	Δ	t_0	Δ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All	0.281	0.098	0.129	-0.023	0.016	0.121	0.113
High-skilled	0.113	-0.077	0.138	0.003	0.018	-0.08	0.12
Low-skilled	0.077	-0.071	0.075	-0.09	0.013	0.019	0.062
Initially in public	0.272	-0.016	0.16	-0.063	0.009	0.047	0.151
Initially in private	0.338	0.249	0.078	-0.004	0.021	0.253	0.057
	Panel B: Birth Cohort 1985–1989						
Migrant-Native Gap in	PE	Log income		Native EE		Remaining	
		t_0	Δ	t_0	Δ	t_0	Δ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All	0.051	0.031	0.052	-0.056	0.015	0.087	0.037
High-skilled	0.038	0.023	0.062	-0.014	0.019	0.037	0.043
Low-skilled	-0.008	-0.026	0.031	-0.105	0.011	0.079	0.02
Initially in public	0.084	-0.026	0.096	-0.045	0.005	0.019	0.091
Initially in private	0.114	0.153	0.042	-0.041	0.017	0.194	0.025

Note: t_0 indicates the gap in the initial year. Δ indicates the three-year difference between t_3 and t_0.

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