Talent Incentives as Place-Based Policy: Lessons from China

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Abstract

Governments increasingly pay skilled workers to relocate, hoping to stimulate local growth. Between 2000 and 2019, 209 of 285 prefecture-level cities adopted talent incentive programs for skilled people from elsewhere in China, and 102 also offered an entrepreneurship program. Exploiting the staggered rollout, we draw four lessons. First, the programs attracted the people they targeted: graduate-degree in-migration rose by about 57 percent. Second, they changed the mix of local activity more than its size: knowledge-intensive firms rose by about 7 percent, GDP by only about 2.5 percent, and patents did not rise. Third, the response varied by package: with an entrepreneurship program, foreign investment rose by roughly a third and tax revenue by about 8 percent; with research funding instead, neither rose. Fourth, the cost is likely fiscal: indebted cities were more likely to adopt, and most programs set no end date. These results leave a central question for future work: whether such local gains add up to aggregate welfare gains or merely shift activity from one city to another.

1 Introduction

Skilled people are scarce, and the competition for them has intensified. People are often educated in one place, but because they are mobile, the skills they acquire there may be put to use elsewhere, so where skilled workers locate is itself a subject of competition (Kerr et al., 2016). The recent race for talent in artificial intelligence shows how hard firms now bid for skilled workers, but this kind of bidding war for talent is not new. Universities, research institutes, and governments have long competed for the same people. Unlike firms, governments can compete through place-based policy, by making a location more attractive. Skilled workers may raise the productivity of those around them (Moretti, 2004), so where they settle shapes both aggregate output and regional inequality (Fajgelbaum and Gaubert, 2020).

Such policies have traditionally targeted firms, aiming to spur local demand, jobs, and growth. Increasingly, they also pay people to relocate. Italy and Portugal exempt returning skilled emigrants from much of their income tax, Malaysia taxes returning experts at a flat 15 percent, and in the United States, Tulsa Remote has paid remote workers US$10,000 to move to the city since 2018.

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Chinese cities have run such programs on a larger scale than any of these. In 2000, five of 285 prefecture-level cities paid skilled workers a housing or living subsidy. By 2019, 209 had adopted one, nearly three in four. Because so many cities adopted at different times, China offers a unique setting to study a race for talent among cities.

It is not obvious that such programs work. Because skilled workers are increasingly mobile, subsidies can pull them toward the wrong places: cities that lack the resources, social capital, or other support they need to work effectively, or where good job matches are harder to find. The places that pay may also lack the agglomeration economies, such as access to inputs from other sectors, and knowledge spillovers that would let them realize the gains. Or the subsidies may be too small to attract anyone at all. Despite how widespread these programs have become, little is known about their effects. This chapter asks four questions. Which cities chose to pay? Did the targeted people come? What followed in the cities that paid? Did the answers depend on what the package offered beyond the cash?

The literature points to when such programs might pay off. The subsidy lowers the cost of moving. If the package offers more than cash, it also helps newcomers start a firm and put down roots in a place where they have no prior connections or social capital. Skilled migrants may draw in firms that hire them or found their own, often in knowledge-intensive sectors. Their arrival could raise productivity and innovation through spillovers, and firms with skilled founders grow faster (Queiró, 2022). Newcomers also spend locally, which adds to local demand. If skilled workers are too concentrated in the largest cities, as Fajgelbaum and Gaubert (2020) find for the United States, paying them to locate elsewhere could also raise efficiency.

The programs could instead divert skilled workers from where they are most productive and spread them too thin, weakening agglomeration (Gaubert, 2018). Talent would then be misallocated across the country and growth would be lower (Kline and Moretti, 2014a). The risk is smaller where the cities that pay are already large and dense, since there the programs reinforce agglomeration and could raise growth. That gain, however, may come through a brain drain from the lagging places that most need skilled workers, widening regional gaps (Fajgelbaum and Gaubert, 2020; Gaubert et al., 2025).

Finally, someone has to pay the bill. Because the programs pay a few people rather than large firms, the cost to public finances may be small, but the money could otherwise fund local amenities and public goods. Newcomers may also raise housing costs where housing is already tight, which could provoke a backlash from incumbent residents. Since which of these forces dominates in practice is an empirical question, we track outcomes on both sides: in-migration by skill, firm entry by sector, foreign direct investment, tax revenue, output, and patents for the gains, and local debt and house prices for the costs.

Existing evidence covers only part of this ground. Most studies examine incentives that move people across borders, especially programs that bring emigrants home or recruit skilled people from abroad (Kleven et al., 2013, 2014; Del Carpio et al., 2016; Bassetto and Ippedico, 2025). Yet there is limited evidence on policies that move people within a country at scale (for exceptions, see

Yoo, 2025; Du et al., 2025), on programs paid from municipal budgets, and on outcomes beyond whether people move, such as firm entry, foreign direct investment, public finances, and housing costs. Nor do we know which components of a package go with which outcomes, in particular the entrepreneurship programs that many packages now include.

Chinese talent incentive programs have three features that make them an attractive context for studying the effects of such policies. First, the programs are documented at the local level, so we can collect the documents and observe when each city adopted. Because adoption was voluntary and staggered, we can compare early adopters with later adopters and with cities that had not yet adopted. Second, each package combines several components, such as an entrepreneurship program or research funding alongside the subsidy, so we can ask which components go with which outcomes. Third, data exist on the outcomes the programs could change: skilled migration from the population censuses, firm entry by sector from business registrations, patents, foreign direct investment, tax revenue, output, local debt, and house prices from city yearbooks. We code adoption for 285 prefecture-level cities from 2000 to 2019, transcribe 360 policy documents from 173 cities, and exploit the rapid spread of these programs to estimate their effects.

Our first question is which cities chose to adopt such programs. Early adopters were richer, larger, and more indebted than later ones. Once we adjust for the national wave of adoption, debt is the only trait that predicts timing on its own. Ten more percentage points of debt over GDP raise a city's yearly chance of adopting by about 14 percent. Debt may reflect a bet on growth, with indebted cities attracting talent to grow out of their debt. It may also reflect past housing projects that left unsold homes, which talent programs could fill. If so, cities with weak house prices, a sign of excess supply, should have adopted sooner. They did not. Housing price growth does not predict adoption, which tilts the evidence toward the bet on growth.

These differences between adopters and non-adopters matter for how we estimate the programs' effects. We compare how outcomes change in a city after it adopts with how they change, over the same years, in cities that have not yet adopted or never do. Because the comparison is in changes, it is unaffected by adopters being richer or larger to begin with. What it requires is that, absent the program, outcomes in adopting cities would have followed the same path as in the comparison cities. Two pieces of evidence support this. The traits we observe predict timing only modestly, and most outcomes show no difference between the two groups before adoption. Together, they support reading our estimates as the effects of the programs on the cities that adopted them, rather than as trends those cities were already on.

With this staggered difference-in-differences design, we find that the programs attracted the skilled workers they targeted. Relative to the comparison group, in-migration of graduate-degree holders rose by about 57 percent, and in-migration of all college graduates by about 18 percent. These percentages are large partly because graduate migration between any two cities starts from a small base.[1] Non-college individuals, whom the programs did not target, did not respond

[1]Graduate migration between most pairs of cities is zero in a given census wave. Because we measure flows in logs, the estimate covers only city pairs with at least one graduate migrant, so it captures growth in existing flows rather than the opening of new ones.

(Section 5.2).

Economic activity followed the newcomers. The stock of registered firms in knowledge-intensive sectors rose by about 7 percent. Local output rose more slowly, by only about 2.5 percent from the third year after adoption, and patenting did not respond (Section 5.3). The programs thus changed what these cities produce more than how much they produce, with no measurable rise in innovation. This result differs from programs that pay a city's own graduates to return home, which raised local productivity (Du et al., 2025). One explanation is that newcomers from elsewhere lack the local ties that help returnees put their skills to use.

If newcomers need more than cash to put their skills to use, what else the package offers should matter. We therefore split adopters into three groups: the subsidy alone, the subsidy with an entrepreneurship program, and the subsidy with research funding. When the subsidy came with an entrepreneurship program, foreign direct investment rose by about a third, tax revenue by about 8 percent, and more knowledge-intensive firms entered. With the subsidy alone, tax revenue also rose, though the patterns are weaker. Where the package came with research funding, firm entry rose less, and neither foreign investment nor tax revenue rose. Patenting did not rise under any package.[2]

One interpretation is that the programs attracted the skilled workers whom foreign firms needed to hire, and that the new foreign investment and knowledge-intensive firms in turn raised tax collection. The new activity was more formal and more taxable, but it added less to output. Because our estimates are relative, part of this activity may have been diverted from the cities in the comparison group, where it would otherwise have located, so the country as a whole need not have gained more foreign investment or knowledge-intensive firms. What a package includes thus shapes which activity a city attracts. Whether that activity adds to national output, however, is a separate question.

We also take a first look at the costs. With an entrepreneurship program, local government debt rose by 3.1 percentage points of GDP after adoption, but it was already rising beforehand, so we cannot attribute the increase to the program. The pattern instead fits our finding that more indebted cities were the ones that adopted. With the subsidy alone, debt rose by a similar 2.8 points, though imprecisely, and with research funding by an insignificant 1.3 points. House prices did not change.

The more lasting cost may be the commitment itself. Most policy documents name no end date, so the programs are a standing obligation. Because we do not observe take-up, the number of recipients, our evidence on costs is indirect. Measuring what the programs cost, and whether the gains justify it, requires tracing how a subsidy in one city moves people, firms, and tax revenue across all cities. Our ongoing work takes up this question in a dynamic model of migration, trade, and entrepreneurship (Du et al., 2026). In that model, skilled people choose not only where to live but also whether to work for others or start a firm. A subsidy that rewards moving may draw

[2]Each package's effects are measured against comparison cities; the differences between packages are not statistically significant (Section 5.4).

skilled people away from starting firms, and it is paid for with public goods that residents who receive no subsidy also rely on.

We begin in Section 2 by placing these programs within the broader literature on place-based policy, focusing on programs that pay people rather than firms. Section 3 describes the Chinese programs and how we measure them. Section 4 asks which cities chose to adopt. Section 5 sets out the design and asks whether the targeted people came, what followed, and whether the answers depended on what the package offered. Section 6 concludes and draws lessons for policy.

2 Place-Based Policy That Pays People

The place-based policies most studied in the literature pay firms to locate or invest in a place, or invest in places directly, from Empowerment Zones in the United States to Special Economic Zones in China and elsewhere (Alder et al., 2016). A large literature studies both their efficiency and their distributional consequences. The efficiency case rests on spillovers that people and firms do not take into account when they choose where to locate, while the distributional case rests on limits to transfers between people (Fajgelbaum and Gaubert, 2020, 2025; Gaubert et al., 2025). The evidence is mixed. Some evaluated programs brought sizable local gains, but critics have long worried that such programs generate large misallocation and distort economic behavior, and that much of a local gain comes at another place's expense.[3] In theory, spatial inequality can also be addressed with transfers aimed at groups of people, such as workers of a given skill in a given place (Fajgelbaum and Gaubert, 2020). In China and in many other countries, a growing class of place-based policies targets people.

Table 1 lists thirteen programs that pay individuals, rather than firms, to locate in a place, either across national borders or across jurisdictions within a country. Each row records when the program began, who runs and funds it, what it pays, and whom it makes eligible.[4] National governments run ten of them. Most of these aim to bring back nationals or to attract highly skilled people from abroad, and recent studies find that people respond (Kleven et al., 2013, 2014; Del Carpio et al., 2016; Bassetto and Ippedico, 2025). The other three run below the national level: China's city talent incentive programs, Tulsa Remote, which a foundation funds, and Ascend WV, which a university

[3] In the United States, Empowerment Zones raised local employment and wages at modest efficiency cost (Busso et al., 2013a), while the agglomeration gains of the Tennessee Valley Authority were offset by losses in the rest of the country (Kline and Moretti, 2014a), a displacement concern that recurs across programs (Kline and Moretti, 2014b; Neumark and Simpson, 2015). When jurisdictions compete for mobile firms, they can bid away the surplus (Slattery, 2025). In sales-tax competition among US counties, ignoring how each county responds to the others understates the welfare effects of limiting that competition (Agrawal et al., 2025).

[4] Program rules: Talent incentive programs, China: municipal policy documents and the authors' coding; Thousand Talents: Central Organization Department rules (2008, 2011); Tulsa Remote: program website (tulsaremote.com); Ascend WV: program website (ascendwv.com); Italy: Law 238/2010 and Revenue Agency (agenziaentrate.gov.it); Spain: Laws 62/2003 and 28/2022 and Tax Agency (agenciatributaria.gob.es); Non-Habitual Resident, Portugal: Decree-Law 249/2009 and Law 82/2023; Regressar, Portugal: program website (programaregressar.gov.pt), Portaria 214/2019, and Law 82/2023; Ireland: Revenue (revenue.ie); Denmark: Tax Agency (skat.dk); Malaysia: TalentCorp (talentcorp.com.my); Chile: CORFO officials in WIPO Magazine (2014); Canada: IRCC (canada.ca) and SOR/2018-72. We checked each entry against the program's own rules or website.

runs with gifts. These local programs are our focus. We also leave aside forced relocation and study programs that pay people to move by choice.[5]

The instruments range from cash grants and housing subsidies to flat tax rates, income-tax exemptions, and residence rights. Italy's Controesodo, for example, exempted 70 to 80 percent of a returning graduate's income from income tax (Bassetto and Ippedico, 2025). Those eligible range from workers who meet an education requirement, as in China's cities, and remote workers, as in Tulsa, to the most common targets, returning emigrants and researchers and high earners from abroad. A feature that has received less attention is that nine of the thirteen programs also cover founders or business income. They target entrepreneurs and start-ups rather than established firms.

Most evaluations of programs that pay people to locate study national programs. Several of these aim at nationals abroad, whose ties of family, nationality, and place may draw them home, and the pool is large: about 28 million skilled migrants lived in OECD countries in 2010, up about 130 percent since 1990 (Kerr et al., 2016). Italy's exemption raised the return rate of skilled emigrants by about 39 percent, so that about one in four eligible returnees came because of it. The exemption is costly at first, because it also pays those who would have returned anyway, but it pays for itself in present value if enough of the returnees it drew stay after it ends (Bassetto and Ippedico, 2025). Malaysia's Returning Expert Programme raised the probability of return by 40 percentage points for applicants who already held a job offer, and had no clear effect on those without one (Del Carpio et al., 2016). Other national programs recruit people from abroad regardless of nationality. Spain's regime raised the share of eligible foreign football players in its league by about half (Kleven et al., 2013). Denmark's scheme for researchers and high earners from abroad doubled the number of foreigners just above the eligibility threshold relative to those just below, and employers captured part of the tax break through lower pretax pay (Kleven et al., 2014).

For China, we build on a small literature on the country's own programs for skilled people. Scientists recruited by the Young Thousand Talents program published more papers as last author after their return, a gain explained by funding and team size (Shi et al., 2023). Hometown-return incentives drew college graduates back to their home cities and raised local productivity and wages, with larger gains where social capital is high, consistent with returnees putting their local ties to use (Du et al., 2025). These studies do not examine programs open to any graduate or skilled worker from anywhere, with broader packages, or the entrepreneurship programs and other components of those packages.

We emphasize the entrepreneurship side of these programs. Some programs exist only for founders. Start-Up Chile pays early-stage founders a seed grant and a work visa, to bring founders from abroad and change the local ecosystem (Gonzalez-Uribe and Leatherbee, 2018), and Canada's Start-up Visa grants permanent residence to founders backed by a designated investor. Others add founders to a broader program, such as the Thousand Talents track for entrepreneurs who

[5]During the send-down movement of 1968 to the late 1970s, about 16 million urban youths were sent to the countryside, and Chen et al. (2020a) find that their presence raised the schooling of rural children.

Table 1: People-Targeted Place-Based Programs

Program	Start	Instrument and headline generosity	Eligible people	Founders	Studies
A. Run by cities, universities, or private funders					
Talent incentive programs, China (city governments)	2000	Housing and living subsidies; hukou	Workers who meet a city eligibility requirement	Yes	This chapter; Du et al. (2026)
Tulsa Remote, US (foundation-funded)	2018	Cash grant, US$10,000	Remote employees of out-of-state firms	No	Yoo (2025)
Ascend WV, US (university-run, gift-funded)	2021	Cash grant, US$12,000 over two years	Remote workers and remote business owners	Yes	–
B. Run by national governments					
Researcher scheme, Denmark	1992	Flat 27% tax for up to 7 years (30% for 3 years at the start)	Researchers and high earners from abroad	No	Kleven et al. (2014)
Beckham law, Spain	2004	Flat 24% tax up to EUR 600k for 6 years	Movers not resident for 5 years	Yes	Kleven et al. (2013)
Thousand Talents, China	2008	One-time RMB 1m; housing, hukou, and tax privileges	Overseas PhDs aged 55 or under	Yes	Shi et al. (2023)
Non-Habitual Resident, Portugal	2009	Flat 20% tax on high-value income for 10 years; closed 2024	Movers not resident for 5 years	Yes	–
Controesodo and impatriati, Italy	2010	Income-tax exemption of 70–80% (2010 scheme) or 50% for 5 years (2024)	Graduate returnees; skilled movers	Yes	Bassetto and Ippedico (2025)
Start-Up Chile	2010	US$40,000 seed grant, no equity taken (pilot); work visa	Early-stage founders, at first from abroad	Yes	Gonzalez-Uribe and Leatherbee (2018)
Returning Expert, Malaysia	2011	Flat 15% tax for 5 years; car-duty exemption	Citizens employed abroad 3+ years	No	Del Carpio et al. (2016)
SARP, Ireland	2012	30% of income above EUR 75–125k exempt from income tax	Employees posted from abroad by employers	No	–
Start-up Visa, Canada	2013	Permanent residence; no public cash; paused 2026	Founders backed by a designated investor	Yes	Lee and Glennon (2023)
Regressar, Portugal	2019	Grant of 6 IAS (EUR 2,615 in 2019) plus moving costs; 50% income-tax exclusion for 5 years	Emigrants back after 12+ months abroad	Yes	–

Notes: Programs that pay or privilege individuals, not firms, to locate, return, or found a firm in a place. Start is the year the program was introduced, and for the Chinese city programs the first year of our panel, when 5 of 285 prefectures already paid a subsidy; Denmark's scheme applied to contracts from June 1991 and Italy's to returns from January 2009. Amounts are headline terms, and most have changed over time; IAS is Portugal's social support index. Founders: whether founders or business income are eligible. Studies: studies of the program that we found, – where we found none; for the Chinese city programs, this chapter and our companion paper; Spain on football players; Thousand Talents on its Young Thousand Talents sub-program. *Sources:* Official program rules and websites, listed in footnote 4.

bring their own technology and venture capital to a high-tech park (Zweig and Wang, 2013), or Ascend WV, which accepts owners of remote businesses.[6] Apart from Ascend WV, these programs run at the national level. In China, most cities that adopted a talent incentive program also ran an entrepreneurship program. Founders build firms that hire workers and may raise output and productivity, and Chinese cities valued returnee founders for the jobs and tax base their firms bring (Zweig, 2006).

Of the two founder programs that have been studied, one shows that residence rights move founders and the other that cash alone does little for their ventures. Canada's visa raised the probability that foreign-educated founders based in the United States founded their firm in Canada by about 70 percent (Lee and Glennon, 2023). In Chile, the cash and shared office space had no detectable effect on venture outcomes, while the program's entrepreneurship training raised fundraising (Gonzalez-Uribe and Leatherbee, 2018). Section 3 describes how the Chinese packages support founders.

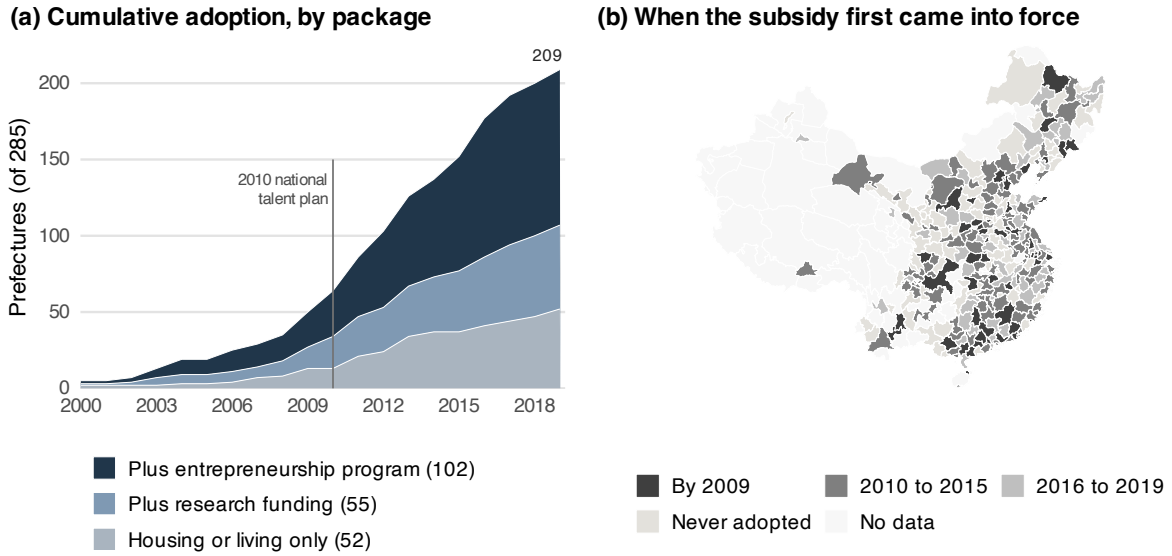
The Chinese city programs differ from those studied before in three ways. First, the literature mostly studies people who move across borders, while the Chinese programs move people within a country. Such moves are far more common but less studied, and only three of the thirteen programs in Table 1 pay for them. Second, the Chinese programs are paid from municipal budgets. The other two local programs, Tulsa Remote and Ascend WV, each run in a single place and are financed with private money. When a city budget pays, the money is not spent on local public goods and services, so where the programs go and what they cost must be studied together. Third, most prior studies measure whether people come. We add outcomes that could follow from their arrival: firm entry, foreign direct investment, and public finances, including tax revenue. We also ask how these outcomes vary with what the package offers, in particular whether it includes an entrepreneurship program or research funding. These components go with different outcomes (Section 5.4), which matters for how we think about adopting such programs.

3 Talent Incentive Programs in Chinese Cities

In this section, we describe the talent incentive programs of Chinese cities and highlight three features that our empirical analysis and identification strategy use in Sections 4 and 5: when a city's subsidy began, what else its package included, and whom it made eligible. The policy documents, including opinions, measures, and implementation rules, come from city government websites, mostly those of the municipal people's government and its Human Resources and Social Security Bureau, and Ma et al. (2026) document how they were collected. We build on and extend their database, adding the entrepreneurship programs, which we code when a document names start-up funding or entrepreneurship explicitly. We code these documents into a panel of 285 prefecture-level cities from 2000 to 2019, which defines adoption, our treatment. We also transcribed 360

[6] Regressar offers returning emigrants a credit line for business investment. The tax regimes of Italy (until 2023), Spain (since 2023), and Portugal cover business income.

Figure 1: When and Where Cities Adopted, 2000–2019



Notes: Panel (a) gives the cumulative number of the 285 prefectures with a housing or living subsidy in force, by the package in the adoption year: the subsidy alone (52 by 2019), with research funding (55), or with an entrepreneurship program, with or without research funding (102). The vertical line marks the 2010 national talent plan. Panel (b) shades each prefecture by the period in which its subsidy first came into force: by 2009 (50), 2010 to 2015 (102), 2016 to 2019 (57, one without a map boundary), or never (76). The 102 program adopters in (a) and the 102 cities of 2010 to 2015 in (b) are different groups. *Source:* Authors' coding of municipal policy documents, 2000–2019.

documents from 173 cities to record each program's requirements, tiers, duration, and coverage of incumbents.

The programs grew out of an older competition for returning scholars. In the early 1990s, the central government moved from restricting study abroad toward letting scholars come and go freely, and cities soon competed for returnees with tax breaks, housing, schooling, jobs for spouses, residence permits, and start-up parks (Zweig, 2006). The first city programs appeared as early as 2000, but they were small and isolated experiments. The turning point came in 2010, when the central government issued the National Medium- and Long-Term Talent Development Plan (2010–2020), which made talent a national priority. The central government encouraged the programs but left their design and implementation to city governments. The programs are therefore place-based and talent-targeted: the benefit is tied to settling in the city, and eligibility requires a minimum level of education, a professional title, or research output. Cities pay for the programs from their own budgets, which rely mostly on land-sale revenue and local taxes, and on borrowing through local government financing vehicles against future land revenue (Chen et al., 2020b). Local officials also have strong reasons to pursue growth, because their promotion depends on the economic performance of their jurisdiction (Li and Zhou, 2005; Zhou, 2007).

Figure 1 shows that adoption came in two phases: isolated programs at first, then a rapid rise after the 2010 plan. Panel (a) gives the cumulative number of adopters, stacked by package, and panel (b) maps the period in which each city first adopted. Five prefectures had a subsidy in 2000,

and a decade later only 50 did. After the national plan, the number rose to 152 by 2015 and 209 by 2019. Adoption came earlier in the east than in the west. By 2009, 22 percent of eastern prefectures had adopted against 11 percent of western ones, and by 2019 every prefecture in the Yangtze River Delta had adopted against 61 percent of those in the west.[7] Some programs set large targets. For instance, in 2017, Wuhan aimed to keep one million university graduates in the city within five years and offered them housing at 20 percent below market prices.[8]

Targeting scholars and other high-skilled workers has become increasingly common in China, and the target has shifted from a small elite of returning researchers to domestic graduates at scale. Overseas or returnee targeting appears in 53 percent of the documents dated 2000 to 2010 but in only 29 percent of those dated 2016 to 2019. Over the same years, restrictions to graduates of elite universities rose from 4 to 25 percent of documents. The later programs, which account for most adoptions, took over the earlier incentives and modified them for domestic graduates.

Each program sorts eligible applicants into lettered or numbered tiers, each with its own bundle. Packages and amounts vary across cities. Hangzhou's E class, for instance, covers its senior talent, mainly holders of senior professional titles and doctorate holders who publish in core journals. It offers them a rental unit or RMB 1,200 (about US$190) a month, about a tenth of the city's GDP per person over a year, or RMB 126,000 (about US$20,000) over ten years. In contrast, Hebi's E class reaches any full-time bachelor's graduate, with a one-time housing payment of RMB 30,000 (about US$4,300), about half of the city's GDP per person, and a rent subsidy of RMB 500 (about US$70) a month.[9]

Beyond the tiers, nearly every package carries a housing or living subsidy. The three groups stacked in panel (a) of Figure 1 do not overlap. Fifty-two began with a housing or living subsidy alone, 55 added research funding, and 102 carried an entrepreneurship program from the start.[10] Living support appears in 95 percent of packages in the adoption year and housing subsidies in 86 percent. Research funding, entrepreneurship programs, career provisions, and hukou each appear in 46 to 59 percent of packages at adoption and in 62 to 74 percent by 2019. In the city-years from 2011 to 2014 in which a city paid a housing or living subsidy and the documents record an amount, they promise a median one-time housing subsidy of RMB 50,000 for the lowest tier and

[7]The east comprises the 100 prefectures of Beijing, Tianjin, Hebei, Liaoning, Shanghai, Jiangsu, Zhejiang, Fujian, Shandong, Guangdong, and Hainan; the west comprises the 85 prefectures of the twelve provinces of the Western Development program; the Yangtze River Delta comprises the 25 prefectures of Shanghai, Jiangsu, and Zhejiang. Central and northeastern prefectures adopted about as early as eastern ones: 19 percent had adopted by 2009.

[8]Wuhan's 2017 target and housing discount: Ministry of Education, January 18, 2018 (http://www.moe.gov.cn/jyb_xwfb/moe_2082/zl_2017n/2017_zl85/201801/t20180118_325012.html), and Renmin Zhengxie Wang, September 25, 2017 (https://www.rmzxw.com.cn/c/2017-09-25/1813608.shtml).

[9]Hangzhou, Opinions on the Recruitment and Development of High-Level Talent, Innovative and Entrepreneurial Talent, and Teams (January 21, 2015), item 4, item 19, and the attached classification catalogue. Hebi, Measures for the Recognition and Support of High-Level Talent and Interim Implementing Measures for High-Level Talent Housing Support (2019). Ten-year values multiply a year of payments by 8.75, the value of a payment made each year for ten years discounted at 2.5 percent. Dollar values use the average exchange rate of the document year, 6.23 yuan per dollar in 2015 and 6.91 in 2019. GDP per person divides the city's GDP by its registered population in the document year, from the municipal statistical yearbooks.

[10]The last group combines 67 adopters whose first package joined housing or living support, research funding, and an entrepreneurship program with 35 whose package had an entrepreneurship program but no research funding.

RMB 250,000 across tiers. At the median house price in those city-years, about RMB 4,500 per square meter, these sums buy about 11 and 56 square meters. We can think of the value of a city's housing and living subsidy, with monthly payments valued over their duration, relative to the price of a 100-square-meter dwelling in the city as its subsidy intensity.

We count a city as adopting when its documents provide a housing or living subsidy, whether or not they record its amount. Because nearly every package includes such a subsidy, this indicator captures most of the variation in the programs. We treat one other form of variation with care: rules that grant hukou and nothing else. Since 2017, for example, Chengdu has granted hukou to any full-time bachelor's graduate who presents a diploma, but offers no cash.[11] We do not count such hukou-only rules as adoption, because they coincide with the national hukou reform of the same years (Huỳnh et al., 2026), and we cannot fully separate the two (Section 5.2).

Although it is reasonable to treat adoption as a binary decision, we cannot overlook how the packages differ in what they contain. Many policies fund not only housing but also start-ups, through grants, incubators, and venture financing, and the research of the scholars they recruit. Hangzhou's 2015 program, for instance, gives returnees who start firms RMB 30,000 to 200,000 and funds outstanding and key projects with RMB 500,000 and RMB 1 million. Of the 133 cities that ever ran an entrepreneurship program, 102 had one when they first adopted and 31 added one later. Only 37 city-years have a program without a subsidy, all in years after the city's subsidy lapsed. The program almost always arrived with the subsidy, so we cannot separate the effect of the entrepreneurship program alone. Instead, we compare packages with and without it, which measures what the program adds to a housing or living subsidy.

To gauge how many people the programs reach, we take the lowest qualification that earns any benefit. Across the 173 cities whose documents we transcribed, that requirement is a doctorate in 11 percent, a master's degree in 27 percent, a bachelor's degree in 34 percent, and a junior college degree in 24 percent, and 4 percent name no degree. A master's degree therefore meets the requirement almost everywhere, so the migration of graduate-degree holders is the closest test of whether the programs reached their target. Over the 166 cities whose documents name a degree, weighted by labor stock, 4.8 percent of workers met the requirement in 2010, about one in twenty, and 6.9 percent in 2015, about one in fifteen. Limits on age, full-time study, and elite universities narrow the eligible group further.[12]

On the one hand, few people qualify. On the other hand, the commitments are loose. Of the documents, 79 percent set no expiry date, and 54 percent also cover incumbents, the skilled workers already in the city whom it is trying to keep. We cannot study this margin, because we observe neither who applies nor who receives the benefit, so we do not know how far the programs change

[11] Chengdu, Opinions on Policy Measures to Innovate Factor Supply, Cultivate the Industrial Ecosystem, and Raise the Industrial Capacity of a National Central City, item 2.

[12] In the largest cities the requirement decides who may settle among the many who want to, rather than attracting more people. In 2018 Shanghai let undergraduates of Peking and Tsinghua Universities settle directly, and in 2022 it granted settlement on arrival to graduates of the world's top 50 universities (as reported by China Securities Journal, https://news.cnstock.com/news,bwkx-202206-4896075.htm). In such a city a lower requirement can change which graduates settle more than how many arrive.

the decision to stay.

Given these requirements and our measure of adoption, the rest of this section describes the data that link adoption to economic outcomes. Section 4 first examines the correlates of the adoption decision, and Section 5 turns to the local effects of the programs on economic activity.

Data

We rely on five sources of data to study which cities adopt talent incentive programs and the local effects of adoption. First, the 2000 and 2010 Population Censuses and the 2005 and 2015 one-percent Population Sample Surveys allow us to track migration flows by origin and destination, as well as the education of migrants, across four periods. Second, we measure business activity, particularly firm entry, using the business registrations of all 285 cities from 2001, by industry, which come from the State Administration for Industry and Commerce. Third, we also measure economic activity at a more informal level, including business owners, using two national household surveys, the Chinese General Social Survey and the China Household Finance Survey, from 2010 to 2019.

Fourth, we augment these with counts of patents at the city-industry-year level from the China National Intellectual Property Administration. Fifth, we measure output, investment, fiscal variables, and housing at the local level using municipal statistical yearbooks, as compiled in the China City Statistical Yearbooks, from 2000 to 2019. Tax revenue is available from 2005, local government debt from 2006, and house prices from 2003. We treat reported zero debt as missing. We measure foreign direct investment in US dollars, as the yearbooks report it, and deflate nominal values by the city consumer price index.

4 Who Adopts

Adoption, especially in the early years, was not random. Cities that adopted early may differ from those that adopted later in ways that shape the outcomes we study, including migration, firms, and public finances. They may have had a favorable location, more wealth, or a greater concentration of human capital. Such differences could bias our estimates, because any gains could reflect favorable initial conditions rather than the programs themselves. In this section, we ask which cities adopted first and which traits predict the city that adopts next.

We first compare early and later adopters in levels. Of the 235 prefectures with no subsidy before 2010, 102 first adopted in 2010–2015 and 133 adopted later or never. Early adopters were more indebted, richer, larger, and somewhat more skilled. Their local government debt was 8.45 percent of GDP in 2009, against 5.93 percent among later adopters, a gap of 2.5 percentage points (standard error 1.05) that is significant at the 5 percent level. Their GDP per capita was RMB 35,900, against RMB 23,600, and they had 2.7 million workers, against 2.2 million. Both gaps are statistically significant. Their college share of workers was 7.4 percent, against 6.6 percent, a gap that is not significant. Early adopters were thus different places from later ones. For this reason, our design

does not compare levels. It compares changes in adopting cities with changes in cities that have not yet adopted (Section 5.1).

Because these traits move together, the raw gaps cannot say which of them matters. We therefore ask which traits predict the next city to adopt, using a discrete-time hazard of first adoption:

$$\Pr\left(A_{ct} = 1 \mid A_{c,t-1} = 0\right) = 1 - \exp\left[-\exp\left(\delta_t + X'_{c,t-1}\beta\right)\right], \tag{1}$$

where A_{ct} indicates that city c has adopted a housing or living subsidy by year t, δ_t are year effects, and $X_{c,t-1}$ holds the city's traits in the year before: debt over GDP, log GDP per capita, the college share of workers, log population, log fiscal revenue, GDP growth, and house-price growth. The sample consists of the cities still without a subsidy in each year from 2007 to 2019. A city leaves the sample when it adopts, and cities with a subsidy in 2000 are excluded. The year effects absorb the national wave, so a trait predicts adoption only if cities with more of it adopted sooner than other cities in the same year. Each element of β is the log of the ratio of two cities' yearly chances of adopting when they differ by one unit of that trait. Standard errors are clustered by city. Table 2 reports the estimates, from 1,776 city-years, 231 cities, and 160 adoptions.

Debt is the only trait that predicts timing on its own. Ten more percentage points of debt over GDP raise a city's yearly chance of adopting by about 14 percent (0.128, standard error 0.064). The 2.5-point gap in debt between early and later adopters thus corresponds to a yearly chance of adopting only about 3 percent higher. None of the other six traits predicts timing on its own: log GDP per capita, the college share of workers, log population, log fiscal revenue, GDP growth, and house-price growth. Jointly, however, the seven traits do predict adoption. A Wald test rejects that their coefficients are all zero ($p < 0.001$). Insignificant coefficients on traits that move together mean that none of them adds to the others, not that they are irrelevant. The debt coefficient does not rest on one or two cities. Dropping one city at a time, it remains significant at the 10 percent level in all 231 fits, with p between 0.02 and 0.06. A logit in place of the complementary log-log gives a similar coefficient, 0.160 (0.076). Debt over GDP is thus the one trait that stands out in predicting when a city adopts.

Taking the debt coefficient at face value, why would indebted cities adopt earlier? Two stories could explain it. In the first, a city that has borrowed against future land revenue uses that capacity to fund a package that attracts skilled workers. The city is betting on growth that would service its debt. In the second, debt records past projects, housing projects in particular, that left unsold and perhaps empty housing. Talent programs would then be a way to fill that housing, and debt would measure how much the city had overinvested.[13] If programs respond to a weak housing market, falling house prices should predict adoption. House-price growth does not predict adoption (Table 2: 0.009 per 10 percentage points, standard error 0.087). Within a prefecture over time, lagged house prices do not predict how generous its subsidy is (Wang et al., 2026), although in their 2019 cross-section subsidies are larger where house prices are higher. This evidence is suggestive and

[13]The December 2015 Central Economic Work Conference listed hukou reform under the task of reducing unsold housing (Ministry of Finance, December 22, 2015).

Table 2: Which Traits Predict When a City Adopts

	Coefficient (log hazard ratio) (1)	Change in yearly chance of adopting (2)
Debt over GDP, per 10 points	0.128**	+14%
	(0.064)	
Log GDP per capita	0.266	+30%
	(0.343)	
College share of workers, per 10 points	0.031	+3%
	(0.173)	
Log population	0.380	+46%
	(0.300)	
Log fiscal revenue	0.192	+21%
	(0.293)	
GDP growth, per 10 points	−0.106	−10%
	(0.147)	
House-price growth, per 10 points	0.009	+1%
	(0.087)	
City-years at risk	1,776	
Cities / adoptions	231 / 160	
Year effects	Yes	
Pseudo R^2	0.083	
Traits jointly zero, p	<0.001	

Notes: Discrete-time hazard of first adoption (equation (1)), complementary log-log link: the yearly probability that a city still without a housing or living subsidy adopts one, on its traits in the year before and year effects, 2007–2019. Cities with a subsidy in 2000 are excluded, and a city leaves the sample when it adopts. Column (2) is exp(coefficient) − 1. Standard errors clustered by city in parentheses. The pseudo R^2 includes the year effects; the last row is a Wald test that the seven trait coefficients are jointly zero. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Sources:* Municipal statistical yearbooks, the 2005 one-percent population sample survey, the 2010 population census, and authors' policy coding.

tilts toward the bet on growth.

Comparing changes is credible to the extent that, once the national wave is known, the timing of adoption is as good as random. We test how predictable timing is out of sample. We fit the hazard on 2008–2015 with the five slow-moving traits of Table 2 (all but GDP growth and house-price growth), and in each year of 2016–2019 we rank the cities at risk by their predicted chance of adopting. We measure the ranking by the area under the curve (AUC), the probability that a city that adopted in a given year is ranked above one that did not. An AUC of 0.5 is a coin flip and 1 a perfect ranking. The AUC is 0.63 (95 percent interval 0.53 to 0.71), against 0.55 for debt alone. The fifth of cities with the highest predicted risk adopted at 27.5 per 100 cities a year, against 14.9 on average. The slow-moving traits thus predict later adopters only modestly. Recent shocks (a drop in revenue, a jump in debt, a house-price boom, or neighbors in the province adopting) add nothing, with likelihood-ratio $p = 0.79$ on their own and 0.36 when added to the traits. Our main specifications therefore use the staggered difference-in-differences design of Section 5. As a check, Figures A1 and A2 in the appendix compare adopters only with cities in the same half of predicted risk. The effects on debt and house prices barely change, and the effect on the stock of

knowledge-intensive firms falls from 0.068 to 0.042 but remains significant.

5 Local Effects of Adoption

Relative to comparison cities, adopting cities drew more graduate-degree holders, and knowledge-intensive firm entry, foreign investment, and tax revenue rose. Foreign investment and tax revenue did not rise where the package added research funding without an entrepreneurship program.

5.1 Research Design

Given the data of Section 3, we adopt the dynamic difference-in-differences estimator of de Chaisemartin and D'Haultfœuille (2026). We compare adopting cities with cities that have not yet adopted or never adopt, around the time of adoption. The treatment D_{gt} is the binary indicator of Section 3, equal to one in each period in which the documents of unit g provide a housing or living subsidy and zero otherwise. Let F_g be the first period in which unit g is treated, with $F_g = \infty$ for units that never adopt, and let T be the last period in the data. Each coefficient

$$\mathrm{DID}_\ell = \frac{1}{N_\ell} \sum_{g:\, F_g - 1 + \ell \le T} \left[\left(Y_{g,F_g-1+\ell} - Y_{g,F_g-1} \right) - \frac{1}{N_\ell^g} \sum_{g':\, F_{g'} > F_g - 1 + \ell} \left(Y_{g',F_g-1+\ell} - Y_{g',F_g-1} \right) \right] \tag{2}$$

compares the change in an adopter's outcome Y from the period before adoption, $F_g - 1$, to ℓ periods after, with the change over the same periods in the N_ℓ^g units that had not yet adopted by $F_g - 1 + \ell$ or never would. The estimator averages over the N_ℓ adopters observed at that horizon. It then averages these effects across horizons, weighting each by its number of adopters, and this average is the effect reported in each figure.[14] Each coefficient is a relative effect. It cannot tell whether adopting cities gained at the expense of the comparison cities.

We estimate two specifications, one for each kind of data. In the first, the unit is an origin-destination city pair observed in the four census waves, so event time counts census waves relative to the destination's adoption, from $\ell = -2$ to 2, with $\ell = -1$ as the reference wave. We cluster standard errors by destination city. In the second, the unit is a city observed each year, and event time runs from $\ell = -4$ to 5 years, with standard errors clustered by city. To study heterogeneity by package, we split adopters by the package in the adoption year and re-estimate the same design on each group, with that group's not-yet adopters and the never adopters as controls. The split has two groups, 102 adopters with an entrepreneurship program and 107 others, or three, 52 with the subsidy alone, 55 with research funding, and 102 with an entrepreneurship program.

We use this estimator for three reasons. First, when adoption is staggered and its effect differs across cities or over time, a two-way fixed-effects regression estimates a weighted average of the effects in each city and year, and some of the weights can be negative (de Chaisemartin and

[14] Without lapses, the average is $\sum_{\ell \ge 0} N_\ell \, \mathrm{DID}_\ell / \sum_{\ell \ge 0} N_\ell$, the average total effect per unit of treatment of de Chaisemartin and D'Haultfœuille (2026). Placebo coefficients compare the change from $F_g - 1 - \ell$ to $F_g - 1$ in the same way.

D'Haultfœuille, 2020). Second, the effects are likely to differ across cities. Adoption is partly driven by initial differences across places, since richer and more indebted cities adopted earlier (Section 4). Thus, the same traits may shape the effects of adoption beyond what city and year effects absorb. Third, twenty cities dropped the subsidy during the study period, and ten of them later reinstated it. This estimator allows the effects to differ across cities and over time and accommodates such reversals in policy.

The identifying assumptions for a causal interpretation of these coefficients are parallel trends and no anticipation. Absent adoption, outcomes in adopting cities would have evolved as in cities that had not yet adopted, and outcomes do not respond before the subsidy is in force. There are three main threats to identification. First, unobserved local demand for skilled workers, driven not by adoption but by something else, could rise at the time of adoption and violate parallel trends. We show that flows of migrants without a graduate degree do not change after adoption, but this contrast cannot rule out a skill-biased shock, and the coefficients before adoption give only partial evidence against such a shock. Second, adoption is predicted by fiscal position (Section 4), so fiscal outcomes are the most exposed. Controlling for lagged debt over GDP moves the estimates for foreign investment, tax revenue, output, patents, and house prices by at most 0.023 log points.[15] Third, other policies that also draw skilled workers or raise local demand may have changed at the same time. We address the most prominent, the national hukou reform of 2014 (Huỳnh et al., 2026). We return to each threat after presenting the results.

5.2 Did the Targeted People Come?

We first ask whether the targeted people came. Graduate-degree holders, with a master's degree or more, give the clearest test, because the eligibility requirements target them most directly and education is the only measure of skill in the census. Using the census data of Section 3, we count migrants between every pair of cities in each of the four census waves. For each adopting destination, the estimate compares log in-migration from each origin city before and after adoption with the same flow into destinations that had not yet adopted or never did, as in equation (2). Figure 2 reports the results.

Graduate-degree in-migration rises by more than half, while migration without such a degree does not change. In panel (a) of Figure 2, the average effect after adoption is 0.453 (0.162) log points, about 57 percent more graduate migrants along a city pair than in the comparison destinations. Panel (b) reports the estimates for migrants without a graduate degree. They stay close to zero, at 0.002 (0.046). Since we define a migrant as a person whose city of residence changed over the preceding five years, the estimate misses temporary moves and moves reversed within the past five years.

We observe only one census wave before the reference period, so the test of parallel trends

[15] Debt is reported from 2006, so the control limits these estimates to 2007 onward, and each is compared with the estimate on the same city-years. Letting outcomes trend with each city's debt over GDP in 2009 instead, which keeps every year, moves them by at most 0.035.

rests on a single coefficient. Neither panel shows a significant difference from zero before adoption ($p = 0.30$ for graduates and 0.46 for others). The graduate coefficient two waves before adoption is positive, however, at 0.369 (0.358), so graduate inflows may have dipped just before adoption, and part of the rise may be a recovery.

The rise in high-skill in-migration comes mostly from destinations whose package carried an entrepreneurship program in addition to the housing or living subsidy. This concentration may reflect the combination of a housing subsidy with support for start-ups, which draws more graduate-degree holders to the places that target them. Too few city pairs with graduate migrants enter the other two packages, the subsidy alone or with research funding, to estimate their responses separately. Overall, the targeted people came, while migration without a graduate degree did not rise and, if anything, fell two waves after adoption.

Graduate migrants are rare in the data, and few city pairs carry their flows. Yet graduate degrees are not the only measure of skill. Du et al. (2026) define high-skill workers as the college-educated, including holders of three-year junior college degrees, which captures many more flows, and some packages also target people without a graduate degree. This broader definition of skill is thus another check on the result. We find that college-educated in-migration shows the same pattern, with a smaller but more precise effect of 0.165 (0.046), about 18 percent, and no significant difference before adoption ($p = 0.32$).[16] Migration without a college degree again does not change, at -0.024 (0.047).

The broader measure also allows us to estimate the effects of different combinations in the package, in particular research funding. College in-migration rises by 0.217 (0.081) where packages added research funding and by 0.201 (0.063) where they added an entrepreneurship program, but by only 0.047 (0.068) where they paid the subsidy alone. Therefore, we find consistent evidence that the programs drew skilled workers, whether skill is measured by a graduate or a college degree. For workers without a degree, we find no sign that the programs drew them to the places that offered incentives. One way to interpret this null result is as a placebo, showing that the programs attracted the people they targeted.

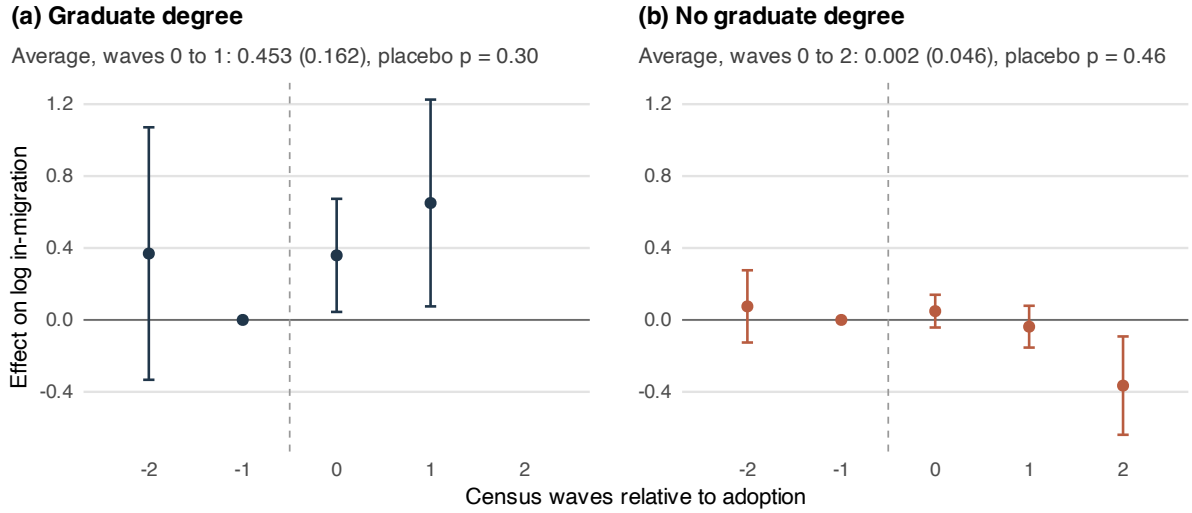
5.3 What Followed: Firms, Foreign Investment, Tax Revenue, Output, and Patents

Talent incentive packages aim not only to bring people but also to shape what those people do once they arrive. Among the potential economic outcomes, one key measure is business activity. Bringing more skilled people may bring more firms, either because skilled workers make firms more productive, which attracts firms and helps them survive, or because the newcomers build firms themselves. In this subsection, we estimate the effects of adoption on business registrations across all 285 cities, and we ask in which industries the programs raised firm entry most.

We separate sectors into two groups. The first is knowledge-intensive sectors, which Eurostat

[16] Hukou liberalization accelerated nationally after 2014, overlapping the fastest phase of adoption. Dropping the 66 destination cities first treated during the 2014 to 2017 reform window, where the two policies are hardest to tell apart, raises the college-educated estimate to 0.227 (0.069).

Figure 2: In-Migration with and without a Graduate Degree



Notes: Event-study estimates of the effect of adoption on log city-pair in-migration of migrants with a graduate degree, master's or higher (a), and without one (b), census waves 2000–2015, dropping pairs with no migrants. Estimator of de Chaisemartin and D'Haultfœuille (2026), with not-yet-adopting and never-adopting destinations as controls. Event time counts waves, with wave -1 as the reference. Panel (a) has no estimate for wave 2, and wave 2 in (b) rests only on destinations first treated in 2005. Subtitles give the average effect after adoption (standard error) and the placebo p-value. Bars are 95 percent confidence intervals, clustered by destination. *Sources:* Population censuses (2000, 2010), one-percent population sample surveys (2005, 2015), and authors' policy coding.

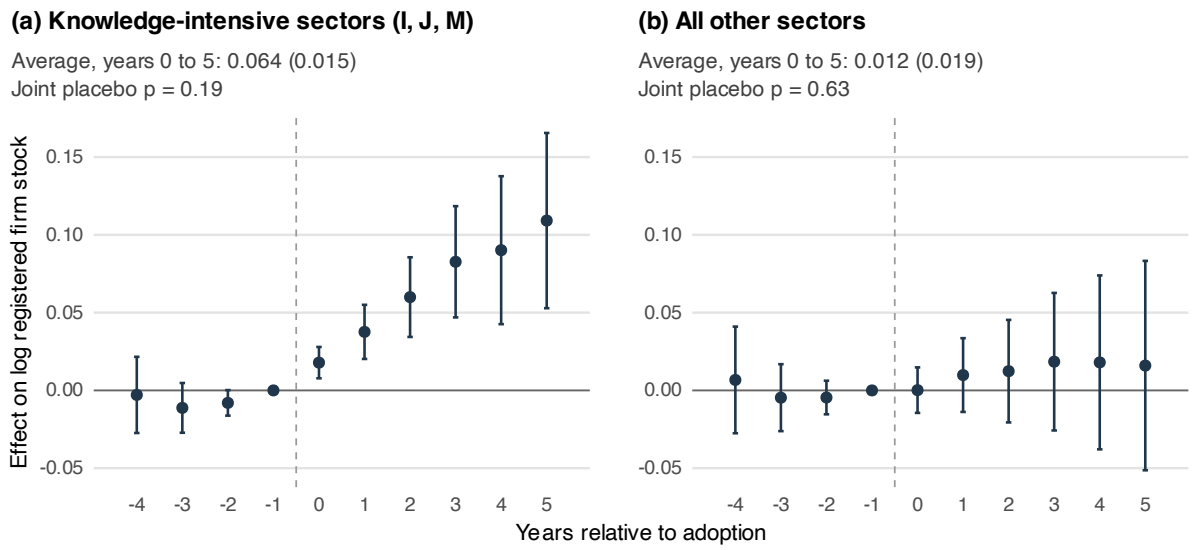
defines by the share of tertiary-educated workers in the workforce. Without education data by sector for China, we proxy for them with sections I (information transmission, software, and information technology services), J (finance), and M (scientific research and technical services) of the national industry classification.[17] These sections accounted for 6 percent of new registrations in 2005 to 2009. All other sectors form the second group.

Figure 3 reports the log stock of registered firms for knowledge-intensive sectors in panel (a) and all other sectors in panel (b). Before adoption, the stock of knowledge-intensive firms does not grow differently in adopting cities than in comparison cities ($p = 0.19$). After adoption, it rises significantly and builds over time, by 0.064 log points (0.015) on average over years 0 to 5, about 7 percent, and by 0.11 log points five years after adoption. The stock in other sectors does not change, at 0.012 (0.019). The entry rate, new registrations over the previous year's registered stock, shows the same pattern. It rises by 1.604 percentage points (0.491) on a mean of 21.8 percent in knowledge-intensive sectors, also about 7 percent, and does not rise significantly elsewhere, at 0.659 (0.542).

In other words, firm entry rises in knowledge-intensive sectors, either because firms there use skilled workers more heavily or because skilled newcomers start firms there themselves. Using the

[17] Eurostat's knowledge-intensive services also include education, health, and public administration, which are largely public in China, and legal, accounting, and consulting services, which the Chinese classification groups with leasing in section L.

Figure 3: Registered Firms by Sector Knowledge Intensity



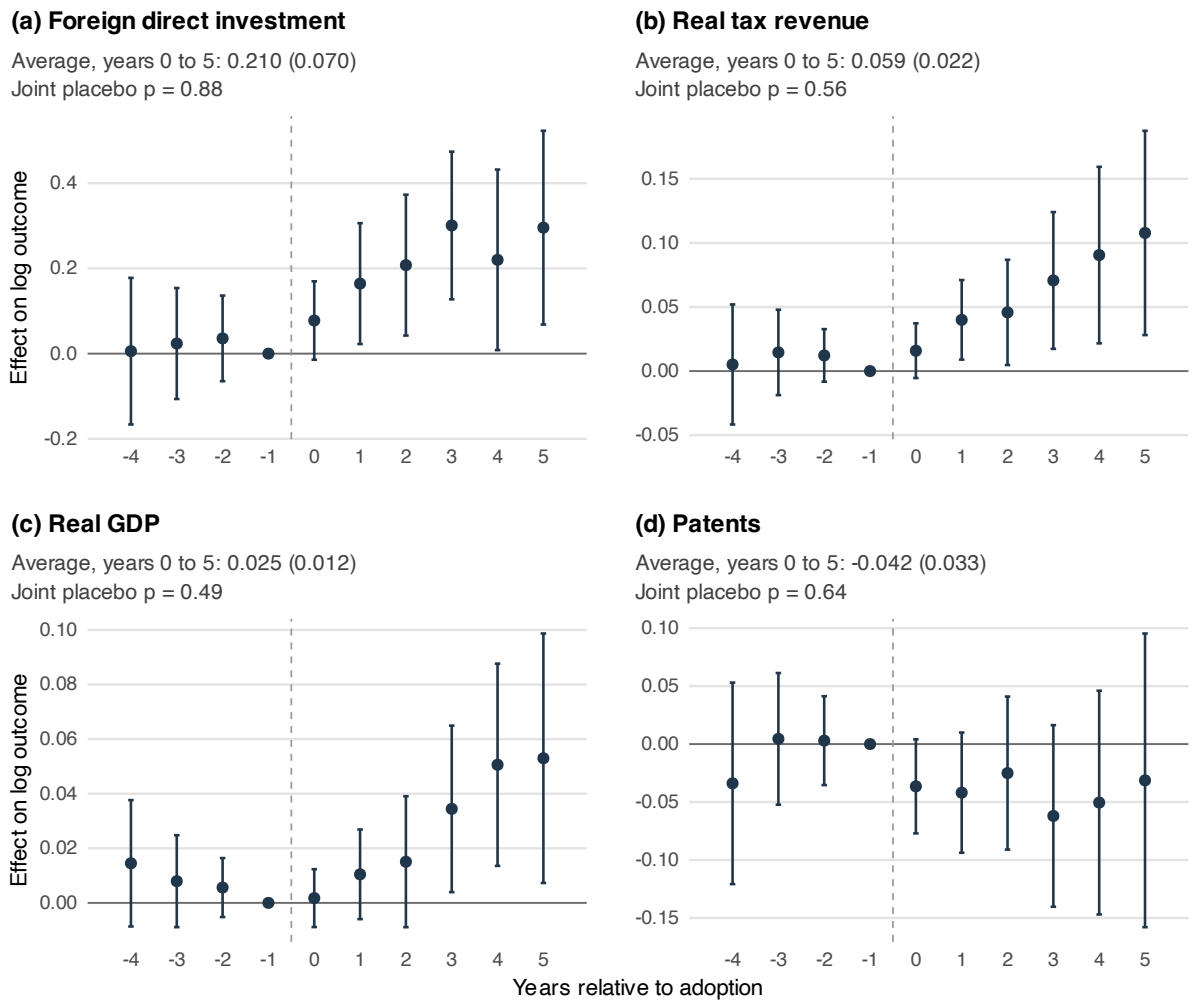
Notes: Event-study estimates of the effect of adoption on the log stock of registered firms in knowledge-intensive sectors (sections I, J, and M of GB/T 4754-2011) (a) and in all other sectors (b), 285 cities, 2001–2019. Estimated as in Figure 2, with event time in years and standard errors clustered by city. Subtitles give the average effect over years 0 to 5 (standard error) and the p-value of the joint test that the three placebo coefficients are zero. *Source:* Business registration records.

household survey, Du et al. (2026) also find a rise in entrepreneurship by the skill of the entrepreneur. The number of high-skill business owners, counting entrepreneurs together with the self-employed, rises by 0.264 log points (0.118), while the low-skill estimate is small and imprecise, at 0.066 (0.070). Together, the registry and the survey show more formal firm entry in knowledge-intensive sectors and more business ownership among skilled workers. Overall, skill-intensive firms increased in adopting cities.

Beyond where workers and new firms located, did adoption bring investment, revenue, output, and innovation? Figure 4 reports four outcomes for all adopters, estimated with the estimator of de Chaisemartin and D'Haultfœuille (2026): foreign direct investment and tax revenue, which measure whether the city gained activity and a tax base, real GDP, which measures local production, and patents, which measure innovation. For each outcome, we test whether the three coefficients before adoption are jointly zero. The test does not reject at the 5 percent level for any of the four outcomes (the smallest p-value is 0.49), so adopting and comparison cities followed parallel paths before adoption.

Across all adopters, foreign investment rises by 0.210 log points (0.070) and tax revenue by 0.059 (0.022) (panels (a) and (b)). Over the six years after adoption, foreign investment is thus about 23 percent higher and tax revenue about 6 percent higher than in comparison cities. For a program that pays people rather than firms, the response of foreign investment is large. It is close to what Wang (2013) finds for a special economic zone, which raises a city's foreign investment by about 0.22 log points when the zone opens and by more over time.

Figure 4: Foreign Investment, Tax Revenue, Output, and Patents after Adoption



Notes: Log foreign direct investment in US dollars (a), log real tax revenue, 2005–2019 (b), log real GDP (c), and log patents, dropping city-years with none (d), for all 209 adopters. Tax revenue and GDP are deflated by the city consumer price index. Estimated and reported as in Figure 3. *Sources:* Municipal statistical yearbooks and city-by-industry patent counts from the China National Intellectual Property Administration.

Real GDP, by contrast, rises modestly and slowly (panel (c)). The average effect is 0.025 log points (0.012), about 2.5 percent. The rise becomes significant only three years after adoption and reaches about 5 percent by year five. This is far below the roughly 20 percent rise in GDP that Alder et al. (2016) find after a city opens a major special economic zone. Productivity, measured by total factor productivity, does not change, and patents do not rise (panel (d)). If anything, patents decline, by about 4 percent (0.042 log points, 0.033), although not significantly, which suggests that innovative activity may give way to investment and production.

Taken together, adopting cities gained foreign investment, a tax base, and some output. These patterns suggest that adoption changed what locates in these cities rather than how productive it is. One interpretation is selection in the kind of workers the programs draw. The newcomers may be

skilled workers whom foreign investors and knowledge-intensive firms need to hire, which would raise foreign investment, firm entry, and tax revenue. They need not be the inventors or the most productive workers, who would raise patenting or measured productivity. An alternative view fits the same patterns without selection among workers. Because our estimates are relative, part of the gain in adopting cities may be investment and firms that would otherwise have located in comparison cities. Activity that moves between cities raises investment, firms, and tax revenue where it lands, but it need not raise patenting or productivity. Next, we turn to the components of the packages, to see how much the additional components, an entrepreneurship program or research funding, drive these results.

5.4 Responses by Package Content

We now split adopters by the package in the adoption year, on top of the housing or living subsidy that every adopter offers: the subsidy alone (52 adopters), the subsidy with research funding (55), and the subsidy with an entrepreneurship program, with or without research funding (102). We also compare adopters with and without an entrepreneurship program, which pools the first two packages. With this heterogeneity, we explore the role of each component in the results so far. For migration, Section 5.2 already showed that skilled in-migration rises with an entrepreneurship program or research funding but not with the subsidy alone.

Next, Figure 5 reports foreign investment and real tax revenue for each of the three packages. With an entrepreneurship program, foreign investment rises significantly, by 0.277 log points (0.081), about a third, and the rise is already 0.13 (0.05) in the adoption year. Without one, it shows no persistent increase, at 0.122 (0.100) (panel (a)). Within the latter group, foreign investment rises by an insignificant 0.163 (0.114) with the subsidy alone and does not rise with research funding, at -0.023 (0.136).

Tax revenue follows the same split (panel (b)). It rises by 0.078 log points (0.029) with an entrepreneurship program, and the rise builds from 0.02 in the adoption year to 0.12 five years later, a horizon that 49 of the 82 cities in the estimate reach. Without a program, it rises by a smaller and insignificant 0.044 (0.029). Within the group without a program, tax revenue rises with the subsidy alone, by 0.076 (0.036), but not with research funding, at -0.008 (0.043).

The stock of knowledge-intensive firms also grows more with an entrepreneurship program, by 0.089 log points (0.022), than with the subsidy alone, 0.049 (0.026), or with research funding, 0.055 (0.024). In percent, the stock is about 9 percent larger with an entrepreneurship program, against about 5 percent with the subsidy alone and 6 percent with research funding. The gap is sizable: the stock of knowledge-intensive firms in never-adopting cities grows by about 9 percent a year, so an entrepreneurship program adds about one year of growth, and the other packages about half a year. In other words, graduate migration, foreign investment, and tax revenue all rise significantly when the package carries an entrepreneurship program. One reading is a chain: the program draws graduates, their skills attract foreign investment, and the added activity raises tax collection. Part of this chain may run through firm entry into knowledge-intensive activities.

In contrast, output and patents do not rise much in any group. Real GDP rises slightly but not significantly, by 0.032 (0.017) with an entrepreneurship program and 0.020 (0.014) without. Patenting does not rise in any group, even where the package funded research, at -0.052 (0.057) with research funding, and total factor productivity does not move either.

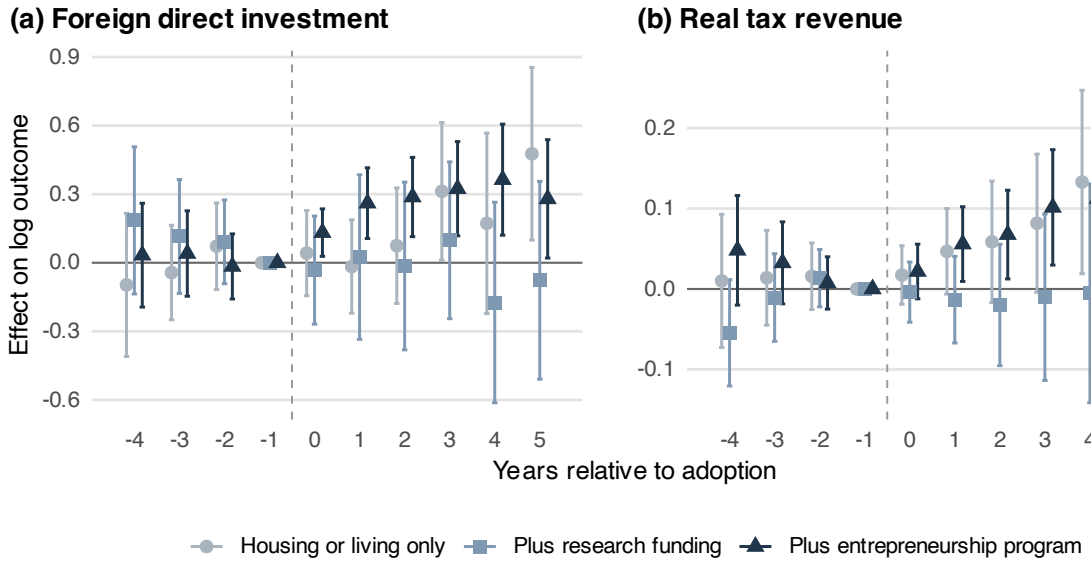
Why does output barely respond when investment and tax revenue rise? Foreign investment can rise a lot while output moves little, because it is a small part of a city's economy. Foreign investment is about 1 percent of GDP in the median city, so a rise of about a third adds only about 0.3 percent of GDP. Likewise, knowledge-intensive sectors accounted for only 6 percent of new registrations in 2005 to 2009, so faster growth in these sectors moves aggregate output little. Some of the new activity may also be processing for sale to other places, which adds investment and registered firms but little local value added.

Patents, in turn, may not be the right measure of what the newcomers do. The new firms are in software, finance, and research services, which patent little, while patents come mostly from incumbent manufacturers and universities. A flat patent count is what we would expect even if the newcomers raised activity in these sectors.

Overall, these results suggest that the programs draw skilled people and that, with an entrepreneurship program, they also raise firm entry, foreign investment, and taxable activity. Without such a program, newcomers may work in universities or research institutes, which generate less output and little taxable activity and are not the manufacturing activity that foreign investment seeks. What comes with the cash matters. Research funding brings people but not investment or tax revenue, the subsidy alone brings tax revenue but not people, and an entrepreneurship program moves people and money together. The programs, especially those with an entrepreneurship program, change the kind of activity that locates in the city. The new activity is more formal, organized in registered firms, more linked to foreign investors, more taxable, and more concentrated in knowledge services. The programs do not raise how much the city produces in total or how much it invents.

These readings should be taken with care, for three reasons. First, the gaps between packages are not significant. The closest is foreign investment, where the gap between an entrepreneurship program and research funding is close to significant at the 5 percent level ($p = 0.06$), followed by tax revenue, where the same gap has $p = 0.10$. What the estimates show is that the responses are significant mostly where the package carried an entrepreneurship program. Second, cities chose their packages. Each package's estimate compares its own adopters with not-yet-adopting and never-adopting cities, and it is valid if those adopters would have followed the same paths as their comparison cities. The gap between two packages, however, compares different cities, so it combines the effect of the added component with differences between the cities that chose it. Research-funding adopters were richer, with a median 2010 GDP per capita of about RMB 33,000 against RMB 25,000, and adopted about a year earlier, so they may have had less to gain from the same package, and their estimates come from different years. Observed traits explain only about 12 percent of the variation in what a package contains, and debt predicts none of it, but

Figure 5: Foreign Investment and Tax Revenue by Package



Notes: Log foreign direct investment (a) and log real tax revenue (b), by the package in the adoption year, as in panel (a) of Figure 1. Each package is estimated on its own adopters, with not-yet-adopting and never-adopting cities as controls, and otherwise as in Figure 4. Averages are in the text. No placebo test rejects ($p \geq 0.17$). *Source:* Municipal statistical yearbooks.

differences we do not observe, such as what each city expected to gain, could shape both its choice and its response. Third, as noted in Section 5.3, the estimates are relative, so part of the gains may have moved from comparison cities. How much is reallocation rather than creation, how worker selection changes aggregate productivity, and how far spillovers reach require a quantitative model, which Du et al. (2026) develop.

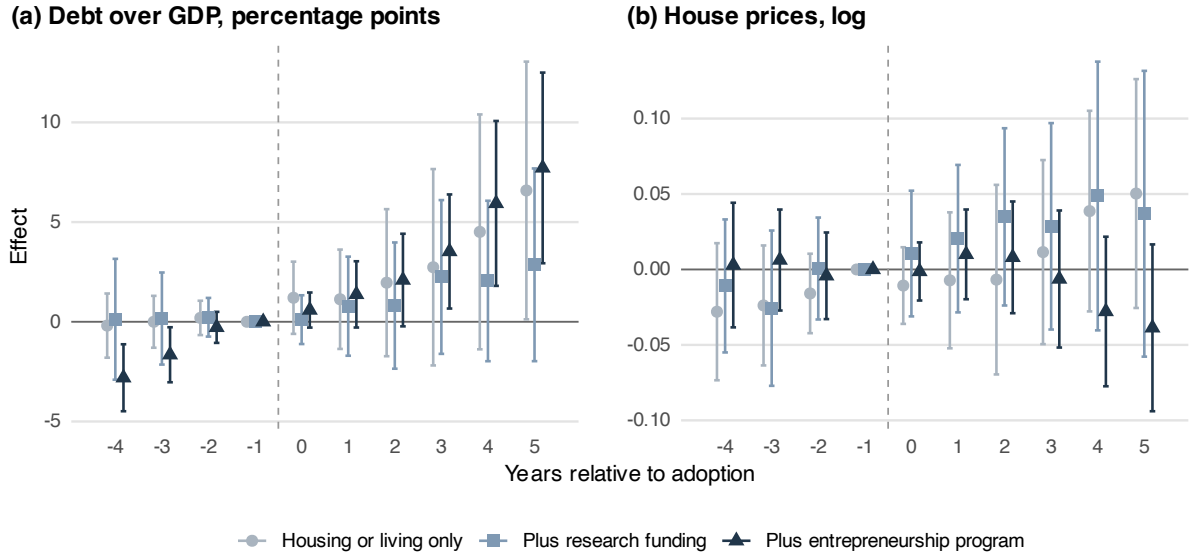
So far, the treatment is binary. The package groups pay about the same for housing and living, with a median subsidy intensity of 0.84 with the subsidy alone, 1.03 with research funding, and 0.90 with an entrepreneurship program, so the packages differ in what comes with the cash more than in the cash itself.[18] Paying for these packages also costs the city, which raises the question of who pays.

5.5 A First Look at the Costs: Local Debt and House Prices

So far, we have looked at the potential gains of the places that adopt such policies. The costs can show up in several ways. The first is the local government budget. Given the modest number of migrants relative to the size of Chinese cities, the programs are unlikely to move total budgets

[18]Subsidy intensity values monthly payments over their coded duration, ten years at 2.5 percent when no duration is coded, and fills missing amounts from other years of the same city. Between 43 and 56 percent of each group lies above the overall median. Intensity also varies with how long payments last and, at the top tier, with research funding, where packages with research funding pay 2.69 against 1.52 without (recorded for 132 of 209 adopters). Du et al. (2026) examine how the effects vary with intensity and use it to study the cost of the programs in a quantitative model.

Figure 6: Local Debt and House Prices after Adoption



Notes: Local government debt in percentage points of GDP, 2006–2019, with reported zeros treated as missing (a), and log nominal residential house prices per square meter, 2003–2019 (b), by package, estimated as in Figure 5. The debt estimates rest on 51, 54, and 100 adopters, and the house-price estimates on 50, 54, and 102. The placebo test rejects only for debt with an entrepreneurship program ($p = 0.007$; elsewhere $p \geq 0.48$). *Source:* Municipal statistical yearbooks.

much. Indeed, total real revenue does not change significantly after adoption, at 0.032 log points (0.019), and in Du et al. (2026) the only budget line that responds is social security and employment spending, which rises by 0.032 log points (0.016) and also carries pensions and social assistance. The program bill that the companion model infers from this rise is about 0.4 percent of expenditure in subsidizing cities. In this subsection, we look at two other ways the costs can show up, through local debt and through house prices.

Panel (a) of Figure 6 reports local government debt in percentage points of GDP, the usual gauge of its burden. In the year before adoption, the ratio averages 12.8 percent among adopters. One caveat is that we do not observe take-up, how many people receive the subsidy, so we cannot tie the path of debt to the program's bill. With the subsidy alone, debt rises steadily after adoption, from 1.2 points of GDP in the adoption year to 6.6 points (3.3) five years later, an average of 2.8 points (1.8), about a fifth of the pre-adoption ratio, with no rise before adoption ($p = 0.94$). With research funding, it rises by a smaller and insignificant 1.3 points (1.3) ($p = 0.89$). With an entrepreneurship program, debt rises significantly, by 3.1 points (1.1), but it was already rising before adoption, and the coefficients before adoption are jointly significant ($p = 0.007$).[19] There, the rise after adoption continues the fiscal path that also predicted adoption (Section 4), and we do not read it as the cost

[19]With an entrepreneurship program, debt four years before adoption stood 2.8 points of GDP (0.9) below its level in the year before adoption. A few highly indebted cities do not drive the ratio: winsorizing it at the 1st and 99th percentiles gives 2.7, 1.3, and 3.0 points for the three packages. Log debt and log debt per capita rise by less, but too imprecisely to confirm or rule out the rise in the ratio. For all adopters, they move by 0.031 (0.076) and 0.002 (0.076), against about 0.16 implied by the 2.2-point rise in the ratio. With the subsidy alone, log debt also rises before adoption ($p = 0.03$).

of the program. These patterns suggest that the programs come with people and activity but may also come with a fiscal burden, or with cities taking on more risk by borrowing against the growth they expect.

On the other hand, the costs of in-migration can also show up in housing. Given the size of the inflows, we would not expect much pressure on prices or congestion. But the subsidy goes to skilled newcomers, so any rise in housing costs would fall on incumbent buyers and renters, and it is not clear in advance how housing costs would change. Whether the subsidy reaches the migrant or the landlord depends on housing supply. If house prices rose with adoption, part of the transfer would be capitalized into land values and shift toward incumbent owners. Panel (b) of Figure 6 reports log house prices separately by package. Regardless of the package, there is no clear price response: prices move by 0.010 (0.023) with the subsidy alone, 0.029 (0.029) with research funding, and -0.007 (0.017) with an entrepreneurship program. For all adopters, the average effect is 0.006 (0.014), so the 95 percent interval for the price level runs from about -2.0 to 3.4 percent.[20] Busso et al. (2013b) find a similar pattern for the US Empowerment Zones, which raised local employment and wages without large increases in rents, so that local workers rather than landowners captured most of the gains. By contrast, Zhang et al. (2023) report that talent settlement policies raised prices by about 26 percent across 70 large and medium-sized Chinese cities over 2013 to 2019, a sample of larger cities and a different definition of treatment from ours.

These results concern average prices per square meter only, not rents or the range of prices. In China, local governments also manage prices. Cities that depended on land sales and land-backed debt restricted supply to hold prices up during 2020 to 2022 (Chang et al., 2026), the same fiscal position that predicts adoption. Without a measure of quantities, such as the housing stock, floor space, or vacancies, we cannot tell whether housing adjusted through quantities rather than prices.

6 Conclusion

Over two decades, Chinese cities have offered skilled people incentives to relocate, and their experience offers several lessons for place-based policies aimed at talent. The talent incentive programs attracted the people they targeted. Graduate-degree holders moved to the cities that paid and knowledge-intensive firms followed, but output rose by much less and patenting did not rise at all, suggesting that the programs changed the mix of local activity more than its size. Foreign investment and tax revenue rose most where the package included an entrepreneurship program. Cities with more debt were more likely to adopt, 79 percent of policy documents set no end date, and 54 percent extend benefits to workers already living in the city.

Two related studies help interpret these lessons. Ma et al. (2026) follow the careers of academic economists in China and find that housing and living subsidies influence where they take jobs, whereas research funds and career support do not. The programs draw economists from lagging regions to developed ones and among developed regions, but not from developed regions to

[20] The interval for log prices is $[-0.020, 0.033]$. Prices deflated by the city consumer price index move by 0.007 (0.014).

lagging ones. Read with our results, this suggests that the subsidy is what brings people, while the rest of the package affects what happens after they arrive. It also suggests that lagging cities will struggle to attract skilled workers away from richer ones, and may do better with their own graduates who left. Du et al. (2025) find that programs paying locally born graduates to return raised productivity and wages in their home cities, with the largest gains where social capital is high. Their accounting exercise suggests that these gains are not fully offset by losses elsewhere.

The package lesson also points to a design choice. A subsidy for moving, here for housing or living costs, may not be enough on its own, echoing Fajgelbaum and Gaubert (2025), who argue that housing policy alone cannot correct the externalities that place-based policy targets. Our ongoing work points to one reason. A subsidy that rewards moving makes wage work more attractive than starting a firm, and so can pull skilled people out of entrepreneurship (Du et al., 2026). Support for new firms may offset this, consistent with the larger rise in knowledge-intensive firms where packages included an entrepreneurship program.

What these local estimates leave open is whether the programs pay off for the country as a whole. Public money that could provide services to a large population goes instead to a smaller group. Because many cities compete for the same people, one city's gain may be another's loss. Answering this requires a framework in which migration, trade, and firm entry respond together across cities, which our ongoing work develops. In the meantime, cities can make their programs easier to judge by recording who receives the subsidy and how many, capping spending, planning how to finance it over time, and preventing people from moving between cities only to collect it, for example by paying in installments that stop if the recipient leaves.

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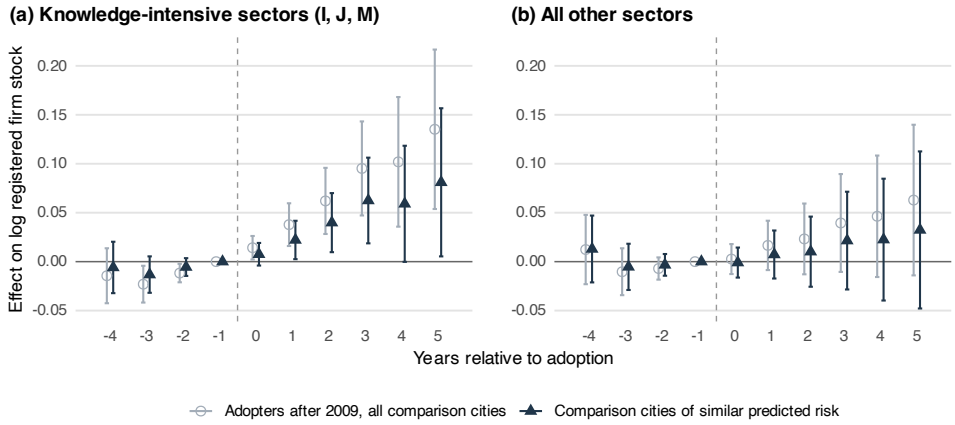
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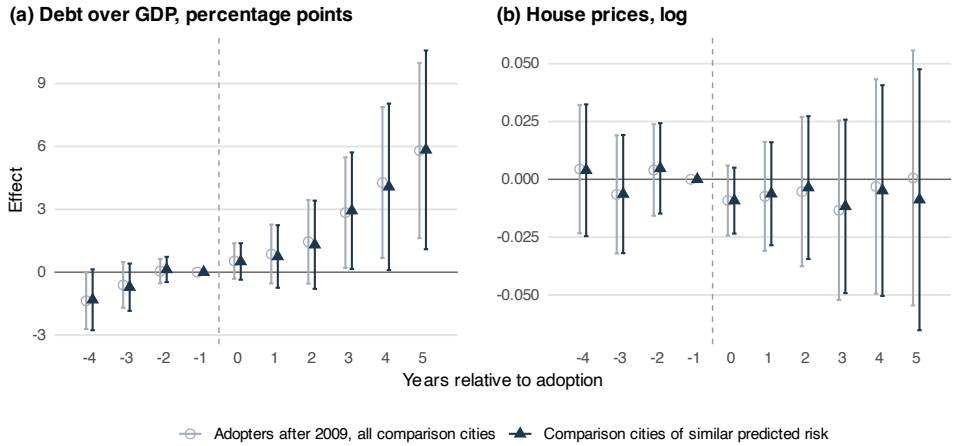
A Additional Figures

Figure A1: Registered Firms, Comparing Cities of Similar Predicted Risk



Notes: Log stock of registered firms in knowledge-intensive sectors (a) and in all other sectors (b), estimated as in Figure 3 on the 189 prefectures with no subsidy by 2009 and all five traits observed. Predicted risk is each city's index from the hazard of Table 2 with the five slow-moving traits (debt over GDP, log GDP per capita, college share of workers, log population, log fiscal revenue) at their 2008–2009 levels, split at the median. Average effects over years 0 to 5, all comparison cities against similar risk: 0.068 (0.019) against 0.042 (0.017) in (a), placebo $p = 0.06$ and 0.46; 0.029 (0.020) against 0.014 (0.019) in (b), $p = 0.20$ and 0.46. *Sources:* Business registration records, municipal statistical yearbooks, and authors' policy coding.

Figure A2: Local Debt and House Prices, Comparing Cities of Similar Predicted Risk



Notes: Local government debt in percentage points of GDP (a) and log house prices (b), defined as in Figure 6 but pooling all adopters. Sample, predicted risk, and comparisons as in Figure A1. Average effects over years 0 to 5, all comparison cities against similar risk: debt 2.3 (1.0) against 2.2 (1.1) points, placebo $p = 0.10$ in both (a); house prices -0.007 (0.015) against -0.008 (0.014), $p = 0.68$ and 0.67 (b). *Source:* Municipal statistical yearbooks and authors' policy coding.