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Working Paper

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GLO Discussion Paper, No. 498

Provided in Cooperation with:
Global Labor Organization (GLO)

Suggested Citation: Jin, Zhangfeng (2020) : Welfare Magnets and Internal Migration in China, GLO Discussion Paper, No. 498, Global Labor Organization (GLO), Essen

This Version is available at:
<http://hdl.handle.net/10419/214883>

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Welfare Magnets and Internal Migration in China

Zhangfeng Jin¹

March 18th, 2020

Abstract: This study examines the causal effects of welfare benefits on internal migration decisions. Using a quasi-experimental migration reform across 283 Chinese cities from 2002 to 2015, combined with a difference-in-differences setup, I show that improved welfare benefits substantially increase migration. The observed impact is more pronounced for individuals such as the young, women and medium-low-skilled workers. It is relatively smaller in destinations exposed to larger positive demand shocks, suggesting that improved welfare benefits reduce migration costs. And it persists over the long term. All these findings confirm the existence of sizable welfare magnet effects.

Keywords: Welfare Magnets; Internal Migration; China; Difference-in-differences

JEL Codes: H31; J61; O15; F66

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1 Introduction

Widening income gaps between the richest and poorest people, both within and between countries, are threatening economic globalization. People residing in some places are gaining more opportunities from economic integration than other people. Migration that reallocates workforces from less-productive to more-productive areas should have been more efficient in reducing these gaps had migration costs been lower. Unfortunately, labor integration has been much less successful compared with economic integration over the past decades (Hatton and Williamson 2005). Although the drivers of migration have already been studied extensively, far less is known about the causal effects of welfare benefits on migration decisions (Agersnap, Jensen, and Kleven 2019). In particular, whether poor portability of welfare benefits prevents some people from approaching jobs created by the economic globalization is a fundamental question for sustainable development.

I study this question in the context of the large-scale internal migration in China that has changed the livelihoods of hundreds of millions of people. Specifically, I examine whether and how improved welfare benefits in migration destinations affect migration decisions of rural people, who dominate internal migration in China.

This study exploits quasi-experimental, city-level variation in the timing of migration reform adoption. The migration reform in question made it easier for migrants from rural areas to obtain a local *Hukou* at the destination. *Hukou* is a form of registration that determines full access to local welfare benefits (e.g. schooling, healthcare and pension). The migration reform substitutes a rigid annual quota system with a more-flexible entry-conditions system as the basis for the local *Hukou* application. Unlike the rigid annual quota system, the new system doesn't restrict the total number of people who can obtain a *Hukou* at the destination de jure.² The distinctive feature of the

² Given the fact that not everyone is eligible for entry conditions in the short-term (e.g. certain amount of working time at the destination is required), the total number of people who can obtain a *Hukou* at the destination de facto is still restricted but much less than that of the old system.

migration reform makes it possible to examine the casual effects of improved welfare benefits in the destination.

The *Hukou* System in China was formally established by the Chinese authorities in 1958 to control internal migration in a fashion similar to an internal visa system. Under the system, Chinese citizens received local *Hukou* registration for their place of birth. Residents were not allowed to migrate away from their registered places without official permits. Since the early 1980s, the *Hukou* System has been relaxed gradually, allowing people to work temporarily in other places without registration. Nevertheless, even under the relaxed policy, unregistered migrants were denied full access to local welfare benefits unless they successfully obtained a local *Hukou*, which was strictly controlled under an annual quota system. As a result, the *Hukou* System delinked labor migration from portability of welfare benefits. This situation, along with recent reforms, provides a unique case to study the consequences of improved welfare benefits on migration decisions.

A substantial literature has studied the determinants of migration decisions. These studies have found factors such as income prospects, financial constraints, social networks and risk sharing are important for migration decisions (Kennan and Walker 2011; Angelucci 2015; Munshi 2003; Munshi and Rosenzweig 2016; Morten 2019; Dustmann and Okatenko 2014; Chen, Jin, and Prettnner 2020). However, the impact of welfare benefits on migration decisions is far from clear cut. One strand of literature shows that poor portability of employer-provided benefits (e.g. health insurance) may affect job mobility through job-lock or job-push channels (Madrian 1994; Anderson 1997). Another strand of literature shows ambiguous effects of welfare benefits on migration decisions (Borjas 1999; Kaushal 2005; Agersnap, Jensen, and Kleven 2019). Few studies have examined the effects of welfare benefits in developing countries where the social welfare system is far from complete. Finally, there is relatively little causally interpretable evidence on the impact of welfare benefits on migration decisions, with the availability of such evidence being largely constrained by a lack of comparable

data on relevant institutions in various areas.

This study fills the research gap by using a quasi-experimental migration reform across 283 Chinese cities from 2002 to 2015. The empirical strategy exploits a difference-in-differences framework based on comparisons between cities that adopted the migration reform earlier and the remaining cities that did not. The identifying assumptions are that the decision of whether and when to adopt the migration reform is uncorrelated with any prior trends in the outcomes of interest, and that the timing of the migration reform does not coincide with any city-specific shocks that are isolated to the treated group or with other policies that might influence the outcomes of interest.

In support of this assumption, I test whether there are any treatment effects in the pre-treatment period to confirm the validity of the difference-in-differences setup. In particular, I use an estimator proposed by de Chaisemartin and D'Haultfoeuille (2019) to cope with the concern that a two-way fixed effect model may not work well when there are heterogeneous treatment effects (e.g. by groups or over time) due to the negative weights problem. In addition, I conduct permutation tests to confirm that the main results are not driven by the timing pattern of the migration reform. Moreover, I control several simultaneous policies to confirm that these do not contaminate the main results. Finally, I conduct two additional placebo analyses by focusing on individuals less affected by the migration reform, and by analyzing alternative census data reflecting a period prior to the migration reform.

Improved welfare benefits may affect migration decisions through two different channels. First, they may be accompanied by corresponding adjustments (reductions) in total wages in the destination and thus exert little positive impact on migration decisions. Second, they may reduce the expected costs of migration, particularly for individuals who are more dependent on local welfare benefits (e.g. low-skilled workers), and in turn increase migration. If the first channel dominates, the impact of improved welfare benefits on migration decisions will become smaller and even negligible in the

long term. If the second channel dominates, the impact of improved welfare benefits on migration decisions will become smaller for more-skilled individuals or individuals with higher expected migration returns.

This study reports four main findings. First, improved welfare benefits in the destination substantially increase migration from rural areas, suggesting that poor portability of welfare benefits is an important obstacle deterring migration in developing countries. Second, the impact is more pronounced for the young, women and medium-low-skilled people, implying that the poor portability of welfare benefits has a disproportionately negative impact on relatively disadvantaged groups. Third, the impact persists in the long term, casting doubt on the view that improved welfare benefits will be compensated for by lower total wages in the destination and in turn exert little impact on migration decisions. Finally, the impact is smaller in destinations exposed to relatively larger positive demand shocks, suggesting that improved welfare benefits affect migration decisions through the migration cost channel. All these findings confirm the existence of sizable welfare magnet effects.

This paper joins a young literature on the consequences of poor portability of welfare benefits, either within- or between-countries (Holzmann and Werding 2015; Holzmann 2018; Holzmann and Wels 2018). The finding that improved welfare benefits substantially increase migration echoes several empirical studies testing the welfare magnet hypothesis in developed countries (Borjas 1999; Kaushal 2005; Razin and Wahba 2015; Agersnap, Jensen, and Kleven 2019).

This paper also contributes to the growing literature on quantifying the effects of the *Hukou* System in China (Sun, Bai, and Xie 2011; Kinnan, Wang, and Wang 2018; Fan 2019; Tian 2019). In particular, this paper proposes a novel approach to capture variation in the portability of welfare benefits at the city level from 2002 to 2015, which is also useful to investigate other consequences of an active labor market policy in the context of China such as wage and technology adoption (Clemens, Lewis, and Postel

2018).

Moreover, this paper is relevant to a number of studies exploring the impact of trade liberalization on internal migration in China (Zi 2016; Facchini et al. 2019; Tian 2019). The finding that the demand shock induced by trade liberalization weakens the role of welfare benefits in determining migration decisions sheds light on the coexistence of large-scale internal migration and strict domestic migration control policies in the 2000s.

Finally, the paper adds to several recent studies that found that a two-way fixed effect model identified the effects if treatment effects are constant across groups and over time. However, if the constant effect assumption is violated, the two-way fixed effect model may be problematic since it identifies weighted sums of the average treatment effects (ATE) in each group and period, with weights that may be negative (de Chaisemartin and D'Haultfoeuille 2019; Abraham and Sun 2018; Athey and Imbens 2018; Callaway and Sant'Anna 2019; Imai and Kim 2018; Goodman-Bacon 2018).

2 Backgrounds

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 a *Hukou*, either agricultural or nonagricultural, registered in a specific place. Nonagricultural *Hukou* holders were usually born in urban areas, where they had access to local public employment such as state-owned firms as well as welfare benefits provided by the Chinese authorities.³ By contrast, agricultural *Hukou* holders were usually born in rural areas, where they worked in the agricultural sector and were largely self-sufficient. The *Hukou* System was established to prevent residents from migrating from their registered locality to unregistered places. For example, rural residents were not allowed to migrate to urban areas or other rural areas unless official

³ The Chinese economy was largely planned-oriented prior to the reform and opening up policies adopted in 1979. As a result, most urban employment was provided by the government, employment in the private sector was negligible.

permits from Chinese authorities were provided.⁴ Several studies have provided more details on the *Hukou* System (Cheng and Selden 1994; Chan and Zhang 1999).

Since the early 1980s, the *Hukou* System 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 adopted reform and opening-up policies, and to cope with increasing labor surplus in rural areas after China introduced the Household Responsibility System in agricultural production (Lin 1988; 1992).⁵ Nevertheless, migrants' job opportunities were still restricted to certain labor-intensive industries in which local natives were reluctant to work (Knight and Song 1999; Knight, Song, and Huaibin 1999), and, more importantly, they were denied full access to local welfare benefits unless a local *Hukou* was obtained (Cai 2011; Song 2014). Application for a local *Hukou* was still strictly regulated under an annual quota system.

During the late 1990s, the Chinese authorities adopted a national migration reform to make it easier for migrants to obtain a local *Hukou* in small cities and towns. However, as the majority of migrants worked in medium and larger cities, the impact of this reform was limited. More recently, an additional migration reform was introduced at the national level in July 2014, when the Chinese authorities announced a decree to make it easier for migrants to obtain a local *Hukou* in the remaining medium and large cities except for a few mega-cities such as Shanghai and Beijing. In between these two national reforms, local authorities also experimented with regional migration reforms in a piecemeal fashion (Jin 2018). This study investigates comparable migration reforms gradually adopted by local authorities from 2002 to 2015. The migration reform substitutes a rigid annual quota system with a more-flexible entry-conditions system as the basis for the local *Hukou* application. I will clarify the criteria below to make sure

⁴ To some extent, although the *Hukou* System operate within a country, it works much like green card/visa systems typically function across countries.

⁵ There was also significant variation in relaxing the *Hukou* System at the local level (Jin 2018).

that the various local reforms are sufficiently comparable.

3 Data and Variables

3.1 Migration Inflows

The main dataset is a 20% random sample of survey results from the 2005 Chinese Population Census conducted by the National Bureau of Statistics. The sample includes about 260 million individuals. The census data consist of information such as age, gender, education degree, *Hukou* type (agricultural or nonagricultural), place of *Hukou* registration (urban or rural), migration history, and so on. In particular, the census data provide information on the timing of moving away from one's registered place. I focus on migrants from rural areas aged between 15 and 59. I construct a panel data set on annual migration inflows from rural areas for 283 prefecture-level cities from 1999 to 2005.⁶

To construct the indicator of migration inflows, I define migrants from rural areas at year t as individuals who moved away from their registered rural place at year t . I 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 i at year t . Specifically, for all individuals who moved away from their registered place between October t and September $t + 1$, I define them as migration inflows in the period t , $t \in \{1999, 2000, 2001, 2002, 2003\}$. Migration inflows in the periods of 2004 and 2005 are defined somewhat differently based on information available from the survey.⁷ I define migration inflows in the period of 2004 as individuals who moved away from

⁶ The constitution of China provides for three de jure levels of government. Currently, however, there are five practical (de facto) levels, consisting of local government (province, autonomous region, municipality, and special administrative region), prefecture, county, township, and village. In this paper, prefecture-level city and city are interchangeable for simplicity. In practice, the provincial governments released general guidance on the migration reform and the city level governments implemented the migration reform accordingly. More details on the migration reform adoption will be introduced later.

⁷ The 2005 Chinese Population Census was conducted on November 1st, 2005. According to the survey questionnaire, people were asked on the timing of moving away from their registered place: 1). Never; 2). Less than half a year; 3). Between half a year and one year; 4). Between one year and two years; 5). Between two years and three years; 6). Between three years and four years; 7). Between four years and five years; 8). Between five years and six years; 9). More than six years.

their registered place between October 2004 and March 2005, and I define migration inflows in the period of 2005 for individuals who moved away from their registered place between April 2005 and October 2005. The shorter periods of 2004 and 2005 may make our estimates biased downward if it takes more time for people to fully respond to the migration reform. This problem may not seriously affect the estimated results when looking at migration inflows in the long term. In addition, using the retrospective data to construct a panel data set on migration inflows may raise concerns on the comparability of migration inflows over time. For example, some migrants may re-migrate to other regions or obtain a local *Hukou* and become a new “local native” at certain points in time, which could lead to underestimation of the observed annual migration inflows. To reduce these concerns, I conduct robustness checks using two alternative indicators of migration inflows constructed from comparable census data in 1990, 2000, 2005 and 2010.⁸

3.2 Improved Welfare Benefits

It’s very difficult to directly measure improved welfare benefits across regions and over time. In this paper, I resort to a novel migration reform that improves migrants’ access to local welfare benefits in the destination, to proxy for improved welfare benefits. The migration reform was adopted by local governments at different points in time. I collect the timing of migration reform adoption from official documents of 283 prefecture-level cities.⁹ In addition to using variation in treatment timing, I additionally construct an indicator out of the share of local agricultural *Hukou* holders among the total local registered population prior to the reform adoption in order to capture variation in the coverage of the migration reform, or treatment exposure across migration reform cities. The rationale is that the migration reform mainly affects the probability of obtaining a

⁸ The first indicator is migration inflows in the survey year, constructed using census data from 2000, 2005 and 2010. The second indicator is net migration inflows in the past five years constructed using census data from 1990, 2000, 2005 and 2010. The main results are robust using alternative indicators of migration. More details can be found in the Appendix.

⁹ The migration reform was extended to the national level on July 2014, and I assume that all the remaining non-reform cities before July 2014 went on to adopt the migration reform in 2015.

local *Hukou* for agricultural *Hukou* holders.¹⁰ Therefore, cities with relatively more agricultural *Hukou* holders (relative to nonagricultural *Hukou* holders) are more exposed to the migration reform compared with other cities with relatively fewer agricultural *Hukou* holders.

The principal element of the migration reform was to substitute a rigid annual quota system with a more-flexible entry-requirements system for assessing local *Hukou* applications. Unlike under the rigid annual quota system, there's no longer a strict cap on the number of migrants eligible to obtain a local *Hukou* de jure. The entry conditions generally consist of having stable accommodation and a fixed job (or another stable source of income). Some differences exist across cities with respect to defining stable accommodation or fixed jobs. Constructing the indicator of migration reform adoption relies on the assumption that the change from an annual quota system to an entry-conditions system is more pronounced than the heterogeneous entry conditions employed across reform cities. To further allow for differences in entry conditions, I borrow an indicator of strictness of the *Hukou* System constructed by Fan (2019).¹¹

Figure 1 shows the cumulative portion of cities that had adopted the migration reform from 2002 to 2015. I find that 103 cities adopted the migration reform from 2002 to 2005, accounting for 36% of all cities. I also find that the number of reform cities increased dramatically from 2002 to 2005 and then increased relatively steadily until 2014. The sharp increase in reform cities after 2014 was caused by the extension of the migration reform to the national level.

[Figure 1]

¹⁰ It is the agricultural *Hukou* holders who are affected by the old system the most.

¹¹ I find that the coefficient of the migration reform is still significant after controlling for the strictness of the *Hukou* System, as constructed by Fan (2019).

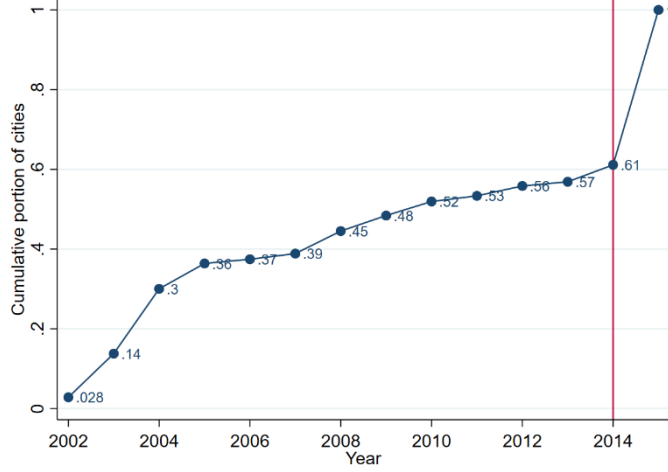


Figure 1 Cumulative portion of cities that had adopted the migration reform from 2002 to 2015
 Note: The migration reform was extended to the national level in July 2014, and I assume that all the remaining non-reform cities from before July 2014 adopted the migration reform in 2015.

4 Model Specifications and Identification Strategies

The regression specification evaluates the effects of improved welfare benefits as a quasi-experiment. Treatment is the degree of exposure to the migration reform. I use difference-in-differences with continuous treatment, following Card (1992):

$$y_{st} = \alpha' I_s + \beta' I_t + \gamma \left(I_{t \geq t_{reform}} \cdot agri_s^{2001} \right) + \varepsilon_{st} \quad (1)$$

where y_{st} is the logarithm of migration inflows in city s in year t , I_s is the vector of city fixed effects, I_t is the vector of time fixed effects, $I_{t \geq t_{reform}}$ is an indicator for an observation after the migration reform, L_{st}^{agri} is the stock of agricultural *Hukou* holders, L_{st} is the stock of both agricultural and nonagricultural *Hukou* holders, and $agri_s^{2001}$ is the fraction of agricultural *Hukou* holders L_{st}^{agri} / L_{st} in city s in 2001, the year before initiating the migration reform. The error term is ε_{st} , α and β are vectors of coefficients to be estimated, and γ is the coefficient of interest. Assuming that trends in the outcome would have been similar in cities most affected by the migration reform to trends in unaffected cities had the migration reform not occurred, the estimate $\hat{\gamma}$ captures the effect of welfare benefits.

The variation in treatment exposure comes from two sources. The first is cross-city

differences in exposure to the migration reform, driven by the ratio of agricultural *Hukou* holders to the total registered population prior to the migration reform. The second is cross-city variation in the timing of when or whether cities adopted the migration reform. The assumptions underlying the identification of parameter γ are similar to all difference-in-differences analyses: the decision of whether and when to adopt the migration reform must be uncorrelated with any prior trends in migration outcomes, and the timing of the migration reform cannot coincide with any city-specific shocks that are isolated to the treated group or with other policies that might influence migration outcomes.

Recent studies show that the two-way fixed effects (FE) regression above identifies the effect under the standard “common trends” assumption when the treatment effect is constant across groups and over time. However, it is often implausible that the treatment effect is constant. If the constant effect assumption is violated, the two-way FE regression identifies weighted sums of the average treatment effects (ATE) in each group and period, with weights that may be negative (de Chaisemartin and D’Haultfoeuille 2019; Abraham and Sun 2018; Athey and Imbens 2018; Callaway and Sant’Anna 2019; Imai and Kim 2018; Goodman-Bacon 2018). Due to the negative weights, the linear regression estimand may for instance be negative while all the ATEs are positive. To check the potential negative weights problem, I resort to an estimator proposed by de Chaisemartin and D’Haultfoeuille (2019). The estimator also allows me to check the common trends assumption of the difference-in-differences setup even in the presence of heterogeneous treatment impacts across groups. If the secular trends hold, I should not find any significant treatment effects in the pre-treatment period.

The second potential identification problem—that is, the potential of unobserved city specific shocks correlated with the migration reform—is more difficult to investigate. However, there is much variation in the timing of adopting the migration reform, as shown in Figure 1, making it very unlikely that there are secular shocks that are systematically correlated with the timing of the migration reform and only influence

the affected cohorts. I use permutation tests to support the contention that unobserved shocks correlated with the timing of the migration reform are not biasing the estimates. I perform permutation tests for all of the outcomes that randomly reassign the timing of migration reform across cities. I do this in two ways: first, I randomly assign the timing of the migration reform between 2002 and 2015 across cities, and second, I randomly assign the timing of the migration reform to match the timing distribution shown in Figure 1.

In addition, I use the same dataset to construct migration from urban areas. As the migration reform mainly makes it relatively easier for rural migrants to obtain a local *Hukou*, I should not find similar impacts on migration from urban areas if the estimates are solely driven by the migration reform. Otherwise, the estimates are driven by other unobserved city shocks.

Moreover, I use an alternative wave of census data from 2000 to construct migration inflows from 1995 to 2000. I also use this new dataset to conduct a placebo analysis. If there are no secular trends, I should not find any significant impact using these counterfactual migration inflows prior to the migration reform.

Finally, the existence of alternative policies that were passed concurrently with the migration reform is a more serious threat to identification. To address the concern that other simultaneous policies would contaminate the results, I control a number of alternative policies that occurred concurrently with the migration reform that could impact migration outcomes. The first one is reductions in trade policy uncertainty. The second one is changing strictness of *Hukou* regulations over time. The third one is minimum wage regulation. The fourth and final one is abolition of agricultural tax. If the migration reform works, controlling these simultaneous policy shocks should not significantly change the main results.

5 Empirical Results

This section mainly answers four questions. First, do improved welfare benefits affect migration decisions? Second, are the effects of improved welfare benefits heterogeneous across groups (e.g. by age, gender and skill)? Third, does the impact persist over the long term? Fourth, does the impact respond to a demand shock in the destination?

5.1 Welfare benefits and migration decisions

I start with estimating the impact of improved welfare benefits on the logarithm of migration inflows from rural areas.¹² Main results are reported in Table 1. Column 1 shows that the coefficient of interest is 0.62. Specifically, for cities with an average treatment exposure, improved welfare benefits increase rural migration inflows by 53% on average, suggesting that welfare benefits in the destination substantially affects migration.¹³

A crucial assumption to justify the difference-in-differences approach above is the common parallel trends in the pre-treatment period. To test this assumption, I resort to an estimator proposed by de Chaisemartin and D'Haultfoeuille (2019), which is robust when there are heterogeneous treatment effects across groups and over time. If the common parallel assumption is valid, I should not find any significant impacts in the pre-treatment period. Figure 2 shows the treatment effect in the pre-treatment periods. I find consistent evidence that the treatment effect is close to zero and not statistically significant at the conventional level in the pre-treatment period. By contrast, I find a significant increase in migration inflows after adopting the migration reform. Further estimation reveals that the negative weight problem is not obvious in this exercise.

[Figure 2]

¹² I find consistent results using the migration inflows from rural areas in levels as the outcome variable. More details can be found in the Appendix.

¹³ I find similar results when considering homogenous treatment exposure across the migration reform cities. More details can be found in the Appendix.

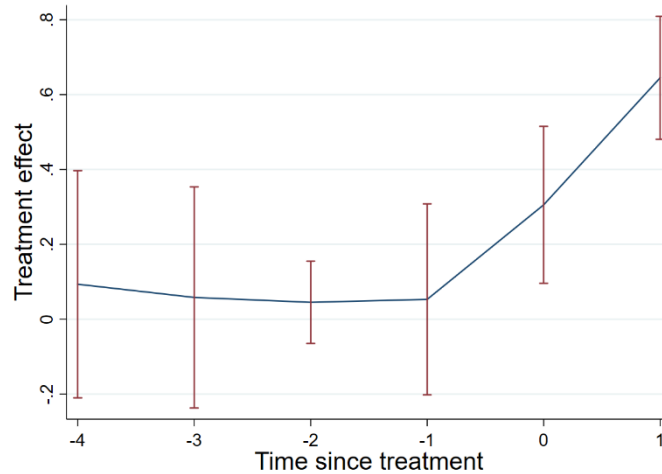


Figure 2 Impact of improved welfare benefits on migration (in logarithm)

Note: I follow the estimation strategy proposed by de Chaisemartin and D’Haultfoeuille (2019) to draw this figure.

As discussed above, of primary concern in the identification strategy is the existence of secular trends that differ systematically with treatment exposure. The estimates above suggest such trends are not biasing the estimates. As an additional test of whether the timing pattern of the migration reform is driving the results, I perform permutation tests for all of the outcomes that randomly reassign the timing of the migration reform across cities (Lovenheim and Willén 2019). I do this in two ways: first, I randomly assign the migration reform dates between 2002 and 2015 across cities, and second, I randomly assign the migration reform dates to match the timing distribution shown in Figure 1. I perform the permutations 300 times for each outcome and calculate the percentage of times the simulated estimate is larger than the actual estimate. These results therefore represent p-values of the null hypothesis that any combination of the migration reform dates across cities would generate the same pattern of treatment effects. I reject such a null at the 1 percent level for every outcome in both panels. These results suggest that the baseline estimates are not identified off of secular trends or endogenous timing of the migration reform.

As I have discussed before, the migration reform mainly improves welfare benefits for agricultural *Hukou* holders, or migration from rural areas. If the estimation results are

Table 1 Impact of improved welfare benefits on migration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Rural Migration	Urban Migration	Counterfactual Migration	Simultaneous Policies	Ages 0-14	Ages 15-29	Ages 30-44	Ages 45-59
Treatment effect	0.624 (0.116)	-0.052 (0.094)	-0.077 (0.090)	0.566 (0.124)	0.525 (0.101)	0.710 (0.125)	0.601 (0.128)	0.424 (0.120)
Observations	1801	1779	1516	1468	1633	1723	1710	1444
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	
	Ages 60+	Male	Female	Low skill	Medium-low skill	Medium-high skill	High skill	
Treatment effect	0.209 (0.109)	0.571 (0.122)	0.633 (0.113)	0.442 (0.125)	0.655 (0.123)	0.378 (0.130)	0.232 (0.216)	
Observations	1128	1737	1775	1641	1761	1421	710	

Notes: this tables shows the impacts of improved welfare benefits on migration decisions. Column 1 shows the impact of improved welfare benefits on migration inflows from rural areas. Column 2 shows the impact of improved welfare benefits on migration inflows from urban areas. Column 3 show the impact of improved welfare benefits on counterfactual migration inflows from rural areas (1995-2000). Column 4 shows the impact of improved welfare benefits on migration inflows from rural areas by controlling for a number of simultaneous polices such China's accession to the WTO, minimum wage regulation, abolition of agricultural tax and the indicator of Hukou regulation proposed by Fan (2019). Columns 5-9 show the heterogeneous impacts of welfare benefits on migration decisions by age. Columns 10-11 show the heterogeneous impacts of welfare benefits on migration decisions by gender. Columns 12-15 show the heterogenous impacts of welfare benefits on migration decisions by skill. Standard errors are clustered at the city level. Standard errors in parentheses.

driven by the migration reform, I should not find similar impacts for nonagricultural *Hukou* holders, or migration inflows from urban areas. Using the same empirical approaches, I estimate the impact of the migration reform on urban migration inflows. Results are reported in Column 2. I find that the coefficient of interest is negative and is not statistically significant at the conventional level, reducing the possibility that the estimation results are driven by other unobservable city specific shocks.¹⁴

In addition, I use the census data from 2000 to conduct a placebo analysis. Specifically, the 2000 census provides details on the timing of arriving at the current destination, which allows me to construct rural migration inflows from 1995 to 2000. If the main estimates are not driven by pre-treatment differences, I should not find any significant impact of the migration reform using the counterfactual migration inflows. Using the same empirical approaches, I estimate the impact of the migration reform on counterfactual rural migration inflows. Results are reported in Column 3. I find that the coefficient of interest is negative but is not statistically significant at the conventional level, further reducing the concern that the main estimates are driven by pre-treatment differences.

Finally, I deal with potential concerns that the estimates above are contaminated by simultaneous policy shocks. The first policy shock is China's accession to the World Trade Organization, which is likely to increase migration inflows and to induce local authorities to adopt more migrant-friendly regulations (Facchini et al. 2019; Tian 2019). I measure the impact of the trade liberalization following Facchini et al. (2019). The second policy shock involves dynamics of the strictness of the *Hukou* System that may not be fully captured by the migration reform. I borrow an indicator of *Hukou* strictness from Fan (2019). The third policy is minimum wage, which is likely to affect labor supply (Fang and Lin 2015; Gan, Hernandez, and Ma 2016). I collect official data on the minimum wage at the city level over time. The last policy shock is abolition of the

¹⁴ It could be that the increase labor demand during development was for low-skilled rural workers and not for those skilled from the cities, and hence had nothing to do with the migration reform.

agricultural tax, which is likely to affect rural people's income and resulting incentives to migrate outside (Shuo Chen and Wang 2014; S. X. Chen 2017). I measure the impact of abolishing the agricultural tax following S. Chen and Wang (2014). Main results are reported in Column 4. I do not find evidence that the main results change significantly after controlling for all these policies.

5.2 Who is more responsive to the improved welfare benefits?

I further explore whether the effects of improved welfare benefits are heterogeneous across groups. First, I examine whether the impact of improved welfare benefits varies by age. I find that the impact decreases with age for the working population. Improved welfare benefits increase migration inflows by 63% for people aged between 15 and 29, while the impact decreases to 33% for people aged between 45 and 59. By contrast, the impact becomes much smaller for people aged 60 and above (14%). Second, I examine the impact of improved welfare benefits by gender. I find that improved welfare benefits increase rural migration inflows by 48% and 54% for men and women, respectively, suggesting that women's migration decisions are more responsive to the improved welfare benefits. Third, I examine the impact of improved welfare benefits by skill. I find that medium-low-skilled people are most responsive to improved welfare benefits (56%), following by low-skilled people (35%). By contrast, I do not find any significant impact for high-skilled people. Main results are reported in Columns 5-15.

Overall, I find significant evidence that the effects of improved welfare benefits are heterogeneous across groups. The impact is more pronounced for the young, women, and medium-low-skilled people, suggesting that the poor portability of welfare benefits would prevent some relatively disadvantaged people from migrating for a better job.

5.3 The long-term impact of improved welfare benefits

While the findings described so far indicate that improved welfare benefits substantially increase migration, another important question is whether the impact persists in the long

term. To resolve this uncertainty, I use census data from 2010 to construct migration inflows from 2004 to 2010, following similar approaches to those described above. The more-recent panel data allow me to focus on the impact of improved welfare benefits on net migration inflows over the long term.

Using a two-way fixed effects model, I find that improved welfare benefits increase rural migration inflows by 1.1%, which is not statistically significant at the conventional level. Further investigation shows that the negative weights problem is noteworthy in this exercise.¹⁵ Using the new estimator proposed by de Chaisemartin and D’Haultfoeuille (2019), I find that improved welfare benefits substantially increased rural migration inflows five years after the migration reform adoption. Moreover, the observed impact increased over time, casting doubt on the view that the poor portability of welfare benefits will be compensated for by lower total wages at the destination and will exert little impact on migration decisions. Main results are reported in Figure 3.

[Figure 3]

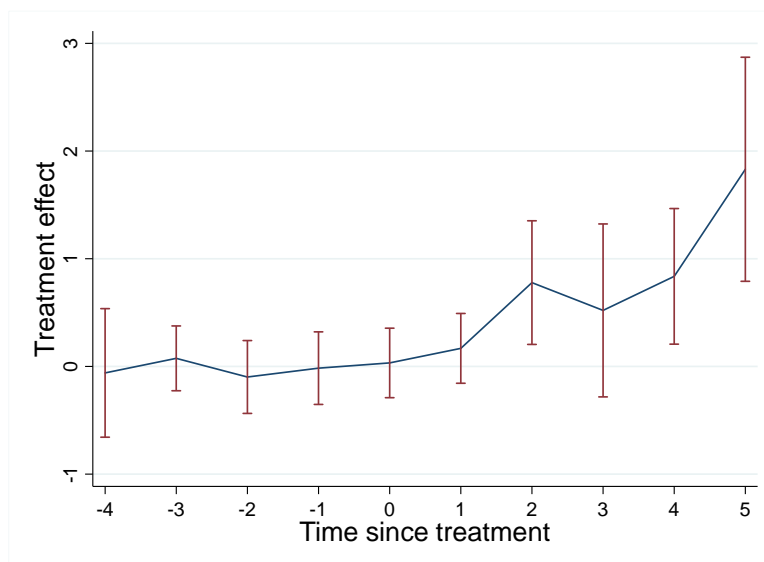


Figure 3 Impact of improved welfare benefits on migration using 2010 census data

Notes: I follow the estimation strategy proposed by de Chaisemartin and D’Haultfoeuille (2019) to draw this figure.

¹⁵ I use the STATA command of *twowayfeweights* to check the negative weight issue. Under the common trends assumption, beta estimates a weighted sum of 770 ATTs. 520 ATTs receive a positive weight, and 250 receive a negative weight. The sum of the negative weights is equal to -0.28.

5.4 Demand shock, welfare benefits and migration decisions

Why do improved welfare benefits matter for migration decisions, particularly for relatively disadvantaged people? A possible explanation is that improved welfare benefits reduce the migration cost associated with access to local welfare benefits. If this is true, the impact of improved welfare benefits should be smaller for individuals exposed to relatively higher expected migration returns, consistent with previous findings that more skilled people are less responsive to the improved welfare benefits.

To further test this hypothesis, I resort to an exogenous positive demand shock induced by trade liberalization and examine how the positive demand shock affects the impact of improved welfare benefits. Since the positive demand shock in the destination improves migrants' job prospects and in turn increases their expected migration returns, it should dwarf the role of migration costs in determining migration decisions.

I construct an indicator of Normal Trade Relations (NTR) gap to capture reductions in trade policy uncertainty at the city level (Pierce and Schott 2016; Facchini et al. 2019). A larger reduction in the NTR gap can be considered as a larger positive demand shock in the destination. To explore how the impact of improved welfare benefits responds to the positive demand shock, I add the interaction term between the indicator of NTR gap and the impact of improved welfare benefits. The coefficient of the interaction term captures the extent to which the positive demand shock affects the impact of improved welfare benefits. Main results are shown in Table 2. I find that a 10-percentage-point increase in NTR gap reduces the impact by 0.19, suggesting that the impact of improved welfare benefits is smaller in destinations exposed to larger positive demand shocks. This finding further supports the argument that improved welfare benefits affect migration decisions through the migration cost channel.

[Table 2]

Table 2 Demand shock in the destination and the impact of improved welfare benefits

	(1)	(2)
Treatment effect	0.626 (0.118)	1.278 (0.375)
Treatment effect #NTR gap		-1.973 (1.057)
Observations	1766	1766

Notes: The table shows how a positive demand shock in the destination affects the elasticity of migration to improved welfare benefits. Standard errors are clustered at the city level. Standard errors in parentheses.

6 Conclusion

This study, to the best of my knowledge, is the first one to explore the casual effects of welfare benefits on migration decisions in developing countries. Using a quasi-experimental migration reform across 283 Chinese cities from 2002 to 2015, I estimate the impact of improved welfare benefits on migration in a difference-in-differences framework. The most important lesson of this study is that improved welfare benefits substantially increases migration, particularly for relatively disadvantaged people. These findings confirm the existence of sizable welfare magnet effects in China.

Another important finding is that welfare benefits in the destination affect migration decisions through the migration cost channel. As a result, the poor portability of welfare benefits in the destination contributes to rising income inequality by preventing relatively disadvantaged people from migrating for better jobs. Last but not least, this study proposes a novel approach to capture variation in the portability of welfare benefits at the city level from 2002 to 2015, which is also useful for investigating the causal impact of an active labor market policy on other outcomes such as wage and technology adoption (Clemens, Lewis, and Postel 2018).

There are some limitations to this study. The first one involves the interpretation of the results. Given the fact that the *Hukou* System is also associated with labor market discrimination, an alternative interpretation of the findings could be that the studied

migration reform improves migrants' job opportunities in the destination and in turn affects migration decisions. If this is the case, I should find a larger impact in destinations exposed to larger positive demand shocks. However, this hypothesis is not supported, reducing the concern of misinterpretation to some extent. The second limitation is that we cannot extend the main findings to urban households, which are not substantially affected by the migration reform.

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Appendix

The appendix consists of five parts. Part one provides more details on the migration reform adoption. Part two presents summary statistics for main variables. Part three uses alternative indicators of migration inflows variable to estimate the main results. Part four adopts alternative indicators of the migration reform variable to estimate the main results. Part five examines the impact of the migration reform on the probability of obtaining a local *Hukou*.

A1 More details on the migration reform

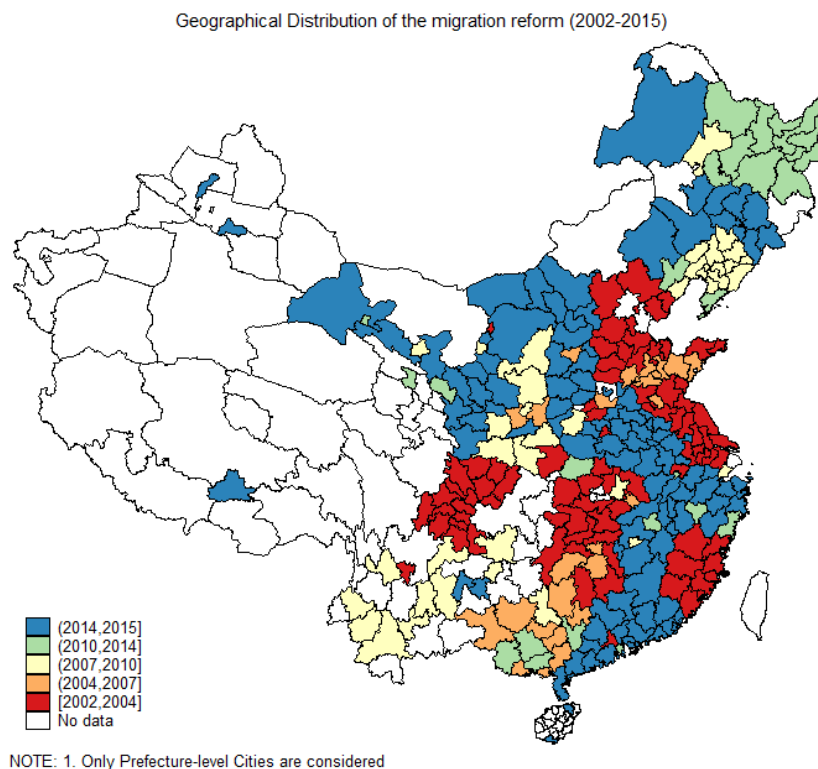


Figure A1 Geographical distribution of migration reform adoption across cities from 2002 to 2015
Notes: This figure shows the geographical distribution of the migration reform adoption across cities. The migration reform was extended to the national level on July, 2014, and I assume that all the remaining non-reform cities before July, 2104 will adopt the migration reform in 2015.

Table A1 General contents of the migration reform

	Institutional Framework	Details
Before adopting the migration reform	Annual Quota System	Obtaining a local <i>Hukou</i> is strictly controlled by the annual quota system, particularly for agricultural <i>Hukou</i> holders; recruited by the state-owned firms is one of main channels
After adopting the migration reform	Entry Conditions System	Migrant workers satisfying the entry conditions (e.g. stable accommodation, a fixed job) are eligible to obtain a local <i>Hukou</i>
Main Impacts	Direct impact: higher probability of obtaining a local <i>Hukou</i> , particularly for agricultural <i>Hukou</i> holders Indirect impact: higher probability of migration	

Notes: This table shows the general contents of the migration reform. The general contents of the migration reform were released by the provincial-level governments and the city-level governments implemented the reform more specifically.

Table A2 Specific contents of the migration reform at the city level

Name of City	Name of Province	Date of Adoption	Stable Accommodation Required or not	A Fixed Job Required or not
Fuzhou	Fujian	200203	YES	NO
Quanzhou	Fujian	200207	YES	NO
Xiamen	Fujian	200308	YES	NO
Shijiazhuang	Hebei	200310	YES	YES
Hengshui	Hebei	200311	YES	NO
Handan	Hebei	200404	YES	NO
Foshan	Guangdong	200407	YES	YES
Mianyang	Sichuan	200306	YES	YES
Yibin	Sichuan	200307	YES	YES
Zigong	Sichuan	200408	YES	NO
Ezhou	Hubei	200411	YES	YES
Huangshi	Hubei	200507	YES	YES
Xiangtan	Hunan	200501	YES	YES
Chenzhou	Hunan	200405	YES*	YES
Changde	Hunan	200411	YES	YES
Weihai	Shandong	200410	YES	YES
Taian	Shandong	200512	YES	NO
Qingdao	Shandong	200708	YES	YES
Wuhu	Anhui	200309	YES	YES
Suzhou	Jiangsu	200305	YES	YES **
Nanjing	Jiangsu	200407		Either condition is OK
Huaian	Jiangsu	200310		Either condition is OK
Kunming	Yunnan	200801	YES	YES

Notes: This table shows specific contents of migration reform adoption using a number of reform cities for comparison. * renting a local accommodation is OK. ** not required for local people.

A2. Summary statistics

A2.1 Migration inflows

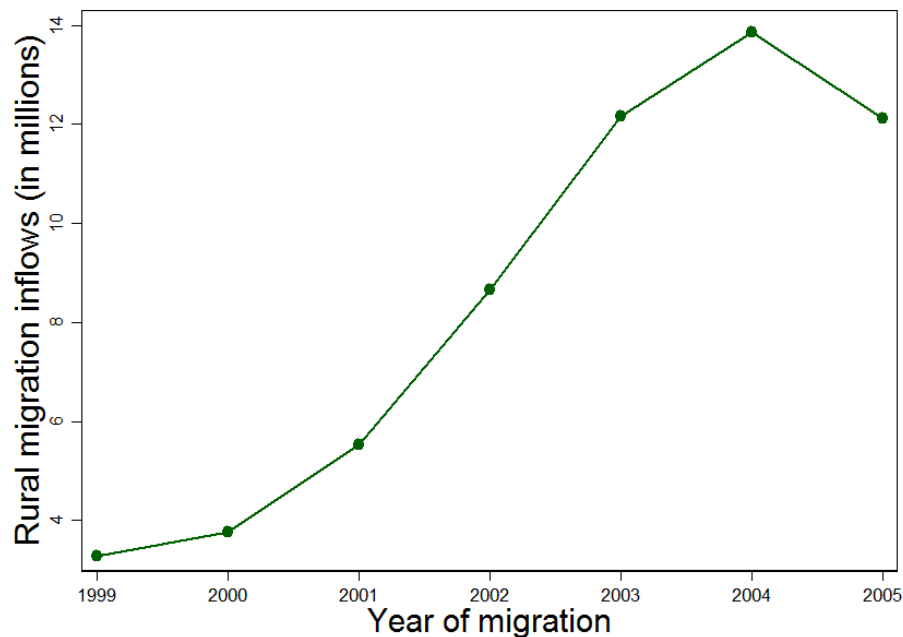


Figure A2 Rural migration inflows to all cities from 1999 to 2005

Notes: This figure shows the aggregated number of annual migration inflows from rural areas to all 283 prefecture-level cities using the census data in 2005.

A2.2 Indicator of trade liberalization

To measure the demand shock induced by trade liberalization, I resort to an indicator of Normal Trade Relations (NTR) gap adopted in the literature (Pierce and Schott 2016; Handley and Limão 2017; Facchini et al. 2019). The NTR gap is defined as the difference between the Normal Trade Relations (NTR) tariffs, reserved to WTO members and applied to China's exports to the U.S. since the early 80s, and the non-NTR rates, which are instead the higher tariff rates applies to nonmarket economies and originally established under the Smoot-Hawley Tariff Act introduced in 1930. The latter would have been applied to China if Congress failed to extend MFN status to China in any year before China's entry in the WTO. More precisely, the NTR gap for product i is defined as: $NTR\ gap_i = non\ NTR\ rate_i - NTR\ rate_i$. I construct the NTR gap using data on NTR rates for 2000, the year before China's accession to WTO. To carry out the empirical analysis, I 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, I use the firm-level export information available in China's Customs Data. Specifically, the NTR gap in city j is defined as $NTR\ gap_j = \sum_i \frac{Exp_{ij}}{Exp_j} * NTR\ gap_i$, where Exp_{ij} are the exports of good i from city j and Exp_j are total exports of the city.

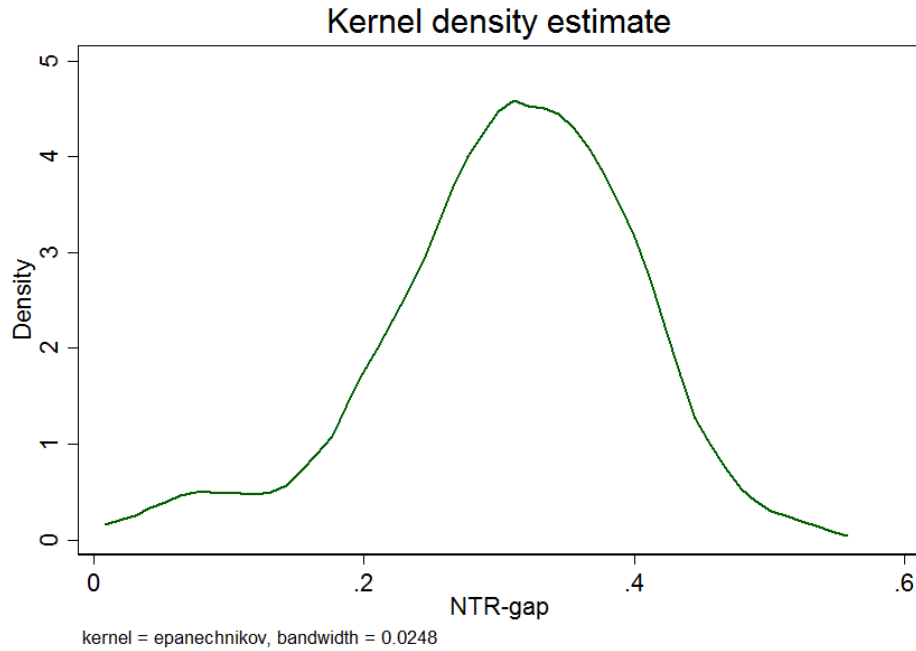


Figure A3 Distribution of NTR-gap across cities

Notes: This figure shows the distribution of NTP-gap across cities. I construct the NTR-gap following the literature (Pierce and Schott 2016; Handley and Limão 2017; Facchini et al. 2019).

A2.3 Summary Statistics

Table A3 Summary statistics

VARIABLES	(1) N	(2) mean	(3) min	(4) max
Rural migration inflows	1,834.00	31,616.12	0.00	1,242,151.00
Rural migration inflows (log)	1,801.00	9.43	4.65	14.03
Treatment variable (discrete)	1,834.00	0.12	0.00	1.00
Treatment variable (continuous)	1,834.00	0.09	0.00	0.90
Minimum wage (monthly)	1,528.00	351.41	215.00	690.00
Abolition of agricultural tax	1,822.00	5,836.90	0.00	34,493.00
NTR gap	1,799.00	0.31	0.03	0.53
Hukou indicator by Fan (2019)	1,834.00	1.70	0.00	6.00

Notes: This table shows summary statistics for main variables. I use micro census data in 2005 to construct the indicator of rural migration inflows from 1999 to 20015. I calculate the NTR gap at the city level using China custom data in 2000. I borrow the *Hukou* indicator from Fan (2019). The minimum wage and abolition of agricultural taxes at the city level are collected from official documents.

Table A4 Number of migration inflows by the timing of out-migration

Period	Number of migrants (million)	Percent
1998.10 and before	14.77	19.92%
1999.10-2000.09	3.27	4.42%
2000.10-2001.09	3.75	5.06%
2001.10-2002.09	5.52	7.45%
2002.10-2003.09	8.66	11.68%
2003.10-2004.09	12.15	16.39%
2004.10-2005.03	13.86	18.70%
2005.04-2005.10	12.11	16.34%
All	74.13	100%

Notes: This tables shows the number of migration inflows by the timing of out-migration using census data in 2005.

Table A5 Composition of migration inflows by the timing of out-migration

year	Age	Male	Skilled
1999	27.39	0.49	0.37
2000	27.97	0.49	0.39
2001	28.01	0.48	0.37
2002	27.47	0.48	0.37
2003	27.05	0.47	0.35
2004	28.03	0.49	0.31
2005	27.56	0.56	0.37

Notes: This table shows the composition of migration inflows by the timing of out-migration. Skilled people are defined as those individuals with high school degree or above.

A3. Alternative indicators of migration inflows

A3.1 Migration inflows in levels

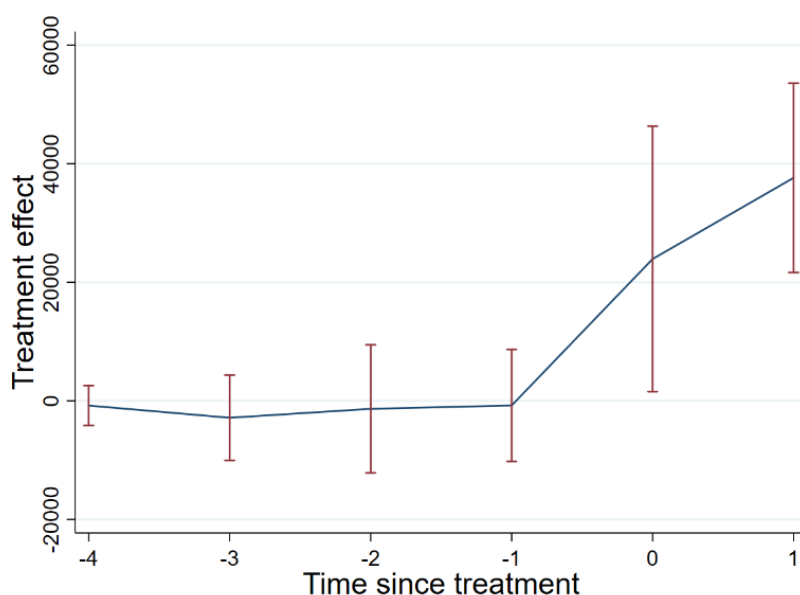


Figure A4 Impact of improving welfare benefits on migration inflows (in levels)

Notes: I follow the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2019) to draw this figure.

A3.2 Other indicators of migration

A potential concern of the migration variable is that I may underestimate the migration inflows de facto, partly because some migrants would obtain a local *Hukou* and partly because some migrants would return back home or re-migrate to other cities. The underestimation would be more serious the farther away from the survey year. To cope with this problem, I propose two alternative indicators of migration. First, I resort to census data in 2000, 2005 and 2010, which allow me to calculate the recent migration inflows (e.g. migrant inflows in the survey year) in each survey year. While this indicator of migration is available for every five years, it captures the recent migrants who are much less likely to obtain a local *Hukou* or to migrate to other places. Second, I resort to an alternative piece of information on whether you are living in a difference place compared with the place you lived in five years ago to construct an indicator of migration. While this indicator of migration inflows is only available for inter-provincial migration, it also allows me to capture migrants who obtain a local *Hukou*. I resort to census data in 1990, 2000, 2005 and 2010 to construct this indicator of migration. The main results are reported in Table A6. I find the main results are still very significant but a bit smaller than before, reducing the concern that the main results

are mainly driven by the construction of the migration variable.

Table A6 Impact of improving welfare benefits using alternative indicators of migration

	(3)	(6)
	Migration inflows over the past five years	Rural migration inflows in the survey year
Treatment effect (continuous)	0.404 (0.138)	0.887 (0.239)
Observations	1019	777

Notes: The table shows the impact of improving welfare benefits using alternative indicators of migration. Column 1 construct an indicator of migration inflows over the past five years using census data in 1990, 2000, 2005 and 2010. Column 2 constructs an indicator of rural migration inflows in the survey year using census data in 2000, 2005 and 2010. Standard errors are clustered at the city level. Standard errors in parentheses.

A4. Alternative indicators of the migration reform variable

A4.1 Homogeneous treatment exposure

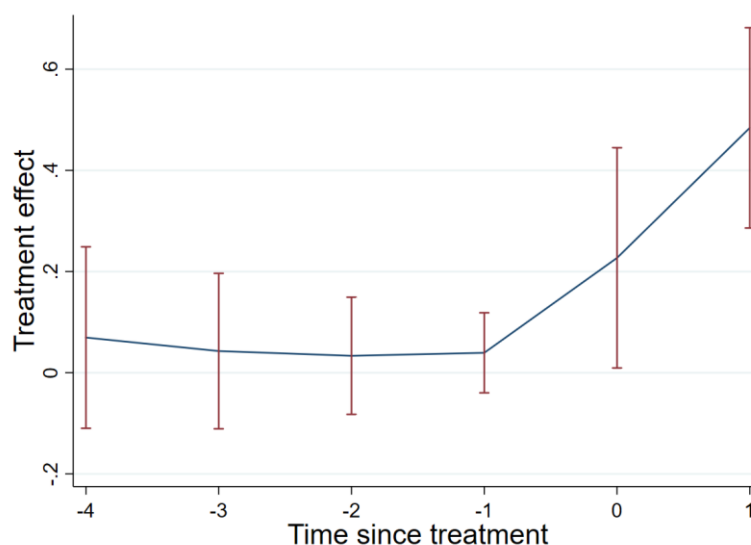


Figure A5 Impact of improving welfare benefits on migration inflows with homogeneous treatment exposure (in logarithm)

Notes: I follow the estimation strategy proposed by de Chaisemartin and D'Haultfoeuille (2019) to draw this figure.

A4.2 Migration reform in 2005

As I have shown before, the definition of migration inflows in 2005 is a bit different from migration inflows in other years. I further check whether the main results are

driven by the migration reform impact in 2005. I use two ways to conduct the robustness checks. First, I drop cities that adopted the migration reform after the survey month in 2005. Second, I drop all cities that adopted the migration reform in 2005. The main results are shown in Table 16. I find that both results are still very robust, reducing the concern that the main results are driven by the migration reform impact in 2005.

Table A7 Impact of improving welfare benefits using alternative definition of migration reform

	(2)	(4)
Treatment effect	0.607 (0.116)	0.571 (0.115)
Observations	1781	1698

Notes: This table shows the impact of improving welfare benefits using alternative definitions of the migration reform variable. Column 1 drops cities that adopted the migration reform after the survey month in 2005. Columns 2 drop all cities that adopted the migration reform in 2005. Standard errors are clustered at the city level. Standard errors in parentheses.

A5. Migration reform and the probability of obtaining a local *Hukou*

A key assumption of this study is that the migration reform does make it easier for migrants who are agricultural *Hukou* holders to obtain a local *Hukou* in the destination. Unfortunately, I do not have annual data on the number of migrants obtaining a local *Hukou* for each city and thereby cannot test this assumption directly. Nevertheless, I can still indirectly check the validity of this assumption using the annual number of local nonagricultural *Hukou* holders. Changes in the annual number of local nonagricultural *Hukou* holders may come from a number of sources. The first source comes from natural population growth. The second source comes from migrants who obtain a local *Hukou*. The third source comes from out-migrants who obtain a local *Hukou* in other cities. The last source comes from local natives who convert from an agricultural *Hukou* to a nonagricultural *Hukou* due to reasons such as land expropriation. I can control for the first source and merge the second and third sources together as net migration inflows. However, I do not have information on the number of people who convert to a nonagricultural *Hukou* due to land expropriation.¹⁶

¹⁶ I resort to an additional survey data to examine whether land expropriation accounts for a majority of new urban *Hukou* holders. According to Chinese Household Income Project (CHIP) in 2002, the proportion of urban

I examine whether the migration reform affects the number of local nonagricultural *Hukou* holders. I use China City Statistical Yearbook (2000-2006) to construct the annual number of nonagricultural *Hukou* holders from 1999 to 2005. The estimation approach is the same as before. Table A8 shows the impact of the migration reform on the number of local nonagricultural *Hukou* holders. I find that adopting the migration reform with an average treatment exposure increases the number of nonagricultural *Hukou* holders by 8.6%. Therefore, I do witness a significant increase in the number of nonagricultural *Hukou* holders after adopting the migration reform, reducing the concern that the migration reform did not work as it intended.

Table A8 Impact of the migration reform on the number of nonagricultural *Hukou* holders

	(3)
Treatment effect	0.119 (0.029)
Observations	1759

Notes: The table shows the impact of the migration reform on the number of nonagricultural *Hukou* holders. Standard errors are clustered at the city level. Standard errors in parentheses.

Hukou holders caused by land expropriation to total urban *Hukou* holders was 1.24%, which increased to 6.4% when ignoring people who were born with an urban *Hukou*.