Industrial Transfer Policy in China: Migration and Development*
PRELIMINARY VERSION

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Abstract

China's Industrial Transfer Policy is a novel place-based development policy of momentous scale. The policy aims to develop inland cities into manufacturing hubs by transferring people and jobs away from China's congested coastal areas. We use a detailed migrant survey to estimate the impacts of the ITP on targeted cities by matching cities in their propensity to receive an ITP status. The ITP status leads to a short-lived growth of migrant inflows up to 60%. The estimate represents 2 to 7 million internal migrations within China. Migrants of coastal origin and working in manufacturing respond more strongly to the policy and we document wage benefits and skill premia for manufacturing sector migrant. However, the inflow of migrants into ITP manufacturing sectors is fully offset by the exit of native workers, leading to no net increases in manufacturing employment and increases in local service sectors. We find no evidence of local upgrading in manufacturing in terms of output, capital use, TFP, startups or pollution.

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, recent generations of place-based policies for national development cover larger areas and vast numbers of people, and aim to steer the urban system, instead of areas within cities.

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In 2010, the Chinese government initiated the Industrial Transfer Policy (ITP). In China's central and western regions, one by one, cities received an "industrial transfer model zone" designation or ITP status. Among other benefits, the ITP status furnishes preferential government treatments, infrastructure investments, and credit. At the same time, coastal cities with manufacturing areas are encouraged to evict undesired industries; and subsidies are available for firms to shut down production lines near the coast, especially in low-grade or polluting manufacturing industries. The aim of the policy is to develop a set of new inland manufacturing hubs that assume the role of coastal cities in production chains. The transfer of manufacturing jobs to inland cities would relieve congestion and free up space for new industries in China's coastal cities.

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 emphasized that "the western regions should take large responsibilities in receiving industrial transfer from the eastern regions", although no formal figures have been published on the budget or on quantifiable targets. The fourfold increase in investment in China's central provinces during the policy years is often interpreted as a result of Industrial Transfer Policy (Ang, 2018). The policy is new in its aims to relocate people and industries away from top performing locations on the coast towards inland regional centers. That movement contrasts most place-based policies, as such policies often target leading areas in the developing world, and they target lagging areas in the developed world (Neumark and Simpson, 2015). This paper is the first, to our knowledge, to formally identify impacts of the Chinese Industrial Transfer Policy.

The case for Industrial Transfer Policy finds mixed support in the economic literature. Place-based policy in China primarily took the form of Special Economic Zones. They cause increases in output, investment and employment (Lu et al., 2019; Alder et al., 2016; Wang, 2013), suggesting that place-based policies can be effective in the Chinese context. However, differently from Special Economic Zones, Industrial Transfer Policy actively encourages migration. China is arguably less urbanized than ideal, as considerable restrictions on migration prevent workers from moving to the largest cities, foregoing agglomeration benefits and allocative efficiency (Zilibotti, 2017; Meng, 2012; Au and Henderson, 2006a,b). The Industrial Transfer Policy intends to relocate workers out of the largest cities into lower-ranked cities, potentially amplifying the loss of scale economies. Moreover, earlier Chinese place-based policies show considerable deadweight losses on the labor market from unintended worker movements (e.g., Koster et al., 2019), which can also occur if migration is encouraged. Finally, there is only emerging evidence that migration can change the sectoral structure or productivity of cities (e.g., Hao et al., 2020). Virtually all evidence for successful policies for local industrialization relies on direct investment in manufacturing plants (in SEZs and larger scale policies such as the Third Front, Fan and Zou, 2021). However, rather than being fueled by local plant investment, ITP is driven by migration policies between specified origins and destinations. Taken together, the economic literature offers no clear-cut recommendation on the relevant impacts of the Industrial Transfer Policy.

This paper examines the impact of Industrial Transfer Policy status assignments on migration choices. Using a representative survey of over 1 million internal migrants in China from 2010 onward, it shows how an ITP status changes the number and type of migrants that arrive in the city, and the consequences of migration for the city's development. The level of detail of the survey allows us to estimate bilateral migrant flows into specific cities and to characterize the composition and origins of the migrants. We connect the survey to several other data sources to examine if and how migrants' choices generate development in industrial structure and production in the destination cities.

To identify the causal impact of Industrial Transfer Policy status assignments, we proceed 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, a city's sectoral structure, wage levels and output, along with the precise industry specialization pattern, are the best predictors for an ITP status assignment. In the second step, 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 detail in the migrant surveys allows for a rich fixed effects structure to rule out confounding explanations for migration, in addition to controlling for shocks that parallel with the matched control cities. We difference out time-invariant explanations of migration at the origin-destination pair such as distance, cost of migration and cultural similarities, thus controlling for the location and geographical access of the destination city. We also difference out origin-year fixed effects to exclude that confounding push factor shocks such as overcrowding, political change or province-level urbanization plans explain our results. In addition, we include destination city fixed effects to identify off 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 of the migrant survey also allows us to investigate the heterogeneity that characterizes place-based policy impacts (Becker et al., 2013), of which the migrants origins and industries of employment are particularly relevant for ITP.

We find considerable increases in migration inflows after cities receive an Industrial Transfer Policy status. The migrant inflow rises by around 60% after a city receives the ITP status, but reverts to pre-policy levels soon after. When embedding our estimates in a model for migration choice, the 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 reveals that many of the migrants originate from coastal areas in our counterfactuals, even though there is no evidence that coastal workers are more sensitive to the policy than others in their migration decision. The size, geography, and large stock of current migrants within coastal areas makes those areas likelier to supply migrants to ITP cities. Migrants from coastal areas earn higher wages in ITP cities, but other migrants and natives experience no wage change, or wage declines. We find circumstantial evidence suggesting that (hukou-related) amenities, such as access to education, may also have motivated the migrants.

Next, we examine the consequences of the ITP status assignment for structural development and industrial upgrading in the receiving city. The evidence is mixed. The share of employment in the manufacturing sector in ITP cities declines. More migrants arrive, but they are less likely than natives to work in manufacturing. Moreover, native workers leave the manufacturing sector to find employment in hospitality and retail. When a city receives the ITP status, new migrants to the city are more likely to be from a rural origin. Migrants from coastal origins specifically are more likely to work in targeted industries such as manufacturing, while we find no such evidence for migrants from other origins, leading the aggregate impacts to be small. We find no impacts of ITP on official GDP or GDP per capita and on (official and satellite-based) pollution and nightlight measures of targeted cities. For firms in targeted cities, we find no changes to size, capital intensity, or productivity for existing firms, while startups are less capital intensive. 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 away from the coast, and 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

This study relates to a growing set of studies on the extensive place-based strategies that shape China's economy. Many of the Chinese government's place-based policies, such as special economic zones and science parks, have improved productivity in the targeted areas and increased human and physical capital investments and output (Wang, 2013; Alder et al., 2016; Koster et al., 2019; Lu et al., 2019). The policies frequently target highly and improve highly performing areas but may generate dead weight losses. Zheng et al. (2017) show that industrial parks can (inadvertently) create production and consumption subcenters, thereby generating edge cities. Koster et al. (2019) show that science parks come with substantial relocations of workers, potentially causing sizable dead-weight losses from such policies.

Our study differs from the literature on place-based policies in two main ways. First, it adds to the evidence of the effectiveness of place-based policies in emerging economies. The results from a larger literature on place-based policies in advanced economies do not generally translate to developing countries (Neumark and Simpson, 2015). In emerging economies, place-based policies frequently target leading areas instead of lagging areas (as place-based policies in the Europe and the U.S. frequently do). The targeted areas in China often have different stages of industrial development, financial needs and institutional settings than the areas studied in related literature. Second, ITP differs considerably from conventional place-based policies such as science parks or special economic zones, applied in China and many other emerging economies. ITP has a larger geographical coverage and affects more people. Importantly, its goal differs: it aims to transfer industry and people between locations, rather than fostering them in one targeted location. As a consequence, migration and labor mobility cannot be viewed as unintended dead weight losses. In terms of scope, ITP is more in line with the Great Western Development Programme (GWD) that started in 2000, which covers over a quarter of the Chinese population. GWD investment in Western provinces has increased output and industrialization, but not productivity and employment (Jia et al., 2020). Our findings differ in that ITP-induced migrants do find jobs and that upgrading and capital intensification are limited, but the findings are similar in reporting no productivity effects. Differences in policy instrument use might explain why we find jobs instead of capital and output growth: where GWD was driven by capital investment, ITP focuses on the transfer of activity between regions.

A related literature questions whether migration can bring about development and industrial upgrading. Migrants may 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); but the evidence of such upgrading is mixed, and scant for China (Dustmann et al., 2016). In China, internal migrants make large contributions to local productivity compared to locals, show little substitution for local workers (Combes et al., 2015, 2019) and drive high-skilled worker clustering (Fu and Gabriel, 2012). Internal migration is arguably the principal driver of 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 explanation is that we focus on migration specifically encouraged by a policy, whereas virtually all other literature describes migration in spite of China's sizable migration restrictions. Evidence on how spatial policies encourage migration in China and their impacts on wages is scarce in general, in particular relative to the evidence available on Hukou restrictions. Correspondingly, most literature on China's internal migration argues that rural migrant workers fuel the expansion of large-city manufacturing sectors (Hao et al., 2020; Garriga et al., 2017). Here, we examine the opposite movement: workers from cities with more advanced manufacturing sectors move into smaller cities with less advanced manufacturing sectors. The impact of migrants on development differs between these two migration movements. The presumption of ITP is that migrants bring manufacturing knowledge or experience to targeted cities; not that they enter an urban manufacturing sector from a rural background with little manufacturing experience. A second difference is that we leverage a comparatively detailed source of individual migration data that permits controlling for an extensive set of explanations of migration, including, for instance, cultural and institutional differences between the origin and destination, and unemployment rates or other economic shocks at the origin of the migrant that encourage outmigration. Hence, direct policy incentives explain our empirical findings, rather than the push-factors of migrants.

As the Industrial Transfer Policy relocates millions of people, our results also connect to the question whether the Chinese urban system is optimally organized. The allocation of the 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 (Lucas, 1988; Hsieh and Moretti, 2019; Rossi-Hansberg and Wright, 2007; Henderson, 2005). In China, the productivity effects of city size outpace those of the Western world (Chauvin et al., 2017). With approximately 244 million[1] internal migrants, in China, 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 and may have reduced aggregate output considerably (Zilibotti, 2017; Meng, 2012; Au and Henderson, 2006a,b). 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.

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 surging manufacturing output, employment and productivity. 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

[1] This value is taken from official statistics in 2017.

"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 region 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."[2]

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.[3]

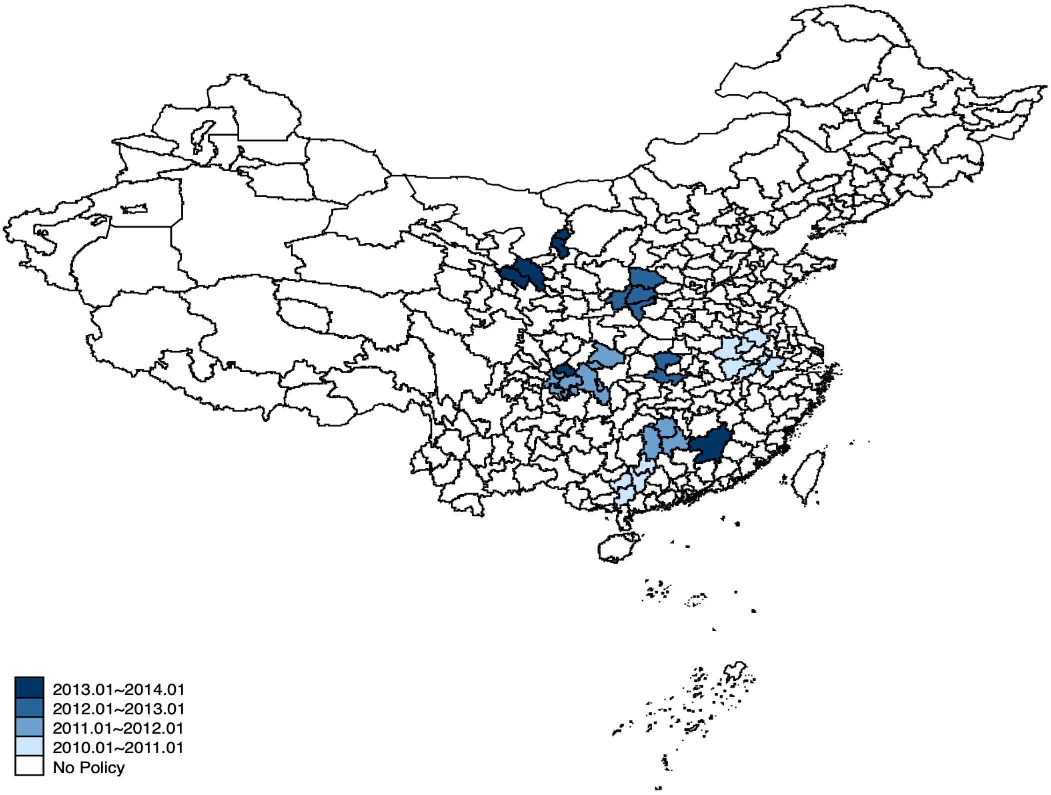
The second goal is to promote industrialization and local development in the inland cities. In coastal areas, the demand for manufacturing goods has rapidly pushed up wages and production costs. Increasing the manufacturing capacity in the inland cities would preserve China's exporter status in such 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 32 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

[2]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.

[3]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.

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



Notes.

manufacturing base. Our analysis, in which we predict the assignment of ITP status in an auxiliary regression, found that manufacturing (or secondary sector) activity predicts ITP status assignment.

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 onwards. Although these items are centrally financed, their exact costs and their precise impacts on subsidies, wages, or credit provision are not publicly available. 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 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 ITP status assignment impact. 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 that tend to migrate to the city: the status might come in times of regional boom or downturn, for example. 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}). The equation is as follows:

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

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 is that the policy is not targeted randomly. 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 $E(ITP_{dt}) = \gamma l_{dt} + v_{dt}$, 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:

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

Here, the first term is the measure of policy impact, and we need the assumption $cov(ITP_{dt}, \gamma l_{dt}) = 0$, to interpret β as a treatment effect. 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 rule out that the ITP's targeting to specific cities explains our estimate of the policy's impact, we first match every city that receives a policy to another city that has a similar propensity to receive the ITP status. This is similar to Schweiger et al. (2022), but as we are dealing with city-level treatment in a dyadic dataset, we proceed in two steps. 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 variables x_d used for treatment assignment are discussed in the next section. We obtain the predicted treatments, $IP\hat{T}_d$.[4] 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, $cov(ITP_{dt}, \gamma l_{dt})$ 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 2). As a second step, we run the regression of interest on the subsample for which the treatment bins are defined, adding a yearly treatment-bin fixed effect:

$$\log M_{odt} = \beta ITP_{dt} + \alpha_{mt} + \alpha_{od} + \alpha_{ot} + u_{odt}. \quad (3)$$

In this equation, we omit the pre-announcement year as the reference category, such that β measures the additional expected migrants associated with ITP (and any leads or lags) relative to the year before announcement and conditional bin-year, origin-year and pair fixed effects.

The above equation is estimated as a pseudo-Poisson model. In the bilateral setting, migration flows of zero occur, and taking logs would lead to a nonrandom loss of observations. Hence, we estimate the equation by maximum likelihood as a Poisson model (Santos Silva and Tenreyro, 2006). As our

[4]Schweiger et al. (2022) argue against matching on propensity as it causes larger geographical distances between matched cities. However, the larger geographical distance is less of an issue in our institutional setting, and may be more effectively controlled for with our fixed effects strategy dealing with migrant origin shocks. To check the sensitivity, we re-estimate our main results with different matching strategies and check stability with respect to within-province or between-province matching.

variable of interest is at the destination level (with multiple origin flows ending in the same destination), $\log M_{odt}$ and ITP_{dt} are not independent across observations. We adjust our standard errors for this level difference by clustering at the destination city level.

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. From the surveys, we construct the year-by-year migration flows. We extract the surveys of workers who at the time of the survey had migrated within the last three years. 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.[5]

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.[6]

We supplement the migration data with information from the census and city yearbook. The descriptive statistics for the variables of interest are reported in Appendix A.

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.

[5]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 percentage of lowest flows between 1% and 15%, we find very minor changes in our results (at most around 6% of the coefficient magnitude).

[6]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 B.2 in the Appendix shows the sensitivity of our main results to lengthening or shortening the horizon for inclusion.

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 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 detailed sectoral (8) employment shares.[7] We use values from the city yearbook from before ITP to predict later ITP status assignment. 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 signficantly 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 the 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 explores the contribution of individual covariates by considering how dropping them leads to changes in explanatory power, the eventual set of matched 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 dropped from the equation. 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. 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

[7]There is some evidence for other policies 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 links with the institutes predict ITP status assignments.

of migrant flows, and somewhat lower distances to the coast (though well within a standard deviation across the groups).

Table 1: Descriptives

	(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	

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.

To assess the matching of ITP cities more formally, Table 2 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.

Table 2: Treatment propensity explained from actual treatment in different samples

Treatment propensity explained from actual treatment in different samples

VARIABLES	(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

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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

To ensure that our results do not arise with the choice of a matching method, we explore alternative matching strategies in Appendix C. In particular, to circumvent instances in which the cities' propensity 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.[8]

[8] 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.

5 Results on migration and urbanization

5.1 Impact on migration flows

Table 3: The impact of ITP on (log) migration with and without matching and fixed effects

The impact of ITP assignment on migration with matching and fixed effects.

VARIABLES	(1) Main	(2) Leads	(3) Static	(4) GTATE	(5)	(6)	(7) Stock	(8) Stock
ITP destination (t+2)		0.12						
		(0.16)						
ITP destination (t)	0.13	0.13		0.22	0.21	0.16	0.10	
	(0.17)	(0.17)		(0.19)	(0.19)	(0.18)	(0.13)	
ITP destination (t-1)	0.48**	0.47**		0.44**	0.47*	0.40*	0.31**	
	(0.22)	(0.22)		(0.17)	(0.25)	(0.24)	(0.14)	
ITP destination (t-2)	0.17	0.17		0.18	0.25	0.14	0.31**	
	(0.21)	(0.21)		(0.16)	(0.26)	(0.24)	(0.14)	
ITP static			0.28**					0.16*
			(0.13)					(0.09)
Observations	3,624	4,097	4,120	4,120	3,624	3,624	1,926	2,042
Matched bin year FE	yes	yes	yes	yes	yes	yes	yes	yes
Origin year FE	yes	yes	yes	yes	no	yes	yes	yes
Origin Destination FE	yes	yes	yes	yes	yes	no	no	no
Destination FE	no	no	no	no	no	yes	yes	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. GTATE refers to a group-time average treatment 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 city level. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

Table 3 presents the estimates of the impact of ITP on migration. 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. In Appendix D, we confirm the absence of pre-policy differences when allowing for additional treatment lags. We also confirm that the impact on migration flows turns insignificant after the first year when looking up to 5 years (though that leads coefficients to be identified from different groups of cities).

Column 3 uses a single treatment indicator to estimate the static impact, of permanent treatment. Its estimate is significantly below the peak of impact in the dynamic estimates from column 2. Column 4 shows estimates for group-time treatment effect. Direct outcomes comparisons are restricted within match pairs such that we do not identify off forbidden comparisons in the sense of **?**. However, the origin-year fixed effects in the bilateral migration model can still induce dependence between observations. We estimate the coefficient in subgroups of treatment cohorts. The results suggest little change to the coefficients.

Column 5 and 6 alter the fixed effect strategies applied. 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 estimates slightly less precisely. Columns 7 and 8 consider the impact of ITP on migrant stocks, instead of migrant year-by-year 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. That is not surprising as migrants after ITP announcements show similar spans of stay (see 5.1.1). The static stock impact estimate amounts to around 17% elevation. The estimate is broadly consistent with column 3, as Chinese migrants in our sample have relatively short spans of stay.

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5.1.1 Migrant spans of stay

The result in Table 3 suggest that the rise in migrant inflow is short-lived. The effect 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 D, 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 3 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 their status assignments in zones, which could justify adjusting standard errors for the zone-level assignment. Clustering standard errors at the zone level leads to no changes on the conclusions on significance in any of the coefficient.

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.[9] 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 E. 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 is below 0.05 in the randomized distribution. Altogether, the differences between city-clustered standard errors and bootstrapped or randomized standard errors do

[9]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.

not lead to different conclusions.[10]

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 control cities may receive fewer mirgrants. In section 5.3.2, we cast our estimates in a general equilibrium framework to quantify such diversions. We find control cities on average lose around 2% migrant inflows. This suggests that the general equilibrium effect is not very large relative to estimated impact (i.e., less than 5% of the estimated impact).

In addition, we examine whether migrants in ITP cities and their control cities have very similar origins. A high similarity in migrant origins could mean that the ITP city diverts more migrants from the control city. For every city, we calculate the shares of migrant origins in the year 2009. Then for every possible city pair, we calculate the linear as well as Euclidean distance between the migrant origin share patterns. We find no evidence that matched city pairs have higher similarity in migrant origin shares than other city pairs.

5.1.4 Alternative matching strategies

The impact of the policy are comparable across different methods of matching ITP cities with non-ITP cities. Appendix C 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. To matched 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 controls cities to the set that is outside its province. A comparison between more distant cities prevents any negative localized spillovers to bias our estimate upward. By contrast, the coefficient estimates are somewhat larger when matching is only permitted across provincial borders, though within the confidence interval.

5.1.5 Robustness to the institutional context

Some cities that received the ITP status are under distinct governance or organization, or might be subject to coincident policies. In Table F.1 in Appendix F, we explore whether our results are explained by excluding such cities from the sample. 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. Excluding any of these observations does

[10] 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.

not change our results. Second, we exclude cities that saw potentially confounding policies in columns 4 and 5 in Table F.1. Reforms to the hukou system can confound with migration changes. Most of the local changes to the hukou system from the 1990s were gradual nudges loosening the restrictions, but 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). As this policy shift occurred in the timeframe of the ITP status assignments, we confirm that an analysis of the pre-2014 sample produces similar coefficients. Another substantial national policy that also has the goal of industrialization in targeted regions is the Great Western Development Program (Jia et al., 2020). To ensure that our estimates are not driven by such confounding policies, we also report estimates from a sample without central provinces, and this does not meaningfully alter the coefficient estimates.

5.2 Which migrants respond to ITP?

The ITP policy plausibly target specific migrant groups. In this section, we examine the impacts of ITP policies on targeted subsets of migrants by identifying the changes in the flow of migrant types mentioned in the policy documents and their wages developments.

Table 4 shows coefficients of migrants shares change following the ITP policy. The estimating equation is

$$\log share_{odt} = \beta ITP_{dt} + \alpha_{mt} + \alpha_{od} + \alpha_{ot} + u_{odt}, \tag{5}$$

where β measures share changes following the ITP policy, using the same identification as before. However, the dependent variable is the share of migrants in the migration flow that satisfies a criterion, such as working in the manufacturing sector.

Table 4, column 1 shows a significant increase of 2 percentage points in the share of migrants that are employed in the manufacturing sector. This represents a substantial shift, as in ITP cities, 5-6% of migrants work in manufacturing. No significant differences compared to the match cities relative to the pre-policy year can be detected after the year of announctment. Column 2 shows that there is a significant increase of migrants with a coastal originof 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 percentage point increase of 2% 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: a coefficient 0.2 represents an over 50% increase 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 over 50% increase. Skilled migrants, however, show no significant change in the share of overall migrants.

Table uses the same identification (see eq. 5) to identify the impacts of ITP policies on the log wages of migrant groups. In line with the composition changes (Table 4, column 1), ITP cities see a 2% wage rises among migrants. Wages among migrants in the manufacturing sector rises faster, peaking at 6% in the year after (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 look more pronounced. They are broken down by group in column 6 to 8. Coastal origin migrants in the manufacturing sector see sharper rises

Table 4: Migrant flow composition

VARIABLES	(1) Manuf	(2) Coast	(3) Rural	(4) Skill	(5) Manuf Coast	(6) Manuf Rural	(7) Manuf Skill	(8) 40+
ITP destination (t)	0.02**	0.01	0.05***	-0.02	0.02***	0.02**	0.01	0.03**
	(0.01)	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
ITP destination (t-1)	0.01	0.03**	0.05**	-0.01	0.01**	0.00	-0.00	0.02
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.00)	(0.01)
ITP destination (t-2)	-0.02	-0.01	0.01	-0.02	-0.01*	-0.01	-0.00	0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	6,386	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	0.45
Origin year FE	yes	yes	yes	yes	yes	yes	yes	yes
Origin Destination FE	yes	yes	yes	yes	yes	yes	yes	yes
matched bin year FE	yes	yes	yes	yes	yes	yes	yes	yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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) and is older than 40 (40+). "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: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

than general coastal origin migrants and than other migrants in the manufacturing sector. However, drilling down into the subgroups implies a loss of observations. Within the manufacturing sector, rural migrants see similar wage increases as migrants with other Hukous (compare columns 7 and 2). Substantial wage increases around 7% concentrate with high-skilled migrants in the manufacturing sector (column 8). That is striking as i) other skilled migrants saw no such wage increase (column 5) and the results of Table 4, columns 4 and 7 show limited changes in migration response for skilled migrants.

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., 2020).

main writeup -highe runemployment -higher selfemployment and service employment. Largely accounted by selfemployed services. -wages up in both -children brought: hukou -no evidence of housing support

Table 5: Wage development for different migrants

VARIABLES	(1) Overall	(2) Manuf	(3) Coast	(4) Rural	(5) Skill	(6) Manuf Coast	(7) Manuf Rural	(8) Manuf Skill	(9) 40+
ITP (t)	0.02**	0.03***	0.06***	0.02***	-0.00**	0.05	0.04***	0.06***	0.06***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.05)	(0.01)	(0.00)	(0.01)
ITP (t-1)	0.02	0.06***	0.04***	0.02	-0.00***	0.09**	0.06***	0.08***	0.07**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.04)	(0.01)	(0.00)	(0.03)
ITP (t-2)	0.01	0.04***	0.06***	0.01	-0.00**		0.04***	0.08***	0.04
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)		(0.01)	(0.01)	(0.03)
Observations	3,162	3,162	3,162	3,162	3,131	2,201	3,069	2,635	2,573
Origin year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Origin Destination FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Match bin year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Sample								Matched	

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

Notes. 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) and older than 40 (40+). "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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 6: Non wage benefits from migration associated with ITP

VARIABLES	(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

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

VARIABLES	(1) Wage Service	(2) Wage Self empl
ITP (t)	0.07*	0.02
	(0.04)	(0.01)
ITP (t-1)	0.05	0.03*
	(0.04)	(0.02)
ITP (t-2)	0.04	0.02
	(0.04)	(0.02)
Observations	3,162	3,162
Sample	Matched	Matched
Origin year FE	yes	yes
Origin Destination FE	yes	yes
Match bin year FE	yes	yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes. ”Unempl.” denotes the share of unemployed people. ”Selfemp” denotes the share of self-employed migrant workers. ”Children” denotes the share of migrants that cohabit with children. ”Housing subsidy” denotes the share of migrants who live in government-subsidized housing. ”Company Accommodation” 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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

5.3 Host city development

The second stated objective of the Industrial Transfer Policy is to advance the targeted cities in terms of industrial development. We assess three related outcomes of upgrading. First, combining migrant surveys and the City Statistical Yearbook, we quantify the changes in sectoral employment in targeted cities and decompose the migrant contribution. Second, we examine changes to type of migrants that enter targeted cities and changes in city-level outcomes. Third, we analyse the impact of ITP statuses on direct and indirect measures of city-level output and firm-level production outcomes.

5.3.1 Sectoral changes

A stated objective of the ITP policies is to upgrade the industrial structure of the designated cities – specifically to expand the local manufacturing sector. The expansion of the manufacturing sectors can occur along different margins: through changed employment patterns among migrant 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:

$$e_{manufacturing} = e_{manf}^{migrant} * s^{migrant} + e_{manf}^{native} * s^{native}, \tag{6}$$

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:

$$\frac{ds_s}{dITP} = \underbrace{s^{migrant}\frac{de_{manf}^{migrant}}{dITP}}_{\text{change in migrant specialization}} + \underbrace{s^{native}\frac{de_{manf}^{native}}{dITP}}_{\text{change in native specialization}} + \underbrace{\frac{ds^{migrant}}{dITP}\left(e_{manf}^{migrant} - e_{manf}^{native}\right)}_{\text{composition change}} \tag{7}$$

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 change, which increases the manufacturing employment share if migrants tend to have higher employment shares in manufacturing.

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

$$\log e_{dt} = \beta ITP_{dt} + \alpha_{mt} + \alpha_d + u_{dt}, \tag{8}$$

where e_{dt} is the share of natives or migrants emnployed in a given sector. 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 Table J.1 in the Appendix J. We employ our baseline estimate for the 1-year lag of the ITP status assignment on the migration flow to recover $\frac{ds^{migrant}}{dITP}$, using 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, $e_{manf}^{migrant} - e_{manf}^{native}$. 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 7 shows the percentage point estimated changes in sectoral employment shares in a city in the year after it receives an ITP status with 95% confidence intervals. The changes are decomposed into the direct impact of receiving more migrants, changes to the composition of incoming migrants, and changes to the employment 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 insignificant. The migrant specialization is signifint and positive in the year of announcement, accounting for over 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, 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.[11] 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.

5.3.2 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:

$$U_{iodt} = b_{iod} a_{dt} c_{idt}^{1-\alpha} h_{idt}^{\alpha t} \tag{9}$$

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

[11] 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.)

Table 7: City-level sectoral change

	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
transict	-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

	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
transict	-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

	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
transict	-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.Transport includes ICT infrastructure. Hospitality includes hotels, restaurants and cafes. 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 1-year lagged 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.

origins may derive different utility from living in the same location and consuming the same quantities. The preference shock could include different aspects, 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:

$$V_{iodt} = \zeta \frac{b_{iod} a_{dt} w_{idt}}{r_{idt}^{\alpha}}, \tag{10}$$

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:

$$\pi_{odt} = \frac{B_{od}\left(a_{dt}\frac{w_{dt}}{r_{dt}^{\alpha}}\right)^{\varepsilon}}{\sum_d B_{od}\left(a_{dt}\frac{w_{dt}}{r_{dt}^{\alpha}}\right)^{\varepsilon}} = \frac{x_{odt}}{x_{ot}}. \tag{11}$$

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:

$$\log M_{odt} = \log B_{od} + \varepsilon\left(\log a_{dt} + \log w_{dt} - \alpha \log r_{dt}\right) - \log x_{ot} \tag{12}$$

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.

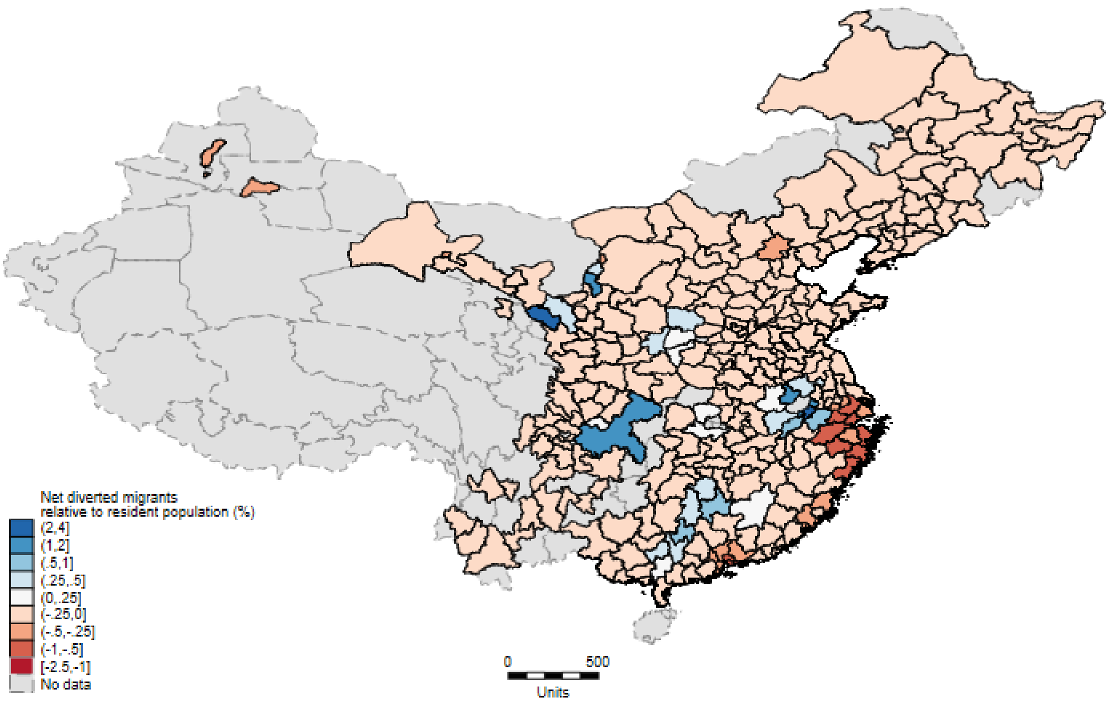
Migration choices show two margins of adjustment: the diversion of existing migrants and the generation of new migrants. By their definition, the migrant surveys only enable us to estimate the impact of the ITP from the choices of existing migrants. For that reason, we first develop a counterfactual 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 H.

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:

$$\text{inflow}_d = \sum_o \left(\pi_{od}^{cf} - \pi_{od} \right) Pop_o. \tag{13}$$

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.

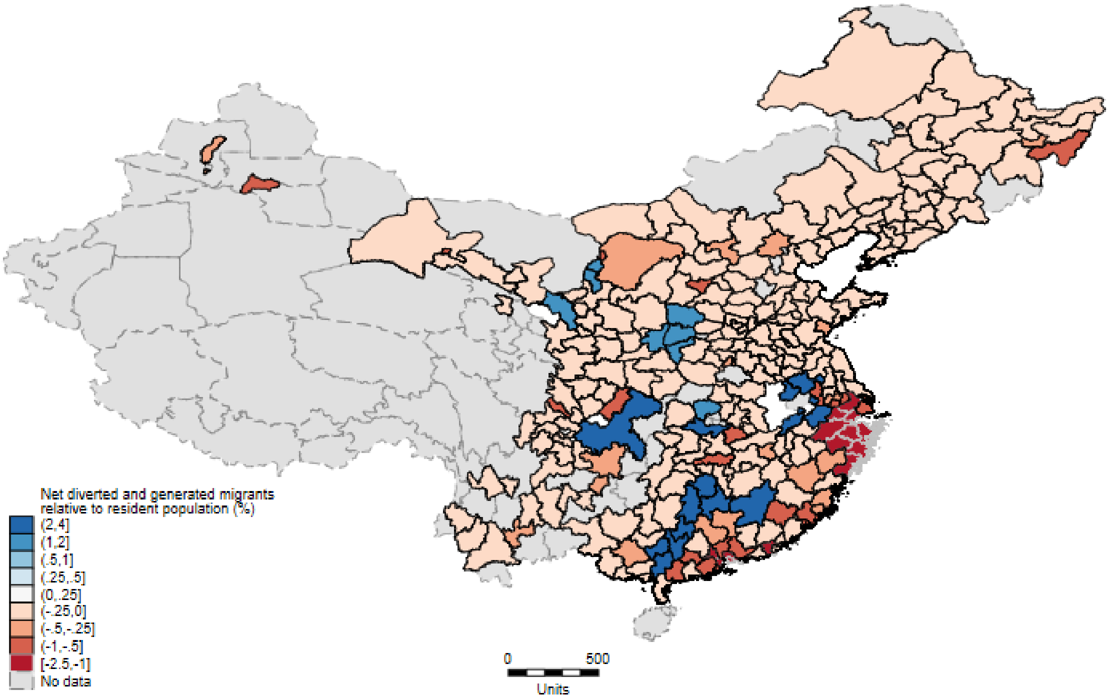
Figure 2 shows the number of additional diverted migrants resulting from the ITP status assignments, as a percentage of the resident population. The cities that receive an ITP status are the beneficiaries in terms of population change: the number of additional migrants is approximately 0.7% of the resident 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 just over 1% 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 H. 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 3% 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.

5.3.3 Output and firm production

To examine the aggregate development of cities that are assigned ITP statuses, we track the development of measure of output. 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:

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

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

Table 8 shows regressions that explain the log of the GDP level or the log of GDP per capita 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. To examine the consequence of the matching strategy, the regressions in Columns 3 and 6 show the results a the full-sample regression without matching. Unconditional on matching, the ITP status is negatively associated with both the GDP and the GDP per capita. That is consistent with ITP targeting cities that are less developed.

We use nightlight intensity to verify our results outside Chinese government statistics. 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 in Appendix K. 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.

Table K.2 in Appendix K shows regressions that examine the developments after the ITP announcement, of 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.[12]

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.[13] Firms may adjust their hiring and substitute capital for labor. Additionally, ITP policies may have incentivized firms to adopt different production methods.

We employ the survey of manufacturing firms in the NBS Annual Survey of Industrial Firms be-

[12] 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.

[13] In regression that explain native workers' wages following the strategy of eq. 14 reported in Table I.3, we find no impacts of ITP.

Table 8: Impact of ITP status on city log GDP or log GDP per capita

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

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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

tween 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 Conclusions NEEDS UPDATING

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.

The successes in terms of upgrading the targeted cities are more mixed. On the one hand, we find that migrants from coastal origins end up in targeted industries and earn higher wages. However, the transformation of the cities is limited. We find only minor changes in the overall employment shares in targeted industries, and no evidence in changes of output, proxies of economic activity, pollution, or changes in firm operations. Natives do not earn higher wages and do not move into the targeted industries. 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. As we find no evidence that ITP policies also structurally upgrade second rank cities, the long-term growth and structural change prospects of such policies seem limited. An analysis of the welfare impacts of the ITP is a risky endeavor for two reasons. First, standard welfare interpretations of single-location place-based policies consider the relocation of firms or people as a chief welfare loss. However, in the Industrial Transfer Policy, relocation is precisely the aim, so welfare statements that interpret mobility as a dead weight loss may be inaccurate. Second, an informed welfare analysis would ideally rely on a cost-benefit trade-off, but a precise view of the exact budgets and instruments involved in ITP is lacking.

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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. The demographic characteristics of the data sources overlap fairly precisely. 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. To check any sensitivity to this issue, we also check the stability of our results between using weighted and unweighted migrant number estimates (noting that in our preferred specifications, we express policy impacts in relative change). Table A.1 presents key demographic characteristics of migrants who are CMDS respondents and those who are census respondents.

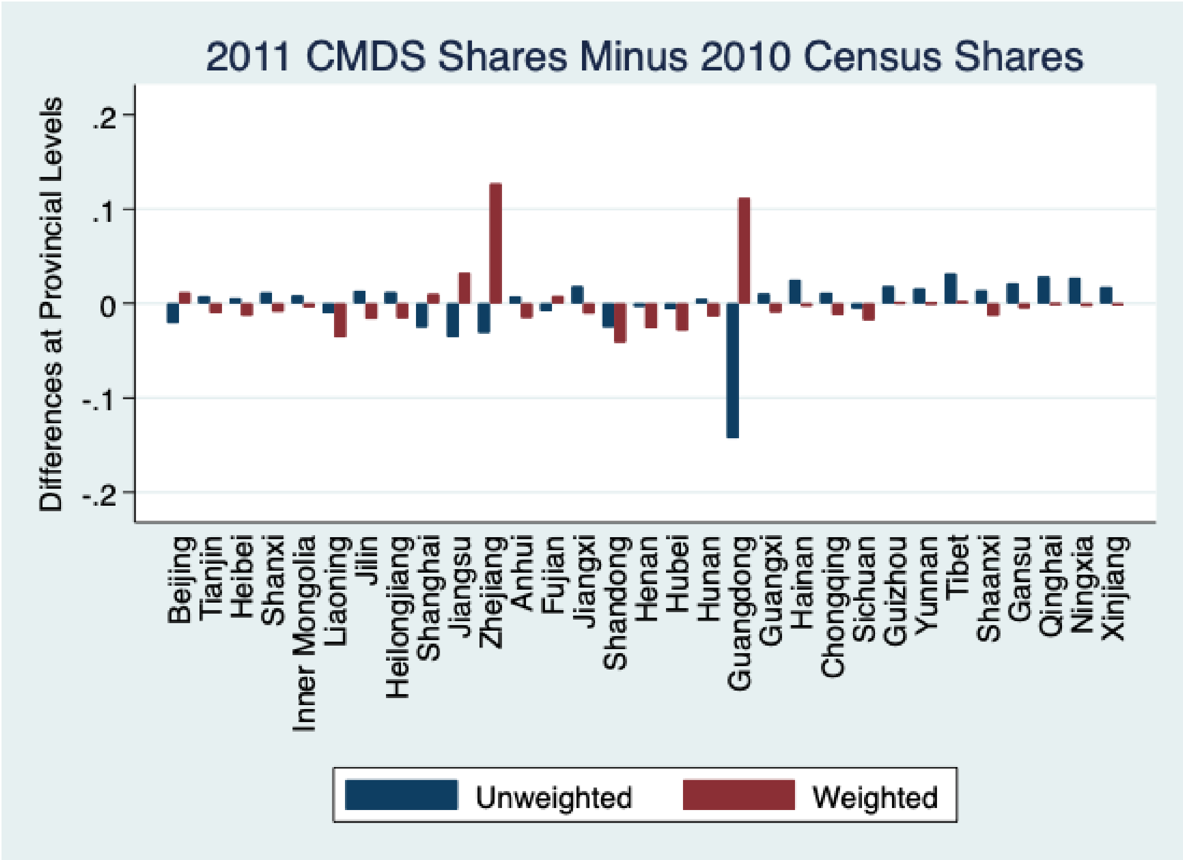
Table A.1: CMDS as a Nationally Representative Sample

	(1) 2011 CMDS	(2) 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.

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.

Figure A.1: Comparison of sampling



Notes. Differences between the CMDS and the Census in Shares of Migrants in 31 Provinces

B Matching on the propensity of receiving an ITP status

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

Logit treatment equation	
	(1)
VARIABLES	odds ratio
log GDP/cap	0.00*
	(0.00)
log wage	5.93e+205**
	(1.40e+208)
Secondary industry as percentage to GDP (%)	0.58
	(1.91)
c.lgdpcap#c.lgdpcap	0.00**
	(0.00)
c.lgdpcap#c.lwage	8.09e+11**
	(1.05e+13)
c.lgdpcap#c.gdpsharesec	1.41*
	(0.28)
c.lwage#c.lwage	0.00**
	(0.00)
c.lwage#c.gdpsharesec	0.83
	(0.36)
c.gdpsharesec#c.gdpsharesec	0.99
	(0.00)
log employment	0.60
	(0.26)
Tertiary industry as percentage to GDP (%)	1.13
	(0.09)
Distance to coast	1.00
	(0.00)
Distance to coast squared	1.00
	(0.00)
Observations	280

seEform in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes. Results of a 2009 cross-sectional logit estimation of the city ever receiving an ITP status. Interacted set of the core policy variables. "Second. ind. share" is short for Secondary industry as a percentage of GDP. The table reports odds ratios. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

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

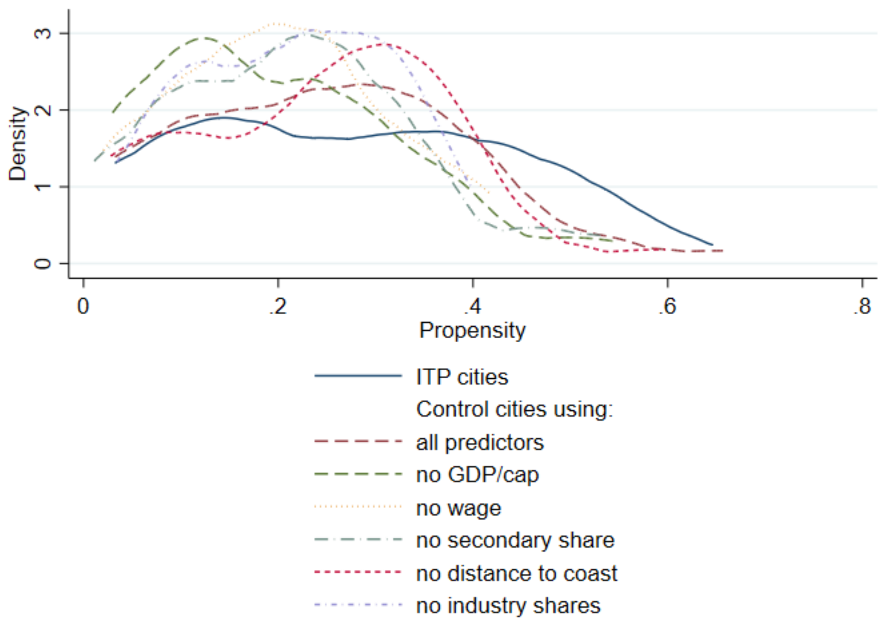


Table B.2 repeats the baseline regression with different definitions for the migrants that are included in the count. In the baseline, the migrants observed up to three years after migrating are included. Column 1 reports the results when using a two-year period of inclusion, showing slightly more pronounced effects of these policies. The results based on a four-year horizon are comparable to those of the three-year horizon.

Table B.2: 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)
VARIABLES	recent 2 yrs	recent 2 yrs.	recent 4 yrs.	recent 4 yrs.
ITP destination (t+2)		0.0120		0.201
		(0.214)		(0.178)
ITP destination (t)	0.300*	0.217	0.108	0.0864
	(0.156)	(0.202)	(0.112)	(0.134)
ITP destination (t-1)	0.462***	0.533**	0.431***	0.398**
	(0.161)	(0.263)	(0.119)	(0.191)
ITP destination (t-2)	0.195	0.304	0.186	0.241
	(0.161)	(0.249)	(0.120)	(0.173)
Observations	3,134	3,185	3,799	3,833
Match bin year FE	yes	yes	yes	yes
Origin Destination FE	yes	yes	yes	yes
Origin year FE	yes	yes	yes	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. 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 Different matching strategies

This Appendix explores alternative strategies to match ITP cities to cities with a similar likelihood of receiving the ITP status. Table C.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 C.1: Impact of ITP on migration stocks with difference reference groups

Impact of IPT on migration stocks with difference reference groups				
	(1)	(2)	(3)	(4)
VARIABLES	closest match	Conservative	CEM	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

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. 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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

D Dynamic structure of the policy

D.1 Impact dynamics

The results in Table 3 suggest a short-lived impact of ITP statuses on migration flows. In Table D.1, 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 D.1: Impact of ITP on log migration flow with different lags

The impact of ITP assignment on migration with matching and fixed effects.

VARIABLES	(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

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

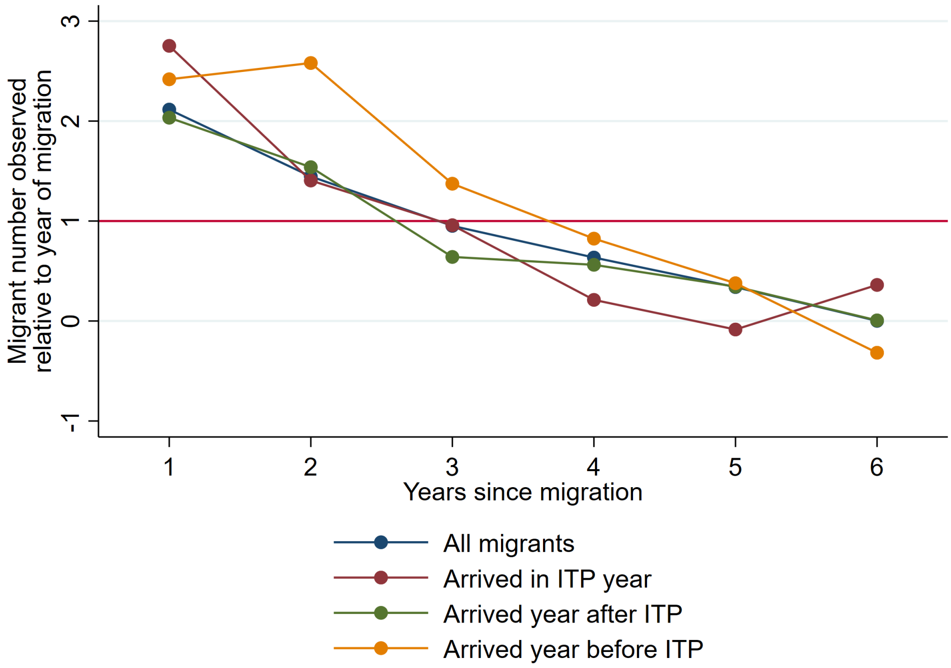
D.2 Migrant attrition

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 D.1 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.

Figure D.1: Development of number of migrants observed by year after arrival for different cohorts



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:

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

In this regression, $D\left(t = 1,2,3,4,5\right)$ 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 $D\left(ITP_{d\tau}\right)$ 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 D.2 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.

Table D.2: Cohort size development: Migrants observed relative to year of migration

	Migrants observed relative to year of migration)			
	(1)	(2)	(3)	(4)
VARIABLES	in ITP year	in ITP year	at most 2 years after ITP	anytime after ITP
yearsincemigration = 1	2.44***	2.49***	2.56***	2.54***
	(0.23)	(0.23)	(0.25)	(0.23)
yearsincemigration = 2	1.53***	1.58***	1.65***	1.61***
	(0.22)	(0.22)	(0.24)	(0.25)
yearsincemigration = 3	0.98***	1.01***	1.12***	1.08***
	(0.25)	(0.26)	(0.29)	(0.29)
yearsincemigration = 4	0.77***	0.78**	0.89**	0.89**
	(0.29)	(0.32)	(0.37)	(0.35)
yearsincemigration = 5	0.45*	0.46*	0.56*	0.49
	(0.25)	(0.27)	(0.30)	(0.31)
1.ITPaffected#1.yearsincemigration		-0.92	-0.86	-0.94
		(1.30)	(0.61)	(1.49)
1.ITPaffected#2.yearsincemigration		-0.87	-0.63	0.00
		(1.07)	(0.49)	(1.42)
1.ITPaffected#3.yearsincemigration		-0.24	-0.57	-0.17
		(0.94)	(0.49)	(1.13)
1.ITPaffected#4.yearsincemigration		0.12	-0.30	-0.31
		(0.48)	(0.42)	(1.09)
1.ITPaffected#5.yearsincemigration		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

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

E Randomization inference

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

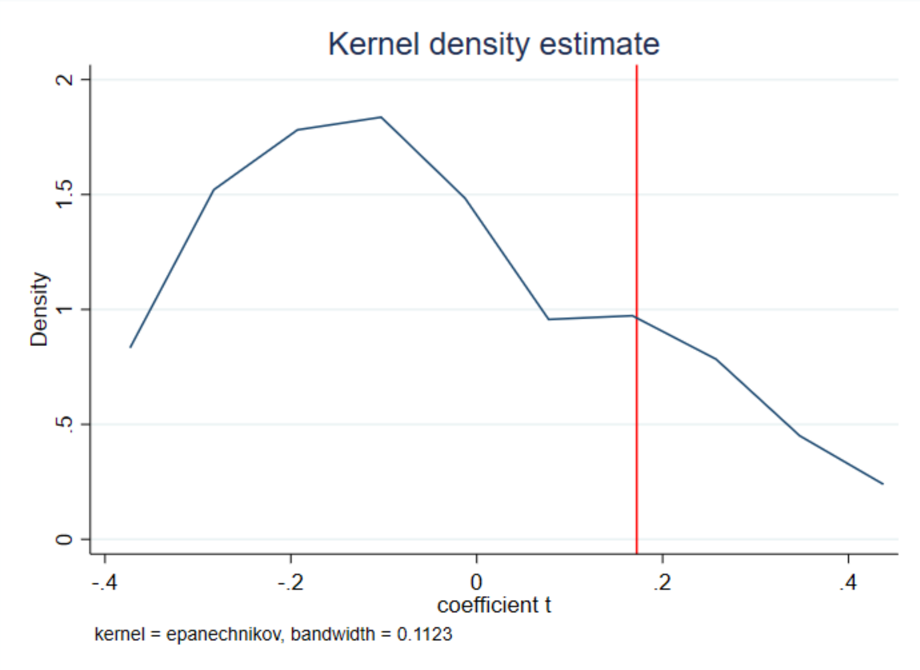


Figure E.2: Randomization inference coefficient distribution for the coefficient of 1 year after announcement

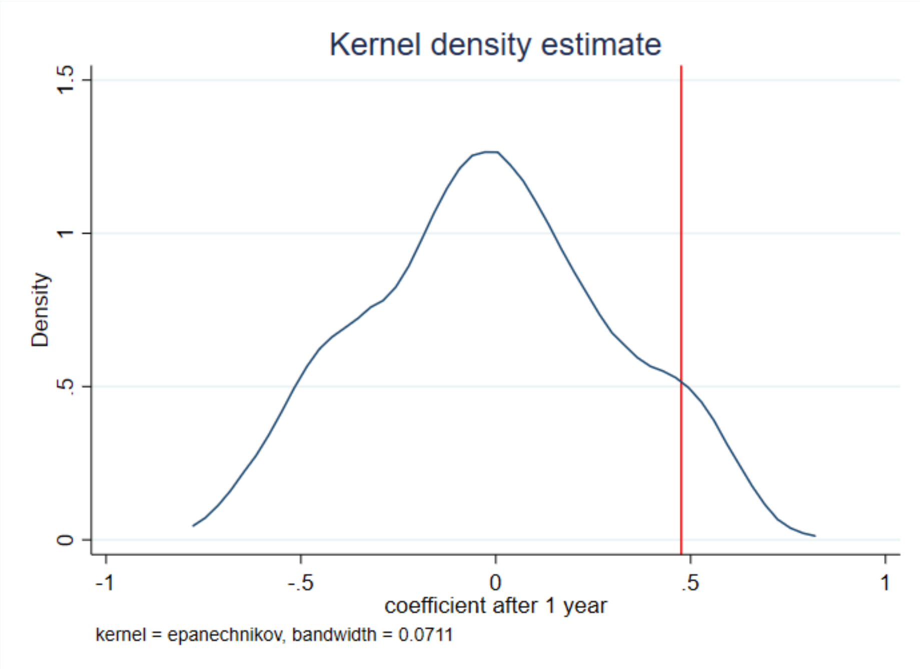
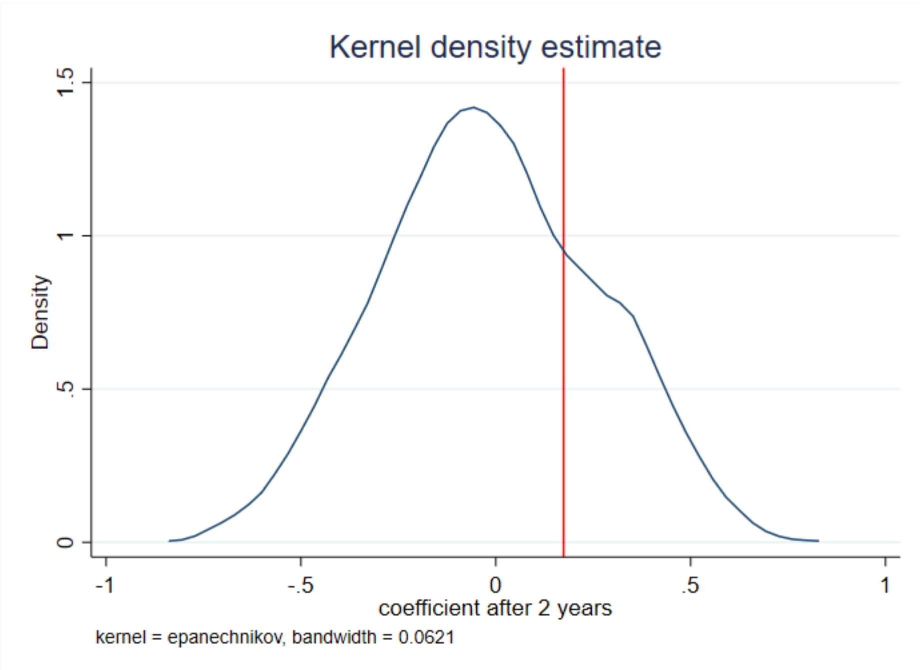


Figure E.3: Randomization inference coefficient distribution for the coefficient of 2 year after announcement



F Results when excluding special institutional areas

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

Impact of IPT on migration flow: different subsamples

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

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). Column (1) excludes Chongqing. Column (2) excludes Guangxi and Ningxia. Column (3) excludes Henan, Shaanxi and Shanxi. Column (4) excludes western provinces. Column (4) excludes observations after 2014. Standard errors clustered at the city level. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

G Estimated sector change by year since the policy

Table G.1: Impact of ITP on sectoral employment in the 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
transict	-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

Notes. Estimated impact of ITP on the employment shares of different sectors in the ITP city; in percentage point changes.Transport includes ICT infrastructure. Hospitality includes hotels, restaurants and cafes. 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 10,000 draws from the coefficient distributions for announcement year 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.

Table G.2: Impact of ITP on sectoral employment 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
transict	-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.Transport includes ICT infrastructure. Hospitality includes hotels, restaurants and cafes. 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 two year lagged 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.

H 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:

$$\begin{aligned}
\frac{d\pi_{d1,d1}}{dITP} &= \pi_{d1,d1}\frac{\frac{d\omega_d}{dITP}}{\omega_d} - \pi_{d1,d1}\frac{\left(1-\pi_{o,d1}\right)\frac{d\left(\sum_{d'} B_{od'}\omega_{d'}\right)}{dITP} + B_{d1,d1}\omega_d\frac{\frac{d\omega_d}{dITP}}{\omega_d}}{\left(1-\pi_{o,d1}\right)\sum_{d'} B_{od'}\omega_{d'} + B_{d1,d1}\omega_d} \\
&= \frac{\frac{d\omega_d}{dITP}}{\omega_d}\pi_{d1,d1}\left(1-\pi_{d1,d1}\right) - \pi_{d1,d1}\frac{\left(1-\pi_{o,d1}\right)\frac{\frac{d\left(\sum_{d'} B_{od'}\omega_{d'}\right)}{dITP}}{\sum_{d'} B_{od'}\omega_{d'}}\left(_{d'} B_{od'}\omega_{d'}\right)}{\left(1-\pi_{o,d1}\right)\sum_{d'} B_{od'}\omega_{d'} + B_{d1,d1}\omega_d} \\
&= \pi_{d1,d1}\left(1-\pi_{d1,d1}\right)\left(\frac{\frac{d\omega_d}{dITP}}{\omega_d} - \frac{\frac{d\left(\sum_{d'} B_{od'}\omega_{d'}\right)}{dITP}}{\sum_{d'} B_{od'}\omega_{d'}}\right) \qquad \text{(H.1)}
\end{aligned}$$

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 $outmigrants_{d1} = \left(1-\pi_{o,d1}\right) * newmigrants_{d1}$. 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:

$$\text{diversion}_d = \frac{\sum\limits_o \left(\pi_{od}^{ITP} - \pi_{od}\right) \text{flow}_{od}}{\text{population}_d}, \tag{H.2}$$

whereas the margins of newly generated migrants is:

$$\text{creation}_d = \frac{\sum\limits_o \left(\pi_{od}^{ITP}\right) \Delta outmigrants_o}{\text{population}_d}, \tag{H.3}$$

These can be combined into:

$$\text{combined}_d = \frac{\sum\limits_o \left(\pi_{od}^{ITP} - \pi_{od}\right) \text{flow}_{od} + \sum\limits_o \left(\pi_{od}^{ITP}\right) \Delta outmigrants_o}{\text{population}_d}. \tag{H.4}$$

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.

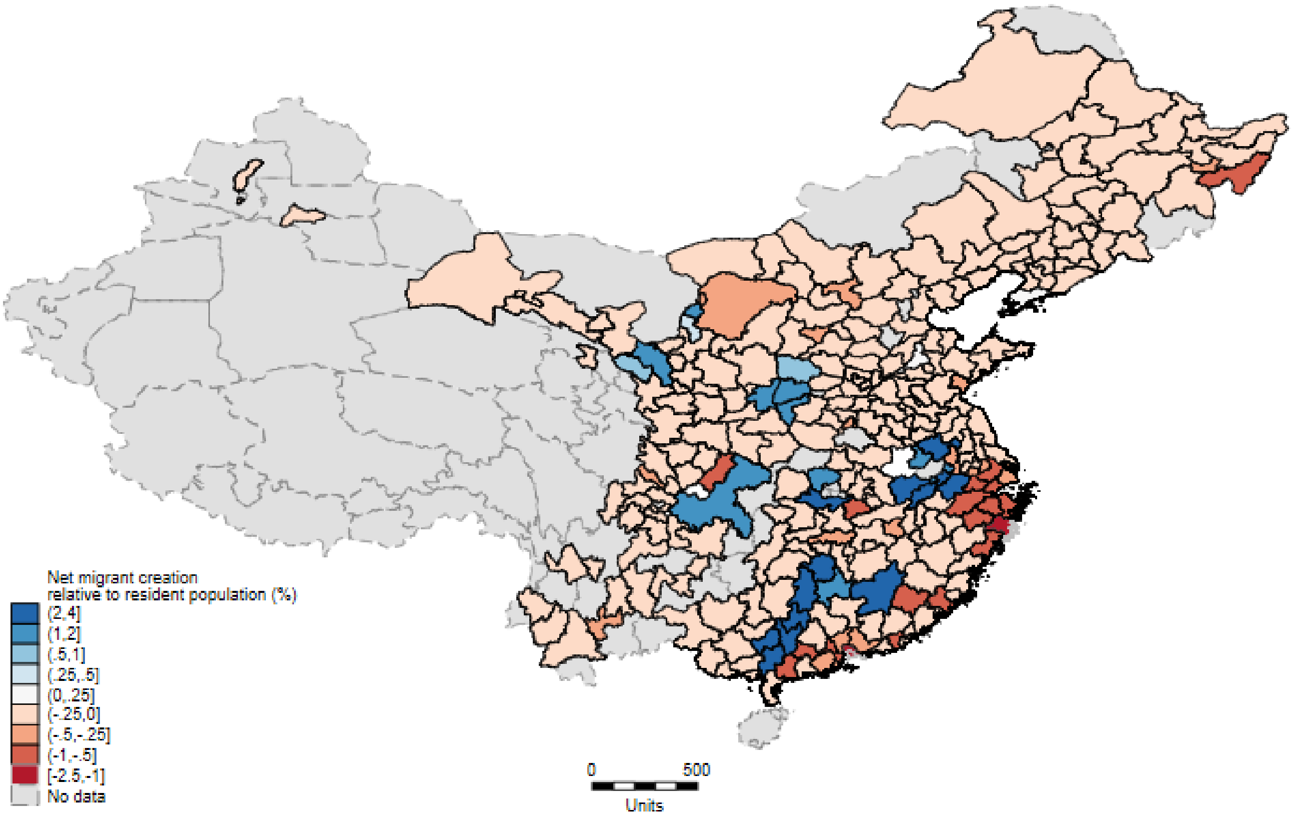
Finally, the city-level changes in size can be aggregated to measure how far the aggregated pattern of urban population differs between a scenario with and without the ITP policy.

$$\begin{aligned} \text{Diversion} &= \sum_d |\text{diversion}_d| \\ \text{Creation} &= \sum_d |\text{creation}_d| \\ \text{Combined} &= \sum_d |\text{diversion}_d + \text{creation}_d| \end{aligned} \tag{H.5}$$

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 H.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 H.1, combined with the diversion numbers from Figure 2, produce the combined impacts shown in Figure 3.

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



The distribution of changes from either margin are plotted in Figure H.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 H.3 plots the estimated rate of outmigration by area, as predicted by equation H.1.

Figure H.2: Population change distributions

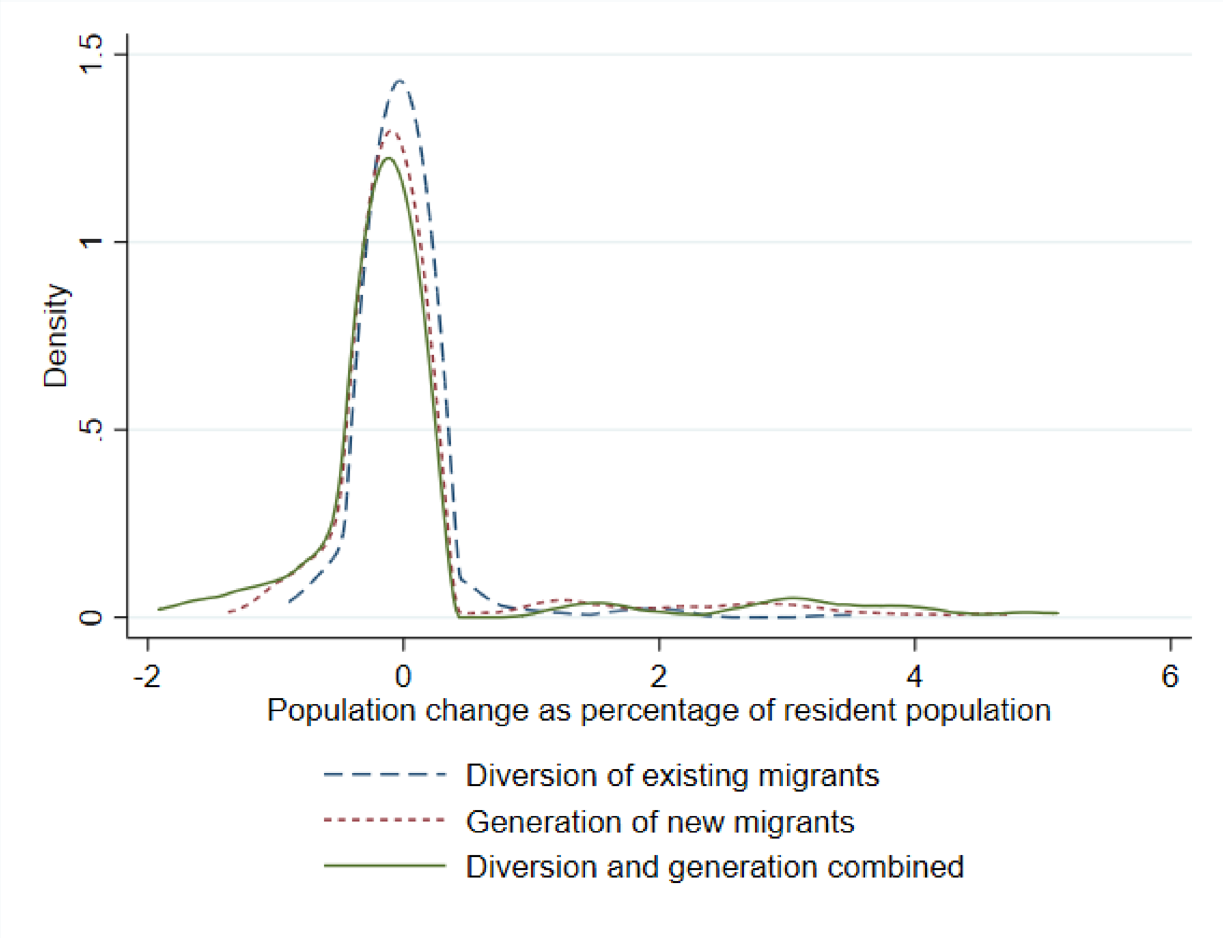
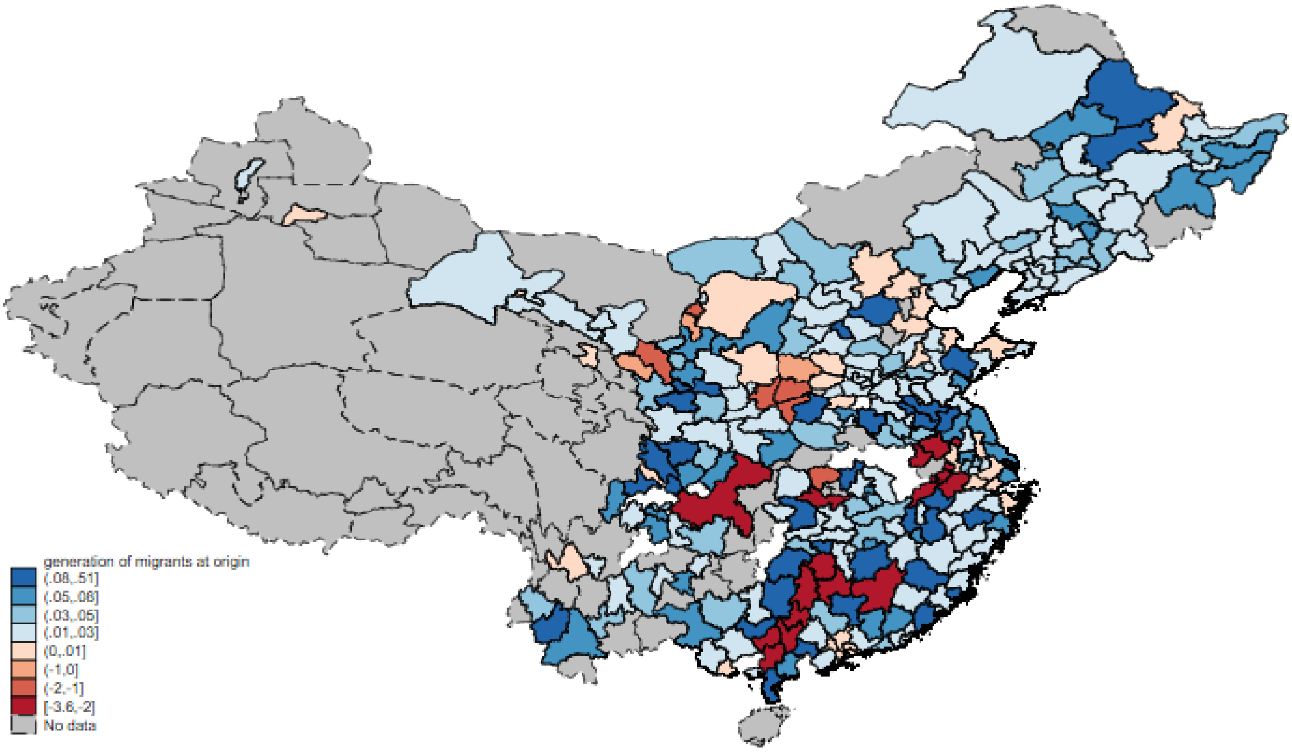


Figure H.3: Percentage of resident population turning to new migrant after ITP



I Wage impacts

Cities that receive an ITP assignment may experience pay increases across all migrants. However, pay increases could also confine to newly attracted migrants, who were incentivized to move. A concentration of wage gains in new migrants could arise if migrants have poor bargaining positions; select into specific incentivized industries after ITP; or if new migrants are in sufficiently segmented parts of the labor market.To formulate potential wage differences between newly attracted migrants and other migrants in the Mincerian regression model, we incorporate two sets of indicators variables:

$$logw_{it} = \sum_k \beta_k Observed[t(ITP) - k] + \sum_k \gamma_k Migrated[t(ITP) - k] + X_{it} + \alpha_d + \alpha_{mt} + u_{it} \tag{I.1}$$

where $Observed[t(ITP) - k]$ is an indicator if migrant i is observed k years after their host city receives an ITP status. The controls X_{it} include sex, age (squared), years of schooling (squared), marital status, hukou status and parental status. The fixed effects α_d and α_{mt} control for host city and matched city pair year fixed effects. The parameter β_k identifies whether a migrant in an ITP city earns higher wages relative to a migrant in a control city when observed k years after the ITP status is assigned.

The results of the Mincerian regressions are reported in Table I.1. The regression in column 1 explains wages relative to the ITP status assignment. Migrants observed in ITP cities during the ITP year show no significant difference in wages on average. As these migrants may include very long-term migrants, we restrict the sample by years since migration to at most 10 years (column 2) or at most 5 years (column 3). The results show a decrease in wages of around 10% in the year after an ITP status, at 10% and 5% confidence intervals.

The regression in column 4 substitutes the observation year impact estimate of ITP, $\sum_k \beta_k Observed[t(ITP) - k]$, for the migration year estimate of ITP, $\sum_k \gamma Arrived[t(ITP) - k]$. The term $Migrated[t(ITP) - k]$ is an indicator if migrant i arrived k years after their host city received an ITP status. The parameter γ_k estimates whether migrants who arrived k years after the ITP status earned higher wages than their peers in control cities. The estimate for the year of arrival relative to ITP does not generally coincide with the year of observation relative to ITP: a migrant entering the year before status assignment may experience a wage increase that all migrants experience when the status is assigned (β_0) but will not get the premium for being in the cohort that arrived in the ITP year (γ_0). We find no evidence that the arrival year in the destination city relative to the ITP policy assignment has significant impact on the wage. When introducing the terms $\sum_k \beta_k Observed[t(ITP) - k]$ and $\sum_k \gamma Arrived[t(ITP) - k]$ simultaneously, neither is significant, but the results are not reported for the potential collinearity issues that may bias the interpretation (as migrants cannot be observed before they arrive.

The regressions in columns 5 and 6 of Table I.1 show the regression in which the wage in the observed year $Observed[t(ITP) - k]$ and the cohort year $Arrived[t(ITP) - k]$ are interacted with an indicator for the coastal origin of the migrant. They show significant wage premia from ITP for migrants from coastal origins, generally over 10 percentage points higher than migrants from non-coastal origins, both in years of observation and in arriving cohorts around the ITP status assignment. The interaction terms form a direct test of the difference in impact by origin. Figure **??** reports the results of the regression in column 5, with the coefficient estimated by group.

Table I.1: Impact of ITP on wage by arrival and observation years

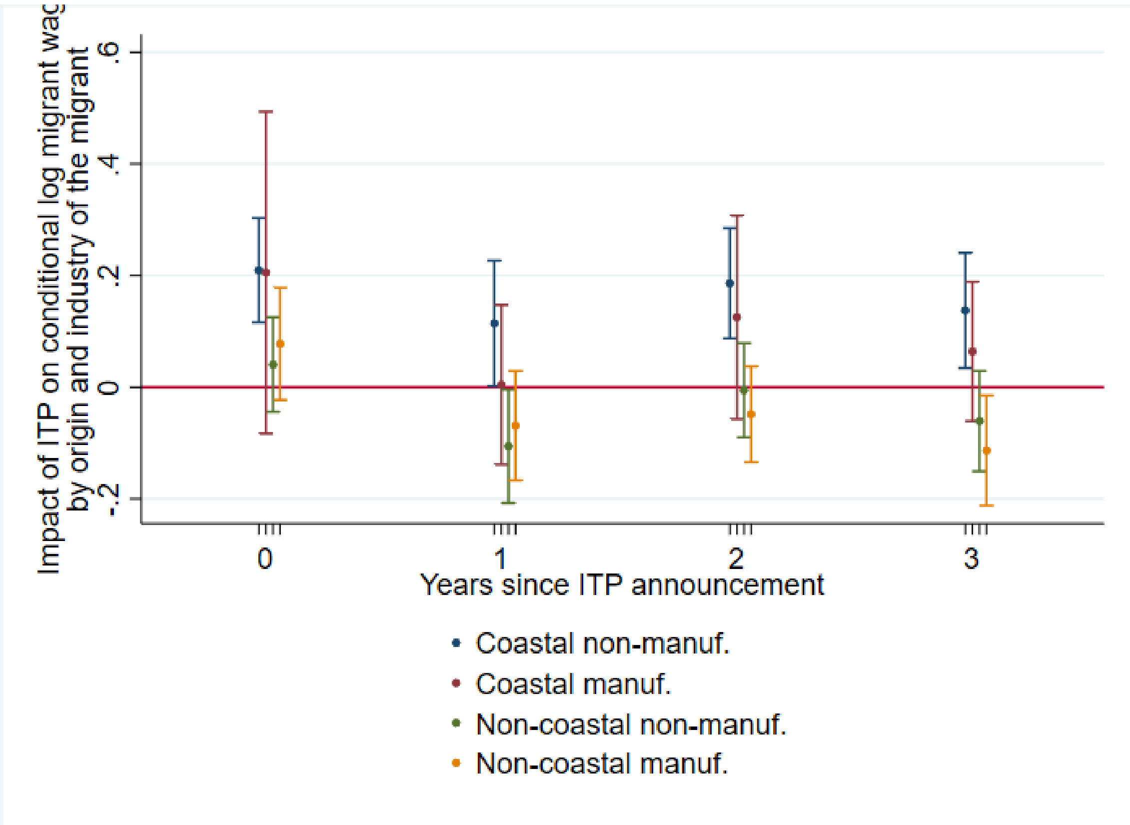
Impact of ITP on wage by arrival and observation years

VARIABLES	(1) any	(2) <=10yr	(3) <=5yr	(4) <=10yr	(5) <=10yr	(6) <=10yr
Observed in year of ITP	0.04	0.06	0.01		0.05	
	(0.05)	(0.04)	(0.04)		(0.04)	
Observed 1 year after ITP	-0.08	-0.09*	-0.12**		-0.10*	
	(0.05)	(0.05)	(0.06)		(0.05)	
Observed 2 years after ITP	-0.00	0.00	-0.00		-0.01	
	(0.05)	(0.04)	(0.04)		(0.04)	
Observed 3 years after ITP	-0.06	-0.05	-0.06		-0.07	
	(0.05)	(0.04)	(0.05)		(0.04)	
Arrived in year of ITP				-0.02		-0.00
				(0.02)		(0.02)
Arrived 1 year after ITP				-0.01		-0.00
				(0.02)		(0.02)
Arrived 2 years after ITP				-0.01		-0.03
				(0.02)		(0.02)
Arrived 3 years after ITP				-0.02		-0.04**
				(0.02)		(0.02)
Obs. ITP year X coastal					0.13***	
					(0.02)	
Obs. 1 yr X coastal					0.17***	
					(0.03)	
Obs. 2 yr X coastal					0.16***	
					(0.03)	
Obs. 3 yr X coastal					0.16***	
					(0.03)	
Arr. ITP year X coastal						0.11***
						(0.03)
Arr. 1 yr X coastal						0.09***
						(0.03)
Arr. 2 yr X coastal						0.18***
						(0.05)
Arr. 3 yr X coastal						0.16**
						(0.07)
Observations	81,956	72,864	55,289	58,202	72,864	38,630
R-squared	0.22	0.23	0.23	0.24	0.23	0.24
controls	yes	yes	yes	yes	yes	yes
matched bin year FE	yes	yes	yes	yes	yes	yes
city FE	yes	yes	yes	yes	yes	yes
industry year FE	yes	yes	yes	yes	yes	yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Based on eq. **??**. "Observed" refers to the year of observation relative to the assignment of ITP status. "Arrived" refers to the year that the observed migrant arrived relative to the assignment of an ITP status. The column titles indicate the number of years since migration for a migrant to be included in the sample: any, maximum 10 years, and maximum 5 years. The controls are sex, age (squared), years of schooling (squared), marital status, hukou status, and parental status. The regressions in column 5 and 6 additionally contain a dummy for the coastal origin of the migrant. Standard errors clustered at the city level. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Figure I.1: Wage impacts of ITP by origin of the migrant and employment status in manufacturing



Note: Estimated from the regression equation $logw_{it} = \sum_k \beta_k O_i \times Manuf_i Observed[t(ITP) - k] + X_{it} + O_i + \alpha_d + \alpha_{mt} + u_{it}$ (see eq. **??**), where O_i is the indicator variable for the coastal origin of the migrant and $Manuf_i$ is the indicator for employment in the manufacturing sector. The regression controls for sex, age and age squared, schooling and schooling squared,industry, marital status, hukou status, and children. Confidence intervals based on destination-year clustered standard errors.

As the ITP policy targets employment in manufacturing, we also split wage impacts inside and outside manufacturing. We find no significant difference in wage development for migrants in manufacturing following an ITP status in the destination city, relative to peer cities. To explore whether manufacturing work was particularly incentivized for migrants of coastal origins, we also include the full interaction set (coastal origin by manufacturing employment status).

Figure I.1 summarizes the wage coefficients by year of observation since the ITP status announcement by migrant origin and status in manufacturing employment. The differences in wage premia are primarily driven by the origin of the migrant, and there are smaller difference in outcomes between employment in manufacturing or elsewhere within the origin group. Similarly, in unreported (space-consuming) regressions, we find no difference among migrants, whether or not split by origin, in broadly defined affected sectors or among migrants with a rural Hukou status.

I.1 Wages and migrant flow elasticity

In this subsection, we examine the impact of wages on migrants flow. First, introduce migrant wages directly into the baseline migration equation (3). Table I.2 show the results. Column 1 shows that migrant wages have a significant and positive effect on the flow, implying a cross-sectional elasticity of the flow to the contemporaneous wage of around 0.23.

Column 2 of table I.2 shows that conditional on match-bin fixed effects, in the matched sample, the elasticity is estimated to be significantly higher. However, the estimate of the impact of ITP on the flow is virtually unchanged (column 4 reports the impacts unconditional on wages for comparison). Column 3 additionally tests whether pre-policy wage rates among migrants predict the flow. We find no significant impact of the wages pre-policy on the policy-induced migration flow.

Table I.2: Impact on log migrant flow: wages

	(1)	(2)	(3)	(4)
Log migrant wage (t+1)			0.03	
			(0.49)	
Log migrant wage	0.23*	1.50***	1.52**	
	(0.13)	(0.58)	(0.60)	
Log migrant wage (t-1)	-0.01	1.55***	1.54***	
	(0.07)	(0.55)	(0.47)	
ITP (t)		-0.19	-0.19	0.13
		(0.36)	(0.33)	(0.17)
ITP (t-1)		0.48*	0.48**	0.48**
		(0.26)	(0.24)	(0.22)
ITP (t-2)		-0.03	-0.03	0.17
		(0.32)	(0.29)	(0.21)
Observations	23,533	1,118	1,118	3,624
Sample	full	matched	matched	matched
Origin year FE	yes	yes	yes	yes
Origin Destination FE	yes	yes	yes	yes
Match bin year FE	no	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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

I.2 Native wages

Table I.3 shows the results of a city-level wage regression for wages of natives. The estimating equation is:

$$logw_{dt} = \beta ITP_{dt} + \alpha_d + \alpha_{mt} + u_{it} \qquad \text{(I.2)}$$

The results in Table I.3 show no evidence of average wage changes among natives with the ITP status. The results in column 1 show a point estimate of around 3% wage decline but the estimate is imprecise. To test for any pretrends, column 2 introduces the 2-year lead of wages.

Table I.3: Impact of ITP on log native wage

	Change in native wage	
	(1)	(2)
VARIABLES	log native wage	log native wage
ITP (t+2)		0.00
		(0.02)
ITP (t)	-0.01	-0.01
	(0.02)	(0.02)
ITP (t-1)	-0.03	-0.03
	(0.02)	(0.02)
ITP (t-2)	-0.05	-0.05
	(0.03)	(0.03)
Observations	202	222
matched bin year FE	yes	yes
city FE	yes	yes
year FE	yes	yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

J Results tables for migrant and native specialization following ITP

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

Panel a: Migrants

VARIABLES	(1) manuf	(2) mining	(3) utility	(4) constru	(5) transict	(6) retail	(7) horeca	(8) financerecom
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

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel b: Natives

VARIABLES	(1) manuf	(2) mining	(3) utility	(4) constru	(5) transict	(6) retail	(7) horeca	(8) financerecom
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

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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). Transport includes ICT infrastructure. Hospitality includes hotels, restaurants and cafes. Finance includes real estate and commercial services. Robust standard errors. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

K Nightlight intensity and pollution changes after ITP status assignment

Table K.1: Impact of ITP on log nightlights

	Change in log lights				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	lnightlight	lnightlight	lnightlight	lnightlight	lnightlight
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

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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

VARIABLES	(1) lpm_mean	(2) lpm_median	(3) lpm_max	(4) lpm_min	(5) lsoot	(6) lso2	(7) lwastewater
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

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes. LPM refers to the log of the mean (column 1) median (column 2), maximum (column 3) and minimum (column 4) of 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

VARIABLES	(1) employment	(2) revenue	(3) capitalpc	(4) tfp	(5) employment	(6) revenue	(7) capitalpc	(8) 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

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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