TI 2024-020/VIII
Tinbergen Institute Discussion Paper

Industrial Transfer Policy in China: Migration and Development

Michiel Gerritse[1]
Zhiling Wang[2]
Frank van Oort[3]

1 Erasmus University Rotterdam and Tinbergen Institute

2 Erasmus University Rotterdam and Tinbergen Institute

3 Erasmus University Rotterdam and Tinbergen Institute

Industrial Transfer Policy in China: Migration and Development*

MICHIEL GERRITSE†
Erasmus University Rotterdam, Tinbergen Institute

ZHILING WANG
Erasmus University Rotterdam, Tinbergen Institute

FRANK VAN OORT
Erasmus University Rotterdam, Tinbergen Institute

March 21, 2024

Abstract

China's Industrial Transfer Policy (ITP) is a novel place-based development policy of unprecedented scale. The policy targets a set of inland cities aiming to i) grow them in size and ii) restructure them into manufacturing hubs. These cities would eventually relieve pressure in China's coastal manufacturing hubs. We use a detailed migrant survey to estimate the impact of ITP on targeted cities by matching cities on policy assignment propensities. The ITP status led to a rapid but short-lived growth of migrant inflows up to 60%, representing 2 to 7 million internal migrations. Migrants in manufacturing and from coastal origins show stronger migration and wage responses. However, high skilled migrants respond less elastically, and migrant employment in manufacturing is offset by the exit of native workers. Additionally, manufacturing industries in targeted cities show no development in terms of output, pollution or production strategies. The ITP expands the population of targeted cities, but the evidence for a restructuring of the cities is weak.

Keywords: migration, urbanization, development, wage, place-based policy, China.
JEL-codes: R58, H50, O20, P25, J38

1 Introduction

In emerging economies, place-based policies are rapidly gaining importance as instruments of development. Place-based policies fuel the growth of cornerstone cities and regions by providing them with infrastructure, investment, and favorable regulation. Special Economic Zones, for instance, are ubiquitous across the leading cities in China, India, and Africa. However, place-based policies for national development in developing countries have recently shifted to cover large areas and vast numbers of people. They employ integral sets of instruments in "big-push" interventions, aiming to direct the urban system instead of single cities or areas (Duranton and Venables, 2021; Grover et al., 2022).

*We thank Siqi Zheng, Hans Koster, Clément Imbert and participants in Rotterdam, Groningen, Utrecht, Barcelona, Stavanger, A Coruna, Guangzhou, Beijing, the Chinese Economic Society, the Cambridge Pembroke Chinese Migration Workshop, the Urban Economics Workshop at Peking University, and the European Economic Association (Barcelona) for comments that helped improve the paper.
†Corresponding author. Email *gerritse@ese.eur.nl*; Burg. Oudlaan 50, 3062 PA Rotterdam.

In 2010, the Chinese government initiated the Industrial Transfer Policy (ITP) in an effort to guide China's national development. The motivation was to establish a set of inland manufacturing cities that could host a transfer of economic activity from congested coastal areas. Year by year, groups of China's central and western cities received "industrial transfer model zone" statuses. The ITP status entitles the cities to preferential government treatments, infrastructure investments, and credit, amongst other benefits. At the same time, coastal cities were encouraged to evict low-grade or polluting manufacturing industries with subsidies. The Chinese Industrial Transfer Policy is the first and largest of its kind so far, playing a central role in recent planning directives in China. On repeated occasions, the Chinese Premier Li Keqiang has emphasized that "the western regions should take large responsibilities in receiving industrial transfer from the eastern regions".[1] No formal figures have been published regarding the budget or quantifiable targets, but news coverage suggests that ITP statuses come with multi-billion dollar spending packages, and the fourfold increase in investment in China's central provinces during the policy years is often interpreted as a result of the Industrial Transfer Policy (Ang, 2018).

The two central aims of the ITP are to increase the size of selected inland cities, and to upgrade those cities to manufacturing centers in the Chinese urban system. Indirectly, a new set of manufacturing hubs would relieve congestion and wage pressures in China's coastal cities, thus enabling the development of new industry on the coast. The ITP stands out from most place-based policies in developing economies (Neumark and Simpson, 2015) for two reasons. First, the scale of ITP is unprecedented, as it covers millions of people and has ambitions to change China's urban system in the long run. Hence, ITP is a "big push" policy, unlike most place-based policies, which are often highly localized in zones. Second, the cities targeted in ITP are distinctly not the economically leading areas of China. Virtually all other place-based policies in developing economies stimulate leading areas, fostering agglomeration benefits or technological spillovers, for instance. Hence, ITP reflects a change from China's broadly successful place-based policies, such as Special Economic Zones and Science Parks.

Previous studies on place-based policies do not offer clear guidance on the anticipated effects of the Industrial Transfer Policy (ITP). In China, place-based policies mainly took the form of Special Economic Zones (SEZs) located in leading coastal and border areas. SEZs and the related industrial parks bring about increases in local output, investment, and employment (Kahn et al., 2021; Lu et al., 2019; Alder et al., 2016; Wang, 2013), indicating that place-based policies have the potential to accelerate local development in China. However, ITP differs from SEZs as it targets much larger inland areas and results in migration towards lower-ranked cities. Since China has significant migration restrictions and is often considered less urbanized than desired (Zilibotti, 2017; Meng, 2012; Au and Henderson, 2006a), such migration could result in the loss of agglomeration benefits and allocative efficiency, compared to policies that target the large coastal cities. In most place-based policies, worker movements typically lead to substantial inefficiencies in the labor market (e.g., Koster et al., 2019, on Chinese policies). In the case of ITP, by contrast, migration might not signal inefficiencies, as targeted spatial migration is an explicit objective of the policy. The "transfer" of people is the intended mechanism of structural change. However, there is only emerging evidence that migration or its guiding policies promote productivity or industrial specialization (e.g., Hao et al., 2020). The existing evidence instead primarily points to investment incentives for local manufacturing plants as successful local development policies, such as SEZs and larger-scale programmes such as the Third Front, an industrialization initiative in China's interior from 1964 to 1971 (see, e.g. Fan and Zou, 2021). Furthermore, most of the available evidence

[1] See the following news coverage for example. (1) Li Keqiang: Industrial Transfer Promotes Chinese Economy to Upgrade. https://www.gov.cn/xinwen/2014-06/25/content_2708165.htm (2) Li Keqiang in Hunan: Speeding up Exchange of New and Old Systems to Enhance Development. http://www.xinhuanet.com/politics/2018-06/12/c_1122975698.htm (3) Li Keqiang: The Middle and West Regions Should Create Better Conditions to Take Over Transferred Industries from the East. https://www.gov.cn/xinwen/2019-10/15/content_5440235.htm (4) Li Keqiangin Ningxia: Deepening the Development of the Western Regions. https://www.gov.cn/ldhd/2012-09/13/content_2224387.htm

relates to development policies of a much smaller geographical scale than ITP. The extensive geographical coverage and the diverse targets of the ITP, including the industrial structure, demography, and development, qualify ITP as a "big push" (Duranton and Venables, 2021). Its evaluation requires different identification than small scale policies do (Neumark and Simpson, 2015). As studies of place-based policies predominantly cover smaller policies and use different methodologies, the existing economic literature does not provide clear guidance on the impacts of the Industrial Transfer Policy.

This paper examines how the Industrial Transfer Policy impacts migration and development in its targeted cities. Using a representative survey of over one million internal migrants in China from 2010 onward, we estimate how the assignment of an ITP status changes the number and type of migrants arriving in targeted cities, and the consequences for the industrial and economic development of the cities. The survey's high level of detail enables us to estimate the destinations, origins and compositions of inter-city migrant flows. To explore the impact of migration on industrial structure and production in destination cities, we link the survey with several other data sources.

We identify the causal impact of Industrial Transfer Policy status assignments in two steps. First, we match targeted cities with cities that were similarly likely to receive the status, but never received it. In line with the policy's stated aims, the best predictors for an ITP status assignment are the city's sectoral structure, wage levels, output, and the precise industry specialization pattern. Second, we estimate how migration outcomes differ between ITP cities and their closest matches. ITP cities are similar to their matched cities in observable characteristics, and we find no outcome differences before the status assignment is announced. The detailed information collected from the migrant surveys permits a rich fixed effect structure that enables us to eliminate confounding explanations for migration. We control for common developments that also affect control cities with yearly fixed effects for the pair of matched cities. By differencing out time-invariant explanations of migration at the origin-destination pair, such as distance, cost of migration and cultural similarities, we can also control for the location and geographical accessibility of the destination city. We also difference out origin-year fixed effects to eliminate confounding push factor shocks, such as overcrowding, political change, coinciding policies that might attract migrants away from their origins, or province-level urbanization plans that might otherwise explain our results. Furthermore, we incorporate destination city fixed effects to identify within-city year-on-year shocks to migration to avoid that time-invariant pull factors, such as location and initial industrial conditions, explain our results. The detail in the migrant survey also enables us to study the heterogeneity of policy impacts across industries and the locations of people, which characterizes place-based policy impacts (Becker et al., 2013), and specifically ITP.

We find considerable increases in migration inflows after cities receive an Industrial Transfer Policy status. The migrant inflow rises by around 60% soon after a city receives the ITP status. Embedded in a migration choice model, our estimates imply that approximately 2.3 million migrants changed their destination, and that an additional 5.1 million people became migrants due to the policy. The structural interpretation suggests that many migrants originate from coastal areas, relative to the origins' native population.

The industrial development in targeted cities following the migrant inflow is more limited. We find evidence that larger shares of the migration flows into ITP cities end up in manufacturing than before the policy, specifically if the migrants originate from the coasts. These groups also earn higher wages. However, the shares of highly skilled migrants in the migration flows do not rise despite steep wage increases, suggesting an inelastic response in this targeted group. In the aggregate, the inflow of migrant manufacturing workers is small, and it is counteracted by native workers' exits from manufacturing industries. As a consequence, the manufacturing employment shares decline in the ITP cities. We find minor impacts of ITP on official GDP or GDP per capita, and confirm that this response is limited in (official and satellite-based) nightlight and pollution measures of targeted cities. Neither do firms and startups show upgrades in size, capital intensity or productivity. Taken together, the most prominent consequences of the Industrial Transfer Policy are in the substantial relocation of workers into a second-tier range of cities, while the evidence of industrial upgrading of the targeted cities is limited.

Before laying out the main analysis, we detail our contribution to the literature and the background of the Industrial Transfer Policy.

2 Related literature

A growing literature evaluates the extensive place-based strategies that shape China's economy. Most Chinese place-based policies, including special economic zones and science parks, have a relatively small-scale focus. They generally improve productivity in the targeted areas and increase human and physical capital investments and output (Wang, 2013; Alder et al., 2016; Koster et al., 2019; Lu et al., 2019). Chinese place-based policies commonly target leading local areas, but may generate considerable spatial inefficiencies: science parks come with dead weight losses from the relocation of workers (Koster et al., 2019), and industrial parks create unintended production and consumption subcenters, giving rise to edge cities (Zheng et al., 2017).

ITP has a larger geographical coverage and affects more people than local policies do. Very few papers evaluate Chinese policies of such a large geographical scale. Most closely related, there is emerging evidence on the Great Western Development Programme (GWD), which started in 2000 and covers over a quarter of the Chinese population (Jia et al., 2020). The GWD increased output and industrialization in Western provinces, but not productivity and employment. Our findings differ in that ITP induces migration and those migrants find employment, but we find little upgrading or capital intensification. A potential explanation for the different outcomes is that the policies use different instruments, as the GWD focuses on capital investment, while ITP focuses on the transfer of people and activity between regions.

The evidence for the effectiveness of comparable place-based policies outside China is small too. Yet, many developing countries are pursuing multi-region place based policies (Duranton and Venables, 2021; Grover et al., 2022) - examples include the Argentinian Plan Belgrano, which redistributes economic development in ten provinces simultaneously, and the Upper Egypt development programme. In terms of size, the Chinese ITP compares to regional cohesion policy of the European Union, the largest concerted effort in the world to achieve territorial cohesion, with a larger evidence base (Barca et al., 2012). Similarly, the policies reflect limited budget at the macro-level (Cohesion Policy is around 0,31% of EU GDP), but have considerable local imprint: for instance, Cohesion Policy replaced national investment policies in regions in Hungary, the Baltic States, Romania, Bulgaria and Portugal completely. However, the focus in Europe is on underdeveloped regions (Diemer et al., 2022), while the cities targeted in ITP are performing well within their regions. Moreover, the more extensive results from evaluations of place-based policies in advanced economies do not generally translate well to developing countries such as China, because the targeted areas in China often have different stages of industrial development, financial needs, and institutional settings than the areas studied in related literature (Neumark and Simpson, 2015).

Methodologies to evaluate small-scale place-based policies do not generally work for larger scale policies such as ITP. Identification strategies that use spatial discontinuities, narrowly defined control areas, or restrictions in eligibility (Neumark and Simpson, 2015; Di Cataldo, 2017) do not provide suitable counterfactuals for the city-level selections in ITP. Our paper proposes a matching strategy for larger scales to consider several of the stated aims of the policy as outcome (Schweiger et al., 2022). Cast in a migration model, the matching approach is novel in this literature.

The effectiveness of ITP policies is closely tied to a related literature that questions whether migration can be a vehicle for development and industrial upgrading. In the context of international migration, migrants can increase the size, productivity and innovation rates of local skill-intensive firms, attract new firms and generate higher wage jobs (e.g., Beerli et al., 2021; Peri, 2012; Gray et al., 2020). The evidence of such upgrading tends to be mixed, depending on the approach employed (Dustmann et al., 2016). For internal migration, there is evidence of a role of migrants in development. In China, internal migrants contribute strongly to local productivity compared to local workers, and their employment shows little substitution for local workers (Combes et al., 2015, 2020). Moreover, internal migrants are a driving force of the clustering of highly skilled workers (Fu and Gabriel, 2012). Internal migration is central to China's structural change from agriculture to industry, and the cause of rising wages and house prices in China's largest cities (Hao et al., 2020; Garriga et al., 2017).

In contrast to related literature, we find little evidence of production upgrading and industrial change driven by migration. One possible explanation is that we focus on migration specifically encouraged by a policy, whereas virtually all other literature describes migration by choice, which is often in spite of China's sizable migration restrictions. The far larger evidence base on relaxation of hukou restrictions documents that manufacturing industries in large cities are fueled by internal migration (Hao et al., 2020). However, the ITP encourages the opposite movement: workers from cities with more advanced manufacturing sectors move into smaller cities with less advanced manufacturing sectors. Hence, rather than being attracted to established manufacturing clusters, workers under the ITP are expected to bring manufacturing knowledge or experience to targeted cities with smaller manufacturing bases. A second difference with most studies on Chinese migration is that our analysis employs a comparatively detailed source of individual migration data that permits controlling for an extensive set of explanations of migration. These explanations include, for instance, cultural and institutional differences between the origin and destination, as well as unemployment rates or other economic shocks at the origin of the migrant that encourage out-migration. Therefore, our empirical findings can be explained by direct policy incentives, rather than the push-factors of migrants.

Finally, our study connects to the question whether the Chinese urban system is optimally organized, as the Industrial Transfer Policy relocates millions of people. The allocation of the Chinese population over cities has significant aggregate growth consequences: the size of cities affects the aggregate allocative efficiency, the aggregate rate of technological progress, and the scale economies in production, for instance (Rossi-Hansberg and Wright, 2007; Henderson, 2005). Similarly, there is evidence that earlier (politically driven) urbanization has disciplined local markets and increased firm-level productivity (Bo, 2020). In China, the productivity effects of city size outpace those of the Western world (Chauvin et al., 2017). With approximately 163 million cross-city internal migrants in China[2], the potential benefits from agglomeration are large. However, Chinese migration policies may thwart an efficient allocation. Migrants experience social and financial frictions and frequently need to give up hukou rights when moving (Zhang and Zhao, 2013; Wang and Chen, 2019). The hukou system restricts the labor supply in larger cities, leading to an overemployment in manufacturing (Ngai et al., 2019) and likely considerable losses of aggregate output (Zilibotti, 2017; Meng, 2012; Au and Henderson, 2006a). Similarly, a relative undersupply of land in China's largest cities may have led to a reduction in aggregate productivity (Fu et al., 2021). Our paper provides no evidence on the Chinese urban system as a whole, as it does not evaluate productivity effects in the comprehensive set of Chinese cities. However, the results based on the targeted cities caution against expectations of development in a second-tier rank of cities: local wages do not improve, and the transfer of output, productivity and economic structure is limited.

[2]This value is taken from official statistics in 2017, which is 1 year after the analysis period. The total number of internal migration is 244 million, among which 67% are cross-city migration instead of cross-county within-city migration.

3 Industrial Transfer Policy

China's development since 2000 has been spatially uneven. Since China's entry into the World Trade Organization, its coastal areas, with urban agglomerations such as Pearl River Delta, have experienced rising manufacturing output, paired with wage rises driven by capital and technological change as well as migration into big cities (Ge and Yang, 2014; Ma and Tang, 2020). Over the last two decades, however, rising wages and prices on the coast have driven people and industry inland in search of lower production cost, lower land prices, and lower congestion. The size of the inland migration flows and the corresponding move of firms and industries have led the Chinese government to identify inland migration as one of the central planning challenges. As people and industries moved westward simultaneously over the last decade, planners have coined the term 'industrial transfer' to describe the inland flows. Planners view the geography of industrial transfer as a domestic version of the Asian "flying geese model", in which manufacturing sectors filter down and travel westward to economically less advanced areas (Ang, 2018). At the same time, eastern coastal areas have upgraded their manufacturing industries by concentrating high-performance clusters and allowing less desired industries to leave. This is in line with economic rationales to outplace standardized and mature industrial functions and focus on knowledge intensive industries at earlier stages in their product life cycle (Vernon, 1960; Thompson, 1968; Duranton and Puga, 2001).

Against this background, the migration of capital and investment out of Chinese coastal areas into central provinces has played a pivotal role in Chinese policy since 2010. The inception of this migration, however, can be traced back further; China's eastern coastal regions have historically outperformed other regions economically. The differences were exacerbated by export-oriented growth following a 1979 economic reform that cultivated open cities and special economic zones on the coast. By the end of 1999, the eastern regions alone accounted for over half of the nation's GDP. Not long after, these growing disparities led to a government priority of harmonious regional development. In 2000, the central government hatched the "Great Western Development Strategy" soon to be followed by the "Northeast China Revitalization" in 2003 and the "Rise of Central China" in 2006. The policies all involved a substantial number of infrastructure projects that sought to boost economic growth in these regions that lagged the economic development at the coast. The incentives for migration to inland areas embedded in these policies laid the foundation for industrial transfer, even though that goal was never explicitly articulated.

Industrial transfer was elevated to the highest policy agenda in 2010 (Ang, 2018). To promote industrial transfer from a national strategic perspective, the State Council issued the "Guidelines for the Implementation of the Industrial Transfer to the Central and Western Regions" (State Council of the People's Republic of China, 2010). As stated in the first paragraph of the Guideline, "The middle and the western regions should use their advantages of abundant natural resources, low factor costs, and huge market potentials to actively receive industrial transfer both domestically and internationally. This would speed up industrialization and urbanization in middle and western regions, coordinate harmonious regional development and promote industrial upgrading in eastern coastal regions to optimize the industrial specialization of labor."[3]

[3]Industrial transfer policy follows a blueprint from the coastal Guangdong Province. In 2008, Wang Yang, the Communist Party Secretary of Guangdong province, called for a switch from labor-intensive and polluting industries to high-end and high-tech industries. The more proverbial metaphor that accompanied the policy shift towards industrial upgrading was to "empty the cage and change the bird". In Guangdong, the aim was to free up much needed space in the Pearl River delta by encouraging less desired industries to move inland within the province. By 2010, as the central government more expressly recognized that rising labor costs were threatening China's position as a world factory, the Guangdong policy served as a prototype for the national development policy. The envisioned outcomes were to have both a sustainable and innovative coastal industry as well as to retain low cost stages of production chains. Within Guangdong province, there are some province-level industrial transfer zones, which are at a much smaller scale than the national-level industrial transfer zones studied as the treatment in this paper and unlikely to confound our analysis.

The goals in the guiding ideology show two distinct aims for the inland regions. The first is to attract more migrants and to accelerate the pace of urbanization. The urbanization of inland areas expands the number of cities that can harbor vast amounts of inland workers that have moved to the coastal manufacturing areas since the 2000s. The aim of maintaining the people's proximity to their homelands is a deep-rooted government objective (State Council of the People's Republic of China, 2014a, 2015) and is also enshrined in the hukou system that curbs within-country migration.[4]

The second goal is to promote industrialization and local development in the inland cities. In coastal areas, the demand for manufacturing goods has rapidly pushed up wages and production costs. Increasing the manufacturing capacity in the inland cities would preserve China's exporter status in such industries by reducing local production costs. In China, incoming manufacturing industries are regarded as sources of income, not least by local government officials. ITP status assignments were paired with the provision of subsidies and administrative access to designated cities in inland regions in the hope that plants would directly relocate from coastal areas to the targeted inland cities. The differences between the origins and destinations are such that the transferring industries can offer higher-grade production in the destination cities, even if these industries were comparatively low-grade or polluting in their location of origin. The reciprocal ambition was that the coastal cities would have more space to develop higher-grade and cleaner production.

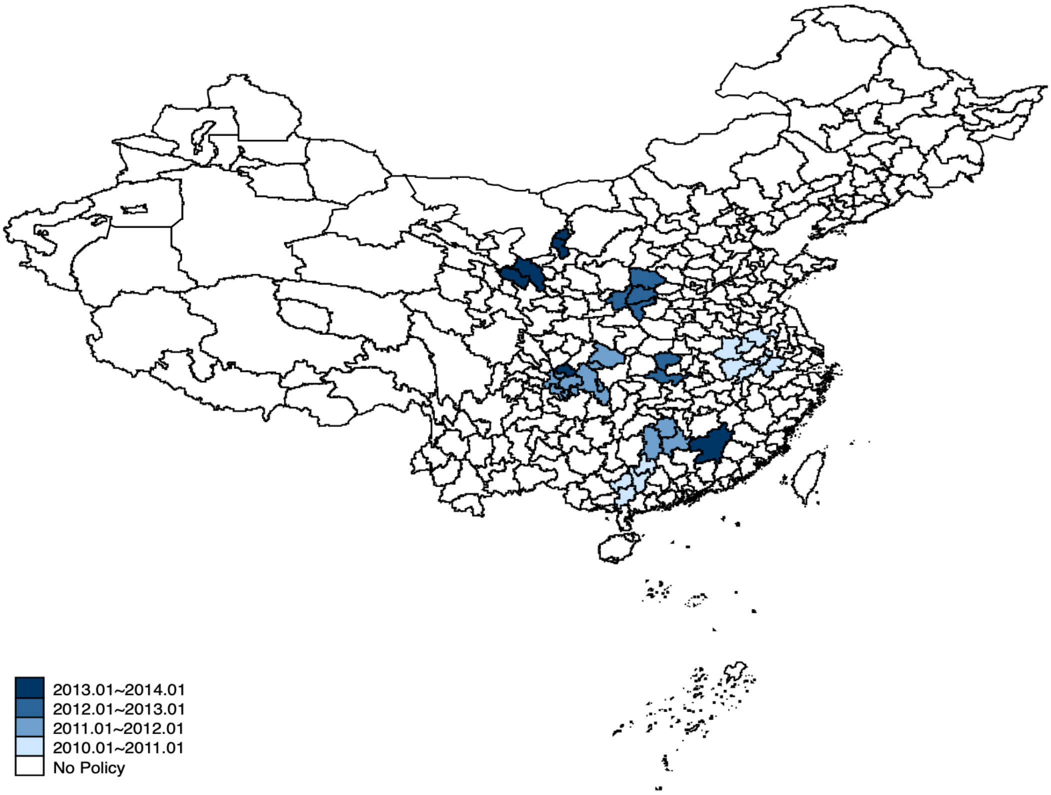
Between 2010 and 2014, the Chinese central government assigned 29 prefecture-level cities an ITP status in 10 zonal phases. The assignment was staggered and totaled approximately 338,700 square kilometers, mapped in Figure 1. All assigned cities were inland. No other official selection criteria were published. The set of targeted cities share a sizable manufacturing base and comparatively low migrant stocks, as implied by the policy objectives, but was varied otherwise. For example, the Jin Shan Yu zone contains four cities on the border of three provinces and is located near the Yellow River, putting it in an advantageous position for transport. In several other cases, the selected cities were central to a larger, less developed province, such as Ningxia. Some zones, such as the Anhui zone, may have been selected for their manufacturing base. Our auxiliary regressions to predict cities' ITP statuses show that the assignment coincides with manufacturing (or secondary sector) activity. Following the initial wave of assignments, three cities in Hunan received an ITP status in 2018, and in 2023, five cities were added in Inner Mongolia and Jilin. There are no migration data surrounding these more recent assignments.

The precise instruments of ITP are reported with far less precision than the policy's goals. A city that becomes a "recipient of a national industrial transfer model zone" assignment ("Guo Jia Ji Cheng Jie Chan Ye Zhuan Yi Shi Fan Qu") is supported by tax reductions, loans, lower entry level requirements for labor-intensive sectors, priorities in assigning industrial land, subsidies for processing trade enterprises and R&D transfers from 2010 onward. Although these items are centrally financed, their exact costs and their precise impacts on subsidies, wages, or credit provision are not publicly available. The amounts of funding is occasionally mentioned in secondary sources. Some examples include the cities of Hengyang (4 bln dollars), Chenzhou (3 bln), Yonzhou (3 bln), and Lanzhou (6 bln).[5] Across these examples, the expenditure amounts to around 900 dollars per capita. Groups of treated cities are reported to receive larger spending, with over 800 bln dollar cumulatively in the Wanjiang zone and over 200 bln dollar cumulatively in the Hunan province. For cities, an assignment under ITP is a coveted status. The local news, commercial groups and the industrial park websites consistently report on the status assignment

[4]Established in the 1950s, the hukou system records a household's official place of residence and classifies the household as either agricultural (rural) or nonagricultural (urban). At birth, people inherit the hukou status from parents, including both the hukou type and the place of registration.

[5]The cities for which spending is reported may have considerably different spending levels than the cities without reporting. The respective sources include: `https://news.sina.com.cn/o/2012-08-04/113424906626.shtml` and `http://www.achie.org/news/jkq/20150811276.html`. The sources for city groups are `http://tradeinservices.mofcom.gov.cn/article/difang/maoydt/202106/116919.html` for Wanjiang and `https://www.gov.cn/guowuyuan/2018-06/12/content_5298030.htm` for Hunan.

Figure 1: Location and announcement date of Industrial Transfer Policy areas up to 2014



Notes: The announcement time of the 10 Industrial Transfer Zones (including their city names) is as follows: (1) Wanjiang Zone (Hefei, Wuhu, Ma'anshan, Tongling, Anqing, Chizhou, Chuzhou, Xuancheng, and Lu'an) in January 2010; (2) Guidong Zone (Wuzhou, Yulin, Guigang, and Hezhou) in October 2010; (3) Chongqing Zone (Chongqing) in February 2011; (4) Xiangnan Zone (Hengyang, Chenzhou, and Yongzhou) in October 2011; (5) Hubei Zone (Jinzhou and Jinmen) in December 2012; (6) Jinshanyu Zone (Sanmenxia, Yuncheng, Linfen and Weinan) in May 2012; (7) Gansu Zone (Lanzhou and Baiyin) in March 2013; (8) Sichuan Zone (Guang'an) in April 2013; (9) Gannan Zone (Ganzhou) in June 2013; (10) Ningxia Zone (Yinchuan and Shizuishan) in January 2014.

process. Local government officials generally signal and publish favorable conditions and draft plans to attract the firms associated with the transfer. Similarly, there is a large coverage of the immediate investment conferences that link local government officials, the representatives from the chambers of commerce and the firm executives who manage transfer procedures.

4 Empirical Strategy and data

We use a standard gravity equation to examine the impact of ITP status assignments on migration. The regression equation explains the log of the bilateral migrant flows from the assignment of an ITP status in the destination city. We estimate the impact of the ITP status by using a treatment dummy ITP_{dt} for cities in the years in which there is an active policy. The coefficient of the dummy is a naive measure of the impact of an ITP status. Plausibly, cities that receive the ITP status are different in their capacity to attract migrants. They serve as regional centers or have specific industrial structures, for instance. Similarly, the ITP status announcement might correlate with shocks in the origins of the migrants who tend to migrate to the city. For instance, the targeted city may be near sources of migrants who are targeted by simultaneous policies, or proximity to areas with poorer economic performance might lead to larger inflows of migrants. To control for such confounding explanations of migration changes, we introduce a fixed effect at the origin-destination level (α_{od}) and at the origin-year level

(α_{ot}). Note that the origin-destination pair fixed effects also controls for time-invariant explanations of the policy assignment (as a city-level fixed effect would be subsumed in α_{od}).The equation is as follows:

$$\log M_{odt} = \beta ITP_{dt} + \alpha_{od} + \alpha_{ot} + u_{odt} \tag{1}$$

The estimate of β yields a naive measure of the policy's impact, as it merely provides a measure of the correlation of the policy with the migration flows, conditional on fixed effects.

A concern could be that the policy responds to city-level (time-varying) shocks that also affect migration. In that case, ITP_{dt} is endogenous and the estimate of β conflates the likelihood of receiving the policy with the impacts of the policy. Suppose that the policy targets some time-variant city characteristic l_{dt}, such that the likelihood of receiving the policy is $E\left(ITP_{dt}\right) = \gamma l_{dt} + v_{dt}$, where v_{dt} is I.I.D (note that time-invariant target characteristics are absorbed in the fixed effects). Hence the error term u_{odt} may correlate with ITP_{dt}, leading to an estimate of β as:

$$\hat{\beta} = \frac{cov\left(M_{odt}, ITP_{dt}\right)}{var\left(ITP_{dt}\right)} + \frac{cov\left(ITP_{dt}, \gamma l_{dt}\right)}{var\left(ITP_{dt}\right)}. \tag{2}$$

Here, the first term is the measure of policy impact, and the interpretation of β as a treatment effect requires that $cov\left(ITP_{dt}, \gamma l_{dt}\right) = 0$. That assumption is not easily tested, and it is not very plausible either: for instance, the ITP status might target areas that send out many migrants to coastal areas, for instance.

To tell apart the policy's impact from selection effects, we match every targeted city to another city that has a similar propensity to receive the ITP status. The strategy is a common city-level policy identification (Schweiger et al., 2022; Yu and Zhang, 2022) but we apply it in two steps, as we deal with dyadic migration observations. First, we estimate a logit regression for treatment as $ITP_d = \delta x_d + v_d$. We use pre-policy values to predict whether a city is ever assigned the ITP status. The covariates x_d used to predict treatment assignment are discussed in the next section. From the logit estimates, we obtain the predicted treatments, $IP\hat{T}_d$. We match every treated city to a comparison city that has the lowest difference in treatment probability, classifying them in bins indicated m (for "matches"). Within the treatment bin m, $cov\left(ITP_{dt}, \gamma l_{dt}\right)$ is now close to zero: conditional on the similarity between the treated and the comparison city, and on city, pair-year and origin-year fixed effects, the targeting of the ITP status no longer explains differences in the migration patterns. To check this assumption, we also verify that the treatment probability does not predict treatment conditional on the bin fixed effects (see Table B.3).

As a second step, we run the regression of on the sample of ITP cities and their matched cities, adding a yearly treatment-bin fixed effect:

$$\log M_{odt} = \sum_k \beta_k ITP_{d,t-k} + \alpha_{mt} + \alpha_{od} + \alpha_{ot} + u_{odt}. \tag{3}$$

In this equation, we omit the pre-announcement year as the reference category. The coefficients β_k measure the ITP city's additional expected migrants over the control city in year k relative to the policy announcement, compared to the year before the announcement of the ITP status and conditional on bin-year, origin-year and pair fixed effects. We estimate the gravity model as a pseudo-Poisson model to avoid non-random sample selection from origin-destination pairs for which zero migrants are observed (Santos Silva and Tenreyro, 2006). The treatment indicator ITP is not independent across observations because multiple migrant flows are destined to the same treated city. We adjust our standard errors by clustering at the destination city level in our baseline results.

4.1 Main variables

We primarily draw our data from the China Migrants Dynamic Survey (CMDS). The CMDS is the most recent large-scale micro dataset on migration in China. The surveys are targeted at migrants who, without having local hukou at the destination cities, have moved across county boundaries and have lived in their current city for at least 1 month. The questionnaire covers individual demographics, employment, income, health status, use of public services, etc. The respondents were between the ages of 15 and 60 at the time of the survey, and one respondent answered questions for the household unit. The sampling frame at the 31-provincial unit level was established by the annual report of the migrant population. The provincial units are allocated different sample sizes according to their rank at the migrant population level. The sample size of each provincial unit ranges from 2,000 to 15,000 observations every year. Within the provinces, the survey selected the respondents by using multistage, clustered sampling based on the probability proportionate to size technique. In total, the survey collected information from 128,200, 158,556, 198,795, 200,937, 206,000, 169,000 and 169,989 respondents for the years 2011, 2012, 2013, 2014, 2015, 2016 and 2017, respectively. In Appendix A, we verify the representativeness of the CMDS data by comparing the key statistics to the census data when the two overlap.

We define a person to be a migrant if he or she moves across cities.[6] From the surveys, we construct the year-by-year migration flows. We extract the surveys of workers who had migrated within the last three years at the time of the survey. Based on sampling weights for the years 2008 to 2017, we construct statistical estimates of the migrant inflows. In this way, 375,499 migrant workers are represented in 90,210 (31 provinces, 291 prefecture-level cities, and for a period of 10 years) origin-destination-year cells in total.[7]

We use surveys based on recent migration as this minimizes sample attrition. If a migrant moves into a city in one year but leaves in a second year, then he or she is not registered in a survey in the third year. It is plausible that workers engage in such short stays for short-term jobs, as they typically have to give up hukou rights in the location of work. The choice to limit the sample to recent migrants exclusively implies dropping approximately 56% of the original sample. Extending back the sample in time to include more observations does not qualitatively alter our conclusions, suggesting that attrition is not a first-order problem in representativeness and that the sample of recent migrants is representative.[8]

Regarding supplementary data, we assemble various city-level outcome variables from China City Statistical Yearbooks (GDP, wage, employment), Annual Survey of Industrial Firms (firm-level revenue, productivity) and Defense Meteorological Satellite Program (nightlights intensity).

4.2 Matching cities

The identification of the ITP impacts requires that treated cities are paired with sufficiently similar cities in a comparison group. We discuss our strategy and main diagnostics in brief in this section and refer to more extensive results in Appendix B.

Our strategy is to predict the ITP assignment from a logit regression and to pair every ITP city with a city that had a similar likelihood of receiving the ITP status that year. We focus on the status assignments' stated criteria comprising the level of development and the sectoral orientation: wage

[6] As we aim to evaluate city-level development outcomes, we focus migration measures on the cross-city migrants as well and remove cross-county within-city migrants from our analysis.

[7] A concern could be that for smaller flows, there is inaccuracy or oversampling. To examine the potential sensitivity of our results, we have rerun our main results setting the smallest observed flows to zero. Varying the definition of a small flow between the smallest 1% and the smallest 15% of the flows in the data, we find very minor changes in our results (at most around 6% of the coefficient magnitude).

[8] In Appendix C, we show the sensitivity of our main results to lengthening or shortening the horizon for inclusion as recent migrants.

levels, output levels per person, and secondary sector employment shares. We include the squared and interaction terms to accommodate a large set of plausible functional forms. In addition, we control for plausible correlates from policy documents: absolute (log) employment size of the area, tertiary sector sizes (and implicitly for the primary sector employment shares), the distance to the coast (squared) is used to account for distances to plausible migrant origins, and the urban employment distributions across eight sectors.[9] We use cross-sectional values from the City Yearbook from before ITP to predict later ITP status assignments. The equation to predict ITP status assignment at city d is as follows:

$$ITP_d = \beta_l \left[X_d\right] + \beta_i \left[X_d\right]^2 + \gamma Z_d + u_d, \tag{4}$$

in which $[X_d]$ refers to the set of log wages; log GDP per capita and secondary sector employment shares, and $[X_d]^2$ is the full set of interactions and squared terms within that set. The covariates added linearly are in set Z_{dt}.

The predicted probabilities of receiving the ITP status are on average 27% for the ITP cities, ranging up to 64%. That is significantly higher than the unconditional probability of 10%. The interactions in the covariate set do not facilitate an easy interpretation. When we estimate a linear assignment equation to facilitate the interpretation, the coefficients are consistent with the policy objectives. Cities that specialize in the secondary sector are more likely to receive treatment (indeed, dropping the variable that describes the size of the secondary sector from the equation leads to a positive coefficient on manufacturing employment). Lower GDP per capita is associated with higher treatment probability, although that may be partly offset by the (correlated) wage levels. The distance to the coast shows no direct significant contribution to ITP status assignment, though has noticeable impact on the treatment propensity distribution when removed (see Figure B.1). Overall city size as measured by employment has an insignificant association with the treatment. Specialization in service-oriented industries (ICT, finance, communication) shows no impact on treatment probabilities, and neither does the tertiary sector's share in a city's GDP. Mining is negatively associated with treatment probability. Table B.1 shows the coefficients for the baseline matching equation. Table B.2 explores the contribution of individual covariates by considering how omitting them changes the explanatory power, the set of original cities, and the propensity score differences within each set. The log GDP per capita and the share of employment in the secondary sector lead to the largest drops in precision when omitted from the covariate set. Additionally, to compare the distributions of status assignment probability between ITP cities and their matched cities, Figure B.1 plots the density functions of assignment probabilities for ITP cities and the sets of matched cities obtained using different types of controls. Omitting wages or GDP per capita in particular leads to fewer matched cities with high probabilities of status assignment.

Table 1 shows the balance of covariates and outcomes for ITP cities and their control cities for the year 2010, with the full sample statistics for comparison.[10] In the migrant surveys (panel a), compared to the full sample, the ITP and matched cities show relatively low levels of migrant inflows (and the standard deviations of treated and control cities a lower than the full-sample standard deviation). For the same groups, panel (b) shows the city-level characteristics, extracted from the China City Statistical Yearbook issued by the National Bureau of Statistics (NBS). Compared to the full sample, the ITP and matched cities show lower levels of GDP and wages, lower migrant stock and low pre-policy growth of migrant flows, and somewhat lower distances to the coast (though well within a standard deviation across the groups).

Additionally, we verify that treatment does not predict treatment propensity conditional on match-

[9]For other policies, there is some evidence that assignments are tied to political and social connections of local leaders (e.g., Yao and Zhang, 2015). Exploiting the institutes of education of local secretaries, we find no evidence that personal links through institute of education predict ITP status assignments.

[10]Maps of the matched city pairs are available from the authors *and supplemented for review at the end of the manuscript*.

Table 1: Descriptives

Panel a: bilateral city-pair characteristics of migration flows

	(1) Full		(2) ITP		(3) Matched	
	mean	sd	mean	sd	mean	sd
Migrant flows	4.36	32.23	1.00	7.20	2.82	15.24
Migrant flows (rescaled)	4361.04	32231.71	1001.76	7200.19	2816.21	15236.87
Zero migrant flows (share)	0.68	0.47	0.66	0.48	0.76	0.43
Migrants with college diploma (share)	0.04	0.15	0.04	0.14	0.03	0.14
Migrants with high school diploma (share)	0.11	0.26	0.13	0.28	0.08	0.23
Migrants working in affected industry (share)	0.10	0.25	0.09	0.24	0.08	0.23
Rural migrants (share)	0.25	0.41	0.28	0.43	0.19	0.38
Migrants older than 40 (share)	0.07	0.20	0.08	0.21	0.06	0.19
Observations	9021		899		899	

Panel b: city-level characteristics

	(1) Full		(2) ITP		(3) Matched	
	mean	sd	mean	sd	mean	sd
Secondary GDP share	50.86	10.65	51.83	9.11	50.91	9.23
Tertiary GDP share	35.63	8.74	34.07	5.41	34.75	6.28
Immigration 2010 ZHILING - what unit?	138.47	463.20	31.05	55.04	87.30	243.31
Immigration flow growth 09-10	88.70	296.42	19.48	39.87	55.76	160.59
GDP/cap	33388.97	22824.78	26311.21	15366.22	23961.38	11259.66
Wage	31339.40	7542.84	30642.49	5120.07	29476.39	5356.68
Distance to coast	618.36	483.87	529.66	346.87	560.09	358.30
Observations	285		29		29	

Note: Start of sample statistics (mean and standard deviations) for the full sample, the ITP cities and the cities matched to ITP cities. Panel a has more observations, because it shows statistics at the city-pair level.

ing. Table B.3 in the Appendix shows that unconditionally, treatment and treatment propensity are closely associated, but there is no such association within the matched sample or conditional on matched pair fixed effects. The results section also shows tests for outcome differences between ITP and matched cities preceding the policy, in migration in the years preceding the policy, the migration stocks in 2005, and related outcomes such as GDP, wages and domestic and cross-border investments.

5 Results on migration and development

5.1 Impact on migration flows

Table 2 presents the estimates of the impact of ITP on migration. The omitted reference group is ITP destination (t+1), which is the year before the ITP status is assigned. The outcomes of interests are ITP destination (t, t-1, t-2), which measure the the impacts on migration flows in the year of the status assignment and the two following years. The preferred specification in column 1 contains yearly fixed effects for the matched pairs, in addition to yearly fixed effects for the origin of the flow and a time-invariant destination fixed effect. The coefficients show a significantly higher migrant inflow in ITP cities one year after announcement, but not in other years. The inflow is around 60% ($e^{0.48} - 1$) higher than in the pre-announcement year.

Column 2 shows that before the announcement, there are no significant differences between the migration inflows of ITP cities and control cities. The coverage of the migrant survey combined with our definitions for observation restrict the pretrend test to two years before the policy announcement. In

Table 2: The impact of ITP on (log) migration

	(1) Main	(2) Leads	(3) Static	(4) GTATE	(5) Treated	(6)	(7)	(8) Stock	(9) Stock	(10) Stock	(11) Aggr.
ITP (t+2)		0.12									
		(0.16)									
ITP (t)	0.13	0.13			0.08	0.21	0.16	0.10	0.11		0.21
	(0.17)	(0.17)			(0.15)	(0.19)	(0.18)	(0.14)	(0.12)		(0.18)
ITP (t-1)	0.48**	0.47**			0.33**	0.47*	0.40*	0.28*	0.26**		0.47**
	(0.22)	(0.22)			(0.14)	(0.25)	(0.24)	(0.15)	(0.12)		(0.24)
ITP (t-2)	0.17	0.17			0.17	0.25	0.14	0.27*	0.23*		0.25
	(0.21)	(0.21)			(0.16)	(0.26)	(0.24)	(0.16)	(0.13)		(0.25)
ITP static			0.28**	0.27**						0.17*	
			(0.13)	(0.12)						(0.10)	
ITP 2005									0.17		
									(0.15)		
Observations	3,624	4,097	4,120	4,120	1,927	3,624	3,624	2,895	3,587	3,386	226
Fixed effects											
Match-year	yes	yes	yes	yes	no	yes	yes	yes	yes	yes	yes
Origin-year	yes	yes	yes	yes	yes	no	yes	yes	yes	yes	no
O-D	yes	yes	yes	yes	yes	yes	no	yes	yes	yes	no
Destination	no	no	no	no	yes	no	yes	no	no	no	yes

Notes. Estimated with a pseudo-Poisson model. "ITP" refers to the industrial transfer status at the destination. GTATE refers to a group-time average treatment effect. Treated refers to a sample of only treated cities. Stock refers to regressions that use the migrant stock as a dependent variable. Aggr. refers to a sample in which flows from different origins are aggregated by destination and year. The time indicator "t" indicates that the dummy is one in the year of an announcement. The indicator "t-1" refers to the status assignment in the year before (the coefficient measures the impact one year after assignment). Standard errors clustered at the city level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Appendix C, we confirm the absence of pre-policy differences when allowing for additional treatment lags. The impact of the policy also tends to fade after the first year since announcement, suggesting that population adjustments take place relatively quickly.

Column 3 shows the estimate when using a single, static treatment indicator: it identifies the average impact in the years after the ITP status assignment. At 0.28, the average impact is significantly below the peak of impact in the dynamic estimates from column 2, but higher than the first year and third year. The estimated coefficient might suffer from forbidden comparisons in the sense of Callaway and Sant'Anna (2021). The scope for bias is small, as our fixed effects strategy restricts to comparisons within matched pairs by year. However, the origin-year fixed effects in the bilateral migration model can still induce dependence between observations. In column 4, we report the single treatment indicator estimates identified within subgroups of treatment cohorts. The identification by cohort of treatment leads to very similar estimates.

Column 5 shows the estimates for the sample of treated cities. The estimates are not identified relative to a control city. The coefficients, here identified from within-variation in treated cities only, are slightly smaller than when a comparison to matched cities is made. This suggests that the counterfactual scenario from matched cities shows some declines in migration.

Columns 6 and 7 show results for alternative fixed effect strategies. The coefficient estimates show limited change when omitting origin-year fixed effects, or when substituting origin-destination fixed effects for destination-only fixed effects. The impact is estimated slightly less precisely. In unreported regressions, we use other fixed effects specifications from the migration literature: replacing origin-year fixed effects with year fixed effects, or for origin fixed effects and year fixed effects. These lead to very minor shifts in the estimated migration impacts of ITP.

Columns 8 to 10 of Table 2 consider the impact of ITP on migrant stocks, instead of flows. In column 7, a change in the stock is detected in the year after announcement, and the change persists into the second year after announcement. It is not surprising to observe longer impacts in stocks than in flows, as migrants who arrive after ITP announcements show similar spans of stay as in unaffected cities (see 5.1.1). As the spans of stay of migrants are relatively short (around 30% of migrants in our data arrived 1 year or less before observation), it is not surprising that the stock impacts are high relative to the flow impacts. The analysis of migrant stocks permits introducing an earlier lead of migrant stock from the 2005 census in the regression as an alternative pretrend test. We find no significant differences between cities within matched pairs in the 2005 migrant stocks.[11] Column 9, finally, shows estimates of a static treatment effect on the stock, at around 17%. The difference between the dynamic and static impact estimate is comparable between the flow and the stock estimates.

Column 11 of Table 2 shows the result for a regression at the city level, instead of the city-pair level. The dependent variable is the migrant flow aggregated by city-year. Fixed effects are included at the level of the city and the matched pair-year combination. The results for the city level are similar to those that take into account origin shocks and bilateral characteristics (e.g. column 2). The similarity in results suggests that the bilateral perspective, or the way we control for city-pair or origin-specific characteristics does not explain our main results.

We offer two additional tests for pretrends. Appendix C.1 uses a different migrant definition to test for more pre-policy years (as an alternative to our 2-year pre-policy test in the flows and the 2005 migrant stock cross-section in the census). We use the arrival year of observed migrants to estimate earlier inflows of migrants up to five years before the policy. This strategy is more sensitive to attrition in the local migrant stock, but we find no evidence of attrition differences for ITP cities (Appendix C). The regressions based on migrant flows by arrival year show similar impact estimates of ITP status announcements, but no significant differences between ITP cities and their matched cities in the five years leading up to the policy. Second, we examine investment into ITP cities, which can be observed for more years preceding the policies. The dynamic specifications in Figure J.2 of Appendix J show no differential investments in ITP cities preceding the ITP announcements, suggesting that while investors may respond to ITP, they do not do not anticipate their investment.

5.1.1 Migrant spans of stay

The result in Table 2 suggest that the rise in migrant inflow is short-lived. The impact on the stock of migrants could be more long-lasting, if migrants who respond to ITP policies stay longer in the host city than other migrants do. In Appendix C, we estimate whether the ITP policy has impacts on the length of stay of the migrants. The cohorts that arrive during or just after the city's ITP status assignment have similar attrition rates to migrants in general and to the migrants in the same years in matched cities, suggesting that ITP-induced migrants do not stay in their destination longer than other migrants do.

5.1.2 Inference under different standard errors

The standard errors for the coefficients of interest in Table 2 can be estimated using different approaches. City-level clustered standard errors, as reported in the Table, take into account the fact that multiple observations of migration flows experience the same policy exposure at the destination. As cities received

[11] We also explain the number of investment projects into cities from the ITP status in Appendix J. Up to four years before the policy, we find no pretrends in investment from domestic or foreign investors, or from investors from Honkong, Macao and Taiwan.

their status assignments in zones, which could justify adjusting standard errors for the zone-level assignment. Clustering standard errors at the zone level, in case cities are arguably treated as clusters, leads to very similar standard error estimates.

Arguably, the number of treated city pairs is limited by construction in this sample, as the potential number of cities in which such a policy can be applied is theoretically limited. To check the sensitivity to assumptions made on the asymptotics of the clusters, we re-estimate the standard errors by using a wild-bootstrap at the cluster level. Following Cameron and Miller (2015), we randomly sample clusters to construct a distribution of z-values for our coefficients of interest.[12] In the specification of interest (column 1), the estimated impact in the year of the announcement remains insignificant, and the coefficient for impact in the year after the assignment has a p-value of 0.02.

In addition, we report the standard errors from a randomization inference framework. For every bin, we randomly select one of the two cities for a hypothetical policy assignment, and then estimate the model. We construct a placebo distribution of the coefficients and z-scores out of 1,000 of such randomized assignments (the number of possible hypothetical allocations in 28 city pairs is over 250 million). The resulting distributions are presented in Appendix D. The coefficient for the impact in year of the announcement and two years after announcement are insignificant, while the p-value for the coefficient on the lag of treatment (1 year after announcement) is below 0.05 in the randomized distribution. Altogether, the differences between city-clustered standard errors and bootstrapped or randomized standard errors do not lead to different conclusions.[13]

5.1.3 SUTVA

Our identification requires the assumption that the control cities are unaffected by the ITP status announcement of the treated cities. This assumption is likely violated through general equilibrium effects. As migrants update their destinations, matched cities may receive fewer migrants. In section 5.1.7, we cast our estimates in a general equilibrium framework to quantify such diversions. Matched cities lose around 2% of their migrant inflows on average. This suggests that the general equilibrium effect, at less than 5% of the estimated impact, is not large enough to qualitatively change the conclusions.

In addition, we examine whether ITP cities draw migrants from similar origins as their matched cities. A high similarity could imply that ITP cities divert migrants who would otherwise have gone to their matched city, leading to an overestimate of the impact of the ITP status on migration. First, we construct measures by city pair of the similarity in the origin shares of their migrants (a more detailed explanation is in Appendix B). Then, we estimate whether ITP cities have significantly higher similarities in migrant origins with their matched city, than other city pairs do. Table B.4 shows the results, which provide no evidence of significantly higher similarity in migrant origins for matched cities than for unmatched cities.

5.1.4 Alternative matching strategies

To ensure that our results do not arise with the choice of a matching method, we explore alternative matching strategies in Appendix E. In particular, to circumvent instances in which the cities' propensity

[12]We employ the following steps: i) we estimate a constrained model with the coefficients of interest equal to zero; ii) we multiply the estimated errors by minus one for randomly selected clusters; iii) we construct a prediction of the migration flow based on the cluster-randomized errors and estimate the unrestricted equation by using the predicted migration flow as a dependent variable, saving the z-scores; and iv) we repeat steps ii) and iii) a thousand times to generate a distribution of the z-scores.

[13]Diagnostics on heterogeneity in treatment effects suggest that heterogeneity cannot revert our conclusions. The weights of treatment effects (de Chaisemartin and D'Haultfoeuille, 2020) for the one-year lagged ITP assignment dummy show a standard deviation of 0.005, which suggests a critical standard deviation of over 50 in the treatment effect for the coefficient estimate to be zero, which far exceeds our estimate of 0.08. It should be noted that this diagnostic is based on a linear setting, and our preferred estimate uses a Poisson function which approximates the linear estimate.

scores are similar but their covariates are not, we use coarsened exact matching based on GDP, wages and secondary employment shares to check the stability of the main results.[14]

The impact of the policy is comparable across different methods of matching ITP cities with non-ITP cities. Appendix E explores the use of different methods to match treated cities to control cities. Removing city pairs with very high differences (10 percentage points) in propensity leads to little change in the coefficient estimates. Next to matching based on observed characteristics instead of propensity (King and Nielsen, 2019), we also report results based on coarsened exact matching. We split the sample in quantiles for each of the main matching variables (GDP per capita, wage, secondary sector employment share) and take all cities with corresponding quantile combinations to be the controls group. Six quantiles in every dimension would lead to about 2 cities in every bin if randomly distributed, though we end up with slightly more due to the correlation between GDP and wage. With coarsened exact matching, as before, the coefficient for the first year after announcement is positive and significant, though pointing to a smaller impact on migration. Finally, we restrict each ITP city's set for possible matches to control cities that are outside the provice of the ITP city. A comparison between cities that are not in the same provinces prevents any localized spillovers from biasing the estimates upward, as migrant diversion from cities in the same province could otherwise affect the counterfactual. The coefficient estimates from this analysis are within the confidence intervals or our main specification and, if anything, somewhat larger.

5.1.5 Simultaneous policies and deviating governance

The ITP policies broadly coincide with other migration and development policies. A national reform in 2014 relaxed the registration of hukou for migrants in many middle-sized and small cities (State Council of the People's Republic of China, 2014b; Zhang et al., 2019; An et al., 2024). These do not plausibly affect our estimates, as they affect treated and control cities alike. In Appendix F, we also restrict to analysis to cities treated before 2014, and find very similar results. The most prominent state-led place-based policy that might confound ITP is the Great Western Development (GWD) Program starting in 2000, which targets all of the 12 provinces in Western China.[15] However, the GWD took place much earlier than the start of ITP. Moreover, the ITP targets cities, while the GWD the province level but indiscriminately within provinces. As a robustness check, we restrict our analysis to a sample of cities that are not covered under GWD and find very similar results (Appendix F).

Several cities that received an ITP status are under distinct governance, and might receive differential treatments. Chongqing is a central-governed municipality rather than a normal city regulated by a province. Ningxia and Guangxi are autonomous regions in which a large population of minorities reside. There are four cities in Henan Province, Shanxi Province and Shaanxi Province that are announced as comprising one ITP zone overlapping the three provinces; this zone is the only ITP zone involving multiple-province coordination. Additional analyses in Appendix F show that our results do not change when excluding such cities from the sample.

5.1.6 Origins of migrants

The migration responses may differ by the origins of the migrant due to different moving costs associated with distance. To check the spatial pattern of responses, we first reconstruct the sample by limiting

[14] An alternative would be to match the ITP cities to cities that receive the status later. However, given the limited set of cities and the uneven distribution of the numbers of ITP assignments over individual years, this would lead to a very small set of comparisons.

[15] The Northeast China Revitalization, starting in 2003 and targeting 3 provinces, and the Rise of Central China starting in 2006 and targeting 6 province, are more remotely similar. However, they we initiated significantly before the ITP policy and have conisderably smaller scales than the GWD (Jia et al., 2020).

to across-country within-city migration flows only to implement the baseline regression. Though these respondents are not defined as migrants according to the official definition, the significantly positive results in Table C.4 of Appendix C show that ITP facilitates the agglomeration of labor flow at the localized level.

Next, on a wider geographical scope with larger moving costs, we estimate our baseline regression (3) for migrants who move across cities, allowing the estimated migration response to vary with the distance of the origin of the migrant to the destination city. In Appendix C, Figure C.3 summarizes the estimated coefficient over 500 kilometer bands. The impacts are strongest in the range 500-1000 kilometers between the migrant origin and the destination: in that distance range, both the first after the ITP announcement show a significant coefficient around 0.5 with a p-value below 0.05. In the first year after the ITP announcement, the impact estimate has p-values below 0.1 at all range expect under 500 kilometers. The coefficient estimated for the range 500-1000km shows p-values below 0.10 for all years. Hence, the overall impact shows some concentration on the medium distance range and on the year after the policy announcement. Taking the results all together, we see very localized response within city and moderate responses roughly in 500-1000 kilometers range, which decay with an increasing distance.

5.1.7 The population level impacts of ITP assignments

How would the spatial population distribution of China have looked without the ITP policy? To quantify the change in the population distribution over Chinese cities, we embed the estimated impacts in an ”off-the-shelve” location choice model.

People consume housing and numéraire consumption goods and have a preference for the amenities in a given area. As a result, people develop a city preference based on wages, rents, and local amenities. In addition, depending on their origin, people hold idiosyncratic preferences about potential residential (destination) cities. The heterogeneity in idiosyncratic preferences regulates the elasticity of migration responses to changing wages and rents. We assume that location-specific preferences follow a Fréchet distribution.

Setup

The utility of a person is as follows:

$$U_{iodt} = b_{iod} a_{dt} c_{idt}^{1-\alpha} h_{idt}^{\alpha t} \tag{5}$$

where i is person-index; t is the time index; o is the place of origin and d (”destination”) is the city in which the individual lives. For a migrant, o and d will differ. The person values the consumption of a numéraire good c_{idt}, housing hi_{dt}, and amenities a_{dt} at the location of residence. The term b_{iod} is an individual preference shock parameter. The shock is allowed to vary by person (and by migrants of different origins) and the location of residence, which is termed a destination. People from different origins may derive different utility from living in the same location and consuming the same quantities. The preference shock could include, for instance, the distance, cultural and language differences or the portability of (hukou) rights between the areas.The preference shock parameter is Fréchet distributed: $F(b_{iod}) = e^{-B_{od} b_{iod}^{-\varepsilon}}$.

Optimizing the demand functions gives the indirect utility function:

$$V_{iodt} = \zeta \frac{b_{iod} a_{dt} w_{idt}}{r_{idt}^{\alpha}}, \tag{6}$$

in which ζ is a positive constant. For a given person from origin o, the probability that living in city d is optimal is equal to the probability that destination d yields the highest utility. Integrating over the idiosyncratic preference shock distribution gives the probability that d is the preferred destination as follows:

$$\pi_{odt} = \frac{B_{od}\left(a_{dt}\frac{w_{dt}}{r_{dt}^{\alpha}}\right)^{\varepsilon}}{\sum_d B_{od}\left(a_{dt}\frac{w_{dt}}{r_{dt}^{\alpha}}\right)^{\varepsilon}} = \frac{x_{odt}}{x_{ot}}. \tag{7}$$

This suggests that the desire to live in a location is a function of its amenity value and real wages relative to those of other locations. The shape parameter for the preference distribution allows the location choice elasticity with respect to real wage differences to vary: if ε is large, migrants are more sensitive to real wage differences between cities. If ε is lower, the term B_{od} gains relative importance: inherent preference about living in a city B_{od}, given the migrant's origin, has a larger impact on the migration choice. The term $x_{ot} = \sum_d B_{od}\left(a_{dt}\frac{w_{dt}}{r_{dt}^{\alpha}}\right)^{\varepsilon}$ serves as a multilateral resistance term. It creates a dependence between the location choices: even if a destination's wage, prices and amenities are unchanged, the migration flow will decrease if another alternative becomes more attractive and the denominator x_{ot} rises. At the population level, π_{odt} (the probability that od yields the highest utility out of all residential choices d) yields the stock of migrants choosing the od combination.

Estimating equation

The estimating equation encompasses a structural response of migration to the policy. Writing the migration in logs gives the following:

$$\log M_{odt} = \log B_{od} + \varepsilon\left(\log a_{dt} + \log w_{dt} - \alpha \log r_{dt}\right) - \log x_{ot} \tag{8}$$

in which the policy incidence at the destination d, operates through the term $\varepsilon\left(\log a_{dt} + \log w_{dt} - \alpha \log r_{dt}\right)$ – the log of x_{odt} — and origin-destination fixed effects and origin-year fixed effects control for the time-invariant migrant preference $\log B_{od}$ and for the time-variant multilateral resistance term $\log x_{ot}$. As the policy affects the term $x_{odt} = B_{od}\left(a_{dt}w_{dt}/r_{dt}^{\alpha}\right)^{\varepsilon}$, the analysis takes no particular stance on whether ITP acts on housing markets, labor markets or amenities in general.

Counterfactual

To understand how ITP policies structurally update location patterns, we link the estimating equation above (eq. 3) to a structural interpretation of the location model.

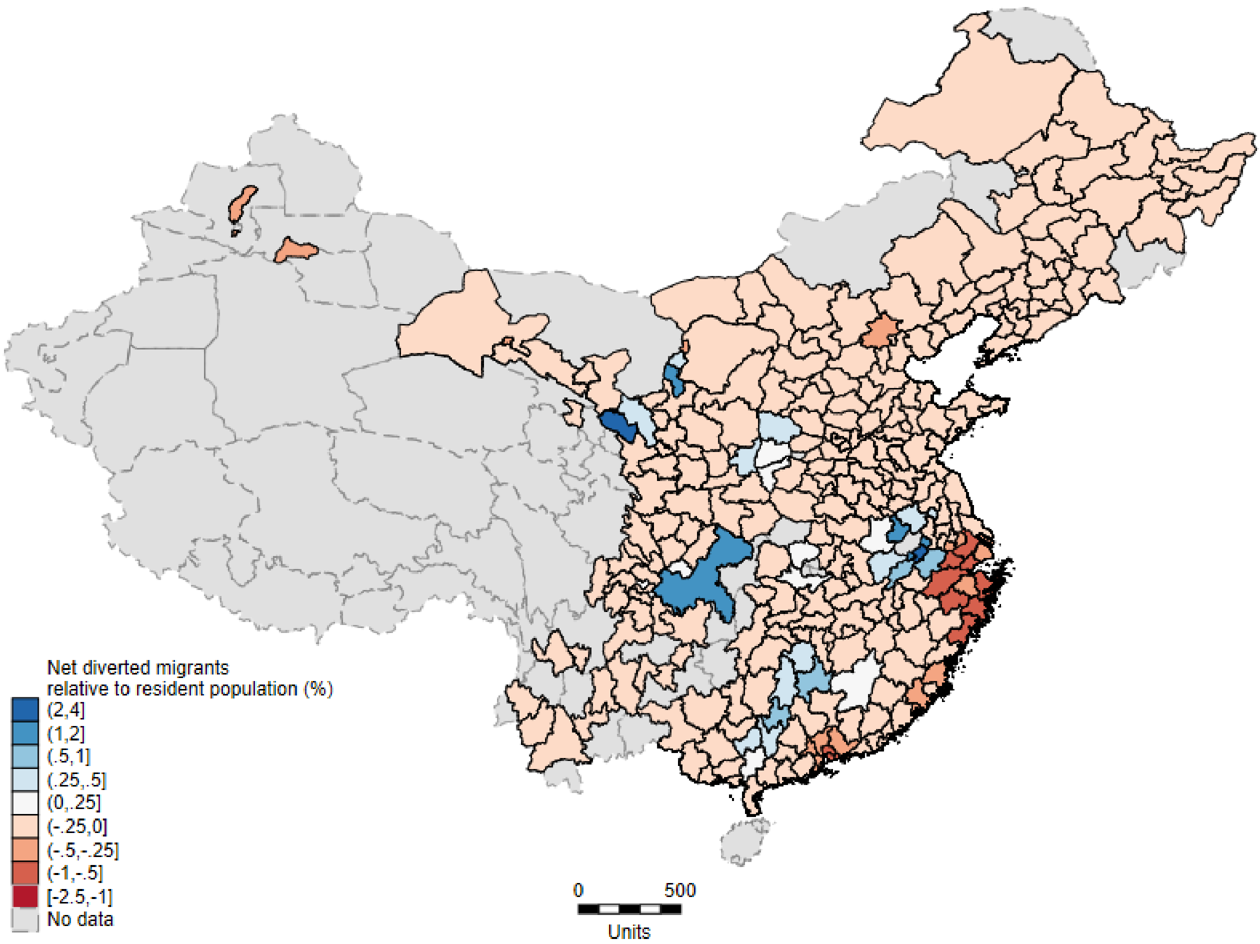
Migration choices show two margins of adjustment: the diversion of existing migrants and the generation of new migrants. By their definition, the migrant surveys only enable us to estimate the impact of the ITP from the choices of existing migrants. For that reason, we first develop a counterfactual estimate on the assumption that ITP only diverts people who already live outside their origin city. In the second part of this subsection, to estimate a counterfactual that allows for the inclusion of new migrants following the instatement of the ITP policy, we impose the parameter of the location choice model on non-migrants in the census data. As this analysis requires more data and assumptions, we detail it in Appendix G.

The estimating equation identifies the impact of ITP policies on $B_{od}a_d w_d/r_d^{\alpha}(= x_{odt})$, and differences out the term $\sum_d B_{od}\left(a_{dt}\frac{w_{dt}}{r_{dt}^{\alpha}}\right)^{\varepsilon} (= x_{ot})$ with a time-varying origin fixed effect for the sake of identification. However, the index x_{ot} is not constant when the policy changes. The term $a_d w_d/r_d^{\alpha}$ updates for some cities, and hence changes the choice set of all migrants. Accordingly, the index needs to be updated when simulating the city-level impacts of such a policy. Using $x_{od}^{observed}$ to shorten $B_{od}\left(a_d w_d/r_d^{\alpha}\right)^{\varepsilon}$ when there is no policy, the migration probability from o to d under the policy is $\pi_{od}^{cf} = \frac{x_{od}^{observed}e^{-\beta_1 ITP_{d,t} - -\beta_2 ITP_{d,t-1}}}{\sum_d x_{od}^{observed}e^{-\beta_1 ITP_{d,t} - -\beta_2 ITP_{d,t-1}}}$. Then, the predicted additional in-migration into a city when the policy starts is as follows:

$$\text{inflow}_d = \sum_o \left(\pi_{od}^{cf} - \pi_{od}\right) Pop_o. \tag{9}$$

where π_{od} is obtained from π_{od}^{cf} while setting the ITP_d indicators to zero. To interpret the counterfactual migrant flow, we scale the change in the inflow to the 2010 cross-section of the number of hukou holders per city.

Figure 2: City size changes from the diversion of migrants (as a percentage of hukou holders)



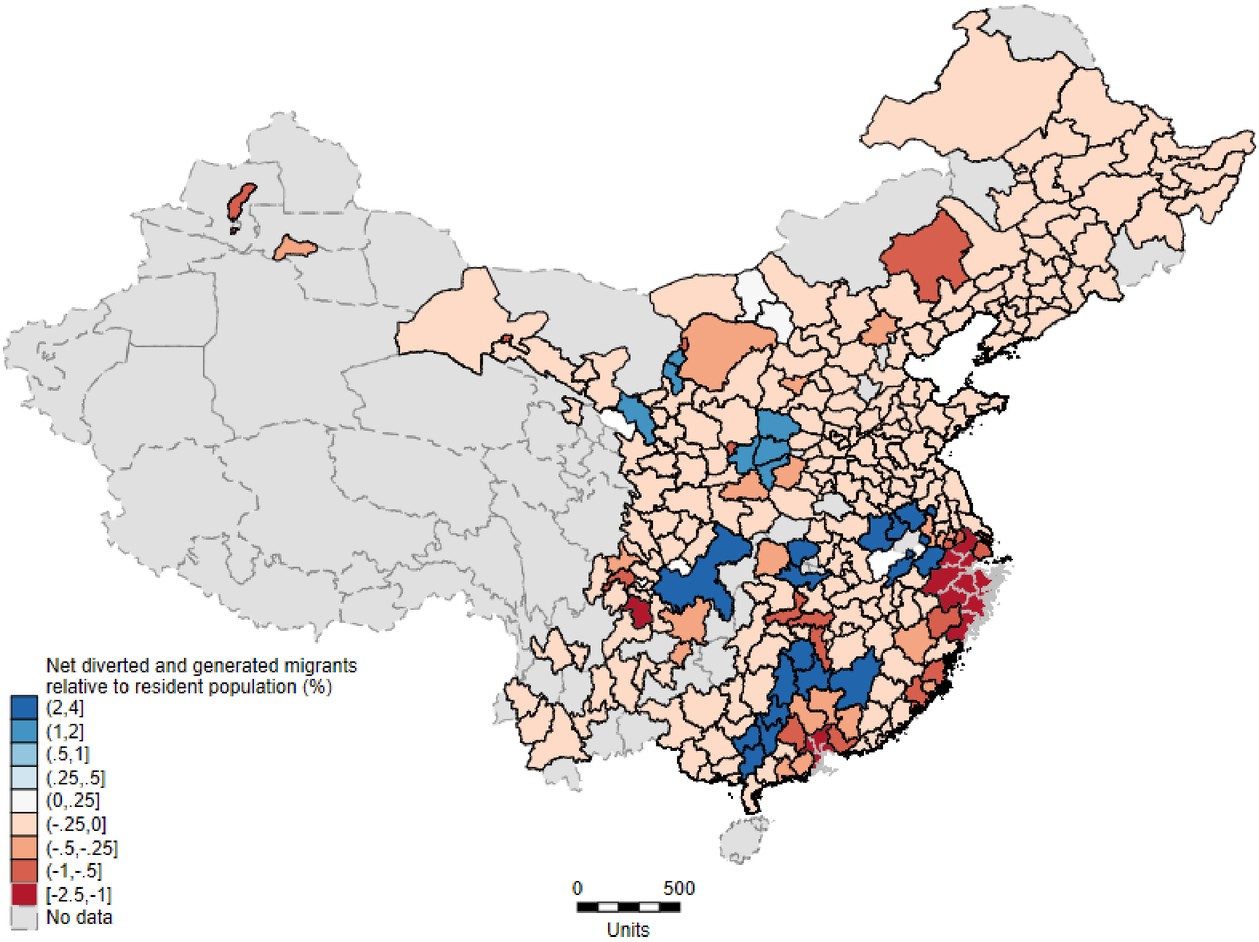
Note: Estimates of the diversion of existing migrants in general equilibrium due to the set of ITP policies, based on the 1-year lagged estimated impact across all ITP status assignments. The change is relative to the 2010 local resident hukou-holding population.

Figure 2 shows the number of additional diverted migrants resulting from the ITP status assignments, as a percentage of the resident population. The cities that receive an ITP status are the beneficiaries in terms of population change: the number of additional migrants is approximately 0.7% of the resident population, reaching up to 3.5%. For most regions, the changes are relatively minor. In coastal areas, the negative impacts are somewhat larger. This is not entirely surprising, as the coastal cities have larger shares of migrants that are assumed to be mobile in this calculation.

Migrant diversion amounts to a cumulative difference in city population sizes of over 2 million people, when comparing the migrants numbers to the counterfactual. When calculating the counterfactual outcomes with 1,000 repeated draws from the coefficient distribution, the number of diverted migrants is 2.31 million, with a standard deviation of 1.29mln and a 90% confidence interval between 0.90 and 4.18 million migrants. This diversion represents 1-2% of China's intercity migrant population.

A second source of changes in migration patterns is the generation of new migrants. Due to the nature of the migrant survey, changes in the non-migrant stock by city are not observed. However, to gain an understanding of what the plausible magnitudes of the migrant generation (that is locals turning into migrants) are, we impose the structure of our location choice model on non-migrant residents. We detail the application of the location model in Appendix G. In brief, the main assumption is that between migrants and locals, the underlying preferences for the (home) location (i.e., locational preferences B) may be different, but the elasticity of the location choice factors to wage, rent, and amenity changes associated with ITP policies are similar.

Figure 3: Population change with diversion and generation of migrants



Note: Estimates of the change in migrant stocks through i) diversion of destination and ii) non-migrants turning migrant; in general equilibrium due to the set of ITP policies, based on the 1-year lagged estimated impact across all ITP status assignments. The change is relative to the 2010 local resident hukou-holding population.

Figure 3 shows the population impacts of the set of ITP policies after including both the diversion of existing migrants and locals turning into migrants. For comparison, the scale is the same as the scale in Figure 2, which shows only the diversion of migrants. The generation of migrants is plausibly a larger source of change in migration patterns; in the counterfactual, 5.1 million new migrants are created from the ITP policy. Combining migrant diversion and migrant creation, the cumulative change in city sizes represents 7.3 million people, or close to 5% of China intercity migrants. This is only marginally less than the sum of the two processes. The reason is that migrant generation and diversion cause migration choices in the same direction; i.e., cities that gain diverted migrants also tend to gain newly generated migrants. Figure 3 shows that population gains are concentrated in the ITP areas (2.9% population growth on average, peaking at 5.1%). The coastal areas, in particular the Yangtze and Pearl River deltas (south of Shanghai and around Hong Kong), lose most population.

5.2 The development of targeted cities

The second aim of ITP is to upgrade cities into urban hubs suited for manufacturing. We examine such upgrading in three sets of outcomes. First, we examine which types of migrants responded most to the policy, in terms of supply and wage. Next, we weigh migrant and native responses to the policy to understand the city-level changes to industry employment shares. Third, we measure the impacts of ITP on broader development outcomes, including GDP and pollution.

5.2.1 Impacts of the policy on targeted migrants

To understand how ITP changes the composition of different types of migrants, we employ our methodology to estimate how shares of different worker types in the migrant flow evolve. The estimating equation is:

$$share_{odt} = \beta ITP_{dt} + \alpha_{mt} + \alpha_{od} + \alpha_{ot} + u_{odt}, \tag{10}$$

which explains the share of the migrant type of interest (such as high skilled migrants) in the total migration flow. The identification is the same as in previous sections. We explain the share of migrants because we connect our results to aggregate impacts in the next section. Second, we estimate equation 10 to explain the log of wages (instead of the shares) of migrants of different types.

In Table 3's panel a, column 1 shows a significant increase of 2 percentage points in the share of migrants that are employed in the manufacturing sector. A 2 percentage point shift in the migrant share is substantial relative to the pre-policy level share of 5-6% of migrants in manufacturing in ITP cities. After the year of announcement, no significant differences compared to the matched cities relative to the pre-policy year can be detected. Column 2 shows a significant increase of migrants with a coastal origin of 3 percentage points, or around 20% of the pre-policy share of migrants with a coastal origin. Columns 3 and 4 show a significant increase in the share of migrants with a rural hukou, but not in the share of skilled migrants, proxied here as those with higher than high school education (the results are similar for migrants with college education).

Columns 5 to 7 identify the impact of ITP on the overall share of migrants in manufacturing. Column 3 implies a 2 percentage point increase in the share of manufacturing sector migrants from coastal origins in the total number of migrants. The impact is comparable to column 1, suggesting that coastal migrants alone could account for the increase in migrant shares in manufacturing. In relative terms, the impact is larger: the 2 percentage points share increase represents an increase of over 50% in the number of coastal origin manufacturing workers. Similarly, migrants with a rural hukou account for large shares of composition changes following the ITP status, accounting for an increase of over 50%. Skilled migrants, however, show no significant change in the share of overall migrants.

Panel b of Table 3 uses the same identification (see eq. 10) to identify the impacts of ITP policies on the log wages of migrant groups. Column 1's result implies that ITP cities see a 2% wage rise among migrants. Wages among migrants in the manufacturing sector rise considerably faster, peaking at 6% in the year after announcement (column 2), and wages among migrants from coastal origins are similarly higher than those of the average migrant (column 3). Skilled migrants, by contrast, see very small wage decreases in all years following the policy.

The wage increases of migrants in the manufacturing sector are more pronounced. Columns 6 to 8 show estimates of wage growth for different migrant types targeted within the manufacturing sector. Coastal origin migrants in the manufacturing sector see sharper rises than general coastal origin migrants, and than other migrants in the manufacturing sector. However, at this level of disaggregation, the number of observations declines. Within the manufacturing sector, rural migrants see similar wage increases as migrants with other hukous (comparing columns 7 and 2). The most substantial wage increases concentrate with high-skilled migrants in the manufacturing sector (column 8), at around 7%.

The increase in the number of migrants in manufacturing, paired with wage rises, suggests that ITP statuses raise the demand for manufacturing migrants. Migrants are significantly more likely to originate from coastal locations, both generally and in the manufacturing sector, and those migrants earn higher wages. This is consistent with the geographical aims of the ITP policy. Among skilled migrants, wage gains concentrate in the manufacturing sector (with slight negative wage impacts outside the manufacturing). However, the wage growth is not paired with growth in the number of high skilled migrants, suggesting that the supply of high-skilled manufacturing migrants is inelastic.

Table 3: Quantity and wage responses by type of migrant

Panel a: share of migrant type in aggregate flow

	(1)	(2) Manuf	(3) Coast	(4) Rural	(5) Skill	(6) Manuf Coast	(7) Manuf Rural	(8) Manuf Skill
ITP (t)		0.02**	0.01	0.05***	-0.02	0.02***	0.02**	0.01
		(0.01)	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)
ITP (t-1)		0.01	0.03**	0.05**	-0.01	0.01**	0.00	-0.00
		(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.00)
ITP (t-2)		-0.02	-0.01	0.01	-0.02	-0.01*	-0.01	-0.00
		(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Observations		6,386	6,386	6,386	6,386	6,386	6,386	6,386
R-squared		0.56	0.80	0.66	0.49	0.63	0.46	0.36
Origin year FE		yes	yes	yes	yes	yes	yes	yes
Origin Destination FE		yes	yes	yes	yes	yes	yes	yes
matched bin year FE		yes	yes	yes	yes	yes	yes	yes

Panel b: wages by migrant type

	(1) Overall	(2) Manuf	(3) Coast	(4) Rural	(5) Skill	(6) Manuf Coast	(7) Manuf Rural	(8) Manuf Skill
ITP (t)	0.02**	0.03***	0.06***	0.02***	-0.00**	0.05	0.04***	0.06***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.05)	(0.01)	(0.00)
ITP (t-1)	0.02	0.06***	0.04***	0.02	-0.00***	0.09**	0.06***	0.08***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.04)	(0.01)	(0.00)
ITP (t-2)	0.01	0.04***	0.06***	0.01	-0.00**		0.04***	0.08***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)		(0.01)	(0.01)
Observations	3,162	3,162	3,162	3,162	3,131	2,201	3,069	2,635
Origin year FE	yes	yes	yes	yes	yes	yes	yes	yes
Origin Destination FE	yes	yes	yes	yes	yes	yes	yes	yes
Match bin year FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes. Dependent variable is the share of migrants that is employed in manufacturing (Manuf), from coastal origins (Coast), has a rural hukou (Rural), has at least a high school education (Skill). Panel b: The dependent variable is the log average wage of migrants workers in manufacturing (Manuf), from coastal origins (Coast), with a rural hukou (Rural), with at least a high school education (Skill). "ITP" refers to the industrial transfer status at the destination. "ITP" refers to the industrial transfer status at the destination. The time indicator "t" indicates that the dummy is one in the year of an announcement. The indicator "t-1" refers to the status assignment in the year before (the coefficient measures the impact one year after assignment). Standard errors clustered at the city level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

In addition to wage premia, migrants may be drawn to ITP cities with other instruments. The specific use of such instruments is not published. We examine anecdotal evidence. In Appendix H, we apply the above methodology (eq. 10) to verify whether groups of migrants with exposures to specific presumed policy instruments respond differentialy. We find some evidence that migrants with children respond more strongly to the policy, indicating that there may be hukou-related benefits for migrants in ITP cities, as schooling in particular may be a factor in the migration decision. We find strong rises in self employment, specifically in services such as catering, which suggests that migrants end up in auxiliary industries. Migrants in ITP cities are significantly more likely to be unemployed, suggesting that the ITP status does not come employment guarantees for migrants. There is no evidence to suggest that migrants in ITP cities live in (government or firm-) subsidized accommodations more frequently than elsewhere.

5.2.2 Sectoral changes

Individual migrants' responses to ITP can add up to industrial changes at the city level. The ITP specifically aims to expand manufacturing industries. The expansion can occur along different margins: through changed employment patterns among migrants or among natives, and through a changed mix between migrants and natives. To decompose the margins of adjustment, we define a city's employment share in manufacturing, e, as the weighted manufacturing shares of migrants and natives:

$$e_{manufacturing} = e_{manf}^{migrant} * s^{migrant} + e_{manf}^{native} * s^{native}, \tag{11}$$

where $e_{manf}^{migrant}$ is the employment share in the manufacturing sector among migrants, and e_{manf}^{native} is the employment share in manufacturing among natives. The shares of migrants and natives in the workforce are $s^{migrant}$ and $s^{native} = 1 - s^{migrant}$, respectively. The assignment of an ITP status can change the overall manufacturing employment rate through three margins:

$$\frac{ds_s}{dITP} = \underbrace{s^{migrant}\frac{de_{manf}^{migrant}}{dITP}}_{\substack{\text{change in migrant}\\ \text{specialization}}} + \underbrace{s^{native}\frac{de_{manf}^{native}}{dITP}}_{\substack{\text{change in native}\\ \text{specialization}}} + \underbrace{\frac{ds^{migrant}}{dITP}\left(e_{manf}^{migrant} - e_{manf}^{native}\right)}_{\text{composition change}} \tag{12}$$

This decomposition shows three margins of adjustment. First, migrants may change their specialization, thus changing their likelihood of working in the manufacturing sector. Second, natives may change their employment shares in the manufacturing sector. Third, the share of migrants in the total population can rise, which increases the manufacturing employment share if migrants tend to have higher employment shares in manufacturing than natives.

To calculate the margins of adjustment, we estimate the change in city-level migrant and native employment shares by sector from the equation:

$$\log e_{dt} = \beta ITP_{dt} + \alpha_{mt} + \alpha_d + u_{dt}, \tag{13}$$

where e_{dt} is the share of natives or migrants employed in a given sector in city d at time t. As before, this equation controls for pair-year fixed effects. The coefficient β for the 1-year lag of the ITP status assignment is our main estimate for the ITP-induced change in employment shares per sector. The results of these regressions are reported in Appendix I. We employ our baseline estimate for the 1-year lag of the ITP status assignment on the migration flow to recover $\frac{ds^{migrant}}{dITP}$. We use the statistics that in

ITP cities, migrants make up up 18% of the population, and 20.1% of migrant observations are in the first year since migration. We use the start-of-sample sectoral employment shares in (eventually) treated cities to proxy the sectoral employment share difference between migrants and natives, $e^{migrant}_{manf} - e^{native}_{manf}$. To obtain a confidence interval around these two margin, we bootstrap the calculation of the expected change to sectoral employment shares. We take the start-of-sample specialization as given and take draws from the estimated coefficient distributions for the migrant flow change and the specialization changes.

Table 4 shows the estimated percentage point changes in sectoral employment shares in a city in the year after it receives an ITP status. Based on the standard deviations of the coefficient estimates, it also reports 95% confidence intervals. The changes are decomposed into the direct impact of receiving more migrants, changes to the employment composition of incoming migrants, and changes to the employment composition of natives. The first row shows a negative composition effect in the year of announcement, as migrants are less likely to work in manufacturing than natives, but it is not significantly different from zero. The migrant specialization is significant and positive in the year of announcement, accounting for a 1.21 percentage point increase in the city employment share in manufacturing. The native specialization shows a negative but insignificant impact on manufacturing employment shares. Jointly, these three channels add to a small but statistically insignificant decline in manufacturing employment shares in ITP cities in the year of announcement.

In the first and second year after a city receives an ITP status (panel b anc c of Table 4), we find significant negative effect om manufacturing employment shares of 3 to 4 percentage points of employment. A back-of-the-envelope estimate suggests that the absolute size of the manufacturing sector employment has not risen with migration.[16] The largest share of the reduction in manufacturing employment shares is explained by the exit of native workers out of the manufacturing sector. Composition effects are negative but smaller, and migrant specialization effects are insignificant during the post-announcement years. During these years, we only find significant increases in employment shares for transport and retail. Retail employment changes follow from a large composition and native employment effects, while transport increases are largely driven by migrant specialization. [17]

[16]The overall manufacturing employment can be written as $N_{manuf} = e_{manuf} * N$. The relative change in employment is $dN_{manuf}/N_{manuf} = de_{manuf}/e_{manuf} + dN/N$. In approximate terms, de_{manuf}/e_{manuf} is around -0.15 (a 3 to 4 percentage point decline in a roughly 20 % employment share of manufacturing. As migrants make up about 20% of population and the ratio of the annual migrant flow to the stock is around 20%, the single year migrant flow increase would need to be around 400% to undo the specialization effect in the absolute employment in manufacturing.)

[17]We re-estimate the specialization equations with secondary and tertiary employment shares from the City yearbook, to corroborate the finding that there is no specialization in manufacturing from another source. We find a statistically significant reduction of the secondary employment share of 4 percentage point in ITP cities in the first and second year after the policy, relative to their matched cities. We also find 4 percentage point higher employment shares in the tertiary industry in the first and second year after an ITP announcement.

Table 4: City-level sectoral change

Panel a: Year of announcement

	Composition			Migrant spec.			Native spec.			Combined		
	Est.	95% CI		Est.	95% CI		Est.	95% CI		Est.	95% CI	
manuf.	-0.31	-0.44	-0.23	1.21	0.35	2.06	-1.37	-3.99	1.33	-0.48	-3.30	2.33
mining	-0.12	-0.17	-0.09	-0.93	-1.99	0.13	0.12	-0.96	1.23	-0.95	-2.43	0.59
utility	-0.06	-0.08	-0.04	0.13	-0.19	0.46	-0.27	-0.77	0.26	-0.19	-0.79	0.44
constru.	-0.05	-0.06	-0.03	0.64	-0.04	1.32	-1.00	-3.90	1.92	-0.40	-3.45	2.55
transport	-0.02	-0.03	-0.01	0.45	-0.13	1.03	0.26	-0.31	0.83	0.70	-0.13	1.50
retail	0.81	0.58	1.12	-0.09	-1.50	1.33	-0.46	-2.32	1.36	0.25	-2.09	2.60
horeca	0.46	0.33	0.63	-0.39	-1.61	0.84	0.62	-1.47	2.68	0.70	-1.79	3.11
finance	-0.15	-0.21	-0.11	-0.05	-0.34	0.23	0.38	-0.34	1.09	0.17	-0.60	0.96

Panel b: One year after announcement

	Composition			Migrant spec.			Native spec.			Combined		
	Est.	95% CI		Est.	95% CI		Est.	95% CI		Est.	95% CI	
manuf.	-0.45	-0.68	-0.29	-0.20	-1.01	0.61	-2.60	-5.21	-0.06	-3.27	-6.03	-0.55
mining	-0.18	-0.26	-0.12	-0.94	-2.06	0.19	-0.24	-1.35	0.88	-1.35	-2.94	0.24
utility	-0.08	-0.12	-0.05	0.31	-0.14	0.75	0.07	-0.44	0.57	0.30	-0.39	0.98
constru.	-0.06	-0.10	-0.04	-0.09	-0.77	0.60	-1.79	-4.48	0.97	-1.93	-4.76	0.86
transport	-0.03	-0.04	-0.02	0.51	-0.08	1.11	0.32	-0.23	0.84	0.80	0.00	1.60
retail	1.14	0.75	1.75	0.08	-1.24	1.43	1.88	-0.12	3.94	3.14	0.66	5.66
horeca	0.65	0.43	0.99	-0.87	-2.78	0.96	0.92	-0.34	2.20	0.72	-1.61	3.00
finance	-0.21	-0.32	-0.14	-0.12	-0.46	0.21	0.28	-0.45	1.04	-0.06	-0.88	0.77

Panel c: Two years after announcement

	Composition			Migrant spec.			Native spec.			Combined		
	Est.	95% CI		Est.	95% CI		Est.	95% CI		Est.	95% CI	
manuf.	-0.33	-0.50	-0.22	-0.88	-1.80	0.05	-2.87	-5.73	-0.08	-4.08	-7.11	-1.16
mining	-0.13	-0.19	-0.09	-1.08	-2.50	0.33	0.16	-0.98	1.28	-1.04	-2.85	0.75
utility	-0.06	-0.09	-0.04	0.02	-0.37	0.41	0.07	-0.51	0.64	0.02	-0.67	0.71
constru.	-0.05	-0.07	-0.03	1.01	-0.17	2.26	-1.90	-4.72	0.99	-0.94	-3.98	2.27
transport	-0.02	-0.03	-0.01	0.81	0.07	1.54	0.42	-0.12	0.97	1.21	0.28	2.13
retail	0.84	0.56	1.26	0.52	-0.80	1.83	0.59	-1.44	2.62	1.96	-0.48	4.44
horeca	0.48	0.32	0.72	-0.52	-2.38	1.34	0.54	-0.85	1.91	0.51	-1.80	2.81
finance	-0.16	-0.24	-0.10	-0.30	-0.82	0.23	0.43	-0.30	1.17	-0.01	-0.94	0.89

Notes. Estimated impact of ITP on the employment shares of different sectors in the ITP city; in percentage point changes. Manuf. refers to manufacturing. Constru. refers to construction. Transport includes ICT infrastructure. Finance includes real estate and commercial services. The columns report the change estimate (first column) with a bootstrapped two-sided 95% confidence interval (second and third column). The bootstrap takes 1,000 draws from the coefficient distributions for migration changes and specialization changes to construct a distribution of point estimates for the individual margins of adjustment and the combined adjustment. Start-of-sample sectoral employment shares among migrants and natives in ITP cities are taken as constant.

5.2.3 Output, wages and production

To examine the aggregate development of cities that are assigned ITP statuses, we track the development of measure of output on the same sample as the migration results. We use the same matching procedure with match bin-year fixed effects, but now adapted to a city panel with a city-level fixed effect:

$$\log GDP_{dt} = \beta ITP_{dt} + \alpha_{mt} + \alpha_{d} + u_{odt}, \tag{14}$$

where the dependent variable is $\log GDP_{odt}$ or a related output measure. Primarily, we use GDP statistics from the Chinese city yearbook.

Table 5 shows regressions that explain the log of the GDP per capita, the GDP level and the average wage of native workers from the ITP status. The results in column 1 show no significant change in the log of GDP per capita between ITP and matched cities. Column 2 introduces the 2-year lead of ITP by means of a pretrend test - it shows no significant difference between ITP cities and their matches before or after the ITP status is assigned. Very similar results arise when looking at the log GDP (columns 4 and 5). The city's log GDP might rise as more migrants arise, even when keeping the GDP per capita fixed. However, the predicted percentage changes in population from our counterfactual model fall well within the confidence intervals of estimated GDP impacts, so no significant result might be expected in this setting. Columns 5 and 6 of Table 5 show regression results when the log of the average wage of non-migrant workers is taken as the outcome. If anything, it shows a slight decrease in the wages of non-migrant workers.

As the city year book covers more years than the migration data, we show extended results from 4 years before to 5 years after the ITP status in Appendix J. We identify small positive impacts of the policy. We find a significant GDP raise of around 0.5% in ITP cities relative to control cities, remaining under 1% of the 5 years after the policy. The GDP per capita estimates and wages show smaller impacts, at around mostly 0.5% following the policy but materializing after 5 years. Hence, these results show very slight increases in output and wages in ITP cities.

The city-level statistics for GDP may mismeasure economic activity for various reasons. To check whether the GDP data might simply not represent output variation, we show the coefficient estimates when not using the identification strategy in Table 5, columns 7 and 8. Unconditional on matching, the ITP status is negatively associated with both the GDP and the GDP per capita. That is consistent the stated policy that ITP does not target the leading cities. In addition, we use nightlight intensity to verify our results outside Chinese government statistics. Appendix K shows the full results. We find marginal evidence that nightlights decline in ITP cities after the ITP assignment relative to matched cities, with no significant difference before the assignment. For verification, we show that nightlight intensity is strongly correlated with the official GDP measures we use, both conditional and unconditional on yearly fixed effects for the ITP cities and matched city pairs.

Pollution is also frequently mentioned in the descriptions of ITP goals. The expected outcome for ITP cities is not necessarily clear. Anecdotally, polluting industries are evicted from coastal areas, suggesting that they might pollute ITP cities instead. However, if the incoming industry is cleaner than the industry already present in the ITP city, the city's emissions might still reduce. In Appendix K, we show the full results for satellite-based local fine particulate matter concentrations (PM 2.5), as well as pollutant measures from official statistics (soot, sulfur dioxide and wastewater). In satellite data as well as the official sources, we find little impact on pollution - only for median particulate matter concentrations do we find marginal evidence for a reduction.[18]

[18] To verify our particulate matter estimates, we check that they are correlated to known sources of pollution, such as density and secondary sector shares. We are unable to confirm such a correlation in official data conditional on city and year fixed effects, and find negative correlations between official pollution measures and population density in unconditional estimates.

Table 5: Impact of ITP status on city log GDP or log GDP per capita

	(1) GDP/cap	(2) GDP/cap	(3) GDP	(4) GDP	(5) wage	(6) wage	(7) GDP/cap	(8) GDP
ITP (t+2)		0.02		0.01		0.03		
		(0.03)		(0.04)		(0.03)		
ITP (t)	-0.02	0.02	-0.04	-0.02	-0.01	0.01	-0.37***	-0.30**
	(0.04)	(0.03)	(0.04)	(0.03)	(0.02)	(0.02)	(0.10)	(0.13)
ITP (t-1)	-0.02	0.02	0.00	0.03	-0.03	-0.01	-0.25***	-0.14
	(0.04)	(0.03)	(0.04)	(0.03)	(0.02)	(0.02)	(0.09)	(0.13)
ITP (t-2)	-0.03	0.02	-0.02	0.02	-0.05*	-0.02	-0.18**	-0.07
	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)	(0.08)	(0.12)
Observations	206	272	206	272	202	268	2,231	2,231
R-squared	0.99	0.99	0.99	0.99	0.98	0.97	0.01	0.00
sample	matched	matched	matched	matched	matched	matched	full	full
matched bin year FE	yes	yes	yes	yes	yes	yes	no	no
city FE	yes	yes	yes	yes	yes	yes	no	no
year FE	yes	yes	yes	yes	yes	yes	no	no

Notes. ”ITP” refers to the industrial transfer status at the destination. The time indicator ”t” indicates that the dummy is one in the year of an announcement. The indicator ”t-1” refers to the status assignment in the year before (the coefficient measures the impact one year after assignment). Robust standard errors. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

An ITP status may change the production strategies of local firms. The migrant inflows into ITP cities could represent a substantial supply shock to the local labor market. Firms may adjust their hiring and substitute capital for labor. Additionally, ITP policies may have incentivized firms to adopt different production methods.

To corroborate our findings with an alternative dataset, we employ the survey of manufacturing firms in the NBS Annual Survey of Industrial Firms between 2011 and 2013 to examine the impact of ITP policies on firm-level choices. The survey's time coverage necessarily restricts the set of ITP observations included in the sample. Conditional on fixed effects for the city or firm, match pair-year and industry-year combinations, we explain firm-level outcomes on the ITP status assignment of the city. We split our analysis in two groups: a sample of startups (with a city fixed effect) and a sample of existing firms (with a firm-level fixed effect).

The results are presented in Table L.1 in Appendix L. The impacts are generally not precisely estimated. For the sample of startups we find negative point estimates for the impact of ITP in firm size, and productivity, but only the negative impact on startup firm revenue is significant at the 10% level. For existing firms, the coefficient estimates for employment, revenue, capital intensity and TFP are closer to zero and all insignificant.

6 Conclusion

Industrial Transfer Policy (ITP) is among the largest efforts of the Chinese government to steer the country's spatial economic development. The policy aims to foster a set of inland secondary (manufacturing) cities by transferring people and production away from coastal areas. There has been little formal impact analysis of these policies, despite their ambitious scale and despite the lack of consensus on the long-run growth prospects of Chinese migration policies (Lu, 2016; Au and Henderson, 2006b).

This paper uses an extensive migrant survey to examine the migration impacts of receiving an ITP status in the city. It compares the developments in cities that were targeted with ITP statuses to those in untreated, similar control cities, and differences out several correlated explanations at the city level and at the level of the migrants' origins. A city that receives an ITP status demonstrates substantial increases in migrant inflows, which reflect a substantial urbanization of the targeted cities. These results hold across different methods of identifying control cities and different forms of inference. Urbanization from ITP policies plausibly entails millions of migrants, with a comparatively large supply of migrants from the coastal areas. This result fits with the policy objectives described and with anecdotal descriptions of the scale of the policy.

Our results show no development of manufacturing industries in targeted cities, which defies the aims of the ITP. Our results suggest several reasons. The targeted migrants respond inelastically, leading their wages to rise but their number to remain modest. At the same time, considerable numbers of native workers leave manufacturing industries in the targeted cities. Rather than growth in the manufacturing sector, we observe growth in ancillary employment, such as transport and catering. Correspondingly, we find no evidence of growth in (proxies of) output, native workers' wages, or local firm entry or production strategies.

Taken together, the estimated population movements confirm the extensive scale of Industrial Transfer Policy described among policymakers and spectators (Ang, 2018). The scale of population movement involved in ITP is emblematic for China's increasing use of large-scale place-based policies to guide its development. Such policies defy the suggestion that restricted labor mobility has led China's largest cities to be too small (e.g., Zilibotti, 2017; Au and Henderson, 2006a); instead, ITP encourages the growth of second rank cities over the growth of the largest cities. Our results cast doubt on the potential of ITP for long-term growth and structural change.

References

Alder, S., Shao, L., and Zilibotti, F. (2016). Economic reforms and industrial policy in a panel of Chinese cities. *Journal of Economic Growth*, 21(4):305–349.

An, L., Qin, Y., J., W., and You, W. (2024). The local labor market effect of relaxing internal-migration restrictions: evidence from China. *Journal of Labor Economics*, 42(1).

Ang, Y. Y. (2018). Domestic flying geese: industrial transfer and delayed policy diffusion in China. *The China Quarterly*, 234(June):420–443.

Au, C.-C. and Henderson, J. V. (2006a). Are Chinese cities too small? *Review of Economic Studies*, 73(3):549–576.

Au, C.-C. and Henderson, J. V. (2006b). How migration restrictions limit agglomeration and productivity in China. *Journal of Development Economics*, 80:350–388.

Barca, F., McCann, P., and Rodríguez-Pose, A. (2012). The case for regional development intervention: place-based versus place-neutral approaches. *Journal of Regional Science*, 52(1):134–152.

Becker, S. O., Egger, P. H., and Von Ehrlich, M. (2013). Absorptive capacity and the growth and investment effects of regional transfers: A regression discontinuity design with heterogeneous treatment effects. *American Economic Journal: Economic Policy*, 5(4):29–77.

Beerli, A., Ruffner, J., Siegenthaler, M., and Peri, G. (2021). The abolition of immigration restrictions and the performance of firms and workers: evidence from Switzerland. *American Economic Review*, 111(3):976–1012.

Bo, S. (2020). Centralization and regional development: evidence from a political hierarchy reform to create cities in China. *Journal of Urban Economics*, 115:103182.

Brandt, L., Van Biesebroeck, J., and Zhang, Y. (2014). Challenges of working with the Chinese NBS firm-level data. *China Economic Review*, 30:339–352.

Buchard, V., da Silva, A. M., Randles, C. A., Colarco, P., Ferrare, R., Hair, J., Hostetler, C., Tackett, J., and Winker, D. (2016). Evaluation of the surface PM2.5 in Version 1 of the NASA MERRA Aerosol Reanalysis over the United States. *Atmospheric Environment*, 125:100–111.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

Cameron, A. C. and Miller, D. L. (2015). A practitioner's guide to cluster-robust inference. *Journal of Human Resources*, 50(2):317–372.

Chauvin, J. P., Glaeser, E., Ma, Y., and Tobio, K. (2017). What is different about urbanization in rich and poor countries? Cities in Brazil, China, India and the United States. *Journal of Urban Economics*, 98:17–49.

Combes, P.-P., Démurger, S., and Li, S. (2015). Migration externalities in Chinese cities. *European Economic Review*, 76:152–167.

Combes, P.-P., Démurger, S., Li, S., and Wang, J. (2020). Unequal migration and urbanisation gains in China. *Journal of Development Economics*, (102328).

de Chaisemartin, C. and D'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, 110(9):2964–96.

Di Cataldo, M. (2017). The impact of EU objective 1 funds on regional development: evidence from the UK and the prospect of Brexit. *Journal of Regional Science*, 57:814–839.

Diemer, A., Iammrino, S., Rodríguez-Pose, A., and Storper, M. (2022). The regional development trap in europe. *Economic Geography*, 98(5):487–509.

Duranton, B. G. and Puga, D. (2001). Nursery cities: urban diversity, process innovation, and the life cycle of products. *The American Economic Review*, 91(5):1454–1477.

Duranton, G. and Venables, A. J. (2021). Place-based policies: principles and developing country applications. In Fischer, M. and Nijkamp, P., editors, *Handbook of Regional Science*, pages 1009–1030. Springer-Verlag.

Dustmann, C., Schönberg, U., and Stuhler, J. (2016). The impact of immigration: Why do studies reach such different results? *Journal of Economic Perspectives*, 30(4):31–56.

Fan, J. and Zou, B. (2021). Industrialization from scratch: The "Construction of Third Front" and local economic development in China's hinterland. *Journal of Development Economics*, 152:102698.

Fu, S., Xu, X., and Zhang, J. (2021). Land conversion across cities in China. *Regional Science and Urban Economics*, 87:103643.

Fu, Y. and Gabriel, S. A. (2012). Labor migration, human capital agglomeration and regional development in China. *Regional Science and Urban Economics*, 42(3):473–484.

Garriga, C., Hedlund, A., Tang, Y., and Wang, P. (2017). Rural-urban migration, structural transformation, and housing markets in china. Technical report, National Bureau of Economic Research.

Ge, S. and Yang, D. T. (2014). Changes in china's wage structure. *Journal of the European Economic Association*, 12(2):300–336.

Ghanem, D. and Zhang, J. (2014). 'Effortless Perfection:' do Chinese cities manipulate air pollution data? *Journal of Environmental Economics and Management*, 68(2):203–225.

Gray, R., Montresor, G., and Wright, G. C. (2020). Processing immigration shocks: Firm responses on the innovation margin. *Journal of International Economics*, 126:103345.

Grover, A., Lall, S. V., and Maloney, W. F. (2022). *Place, Productivity, and Prosperity*. World Bank Group.

Hao, T., Sun, R., Tombe, T., and Zhu, X. (2020). The effect of migration policy on growth, structural change, and regional inequality in China. *Journal of Monetary Economics*, 113:112–134.

Henderson, J. V. (2005). Urbanization and growth. In *Handbook of economic growth*, volume 1, pages 1543–1591. Elsevier.

Jia, J., Ma, G., Qin, C., and Wang, L. (2020). Place-based policies, state-led industrialisation, and regional development: Evidence from China's Great Western Development Programme. *European Economic Review*, 123(17):103398.

Kahn, M. E., Sun, W., Wu, J., and Zheng, S. (2021). Do political connections help or hinder urban economic growth? Evidence from 1,400 industrial parks in China. *Journal of Urban Economics*, 121:103289.

King, G. and Nielsen, R. (2019). Why propensity scores should not be used for matching. *Political Analysis*, 27(4).

Koster, H. R., Cheng, F. F., Gerritse, M., and Van Oort, F. (2019). Place-based policies, firm productivity and displacement effects: evidence from Shenzhen, China. *Journal of Regional Science*, 59(2):187–213.

Lai, F., Liu, C., Luo, R., Zhang, L., Ma, X., Bai, Y., Sharbono, B., and Rozelle, S. (2014). The education of China's migrant children: the missing link in China's education system. *International Journal of Educational Development*, 37:68–77.

Lu, M. (2016). *Great Nation Needs Bigger City*. Shanghai People's Publishing House.

Lu, Y., Wang, J., and Zhu, L. (2019). Place-based policies, creation, and agglomeration economies: evidence from China's economic zone program. *American Economic Journal: Economic Policy*, 11(3):325–360.

Ma, L. and Tang, Y. (2020). Geography, trade, and internal migration in China. *Journal of Urban Economics*, 115:103181.

Meng, X. (2012). Labor market outcomes and reforms in China. *Journal of Economic Perspectives*, 26(4):75–102.

Neumark, D. and Simpson, H. (2015). Place-based policies. In Duranton, G., Henderson, J., and Strange, W., editors, *Handbook of Regional and Urban Economics 5*, pages 1197–1287. Elsevier, Amsterdam.

Ngai, L. R., Pissarides, C. A., and Wang, J. (2019). China's mobility barriers and employment allocations. *Journal of the European Economic Association*, 17(5):1617–1653.

Niu, D., Sun, W., and Zheng, S. (2021). The role of informal housing in lowering china's urbanization costs. *Regional Science and Urban Economics*, 91:103638. Special Issue on Rural-Urban Migration in Honor of Harris and Todaro.

Peri, G. (2012). The Effect Of Immigration On Productivity: Evidence From U.S. States. *The Review of Economics and Statistics*, 94(1):348–358.

Rossi-Hansberg, E. and Wright, M. L. (2007). Urban structure and growth. *The Review of Economic Studies*, 74(2):597–624.

Santos Silva, J. M. C. and Tenreyro, S. (2006). The log of gravity. *Review of Economics and Statistics*, 88(November):641–658.

Schweiger, H., Stepanov, A., and Zacchia, P. (2022). The long-run effects of r&d place-based policies: Evidence from russian science cities. *American Economic Journal: Economic Policy*.

State Council of the People's Republic of China (2010). Guiding opinions on central and western regions undertaking of industrial transfer. Technical report.

State Council of the People's Republic of China (2014a). National new urbanisation plan (2014-2020). Technical report.

State Council of the People's Republic of China (2014b). Opinions on further promoting the reform of household registration system. Technical report.

State Council of the People's Republic of China (2015). Guiding opinions on encouraging return migrants to be entrepreneurs. Technical report.

Thompson, W. A. (1968). *Internal and External Factors in the Development of Urban Economics*. John Hopkins Press, Baltimore.

Vernon, R. (1960). *Metropolis*. Cambridge Mass.: Harvard University Press.

Wang, J. (2013). The economic impact of Special Economic Zones: evidence from Chinese municipalities. *Journal of Development Economics*, 101:133–147.

Wang, Z. and Chen, L. (2019). Destination choices of Chinese rural-urban migrant workers: jobs, amenities, and local spillovers. *Journal of Regional Science*, 59(3):586–609.

Yao, Y. and Zhang, M. (2015). Subnational leaders and economic growth: evidence from Chinese cities. *Journal of Economic Growth*, 20(4):405–436.

Yu, Y. and Zhang, N. (2022). Does industrial transfer policy mitigate carbon emissions? evidence from a quasi-natural experiment in china. *Journal of Environmental Management*, 307:114526.

Zhang, H., Behrman, J. R., Fan, C. S., Wei, X., and Zhang, J. (2014). Does parental absence reduce cognitive achievements? Evidence from rural China. *Journal of Development Economics*, 111:181–195.

Zhang, J., Wang, R., and Lu, C. (2019). A quantitative analysis of Hukou reform in Chinese cities: 2000-2016. *Growth and Change*, 50(1):201–221.

Zhang, J. and Zhao, Z. (2013). Measuring the income-distance tradeoff for rural-urban migrants in China. *IZA Discussion Paper*, (7160).

Zheng, S., Sun, W., Wu, J., and Kahn, M. E. (2017). The birth of edge cities in China: measuring the effects of industrial parks policy. *Journal of Urban Economics*, 100:80–103.

Zilibotti, F. (2017). Growing and Slowing down like China. *Journal of the European Economic Association*, 15(5):943–988.

A Chinese Migration Dynamics Survey: representatives and descriptive statistics

In this Appendix, we document the representativeness of the sample. We compare the constructed migrant sample with the 2010 census. Table A.1 presents key demographic characteristics of migrants who are CMDS respondents and those who are census respondents. The demographic characteristics of the data sources overlap fairly precisely.

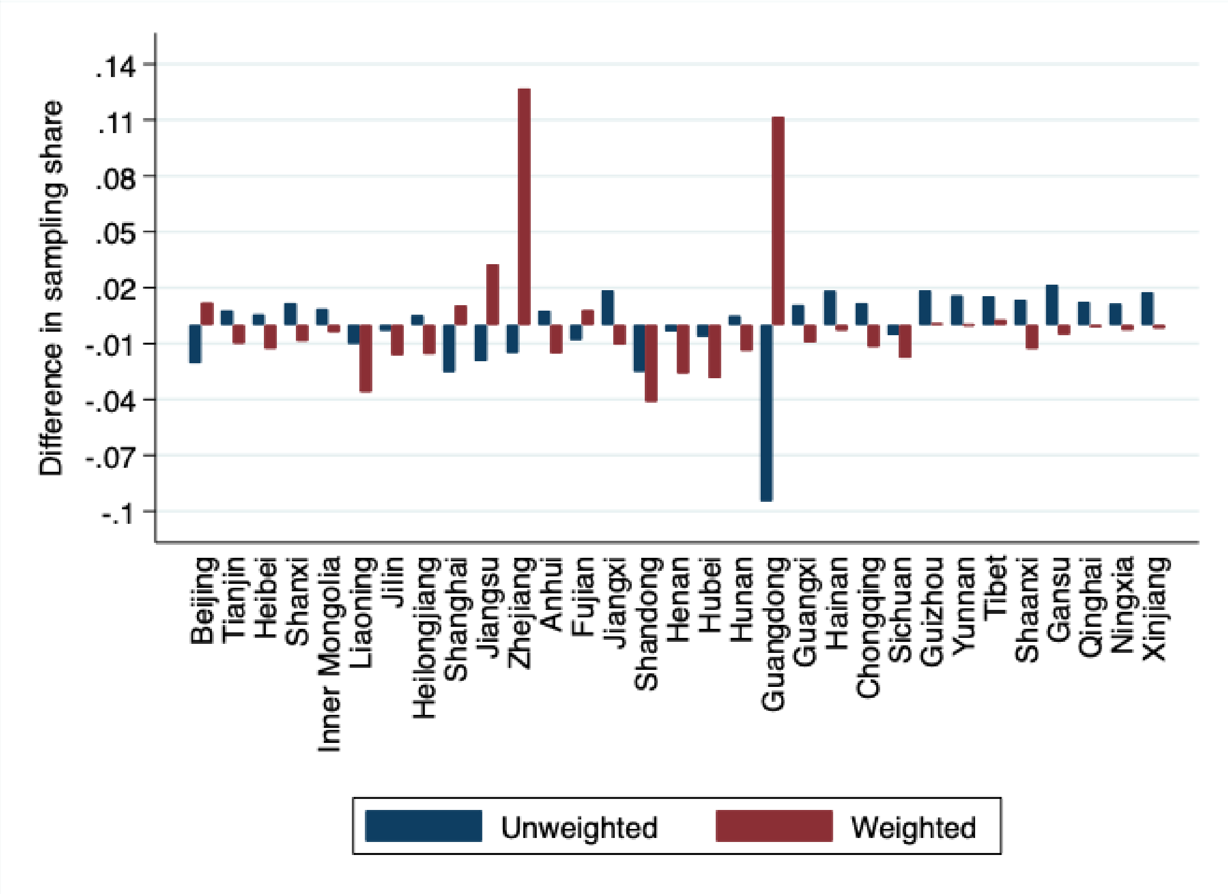
Table A.1: CMDS as a Nationally Representative Sample

	(1)	(2)
	2011 CMDS	2010 Census
Gender		
Male	0.53	0.52
Educational levels		
No schooling at all	0.02	0.02
Primary school	0.14	0.14
Middle school	0.55	0.39
High school	0.21	0.24
College and above	0.08	0.22
Years since migration		
Within 1 year	0.14	0.19
Between 1 and 2 years	0.18	0.21
Between 2 and 3 years	0.14	0.15
Between 3 and 4 years	0.10	0.10
Between 4 and 5 years	0.07	0.06
Between 5 and 6 years	0.06	0.04
More than 6 years	0.31	0.24
Types of migration		
Interprovincial migration	0.50	0.34
Intraprovincial and across-city migration	0.31	0.44
Within-city migration	0.19	0.22

Source: CMDS 2011, Census 2010.

Three potential differences are worth noting. First, migrants with tertiary education are undersampled, which is attributed to a higher rejection rate. This is not uncommon in household surveys, and we assume the rate of undersampling does not correlate to our policy variables. Second, interprovincial migrants have somewhat higher sampling rates than intraprovincial migrants. This is attributed to dialect differences, which makes interprovincial migrants more recognizable to surveyors. There is no evidence to suggest that this sampling ratio changes with ITP instatement. We also control for province-of-origin specific fixed effects in the regression and explicitly check whether our results vary across intra- and interprovincial migrants. Last, the financing of the CMDS survey implies that provinces with fewer migrants receive fewer surveys, with a lower bound of 2,000 migrants surveyed in every province. As ITP cities are often in areas with fewer migrants, the lower bound may imply an oversampling of migrants. However, the statistical weighting scheme of the CMDS does adjust for this potential oversampling. Figure A.1 shows the difference in shares of migrants across provincial units between the 2011 CMDS data and the 2010 population census. When using unweighted counts, as shown in the blue bars on the right of Figure A.1, most of the middle and western provinces show somewhat higher numbers of migrants than the census. In the weighted shares in red, the middle and western provinces are more comparable to the 2010 census, whereas the most popular destinations for migrants, Zhejiang and Guangdong, are counted more than their real shares. The magnitude of the differences is overseeable for all provinces.[19]

Figure A.1: Comparison of sampling between 2011 CMDS and 2010 census



Notes: The X-axis denotes 31 provinces. The Y-axis is the provincial sampling share in 2011 CMDS minus that in 2010 census.

[19]To check any sensitivity to this issue, we also check the stability of our results between using weighted and unweighted migrant number estimates. The results are very similar.

B Matching on the propensity of receiving an ITP status

This Appendix shows supporting results on propensity score matching. The main matching equation contains a fixed set of coefficients and all their interactions and squares. Table B.1 shows the coefficients the main variables included in the baseline matching equation (4), entered without squares and interactions, as the fully interacted set of variables yields a convoluted interpretation. The GDP per capita and the share of secondary industry are significant predictors of a later ITP status, in line with the policy documents. All coefficients need to be interpreted conditional on the other variables in the regression, which explains the large but insignificant of wages (the correlation between log GDP per capita and log wages is 0.7). Log distance to the coast, when entered separately as a log (positive coefficient) and the square of the log (negative coefficient), suggest that the probabilities of receiving an ITP status are lower very near to the coast and very far away from the coast.

Table B.2 explores the contribution of individual covariates by considering how omitting them changes the explanatory power, the set of original cities, and the propensity score differences within each set. Figure B.1 visualizes the difference in propensity to be treated using all combinations of covariates in Table B.2 by plotting the density functions of assignment probabilities for ITP cities and different sets of matched cities.

Table B.1: Logit equation to explain the city ITP status assignment

	(1) odds ratio
log GDP/cap	0.16**
	(0.14)
log wage	10.19
	(16.39)
Secondary industry as percentage to GDP (%)	1.14**
	(0.07)
log employment	0.91
	(0.35)
Tertiary industry as percentage to GDP (%)	1.08
	(0.07)
Log distance to coast	0.80
	(0.22)
Industry share controls	yes
Observations	280

Notes. Results of a 2009 cross-sectional logit estimation of the city ever receiving an ITP status. Linear in main coefficients for interpretation. The industry employment share are measured for: transport; mining; manufacturing; utilities; ICT; retail; horeca; real estate; and telecom. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

To assess the matching of ITP cities more formally, Table B.3 contains regressions that test the association between the treatment propensity and the actual treatment. In the full sample (column 1), an OLS regression shows a clear association of actual status assignment and predicted assignment. The cities receiving an ITP status have an over 18 percentage points higher probability of assignment. In columns 2 and 3, when considering the sample of ITP cities and only their matches, we find no evidence that assignment is associated with higher treatment probability, with and without fixed effect for the matched pairs.

A concern may be that matched cities draw on similar origins for their migrants. If so, the assignment of an ITP status may represent an increasing attractiveness for the control cities. This could constitute a SUTVA violations, leading to an underestimate of the counterfactual migration flow and an

Table B.2: Comparisons of the set of matched cities when using different covariates for matching.

	(1)	(2)	(3)	(4)	(5)	(6)
Observations	283	283	283	283	283	283
Nonlinear covariates						
Log GDP/cap	yes		yes	yes	yes	yes
Log Wage	yes	yes		yes	yes	yes
GDP share secondary sector	yes	yes	yes		yes	yes
Linear covariates						
Yearly fixed effects	yes	yes	yes	yes	yes	yes
Log Employment	yes	yes	yes	yes	yes	yes
GDP share tertiary sector	yes	yes	yes	yes	yes	yes
Sectoral employment shares	yes	yes	yes	yes	yes	
Distance to coast	yes	yes	yes	yes		yes
Distance to coast squared	yes	yes	yes	yes		yes
Mean propensity difference	0.03	0.01	0.00	0.02	0.02	0.00
Control overlap	1.00	0.10	0.14	0.16	0.22	0.09
Pseudo R2	0.24	0.14	0.15	0.19	0.22	0.19
Log likelihood	-70.94	-80.10	-78.90	-75.94	-72.89	-75.39

Notes. This Table displays diagnostics for the set of matched cities when varying the set of covariates used for matching on (logit) propensities. Mean propensity difference is the mean difference in the treatment likelihood within every pair of treated and matched city. Control overlap is the share of control cities that is the same as selected in the set that uses all covariates (column 1). Pseudo R2 and Log likelihood are measures of fit for the logit treatment equation.

overestimate of the impact of ITP. To examine this concern, we construct measures of overlap in migrant origins for pairs of destination cities. We then examine whether matched cities have significantly higher overlap. Define s_{io} as the migrants that city i draws from origin o as a share of all migrants in city i (such that $\sum_o s_{io} = 1$). The linear difference in migrant origins between city i and city j is: $\sum_o |s_{io} - s_{jo}|/2$. The Euclidian distance of the the migrant origin difference is: $\sum_o (s_{io} - s_{jo})^2$

Table B.4 shows regressions of city pairs' measures of overlap in migrant origins on a dummy indicating whether the cities were matched as an ITP city and a control city. In columns 1-4, under both linear and Euclidean measures, and with and without city fixed effects, we find no evidence that matched cities have significantly more overlap in the origin shares of their migrants. Columns 5-8 show the same regressions but now in the sample of matched cities. Similarly, ITP cities have no significantly larger overlap in migrant origins with their matches than with other cities in the matched sample.

Figure B.1: Distribution of propensity to receive ITP status for cities receiving the status and for matched cities using different sets of covariates

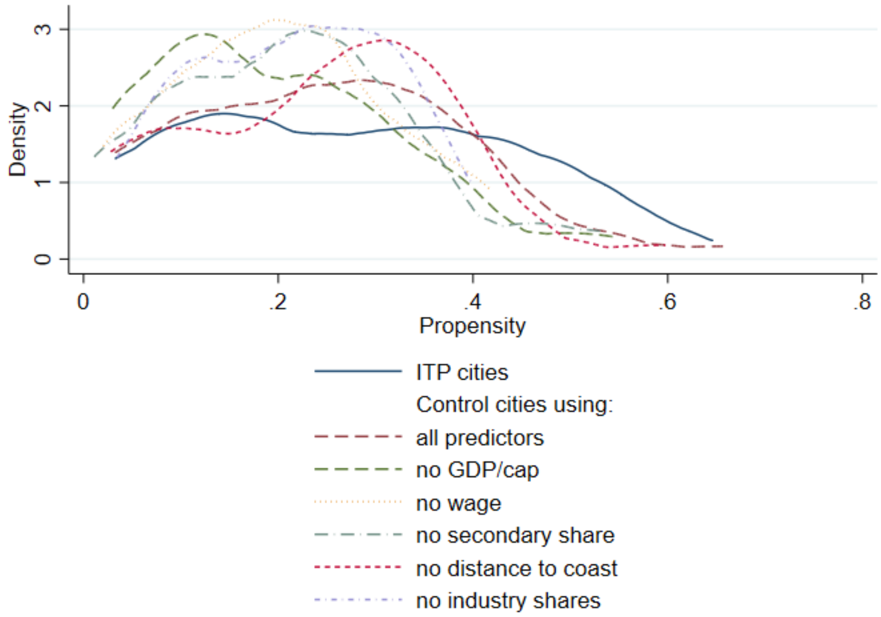


Table B.3: Treatment propensity explained from actual treatment in different samples

	(1) Full sample	(2) Matched sample	(3) Matched sample
Treated	0.18***	0.03	0.03
	(0.03)	(0.04)	(0.02)
Observations	281	58	58
Match bin year FE	no	no	yes
within r2	0.19	0.01	0.96

Notes. Results from a cross-sectional OLS regression that explains estimated treatment propensity from the observed treatment (the assignment of the ITP status). Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table B.4: Similarity in the origins of migrant between matched and non-matched cities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
similarity index	Linear	Euclid	Linear	Euclid	Linear	Euclid	Euclid	Euclid
sample	Full	Full	Full	Full	Matched	Matched	Matched	Matched
Matched	0.04	0.04	0.03	0.02	0.04	0.03	0.03	0.05
	(0.03)	(0.05)	(0.03)	(0.03)	(0.04)	(0.05)	(0.03)	(0.03)
Observations	33,939	33,939	33,939	33,939	7,134	7,134	7,125	7,125
City FE	no	no	yes	yes	no	no	yes	yes
Within R2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Notes. The dependent variable is, for every pair of cities, the similarity index for the overlap in the origins of their migrants. The variable ”matched” is a dummy indicating whether the city pair is a match in the baseline result. Under linear similarity indexes similarity is measured as $\sum_o |s_{io} - s_{jo}|/2$, where s_{io} is the share of city i's migrants that originate from origin o.Under Euclidian similarity, the index is $\sum_o (s_{io} - s_{jo})^2$. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

C Dynamic structure of the policy

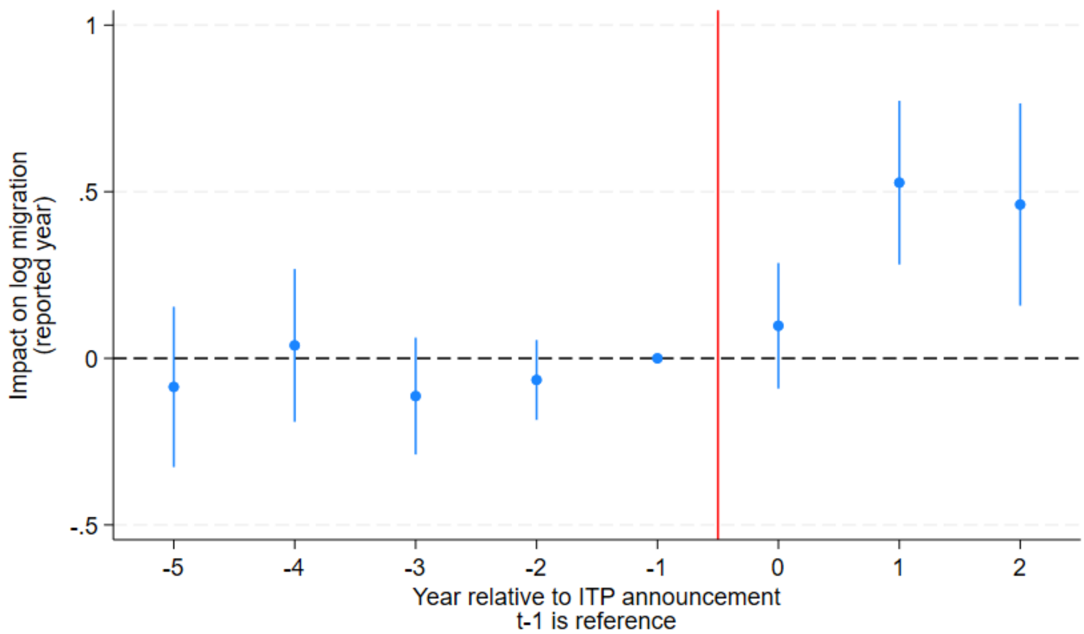
C.1 Pretrends with reported year of migration

The time span of the survey allows calculating the flow of observed migrants only up to two year before the first announcement. Hence, there is only a single year for pretrend testing (i.e. the observation two years before announcement).

As an alternative, we estimate earlier arrival flows of migrants by considering the year in which an observed migrant has arrived in the city. This produces estimates of migrant flows for more years before the policy start. A potential concern is that migrants' attrition rates in ITP cities can look different from peer cities. Lower attrition rates can lead to higher estimates of earlier arrivals. If ITP cities see structurally different attrition rates after the policy, that can generate differential pre-policy estimates of migrant inflows even without the facto differences in arrival numbers. In subsection C.4, we examine this possibility, and find no different attrition rates for ITP cities than for their control cities.

Figure C.1 shows the coefficients for the main regression, now using estimates of migrant arrival based on the migrants' reported years of arrival up to ten years from the observation (our baseline estimates use observation at most 3 years after the reported year of migration). These estimates of migrant arrivals permit examining any differential trends in more years prior to treatment. We find no significant differential between ITP cities and control cities in the five years leading up to the policy assignment. The estimates of the policy impact are similar to the baseline results.

Figure C.1: The impact of ITP status on migration pre-policy



Notes. The number of migrants is estimated as number of observed migrants that report a given arrival year. Estimation is the same as the baseline regression equation (eq. 3). Whiskers indicate the 95% confidence interval. The red line marks marks the split between pre-treatment and the year of announcement. The year before announcement is taken as the reference coefficient.

C.2 Varying definitions of recent migrants

In our main results, we use estimates of the migrants' arrival for migrants who reported migration up to three years before observation. There is a trade-off; the inclusion of a longer horizon since the year of migration leads to more observations, but it also exposes the measure of migrant numbers to spatial differences in attrition rates and biases towards the end of the sample period, i.e., when the difference between the year of migration and the year of observation is necessarily smaller.

Table C.1 repeats the baseline regression with different definitions for the migrants that are included in the count. Column 1 reports the results when using a two-year period of inclusion, showing slightly more pronounced effects of these policies. The results based on a four-year horizon are comparable to those of the three-year horizon. Further horizons up to 6 and 8 years lead to marginal shifts in the coefficients, with the second lag of the policy announcement significant at the 10% level when migrant observations up to eight years since the reported year of migration are included.

Table C.1: Varying the criteria for inclusion in recent migrant observation: impact of ITP on log flow

Impact of IPT on migration flow-varying years chosen for recent migrants								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
reporting horizon	2 yrs	2 yrs.	4 yrs.	4 yrs.	6 yrs.	6 yrs.	8 yrs.	8 yrs.
ITP destination (t+2)		0.0120		0.201		0.251		0.217
		(0.214)		(0.178)		(0.157)		(0.154)
ITP destination (t)	0.300*	0.217	0.108	0.0864	0.0244	0.00792	0.0139	-0.00171
	(0.156)	(0.202)	(0.112)	(0.134)	(0.0953)	(0.120)	(0.0922)	(0.117)
ITP destination (t-1)	0.462***	0.533**	0.431***	0.398**	0.377***	0.346**	0.375***	0.342**
	(0.161)	(0.263)	(0.119)	(0.191)	(0.0976)	(0.151)	(0.0947)	(0.148)
ITP destination (t-2)	0.195	0.304	0.186	0.241	0.130	0.211	0.129	0.212*
	(0.161)	(0.249)	(0.120)	(0.173)	(0.102)	(0.132)	(0.0989)	(0.128)
Observations	3,134	3,185	3,799	3,833	3,953	3,967	3,962	3,973
Match bin year FE	yes	yes	yes	yes	yes	yes	yes	yes
Origin Destination FE	yes	yes	yes	yes	yes	yes	yes	yes
Origin year FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes. Estimated with a pseudo-Poisson model. The different columns denote the time horizon since migration for inclusion in the measurement of migration. "ITP" refers to the industrial transfer status at the destination. The time indicator "t" indicates that the dummy is one in the year of an announcement. The indicator "t-1" refers to the status assignment in the year before (the coefficient measures the impact one year after assignment). Standard errors clustered at the city level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

C.3 Impact dynamics

The results in Table 2 suggest a short-lived impact of ITP statuses on migration flows. In Table C.2, we first extend the horizon to three years post announcement (columns 2 and 3). We find no significant impacts from the second year since announcement onward, and no pre-policy difference between treated and control cities (column 3, coefficient in the 2-year lead).

Table C.2: Impact of ITP on log migration flow with different lags

	(1)	(2)	(3)
ITP destination (t+2)	0.12		0.06
	(0.16)		(0.19)
ITP destination (t)	0.13	-0.01	0.00
	(0.17)	(0.14)	(0.14)
ITP destination (t-1)	0.47**	0.30**	0.33**
	(0.22)	(0.14)	(0.14)
ITP destination (t-2)	0.17	0.07	0.11
	(0.21)	(0.15)	(0.15)
ITP destination (t-3)		0.15	0.24
		(0.16)	(0.17)
Observations	4,097	4,130	4,168
Match bin year FE	yes	yes	yes
Origin Destination FE	yes	yes	yes
Origin year FE	yes	yes	yes

Notes. Estimated with a pseudo-Poisson model. "ITP" refers to the industrial transfer status at the destination. The time indicator "t" indicates that the dummy is one in the year of an announcement. The indicator "t-1" refers to the status assignment in the year before (the coefficient measures the impact one year after assignment). Standard errors clustered at the city level. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

C.4 Migrant attrition

The main results focus on inflows of migrants, suggesting a short-lived elevation of the inflows. It is not uncommon for Chinese migrants to migrate for spells of only a few years. However, if ITP policies attract migrants that typically stay longer, the migrant stock may still be larger in ITP cities over the years. As most assignments are relatively recent and not all later years are covered by the migration surveys, this Appendix presents circumstantial evidence on the development of migrant stocks.

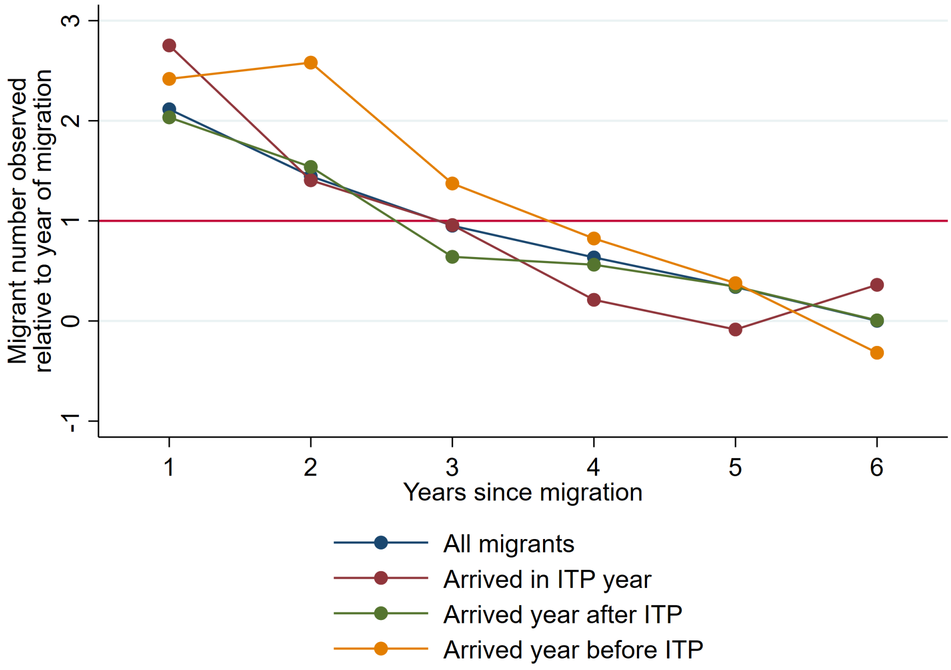
To examine the development of observed migrant numbers, we calculate a cohort's number of observed migrants in a year relative to the number observed in the year of migration as a measure of attrition (indexed at 1 in the year of migration). We then regress the observed ratio on the years since migration. Figure C.2 plots the coefficient per year since migration of the regression, to show how the number of migrants observed develops over time relative to their year of migration. The regression is $\frac{migrant_{d.\tau+t}}{migrants\tau} = \sum_k \beta_k \times D(k)_{dt}$, where $D(k)_{dt}$ is an indicator variable for the years since the cohort migrated. In year 1, the estimate is typically above one, as per sample construction, migrants are less likely to be observed in the year in which they migrate.

The line "all migrants" shows that the migrant stock observed steadily declines over time, with half the number of migrants observed after year 4 as compared to year 1. The line "arrived in ITP year" plots migrant attrition for cohorts that arrived in a city in a year of ITP status announcement. The development of ratios observed of the cohort arriving in the year of ITP assignment or one year after ITP assignment is similar to that of the other migrants. The cohort that arrives in the year before the ITP status assignment is somewhat more likely to still be observed in the assignment year.

For a more formal test of differential developments in the share of staying migrants, we regress the migrant numbers observed on indicators for the cohort's years since migration, conditional on cohort effects. The regression is:

$$\frac{migrant_{d.\tau+t}}{migrants\tau} = \sum_k \beta_{1k} D\left(t = 1,2,3,4,5\right) + \sum_k \beta_{2k} D\left(t = 1,2,3,4,5\right) * D\left(ITP_{d\tau}\right) + \alpha_{mt} + u_{dt}$$

Figure C.2: Development of number of migrants observed by year after arrival for different cohorts



In this regression, $D(t = 1, 2, 3, 4, 5)$ are dummies for every year since migration, explaining the development of $\frac{migrant_{d,\tau+t}}{migrants\tau}$ —the ratio of migrants observed in the year relative to the initial year of the cohort, τ. The interaction with $D(ITP_{d\tau})$ allows for differential effects in the attrition of the migrant stock of an ITP city relative to its matched city (i.e., conditional on pair-year fixed effects α_{mt}.

Table C.3 reports the result of the regression. Column 2 shows a regression in which the cohort's years since migration indicators are interacted with a dummy for cohorts that arrived to a city that received an ITP status in the year of arrival. Cohorts that arrive in the year of announcement show no significantly different decline in observation rates from other cohorts. A joint F-test of the set of interactions between years since migration and the indicator for affected cohorts is not significant. This holds true when defining the affected cohorts to include the cohort that arrives the year after the ITP status, or when including any cohort that arrives in a city with an ITP status.

C.5 Policy impact estimates by distance between the migrant origin and the destination cities

Table C.3: Cohort size development: Migrants observed relative to year of migration

	(1) in ITP year	(2) in ITP year	(3) $\leq$ 2 years after ITP	(4) anytime after ITP
Time since migration				
1 year	2.44***	2.49***	2.56***	2.54***
	(0.23)	(0.23)	(0.25)	(0.23)
2 years	1.53***	1.58***	1.65***	1.61***
	(0.22)	(0.22)	(0.24)	(0.25)
3 years	0.98***	1.01***	1.12***	1.08***
	(0.25)	(0.26)	(0.29)	(0.29)
4 years	0.77***	0.78**	0.89**	0.89**
	(0.29)	(0.32)	(0.37)	(0.35)
5 years	0.45*	0.46*	0.56*	0.49
	(0.25)	(0.27)	(0.30)	(0.31)
ITP interacted with time since migration				
1 year		-0.92	-0.86	-0.94
		(1.30)	(0.61)	(1.49)
2 years		-0.87	-0.63	0.00
		(1.07)	(0.49)	(1.42)
3 years		-0.24	-0.57	-0.17
		(0.94)	(0.49)	(1.13)
4 years		0.12	-0.30	-0.31
		(0.48)	(0.42)	(1.09)
5 years		0.14	-0.22	0.66
		(0.42)	(0.39)	(0.59)
Observations	1,381	1,381	1,381	1,329
R-squared	0.37	0.37	0.38	0.37
bin-year FE	no	yes	yes	yes
city FE	yes	yes	yes	yes
F-test differential trend (p-value)		0.913	0.496	0.872

Notes. OLS regression of a cohort's observed number of migrants relative to the number of migrants that arrived, explained from dummies for the years since migration. "F-test differential trend" reports the p-value for a joint F-test for the dummies for years since migration interacted with an indicator for cohorts that are affected by the ITP status assignment. The different columns vary the definition for whether a cohort is affected as: the cohort in the year of ITP status assignment (2), the cohort in the year of ITP status assignment or 1 year after (3), any cohort that arrives during or after the ITP status assignment (4). Standard errors clustered at the city level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

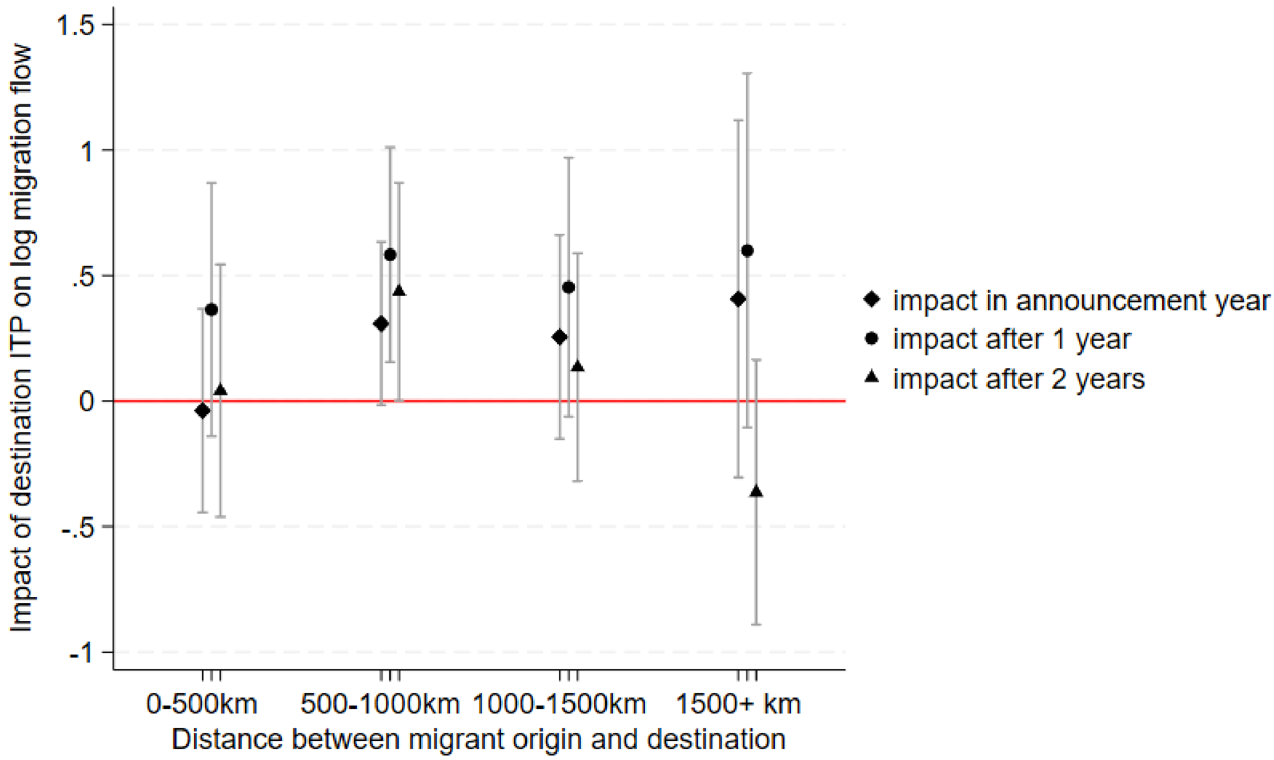
Table C.4: Impact of ITP on within-city migration flow

VARIABLES	(1) Poisson
ITP destination (t)	0.40
	(0.30)
ITP destination (t-1)	0.59***
	(0.14)
ITP destination (t-2)	0.69***
	(0.16)
Observations	104
sample	matched
Origin year FE	yes
Origin Destination FE	yes
matched bin year FE	yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the city level.

Notes. Estimated with a pseudo-Poisson model. "ITP" refers to the industrial transfer status at the destination. The time indicator "t" indicates that the dummy is one in the year of an announcement. The indicator "t-1" refers to the status assignment in the year before (the coefficient measures the impact one year after assignment). Standard errors clustered at the city level. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

Figure C.3: Impact of the ITP status on migrant flows by distance between migrant origin and destination city



Notes: The vertical lines indicate the 95% confidence intervals. The estimates are from the baseline Poisson model with match bin year fixed effects as well as city-level fixed effects, with coefficients varying by distance band as:

$$\log M_{odt} = \sum_k \sum_\delta \beta_{\delta,k} I(\delta) \times ITP_{d,t-k} + \alpha_{mt} + \alpha_{od} + \alpha_{ot} + u_{odt}.$$

In this estimating equation, δ indicates the distance group between the migrant's origin and the destination city (0-500km [representing 14% of the sample used in estimation]; 500-1000km [28%]; 1000-1500km [26%]; 1500+km [32%]). The variable $I(\delta)$ is an indicator variable for the distance band indexed by δ. The years relative to the policy k run from the pre-policy year (used as the baseline) up to two years after the policy announcement.

D Randomization inference

Figure D.1: Randomization inference coefficient distribution for the coefficient of year of announcement

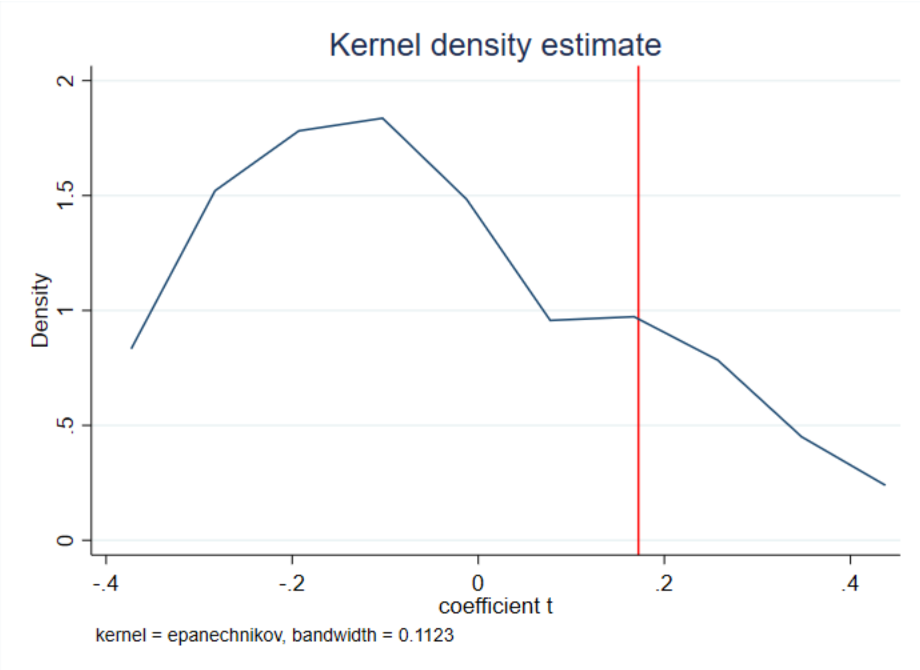


Figure D.2: Randomization inference coefficient distribution for the coefficient of 1 year after announcement

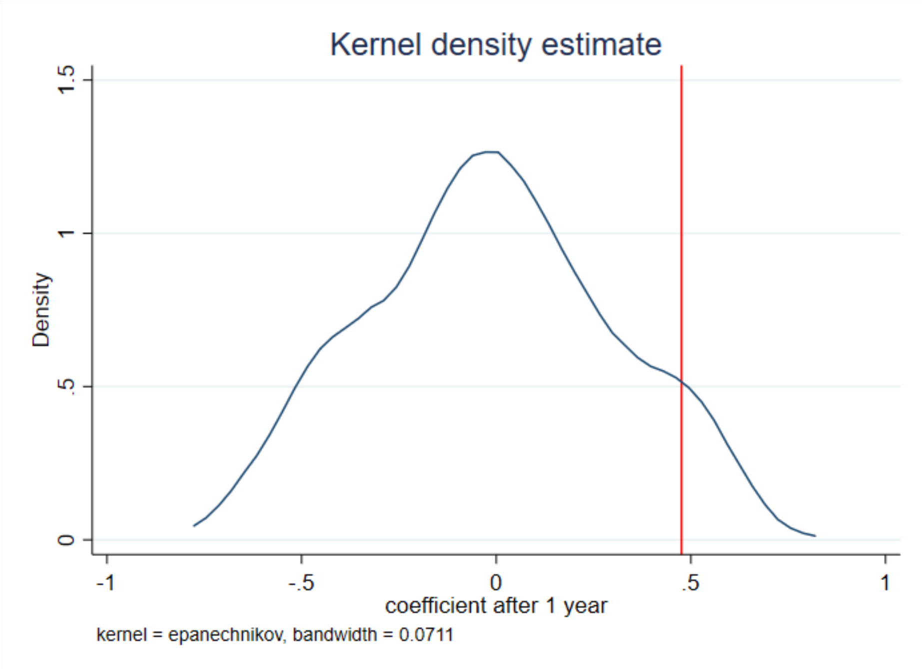
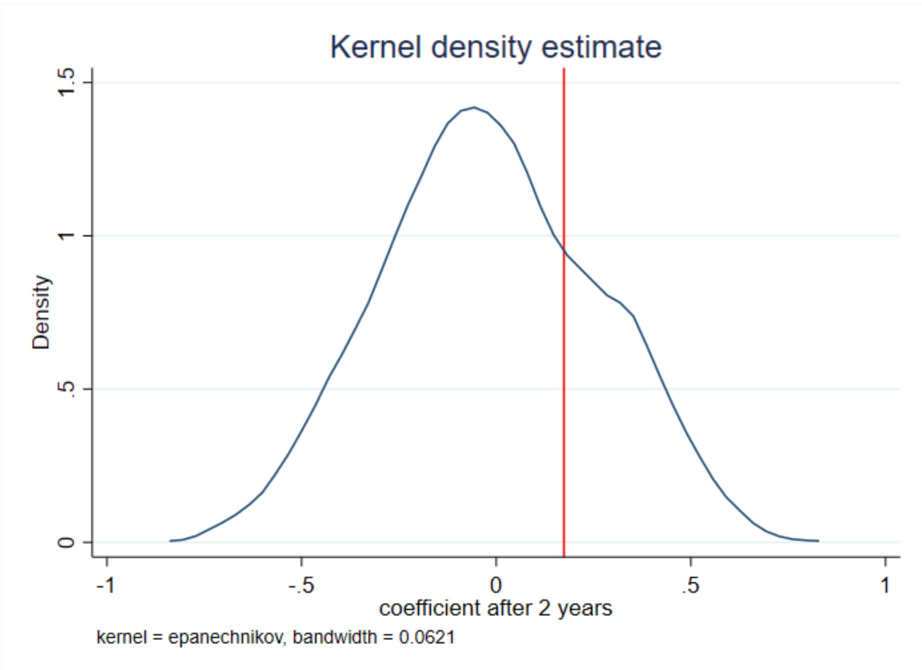


Figure D.3: Randomization inference coefficient distribution for the coefficient of 2 year after announcement



E Different matching strategies

This Appendix explores alternative strategies to match ITP cities to cities with a similar likelihood of receiving the ITP status. Table E.1 shows the results with the coefficients based on matching to the closest match for reference in column 1.

The results in column 2 are based a maximum restriction on propensity differences between the matched cities. Out of the 29 treated cities, two have status assignment probabilities substantially higher than those of all other cities. Such a high assignment probability might imply that those cities are (very) different from the other cities. Pairing these treated cities to comparison cities leads to two pairs for which the treatment probability is considerably different—over 10% points apart in propensity. Dropping these cities from the sample leads to comparable outcomes as in the main sample.

Column 3 drops pair matching in favor of coarsened exact matching. ITP cities are matched with cities in the same of six quantiles on three variables: GDP per capita, wages, and secondary sector size. Between the ITP cities and the control cities, this restricts the measurement of similarity to the main covariates that explain the assignment rather than to the propensity score. This leads to a different, larger set as every ITP city still has at least one match. The coefficient estimate obtained from coarsened exact matching is positive but lower than the coefficient obtained with propensity matching.

One concern with the matching strategy is that comparison cities stem from the same area as the treated city. For instance, if economic circumstances are similar within the provinces, it is particularly likely that the comparison and treated cities will be from the same area. The potential problem could be that the comparison city is not isolated from the policy if it is near a treated city. If workers migrate to the nearby treated city, the matching strategy overstates the policy impact; it measures both the inflow into the treated city and the outflow from the comparison city. Column 4 shows the results based on an alternative matching strategy; it only allows for comparison cities that are not in the same province. That restriction may reduce the similarity between treated and comparison cities, but it improves the isolation of comparison cities from the policy. The results are similar between the two strategies, suggesting that the spillovers from the policy do not overstate our measured impacts.

Table E.1: Impact of ITP on migration stocks with difference reference groups

	(1) closest match	(2) Conservative	(3) CEM	(4) Outside province
ITP destination (t)	0.13	0.06	-0.01	0.23
	(0.14)	(0.13)	(0.08)	(0.15)
ITP destination (t-1)	0.48***	0.49***	0.17**	0.65***
	(0.15)	(0.15)	(0.07)	(0.16)
ITP destination (t-2)	0.17	0.19	0.03	0.30*
	(0.15)	(0.14)	(0.09)	(0.17)
Observations	3,624	3,176	5,832	3,648
Origin year FE	yes	yes	yes	yes
Origin Destination FE	yes	yes	yes	yes
matched bin year FE	yes	yes	yes	yes

Notes. Estimated with a pseudo-Poisson model. The different columns refer to different methods of selecting matches (see text): closest match in terms of propensity of receiving ITP assignment (1); closest match with a maximum on propensity differences (2); coarsened exact matching (3); and closest match with the restriction that the match cannot be situated in the same province (4). ”ITP” refers to the industrial transfer status at the destination. The time indicator ”t” indicates that the dummy is one in the year of an announcement. The indicator ”t-1” refers to the status assignment in the year before (the coefficient measures the impact one year after assignment). Standard errors clustered at the city level. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

F Results when excluding cities potentially confounded by governance setting of by simultaneous development policies.

Our main result is based on estimation of all ITP cities. Nevertheless, these treated cities have distinct features. Some differ substantially in the institutional context or some are subject to coincident policies. Therefore, we analyze on different subsamples to test the robustness of our main result. More specifically, we drop: (1) All cities treated after 2014, which might be subject to one of the national reforms on hukou system; (2) Western provinces, which might be subject to the Great Western Development Program; (3) Chongqing, the central-governed municipality; (4) Ningxia and Guangxi provinces, which reside a large population of minorities; (5) Henan, Shanxi and Shaanxi provinces, which host the only one cross-province ITP zone.

As is shown in Table F.1, the subsample analysis turns out to yield similar estimates as our main result.

Table F.1: Impact of IPT on log migration flow: robustness checks for different subsamples

	(1)	(2)	(3)	(4)	(5)
ITP destination (t)	0.15	0.31	0.16	0.10	-0.02
	(0.17)	(0.27)	(0.18)	(0.17)	(0.12)
ITP destination (t-1)	0.52**	0.76**	0.51**	0.50**	0.28*
	(0.22)	(0.31)	(0.23)	(0.22)	(0.16)
ITP destination (t-2)	0.27	0.38	0.18	0.15	0.01
	(0.22)	(0.31)	(0.23)	(0.21)	(0.17)
Observations	3,318	2,201	3,426	3,014	3,078
Match bin year FE	yes	yes	yes	yes	yes
Origin Destination FE	yes	yes	yes	yes	yes
Origin year FE	yes	yes	yes	yes	yes

Notes. Estimated with a pseudo-Poisson model. ”ITP” refers to the industrial transfer status at the destination. The time indicator ”t” indicates that the dummy is one in the year of an announcement. The indicator ”t-1” refers to the status assignment in the year before (the coefficient measures the impact one year after assignment). Column (1) excludes observations after 2014. Column (2) excludes western provinces. Column (3) excludes Guangxi and Ningxia. Column (4) excludes Henan, Shaanxi and Shanxi. Column (5) excludes western provinces. Standard errors clustered at the city level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

G Census-level counterfactuals

The generation of new migrants in response to ITP policies may be an important source of population adjustment. The main text reports a counterfactual analysis based on the assumption that the stock of migrants remains constant. That analysis is likely to understate that actual number of migrants, as ITP policies may incite new internal migration. This section lays out the assumptions to estimate newly generated migrants.

First, the migration equation effectively identifies:

$$\pi_{od} = \frac{B_{od}\omega_d}{\sum_{d'} B_{od'}\omega_{d'}}$$

where o refers to the province of origin, and $\omega_d = \left(a_d \frac{w_d}{r_d^{\alpha}}\right)^{\varepsilon}$ captures destination-level prices and amenities. This equation does not apply one-for-one for non-migrants, whose origin city, say $d1$, may be their destination city: migrants, even from cities inside the same province, are not likely to hold the same preferences towards the city as natives do.

The main assumption we introduce to produce an estimate of migration responses by locals is that they face the same amenities and prices and elasticities as natives do (ω_d). Differently put: we analyze how many new migrants are produced by ITP policies if natives' differential preferences can be traced back to their idiosyncratic preference B for their home city. As a result, the preference for home is $B_{d1,d1}$ instead of $B_{o,d1}$. In addition, we assume that if natives become migrants, they hold the same preferences for other cities as the current migrants in their province do.

Under this assumption, the choice probability not to migrate (and to be a home resident) satisfies:

$$\pi_{d1,d1} = \frac{B_{d1,d1}\omega_d}{\sum_{d'} B_{od'}\omega_{d'} + B_{d1,d1}\omega_d - B_{o,d1}\omega_d}$$

where $\sum_{d'} B_{od'}\omega_{d'} + B_{d1,d1}\omega_d - B_{o,d1}\omega_d$ is the utility index of migrants from the same province, corrected for the fact that the native holds a different preference for his home city. Noting that $B_{od}\omega_d = \pi_{od}\sum_{d'} B_{od'}\omega_{d'}$, we can write $\pi_{d1,d1} = \frac{B_{d1,d1}\omega_d}{(1-\pi_{o,d1})\sum_{d'} B_{od'}\omega_{d'} + B_{d1,d1}\omega_d}$. Isolating $B_{d1,d1}\omega_d$ gives:

$$B_{d1,d1}\omega_d = \frac{\pi_{d1,d1}}{1-\pi_{d1,d1}}\left(1-\pi_{o,d1}\right)\sum_{d'} B_{od'}\omega_{d'}.$$

The change in the number of natives choosing to live in their home city is a negative measure of the out-migrants in the city. Hence, $-Pop_d \frac{d\pi_{d1,d1}}{dITP}$ is number of new migrants that city $d1$ produces in response to the ITP policy (which could be a negative number). The change in choice probability is: The factors that produce new migrants from city $d1$ are intuitive: they are the relative change in the attraction of the city itself if assigned the ITP status, $\frac{\frac{d\omega_d}{dITP}}{\omega_d}$, minus the change in the multilateral resistance term for migrants originating in that province. That difference is multiplied with $\pi_{d1,d1}\left(1-\pi_{d1,d1}\right)$ to adjust for the fact that natives hold a stronger preferences for their city than migrants do. The term $\pi_{d1,d1}$ is obtained from the 2010 Census; and $\frac{d\omega_d}{dITP}/\omega_d$ and $\frac{d\left(\sum_{d'} B_{od'}\omega_{d'}\right)}{dITP} / \sum_{d'} B_{od'}\omega_{d'}$ are obtained from the structural model.

This estimated number of migrants can be distributed over other cities in a counterfactual, but there is a caveat: some of those predicted migrants would return to $d1$ (and hence would not be migrants). If share $\pi_{o,d1}$ of all migrants in province o goes d_1, then transplanting the assumption about migrant distribution means that the number of migrants from d_1 in other cities is $outmigrants_{d1} = (1-\pi_{o,d1}) * newmigrants_{d1}$. Hence, to circumvent this return of migrants, we premultiply $\frac{d\pi_{d1,d1}}{dITP}$ with $1/\left(1-\pi_{o,d1}\right)$ such that city d_1 contributes -$Pop_d \frac{d\pi_{d1,d1}}{dITP} / \left(1-\pi_{o,d1}\right)$ new migrants to the auxiliary provincial stock of newly generated migrants, of which a share $\pi_{o,d1}$ eventually returns to d_1. In this way, when city d_1 is predicted to generate new migrants, that number of migrant distributes over cities other than d_1.

Using these results, cities can grow or shrink along two margins. The diversion of existing migrants (expressed relative to resident population) is:

$$\text{diversion}_d = \frac{\sum_o \left(\pi_{od}^{ITP} - \pi_{od}\right) \text{flow}_{od}}{\text{population}_d}, \tag{G.1}$$

whereas the margins of newly generated migrants is:

$$\text{creation}_d = \frac{\sum_o \left(\pi_{od}^{ITP}\right) \Delta outmigrants_o}{\text{population}_d}, \tag{G.2}$$

These can be combined into:

$$\text{combined}_d = \frac{\sum_o \left(\pi_{od}^{ITP} - \pi_{od}\right) \text{flow}_{od} + \sum_o \left(\pi_{od}^{ITP}\right) \Delta outmigrants_o}{\text{population}_d}. \tag{G.3}$$

Note that it is not immediately clear whether combined$_d$ is larger or smaller than diversion$_d$, as the correlation between diversion and creation is not clear.

Finally, the city-level changes in size can be aggregated to measure how far the aggregated pattern of urban population differs between a scenario with and without the ITP policy.

$$\begin{aligned} \text{Diversion} &= \sum_d |\text{diversion}_d| \\ \text{Creation} &= \sum_d |\text{creation}_d| \\ \text{Combined} &= \sum_d |\text{diversion}_d + \text{creation}_d| \end{aligned} \tag{G.4}$$

Note that these measures are an overestimate of how many people would need to be relocated to return to the non-ITP situation, because relocating one person would bring two locations closer to the non-ITP situation. By the same token, these measures may be an underestimate of how many people have relocated because they look at city aggregates: if some people moved but other arrived, the city-level change understates the number of people who have moved.

Figure G.1 shows the net inflow (relative to resident population) of migrants following the ITP policy. The dynamics are not surprising: ITP cities receive migrants, while most other areas contribute. The migration generation numbers from Figure G.1, combined with the diversion numbers from Figure 2, produce the combined impacts shown in Figure 3.

The distribution of changes from either margin are plotted in Figure G.2. The diversion number are generally closer to zero than the generation numbers, which show a slight peak around 1.5%. The peak of the combined impact is somewhat larger (just over 2%) as the population changes due to diversion and generation correlate.

Figure G.3 plots the estimated rate of outmigration by area, as predicted by equation **??**.

Figure G.1: Change in population as a result of migrant creation after ITP

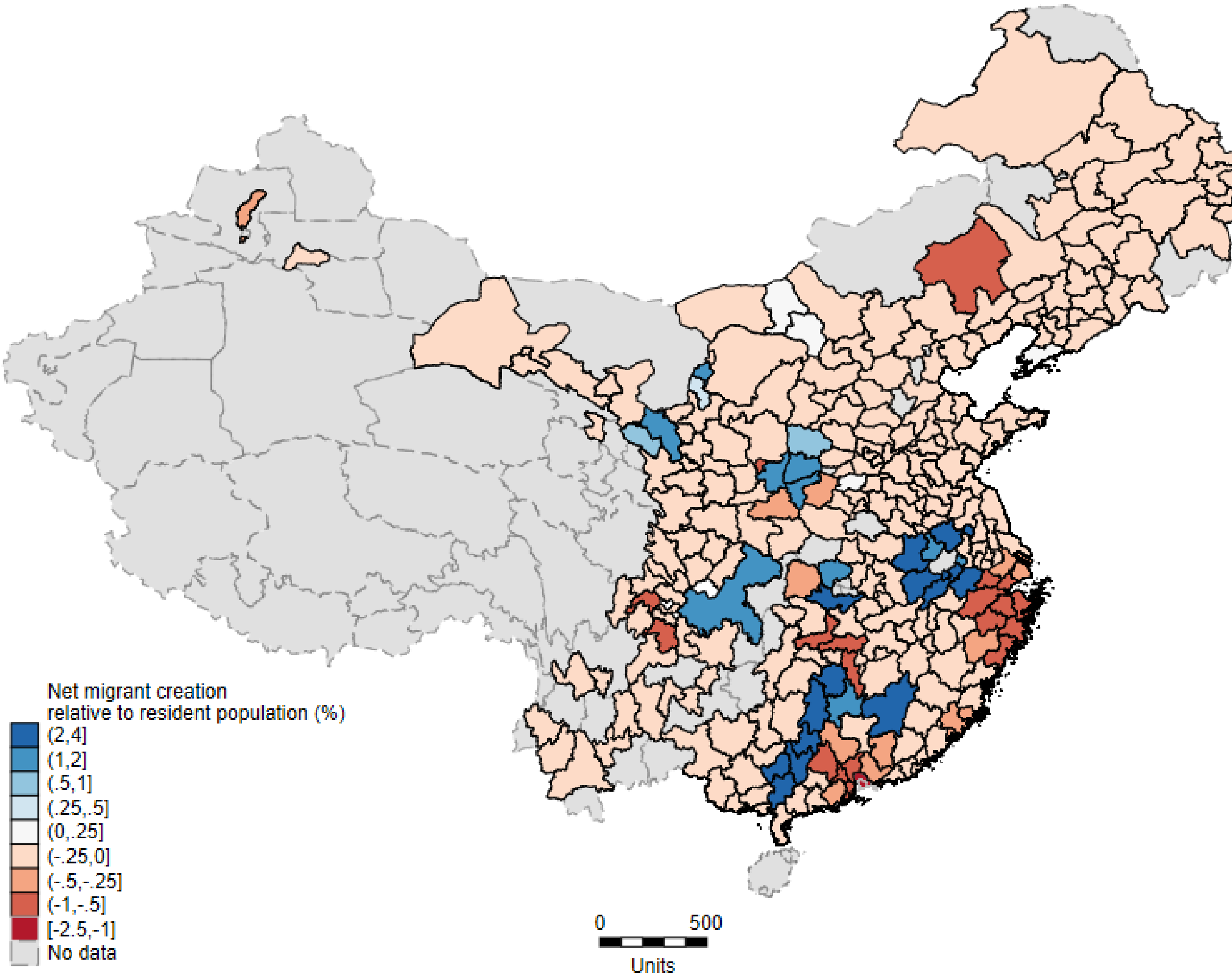


Figure G.2: Population change distributions

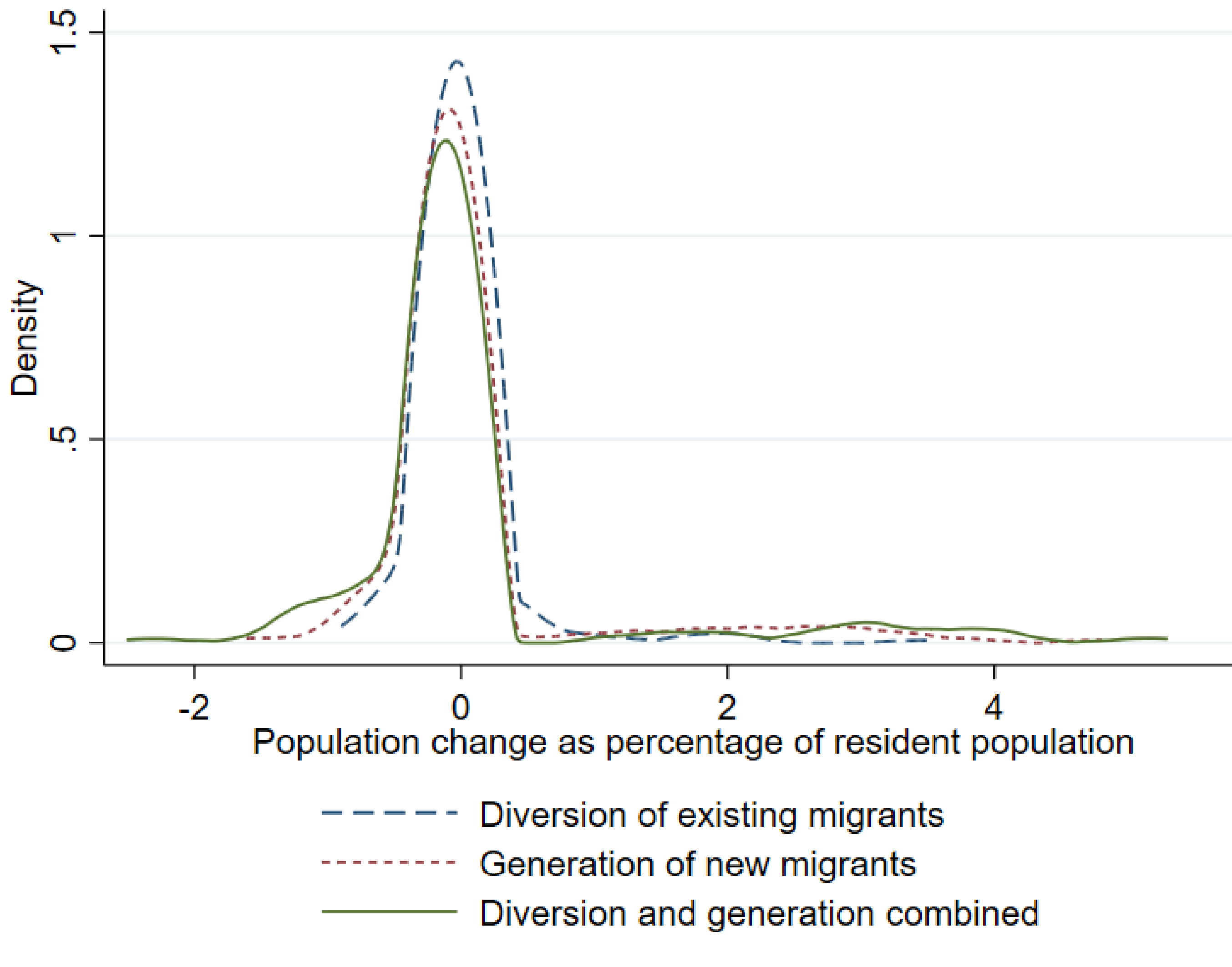
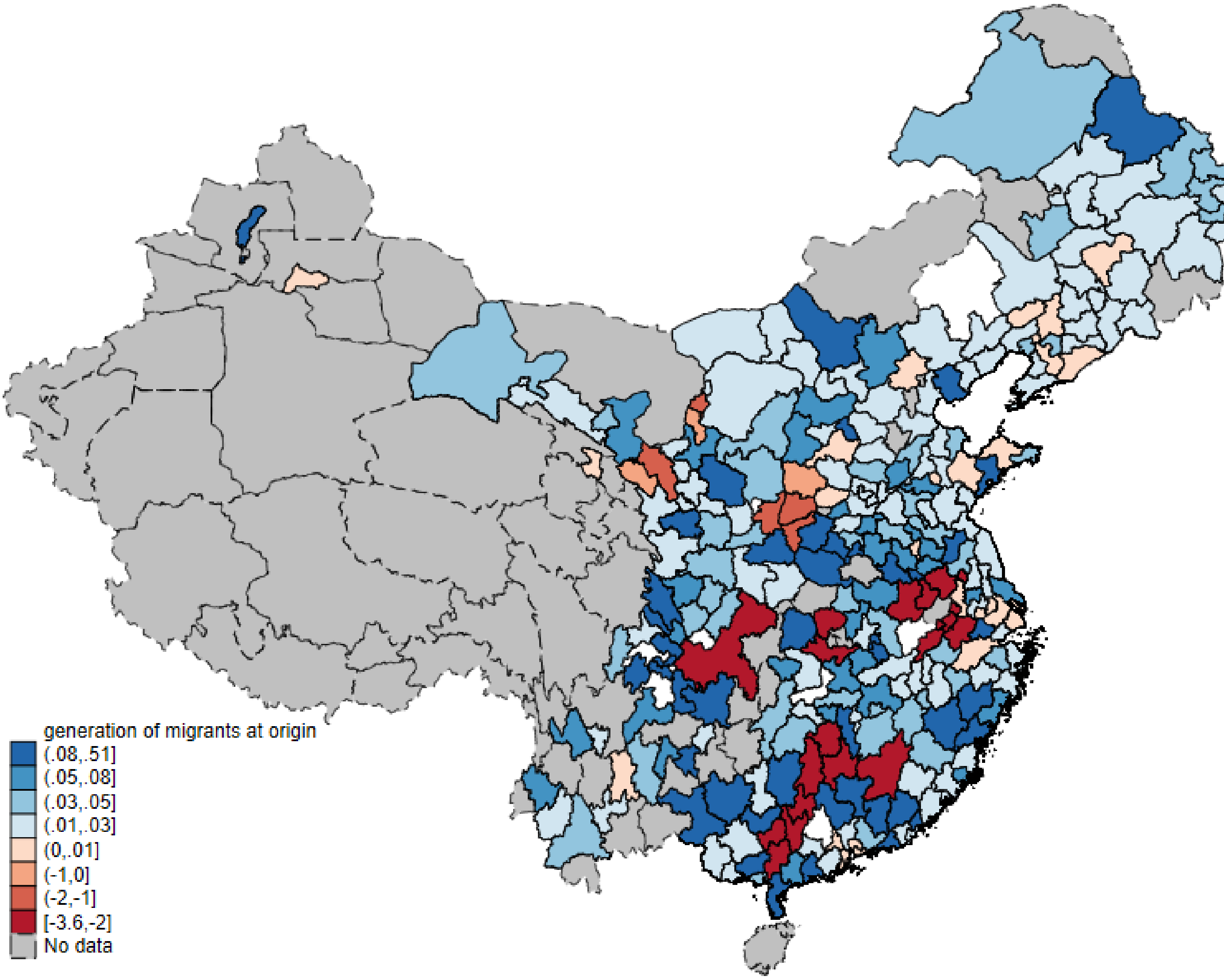


Figure G.3: Percentage of resident population turning to new migrant after ITP



H Secondary benefits

The policy documents supporting ITP list secondary benefits that could motivate migration. Additionally, an ITP status in the city is sometimes viewed as a government commitment to local development, and is plausibly associated with other pull factors, such as lower expected unemployment rates.

First, the ITP policy documents list access to credit as an incentive, potentially leading to larger self-employment rates. Relatedly, local place-based policies and the worker movements they bring about have been argued to generate a demand for self-employed entrepreneurs in retail, accommodation and catering (Zheng et al., 2017). Second, access to education may play a role in the migration decision. As local school access is tied to the local hukou, migrant children in most coastal provinces have no access to public education and often attend local schools of lower quality (Lai et al., 2014). To exploit localized access to public education, migrant workers frequently leave children behind with family members, although this is also associated with poorer educational outcomes (Zhang et al., 2014). In cities that receive an ITP status, the limitations on public school quotas are less strict, allowing migrant children access to public education in the host city—potentially encouraging migration to ITP cities. Finally, migration is frequently supported by government subsidies on housing or provisions of accommodation by the employing companies, reducing the cost of migration (Niu et al., 2021).

Table H.1: Non wage benefits from migration associated with ITP

	(1) Share Unemp	(2) Share Selfemp	(3) Share Selfemp service	(4) Share Services	(5) Share child brought	(6) Share Subs accom.	(7) Share Comp accom
ITP destination (t)	0.05***	0.02	0.03*	0.03**	0.00	0.02	0.02
	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
ITP destination (t-1)	0.06***	0.05***	0.04***	0.03	0.04*	0.01	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
ITP destination (t-2)	0.06***	0.01	0.02	0.01	0.00	0.02	0.02
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	2,414	6,386	6,386	6,386	5,673	5,890	5,890
R-squared	0.52	0.53	0.49	0.52	0.59	0.49	0.48
Origin year FE	yes	yes	yes	yes	yes	yes	yes
Origin Destination FE	yes	yes	yes	yes	yes	yes	yes
matched bin year FE	yes	yes	yes	yes	yes	yes	yes

Notes. ”Unemp” denotes the share of unemployed people. ”Selfemp” denotes the share of self-employed migrant workers. ”child brought” denotes the share of migrants that cohabit with children. ”Subs accom” denotes the share of migrants who live in subsidized accomodation. ”Comp Accom” denotes the share of migrants that live in company accommodations. ”ITP” refers to the industrial transfer status at the destination. The time indicator ”t” indicates that the dummy is one in the year of an announcement. The indicator ”t-1” refers to the status assignment in the year before (the coefficient measures the impact one year after assignment). Standard errors clustered at the city level. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

I Results tables for migrant and native specialization following ITP

Table I.1 reports the estimates from equation (13) to capture the change in city-level migrant and native employment shares by sector.

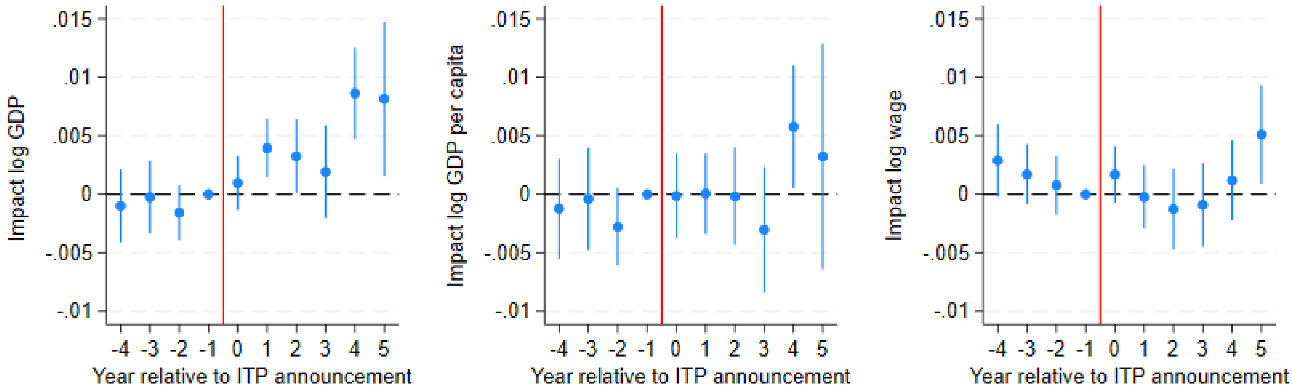
Table I.1: Changes to the sectoral employment shares among migrants and natives

Panel a: Migrants	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	manuf	mining	utility	constru	transport	retail	horeca	finance
ITP (t)	-0.09**	0.00	-0.02	0.02	0.02	0.04	-0.05	0.01**
	(0.04)	(0.04)	(0.02)	(0.03)	(0.02)	(0.05)	(0.04)	(0.01)
ITP (t-1)	0.01	-0.02	-0.02	0.01	0.02	-0.10	0.00	0.01
	(0.04)	(0.04)	(0.02)	(0.03)	(0.02)	(0.06)	(0.03)	(0.01)
ITP (t-2)	-0.04	-0.05	-0.02	-0.00	0.02	-0.01	-0.00	0.02**
	(0.04)	(0.04)	(0.01)	(0.03)	(0.02)	(0.06)	(0.03)	(0.01)
Observations	154	154	154	154	154	154	154	154
R-squared	0.94	0.76	0.68	0.78	0.82	0.85	0.80	0.81
City FE	yes	yes	yes	yes	yes	yes	yes	yes
matched bin year FE	yes	yes	yes	yes	yes	yes	yes	yes
Panel b: Natives	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	manuf	mining	utility	constru	transport	retail	horeca	finance
ITP (t)	-0.02	0.00	-0.00	-0.01	0.00	-0.01	0.01	0.00
	(0.02)	(0.01)	(0.00)	(0.02)	(0.00)	(0.01)	(0.01)	(0.00)
ITP (t-1)	-0.03*	-0.00	0.00	-0.02	0.00	0.02*	0.01	0.00
	(0.02)	(0.01)	(0.00)	(0.02)	(0.00)	(0.01)	(0.01)	(0.00)
ITP (t-2)	-0.04**	0.00	0.00	-0.02	0.01	0.01	0.01	0.01
	(0.02)	(0.01)	(0.00)	(0.02)	(0.00)	(0.01)	(0.01)	(0.00)
Observations	206	180	206	206	206	206	206	206
R-squared	0.96	0.99	0.91	0.95	0.94	0.80	0.76	0.92
City FE	yes	yes	yes	yes	yes	yes	yes	yes
matched bin year FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes. Manuf. refers to manufacturing. Constru. refers to construction. Transport includes ICT infrastructure. Finance includes real estate and commercial services. ”ITP” refers to the industrial transfer status at the destination. The time indicator ”t” indicates that the dummy is one in the year of an announcement. The indicator ”t-1” refers to the status assignment in the year before (the coefficient measures the impact one year after assignment). Transport includes ICT infrastructure. Hospitality includes hotels, restaurants and cafes. Finance includes real estate and commercial services. Robust standard errors. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

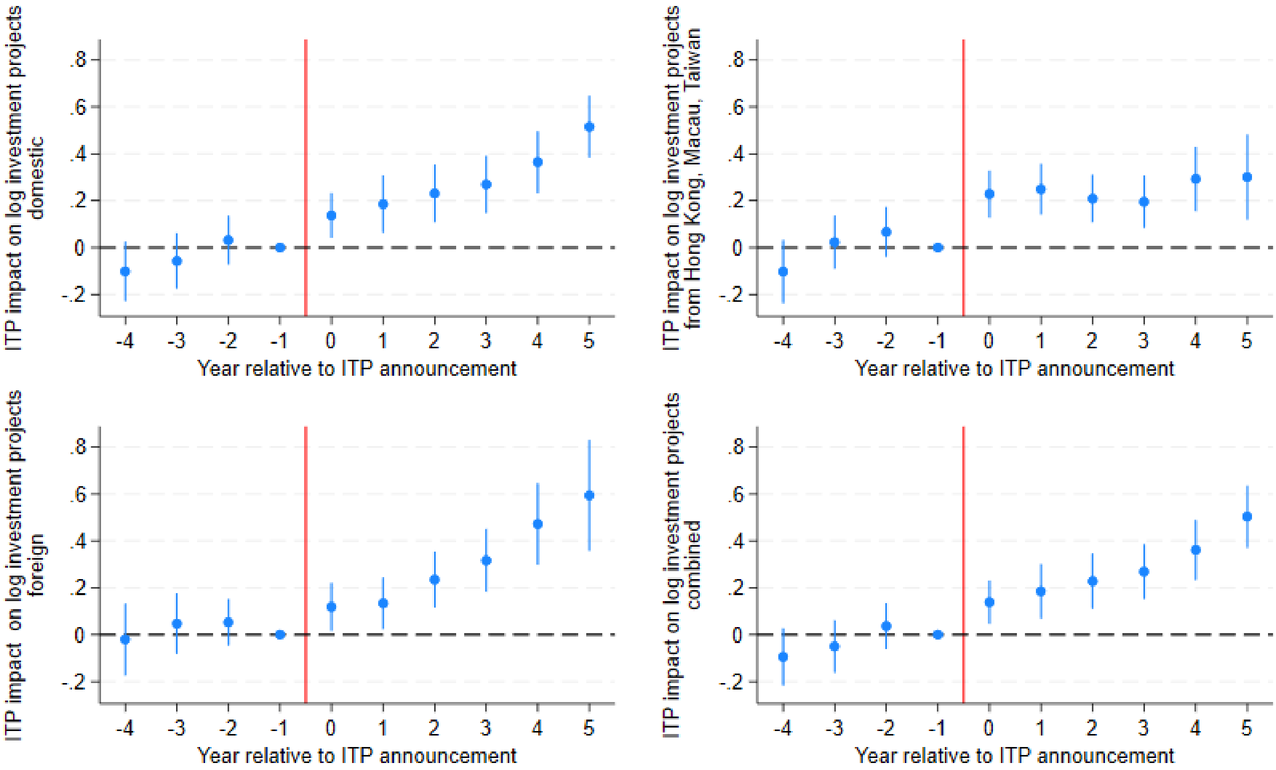
J Year on year impacts on GDP, wage and investment

Figure J.1: Dynamic impacts on GDP and wage



Notes: the vertical red line indicates the timing of the announcement of the policy. The dots indicate the impact of the ITP policy on the log of GDP, GDP per capita, and mean native wages. The estimates are from a Poisson model with match bin year fixed effects as well as city-level fixed effects (see text for details on matching). The lines indicate the 95% confidence intervals based on standard errors clustered at the city level.

Figure J.2: Dynamic impacts on investment



Notes: the vertical red line indicates the timing of the announcement of the policy. The dots indicate the impact of the ITP policy on the log of the number of investment projects by origin. The estimates are from a Poisson model with match bin year fixed effects as well as city-level fixed effects (see text for details on matching). The lines indicate the 95% confidence intervals based on standard errors clustered at the city level.

K Nightlight intensity and pollution changes after ITP status assignment

To check whether our main result for economic activity is robust to non-official GDP measures, We employ the cloud-free composites of the Defense Meteorological satellite program up to and including 2013 and calculate the average nighttime light intensity from the 30-arc second grid for every Chinese city polygon. The results are reported in Table K.1.

Table K.1: Impact of ITP on log nightlights

	(1)	(2)	(3)	(4)	(5)
ITP (t+2)		0.04			
		(0.04)			
ITP (t)	-0.07	-0.06*	-0.21		
	(0.04)	(0.04)	(0.16)		
ITP (t-1)	-0.02	-0.03	-0.08		
	(0.05)	(0.03)	(0.17)		
ITP (t-2)	-0.07	-0.08**	-0.07		
	(0.06)	(0.04)	(0.22)		
log GDP				0.61***	0.45***
				(0.02)	(0.07)
Observations	112	160	1,060	1,589	264
R-squared	1.00	1.00	0.00	0.31	0.74
matched bin year FE	yes	yes	no	no	yes
city FE	yes	yes	no	no	no
year FE	yes	yes	no	no	no

Notes. Outcome variable log nightlights are the logs of city-polygon average luminosities. ”ITP” refers to the industrial transfer status at the destination. The time indicator ”t” indicates that the dummy is one in the year of an announcement. The indicator ”t-1” refers to the status assignment in the year before (the coefficient measures the impact one year after assignment). Robust standard errors. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table K.2 shows impacts of ITP on pollution measures. We employ pollution data from two sources. The city statistical yearbooks from NBS list emission measures of soot; suplhur dioxides and waste water. As there is concern over the accuracy of air pollution measures in official reports (Ghanem and Zhang, 2014), we complement the data with fine particulate matter estimates (PM 2.5, $\mu g/m^3$) from the satellite-based Aerosol Optical Depth retrievals. We calculate them following the standard approach in Buchard et al. (2016).

Table K.2: Pollution

	(1) mean $pm_{2.5}$	(2) median $pm_{2.5}$	(3) max $pm_{2.5}$	(4) min $pm_{2.5}$	(5) soot	(6) so_2	(7) wastewater
ITP destination (t)	-0.04	-0.05	-0.02	-0.03	0.18	0.02	-0.00
	(0.03)	(0.03)	(0.02)	(0.04)	(0.33)	(0.18)	(0.11)
ITP destination (t-1)	0.01	0.01	0.02	0.03	-0.16	-0.12	0.01
	(0.03)	(0.03)	(0.02)	(0.04)	(0.16)	(0.14)	(0.10)
ITP destination (t-2)	-0.05	-0.06*	-0.00	-0.06	-0.02		0.05
	(0.03)	(0.03)	(0.03)	(0.04)	(0.21)		(0.12)
Observations	206	206	206	206	170	204	204
R-squared	0.99	0.99	0.99	0.99	0.94	0.90	0.95
matched bin year FE	yes	yes	yes	yes	yes	yes	yes
city FE	yes	yes	yes	yes	yes	yes	yes
year FE	yes	yes	yes	yes	yes	yes	yes

Notes. All pollution measures in logs. $pm_{2.5}$ refers to the local 2.5 μm particulate matter concentration. Columns 5, 6, and 7 refer to the log of soot and sulphurdioxide concentration and the log of wastewater release, respectively. ”ITP” refers to the industrial transfer status at the destination. The time indicator ”t” indicates that the dummy is one in the year of an announcement. The indicator ”t-1” refers to the status assignment in the year before (the coefficient measures the impact one year after assignment). In column (6), 2 year after ITP treatment is not identified because of missing values in SO_2 emissions in 2017. Robust standard errors. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

L Firm-level analysis

To examine firm-level changes following local ITP policies, we employ Annual Survey of Industrial Firms between 2011 and 2013 conducted by NBS. This dataset includes all industrial firms with sales above 20 million RMB. See Brandt et al. (2014) for more details of this dataset. We restrict our focus to manufacturing firms, which make up 90% of all observations. The sample is further split in two: startups are firms that just opend in the year of observation, existing firms were operational before the announcement of ITP policies. The dependent variables are logarithms of employment size, revenues and capital per capita.

We estimate the equation:

$$\log y_{idt} = \beta ITP_{dt} + \alpha_{mt} + \alpha_{d|i} + u_{dt}, \tag{L.1}$$

where $\log y_{idt}$ is a firm-level outcome and $\alpha_{d|i}$ is city-level fixed effect (for the sample of startups) or a firm fixed effect (for the sample of existing firms). We restrict the coefficient set to one year after the ITP assignment as the time limit of the sample implies that any later lags would be identified from a very small subset of the observations. Table L.1 show the results of the regressions. We only include ITP dummies up until 1 year after the treatment due to the limited years in the sample.

Table L.1: Firm level responses to ITP

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Firm type	startups				existing firms			
Outcome	Log Empl.	Log Rev.	Capital /worker	log TFP	log Empl.	Log Rev.	Capital /worker	TFP
ITP destination	-0.20	-0.18	0.15	-0.14	0.02	0.03	0.04	0.01
	(0.13)	(0.18)	(0.39)	(0.14)	(0.02)	(0.05)	(0.04)	(0.04)
ITP destination (t-1)	-0.07	-0.37*	-0.34	-0.25	0.04	0.00	0.00	-0.08
	(0.17)	(0.21)	(0.34)	(0.16)	(0.03)	(0.07)	(0.04)	(0.05)
Observations	937	979	936	975	41,354	45,999	40,874	41,418
R-squared	0.16	0.14	0.14	0.09	0.91	0.94	0.90	0.91
Industry year FE	yes	yes	yes	yes	yes	yes	yes	yes
Matched bin year FE	yes	yes	yes	yes	yes	yes	yes	yes
City FE	yes	yes	yes	yes				
Firm FE					yes	yes	yes	yes

Notes. Estimates for startups (columns 1 to 4) and for existing firm stock (columns 5 to 8). Log Empl. is the log of employment. Log Rev. is the log of revenue. Cap int. is the capital per worker. log TFP is the log of total factor productivity. TFP is estimated as a the residual of a regression that explains the log of firm revenue from the log of capital, the log of labor and a year fixed effect. The time indicator "t" indicates that the dummy is one in the year of an announcement. The indicator "t-1" refers to the status assignment in the year before (the coefficient measures the impact one year after assignment). Standard errors clustered at the recipient-year level. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

M FOR REVIEW ONLY Matched city pairs on the map

Figure M.1: Wanjiang Zone

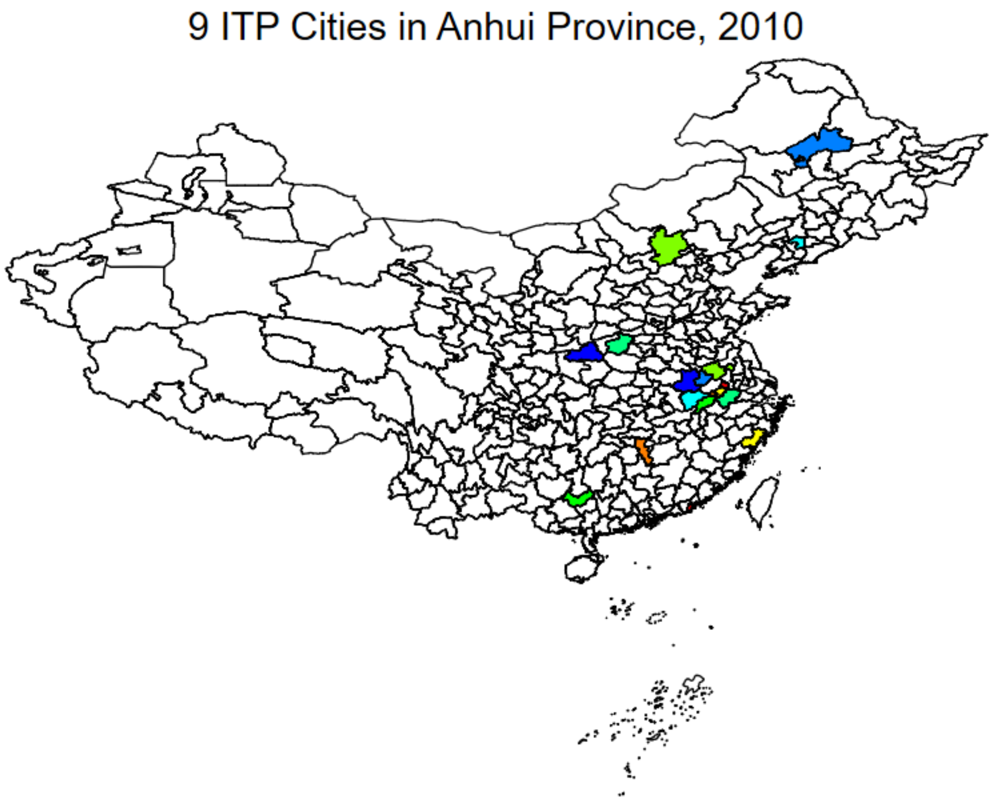


Figure M.2: Guidong Zone

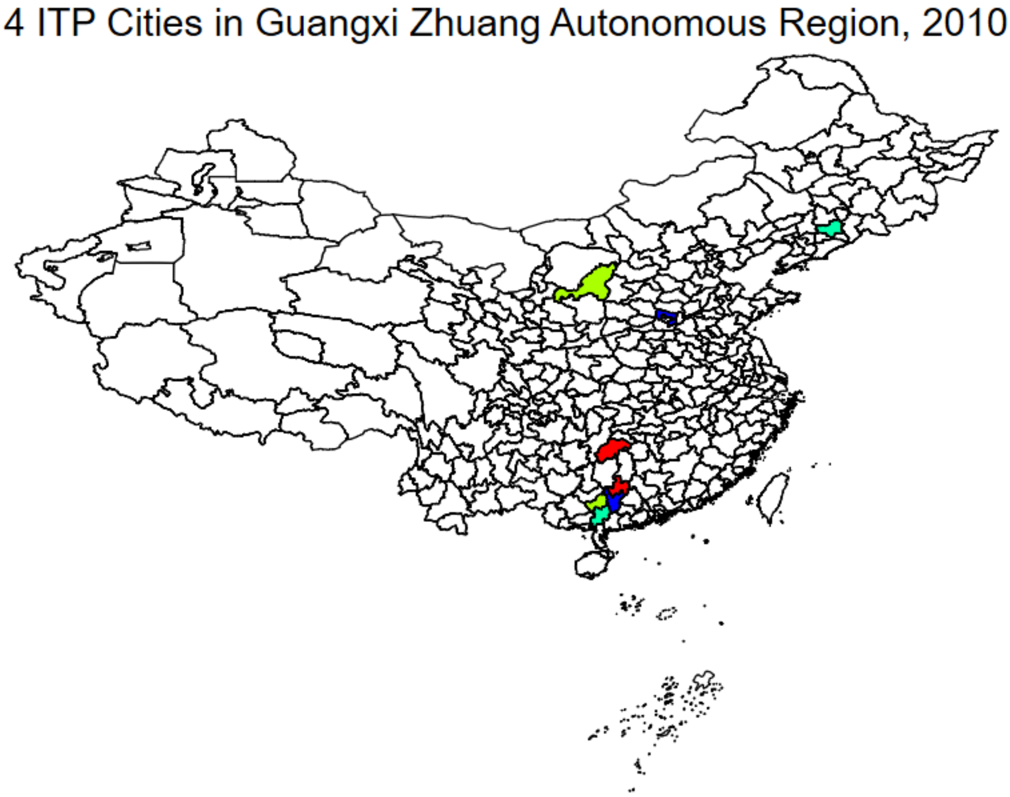


Figure M.3: Chongqing Zone

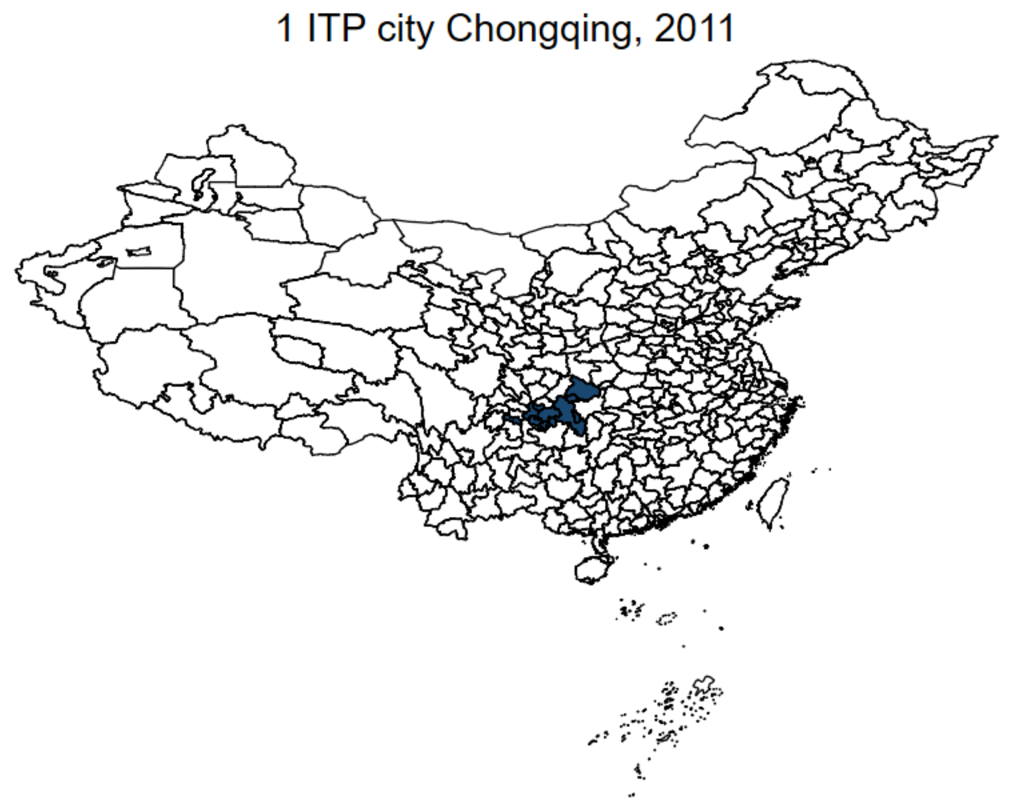


Figure M.4: Xiangnan Zone

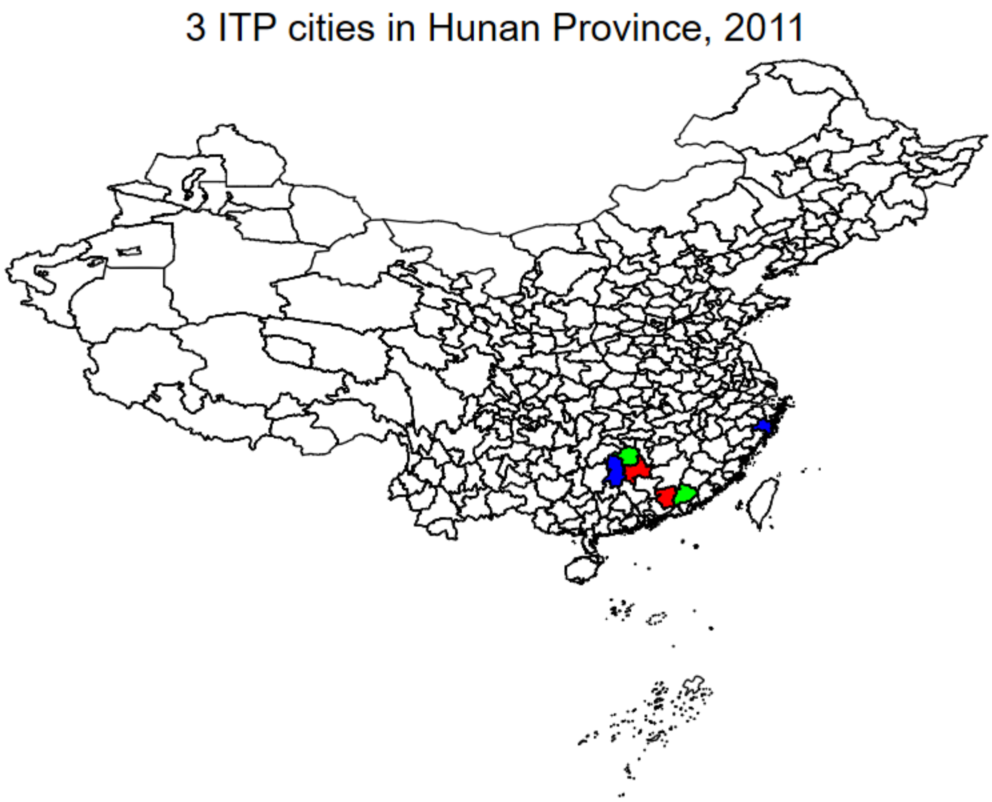


Figure M.5: Hubei Zone

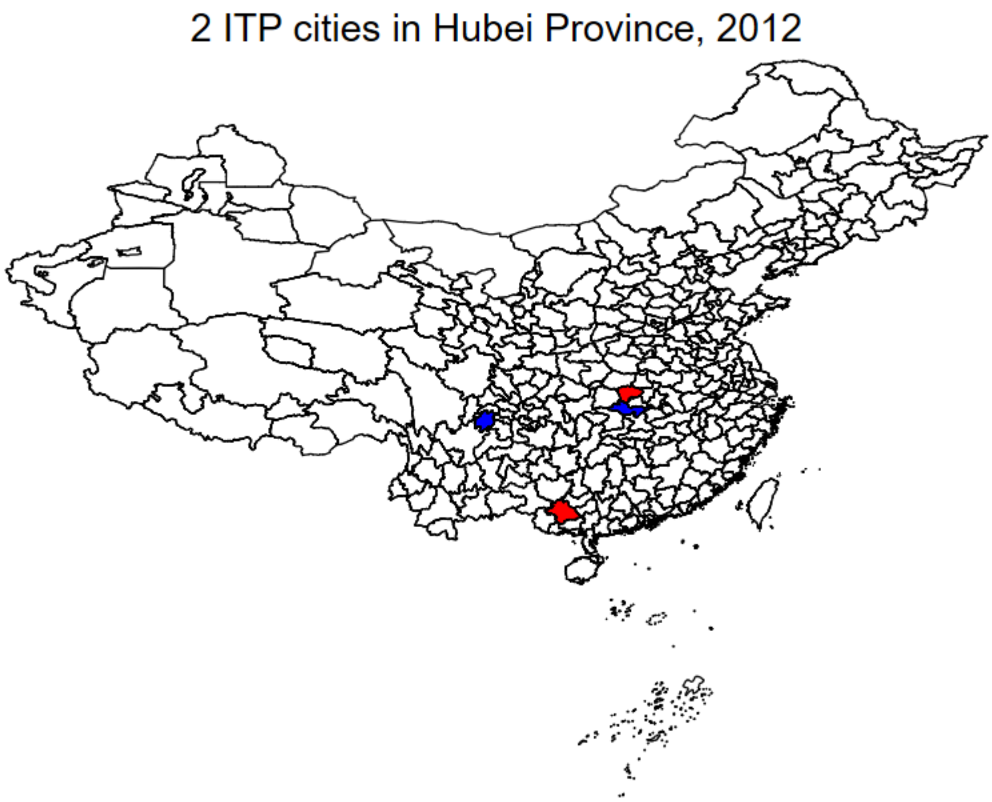


Figure M.6: Jinshanyu Zone

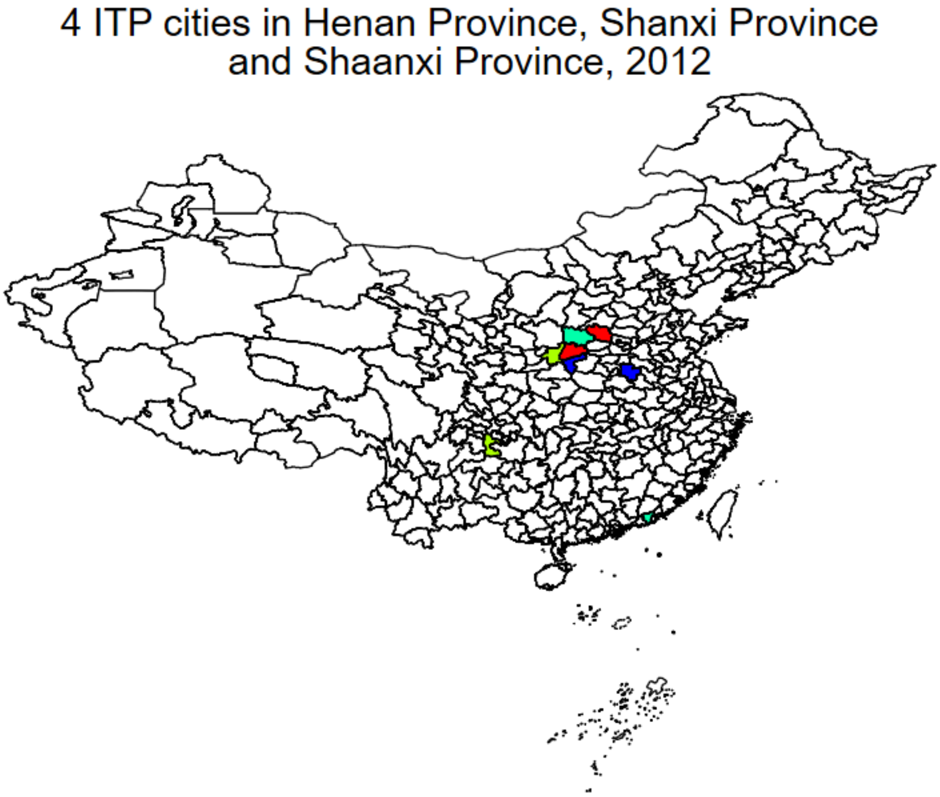


Figure M.7: Gansu Zone

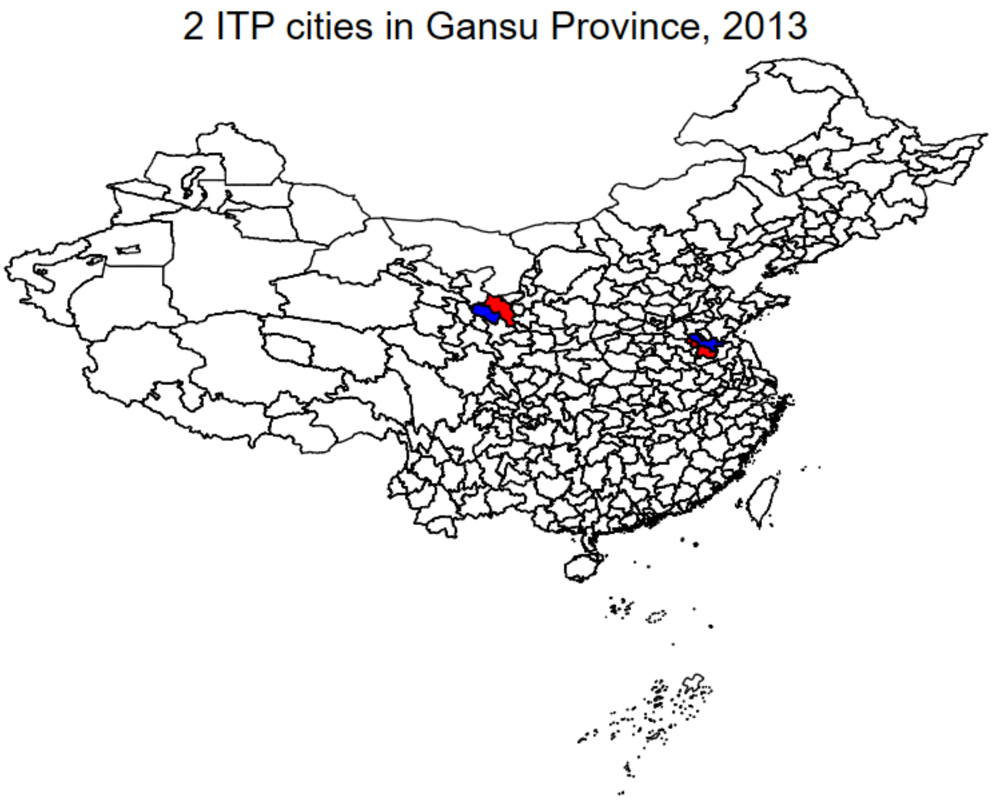


Figure M.8: Sichuan Zone

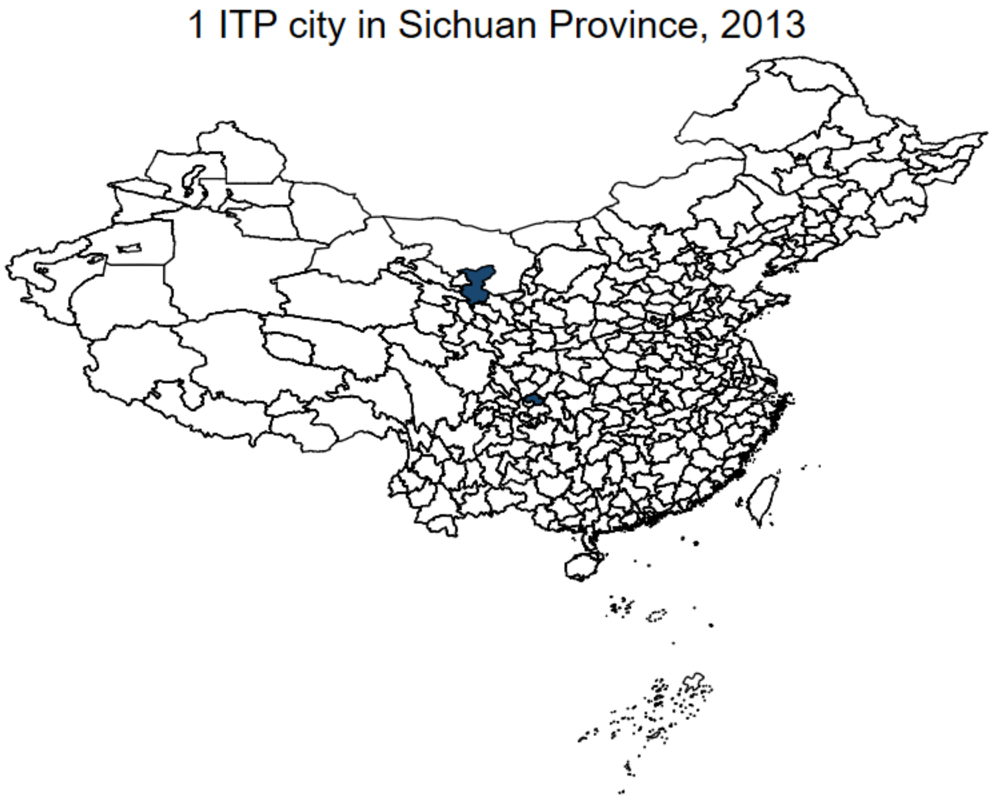


Figure M.9: Gannan Zone

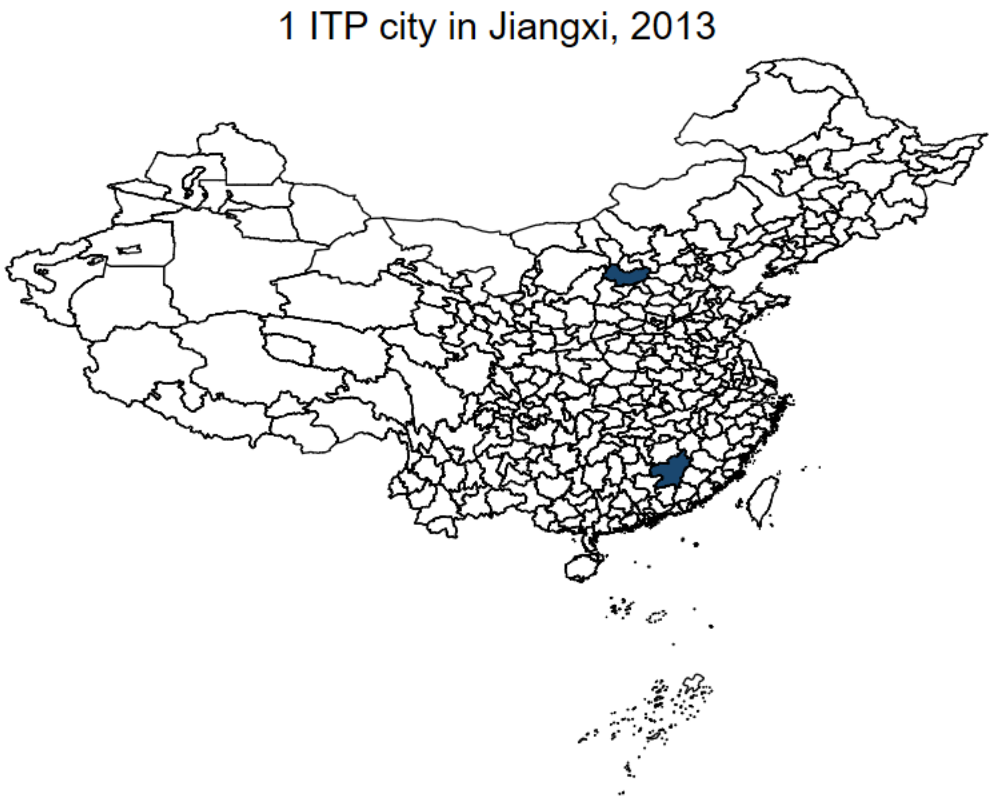


Figure M.10: Ningxia Zone