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Access to local citizenship and internal migration in a developing country: Evidence from a Hukou reform in China☆

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ABSTRACT

This study examines the impact of access to local citizenship (i.e., the status of being local residents with hukou in the context of China) on internal migration by exploiting variations in the timing and intensity of exposure to a hukou reform across 283 Chinese cities. Using population censuses and data we collected on the adoption of the hukou reform, we find that improved access to hukou substantially increases migration. The impact is more pronounced for young and low- and medium-skilled workers. Moreover, the impact persists over the long term. The policy positively affects local labor market probably through increased domestic consumption from migrants who (are prepared to) obtain local hukou in destinations. These findings demonstrate the importance of lifting barriers to local citizenship for internal migration in China. Underlying mechanisms and competing hypotheses are also analyzed.

1. Introduction

Several studies have analyzed how granting citizenship to immigrants affects acquisition of local language and social interactions with natives (Avitabile, Clots-Figueras, and Masella 2013), human capital investments and labor supply (Gathmann and Keller 2018), cooperativeness toward natives (Felfe et al. 2021), children's educational outcomes (Felfe, Rainer, and Saurer 2020), as well as fertility and children's health and social-emotional outcomes (Avitabile, Clots-Figueras, and Masella 2014). Overall, they find that granting

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citizenship to immigrants is a powerful policy instrument to boost economic integration in developed countries with traditionally restrictive citizenship policies. However, the economic effects of granting local citizenship to internal migrants are largely understudied.[1] Many internal migrants in developing countries, unlike their counterparts in developed countries, are still denied access to local citizenship (e.g., the status of being local residents with *hukou* in the context of China).[2] Further analyzing the consequences of granting local citizenship to internal migrants is thereby important, particularly for developing countries.

In this article, we investigate the economic effects of lifting barriers to local citizenship in the context of a large-scale internal migration in China that has changed the livelihoods of hundreds of millions of people over the past four decades. In particular, we examine the extent and how improved access to local citizenship in destinations affect migration decisions of people from rural areas, who dominate internal migration in China, and the resulting labor market consequences in destinations over time.[3]

To estimate the causal effects of lifting barriers to local citizenship, this study exploits quasi-experimental, city-level variations in the timing and intensity of exposure to a *hukou* reform in China. The *hukou* reform made it easier for migrants from rural areas to obtain local *hukou* in destinations. *Hukou*, which is similar to a badge of citizenship in Western society, is a form of registration that determines a person's entitlements, including the right to work, education, and social security (Solinger 1999; Chan and Zhang 1999).[4] Migrants living in destinations without local *hukou* are deprived of the many perquisites of being employed in the local public sector (e.g., state-owned enterprises), denied free compulsory education, and typically could not receive free health care and pension. The *hukou* reform substitutes a rigid annual quota system with a de jure quota-free system that entails some basic entry requirements for local *hukou* application.[5] Unlike the previous rigid annual quota system, the new system neither restricts the total number of people who can obtain local *hukou* in destinations de jure nor sets age and education (skill) limits for the application of local *hukou*.[6] The distinctive nature of this *hukou* reform allows examining the causal effects of improved access to *hukou* in destinations.

The Chinese *hukou* system is an internal passport system used to control internal migration, particularly rural-urban migration, and was formally established by Chinese authorities in 1958. Under the initial system, Chinese people received local *hukou* registrations in their places of birth and were not allowed to migrate away from their registered places without official permits.[7] Beginning in the early 1980s, the government slowly relaxed labor migration, allowing people to work temporarily in other places without registration. However, unregistered migrants continued to be denied access to local citizenship unless they successfully obtained local *hukou*, access to which remained strictly controlled under an annual quota system. Consequently, the *hukou* system delinked labor migration from access to local citizenship in destinations. Since the late 1990s, the government steadily adopted reforms to relax the *hukou* system, making it easier for migrants to obtain local *hukou* in destinations. This situation provides a unique case to study the consequences of improved access to *hukou* on internal migration.

Substantial literature has studied the determinants of migration decisions, finding that income prospects, financial constraints, social networks, risk sharing, and welfare benefits are all important factors in such decisions (Kennan and Walker 2011; Angelucci 2015; Munshi and Rosenzweig 2016; Morten 2019; Agersnap, Jensen, and Kleven 2020). However, the extent and how barriers to (local) citizenship affect migration decisions remain unclear. Although several studies have investigated international migration in the citizenship context, whether or not these studies reveal a causal relationship remains an open question (Fitzgerald, Leblang, and Teets 2014; Leblang 2017; Alarian and Goodman 2017; Agersnap, Jensen, and Kleven 2020). Moreover, research on internal migration in the context is rare, regardless of the fact that many internal migrants continue to be denied access to local citizenship in developing countries such as India, Vietnam, and China. Investigating the research question in the context of internal migration offers at least two advantages. First, it alleviates the identification concerns caused by unobservable factors in international migration. Second, it reduces the concern that region-specific characteristics (e.g., political systems, religion) in destinations dominate citizenship in determining migration decisions. Indeed, the causally interpretable evidence on the impact of lifting barriers to local citizenship on internal

[1] Although it is common to consider citizenship as the status of being a citizen at the national level (Bevelander and DeVoretz 2008), barriers to local citizenship in destinations within the same country have existed in developing countries such as India (Weiner 1978; Abbas 2016; Bhagat and Kumar 2021), Vietnam (Anh 2003; Vo 2020), and China (Solinger 1999; Zhang and Wang 2010; Song and Smith 2021). Citizenship is complex and multi-sided. For example, Isin (2009) defines citizenship as "the right to claim rights." In a textbook on citizenship, Fahrmeir (2007) portrays the French Revolution as "the invention of citizenship." By contrast, according to Prak (2018), local (urban) citizenship prevailed in Europe until the introduction of national citizenship after the French Revolution. This article defines citizenship as a set of (citizenship) entitlements.

[2] The entitlements attached to (local) citizenship in China are not exactly the same as those in developed countries. For example, in the absence of local citizenship, migrants in China were granted with fewer access to welfare benefits in destinations than their counterparts in developed countries. In this paper, we do not attempt to distinguish between entitlements attached to citizenship in China and other developed countries. The interpretation of our main findings may be affected by the extent to which citizenship entitlements in China differ from those in other developed countries.

[3] A closely related literature is the welfare magnet hypothesis, which has been investigated intensively but remains controversial (Borjas 1999; Kaushal 2005; Fiva 2009; Agersnap, Jensen, and Kleven 2020).

[4] Other details on the benefits associated with local *hukou* can be found in Song (2014).

[5] The de jure quota-free system is not one of 'free migration' because some basic entry requirements, such as stable accommodation and jobs in destinations, remain required to apply for local *hukou*. To better determine changes in quota restrictions induced by the *hukou* reform, this paper considers *hukou* and quota reforms as interchangeable. Additional details are presented in Section 4.

[6] Given that not everyone is eligible for entry requirements in the short term (e.g., certain amount of working time at the destination is required), the total number of people who can obtain *hukou* at the destination de facto remains restricted, but much less so than in the previous system. More importantly, the new system increases the chances and reduces the uncertainty of obtaining local *hukou*.

[7] Nevertheless, certain domestic migration caused by public employment and talent programs was allowed.

migration is scarce, given that the availability of such evidence is largely constrained by a lack of comparable data on relevant acquisition of local citizenship in different localities. Accordingly, we generally cannot rule out the possibility that internal migration, particularly in developing countries, is driven by other factors (e.g., income prospects, business cycle fluctuations, and trade liberalization) that have little to do with barriers to local citizenship in destinations.

The present study addresses this gap in the literature by using a quasi-experimental *hukou* reform across 283 Chinese cities. The empirical strategy exploits a difference-in-differences (DID) framework on the basis of comparisons between the cities exposed to the *hukou* reform at different times and with different requirements. The identifying assumptions are as follows. First, the decision about whether, to what extent, and when to adopt the *hukou* reform is uncorrelated with any prior trends in the outcomes of interest. Second, the timing and intensity of exposure to the *hukou* reform do not coincide with any city-specific shocks or policies that might influence the outcomes of interest.

In support of the first assumption, we test whether any treatment effects exist in the pretreatment periods to confirm the validity of the DID setup. In particular, we use an estimator proposed by de Chaisemartin and D'Haultfoeuille (2020) to cope with the concern that a two-way fixed effects model may not work well when heterogeneous treatment effects exist (e.g., by groups or over time). We also control for the effects of covariates using two approaches. As for the second assumption, we conduct permutation tests to confirm that the main results are not driven by the timing pattern of the *hukou* reform. We also control for several confounding policies that vary across cities to confirm that they do not contaminate the main results. We focus on three important city-level policies that may directly affect internal migration in China: (a) reductions in trade policy uncertainty (on exporting goods to the United States) after China's accession to the World Trade Organization (WTO) in 2001; (b) increases in minimum wages in urban areas; and (c) the abolition of agricultural taxes in rural areas. Finally, we conduct two additional placebo analyses by focusing on individuals less affected by the *hukou* reform and by analyzing alternative census data reflecting a period prior to the adoption of the *hukou* reform.

This study reports four main findings. First, improved access to *hukou* in destinations substantially increases migration from rural areas, suggesting that barriers to local citizenship in destinations are indeed an important obstacle deterring internal migration in a developing country such as China. Second, the impact is more pronounced for young and low- and medium-skilled people, suggesting that these groups are more sensitive to barriers to local citizenship in destinations. Third, increased return to migration through higher expected benefits attached to obtaining local citizenship in destinations is a likely underlying mechanism though which improved access to *hukou* facilitates migration. All of these findings demonstrate the importance of lifting barriers to local citizenship for internal migration in China. Lastly, the same policy positively affects the local labor market possibly through increased domestic consumption from migrants who (are prepared to) obtain local *hukou* in destinations. All of these empirical findings are in line with the implications derived in Section 2.

This article makes two additional contributions to the literature. First, it joins an emerging literature on the economic effects of lifting barriers to (local) citizenship, either within- or between-countries. In particular, our finding demonstrates the importance of lifting barriers to local citizenship for boosting economic integration in China, which sheds light on the consequences of barriers to local citizenship in other developing countries such as India (Weiner 1978; Abbas 2016; Bhagat and Kumar 2021) and Vietnam (Anh 2003; Vo 2020). Our finding also complements previous studies that gauge the effectiveness of lifting barriers to citizenship for economic integration in developed countries (Avitabile, Clots-Figueras, and Masella 2013; 2014; Gathmann and Keller 2018; Felfe, Rainer, and Saurer 2020; Felfe et al. 2021).

Second, it sheds light on the labor market consequences of relaxing China's *hukou* system. Notably, previous findings are mostly based on theoretical and simulation-based analysis (Whalley and Zhang 2007; Bosker et al. 2012; Tombe and Zhu 2019; Sieg, Yoon, and Zhang 2020) rather than real-world policies. Among a few empirical studies using real-world policies to estimate the impact of relaxing the *hukou* system on migration decisions, results vary significantly owing to differences in measures of relaxing the *hukou* system and corresponding identification strategies (Sun, Bai, and Xie 2011; Fan 2019). Unlike previous measures that either rely on provincial-level variations in the timing of relaxing the *hukou* system (Sun, Bai, and Xie 2011; Song and Li 2014; Kinnan, Wang, and Wang 2018) or city-level variations in the on-paper entry requirements of obtaining local *hukou* (Fan 2019; Zhang, Wang, and Lu 2019), we manually construct a new data set determining variations in the timing of relaxing the *hukou* system across 283 Chinese cities from 2002 to 2015. A combination of the newly constructed measure and traditional measure of on-paper entry requirements enables us to investigate the impact of an important and understudied aspect of the *hukou* system (i.e., quota restrictions) on internal migration. In addition to estimating the causal impact of relaxing the *hukou* system on migration decisions, the newly constructed measure is useful for investigating other consequences of an active labor market policy in a developing country, including local labor market outcomes, productivity, and technology adoption (Clemens, Lewis, and Postel 2018).

2. Internal Migration Reform and Its Economic Effects

Internal migration in China has been regulated by the Household Registration System (also known as the *hukou* system) since 1958. In the pre-reform era, each individual was born with an agricultural or a non-agricultural *hukou*, registered in a specific place. Non-agricultural *hukou* (NAH) holders were usually born in urban areas, where they had access to local public employment, including state-owned firms as well as welfare benefits provided by local authorities.[8] By contrast, agricultural *hukou* (AH) holders were typically born in rural areas, where they worked in the agricultural sector and were largely self-sufficient. The *hukou* system was established to

[8] Prior to the economic reform and opening-up policies adopted in 1979, the Chinese economy was largely planned. Thus, most urban employment was provided by the government, and employment in the private sector was negligible.

prevent residents, particularly rural residents, from migrating from their registered locality to unregistered places. Although certain domestic migration had already been allowed (e.g., public employment and talent programs), this practice does not apply to most rural-urban migrations in the pre-reform era (Cheng and Selden 1994; Chan and Zhang 1999).[9]

Beginning in the early 1980s, labor migration was gradually relaxed at the national level, and people were allowed to work temporarily in other places without registration. This relaxation was an effort to cope with increasing demand for labor in urban areas after China's reform and opening-up policies and to cope with a growing labor surplus in rural areas after China introduced the Household Responsibility System in agricultural production (Lin 1988; 1992). However, migrants had restrictive access to the local labor market (Meng and Zhang 2001; Song 2014), and were denied access to local welfare benefits unless they obtain local *hukou* (Cai 2011; Song 2014). Applications for local *hukou* continued to be strictly regulated under an annual quota system, and entry requirements of obtaining local *hukou* were often discriminating against less educated (unskilled) and older adults.

In the late 1990s, Chinese authorities adopted a national *hukou* reform to make it easier for migrants to obtain local *hukou* in small cities and towns. However, this reform had a limited impact, as the majority of migrants worked in medium and larger cities. Recently, an additional *hukou* reform was introduced at the national level. In July 2014, Chinese authorities announced a decree to make it easier for migrants to obtain local *hukou* in medium and large cities, with the exception of several mega-cities such as Shanghai and Beijing. During the period between these two national reforms, a number of (medium and large) cities reformed the *hukou* system locally at different points in time (Jin 2018). These local *hukou* reforms are comparable and generally substitute a rigid annual quota system with a de jure quota-free system that entails some basic entry requirements for local *hukou* applications. We discuss these requirements below to elucidate that these local reforms are sufficiently comparable.

Although improved access to local citizenship (*hukou*) is expected to promote internal migration, the magnitude of its effect is subtle and complicated. Here, we propose conceptual discussions of *hukou* migration as a guide to our empirical analysis. Specifically, we sketch the most important tradeoffs for rural residents in deciding whether or not to become migrants, that is, whether or not to move from their rural registered places to an unregistered place. In doing so, we broadly follow the literature on the rational choice between alternatives (Borjas 1989; Akerlof and Kranton 2000; Dustmann and Kirchkamp 2002; Chen, Jin, and Prettner 2020) and assume that rural residents compare their prospects in terms of remaining lifetime utility under the two scenarios of i) migrating or ii) staying put. Rational individuals will choose the option that leads to the higher indirect utility.

Consider a situation in which rural residents have the potential to live for two additional time periods $t = 1, 2$. Utility upon the adoption of the *hukou* reform is additively separable over time with $u(c_t)$ being the utility function for each period, which increases with consumption c_t. For simplicity, we assume that the household has already solved the standard dynamic optimization problem for optimal consumption in each period (Diamond 1965; Chakraborty 2004; Baldanzi, Prettner, and Tscheuschner 2019). Optimal consumption c_t^* then depends i) positively on wages, which rise with individual education level because more educated workers tend to earn higher wages; and ii) negatively on living costs comprising housing costs and the general price level of a certain location; and iii) negatively on migration costs, which increase with the distance of moving away from the place of origin.

Individuals discount the future with discount factor $\beta < 1$, such that indirect utility in case of staying put as a non-migrant is given by

$$U^p = u(c_1^{*p}) + \beta u(c_2^{*p}) + \widetilde{u}(z^p)$$

where the superscript p refers to the scenario of staying put and $\widetilde{u}(z)$ denotes the utility derived from obtaining access to local public services z for family members (e.g., children's education). By contrast, indirect utility of those who choose to move and become migrants is given by

$$U^m = u\left(c_1^{*m}\right) + \beta u\left(c_2^{*m}\right) + \widetilde{u}(z^m)$$

where the superscript m refers to migration. Upon the adoption of the *hukou* reform, individuals calculate

$$U^p - U^m, 0.$$

If $U^p - U^m > 0$, then individuals stay put; if $U^p - U^m < 0$, then they migrate. In case of $U^p - U^m = 0$, people are indifferent and we assume that they simply stay put in this case.

From these considerations, we can obtain the following implications.

(1) For some individuals, for whom staying put in the absence of the *hukou* reform was optimal (e.g., because otherwise they would have had to be separated from their family members owing to restricted access to children's education or they would have to pay more for children's education in destinations), $U^m > U^p$ holds upon the adoption of the *hukou* reform, which induces them to move. Thus, upon the adoption of the *hukou* reform, migration should increase. We call it a local citizenship (*hukou*) magnet effect.

(2) Disadvantaged individuals (e.g., less educated) are more likely to move and become migrants, because advantaged individuals can more easily obtain local *hukou* in destinations without the *hukou* reform. By contrast, disadvantaged individuals experience

[9] Although the *hukou* system operates within a single country, it works much like green-card and visa systems that typically function across countries.

more difficulty obtaining local *hukou* in destinations in the absence of the *hukou* reform, and $U^m > U^p$ holds upon the adoption of the *hukou* reform.

(3) Individuals from places of origin that are farther from potential destinations (e.g., rural areas of other cities) are less likely to move and become migrants, because moving is more costly for them, either financially or non-financially. Thus, it is less likely to have $U^m > U^p$.

(4) Individuals expecting higher wages in potential destinations in the long run are more likely to move and become migrants. For them, it is unclear whether or not migration inflows generate higher wages in potential destinations in the long run.[10] Two opposing general equilibrium effects can be accountable for the ambiguous change in expected wages in destinations in the long run. First, the influx of migrants from rural registered places puts downward pressure on wages in the long run owing to more labor supply in destinations. We call it a labor supply effect. Second, more domestic consumption, such as housing purchases from migrants who (are prepared to) obtain local *hukou*, leads to more labor demand and thereby puts upward pressure on wages in the long run in destinations.[11] We call it a labor demand effect.

In the rest of the paper, we analyze the extent to which these implications are supported by empirical evidence. Apart from examining the causal impact of access to *hukou* on migration, we assess which specific mechanisms are more likely to explain the patterns we find in the data.

3. Data, Variables, and Summary Statistics

3.1. Migration Inflows

The main data set we use is a 20% random sample of the 2005 Chinese Population (mini-) Census and a 3% random sample of the 2010 Chinese Population Census. Both surveys were conducted by the National Bureau of Statistics of China. The sample includes approximately 2.6 and 4 million individuals in 2005 and 2010, respectively. The (mini-) census data contain information such as age, gender, education degree, *hukou* type (AH or NAH), location of *hukou* registration (urban or rural), and migration history. In particular, the census data provide information on the timing of when migrants moved away from their registered *hukou* locations, enabling us to construct annual migration inflows for a number of periods. These migration inflows are directly computed on the basis of observed migrants rather than as a difference in stocks as common in the migration literature (Agersnap, Jensen, and Kleven 2020). We focus on migrants from rural areas who are between 15 and 59 years of age, and construct a panel data set on annual migration inflows from rural areas for 283 prefecture-level cities from 1999 to 2009.[12]

To construct the number of migration inflows, we define migrants in city s at year t (note that rural and urban areas exist in a city) as individuals who were AH holders and moved away from their registered rural *hukou* locations in China and arrived at the urban area of city s at year t.[13] We then aggregate all migrants at the city level, combined with sampling weights, to calculate the total number of migration inflows from rural areas in city s at year t. Given that the census data provide an interval in the duration of migration rather than the exact date of migration, we use the upper bound in the corresponding responses in the 2005 and 2010 population census data to determine the timing of migration inflows from 1999 to 2009 for the study.[14] In addition, the census data do not capture return migrants or those who moved to other destinations but had migrated to city s at year t. Thus, we consider all individuals who were AH holders and moved away from their registered rural *hukou* locations in China and arrived at the urban area of city s between t and $t+1$ and were still living in city s in the census year (2005 or 2010) as migration inflows (from rural areas) to city s in period t,[15] $t \in [1999, 2009]$.

Notably, the constructed migration inflow variable does not capture migrants who obtained local *hukou* in destinations in each period. Thus, the migration inflows observed in the census data underestimate the de facto net migration inflows that occurred. The

[10] Existing studies echo that ambiguous effects of migration exist on local labor market in destinations (Card 2001; Borjas 2003; Card 2005; Dustmann, Frattini, and Preston 2012; Borjas 2013). For example, Borjas (2013) theoretically shows that the wage effect of immigration depends on the relative impact on the potential size of the consumer base versus the size of the workforce.

[11] Intuitively, migrants who (are prepared to) obtain local *hukou* in destinations stay longer in destinations than otherwise, partly because obtaining local *hukou* itself requires a certain amount of migration duration, and partly because migrants' access to local citizenship (*hukou*) reduces their possibility of return migration. Numerous studies have shown that migrants who stay longer in destinations tend to consume more in destinations and to remit less to the places of origin than otherwise (Dustmann 1997; Dustmann and Mestres 2010a; 2010b; Dustmann, Fasani, and Speciale 2017). Dustmann and Gorlach (2016) report further analyses on the consequences of migration temporariness. Moreover, linking urban *hukou* admission to stable accommodation in destinations may not only produce a booming housing market but also create many industries and jobs related to construction, building material manufacturing, and housing services.

[12] China's constitution provides three de jure levels of government. However, five practical (de facto) levels of local government currently exist as follows: provincial (including province, autonomous region, municipality, and special administrative region), prefecture, county, township, and village. For simplicity, we treat "prefecture-level city" and "city" as interchangeable in this article. In practice, the provincial governments released general guidance on the *hukou* reform, and the city-level governments implemented it accordingly. Further details on the adoption of the *hukou* reform will be introduced later.

[13] More details on the definition of migration can be found in **Appendix A1**.

[14] More details on the definition of timing of migration can be found in **Appendix A2**.

[15] More details on the definition of migration inflows can be found in **Appendix A3**.

degree of under-estimation depends on the extent to which existing migrants obtained local *hukou* in destinations. This under-estimation may be less serious in the short term, because it usually takes a certain number of years (e.g., $3-5$ years) for existing migrants to satisfy entry requirements of obtaining local *hukou*. Specifically, we define the de facto net migration inflows that occurred in city s at year t as $M_{s,t,P+T}$, and define the number of migrants who obtained local *hukou* in city s at year t as $M_{s,t,P}$. Therefore, the migration inflows observed in the census data are captured by $M_{s,t,P+T}-M_{s,t,P}$. If more migrants are obtaining local *hukou*, then the number of migration inflows observed in the census data becomes smaller. Thus, even if both $M_{s,t,P+T}$ and $M_{s,t,P}$ are increasing substantially from t to $t+1$, the migration inflows observed in the census data, $M_{s,t,P+T}-M_{s,t,P}$, continue to possibly increase slightly or even decrease during the same period because the increase in $M_{s,t,P+T}$ is offset by the increase in $M_{s,t,P}$. Considering that migration inflows observed in the census data underestimate the de facto net migration inflows that occurred, our main estimate is likely to provide a lower bound for the migration impact of improved access to *hukou*.[16]

To check and alleviate the under-estimation concern, we attempt to approximate the number of migrants who obtained local *hukou* in that city in each year using an alternative data set. Moreover, we create an alternative measure of migration inflows by adding migrants without obtaining local *hukou* (benchmark measure) and migrants who obtained local *hukou*.[17] We also use two alternative measures of migration inflows constructed from comparable population (mini-) censuses in 1990, 2000, 2005, and 2010 for robustness checks.[18]

Another potential concern is that the constructed migration inflow variable does not capture step migration. Step migration occurs when migrants transit through another city before reaching their destinations. For instance, if an individual left his/her *hukou* registered location to city A in 2001 and then transited to city B in 2003, then we would only observe the last destination (i.e., city B). As adjusting for step migration would require strong assumptions on intermediate destinations, which are not observed in the census data, we do not consider step migration. That said, we will show later that the adoption of the *hukou* reform affects migration inflows from rural areas of the same city (intra-city migration) rather than migration inflows from rural areas of other cities (inter-city migration), which is likely to alleviate this concern.

3.2. Improved Access to Hukou

In this study, we focus on a recent *hukou* reform that improves migrants' access to *hukou* in destinations through easier acquisition of local *hukou*.[19] The local governments adopted the *hukou* reform at different points in time from 2002 to 2015.[20] We collected information on the timing of the adoption of the *hukou* reform for 283 prefecture-level cities; to the best of our knowledge, this process is the first attempt to construct such a data set.[21] To do so, we manually collected official documents, official news, as well as information from several well-known law databases for the *hukou* reform in each city and combined all the information to determine the exact timing of the adoption of the *hukou* reform.[22]

The main feature of the *hukou* reform was to substitute a rigid annual quota system with a de jure quota-free system that entails some basic entry requirements for assessing local *hukou* applications. Unlike under the rigid annual quota system, a strict de jure cap no longer exists on the number of migrants eligible to obtain local *hukou* and no de jure limits are set on age and education (skill) for the application of local *hukou*. These changes increased the chances and reduced the uncertainty of obtaining local *hukou* in destinations, particularly for migrants from rural areas who were more affected under the rigid annual quota system.[23] The post-reform entry requirements generally consist of having stable accommodation and a job (or a stable source of income). Some differences exist across cities with respect to defining stable accommodation or jobs. For example, public rental housing is considered stable accommodation in some cities such as Nanjing, whereas house ownership is required in other cities such as Lianyungang. Most cities consider a stable job

[16] Further discussions on other migration-related datasets in China can be found in **Appendix A4**.

[17] We use changes in the stock of NAH holders registered in a city to proxy for the number of (net) migrants who obtain local *hukou* in this city in each year. Note that the calculated migrants who obtained local *hukou* consist of existing migrants, who have stayed in destinations for several years without obtaining local *hukou,* and new arrivals, which may overestimate rural-urban migrants induced by the *hukou* reform. We will provide further details in Section 3.3.

[18] The main results are robust using alternative measures of migration. More details on the alternative measures of migration inflows can be found in **Appendix A5**.

[19] Although obtaining local *hukou* allows migrants to have access to local citizenship as local natives, migrants in certain cities can still possibly have access to *certain* benefits (e.g., education for their children) even without obtaining local *hukou*. That said, these migrants must pay *more* to gain access to these benefits than local natives. More importantly, several *uncertainties* (e.g., annual quotas) are associated with having access to these benefits for migrants without obtaining local *hukou*.

[20] In July 2014, the piecemeal *hukou* reform was extended to the national level, which was also reported by international media such as The Guardian (https://www.theguardian.com/world/2014/jul/31/), The Diplomat (https://thediplomat.com/2014/07/), and The Wall Street Journal (https://www.wsj.com/articles/BL-CJB-23419).

[21] A number of previous studies use similar *hukou* reforms at the provincial level but not at the city level (Sun, Bai, and Xie 2011; Song and Li 2014; Kinnan, Wang, and Wang 2018).

[22] Law databases can be found in http://www/pkulaw.cn/ and http://www.lawyee.org/.

[23] Under the rigid annual quota system, migrants from rural areas must satisfy an additional requirement of applying for local *hukou* (i.e., converting from AH to NAH *hukou*), compared with their counterparts from urban areas. This *hukou* conversion (from AH to NAH) was strictly controlled by an annual quota system. Consequently, migrants from rural areas are more likely to be affected by the *hukou* reform than their counterparts from urban areas in terms of the change from a rigid annual quota system to a de jure quota-free system for local *hukou* application.

as a job in government agencies, state-owned enterprises, collective enterprises, or private enterprises with labor contracts. Several cities also require the minimum time that a labor contract has been signed or the minimum time that a migrant has contributed to local social insurance.

To capture variations in the exposure to the *hukou* reform across cities over time, we not only sort out information on the timing of the adoption of the *hukou* reform but also use all the relevant regulations about *hukou* policies to capture the change in on-paper entry requirements in each city over time. The latter can be viewed as the intensity of the reform and exposure.[24] Lower entry requirements suggest higher intensity. We use the city-level *hukou* (strictness) index from Fan (2019).[25] This index measures the entry requirements faced by migrants in obtaining *hukou* between 1997 and 2010 in each city, in which a higher value of the index implies lower entry requirements of obtaining local *hukou*. The minimum and maximum values of the index are 0 and 6, respectively. A combination of the data we collected on the adoption of the *hukou* reform and the index from Fan (2019) enables us to capture two different city-level variations in the exposure to the *hukou* reform: 1) the timing of the change from an annual quota system to a de jure quota-free system for the local *hukou* application and 2) any change in on-paper entry requirements of obtaining local *hukou*. The different variations enable us to identify the impact of an important but understudied aspect of the *hukou* system (i.e., quota restrictions), and to test whether or not the impact of relaxing quota restrictions for local *hukou* application depends on the on-paper entry requirements of obtaining local *hukou* across the reform cities. The rationale for the latter is that relaxing quota restrictions for local *hukou* application may matter considerably for individuals who satisfy entry requirements but who had been deterred by the annual quota system prior to the adoption of the *hukou* reform.

3.3. Complementary Data Sets

To test the likely mechanisms through which improved access to *hukou* affects migration and competing hypotheses, we complement our main analysis with two additional data sets. The first is the *China City Statistical Yearbook* (1998–2011) constructed by the National Bureau of Statistics of China. We use this data set to construct a panel data set on the total number of NAH holders registered in each city from 1997 to 2008 to capture migrants who obtained local *hukou*.[26] The rationale is as follows: in general, changes in the total number of NAH holders registered in each city are determined by natural population growth rate, migrants who obtained local *hukou*, as well as emigrants who obtained local *hukou* in other cities. We assume that the natural population growth rate is stable over time, which is likely to be true, given China's mandatory one-child policy. Changes in the total number of NAH holders registered in each city thus represent changes in (net) migrants who obtained local *hukou* in that city. If the *hukou* reform in that city makes obtaining local *hukou* easier for existing migrants over time, then we should also find corresponding increases in the total number of registered NAH holders. We use this data set to examine the extent to which the *hukou* reform makes it easier for migrants to obtain local *hukou* over time, which, in turn, helps precisely interpret the estimated migration impact of improved access to *hukou* (e.g., the degree of underestimating migration inflows over time).

Moreover, we use additional information from the *China City Statistical Yearbook* (1998–2011) to construct measures of labor demand relevant to booming housing markets in destinations. Given that some basic entry conditions, such as stable accommodation, are required for migrants to obtain local *hukou* after the *hukou* reform, we expect that this reform's adoption is likely to generate additional labor demand associated with booming housing markets in destinations. For example, the real estate market boom may create many industries and jobs related to construction, building material manufacturing, and housing services. Even if housing purchases are not required for obtaining local *hukou* in destinations, labor demand is likely to increase if migrants who (are prepared to) obtain local *hukou* choose to stay longer and consume more in destinations.

The second data set we use for further exploration is the *Chinese Annual Survey of Industrial Firms* (2000–2008) constructed by the National Bureau of Statistics of China. This survey includes all industrial firms that are either owned by the state or are non-state firms with sales above 5 million RMB.[27] Industry is defined to include mining, manufacturing, and public utilities.[28] We construct a balanced panel data set of 28,637 firms in each year from 2000 to 2008 to conduct the analysis. We focus on two firm-level variables: average wages and total employment. The dataset allows us to examine the dynamic labor market consequences of the adoption of the *hukou*

[24] Given that this measure does not determine unspoken quotas or rules known to local officials who make *hukou* granting decisions but not actually written, this measure alone is insufficient to determine the de facto entry requirements of obtaining local *hukou*. Nevertheless, this situation enables us to investigate the impact of relaxing quota restrictions, conditional on on-paper entry requirements.

[25] Fan (2019) collects news articles, official documents, and government regulations about *hukou* policies at the local level from keywords searched on two comprehensive databases. Following a set of criteria, the author reviews these documents and hand-codes them into a score of 0 to 6 for each city on the basis of the on-paper entry requirements faced by migrants in obtaining *hukou*. These criteria include, for example, whether renting/purchasing local properties and/or working in the destination for a sustained period of time is required for local *hukou* application. A higher score means lower entry requirements of obtaining local *hukou*.

[26] The National Bureau of Statistics of China did not release the number of NAH holders registered in each city after 2008.

[27] A comparison with the full census of firms reveals that 80% of all industrial firms are excluded from the sample. Fortunately, they account for only a small fraction of economic activity. In 2004, below-scale firms employed 28.8% of the industrial workforce, but produced only 9.9% of output and generated 2.5% of exports. More details on the data have been provided by Brandt, Van Biesebroeck, and Zhang (2012).

[28] According to the *China City Statistical Yearbook*, the industrial sector (or secondary industry) accounts for about 49% of the gross domestic product (GDP) and 44% of total employment in 2005. According to the *Population Census Survey*, the number of migrants working in the industrial sector accounts for about 46% of total migrants in 2005. Given that the firm-level data are not representative of migrants working in the service industry and other non-industrial industries, the results from these data should be interpreted cautiously.

Table 1
Summary statistics

Variables	N	Mean	Median	Std. Dev.	min	max
Migration inflows from rural areas at the city level (1999–2009)	3103	59939.63	20254.63	111722.48	0.00	1551815.13
Logarithm of migration inflows from rural areas at the city level (1999–2009)	3065	9.99	9.95	1.48	4.65	14.25
Treatment variable of the quota reform at the city level	3103	0.23	0.00	0.42	0.00	1.00
Treatment variable of the requirements reform at the city level (Fan, 2019)	3103	2.17	2.00	1.39	0.00	6.00
Number of NAH holders registered at the city level (10,000) (1997–2008)	3118	114.44	92.28	85.37	10.79	704.17
Logarithm of number of NAH holders registered at the city level (1997–2008)	3118	4.52	4.52	0.66	2.38	6.56
Logarithm of average wage at the firm level (2000–2008)	257733	2.46	2.44	0.65	–7.38	10.09
Logarithm of total employment at the firm level (2000–2008)	257733	5.43	5.34	1.14	2.08	11.99
Reductions in trade policy uncertainty at the city level (1999–2008)	2730	0.31	0.31	0.09	0.03	0.53
Logarithm of monthly minimum wage at the city level (1999–2008)	2652	5.95	5.94	0.32	5.37	6.91
Abolition of agricultural tax at the city level (1999–2008)	2730	0.65	1.00	0.48	0.00	1.00
Logarithm of total employment in the construction industry at the city level (1998-2010)	3504	9.77	9.69	1.06	5.29	13.07
Logarithm of total employment in the real estate industry at the city level (1998-2010)	3481	7.53	7.54	1.12	4.60	11.75
Logarithm of per capita GDP at the city level (2001)	2778	8.91	8.48	0.65	7.71	11.91
Total population at the city level (2001, in 10,000 people)	2778	102.23	77.25	87.54	16.1	758.23
State-sector ratio at the city level (2001)	2778	0.34	0.32	0.21	0.01	0.93
Coastal provinces (0/1)	2778	0.28	0	0.45	0	1

Notes: We construct the migration-inflow variable using China's population census data in 2005 and 2010. Treatment variables are based on the data we collected on the adoption of the *hukou* reform and the *hukou* strictness index from Fan (2019). We collected the data on the number of NAH holders registered at the city level from the China City Statistical Yearbook (1998–2009). We construct the firm-level variables (e.g., average wages and total employment) from the Chinese Annual Survey on Industrial Firms (2000–2008). In constructing the reductions-in-trade-policy-uncertainty variable, we follow the literature on trade liberalization (Pierce and Schott 2016; Facchini et al. 2019). We obtain the monthly-minimum-wage variable from official documents released by local governments. We construct the abolition-of-the-agricultural-tax variable following the approach of Chen and Wang (2014). We collected the data on the total employment in the construction industry and real estate industry at the city level from China City Statistical Yearbook (1999-2011).

reform in destinations. If improved access to *hukou* leads to reductions in total wages in destinations in the long term owing to more labor supply in the local labor market and thus exerts a negative impact on migration decisions, then we should accordingly find supporting evidence from the long-term effects of the *hukou* reform on local average wages and total employment. Apart from examining the labor supply (labor demand) effect, this data set allows us to test a competing hypothesis that migration inflows are driven by higher wages in reform cities that are independent of or associated with the *hukou* reform itself. Specifically, if more migration inflows in reform cities are driven by higher wages or fast wage growth in reform cities than in non-reform cities, then average wages between reform and non-reform cities in the pretreatment periods should have significant differences.

3.4. Confounding Policies

Finally, we construct variables of three confounding policies at the city level that might be correlated with the timing and intensity of exposure to the *hukou* reform. The first policy variable is reductions in trade policy uncertainty after China's accession to the WTO in 2001, which may have increased migration inflows and induced local authorities to adopt more migrant-friendly regulations (Facchini et al. 2019; Tian 2019). We measure city-level reductions in trade policy uncertainty following the procedure introduced by Facchini et al. (2019).[29] The second policy variable is increases in minimum wages in urban areas, which are likely to affect migrants' labor supply to destinations (Fang and Lin 2015; Gan, Hernandez, and Ma 2016). We collect official data on the monthly minimum wage at the city level over time. The third policy variable is the abolition of agricultural taxes in rural areas, which is likely to have affected rural people's income and thus disincentivizes them to out-migrate (Chen and Wang 2014; Chen 2017). We measure the city-level abolition of agricultural taxes following the procedure introduced by Chen and Wang (2014). Controlling for these policy variables alleviates the concern that the impact of improved access to *hukou* is contaminated by other confounding policies.

Table 1 reports the summary statistics for the main variables. Fig. 1 shows the cumulative percentage of cities that had adopted the *hukou* reform from 2002 to 2015 ("reform cities" hereafter). We find that 103 cities adopted the *hukou* reform from 2002 to 2005, accounting for 36% of all cities. Moreover, the number of reform cities increased dramatically from 2002 to 2005, then increased relatively steadily until 2014, and finally sharply increased in 2015. The sharp increase in reform cities after 2014 was caused by the extension of the *hukou* reform to the national level. Fig. 2 shows the average number of migration inflows between treatment and control groups over time in an event-study setup.[30] We find evidence that the pretreatment trends in the average number of migration

[29] Details on this measure can be found in Section 5.4.

[30] Specifically, we define treatment group as cities that adopted the *hukou* reform between 2002 and 2005, and control group as cities that did not adopt the *hukou* reform between 2002 and 2009. Using the number of migration inflows between 1999 and 2009 as the outcome variable, we define "time since reform adoption" variable as the time lag between the date when migration occurred and the date when the city adopted the *hukou* reform. We do not consider changes in entry requirement of obtaining local *hukou* across reform cities over time in this exercise.

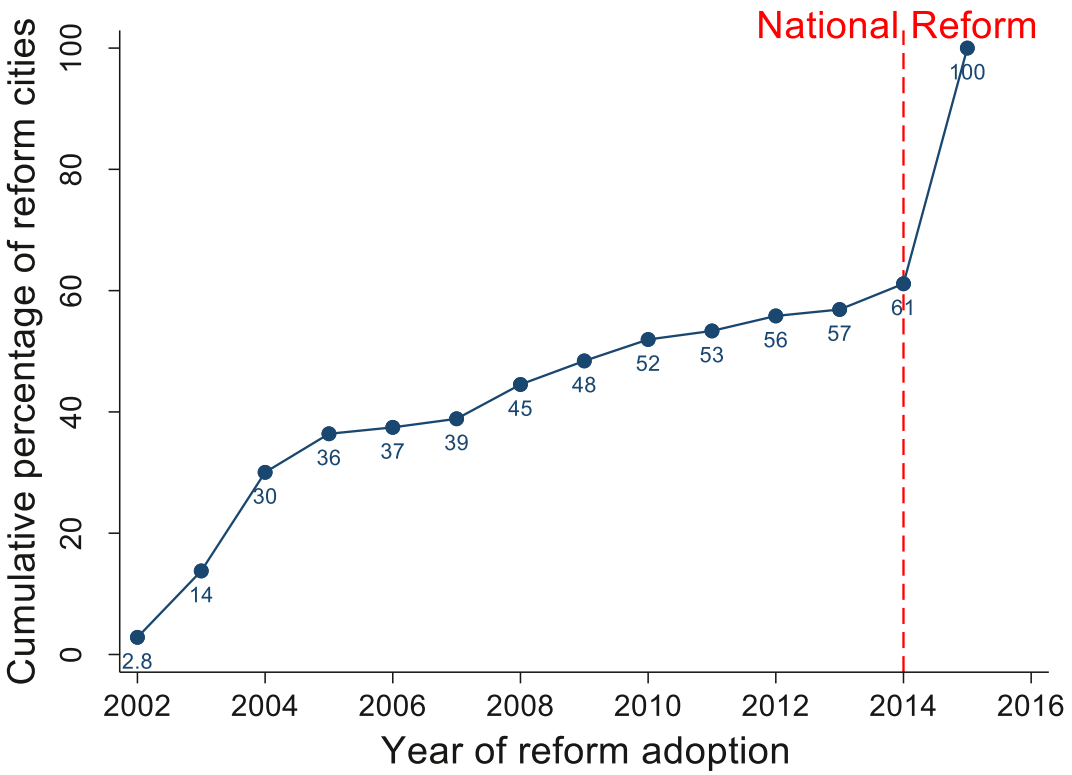

Fig. 1. Cumulative percentage of cities that had adopted the *hukou* reform from 2002 to 2015. Note: The *hukou* reform was extended to the national level in July 2014, and we consider that all of the cities that had yet to adopt the reform before July 2014 did adopt it in 2015.

inflows are similar, but the treatment group slowly diverges from the control group after the adoption of the *hukou* reform.

4. Model Specifications and Identification Strategies

The regression specification evaluates the effects of improved access to *hukou* as a quasi-experiment. Treatment is the degree of exposure to the *hukou* reform. We construct two different treatment variables to capture 1) the change from an annual quota system to a de jure quota-free system for the local *hukou* application (the *quota reform* hereinafter) and 2) any change in on-paper entry requirements of obtaining local *hukou* (the *requirements reform* hereinafter). We estimate these treatment effects in a DID framework, following Chandra, Gruber, and McKnight (2010) and Agersnap, Jensen, and Kleven (2020). The model specification is as follows:

$$y_{st} = \alpha' \mathrm{I}_s + \beta' \mathrm{I}_t + \gamma_1 \, \mathrm{I}_{s,t \geq t_{reform}} + \gamma_2 \, Intensity_{st} + \varepsilon_{st}, \tag{(1)}$$

where y_{st} is the logarithm of total migration inflows from rural China in city s in year t, I_s is the vector of city fixed effects, I_t is the vector of time fixed effects, $\mathrm{I}_{s,t \geq t_{reform}}$ is a dummy for an observation after the adoption of the quota reform in city s (specifically, it can be viewed as an interaction of an indicator variable of being in a city s where the quota reform is adopted and an indicator for being in the post-reform era), $Intensity_{st}$ is a *hukou* index from Fan (2019), measuring the requirements reform in city s in year t. Notably, a higher value of the $Intensity_{st}$ variable is associated with lower on-paper entry requirements of obtaining local *hukou*, and the minimum and maximum values of the index are 0 and 6, respectively. According to the data, the correlation between $\mathrm{I}_{s,t \geq t_{reform}}$ and $Intensity_{st}$ is 0.42, suggesting that the quota reform variable is positively associated with the requirements reform variable. The error term is ε_{st}, and α and β are vectors of coefficients to be estimated; γ_1 captures the treatment effect of the quota reform, ceteris paribus; and γ_2 captures the treatment effect of the requirements reform, ceteris paribus.[31] We cluster standard errors at the city level.

As discussed before, the variations in the exposure to the *hukou* reform come from two sources. The first is cross-city variation in the timing of when or whether cities adopt the quota reform. The second is cross-city variation in the requirements reform. The assumptions underlying the identification of parameter γ are similar to all DID analyses: the decision of whether, to what extent, and when to adopt the *hukou* reform must be uncorrelated with any prior trends in migration outcomes, that is, in the absence of the *hukou* reform, the difference between the "treatment" and "control" group is constant over time, and the exposure to the *hukou* reform cannot coincide with any city-specific shocks or policies that might influence migration outcomes.

[31] In Section 5.9, we also present results by excluding the entry requirements variable.

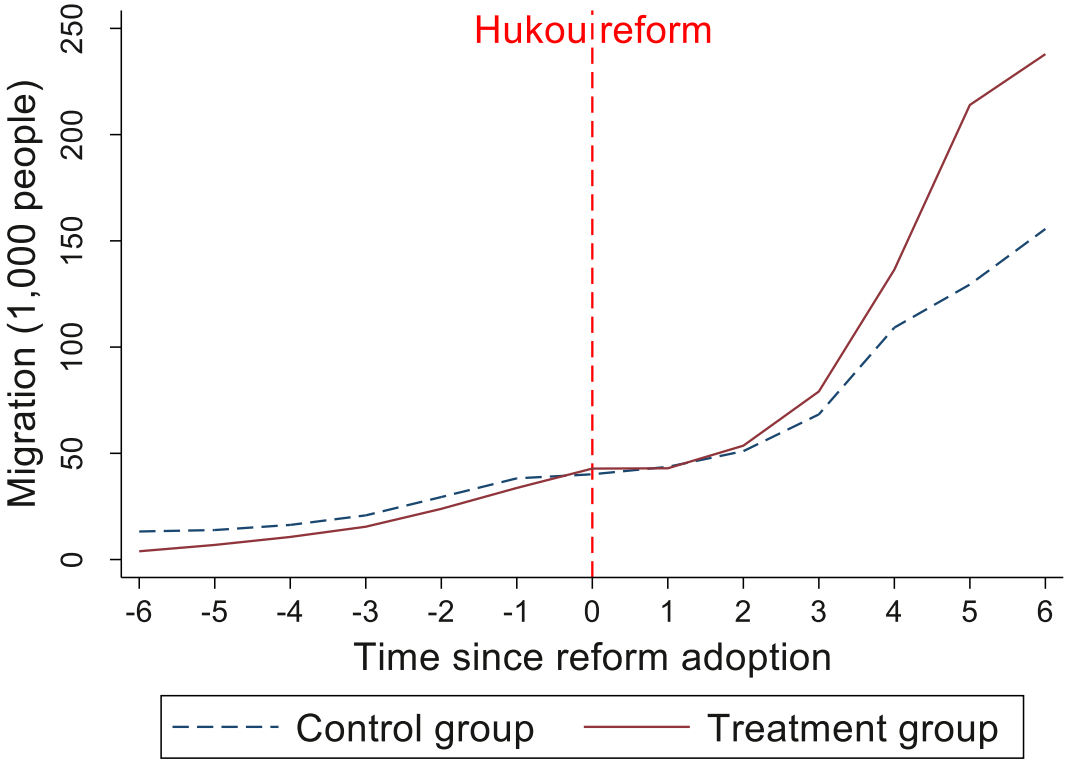

Fig. 2. Number of migration inflows between treatment and control groups over time. Notes: The treatment group consists of cities that adopted the *hukou* reform between 2002 and 2005. The control group consists of cities that did not adopt the *hukou* reform between 2002 and 2009. We use the number of migration inflows between 1999 and 2009 as the outcome variable. We define "time since reform adoption" variable as the time lag between the date when migration occurred and the date when the city adopted the *hukou* reform. We do not consider changes in entry requirements of obtaining local *hukou* across reform cities over time in this exercise.

The identification triggers several potential threats. Recent studies show that the two-way fixed effects regression above identifies the effect under the standard "common trends" assumption when the treatment effect is constant across groups and over time. However, a constant treatment effect is often implausible. If the constant effect assumption is violated, then the two-way fixed effects regression identifies weighted sums of the average treatment effects in each group and period, with weights that may be negative (de Chaisemartin and D'Haultfoeuille 2020; Sun and Abraham 2020; Callaway and Sant'Anna 2021; Athey and Imbens 2022).

Specifically, de Chaisemartin and D'Haultfoeuille (2020) show that parameter γ in Eq. (1) is equal to a weighted sum of the treatment effect in each treated $(s,\ t)$ cell: $\gamma = E\left[\sum_{(s,t):D_{s,t}=1} W_{s,t}\Delta_{s,t}\right]$, where E denotes the expectation, $\Delta_{s,t}$ is the average treatment effect (ATE) in group s and period t, and the weights $W_{s,\ t}$s sum to one but may be negative. Negative weights arise because $\widehat{\gamma}$ is a weighted sum of several DID estimates, which compare the evolution of the outcome between consecutive time periods across pairs of groups. However, the "control group" in some of those comparisons may be treated at both periods, which may lead to the negative weights.[32] Owing to the negative weights, the linear regression coefficient may, for instance, be negative; whereas all of the average treatment effects are positive. To check the negative weights problem, we use the *twowayfeweights* Stata package to compute the weights $W_{s,\ t}$s attached to each average treatment effect in group s and period t. If many weights are negative, then we resort to a new estimator proposed by de Chaisemartin and D'Haultfoeuille (2020) to estimate the treatment effects. This estimator also allows us to check the

[32] To illustrate this possible result, we consider a simple example of a staggered adoption design with two groups and three periods: group 1 is untreated at periods 1 and 2 and treated at period 3, whereas group 2 is untreated at period 1 and treated at periods 2 and 3. In this example, $\gamma = (DID_1 + DID_2)/2$, with $DID_1 = E(Y_{2,2}) - E(Y_{2,1}) - [E(Y_{1,2}) - E(Y_{1,1})]$, $DID_2 = E(Y_{1,3}) - E(Y_{1,2}) - [E(Y_{2,3}) - E(Y_{2,2})]$. The first DID compares the evolution of the mean outcome from periods 1 to 2 in groups 2 and 1. The second one compares the evolution of the mean outcome from periods 2 to 3 in groups 1 and 2. The control group in DID_2 (group 2) is treated in pre- and post-periods. Therefore, under the common trends assumption, it follows from de Chaisemartin and D'Haultfoeuille (2020) that DID_1 is equal to the ATE in group 2 in period 2 (*i.e.*, $DID_1 = E[\Delta_{2,2}]$), and DID_2 is equal to the ATE in group 1 in period 3, minus the change in the group 2's ATE between periods 2 and 3 (*i.e.*, $DID_2 = E[\Delta_{1,3}] - (E[\Delta_{2,3}] - E[\Delta_{2,2}])$). Intuitively, the mean outcome of groups 1 and 2 may follow different trends from periods 2 to 3 either because group 1 becomes treated, or because group 2's ATE changes. Consequently, $\gamma = E[\Delta_{2,2}] + E[\frac{1}{2}\cdot\Delta_{1,3}] + E[-\frac{1}{2}\cdot\Delta_{2,3}]$, where $W_{2,2} = 1$, $W_{1,3} = \frac{1}{2}$, $W_{2,3} = -\frac{1}{2}$. Negative weights arise because $\widehat{\gamma}$ uses treated observations as controls.

common trends assumption of the DID setup when heterogeneous treatment effects exist (e.g., by groups or over time). Our estimates are less likely to be threatened by the endogenous adoption of the *hukou* reform if the common parallel assumption holds in the full sample.[33]

The second potential identification problem, that is, the potential of unobserved city-specific shocks correlated with the *hukou* reform, is more difficult to investigate. However, many variations exist in the timing of when cities adopt the *hukou* reform, as shown in Fig. 1. Thus, unobserved shocks that are systematically correlated with the timing of the *hukou* reform are unlikely. To support formally the contention that unobserved city-specific shocks correlated with the timing of the *hukou* reform are not biasing the estimates, we perform permutation tests for the outcome variable that randomly re-assigns the timing of the *hukou* reform across cities. We perform this process in two ways. First, we randomly assign the timing of the *hukou* reform between 2002 and 2015 across cities. Second, we randomly assign the timing of the *hukou* reform so that the same percentage of cities adopted the *hukou* reform in each year, as shown in Fig. 1.

In addition, we conduct a falsification test using urban-to-urban migrants. We define urban-to-urban migrants in city s at year t as individuals who were NAH holders and moved away from their registered urban *hukou* locations in China and arrived at the urban area of city s at year t.[34] Although the *hukou* reform may affect urban-to-urban migrants through the change in entry requirements of obtaining local *hukou*, it is much less likely to affect these migrants through the quota reform than their counterpart from rural areas. This finding is explained by the fact that an important part of the annual quota system is restricting the conversion from AH to NAH *hukou*, which is inapplicable to urban-to-urban migrants who are NAH holders. Considering that we can separately identify the impact of the quota reform in the data, we should not find similar impacts for urban-to-urban migrants if our main estimates are solely driven by the *hukou* reform. Otherwise, the estimates are driven by other unobserved city-specific shocks.

Moreover, we use an alternative wave of census data from 2000 to construct migration inflows (from rural areas) from 1995 to 2000. Given that these migration inflows occurred prior to the adoption of the *hukou* reform, we use them to conduct a placebo analysis. Specifically, we use migration inflows in city s at year t to construct counterfactual migration inflows in city s at year $t+5$. Accordingly, we construct counterfactual migration inflows from 2000 to 2005 using de factor migration inflows that occurred from 1995 to 2000 in each city. If our main estimates are not driven by other unobserved city-specific shocks, then we should not find any significant impacts of the *hukou* reform for these counterfactual migration inflows.

Furthermore, the existence of alternative policies that occurred concurrently with the *hukou* reform is another threat to identification. To address the concern that other confounding policies would contaminate the results, we control for three other city-level policies that were implemented around the same time as the *hukou* reform and that could impact migration. The first is reductions in trade policy uncertainty after China's accession to the WTO in 2001. The second is the increases in minimum wages in urban areas. The third is abolition of agricultural taxes in rural areas. If the *hukou* reform works according to our hypothesis, then controlling for these confounding policy variables should not change the main results.

Lastly, to mitigate the concern that our estimates are contaminated by city-specific trends, we control for the effects of covariates (e.g., demand shock, GDP per capita, total population, size of state sector, coastal VS inland) using two approaches. The first approach involves interacting these covariates with year dummies, while the second approach entails choosing markedly comparable subsamples based on propensity score matching (Abadie 2005; Brambilla, Lederman, and Porto 2012). If our main estimates are not driven by city-specific trends, then controlling for the effects of these covariates should not significantly alter our main findings.

5. Empirical Results

This section answers three questions. First, to what extent does improved access to *hukou* in destinations affect migration decisions? Second, are the migration effects of improved access to *hukou* heterogeneous across groups (e.g., by age, gender, and skill)? Are the migration effects different between migration from rural areas of the same city and migration from rural areas of other cities? Third, what is the likely underlying mechanism through which improved access to *hukou* in destinations affects migration decisions?

5.1. Pretreatment Trends

A crucial assumption to justify the DID approach is the common parallel trends in the pretreatment periods. Fig. 2 shows that the pretreatment trends are similar. To test this assumption formally, we resort to the new estimator proposed by de Chaisemartin and D'Haultfoeuille (2020).[35] If the common parallel assumption is valid, then we should not find any significant impacts in the pretreatment periods. Fig.s 3a and 3b show the placebo effects for each treatment variable in the pretreatment periods.[36] In each figure,

[33] Rejection of common trends assumption may make our estimates biased. For example, if cities negatively select into the reform adoption (i.e., unattractive cities adopt the reform earlier than attractive cities), then our benchmark estimates may be biased downward.

[34] Given that urban-to-rural migrants represent a small share of emigrants from urban areas, we do not distinguish between urban-to-rural and urban-to-urban migrants, and consider all of them as urban-to-urban migrants in the main analysis.

[35] For each treatment period t, the estimator compares the evolution of the mean outcome between $t-1$ and t in two sets of groups: the reform adopters (i.e., a group switching from being untreated to treated), and those that remain untreated. For each pretreatment period t, the (placebo) estimator essentially compares the outcome's evolution from $t-2$ to $t-1$, in groups that adopt the reform and in those that do not adopt the reform between $t-1$ and t. Further details on the estimator can be found in de Chaisemartin and D'Haultfoeuille (2020).

[36] We use STATA code– – –*did_multiplegt* to draw these figures.

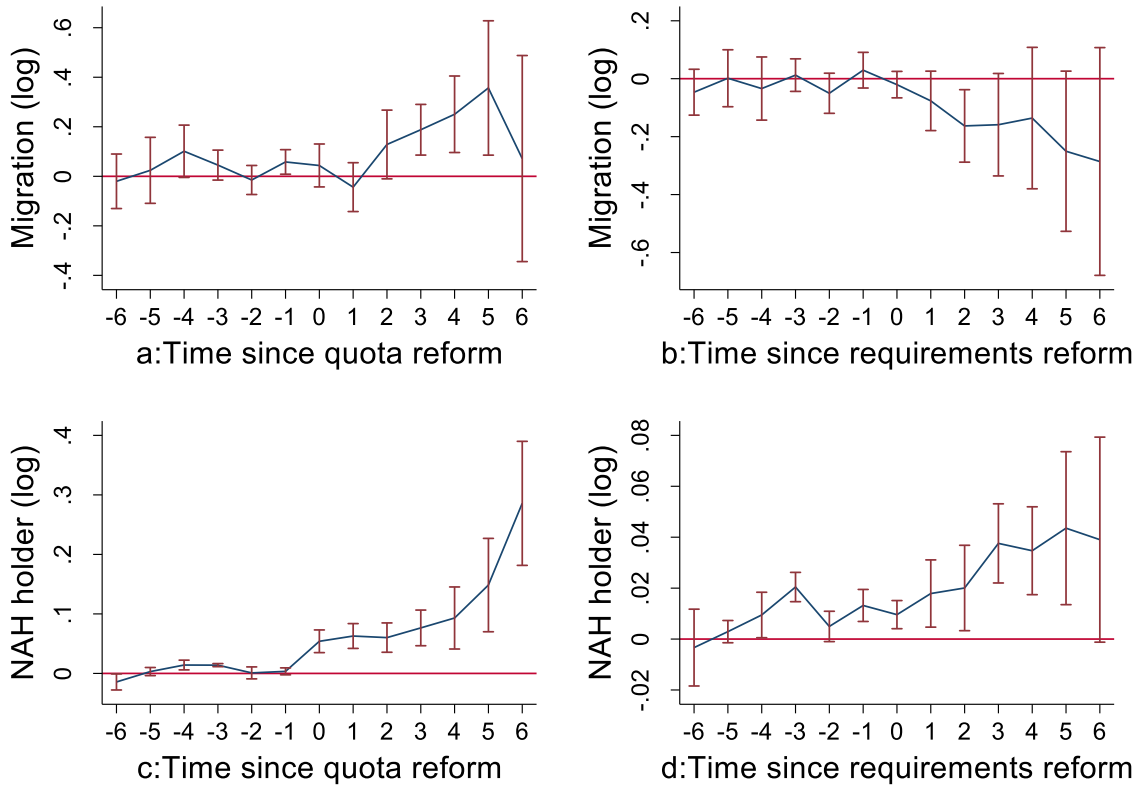

Fig. 3. Effects of improved access to *hukou* on migration (in log) Notes: The upper-left panel (**Fig. 3a**) shows the impact of the quota reform on the number of (net) migration inflows. The upper-right panel (**Fig. 3b**) shows the impact of the requirements reform on the number of (net) migration inflows. The lower-left panel (**Fig. 3c**) shows the impact of the quota reform on the number of NAH holders. The lower-right panel (**Fig. 3d**) shows the impact of the requirements reform on the number of NAH holders. In each panel, the horizontal axis represents the values corresponding to the time relative to the adoption the *hukou* reform, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. All of the outcome variables are in the logarithm form. The panel data on migration inflows from 1999 to 2009 are constructed using the 2005 and 2010 censuses. The panel data on NAH holders from 1997 to 2008 are constructed using the China City Statistical Yearbook (1998–2009). To draw these figures, we have followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

the horizontal axis represents the values corresponding to the time relative to the adoption of the *hukou* reform, which are negative in the pretreatment periods. The vertical axis represents the values corresponding to the change in the outcome of interest (i.e., the logarithm of total migration inflows from rural China). The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. We find consistent evidence that the placebo effects are close to zero and not statistically significant at the conventional level in the pretreatment periods.

5.2. *Improved Access to Hukou and Migration Decisions*

By using equation (1), we estimate the impact of improved access to *hukou* on the logarithm of migration inflows from rural areas.[37] Table 2 reports our main results. Column 1 shows that the coefficient of the $I_{s,t\geq t_{reform}}$ variable (γ_1) is 0.222. Specifically, holding other

[37] We find consistent results using migration inflows from rural areas (in level) and migration inflows from rural areas normalized by the total population of the city as outcome variables. Further details are shown in Fig.s A4a and A4b.

Table 2
Impact of improved access to *hukou* on migration (heterogeneous entry requirements)

Variables	(1) Benchmark	(2) Alternative sample of migrants	(3)	(4) Benchmark	(5) Subsample by age of migrants	(6)	(7)	(8)
	Rural migration	Urban	Counterfactual	Other policies	Ages 15–29	Ages 30–44	Ages 45–59	Ages 60+
$I_{s,t\geq t_{reform}}$	0.222***	−0.047	0.007	0.213***	0.245***	0.186**	0.050	−0.001
	(0.073)	(0.053)	(0.072)	(0.073)	(0.080)	(0.075)	(0.063)	(0.061)
$Intensity_{st}$	−0.028	0.004	−0.014	−0.034	−0.027	−0.007	0.010	−0.015
	(0.024)	(0.017)	(0.029)	(0.025)	(0.028)	(0.023)	(0.018)	(0.018)
Observations	3065	3041	1634	2619	2863	2860	2598	2284
Variables	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Subsample by gender of migrants		Subsample by skill of migrants				Subsample by migration distance	
	Male	Female	Low	Medium-low	Medium-high	High	Intra-city	Inter-city
$I_{s,t\geq t_{reform}}$	0.185**	0.214***	0.129*	0.205**	0.073	0.007	0.175**	0.054
	(0.079)	(0.074)	(0.073)	(0.079)	(0.072)	(0.099)	(0.068)	(0.062)
$Intensity_{st}$	−0.022	−0.022	−0.013	−0.014	−0.028	−0.023	−0.007	−0.043**
	(0.025)	(0.023)	(0.022)	(0.025)	(0.024)	(0.031)	(0.020)	(0.018)
Observations	2881	2915	2783	2902	2561	1828	2881	2745
Variables	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
	Subsample by demand shocks		Alternative specification		Subsample		Another data	Control for Covariates
	Smaller demand	Larger demand	Demand shock	Requirements	Unmatched	Matched	NAH inflows	
$I_{s,t\geq t_{reform}}$	0.306**	0.155	0.339***	0.125	0.219***	0.261***	0.369***	0.199***
	(0.117)	(0.098)	(0.105)	(0.205)	(0.083)	(0.094)	(0.093)	(0.064)
$I_{s,t\geq t_{reform}} \# L_s$			−0.208*					
			(0.122)					
$I_{s,t\geq t_{reform}} \# Intensity_{st}$				0.031				
				(0.059)				
$Intensity_{st}$	−0.023	−0.030	−0.027	−0.034	-0.025	−0.019	-0.021	-0.047**
	(0.033)	(0.035)	(0.024)	(0.024)	(0.025)	(0.032)	(0.031)	(0.019)
Observations	1478	1489	2967	3065	2213	1362	2546	2689

Notes: This table shows the effects of improved access to *hukou* on migration decisions. Column 1 shows the impact of improved access to *hukou* on migration inflows from rural areas. Column 2 shows the impact of improved access to *hukou* on migration inflows from urban areas. Column 3 show the impact of improved access to *hukou* on counterfactual migration inflows from rural areas (1995–2000). Column 4 shows the impact of improved access to *hukou* on migration inflows from rural areas by controlling for three confounding polices, namely, reductions in trade policy uncertainty after China's accession to the WTO, increases in minimum wages in urban areas, and the abolition of agricultural taxes in rural areas. Columns 5 to 8 show the heterogeneous migration effects by age. Columns 9 to 10 show the heterogeneous migration effects by gender. Columns 11 to 14 show the heterogeneous migration effects by skill. Columns 15 to 16 show the heterogeneous migration effects by migration distance. Columns 17 to 18 show the heterogeneous migration effects by demand shocks. Column 19 examines how the migration impact varies by demand shocks. Column 20 examines how the migration impact varies by entry requirements. Columns 21 and 22 show the migration impact based on unmatched and matched subsample. To construct this subsample, we keep those cities that adopted the *Hukou* reform in 2003 and 2004, and those cities that adopted the reform in 2008 and afterwards. Column 23 shows the effect of improved access to *hukou* on NAH inflows (a proxy for migrants who obtained local *hukou*). Column 24 shows the migration impact by controlling for a number of covariates linearly in the model. Standard errors are clustered at the city level and are in parentheses.

* $p < 0.1$,
** $p < 0.05$,
*** $p < 0.01$.

factors constant, the quota reform increases the average migration inflows from rural areas by 24.8%$[(e^{0.222}-1)\cdot 100\%]$. This result suggests that improved access to *hukou* in destinations does substantially affect migration decisions. According to population census data in 2000 and 2010, the average migration inflows (stocks) from rural areas across cities were 53,942 (169,020) and 164,244 (805,151) in 2000 and 2010, respectively. This result implies that the quota reform increases the average migration inflows from rural areas across cities by approximately 13,377 ($=53,942\cdot 0.248$) people per year or 7.9 percent of the stock of migrants across cities in 2000, and approximately 40,732 ($=164,244\cdot 0.248$) people per year or 5 percent of the stock of migrants across cities in 2010 accordingly.[38]

[38] According to Agersnap, Jensen, and Kleven (2020), a policy of benefit reduction in Denmark reduced the net flow of immigrants by approximately 5,000 people per year, or 3.7% of the stock of treated immigrants. A possible explanation for the larger migration effect found in our paper is that internal migrants are more sensitive to provisions of welfare benefits than international migrants, which is somewhat supported by our later findings that the migration impact of improved access to *hukou* is more pronounced for intra-city migration than for inter-city migration. The broader coverage of non-cash benefits (e.g., children's education, health care) in China compared with cash benefits investigated in the Agersnap, Jensen, and Kleven (2020), could be another explanation for the larger migration effect found in this paper.

Moreover, the requirements reform variable (a higher value represents lower on-paper entry requirements of obtaining local *hukou*) is negatively associated with migration inflows from rural areas. Specifically, holding other factors constant, a 1-unit increase in the value of the requirements reform variable leads to 2.8% lower migration inflows from rural areas, which is not statistically significant at a conventional level. These contrasting results from two different treatment variables suggest that the quota reform, or relaxation of quota restrictions, matters more than the requirements reform in affecting migration decisions.[39]

Apart from presenting the pretreatment trends, Fig.s 3a and 3b show the dynamic migration impacts for different treatment variables.[40] Fig. 3a reveals a significant increase in migration inflows shortly after the adoption of the quota reform. Specifically, holding other factors constant, the quota reform increases the average migration inflows from rural areas by 13.6% in the second year of the adoption of the *hukou* reform, and the impact increases steadily to 42.7% in the fifth year of the reform adoption.[41] However, after reaching a peak in the fifth year of the reform adoption, the impact decreases and becomes statistically insignificant in the sixth year of the reform adoption.[42] A possible explanation for the falling migration impact is that the constructed migration variable seriously underestimates the de facto migration inflows in reform cities when a large number of migrants in such cities successfully obtained local *hukou* after a certain period of waiting. That said, without exploring this explanation further, we cannot rule out an alternative possibility that improved access to *hukou* may lead to reductions in total wages in destinations owing to relatively more labor supply in the local labor market. Thus, it exerts a negative impact on migration decisions in the long term (the labor supply effect). We explore this underlying explanation later. Again, we find negative migration impacts of the requirements reform in Fig. 3b.

As mentioned earlier, we check the negative weights problem and compute the weights attached to each average treatment effect in group s and period t. The negative weights problem is more serious for γ_2 but much less so for γ_1 in the data.[43] To reduce the concern that the coefficients of interest, particularly for γ_2, do not reflect the actual treatment effects owing to the negative weights problem, we resort to the new estimator proposed by de Chaisemartin and D'Haultfoeuille (2020) that is valid even if the treatment effect is heterogeneous over time or across groups (Fig.s 3a and 3b show the results).

As discussed above, a primary concern in the identification strategy is the existence of possible unobserved city-specific shocks that differ systematically with treatment exposure. As an additional test of whether the timing pattern of the *hukou* reform is driving the results, we perform permutation tests for the outcome variable that randomly reassign the timing of the *hukou* reform across cities (Lovenheim and Willén 2019). We perform this process in two ways. First, we randomly assign the *hukou* reform dates between 2002 and 2015 across cities. Second, we randomly assign the *hukou* reform dates so that the same percentage of cities adopted the *hukou* reform in each year as shown in Fig. 1. For each approach, we perform the permutations 300 times and calculate the percentage of times the simulated estimate is larger than the actual estimate. Table A2 shows the permutation test results. These results represent p-values of the null hypothesis that any combination of the *hukou* reform dates across cities would generate the same pattern of treatment effects. We reject the null at the conventional level for both approaches. These results suggest that the main estimates are not confounded by the timing pattern of the *hukou* reform.

As discussed before, the *hukou* reform makes it easier for migrants from rural areas to obtain local *hukou*. If the estimation results are driven by the *hukou* reform, then we should not find similar impacts for their counterparts from urban areas. By using the same empirical approach, we estimate the impact of the adoption of the *hukou* reform on urban-to-urban migration inflows. Column 2 of Table 2 reports the results. The coefficients of interest are –0.047(γ_1) and 0.004(γ_2), which are not statistically significant at a conventional level, thus reducing the possibility that the main estimation results are driven by other unobservable city-specific shocks.[44]

In addition, we use the counterfactual migration inflows (from rural areas) from 2000 to 2005 to conduct a placebo analysis. If the main estimates are not driven by other unobservable city-specific shocks, then we should not find any significant impact of the *hukou* reform on these counterfactual migration inflows. By using the same empirical approach, we estimate the impact of the adoption of the *hukou* reform on these counterfactual migration inflows. Column 3 of Table 2 reports the results. The coefficient of interests are 0.007(γ_1) and –0.014(γ_2), which are close to zero and are not statistically significant at a conventional level, further alleviating the concern that the main estimates are driven by unobservable city-specific shocks.

[39] Note that the requirements reform variable only determines on-paper entry requirements of obtaining local *hukou*. Additional exercises suggest that the negative and insignificant coefficient of the requirements reform variable is likely to be driven by the mismeasurement of de facto entry requirements (Table A1 and Fig. A1) rather than that of de facto migration inflows (Column 23 of Table 2 and Fig. A5).

[40] We do not use an event study approach to estimate the dynamic treatment effects because this approach is problematic when heterogeneous treatment effects exist (Sun and Abraham 2020).

[41] As discussed before, a more precise interpretation is as follows: the estimates are the extent to which improved access to *hukou* affects net migration inflows, that is, the number of new arrivals minus the number of migrants who returned to the places of origin or re-migrated to another destination. Thus, the sizable migration effects are attributable to more new arrivals or fewer migrants who returned to the places of origin or re-migrated to another destination after the adoption of the *hukou* reform.

[42] Given that there are relatively few observations used for estimating long-term effects, these effects should be interpreted cautiously. We do not observe an evident impact immediately after the reform adoption probably because we only focus on migrants who did not obtain local *hukou* and prospective migrants take time to respond to the *hukou* reform. Additional evidence suggests that the *hukou* reform increased the number of migrants who obtained local *hukou* immediately after the reform adoption (Fig.s A4c and A4d).

[43] Under the common trends assumption, $\widehat{\gamma_1}$ estimates a weighted sum of 699 average treatment effects on the treated (ATTs). A total of 695 ATTs receive a positive weight, and 4 receive a negative weight. By contrast, $\widehat{\gamma_2}$ estimates a weighted sum of 2,717 ATTs. A total of 1,350 ATTs receive a positive weight, and 1,367 receive a negative weight.

[44] The increased labor demand during development was probably for low-skilled rural workers and not for those skilled from urban areas, and hence had nothing to do with the *hukou* reform. However, this consideration alone would not explain why the treatment effect is significant.

Furthermore, we address the potential concerns that the estimates above are contaminated by other confounding policies in the cities. As mentioned earlier, three such confounding policies exist, namely, the reductions in trade policy uncertainty after China's accession to the WTO, the increases in minimum wages in urban areas, and the abolition of agricultural taxes in rural areas. Column 4 of Table 2 reports our main results. The coefficient of interests are $0.213(\gamma_1)$ and $-0.034(\gamma_2)$. Therefore, the treatment effects are only slightly changed after controlling for all of these policies.

Lastly, we control for the effects of covariates. By interacting these covariates with year dummies, we find that the coefficients of interest are 0.199 (γ_1) and –0.047 (γ_2).[45] We also use propensity score matching to choose markedly comparable subsamples for estimation (Fig. A7).[46] By using the matched sample to estimate the migration impact of improved access to *hukou*, we find that the coefficients of interest are 0.261 (γ_1) and –0.019 (γ_2).[47] These findings indicate that the common parallel assumption holds in the full sample. Overall, our results support the first implication derived in Section 2.

5.3. Heterogeneous Migration Effects

We further explore whether the migration effects of improved access to *hukou* are heterogeneous across groups. We will focus on the treatment effect of the quota reform that is conditional on the requirements reform (i.e., the coefficient of interest is γ_1 hereafter). Columns 5 to 16 of Table 2 report the results. First, we examine whether the migration impact of improved access to *hukou* varies by age. We find that the migration impact decreases with age for the working population. The coefficient of interest is 0.245 for people who are between 15 and 29 years old and 0.186 for people who are between 30 and 44 years old, whereas the coefficient of interest decreases to 0.050 for people who are between 45 and 59 years old and -0.001 for people who are 60 and above. These findings suggest that migration decisions of the younger people are more responsive to improved access to *hukou* than those of their older counterparts. A possible explanation is that younger people are more dependent on local welfare benefits in destinations, such as education for their children. Second, we examine the migration impact of improved access to *hukou* by gender. The coefficients of interest are 0.185 and 0.214 for men and women, respectively, suggesting that migration decisions of both men and women are responsive to improved access to *hukou*. Third, we examine the migration impact of improved access to *hukou* by skill. The coefficient of interest is the largest for individuals with junior high school degrees (0.205), followed by individuals with primary school degree or below (0.129). By contrast, we find smaller migration impacts for individuals with senior high school degrees (0.073) and individuals with college degrees or above (0.007), and they are not statistically significant at the conventional level. A possible explanation is that high-skilled individuals are less sensitive to expected benefits attached to obtaining *hukou* in destinations (e.g., high income enables them to send their children to expensive private schools in destinations or the entry requirements are not binding to them even before the reform).[48] These results are consistent with the second implication derived in Section 2. Finally, we examine the reform impact by migration pattern. We distinguish between migration from rural areas of the same city (intra-city migration) and migration from rural areas of other cities (inter-city migration). The migration impact is more pronounced for intra-city migration (0.175) than inter-city migration (0.054), and the latter is not statistically significant at a conventional level.[49] Thus, migration decisions of rural people in the city are more sensitive to improved access to *hukou*.[50] These results are in line with the third implication derived in Section 2.

Overall, we find evidence that the migration effects of improved access to *hukou* are heterogeneous across groups. The impact is more pronounced for the young and for low- and medium-skilled people. This finding suggests that migration decisions of young and low- and medium-skilled people are more sensitive to improved access to *hukou* in destinations, particularly for migration from the same city.

[45] It is noteworthy that several studies raise concerns about the approach of controlling for covariates linearly in the model (Meyer 1995; Abadie 2005). For example, according to Meyer (1995), introducing covariates in this linear fashion may not be appropriate if the treatment has different effects for different groups in the population. More details on the estimates after controlling for covariates linearly in the model can be found in Column 24 of Table 2 and Fig. A6.

[46] In practice, we define the treatment group as cities that adopted the *hukou* reform in 2003 and 2004, and the control group as cities that adopted the *hukou* reform from 2008 onwards. We investigate how different covariates affect the early adoption of the *hukou* reform using a logit model (Table A3). Note that conducting propensity score matching may drop more similar control groups. Moreover, propensity score matching can generate additional problems because it can only cope with observable variables (King and Nielsen 2019).

[47] Additional details of the estimates using unmatched and matched samples are presented in Columns 21 to 22 of Table 2 and Fig. A8. The fact that estimates based on the (unmatched) subsample are similar to the results using the (unmatched) full sample suggests that the early reformers are representative of all other cities.

[48] An alternative explanation may be that the impact of the *hukou* reform on high-skilled workers' migration responses is underestimated considering that it is much easier for migrants with higher education levels and professional skills to obtain local *hukou*. However, high-skilled migrants can obtain local *hukou* easily even in the absence of the *hukou* reform, making this alternative explanation less likely to be dominant. Fig. A2 shows that the decline in observed migration inflows in the long run is prominent for low- and medium-skilled people but not for high-skilled people, further mitigating this under-estimation concern.

[49] This finding suggests that an increase in migration inflows to cities that have experienced *hukou* reforms does not necessarily imply less migration inflows to cities without *hukou* reforms when intra-city migration dominates the migration effect of the *hukou* reform. This evidence would mitigate the concern that the Stable Unit Treatment Value Assumption is violated in our difference-in-differences setup.

[50] A possible explanation for the insignificant impact of the *hukou* reform on inter-city migration is that inter-city migration involves more costs and concerns (e.g., social networks), and is more complex than intra-city migration, which makes inter-city migration less sensitive to higher expected benefits attached to local citizenship.

5.4. Mechanism Analysis

Why does improved access to *hukou* matter for migration decisions, particularly for young and low- and medium-skilled people? In addition, why does improved access to *hukou* matter more for migration from rural areas of the same city than migration from rural areas of other cities? A likely underlying mechanism is that improved access to *hukou* increases migrants' returns to migration through higher expected benefits attached to local citizenship in destinations, and, in turn, facilitates migration (Sjaastad 1962; Mincer 1978) (i.e., the local citizenship magnet effect). For low-skilled individuals, migrating for a job in urban areas at the expense of separating from their school-aged children left behind in rural areas is not rare for them given the restricted access to public schools in destinations (Zhang et al. 2014; Wang et al. 2019). By contrast, high-skilled individuals are less likely to be affected by such benefits (e.g., high income enables them to send their children to expensive private schools in destinations). Moreover, inter-city migration typically involves more costs of migration, financially and non-financially, than intra-city migration. Our previous findings seem to support the local citizenship magnet effect. In this sub-section, we test whether the increased migration inflows after the reform adoption are driven by migrants' higher expected benefits attached to obtaining local citizenship in destinations or not.

Directly conducting such test is empirically challenging. We resort to a plausibly exogenous positive demand shock induced by reductions in trade policy uncertainty after China's accession to the WTO in 2001[51] and examine how the migration impact of improved access to *hukou* responds to the positive demand shock in destinations. The demand shock in destinations directly improves migrants' job opportunities, particularly in the private sector, and, in turn, increases their expected economic returns of migration, even in the absence of local citizenship. If the increased migration inflows after the adoption of the *hukou* reform are driven by migrants' higher expected benefits attached to obtaining local citizenship in destinations, then a positive demand shock in destinations decreases the migration impact of improved access to *hukou*. This finding is explained by the fact that improved labor market in the private sector, resulting from a positive demand shock, dwarfs the migration incentives associated with the higher benefits attached to obtaining local citizenship in destinations. Otherwise, other factors are attributable to the increased migration inflows after the adoption of the *hukou* reform.

Following the literature on trade liberalization in China, we construct a measure of reductions in trade policy uncertainty about exporting goods from China to the United States or the Normal Trade Relations (NTR) gap at the city level (Pierce and Schott 2016; Handley and Limão 2017; Facchini et al. 2019). Specifically, the NTR gap is defined as the difference between the NTR tariffs reserved to WTO members and applied to China's exports to the US since the early 1980s and the non-NTR rates, which are instead the higher tariff rates applied to nonmarket economies and originally established by the US under the Smoot-Hawley Tariff Act introduced in 1930. The latter would have been applied to China if the US Congress failed to extend the MFN (i.e., most favored nation) status to China in any year before China's entry into the WTO. Precisely, the NTR gap for product i is defined as $NTR\ gap_i = non\ NTR\ rate_i - NTR\ rate_i$. We construct the NTR gap using the data on NTR rates for 2000, the year before China's accession to the WTO. To carry out the empirical analysis, we aggregate the product-level NTR gap measure at the city level using as weights the product shares in the export basket of each city, as observed in 2000. To construct the weights, we use the firm-level export information available in the China Customs Data (also called China Import and Export Data).[52] Specifically, the NTR gap in city s is defined as $NTR\ gap_s = \sum_i \left(\frac{Exp_{is}}{Exp_s} \cdot NTR\ gap_i\right)$, where Exp_{is} are the exports of good i from city s, and Exp_s are the total exports of the city. Cities with a larger NTR gap are supposed to be associated with larger reductions in trade policy uncertainty after China's accession to WTO, which, in turn, lead to larger positive demand shocks on average in destinations.

We compare the migration impact of improved access to *hukou* in destinations exposed to larger positive demand shocks on average with that in destinations exposed to smaller positive demand shocks on average. Specifically, we construct a dummy variable to measure whether or not destinations are exposed to larger demand shocks on average based on the median value of the NTR gap distribution.[53] Columns 17 and 18 of Table 2 report the main results. Holding other factors constant, improved access to *hukou* increases migration inflows by 35.7% on average in destinations exposed to smaller positive demand shocks (below the median value of the NTR gap distribution). By contrast, we find a much smaller migration impact in destinations exposed to larger positive demand shocks (16.7%), which is not statistically significant at the conventional level. To formally test whether the migration impact responds to such a demand shock in destinations, we use the following model specification:

[51] A strand of literature on trade liberalization has shown that reductions in trade policy uncertainty after China's accession to WTO generate a plausibly exogenous labor demand shock for the US and China (Pierce and Schott 2016; Handley and Limão 2017; Facchini et al. 2019). For example, Pierce and Schott (2016) link the sharp drop in US manufacturing employment after 2000 to the reductions in trade policy uncertainty about exporting goods from China to the US that are attributable to China's accession to the WTO. They find that industries more exposed to the change experience greater employment loss, increased imports from China, and risen entry by US firms importing from China and Chinese firms exporting to the US, especially foreign-owned Chinese firms. Facchini et al. (2019) show that Chinese prefectures facing a larger decline in trade policy uncertainty experience more exports from China to the US and larger inflows of migrant workers.

[52] China Custom Data are an annual HS-based transaction-level data compiled by the General Administration of Customs of China. It records information on each import/export transaction, and the variables relevant for our analysis include commodity code (HS 6-digit), partner country, firm type, firm location at the city level, import/export type, transaction value (in USD), and transaction types.

[53] Given that we use reductions in trade policy uncertainties (i.e., NTR gap) to proxy for labor demand exposed to destinations, we construct such a dummy variable to mitigate the concern that certain degree of reductions in trade policy uncertainties are required to generate larger average labor demand in destinations. When using a continuous variable (i.e., NTR gap) for the analysis, the parameter of interest (γ_3) remains negative but becomes statistically insignificant at the conventional level.

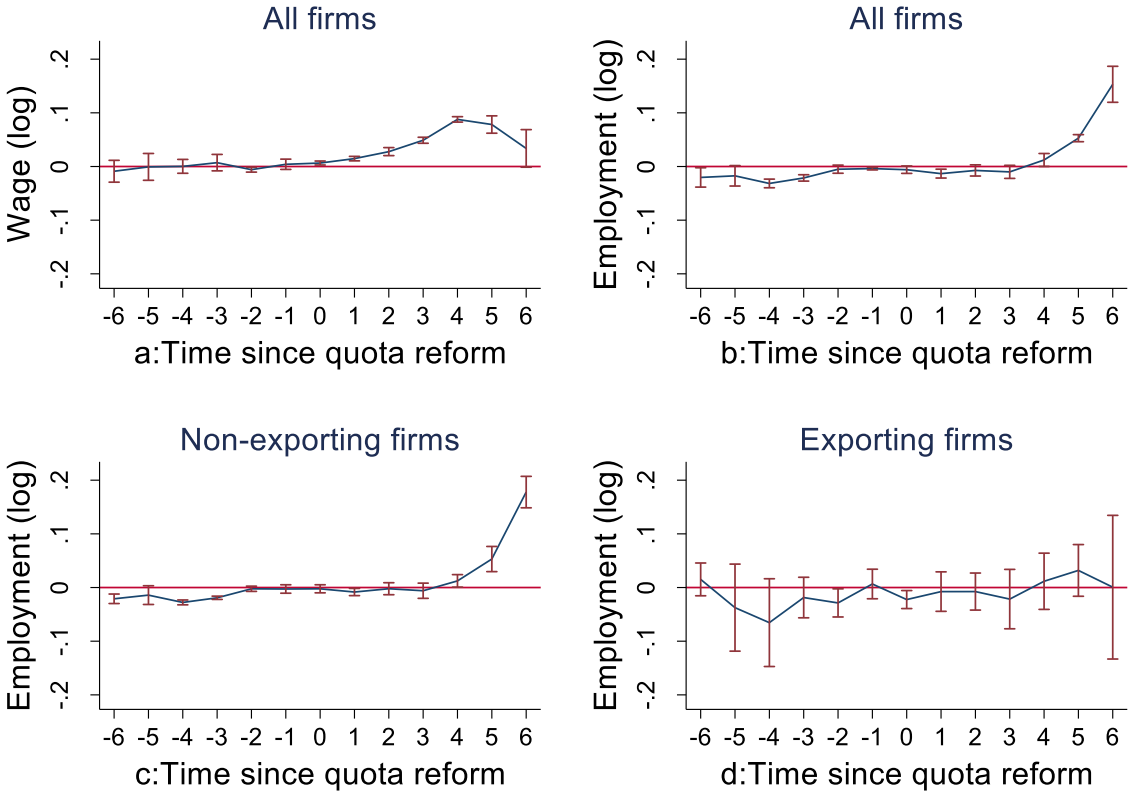

Fig. 4. Effects of improved access to *hukou* on average wage and employment (in log). Notes: The upper-left panel (Fig. 4**a**) shows the impact of the quota reform on the average wage per worker (deflated by CPI) at the firm-level. The upper-right panel (Fig. 4**b**) shows the impact of the quota reform on the total employment for all firms. The lower-left panel (Fig. 4**c**) shows the impact of the quota reform on total employment for non-exporting firms. The lower-right panel (Fig. 4**d**) shows the impact of the quota reform on the total employment for exporting firms. In each panel, the horizontal axis represents the values corresponding to the time relative to the adoption the *hukou* reform, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Outcome variables are in the logarithm form. The panel data on firm-level employment from 2000 to 2008 are constructed using the Chinese Annual Survey of Industrial Firms (2000–2008). We define exporting firms as those firms in which exports accounted for at least 80% of total sales in each year from 2000 to 2008, and define the remaining firms as non-exporting firms. According to the data, exporting firms account for approximately 8.5% of firms in the sample. To draw these figures, we have followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

$$y_{st} = \alpha' \mathrm{I}_s + \beta' \mathrm{I}_t + \gamma_1 \mathrm{I}_{s,t \geq t_{reform}} + \gamma_2 \, Intensity_{st} + \gamma_3 \left(\mathrm{I}_{s,t \geq t_{reform}} \cdot L_s \right) + \varepsilon_{st}, \tag{2}$$

where L_s is a dummy variable for larger positive demand shocks in city s, which is equal to one if NTR gap in city s is above the median value of the NTR gap distribution and equal to zero if NTR gap in city s is below the median value of the NTR gap distribution. Parameter γ_3 captures the difference in the migration impact of improved access to *hukou* between destinations exposed to larger positive demand shocks and destinations exposed to smaller positive demand shocks.

Column 19 of Table 2 reports the main estimates from Eq. (2). The coefficient of interest (γ_3) is -0.208, suggesting that the migration impact of improved access to *hukou* is smaller in destinations exposed to larger positive demand shocks than that in destinations exposed to smaller positive demand shocks.

These findings suggest that increased economic returns of migration through higher benefits attached to obtaining local citizenship in destinations are a likely underlying mechanism through which improved access to *hukou* affects migration decisions, supporting the local citizenship magnet effect.[54]

[54] In addition to the local citizenship magnet effect, improved access to *hukou* may also affect migration decisions through general equilibrium effects. Further discussions can be found Section 5.7.

5.5. A Competing Hypothesis: Wage-induced Migration

Although increased returns to migration through higher expected benefits attached to obtaining local citizenship in destinations are a likely underlying mechanism through which the adoption of the *hukou* reform affects migration, the increased migration inflows after the adoption of the *hukou* reform may be driven by higher wages in reform cities that are independent of or associated with the *hukou* reform itself. We resort to the following approaches to mitigate these concerns. First, we check the changes in migration inflows prior to the reform adoption. If more migration inflows that occurred in reform cities are attributable to higher average wages in destinations, then we should witness more migration inflows in reform cities prior to the reform adoption. However, we do not find such differences in migration inflows between reform and non-reform cities in the pretreatment periods, as shown in Fig. 3a.

Second, we check the changes in average wages prior to the reform adoption. Higher wages in reform cities possibly induced more migration inflows, and the increased supply of migrant workers, in turn, narrowed the wage gap between reform and non-reform cities over time. If this assumption is true, then we should find significant differences in the evolution of the change in average wages between reform and non-reform cities in the pretreatment periods. To test this assumption, we use the new estimator proposed by de Chaisemartin and D'Haultfoeuille (2020) again to show the placebo effects for average wages in the pretreatment periods (as shown in Fig. 4a). However, we find no evidence that significant differences exist in the evolution of the change in average wages between reform and non-reform cities in the pretreatment periods.[55]

Some may continue to argue that the mild increase in average wages shortly after the adoption of the *hukou* reform, shown in Fig. 4a, supports the possibility that the increased migration inflows are driven by higher expected wages. If this assumption is true, then the wage gap between reform and non-reform cities narrows with increased supply of migrant workers over time, which is not supported by empirical results. Specifically, holding other factors constant, the quota reform increases average wages by 2.7% in the second year of the reform adoption, and the reform further increases average wages by 4.9% in the third year of the reform adoption (as shown in Fig. 4a). Some may further argue that these results support the possibility that the increased migration inflows are driven by faster wage growth in reform cities than in non-reform cities. However, this consideration alone could not explain why firms expand substantially while experiencing fast wage growth after the reform adoption (as shown in Fig. 4b) as well as why the migration effect is more pronounced for low-skilled individuals but not for high-skilled individuals. The likely underlying mechanism for the increase in wages and employment after the adoption of the *hukou* reform will be discussed in depth later. Overall, we find no evidence supporting that the increased migration inflows in reform cities are solely driven by higher wages, further strengthening our main argument that improved access to *hukou* substantially affects migration.

5.6. Migration Impact in the Long Term

Although the findings reported thus far indicate that improved access to *hukou* substantially increases migration, particularly for young and for low- and medium-skilled people, another important question is why the migration impact decreases (and even becomes negative) in the long term. At least two different explanations are considered for the falling migration impact observed in Fig. 3a. A seemingly reasonable explanation is that, holding other factors constant, improved access to *hukou* leads to reductions in total wages in destinations owing to relatively more labor supply in the local labor market and thus exerts a negative impact on migration decisions in the long term (i.e., the labor supply effect). However, this explanation is not supported by the mild increase in average wages after the adoption of the *hukou* reform (shown in Fig. 4a).

An alternative explanation is that our constructed migration inflows exclude migrants who obtained local *hukou* and seriously underestimate the de facto migration inflows, particularly in the long term when a larger number of migrants in reform cities successfully obtained local *hukou* after a certain period of waiting. In this case, the falling (observed) migration inflows after the adoption of the *hukou* reform in the long term are caused by migrants who obtained local *hukou* (we call it a *hukou* conversion effect or a sample attrition effect).

An ideal test for the likely underlying explanation would be to examine the impact of the adoption of the *hukou* reform on the number of migrants who obtained local *hukou* over time. The *hukou* conversion effect is more likely to dominate if the adoption of the *hukou* reform increases the number of migrants who obtained local *hukou* in destinations, especially in the long term. By contrast, we should witness fewer migrants who obtained local *hukou* in reform cities in the long term if the labor supply effect dominates. This prediction is justified by the fact that the deteriorating labor market offsets the migration incentives associated with improved access to *hukou* in destinations. Although we cannot directly conduct such an empirical exercise owing to data limitations, we take advantage of the total number of NAH holders registered in each city from 1997 to 2008. Changes in the stock of NAH holders registered in each city are determined by natural population growth, migrants who obtained local *hukou* in the destination city, and emigrants who obtained local *hukou* in other cities. We assume that China's mandatory one-child policy keeps the natural population growth stable. Thus, the impact of the adoption of the *hukou* reform on the total number of NAH holders registered in each city captures the impact of adopting the *hukou* reform on the (net) migrants who obtained local *hukou*.[56]

By using the similar empirical approach shown in Equation (1), we find that the quota reform increases the number of NAH holders

[55] The reform cities refer to cities that adopt the quota reform.

[56] An important assumption here is that the number of emigrants who obtained local *hukou* in other cities did not change after the *hukou* reform. This assumption is likely to be true given the evidence we provided that the *hukou* reform affected intra-city migration, rather than inter-city migration, and that the *hukou* reform did not affect urban-to-urban migration inflows.

registered in each city. Moreover, the requirements reform variable is positively associated with the number of NAH holders registered in each city, suggesting that the requirements reform enables more migrants to obtain local *hukou* in destinations.

We further explore the dynamic impacts of the quota reform on the number of NAH holders, and Fig. 3c reports the main results. The number of NAH holders in reform cities increases slightly in the first four years after the adoption of the *hukou* reform and then increases substantially, particularly in the sixth year after the reform adoption. Specifically, the average number of NAH holders increases by approximately 9.7% in the fourth year of the reform adoption, and the impact increases to 32.9% in the sixth year of the reform adoption. The opposing changes in the number of NAH holders and the number of observed migration inflows after the adoption of the *hukou* reform in the long term support the *hukou* conversion effect, that is, the falling observed migration inflows in the long term are driven by migrants who obtained local *hukou*. In addition, the requirements reform increases the number of NAH holders over time (shown in Fig. 3d). These findings suggest that migrants take a certain period of time to obtain local *hukou*, and the substantial increase in the number of migrants who obtained local *hukou* in reform cities is a likely underlying explanation for the falling impact of the adoption of the *hukou* reform on observed migration inflows in the long term.

Finally, to further support that the falling migration impact is caused by the *hukou* conversion effect rather than other unobservable factors (e.g., measurement error), we compare the dynamic migration impacts of the quota reform across skill groups. If the *hukou* conversion effect exists, we should witness that the falling migration impact is more pronounced for low- and medium-skilled people who are sensitive to the *hukou* reform. Results are shown in Fig. A2. We indeed find that the decline in observed migration inflows in the long run is prominent for low- and medium-skilled people. However, we do not find similar patterns for high-skilled people, mitigating the concern that the surprising dynamics (switching from positive to negative over time) is caused by other unobservable factors.

5.7. Labor Market Consequences in Destinations

To examine the extent and how improved access to *hukou* affect the local labor market in destinations (i.e., general equilibrium effects), we use firm-level panel data from the *Chinese Annual Survey on Industrial Firms* from 2000 to 2008. Again, we use the new estimator proposed by de Chaisemartin and D'Haultfoeuille (2020) to explore the dynamic impacts of the quota reform on average wages and employment, conditional on the requirements reform. We find that improved access to *hukou* increases the average wages by 4.2 percent,[57] and the wage effect is heterogeneous over time. Improved access to *hukou* slowly increases average wages at the firm level in the first four years after the adoption of the *hukou* reform. Then, the positive wage effects decrease substantially, particularly in the sixth year after the reform adoption. Specifically, the average wages increase by 2.7% two years after the adoption of the *hukou* reform, and the wage effect slowly increases to 9% in the fourth year of the reform adoption. Afterward, however, the wage effect decreases gradually to 3.3% in the sixth year of the adoption of the *hukou* reform (Fig. 4a). A possible explanation for the mild increase in average wages after the adoption of the *hukou* reform is that improved access to *hukou* induces more domestic consumption through increased migrants who (are prepared to) obtain local *hukou* in destinations[58] as shown in Fig.s 3c and 3d, and, in turn, leads to more labor demand in the local labor market and the resulting higher wages paid by employers as suggested by the labor demand effect (Borjas 2013; Beerli et al. 2021).[59]

Moreover, improved access to *hukou* increases total employment at the firm level by 2.5 percent on average (Fig. 4b).[60] In contrast

[57] To mitigate the concern that the effect is contaminated by outliers, we drop the observations below the 1st percentile and the observations above the 99th percentile for robustness checks, and we find that the average effect decreases slightly from 4.2% to 3.7% (Fig. 5a). Our results echo the findings from a recent study. Beerli et al. (2021) empirically showed that the abolition of immigration restrictions in Switzerland increased wages of highly educated natives owing to a simultaneous increase in labor demand. They find robust evidence that the reform increased wages of highly educated native workers by around 5%.

[58] According to Chen, Lu, and Zhong (2015), the consumption of migrants without obtaining local *hukou* is 16% to 20% lower than that of local *hukou* holders in destinations possibly because migrants save more for precautionary purposes, expect lower permanent income, and consume much less durable goods owing to high mobility. Apart from the numerous studies in developed countries showing that migrants who intend to stay longer in destinations are consuming more and remitting less to the places of origin than otherwise (Dustmann 1997; Dustmann and Mestres 2010a; 2010b; Dustmann, Fasani, and Speciale 2017), a few recent studies in the context of China indirectly support the argument that migrants who (are prepared to) obtain local *hukou* in destinations are likely to consume more than otherwise. For example, Zhou and Jin (2017) show that a settlement intention in the next five years or longer increases migrant households' monthly per capita consumption in destinations by approximately 13.9%. The overall increases in consumption brought about by migrants who intend to stay longer in destinations could be higher when considerng the actual migration durations as well as the size of migrant households in destinations (Meng, Xue, and Xue 2016; Wang et al. 2019). According to Meng, Xue, and Xue (2016), for every additional child left behind, migrant households in the destination will reduce their per capita consumption by 5%. Similarly, for migrants who are married and have left their spouses behind, their household per capita consumption will be reduced by 12.2%.

[59] Borjas (2013) theoretically shows that the wage effect of immigration depends on the relative impact on the potential size of the consumer base to the impact on the size of the workforce. The wage effect could be positive if immigration dramatically increases the size of the consumer base for the domestic product.

[60] Firm-level studies examining these links are still rare, focus mostly on the United States (Kerr and Lincoln 2010; Ghosh, Mayda, and Ortega 2014). For example, Kerr and Lincoln (2010) show that a 10% growth in the national H-1B population corresponded with a 0.3%-0.6% higher growth in the total science and engineering employment for each standard deviation increase in state dependency. By contrast, our findings show that a migration reform that increases migration inflows by 22.5% to 40.9%, depending on the measure of migration inflows used, leads to an increase in total employment by 2.5% on average.

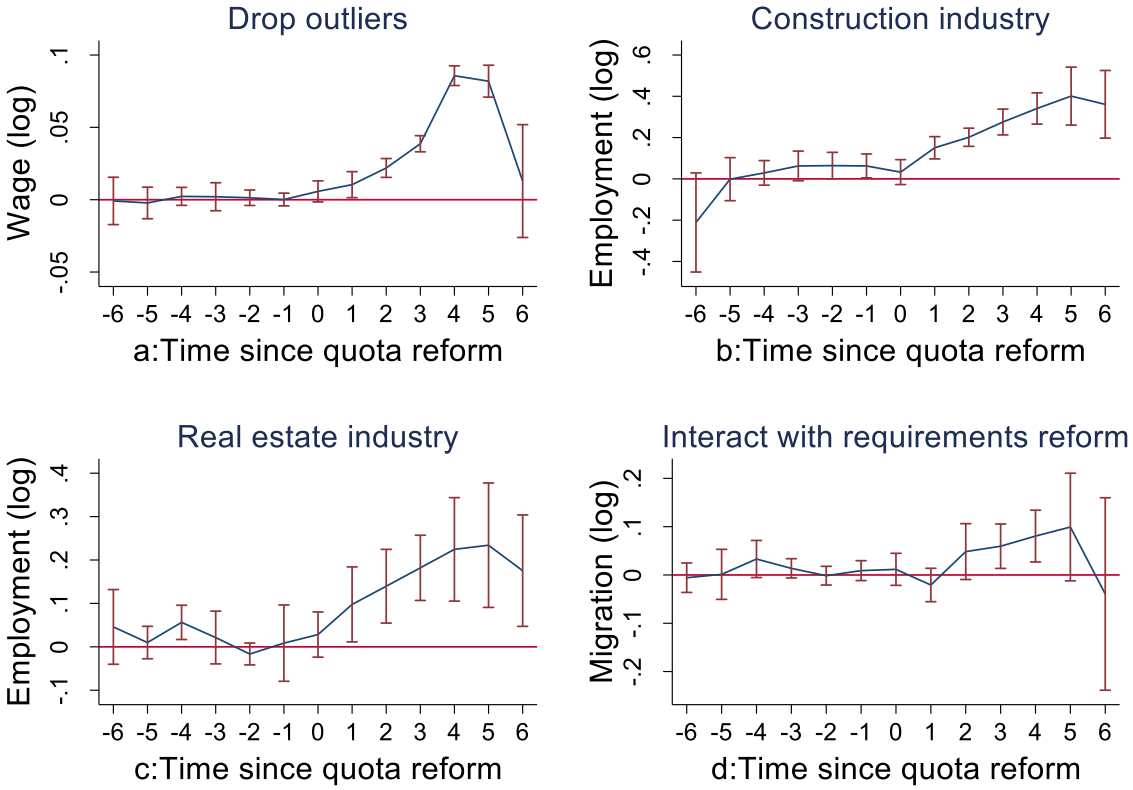

Fig. 5. Effects of improved access to *hukou* on wage, employment, and migration outcomes. Notes: The upper-left panel (**Fig. 5a**) shows the impact of the quota reform on the average wage per worker (deflated by CPI) at the firm-level by dropping observations below the 1st percentile and observations above the 99th percentile of the wage distribution. The upper-right panel (**Fig. 5b**) shows the impact of the quota reform on employment of construction industry. The lower-left panel (**Fig. 5c**) shows the impact of the quota reform on employment of real estate industry. The lower-right panel (**Fig. 5d**) shows how requirements reform affects migration impact of the quota reform or γ_4 in equation (3). In each panel, the horizontal axis represents the values corresponding to the time relative to the adoption the *hukou* reform, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. All of the outcome variables are in the logarithm form. The panel data on migration inflows from 1999 to 2009 are constructed using the 2005 and 2010 censuses. The panel data on employment of construction industry and real estate industry from 1997 to 2008 are constructed using the China City Statistical Yearbook (1998–2009). To draw these figures, we have followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

to the wage effect that increases significantly in the short term but declines in the long term, the employment effect is close to zero and scarcely changed in the short term but increases substantially in the long term; this finding suggests that local firms take time to fully respond to the increasing labor demand possibly induced by increased total consumption from migrants who (are prepared to) obtain local *hukou* in destinations[61] and to expand accordingly. The declining wage effect in the long term also reflects the increased labor supply through firm expansion. All of this firm-level evidence suggests that improved access to *hukou* does not adversely affect wages and employment in destinations.

As an additional test to support the likely underlying mechanism through which improved access to *hukou* positively affects the expansion of firms in destinations, we examine the employment effect by firms' exporting status. Non-exporting firms are more likely to be affected by improved access to *hukou* than their exporting counterparts, if the expansion of firms is largely driven by the increased domestic consumption from migrants who (are prepared to) obtain local *hukou* in destinations rather than the reduced labor costs owing to more supply of migrant workers as suggested by Imbert et al. (2022). Otherwise, other factors would be governing the changes in wages and employment after the adoption of the *hukou* reform. By distinguishing between local exporting and non-exporting firms, we find that the employment effect of the adoption of the *hukou* reform is largely induced by local non-exporting firms (Fig. 4c). The insignificant employment effect for local exporting firms (shown in Fig. 4d) suggests that the increased domestic

[61] Both higher migrant households' per capita consumption and increased size of migrant households in destinations contribute to the increased total consumption from migrants.

consumption from migrants who (are prepared to) obtain local *hukou* in destinations rather than the reduced labor costs is a likely underlying mechanism through which improved access to *hukou* affects the expansion of firms in destinations.[62]

To further support the idea that increased labor demand is a likely underlying mechanism through which improved access to *hukou* positively affects average wages in destinations, we investigate the impact of such an improved access on labor demand associated with booming housing markets in destinations. Given that some basic entry conditions, such as stable accommodation, are required to obtain local *hukou* after the *hukou* reform, we expect that this reform will positively affect the housing market in destinations, thereby likely creating additional labor demand in the corresponding industries and jobs (e.g., construction, building material manufacturing, and housing services). We use the same empirical approach and find that improved access to *hukou* substantially increases employment in the construction and real estate industries (Fig.s 5b and 5c).

Overall, we show that improved access to *hukou* positively affects the local labor market in the long term possibly owing to increased domestic consumption, such as housing purchases from migrants who (are prepared to) obtain local *hukou* in destinations,[63] suggesting that the labor demand effect dominates in the general equilibrium effects. These results are consistent with the fourth implication derived in Section 2.

5.8. Do Lower Entry Requirements Facilitate More Migration Inflows?

Our previous analysis indicates no direct evidence that the requirements reform significantly facilitates migration, ceteris paribus. Nevertheless, the requirements reform may still indirectly facilitate migration. For example, the quota reform would be meaningless if on-paper entry requirements would be so stringent that almost no one would be able to meet them. This sub-section further tests whether or not the migration impact of the quota reform depends on the requirements reform. The abolition or relaxation of the quota system may be markedly effective for cities with low on-paper entry requirements. We test this hypothesis using the following model specification:

$$y_{st} = \alpha' \mathrm{I}_s + \beta' \mathrm{I}_t + \gamma_1 \mathrm{I}_{s,t \geq t_{reform}} + \gamma_2 \, Intensity_{st} + \gamma_4 \left(\mathrm{I}_{s,t \geq t_{reform}} \cdot Intensity_{st} \right) + \varepsilon_{st}, \tag{3}$$

where γ_4 is the coefficient of interest, which captures the extent to which the requirements reform affects the migration impact of the quota reform. Other variables are the same as those in equation (1).

We start by estimating the two-way fixed effects model in equation (3), and find that the coefficient of interest (γ_4) is 0.03, which is not statistically significant at a conventional level (Column 20 of Table 2). The negative weights problem is obvious for γ_4 in the data, suggesting that γ_4 does not reflect the actual treatment effects.[64] Accordingly, we resort to the new estimator proposed by de Chaisemartin and D'Haultfoeuille (2020). Fig. 5d shows the dynamic impacts of the interaction term. We find an increasing positive and significant impact of the interaction term until the sixth year after reform adoption. Specifically, holding other factors constant, an increase in the value of the requirements reform variable by one-unit leads to approximately 10.4% more migration inflows induced by the quota reform in the fifth year of reform adoption. The falling impact in the long term may be caused by migrants who obtained local *hukou* as discussed earlier. Therefore, these findings confirm that the requirements reform does facilitate more migration through the increased migration impact of the quota reform.

5.9. Robustness Checks

We conduct several robustness checks. Table 3 reports the main results. First, in our analyses above, we use the requirements reform variable from Fan (2019) to capture the change in on-paper entry requirements of obtaining local *hukou* across reform cities over time. That said, entry requirements of obtaining local *hukou* are multifaceted, and some of them are qualitative and difficult to capture (e.g., unspoken rules that are known to local officials who make *hukou* granting decisions but not written on paper). To alleviate the concern that the requirements reform variable may not fully capture the de facto change in entry requirements of obtaining local *hukou*, we conduct the following robustness checks by focusing on the quota reform only to estimate the migration impact of improved access to *hukou*. In this case, we ignore any change in entry requirements of obtaining local *hukou* across reform cities induced by the adoption of the *hukou* reform. Columns 1 to 18 of Table 3 report our main results. We find that our main results remain robust.

Second, we use three alternative measures of migration inflows to alleviate the concern that the benchmark measure of migration

[62] Additional evidence suggests that the employment effect is markedly pronounced for non-state-owned and labor-intensive firms. Additional details are provided in Fig. A3.

[63] Note that there are some main concerns of reducing migration restrictions in big cities, such as insufficient resources to support a rapidly growing migrant population, and the public support needed in the long run when migrants lose their land in rural areas but are considerably old to work in labor-intensive jobs. In our context, migrants are still required to satisfy some basic entry requirements, such as stable accommodation and jobs, to obtain local *hukou* after the reform, thereby relatively mitigating this concern. Accordingly, we should remember these potential concerns when interpreting the positive labor market effects of the *hukou* reform.

[64] Under the common trends assumption, $\hat{\gamma_4}$ estimates a weighted sum of 699 average treatment effects on the treated (ATTs). A total of 357 ATTs receive a positive weight, and 342 receive a negative weight.

Table 3
Impact of improved access to *hukou* (homogeneous entry requirements)

Variables	(1) Benchmark	(2) Alternative sample of migrants	(3)	(4) Benchmark	(5) Subsample by age of migrants			
	Rural migration	Urban	Counterfactual	Other policies	Ages 15–29	Ages 30–44	Ages 45–59	Ages 60+
$I_{s,t\geq t_{reform}}$	0.203***	−0.044	−0.006	0.188***	0.227***	0.181**	0.056	−0.010
	(0.068)	(0.052)	(0.066)	(0.068)	(0.073)	(0.071)	(0.061)	(0.058)
Observations	3065	3041	1634	2619	2863	2860	2598	2284
Variables	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Subsample by gender of migrants		Subsample by skill of migrants				Subsample by migration distance	
	Male	Female	Low	Medium-low	Medium-high	High	Intra-city	Inter-city
$I_{s,t\geq t_{reform}}$	0.170**	0.199***	0.120*	0.195***	0.056	−0.005	0.170**	0.025
	(0.075)	(0.069)	(0.069)	(0.073)	(0.070)	(0.095)	(0.066)	(0.061)
Observations	2881	2915	2783	2902	2561	1828	2881	2745
Variables	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
	Subsample by demand shocks		Alternative definition of migration variable			Subsample by dropping certain cities or provinces		
	Smaller demand	Larger demand	Migration 1	Migration 2	Migration 3	Shenzhen& Guangzhou	Ten largest cities	Coastal provinces
$I_{s,t\geq t_{reform}}$	0.282***	0.142	0.343**	0.229**	0.225***	0.191***	0.199***	0.183**
	(0.104)	(0.095)	(0.136)	(0.091)	(0.069)	(0.068)	(0.070)	(0.081)
Observations	1478	1489	841	1097	2467	3043	2955	2264

Notes: This table shows the effects of improved access to *hukou* on migration decisions by considering homogeneous exposure to the *hukou* reform. Column 1 shows the impact of improved access to *hukou* on migration inflows from rural areas. Column 2 shows the impact of improved access to *hukou* on migration inflows from urban areas. Column 3 shows the impact of improved access to *hukou* on counterfactual migration inflows from rural areas (1995–2000). Column 4 shows the impact of improved access to *hukou* on migration inflows from rural areas by controlling for three confounding polices, namely, reductions in trade policy uncertainty after China's accession to the WTO, increases in minimum wages in urban areas, and abolition of agricultural taxes in rural areas. Columns 5 to 8 show the heterogeneous effects of improved access to *hukou* on migration decisions by age. Columns 9 to 10 show the heterogeneous effects of improved access to *hukou* on migration decisions by gender. Columns 11 to 14 show the heterogeneous effects of improved access to *hukou* on migration decisions by skill. Columns 15 to 16 show the heterogeneous effects of improved access to *hukou* on migration decisions by migration distance. Columns 17 to 18 show the heterogeneous effects of improved access to *hukou* on migration decisions by size of exposed positive demand shocks. Columns 19 to 21 use net migration inflows over the past five years (in log), migration inflows in the survey year (in log), and a combination of migrants who obtained local *hukou* and migrants who did not obtain local *hukou* as alternative outcome variables, respectively. Columns 22 to 24 provide additional results by excluding mega cities such as Shenzhen and Guangzhou (Column 22), excluding the 10 largest cities in the sample (Column 23), and excluding coastal provinces in the sample (Column 24), respectively. Standard errors are clustered at the city level and are in parentheses.

* $p < 0.1$,
** $p < 0.05$,
*** $p < 0.01$.

Table A1
Quota restrictions and the impact of on-paper entry requirements on migration

Variables	(1) Full sample	(2) Subsample No quota reform	(3) Quota reform
On-paper entry requirements	-0.028	-0.047*	0.120
	(0.024)	(0.027)	(0.095)
Observations	3065	2366	699

Note: This table shows the relationship between quota restrictions and the impact of on-paper entry requirements on migration. Column 1 reports the impact of reductions in on-paper entry requirements on migration using full sample. Column 2 reports the impact of reductions in on-paper entry requirements on migration using cities that did not adopt the quota reform. Column 3 reports the impact of reductions in on-paper entry requirements on migration using cities that adopted the quota reform. Standard errors in parentheses;
** $p<0.05$, *** $p<0.01$.
* $p<0.1$,

Table A2
p-values of permutation tests

	Migration inflows (in log)
Panel A. Randomly assigning the *hukou* reform dates	
Percent larger than baseline	0.000
Panel B: Randomly assigning the *hukou* reform dates to match de facto distribution of reform dates	
Percent larger than baseline	0.000

Notes: The table shows the proportion of times the estimates from the permutation tests are larger than the baseline estimate. In Panel A, we randomly assign the *hukou* reform dates between 2002 and 2015 across cities. In panel B, we randomly assign the *hukou* reform dates so that the same percentage of cities adopted the hukou reform in each year as shown in Fig. 1. For each approach, we perform the permutations 300 times and calculate the percentage of times that the simulated estimate is larger than the actual estimate. These results represent p-values of the null hypothesis that any combination of the hukou reform dates across cities would generate the same pattern of treatment effects.

Table A3
Determinants of early reform adoption

	(1)
State sector ratio	-0.305*
	(0.167)
Per capita GDP (log)	-0.057
	(0.056)
Urban population (10,000)	0.000
	(0.000)
Coastal provinces	0.036
	(0.089)
NTR gap	0.878**
	(0.380)
Observations	219

Notes: This table reports estimates on the economic drivers of early reform adoption using a logit model. Early reformers are defined as those cities that adopted the *hukou* reform in 2003 and 2004, which accounts for 27.2% of all cities in the data. Non-reformers are defined as those cities that adopted the *hukou* reform in 2008 and afterwards, which accounts for 61.1% of all cities in the data. *State sector ratio* is defined as the proportion of state-owned firms' output relative to the entire manufacturing industry in each city, which is used to proxy for the size of state sector. *Coastal provinces* consist of Jiangsu, Zhejiang, Guangdong, Fujian, Shandong, and Hainan. *NTR gap* measures the reductions in trade policy uncertainty faced by Chinese exporters to the U.S. after China's accession to WTO in each city, which is used to proxy for labor demand. Coefficients refer to the average marginal effects. We use China City Statistical Yearbook (2002), Population Census (2000), Chinese Annual Survey of Industrial Firms, and China Customs Data to construct these variables. Standard errors in parentheses;
***p<0.01.
* p<0.10,
** p<0.05,

inflows underestimates the de facto migration inflows. The first measure refers to actual migration inflows that occurred in the survey year, which capture recently arrived migrants who are much less likely to obtain local *hukou* or move to another destination. We construct a panel data set as used in the main analysis for three waves using the census data from 2000, 2005, and 2010 (this information is unavailable in the census 1990). The second measure refers to (net) inter-provincial migration inflows that occurred over the past five years that also allows for migrants who obtained local *hukou* in destinations. Similarly, we construct a panel data set of four waves using census data from 1990, 2000, 2005, and 2010. The third measure is constructed by adding migrants who did not obtain local *hukou*, as used in the benchmark model, and migrants who obtained local *hukou* using annual data from 1999 to 2008. Note that migrants who obtained local *hukou* can be either existing migrants, who have stayed in destinations for several years without obtaining local *hukou,* or new arrivals. Columns 19 and 21 of Table 3 report the main results. Without considering the requirements reform variable, we find that improved access to *hukou* increases the average migration inflows by 40.9% (first measure, Column 19),

25.7% (second measure, Column 20), and 25.3% (third measure, Column 21), depending on the chosen measure of the migration variable, which is also larger than that in the benchmark (22.5%, as shown in Column 1).

Third, we provide additional results by excluding mega cities, such as Shenzhen and Guangzhou (Column 22 of Table 3), excluding the 10 largest cities in the sample (Column 23 of Table 3), and excluding coastal provinces in the sample (Column 24 of Table 3). We find that our main results remain robust.

6. Conclusion

This study is the first attempt to examine empirically the economic effects of lifting barriers to local citizenship in a developing country. Using a quasi-experimental *hukou* reform across 283 Chinese cities, we estimate the migration impact of improved access to local citizenship (*hukou*) in a DID framework. The most important lesson of this study is that improved access to *hukou* in destinations substantially increases migration from rural areas, particularly for young and low- and medium-skilled people. These findings demonstrate the importance of lifting barriers to local citizenship for economic integration in China, which sheds light on the consequences of barriers to local citizenship in other developing countries. Furthermore, the findings complement previous studies that gauge the effectiveness of lifting barriers to citizenship for economic integration in developed countries.

Another important result of this study is that we find no evidence that improved access to *hukou* would worsen labor market outcomes in destinations. In fact, improved access to *hukou* contributes to the expansion of firms in destinations, together with a mild increase in average wages. The additional evidence that the expansion of firms is largely driven by non-exporting firms rather than exporting firms suggests that the higher domestic consumption from migrants who (are prepared to) obtain local *hukou* in destinations is a likely mechanism through which improved access to *hukou* positively affects local labor market outcomes in the long term. Thus, the local economy benefits from the public spending of improving migrants' access to *hukou* possibly through increased domestic consumption from migrants who (are prepared to) obtain local *hukou* in destinations.

Overall, our findings show that improved access to *hukou* in destinations has contributed to the unprecedented internal migration from rural China in the 2000s. Moreover, this article suggests that lifting barriers to local citizenship for migrants in destinations would positively affect the local labor market possibly by motivating these migrants to stay longer and consume more in destinations. Boosting economic integration through granting local citizenship to internal migrants is particularly important for maintaining economic growth in countries such as China which have been attempting to step away from export-led growth to a consumption-driven growth path in recent years.

Declaration of Competing Interest

None.

Appendix A

A1 Definition of migration

As mentioned in Section 3.1, we define migrants in city *s* at year *t* (note that rural and urban areas exist in a city) as individuals who were AH holders and moved away from their registered rural *hukou* locations in China and arrived at the urban area of city *s* at year *t*. In contrast to the birth place of registration, the census data contain inadequate information to infer unambiguously whether a migrant is currently residing in a rural or urban area. The definition of urban and rural areas is largely based on administrative division, which may change over time. Compared with the definition in the 2005 Chinese Population Census, the urban definition in the 2010 Chinese Population Census removed the criterion of population density for a district to be defined as an urbanized area. Thus, some previous rural areas in the 2005 Chinese Population Census were classified as urban areas in the 2010 Chinese Population Census. The census data contain inadequate information to treat the definition of urban and rural areas consistently over time. Given that rural-to-rural migration represents a small share of emigration from rural areas, we do not attempt to distinguish between rural-to-rural migration and rural-to-urban migration and consider all of them as rural-to-urban migration in the main analysis. Thus, we consider all individuals who were AH holders and moved away from their registered rural *hukou* locations in China and resided in city *s* as rural-to-urban migrants in city *s*.

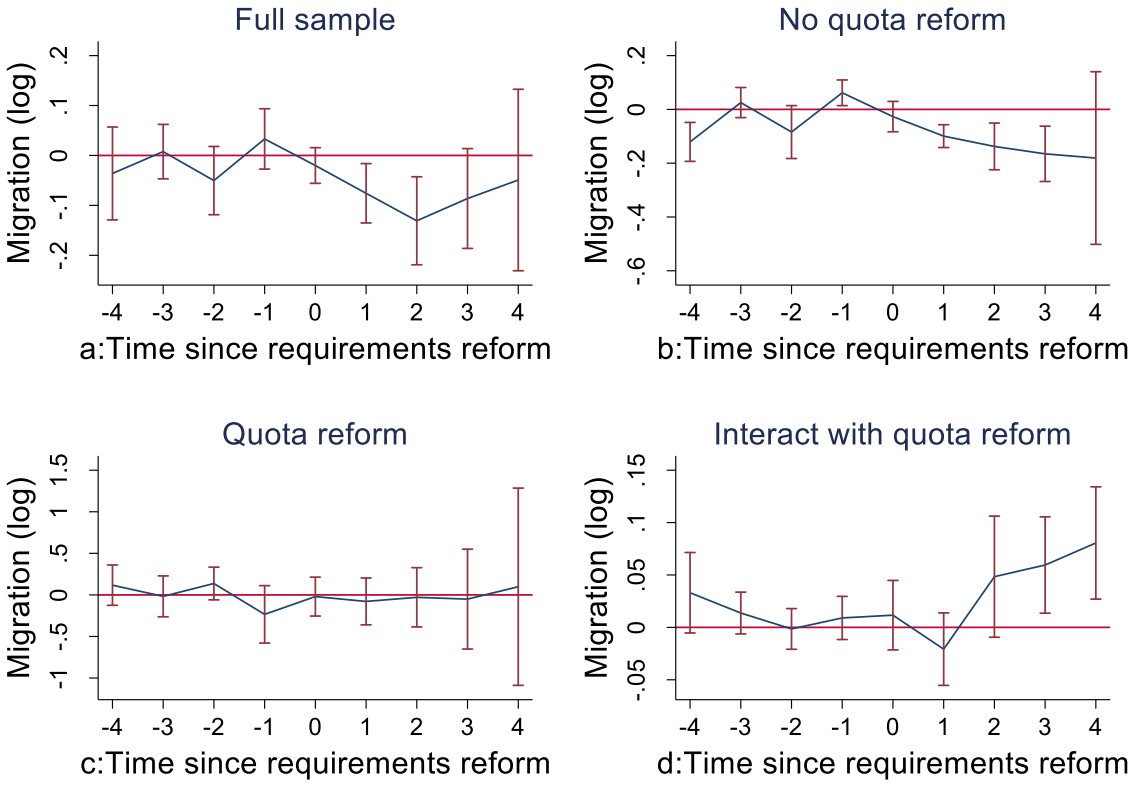

Fig. A1. Quota restrictions and the impact of requirements reform on migration. Notes: The upper-left panel (Fig. A1**a**) shows the impact of the requirements reform on migration inflows (in log) using the full sample. The upper-right panel (Fig. A1**b**) shows the impact of the requirements reform on migration inflows (in log) using cities that did not adopt the quota reform. The lower-left panel (Fig. A1**c**) shows the impact of the requirements reform on migration inflows (in log) using cities that adopted the quota reform. The lower-right panel (Fig. A1**d**) further considers the interaction term between the quota reform and the requirements reform and shows how the quota reform affects the effectiveness of the requirements reform in affecting migration inflows (in log). In each panel, the horizontal axis represents the values corresponding to the time relative to the adoption the *hukou* reform, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. The panel data on migration inflows from 1999 to 2009 are constructed using the 2005 and 2010 censuses. To draw these figures, we have followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

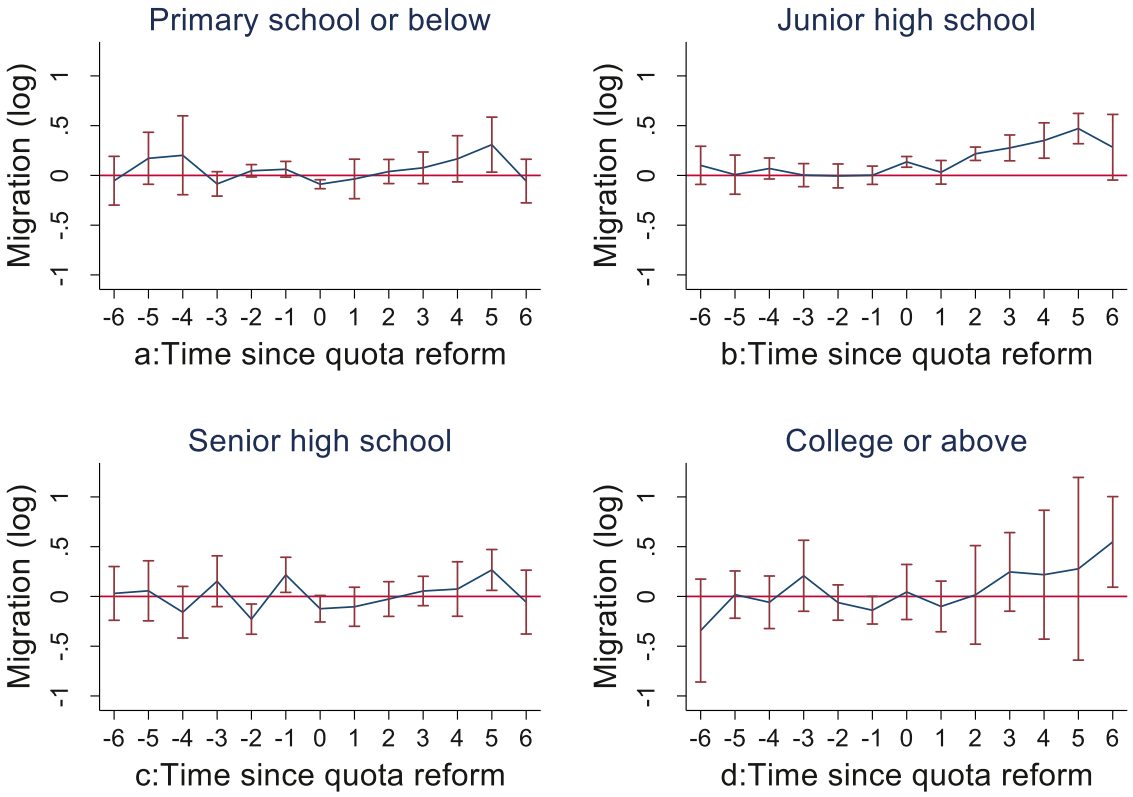

Fig. A2. Heterogeneous effects of improved access to *hukou* on migration by skill. Notes: The upper-left panel (Fig. A2**a**) shows the impact of the quota reform on the number of (net) migration inflows for individuals with primary school degree or below. The upper-right panel (Fig. A2**b**) shows the impact of the quota reform on the number of (net) migration inflows for individuals with junior high school degrees. The lower-left panel (Fig. A2**c**) shows the impact of the quota reform on the number of (net) migration inflows for individuals with senior high school degrees. The lower-right panel (Fig. A2**d**) shows the impact of the quota reform on the number of (net) migration inflows for individuals with college degrees or above. In each panel, the horizontal axis represents the values corresponding to the time relative to the adoption the *hukou* reform, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. All of the outcome variables are in the logarithm form. The panel data on migration inflows from 1999 to 2009 are constructed using the 2005 and 2010 censuses. To draw these figures, we have followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

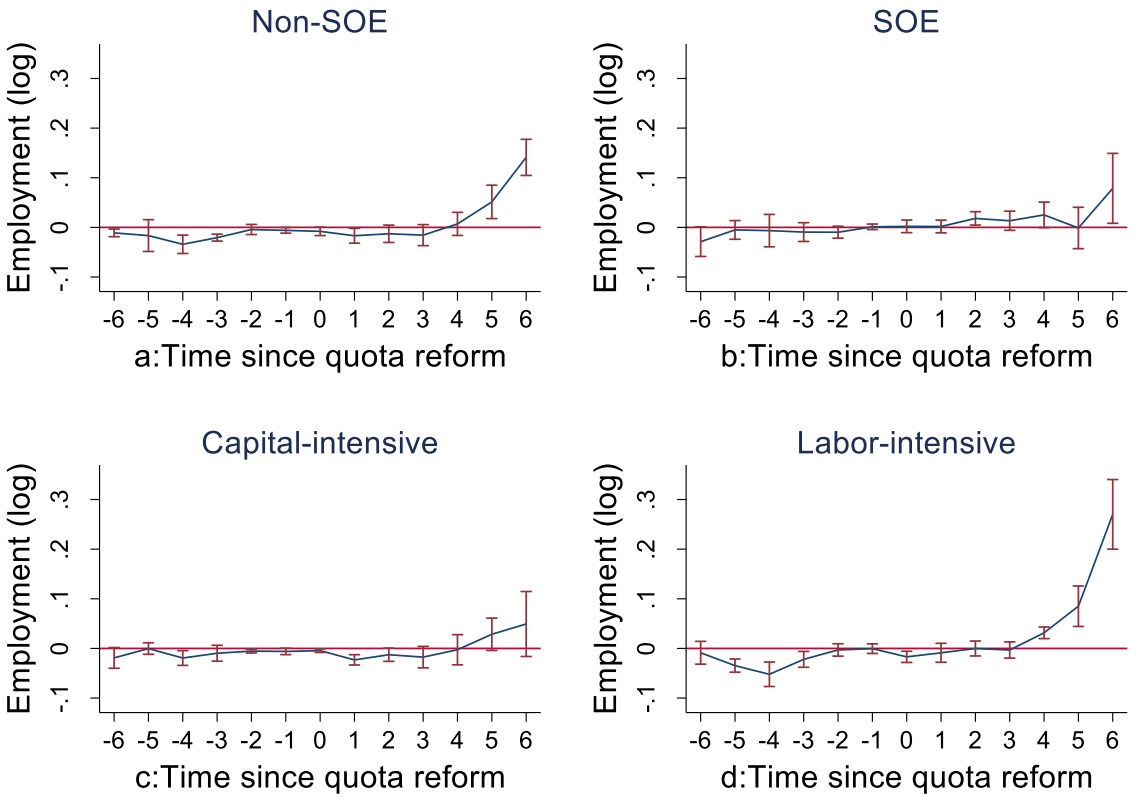

Fig. A3. Heterogeneous effects of improved access to *hukou* on employment by firm ownership and capital intensity. Notes: The upper-left panel (Fig. A3**a**) shows the impact of the quota reform on the total employment for non-state-owned enterprises (non-SOEs). The upper-right panel (Fig. A3**b**) shows the impact of the quota reform on the total employment for state-owned enterprises (SOEs). The lower-left panel (Fig. A3**c**) shows the impact of the quota reform on total employment for capital-intensive firms. The lower-right panel (Fig. A3**d**) shows the impact of the quota reform on the total employment for labor-intensive firms. In each panel, the horizontal axis represents the values corresponding to the time relative to the adoption the *hukou* reform, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pre-treatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. Outcome variables are in the logarithm form. The panel data on firm-level employment from 2000 to 2008 are constructed using the Chinese Annual Survey of Industrial Firms (2000–2008). We define non-SOEs as those non-state-owned firms that did not change their firm ownership from 2000 to 2008, and define the remaining firms as SOEs. We define capital-intensive firms as those firms in which the average capital-labor ratio of the corresponding industry is above the median value of the capital-labor ratio distribution at the industrial level, and define the remaining firms as labor-intensive firms. To draw these figures, we have followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

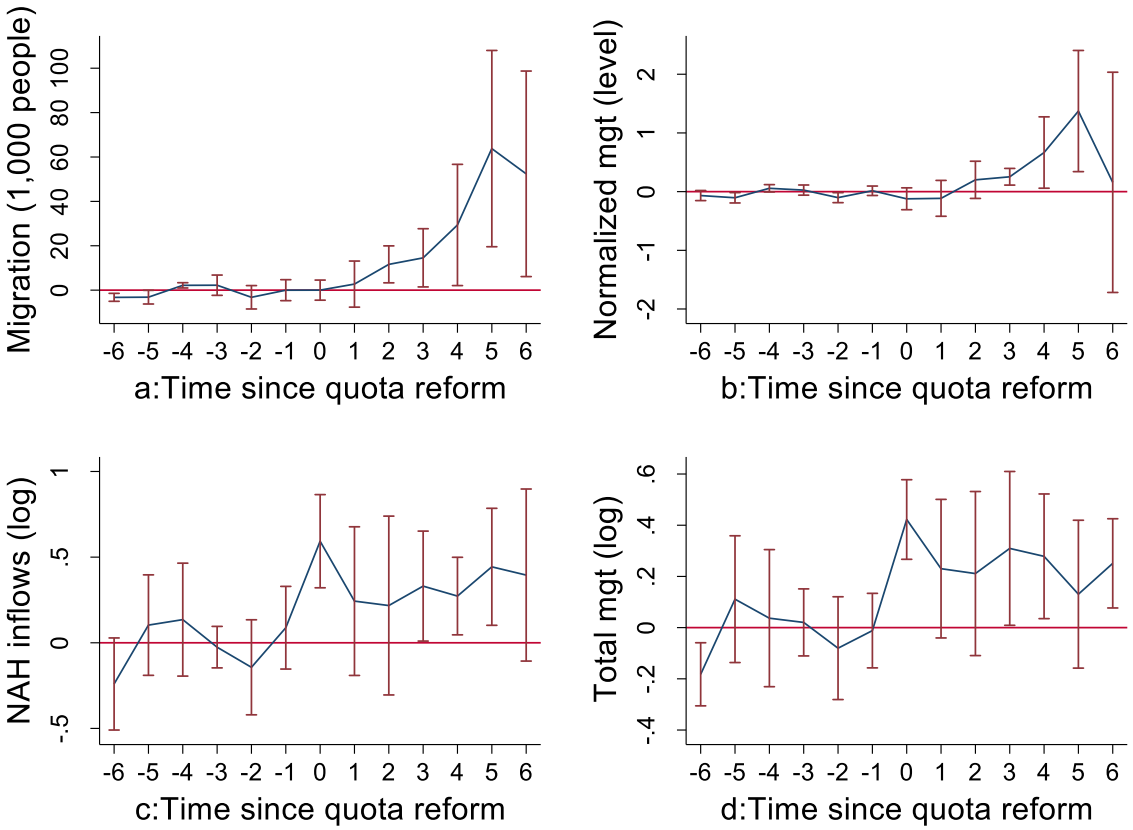

Fig. A4. Effects of improved access to *hukou* on alternative measures of migration. Notes: The upper-left panel (Fig. A4**a**) shows the impact of the quota reform on the migration inflows (in level, 1,000 people). The upper-right panel (Fig. A4**b**) shows the impact of the quota reform on the migration inflows (in level) normalized by total population of the city. The lower-left panel (Fig. A4**c**) shows the impact of the quota reform on the number of NAH inflows (in log). The lower-right panel (Fig. A4**d**) shows the impact of the quota reform on the number of total migration inflows (in log), by adding migration inflows and NAH inflows together. In each panel, the horizontal axis represents the values corresponding to the time relative to the adoption the *hukou* reform, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. The panel data on migration inflows from 1999 to 2009 are constructed using the 2005 and 2010 censuses. The panel data on NAH holders from 1997 to 2008 are constructed using the China City Statistical Yearbook (1998–2009). The total population of the city is calculated using the 2005 census. To draw these figures, we have followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

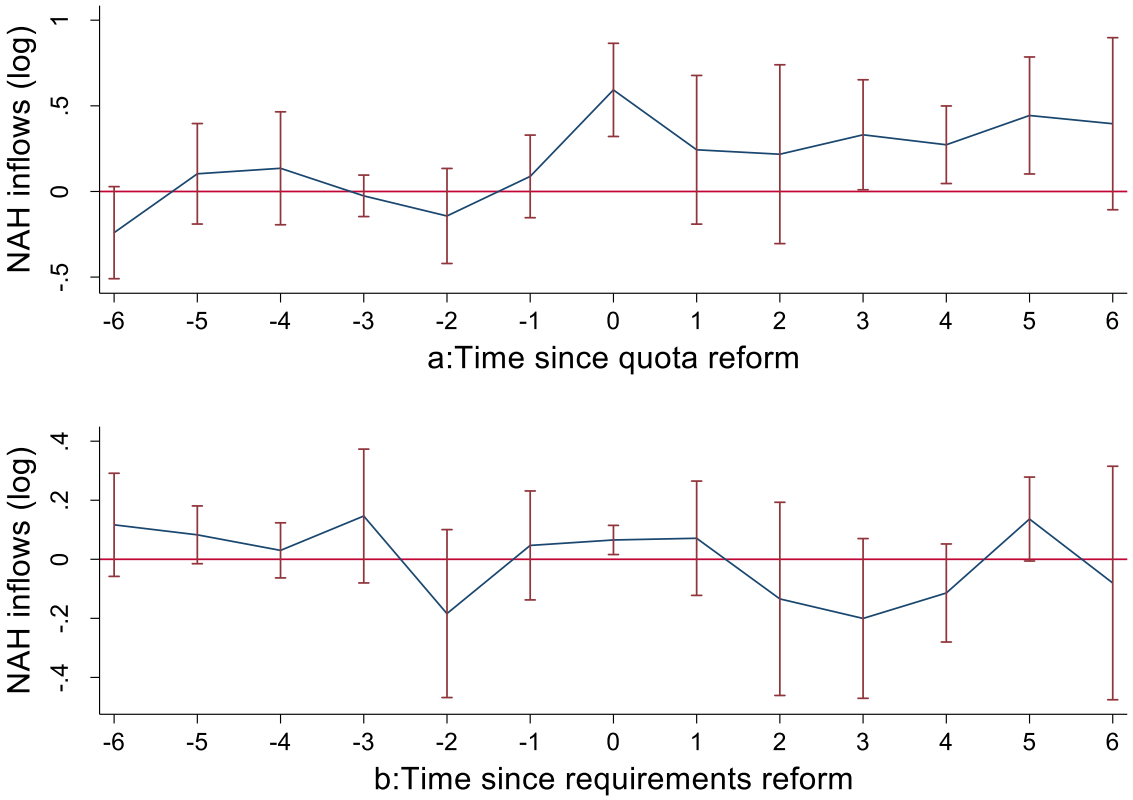

Fig. A5. Effects of improved access to *hukou* on NAH inflows. Notes: The upper panel (**Fig. A5a**) shows the impact of the quota reform for the local *hukou* application on the number of NAH inflows (in log). The lower panel (**Fig. A5b**) shows the impact of the requirements reform for the local *hukou* application on the number of NAH inflows (in log). The horizontal axis represents the values corresponding to the time relative to the adoption the *hukou* reform, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. The panel data on NAH holders from 1997 to 2008 are constructed using the China City Statistical Yearbook (1998–2009). To draw these figures, we have followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

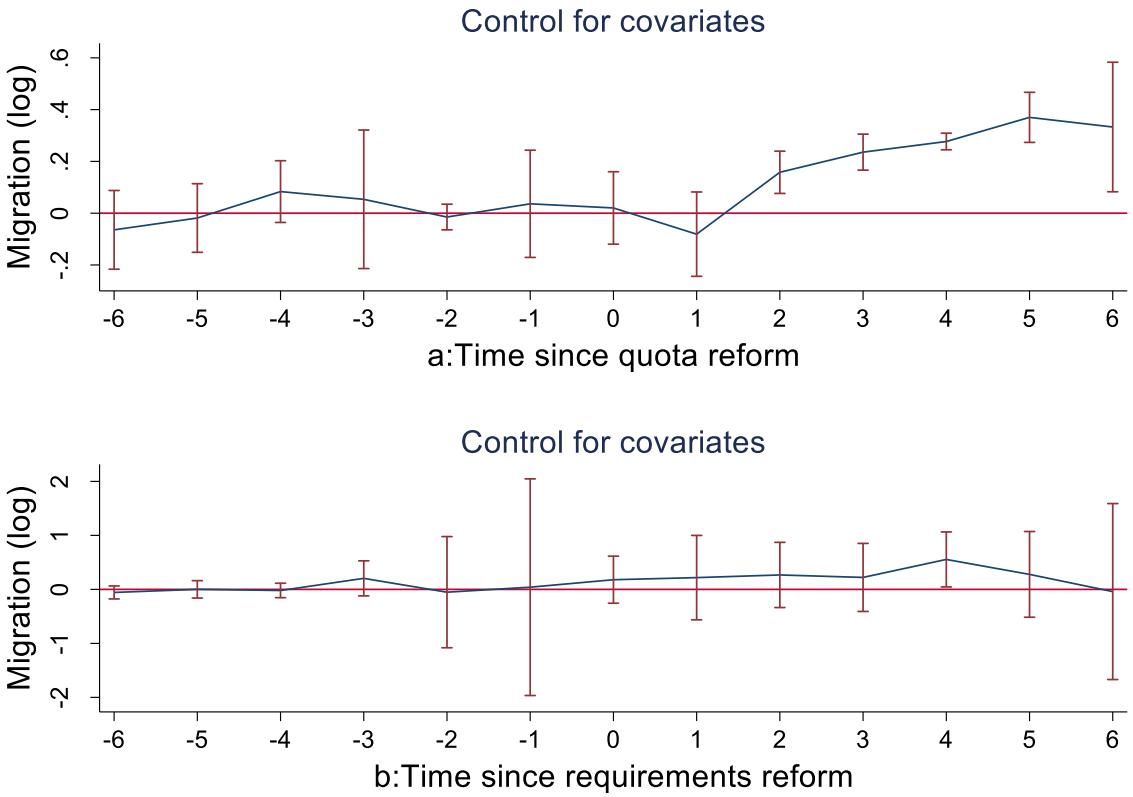

Fig. A6. Effects of improved access to *hukou* on migration after controlling for covariates. Notes: The upper panel (**Fig. A6a**) shows the impact of the quota reform for the local *hukou* application on the number of migration inflows (in log). The lower panel (**Fig. A6b**) shows the impact of the requirements reform for the local *hukou* application on the number of migration inflows (in log). The figure shows the migration impact of improved access to *hukou* after controlling for covariates such as demand shock, GDP per capita, total population, size of state sector, and coastal V.S. inland. Details on the covariates can be found in **Table A3**. We interact these covariates with year dummies in the benchmark model. In each panel, the horizontal axis represents the values corresponding to the time relative to the adoption the *hukou* reform, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. The panel data on migration inflows from 1999 to 2007 are constructed using the 2005 and 2010 censuses. To draw this figure, we have followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

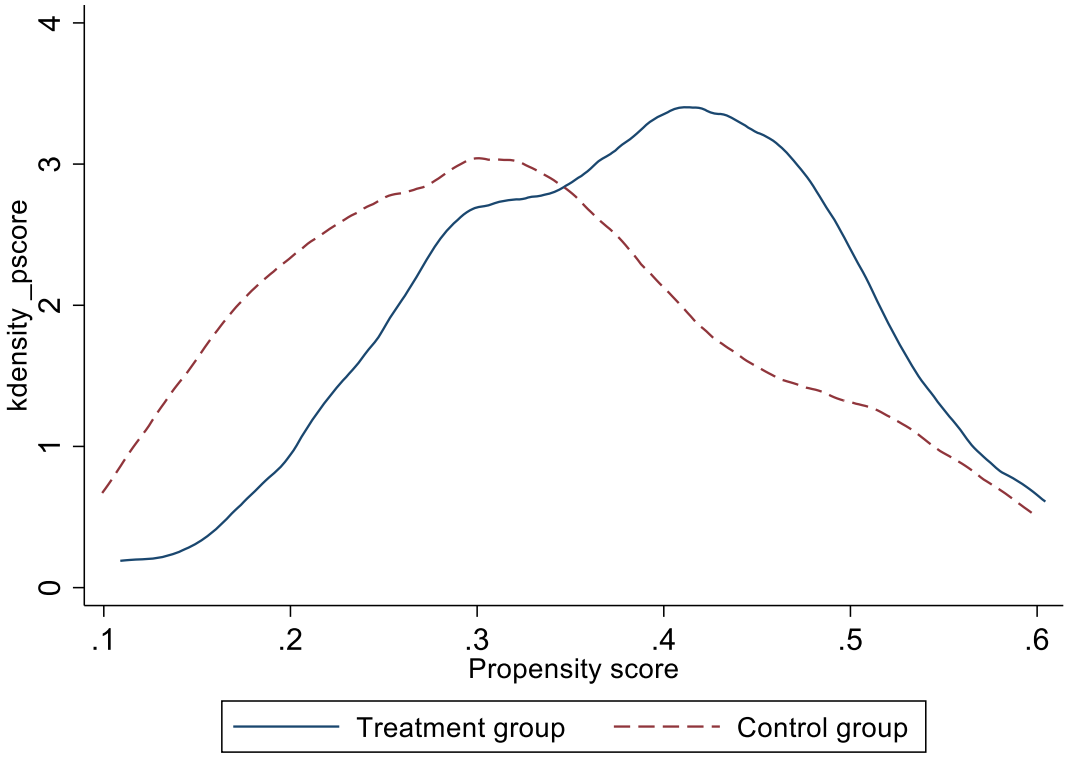

Fig. A7. Propensity Score between treatment and control groups. Notes: The figure shows the distribution of propensity score between treatment and control groups. Treatment group is defined as those cities that adopted the *Hukou* reform in 2003 and 2004, which accounts for 27.2% of all cities in the data. Control group is defined as those cities that adopted the reform in 2008 and afterwards, which accounts for 61.1% of all cities in the data. We use the STATA code *psmatch2* to calculate the propensity score. Details on the pre-treatment variables can be found in **Table A3**.

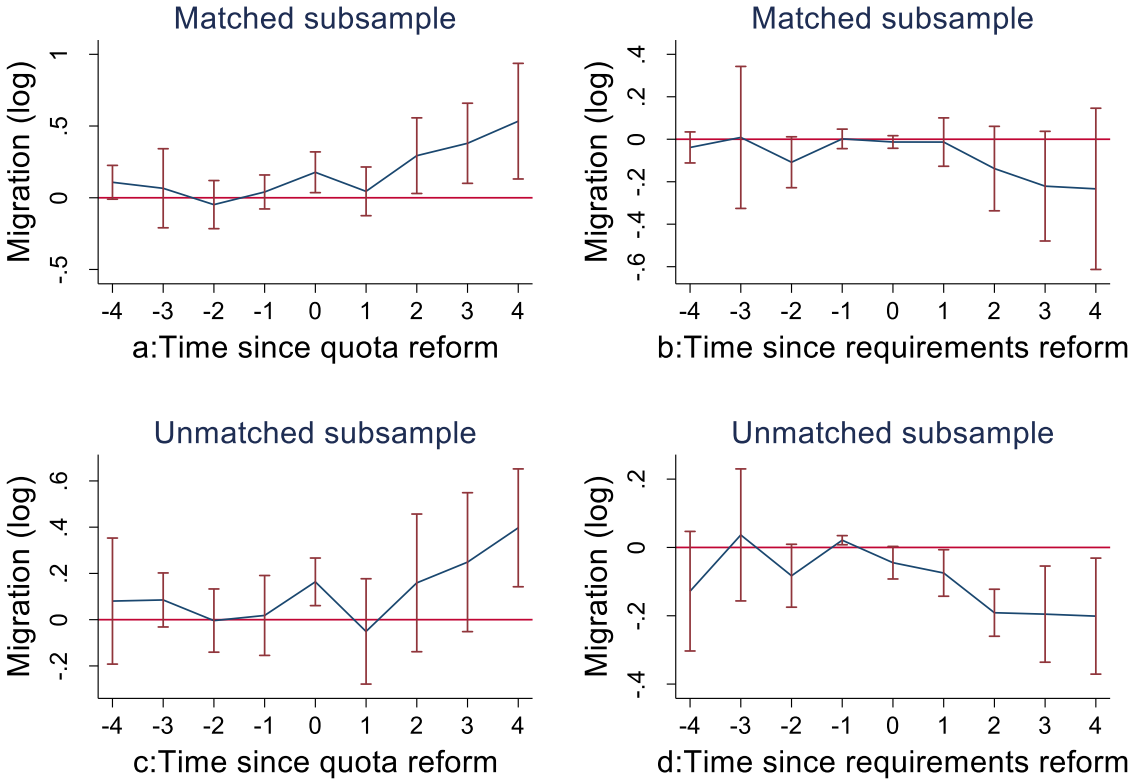

Fig. A8. Effects of improved access to *hukou* on migration using matched and unmatched subsamples
Notes: The upper-left panel (Fig. A8**a**) shows the impact of the quota reform for the local *hukou* application on the number of migration inflows (in log) using the matched subsample. The upper-right panel (Fig. A8**b**) shows the impact of the requirements reform for the local *hukou* application on the number of migration inflows (in log) using the matched subsample. The lower-left panel (Fig. A8**c**) shows the impact of the quota reform for the local *hukou* application on the number of migration inflows (in log) using the unmatched subsample. The lower-right panel (Fig. A8**d**) shows the impact of the requirements reform for the local *hukou* application on the number of migration inflows (in log) using the unmatched subsample. To construct this subsample, we keep those cities that adopted the *Hukou* reform in 2003 and 2004, and those cities that adopted the reform in 2008 and afterwards. We use propensity score matching to find more comparable control group for the early reformers. In each panel, the horizontal axis represents the values corresponding to the time relative to the adoption the *hukou* reform, which are negative in the pretreatment periods, and the vertical axis represents the values corresponding to the change in the outcome of interest. The points represent the estimated treatment effects in the treatment periods and placebo effects in the pretreatment periods, and the error bars represent 95% confidence intervals of the estimated effects and placebos. The panel data on migration inflows from 1999 to 2007 are constructed using the 2005 and 2010 censuses. To draw this figure, we have followed the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2020).

A2 Definition of timing of migration

As mentioned in Section 3.1, we use the upper bound in the corresponding responses in the 2005 and 2010 population census data to determine the timing of migration inflows from 1999 to 2009 for the study. In the survey questionnaire, people were asked how much time had elapsed since they had moved away from their registered *hukou* location. They could select one of the following responses: 1) they had not moved; 2) less than half a year; 3) between half a year and one year; 4) between one and two years; 5) between two and three years; 6) between three and four years; 7) between four and five years; 8) between five and six years; 9) more than six years.[65] We exclude individuals who selected responses 1 and 9 because they are either non-migrants or migrants with incomplete information to infer the timing of migration. Responses 3, 4, 5, 6, 7, and 8, which are the focus of this study, provide the lower and upper bounds of one to two years on the timing of migration. We apply the upper bound in computing the migration duration. For example, for individuals selecting response 4 in the 2005 Chinese Population Census, we use the upper bound (i.e., two years) to

[65] The number of migrants who indicate that they moved away from their *hukou* location more than six years ago accounts for about 24% and 20% of all migrants in Census 2005 and Census 2010, respectively. These migrants are more likely to be NAH holders, consistent with the fact that most rural-urban migrants only stay temporarily in destinations owing to high migration barriers prior to the reform adoption. Moreover, the way we construct migration inflows based on the timing of migration suggests that such a sample selection only affects the length of pre-treatment periods, rather than post-treatment periods, in our estimation. Given these considerations, we do not think that such a sample selection would significantly affect our treatment effects.

determine the timing of migration and thus consider them as migration inflows in 2003. By the same token, we construct migration inflows in 2004, 2002, 2001, 2000, and 1999 for individuals selecting responses 3, 5, 6, 7, and 8. Similarly, we use the 2010 Chinese Population Census to construct migration inflows from 2005 to 2009. In our main analysis, we exclude individuals selecting response 2, which provides the lower and upper bounds of a half-year interval on the timing of migration, to make the one-year period migration inflows consistent across years. That said, we consider individuals selecting response 2 as migration inflows in 2005 (2010) in the 2005 (2010) Chinese Population Census for robustness checks. We find that our main results still hold.

A3 Definition of annual migration inflows

As mentioned in Section 3.1, we consider all individuals who were AH holders and moved away from their registered rural *hukou* locations in China and arrived at the urban area of city s between t and $t+1$ and were still living in city s in the census year (2005 or 2010) as migration inflows (from rural areas) to city s in period t, $t \in [1999,\ 2009]$. The constructed migration inflows are de facto net migration inflows, that is, the number of new arrivals (group 1) minus the number of migrants who returned to the places of origin or re-migrated to another destination after a certain period of time (group 2). We cannot separately identify group 1 or 2 in the census data. Undeniably, failing to identify these migrants will underestimate the de facto migration inflows that occurred. Given that improved access to *hukou* may also reduce the probability of return migration or re-migrating to other destinations, we interpret the main estimates as the extent to which improved access to *hukou* affects net migration inflows.

A4 Other migration-related datasets in China

As mentioned in Section 3.1, the migration inflows observed in the census data underestimate the de facto net migration inflows that occurred, largely because the census data do not track people over time and do not have information on birth location. An alternative approach is using other datasets such as China Labor-force Dynamic Survey and China Family Panel Studies which track individuals over time and record birth locations. That said, these datasets are only able to track individuals in 2010 and afterwards. Also, they are generally representative at the national (and provincial) level but not at the city level. As a result, the census data still provide the most representative city-level migration measures in China.

A5 Alternative measures of migration inflows

As mentioned in Section 3.1, we also use three alternative measures of migration inflows constructed from comparable population (mini-) censuses in 1990, 2000, 2005, and 2010 for robustness checks. The first alternative measure refers to migration inflows that occurred in the survey year, constructed using census data from 2000, 2005, and 2010 (this information is unavailable in the census 1990). The second alternative measure of migration inflows refers to (net) inter-provincial migration inflows that occurred over the past five years (including migrants who obtained local *hukou*) constructed using census data from 1990, 2000, 2005, and 2010. The third alternative measure of migration inflows refers to the sum of the number of migration inflows, as used in the benchmark model, and the number of migrants who obtained local *hukou* using annual data from 1999 to 2008. The first measure takes advantage of the fact that the census data provide more representative migration measures for the census year and thereby focuses on migration inflows that occurred in the census year, which reduces the chance of obtaining local *hukou*, return migration, and moving to another destination. The second measure focuses on (net) inter-provincial migration inflows that occurred over the past five years. Importantly, this measure allows for all inter-provincial migrants with and without obtaining local *hukou.* Although both alternative measures cannot fully exploit variations in the timing of exposure to the *hukou* reform, they alleviate the under-estimation concerns to some degree. The third measure takes advantage of the fact that the migration inflows variable used in the benchmark model mainly captures migrants without obtaining local *hukou*, and that the changes in the number of NAH holders from another data set can be a proxy for the number of migrants who obtained local *hukou* in each year. Note that migrants who obtained local *hukou* can be either existing migrants, who have stayed in destinations for several years without obtaining local *hukou,* or new migrants, implying that this measure may overestimate migration inflows in each year, particularly in the short term when a large number of existing migrants, who stayed in destinations for a number of years, were granted with local *hukou* owing to relaxations of the annual quotas after the *hukou* reform.

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