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The local economic and innovation spillover effects of mega firm subsidiaries

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Abstract

This article studies the local economic and innovation spillovers from subsidiaries of domestically large firms in China. I construct a geo-referenced dataset linking subsidiaries of publicly listed firms (PLFSs) to the universe of patent applications and nighttime lights (NTL), and iteratively select newly established PLFSs in areas without pre-existing nearby listed-firm subsidiaries. Using a staggered difference-in-differences design that compares earlier- and later-entry locations, I estimate the local effect of a single PLFS at a granular spatial scale. PLFS entry increases local economic activity and innovation, raising NTL by 26.4 per cent and local patenting by 95 per cent within 2.5 km. Firm-level decomposition shows both incumbent innovation gains and more innovative entry, suggesting localized innovation growth rather than pure relocation. Further inventor-level analysis provides evidence of creative destruction, as local innovation paths shift, with pre-existing inventors being replaced by new ones. These findings highlight the role of mega-firm subsidiaries in driving local economies and inform the design of firm-specific place-based policies.

Keywords: **Mega firms; place-based policy; Innovation spillovers; Regional development.**

1. Introduction

Over the past few decades, large businesses, or *mega firms*, have increasingly dominated labor markets, supply chains, market shares, and technological progress (Autor et al., 2020b; De Loecker et al., 2020; Yeh et al., 2022; Braguinsky et al., 2023; Hsieh and Rossi-Hansberg 2023; Kwon et al., 2024). This growing influence has driven governments at all levels to compete for subsidiaries of these firms through targeted subsidies, tax incentives, and tailored infrastructure investments. Recent examples include the United States' support for Taiwan Semiconductor Manufacturing Company's investment in Arizona and China's support for Tesla's factory in Shanghai.[1]

1. The United States offered up to $6.6 billion in subsidies and $5 billion in potential loans to attract TSMC under the CHIPS Act. The Shanghai government granted Tesla a reduced corporate income tax rate of 15% for 2019–2023, compared with China's standard 25% rate.

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This strategy can be viewed as a firm-specific form of place-based policy. Rather than broadly lowering entry costs for many firms, as in conventional place-based policies, governments concentrate fiscal and administrative resources on a small number of mega firms expected to stimulate nearby economic activity through subsidiary entry. Yet whether such entry actually raises local economic activity and innovation remains an open empirical question, with systematic evidence still limited.

Theoretically, the sign of this local effect is ambiguous. On the one hand, mega-firm subsidiaries may "grease the wheels" of local development (Murphy, Shleifer, and Vishny 1989) by raising labor demand, attracting suppliers and customers, diffusing knowledge, and generating demonstration effects. Competitive pressure may also induce nearby firms to innovate in order to escape competition (Aghion et al., 2005). On the other hand, these effects may be limited where complementary production factors and human capital are scarce. Entry by a large subsidiary may also crowd out incumbent firms or reduce innovation incentives by lowering profit margins, as emphasized in Schumpeterian models of competition and innovation (Dasgupta and Stiglitz 1980). The sign and magnitude of the net local effect are therefore empirical questions.

The key empirical challenge is that the location choice of mega-firm subsidiaries is endogenous to local fundamentals and policy support. Tax incentives, infrastructure investments, regulatory support, and other place-specific advantages may lower entry costs not only for the mega-firm subsidiary but also for other firms, workers, and innovative activity. Local growth around the subsidiary may therefore combine subsidiary-induced spillovers with the direct effects of the broader policy bundle. Related evidence comes mainly from inward foreign direct investment (FDI) studies, which exploit spatial or sectoral variation in transport costs or policy incentives, but the findings are mixed and often confounded by broader international-policy changes (Cheung and Ping 2004; Lu, Tao, and Zhu 2017; Bao and Chen 2018; Hou et al., 2021; Ding et al., 2024).[2] Moreover, FDI evidence may not generalize to subsidiaries of domestic mega firms, where institutional context and selection forces differ.

I exploit geo-referenced firm-level microdata on the universe of subsidiaries of publicly listed firms (PLFSs) in China's A-share market to identify the local effects of mega-firm subsidiary entry at a highly granular spatial scale. A-share listing provides a transparent and policy-relevant scope for domestically large firms. Using subsidiary addresses, I retain newly established PLFSs whose 2.5 km buffers do not overlap with those of earlier PLFSs. I then compare economic activity and innovation within these 2.5 km neighborhoods before and after entry using a staggered difference-in-differences design (staggered DID), with later-entry locations serving as not-yet-treated controls for earlier-entry locations. This setting has two advantages. First, the nationwide universe of PLFSs and the non-overlap design allow me to estimate the average local effect of a single mega-firm subsidiary, rather than case studies of large-firm entry (Basker 2005; Ellickson and Grieco 2013; Iacovone et al., 2015; Hu et al., 2024). Second, the firm-level timing variation and highly granular outcomes help isolate subsidiary impacts from time-invariant local attributes and broad policy conditions that are common within the same administrative area.

I measure local economic activity by the sum of nighttime lights (NTL) within 2.5 km (Besley and Reynal-Querol 2014; Michalopoulos and Papaioannou 2014; Henderson et al., 2018). I measure local innovation using geocoded patent applications within the same radius, excluding patents filed by the PLFS itself (Guadalupe, Kuzmina, and Thomas 2012; Arkolakis et al., 2018; Autor et al., 2020a). Event-study estimates show no systematic pre-trends, and the staggered DID estimates imply a 26.4 per cent increase in NTL and a 95 per cent increase in local innovation over nine post-entry years. The results are robust to alternative specifications, radii, patent definitions, and sample restrictions.

I then examine how local innovation responds along firm and inventor dimensions. At the firm level, PLFS entry raises patenting among incumbent firms and draws in innovative new firms. The incumbent

2. Positive spillover effects are documented in Haskel et al. (2007), Keller and Yeaple (2009), and Cheung and Ping (2004); negative effects in Haddad and Harrison (1993), Aitken and Harrison (1999), and Lu et al. (2017).

response is difficult to reconcile with a pure crowding-out story and is consistent with competition-induced innovation (Aghion et al., 2005; Igami 2017). Together with the arrival of more innovative entrants, this pattern suggests that the baseline gains reflect localized innovation growth, not merely the within-city relocation of activity toward PLFS locations. At the inventor level, changes in the composition of patenting inventors proxy for shifts in the direction of local innovation. Pre-existing inventors capture established local trajectories, while newly appearing inventors proxy for new technological directions. Patenting shifts away from the former and toward the latter after PLFS entry, pointing to a local creative-destruction mechanism in innovation (Acemoglu and Cao 2015; Kogan et al., 2017). The fine spatial scale enables name-based tracking of inventor mobility, which shows little evidence of inventor inflows and points instead to demonstration effects and supply-chain pass-through.

This article contributes to three related strands of the literature. First, it extends the literature on superstar firms, large establishments, and local spillovers to domestic mega-firm subsidiaries in a developing-country setting. Existing work examines supplier productivity gains from superstar-firm relationships (Alfaro-Urena, Manelici, and Vasquez 2022; Amiti et al., 2024), local knowledge diffusion and inventor mobility (Matray 2021), and the productivity effects of large plants (Greenstone, Homebeck, and Moretti 2010; Bloom et al., 2019). I show that sizable spillovers also arise around within-country subsidiaries of domestic mega firms, with stronger effects in lower-human-capital regions, speaking to the external validity of large-firm spillovers in less developed local economies.

Second, this article speaks to firm-specific place-based policy. Prior evidence shows that bundled interventions such as infrastructure, subsidies, tax incentives, and special zones can improve local outcomes (Bronzini and De Blasio 2006; Kline and Moretti 2014; Ehrlich and Seidel 2018; Criscuolo et al., 2019), but their effects are difficult to attribute to any single component (Neumark and Simpson 2015). By isolating the entry of a single mega-firm subsidiary within narrowly defined, non-overlapping neighborhoods, the design provides a building block for understanding whether concentrating policy resources on a specific large firm can generate local economic and innovation gains. The documented gains among incumbent firms further mitigate concerns that place-based policies operate purely through displacement effects or deadweight loss (Bronzini and De Blasio 2006).

Third, matched patent, firm, and inventor data allow me to examine how local innovation reorganizes after entry. I separate incumbent innovation from innovative entry, assess inventor mobility through name-based tracking, and use changes in inventor composition to proxy for shifts in the direction of local innovation. The inventor-level evidence points to a creative-destruction pattern and suggests that demonstration effects and supply-chain pass-through are more plausible mechanisms (Chu, Tian, and Wang 2019).

The article proceeds as follows. Section 2 describes the data and identification strategy. Section 3 presents the main results. Section 4 examines mechanisms, while Section 5 reports further analyses. Finally, Section 6 concludes and discusses policy implications.

2. Data and identification strategy

2.1 Data

I focus on parent firms listed on China's A-share market as a transparent and consistently observable proxy for domestically large (mega) firms, and use the universe of their subsidiaries as the primary data-set for analysis. Definitions of "mega" or "superstar" firms vary across studies.[3] In the Chinese context, the A-share listed universe provides a practical proxy for domestically large firms under a common disclosure and regulatory framework. In robustness checks, I further tighten the definition by restricting the parent-firm universe to larger A-share firms, such as the top quartile by size within each industry-year.

3. For example, Autor et al. (2020b) focus on the largest firms (e.g., the top 500 among publicly traded U.S. firms), while Braguinsky et al. (2023) define mega firms as the top 50 firms by sales each year among publicly listed firms.

Because some prominent Chinese firms are unlisted or listed offshore, the estimates should be interpreted as spillovers from subsidiaries of domestically listed large firms. We discuss this data-coverage limitation in the identification section.

The core dataset consists of the full list of subsidiaries of PLFSs from 1999 to 2020, sourced from the annual reports in the Chinese Research Data Services Platform (CNRDS). I clean subsidiary names and link them to firm registration records from Qichacha, then geocode subsidiary addresses using the BaiduMap API following Li (2023).[4] Supplementary Appendix A.1 describes the PLFS construction procedure, and Supplementary Appendix A.2 reports summary statistics. Registration records indicate that 67 per cent of PLFS establishments in our sample are greenfield entries rather than acquisitions. This composition supports interpreting PLFS establishments as local entry events in the spirit of the "large plant opening" setting in Greenstone, Hornbeck, and Moretti (2010). CNRDS also provides firm-level characteristics including industry, ownership, sales, employment, and capital assets, which are used as controls.

I proxy local economic activity using nighttime lights. Specifically, I use the harmonized DMSP-OLS-like panel for 1992–2023 from Wu et al. (2021), which extends the DMSP-OLS series beyond 2013 by calibrating VIIRS observations to a comparable scale. For each PLFS location, local economic activity is measured as the sum of digital-number luminosity within a j-km buffer around the subsidiary.

Local innovation is measured using the universe of Chinese patent applications from CNIPA.[5] The data report application dates, IPC classes, patent types, and applicant and inventor information (Dang and Motohashi 2015). I geo-code patents using assignee addresses and count annual applications within the same j-km buffer around each PLFS. I link patent assignees to firm registration records and define local innovation as total patenting within the buffer net of patents filed by the PLFS itself:

$$Innovation_{it} = P^{j}_{it} - P^{PLFS}_{it},$$

where P^{j}_{it} is the total patenting within j km of PLFS i in year t and P^{PLFS}_{it} is patenting by the PLFS itself.

I also use the patent data to study firm entry margins, technological proximity, inventor composition, and inventor mobility in later sections. Auxiliary GIS layers on Special Economic Zones and road networks are used for zoning exclusions and infrastructure controls, with details reported in Supplementary Appendix A.4.

2.2 Identification strategy

The empirical challenge is to isolate the local effect of a new PLFS from pre-existing local advantages and broader policy bundles that may also attract other economic activity. I address this challenge by combining a fine-scale spatial design with staggered DID. The design first selects PLFSs established in areas without prior nearby PLFS establishments, and then compares local outcomes around earlier and later PLFS entries. Later-entry locations serve as not-yet-treated controls for earlier-entry locations.[6]

Using PLFS-level GIS information, I construct non-overlapping local neighborhoods around newly established PLFSs. Starting in 1999, the first year for which subsidiary lists are available, I iterate forward

4. Qichacha is a leading provider in China, delivering detailed information on businesses and corporations, sourced from the government-led National Enterprise Credit Information Publicity System (NECIPS). All Chinese firms are legally obligated to register with this body (Allen et al., 2019).
5. http://www.sipo.gov.cn/
6. We intentionally do not use adjacent never-treated areas as controls, because locations that are never selected by listed firms are likely to differ systematically from PLFS-selected locations in unobserved ways, confounding spillovers with location selection.

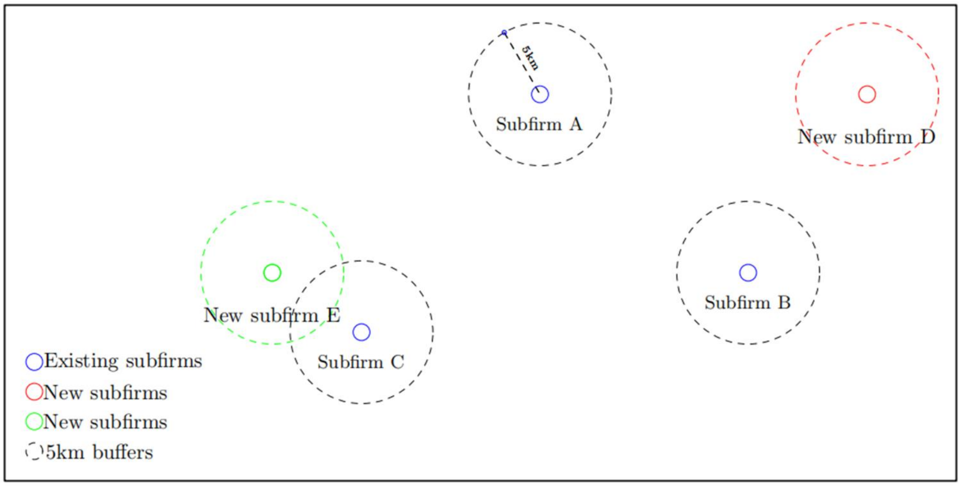


Figure 1. Illustration for the sample selection.

by establishment year. Let I_k denote the set of PLFSs established in year k, and let $I_{\leq k}$ denote all PLFSs established up to year k. For each PLFS i, define its j-km buffer as

$$B_i = \{x : d(x, PLFS_i) \leq j \text{ km}\},$$

where $d(x, PLFS_i)$ is the Euclidean distance from point x to PLFS i. For each year k, I retain newly established PLFSs whose buffers do not overlap with those of any earlier or same-year PLFSs:

$$\tilde{I}_k = \{i \in I_k : B_i \cap B_h = \emptyset, \ \forall h \in I_{\leq k}, h \neq i\}.$$

Figure 1 illustrates this iterative selection rule. Existing PLFSs are shown in blue; a newly established PLFS is retained if its buffer does not overlap with existing buffers, and excluded otherwise. Iterating this procedure through 2020 yields a sample of PLFSs established in areas without prior nearby subsidiary establishments by other PLFSs.

The baseline radius is $j = 2.5km$. This radius captures highly localized activity; for comparison, the average Chinese county has a radius of approximately 25 km. The nonoverlap rule limits contamination from other PLFSs and helps interpret the estimates as the effect of a single subsidiary entry. The baseline procedure yields 9,499 PLFSs between 1999 and 2022 (1.3 per cent of all PLFSs during this period). For robustness, I also use a $j = 5$ km radius, which further avoids contamination but reduces the sample to 5,411 PLFSs. Supplementary Fig. A3 reports the spatial distribution of selected PLFSs, and Supplementary Table A1 reports selected sample characteristics.

The estimation sample spans 2009–2020, avoiding the major macroeconomic disruption around the 2008 financial crisis and the COVID-19 period after 2020. I estimate the following specification:

$$Y_{ict} = \beta PLFS_{ict} + \alpha_i + \lambda_{ct} + \mathbf{X}_{ict}\boldsymbol{\Gamma} + \varepsilon_{ict}, \tag{1}$$

where Y_{ict} is the outcome measured within the j-km buffer around PLFS location i in hosting city c and year t, and $PLFS_{ict}$ equals one if the PLFS at location i has been established by year t. Buffer fixed effects α_i absorb all time-invariant characteristics of the PLFS-location unit, including persistent local attributes and parent-firm attributes that do not vary over time. City-by-year fixed effects λ_{ct} absorb shocks and policy changes common to all locations within the same city-year, including major transportation projects, fiscal transfers, and other city-level place-based policies (e.g., changes in the business environment; Nie *et al.*, 2018). The vector $\mathbf{X}_{ict}$ includes time-varying covariates at the PLFS-location and parent-firm levels.

Given staggered treatment timing, we estimate Equation (1) using the staggered DID estimator of Callaway and Sant'Anna (2021) (hereafter CSDID). With these controls in place, β is identified from within-city variation in treatment timing by comparing outcome trajectories around early-established PLFS locations to those around later-established (not-yet-treated) locations in the same city-year. The key identifying assumption is that, absent PLFS entry, treated and not-yet-treated locations within the same city would have followed parallel trends. The fine spatial scale and city-by-year comparisons help hold fixed broader policy environments and local market conditions, reducing the concern that the estimates simply capture policies that directly attract other firms or innovative activity.

3. Impact of PLFSs on local economy

3.1 Validity of the identification

The identifying assumption in our staggered DID design is that, absent PLFS establishment, outcome trajectories in buffers that are treated earlier would evolve similarly to those treated later (not yet treated). To validate this parallel trends assumption, I adopt the following dynamic event-study specification:

$$Y_{ict} = \sum_{k=-T_l}^{T_u} \beta_k 1\{t - g_i = k\} + \alpha_i + \lambda_{ct} + \mathbf{X}_{ict}\mathbf{\Gamma} + \varepsilon_{ict}, \tag{2}$$

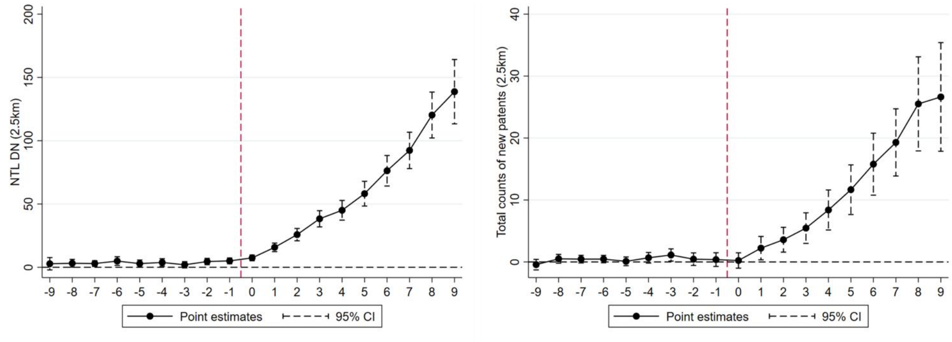
where $1\{t - g_i = k\}$ is an indicator equal to one if year t is k periods away from the establishment year g_i of the PLFS at location i. Our baseline sample spans 2009–2020, so each β_k captures the average difference in Y_{ict} between buffers treated at different times relative to the omitted baseline event time.[7]

Figure 2 presents the dynamic event study results for economic activities and local patenting. For both outcomes, the pre-treatment coefficients are generally close to zero and show no systematic pattern, supporting the parallel-trends assumption. This indicates that the neighborhoods of later-established subsidiaries can serve as a proper control group for those of earlier-established ones, thereby validating our identification strategy. After PLFS establishment, both outcomes increase steadily. Local patenting follows a similar post-entry pattern, suggesting that PLFS entry is associated not only with greater local activity but also with increased innovation in nearby areas.[8]

3.2 Baseline results

Table 1 reports the baseline CSDID estimates. Columns 1–3 use *Light* as the dependent variable. Column 1 includes PLFS and year fixed effects. Consistent with the event-study evidence in Fig. 2, areas around new PLFSs experience a sizable increase in nighttime lights after entry. The ATT is about thirty-nine DN values within a 2.5 km radius, or roughly two additional DN values per pixel, corresponding to a 26.4 per cent increase relative to the pre-treatment mean of 148.8. The effect is also positive under alternative CSDID aggregations by establishment cohort, calendar year, and event time. The dynamic-event

7. Because identification relies on comparisons between early-treated and later-treated (not-yet-treated) locations rather than a fixed set of never-treated controls, a conventional treated-versus-never-treated balance table is not well defined in our setting. In staggered-adoption designs of this form, the relevant analogue is the pre-treatment parallel-trends evidence, assessed through the event-study coefficients for $k < 0$.
8. A small number of pre-treatment coefficients are statistically different from zero, but their magnitudes are small and do not display a systematic pre-trend. Following Rambachan and Roth (2023), I further implement HonestDiD sensitivity analysis under the smoothness restriction. The robust confidence intervals remain strictly positive for all values of M considered, suggesting that the baseline result is robust to smooth deviations from strict parallel trends (see Supplementary Appendix B).



(a) NTL (b) Patent spillover

Figure 2. New subsidiaries and hinterland development.

Table 1. Impact of new PLFSs on local economic development.

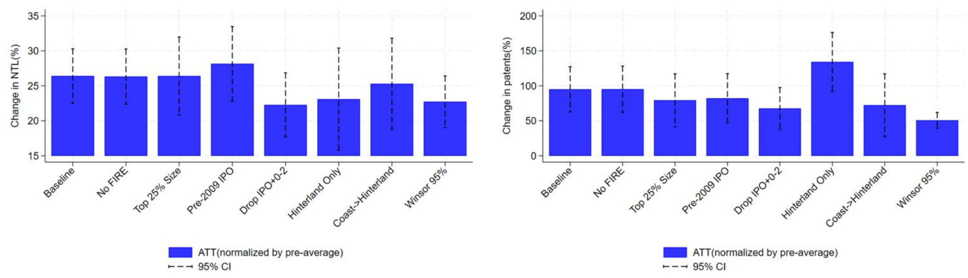
	Dep. var.: *Light*			Dep. var.: *Innovation*		
ATT	39.286***	38.586***	37.620***	7.001***	6.802***	6.049***
	(2.952)	(2.961)	(3.038)	(1.215)	(1.227)	(1.260)
Group-avg ATT	27.725***	26.950***	26.485***	4.515***	4.324***	3.817***
	(2.308)	(2.315)	(2.400)	(0.941)	(0.962)	(0.991)
Calendar-avg ATT	35.367***	34.944***	33.720***	5.471***	5.336***	4.733***
	(2.634)	(2.636)	(2.676)	(1.249)	(1.253)	(1.272)
Dynamic event	61.817***	60.758***	59.517***	11.880***	11.554***	10.434***
	(4.467)	(4.491)	(4.636)	(1.646)	(1.667)	(1.728)
$Avg(Y^{pre})$	148.797	148.797	148.797	7.376	7.376	7.376
Observations	60,577	60,577	59,994	60,302	60,302	59,721
PLFS FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
City controls	N	Y	Y	N	Y	Y
Parent controls	N	N	Y	N	N	Y

Notes: CSDID results reported. The dependent variable is the sum of DN values of NTL within 2.5 km (*Light*) in columns 1–3, and the count of new patents within 2.5 km, subtracting those from PLFSs themselves (*Innovation*), in columns 4–6. $Avg(Y^{pre})$ represents the pre-treatment average of the dependent variable. The city controls are city-year fixed effects. Parent control variables include parent firm size, measured as the natural logarithm of annual income, and its labor intensity, defined as the ratio of labor to capital inputs. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

aggregation implies an average post-treatment increase of about sixty-two DN values, or 42 per cent relative to the pre-treatment mean.

Columns 2 and 3 add controls for potential confounders. City-year fixed effects absorb shocks and policy changes common to all locations within the same destination city, ensuring that the identifying comparison is made among earlier- and later-treated locations within the same city-year. Parent-firm controls further account for time-varying characteristics of the listed parent, including annual sales ($\ln Y$) and labor intensity (L/K), which may affect the subsidiary's financial capacity and local spillover potential. Adding these controls slightly reduces the estimated effects, but the coefficients remain statistically significant and economically stable.

Columns 4–6 repeat the analysis using *Innovation* as the dependent variable. The estimates show a similarly robust increase in nearby patenting. The ATT implies about seven additional patents within 2.5 km of a new PLFS, relative to a pre-treatment mean of 7.38 patents, or a 95 per cent increase. As with NTL,



(a) NTL

(b) Patent spillover

Figure 3. Robustness across alternative samples and specifications.

the innovation effect remains stable after adding city-year fixed effects and parent-firm controls, and the dynamic-event aggregation indicates a larger cumulative post-entry response.

3.3 Robustness checks

I assess the robustness of the baseline findings along four dimensions. Figure 3 summarizes the main robustness checks, while detailed estimates are reported in Supplementary Appendix B.

First, I examine whether the results depend on the definition of mega-firm subsidiaries or on specific establishment events. Excluding subsidiaries whose parents operate in FIRE industries leaves the estimates essentially unchanged, consistent with the small share of FIRE firms in the sample. Restricting parent firms to the top quartile of operating revenue within each industry-year also yields quantitatively similar effects, suggesting that the baseline listed-firm universe is already a reasonable proxy for domestically large firms. To address concerns about IPO-related dynamics (Kim and Weisbach 2008; Bernstein 2015; Larraín et al., 2025), I further exclude subsidiary entries within three years after the parent firm's IPO and restrict the sample to parents listed before 2009; both restrictions deliver comparable estimates. Finally, restricting destinations to inland provinces produces similar effects on *Light* and larger effects on *Innovation*, while a coastal-origin, cross-province subsample remains close to the baseline.

Second, I address the skewness and zero-inflation of the outcome variables. Both NTL and patent counts are nonnegative, right-skewed, and contain many zeros. Because standard staggered-adoption DID estimators are linear, cohort-time weighted procedures, there is no direct nonlinear analogue, such as Poisson PML, that preserves the same identification and aggregation structure. I therefore implement two complementary adjustments. I first winsorize *Light* and *Innovation* at the ninety-fifth percentile to reduce the influence of extreme values. The resulting estimates remain close to the baseline and preserve the same pre-trend and post-entry dynamics. I then re-estimate the baseline specifications using log-transformed outcome densities, defined as the outcome divided by buffer area. The estimates remain positive and follow the same temporal pattern. Because $\log(1+\cdot)$ does not admit a stable percent-change interpretation in zero-inflated settings (Cohn, Liu, and Wardlaw 2022; Chen and Roth 2024), I treat these results as supportive robustness checks rather than as the preferred scale for interpretation.

Third, I vary the spatial threshold and sample period. The baseline uses $j = 2.5$ km, which implies at least 5 km separation between selected PLFS buffers. Increasing the threshold to $j = 5$ km, and therefore requiring at least 10 km separation, yields similar event-study patterns. I also extend the panel back to 2001, the year of China's WTO accession. The estimates remain positive and display the same post-entry dynamics, indicating that the baseline results are not driven by the choice of the 2009–2020 sample window.

Fourth, I test the sensitivity of the innovation measure. A potential concern is that multi-establishment firms may reassign patent ownership across affiliated entities or addresses, weakening the link between patent location and inventive activity. In this setting, the spillover outcome mitigates this concern because it is defined as patenting within the buffer net of patents filed by the focal PLFS itself, and identification relies on within-city timing comparisons with city-by-year fixed effects. The results are also robust to excluding design patents and focusing on invention patents and utility models.

4. Mechanism analysis

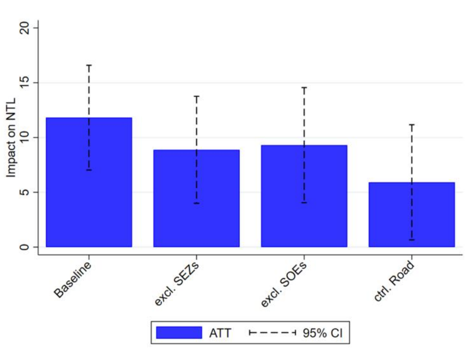
The remaining identification concern is that PLFS locations may be selected into very localized policy interventions within cities. City-by-year fixed effects absorb shocks and policies common to all locations in the same city-year, such as citywide infrastructure projects, fiscal transfers, or changes in the business environment. They do not, however, fully rule out policies targeted at specific neighborhoods, including special zones, local tax incentives, regulatory relaxation, or directed infrastructure investments. In this case, the post-entry growth around PLFSs may partly reflect the direct effects of within-city place-based policies that both attracted the PLFS and induced subsequent entry by other firms, rather than spillovers from the PLFS itself.

This section addresses this concern in two steps. First, I test whether the results are explained by observable neighborhood-level policy and infrastructure channels, focusing on special economic zones and road density. Second, I use detailed patent data and exploit spatial and technological proximity to PLFSs to provide more direct evidence of innovation spillovers.

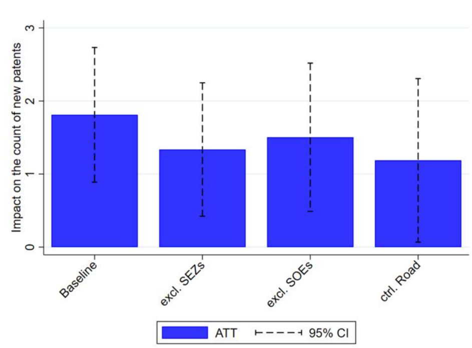
4.1 Isolating the PLFS effect from government intervention

To examine whether the baseline effects are driven by within-city, neighborhood-level place-based policies rather than PLFS spillovers, I rule out several salient policy channels one by one. I first exclude formal zoning policies, captured by Special Economic Zones (SEZs). I then examine targeted infrastructure investment, proxied by road construction and road controls within the 2.5 km buffer. Finally, I use SOE ownership to proxy for broader implicit local targeting and privileged access to government information. Figure 4 summarizes the main results, while detailed CSDID estimates and event-study plots are reported in Supplementary Appendix B.

First, I consider formal zoning policies. SEZs are among China's most prominent place-based policies, providing tax incentives, subsidies, and regulatory advantages to attract firms and promote local development (Wang 2013). By 2006, SEZs accounted for roughly 10 per cent of China's GDP and about one-third of FDI while covering only about 0.1 per cent of national land area (Zheng et al., 2017). To reduce



(a) NTL



(b) Patent Spillover

Figure 4. ATTs under different model specifications with respect to zoning policies.

SEZ-related confounding, I exclude PLFSs whose 2.5 km buffers overlap with any SEZ established by 2022. Because the SEZ data provide point locations and zone areas, I approximate each SEZ as a circle centered at its recorded location, with radius implied by its area, and treat a PLFS as exposed if its buffer intersects this SEZ circle. This rule excludes subsidiaries located inside existing SEZs as well as those later encompassed by newly designated zones. As SEZ designation may itself respond to local agglomeration forces, including those triggered by PLFS entry, this exclusion is conservative and may understate the true subsidiary effect. The filter removes 10.7 per cent of PLFSs, and re-estimating CSDID on the remaining sample yields statistically significant effects on both nighttime lights and local patenting, with magnitudes comparable to the baseline as shown in Fig. 4.

Second, I examine targeted infrastructure investment. In China, local policy packages often bundle land provision with infrastructure upgrades, and transport investment is a salient observable component of such efforts (Zheng et al., 2017; Lu, Wang, and Zhu 2019). I measure road accessibility by the total length of roads within the 2.5 km buffer around each PLFS, using both major roads only—expressways, arterial roads, and secondary or tertiary roads—and all road types. Because road data are available from 2014, this analysis is limited to 2014–2020. Road event studies show no systematic pre-trend before PLFS entry, making it unlikely that subsidiaries were placed in response to pre-existing road construction within these neighborhoods. Road density rises one to two years after entry, which is consistent with infrastructure expansion as a response to subsidiary-induced agglomeration, although it could also reflect anticipated infrastructure improvements. I therefore re-estimate the baseline models controlling for time-varying road length within the 2.5 km buffer. The effects on both nighttime lights and innovation remain positive and statistically significant conditional on road length, with event-study dynamics similar to the baseline. Adding time-varying road length within 2.5 km attenuates the estimates: the *Light* coefficient falls to 0.64 of its value without road controls, and the *Innovation* coefficient falls to 0.79. Interpreting this attenuation as an upper bound on the infrastructure channel suggests that responsive road expansion can account for at most 36 per cent of the *Light* effect and 21 per cent of the innovation effect.

Third, I examine broader implicit local targeting through firm–government connections. Some neighborhood-level support may not be fully captured by SEZs or road investments, such as informal regulatory support, preferential access to land, or privileged information about forthcoming local policies. I use the distinction between SOE-led and non-SOE-led PLFSs as a proxy for this broader channel, since prior work shows that SOEs tend to have stronger political ties and better access to government information (Chen et al., 2011; Houston et al., 2014; Piotroski and Zhang 2014; Pan and Tian 2020; Fang et al., 2023). If the baseline effects were driven by such unobserved policy targeting, effects should be stronger for SOE-led PLFSs. Table 2 shows no such pattern. Estimated effects for SOE-led PLFSs are not larger, and are smaller for NTL. To formalize the comparison, I rescale outcomes by their pre-treatment means and use a randomization-based Monte Carlo procedure that reassigns SOE status within entry-year cohorts

Table 2. Impact of subsidiaries by parent firm types.

	Dep. var.: $\Delta Light_{it}$			Dep. var.: $\Delta Innovation_{it}$		
	SOEs	Non-SOEs	SOEs—Non-SOEs	SOEs	Non-SOEs	SOEs—Non-SOEs
ATT	0.202***	0.279***	−0.077**	0.865***	0.927***	−0.062
	(0.050)	(0.022)	[−0.060, 0.171]	(0.257)	(0.186)	[−0.654, 1.204]
Obs	10,006	50,160		49,966	10,006	

Notes: CSDID estimator reported. The dependent variable is the sum of DN values of NTL within 2.5 km (*Light*) in columns 1–3, and the count of new patents within 2.5 km, subtracting those from PLFSs themselves (*Innovation*), in columns 4–6. Column 1 includes all subsidiaries of SOEs listed firms, while column 2 focuses on those of non-SOEs listed firms. Bootstrap standard errors clustered at the PLFS level are in parentheses, and empirical 95 per cent Monte Carlo confidence intervals are in square brackets. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

while preserving the observed SOE share. The SOE–non-SOE difference is negative for NTL and statistically indistinguishable from zero for innovation, providing little support for the view that the baseline effects are primarily driven by politically connected firms selecting into areas with unobserved local policy support. In addition, excluding SOE-led PLFSs leaves the estimates largely unchanged.

Figure 4 brings these tests together. Overall, the evidence suggests that formal zoning, targeted infrastructure, and broader implicit local targeting cannot fully explain the baseline effects.

These results also help address a data-coverage limitation of the spatial filtering procedure. Because the spatial history is constructed from observed PLFSs among domestically listed firms, I cannot fully screen out subsidiaries affiliated with large firms that are unlisted or listed offshore. If some buffers classified as "clean" contain such unobserved subsidiaries, the isolation criterion is measured with error. This threatens identification mainly if unobserved subsidiaries systematically co-enter around the same time as the focal PLFS, for example due to coordinated fine-scale policy targeting. The absence of pre-trends, together with the evidence against salient within-city intervention channels, makes this explanation unlikely. If unobserved subsidiaries are pre-existing, evolve smoothly, or change independently of focal PLFS timing, they are absorbed by fixed effects or add noise that biases estimates toward zero.

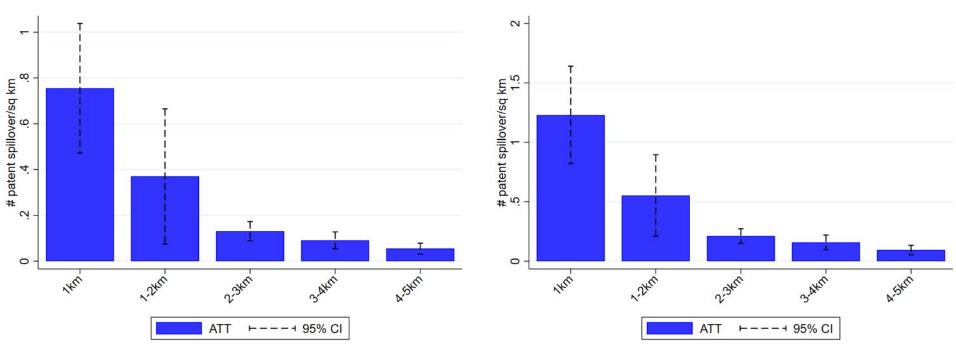
4.2 Direct evidence from the patent data

After excluding several salient government-intervention channels, I use detailed patent data to provide more direct evidence that the innovation response is driven by PLFS spillovers. The key implication is that, if PLFS entry is the source of the shock, the effect should be stronger where exposure to the PLFS is greater, either spatially or technologically.

4.2.1 Spatial proximity

I first examine spatial attenuation using a radius-based design following (Egger et al., 2022). In the 5-km-threshold sample, I partition the area around each PLFS into a 1-km core and four concentric 1-km rings: 1–2, 2–3, 3–4, and 4–5 km. Because these rings differ in area, I measure innovation as patent density, defined as total non-PLFS patent counts divided by ring area.

Figure 5 shows that effects decline sharply with distance. The estimated post-entry patent density is $0.75/\text{km}^2$ within 0–1 km, falls to $0.37/\text{km}^2$ in 1–2 km, and to $0.13/\text{km}^2$ in 2–3 km, with smaller values in the outer rings ($0.09/\text{km}^2$ in 3–4 km and $0.05/\text{km}^2$ in 4–5 km). Panel (b), which reports average post-treatment effects from event-study estimates, yields the same monotone distance gradient. Supplementary Fig. B10 shows that this spatially decaying pattern holds year-by-year in the post period



(a) ATT　　(b) Post-treatment average

Figure 5. Radius analysis for patents.

and becomes more pronounced over time. This gradient treatment pattern with respect to spatial proximity to the PLFSs provides direct evidence of the spillover effects on innovation from PLFSs.

4.2.2 Technological proximity

I next examine whether spillovers are stronger in technologies closer to the parent firm's knowledge base. For each listed parent firm, I construct its cumulative IPC portfolio using patents filed up to year t. A local patent around PLFS i in year t is classified as *technology-related* if its IPC class belongs to the parent firm's prior IPC portfolio, and *technology-unrelated* otherwise:

$$Treated\_S_{ijt} = \begin{cases} 1 & \text{if } Patent_{ij,k \le t} > 0 \\ 0 & \text{if } Patent_{ij,k \le t} = 0 \end{cases} \tag{3}$$

To make effects comparable across groups with different baseline patenting levels, I normalize outcomes by their pre-treatment means and report $\Delta Innovation$.

Table 3 reports the corresponding CSDID estimates. Columns 1–4 show that PLFS entry increases both types of innovation, but the effect is much larger for technology-related patents. Over the nine-year post-entry horizon, the relative increase in related patents is nearly three times that of unrelated patents. The estimates are robust to adding city-year fixed effects and parent-firm controls.[9] Columns 5–6 directly test this technological gradient by estimating CSDID on the within-location gap, $\Delta Innovation^1 - \Delta Innovation^0$. The treatment effect on this gap is significantly positive, indicating stronger spillovers in technologies closer to the parent firm's knowledge base. The positive effect on technology-unrelated patents further suggests that PLFS entry generates both targeted diffusion and broader cross-domain spillovers.

As a robustness check, I adopt a stricter definition of technological relatedness. For each listed parent, I rank IPC classes by patent counts and keep only the set of "core" classes that together account for 90 per

Table 3. Heterogenous impact of PLFSs by IPC sectors.

	$\Delta Innovation$				$\Delta Innovation^1 - \Delta Innovation^0$	
	$Treated\_S = 1$		$Treated\_S = 0$			
ATT	2.984***	2.759***	0.956***	0.839***	2.028***	1.920***
	(0.342)	(0.373)	(0.204)	(0.211)	(0.322)	(0.348)
Group-avg ATT	2.169***	1.984***	0.591***	0.505***	1.578***	1.479***
	(0.305)	(0.344)	(0.168)	(0.174)	(0.315)	(0.350)
Calendar-avg ATT	2.116***	1.963***	0.809***	0.722***	1.307***	1.241***
	(0.277)	(0.290)	(0.205)	(0.209)	(0.229)	(0.241)
Post-avg	5.165***	4.803***	1.496***	1.329***	3.669***	3.474***
	(0.535)	(0.591)	(0.276)	(0.283)	(0.513)	(0.558)
Observations	60,577	59,994	60,577	59,994	60,577	59,994
PLFS FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
City controls	N	Y	N	Y	N	Y
Parent controls	N	Y	N	Y	N	Y

Notes: CSDID estimator reported. The dependent variable is the relative change in innovation count in related sectors in columns 1–2, in unrelated ones in columns 3–4, and the difference of these two in columns 5–6. The city controls are city-year fixed effects. Parent control variables include parent firm size, measured as the natural logarithm of annual income, and its labor intensity, defined as the ratio of labor to capital inputs. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

9. Supplementary Appendix Figure B11 shows event-study estimates for technology-related and technology-unrelated patents, with no systematic pre-trends in either series.

cent of the parent's cumulative patenting. Re-estimating CSDID on patents in these core classes yields an effect of about 3.4 times the pre-entry mean, larger than under the broader relatedness definition. This pattern is consistent with spillovers concentrating in the parent firm's strongest technological domains.

Taken together, the zoning exclusions and the spatial- and technology-proximity results support a subsidiary-driven interpretation: PLFS entry solely and directly increases nearby economic activities and innovation, with effects that attenuate with distance and concentrate in technologies closest to the parent's knowledge base.

5. Further analysis

Having established a causal link between PLFS entry and local economic activity and innovation, I turn to three policy-relevant extensions. First, I examine whether the innovation response comes from pre-existing local firms, newly entering firms, or both. This distinction is important because gains among incumbents indicate intensive-margin innovation rather than pure relocation or replacement, while gains among entrants capture the extensive margin of new firm formation and subsequent innovation. Second, I use inventor-level outcomes to study whether PLFS entry reshapes the local inventor pool and redirects innovation toward new technological trajectories. Since inventors tend to build on specialized paths (Acemoglu and Cao 2015), comparing inventors active before entry with those appearing after entry provides evidence on whether local innovation is reorganized rather than merely expanded. Third, I examine heterogeneity by host-region initial conditions, which speaks to the external validity of mega-firm spillovers across places and stages of development.

5.1 Incumbent firms versus new entries

I first decompose local firms into incumbents (registered before PLFS entry) and post-entry entrants (registered afterward). I then decompose the cumulative change in local patenting over h years after entry $\Delta Innovation_t^h$ into two parts. The first is the change in patenting by incumbents, capturing the intensive-margin response of pre-existing firms (net of any exit). The second is patenting by post-entry entrants, capturing both the extensive margin of firm entry and subsequent innovation by these new firms. Specifically:

$$\Delta \text{Innovation}_t^h = \overbrace{\bar{P}_{t+h}(I_{t+h} \cap I_t) - \bar{P}_t(I_t)}^{\text{Intensive Margin}} + \underbrace{\bar{P}_{t+h}(I_{t+h} \setminus (I_{t+h} \cap I_t))}_{\text{Entry Margin}} . \tag{4}$$

Here I_t denotes the set of active local firms in year t, and $\bar{P}_t(\cdot)$ denotes average patents per firm in year t for the indicated set of firms. The intersection $I_{t+h} \cap I_t$ contains firms active both before and after entry. Firms in $I_t \setminus (I_{t+h} \cap I_t)$ exit by $t+h$, while firms in $I_{t+h} \setminus (I_{t+h} \cap I_t)$ enter after t and are active at $t+h$. The first term therefore captures changes in patenting among continuing incumbents, with exit reflected in the shrinking incumbent set. The second term captures patenting by post-entry entrants, combining the extensive margin of entry and entrants' average patenting intensity.

I then re-estimate the event-study specification separately for incumbent patenting and entrant patenting. For entrants, the pre-entry outcome is mechanically zero, so the estimates should be interpreted as the emergence of new innovative firms after PLFS entry. Table 4, Panel A, reports the baseline CSDID estimates for the firm-level decomposition. Among incumbents, the pre-entry mean is 3.4 patents per year within 2.5 km. PLFS entry increases incumbent patenting by 4.6 patents per year over the nine-year post-entry period, a 132 per cent increase relative to the pre-entry mean. The dynamic estimates show that this incumbent response grows over time, suggesting that pre-existing local firms adjust their innovation activity after the arrival of the PLFS. At the same time, PLFS entry is associated with 4.5 additional patents

Table 4. Further analysis: firm margins, inventor composition, and host-region heterogeneity.

	ATT	S.E.	$Avg(Y^{pre})$	ATT/pre-mean
Panel A. Firm-level decomposition				
Incumbent firms	4.550***	(0.722)	3.438	132.3%
New entrants	4.508***	(0.633)	0.000	–
Panel B. Inventor-level decomposition				
Pre-existing inventors	−16.268***	(1.525)	9.470	−171.8%
Newly appearing inventors	20.338***	(1.213)	0.000	–
Panel C. Host-region human capital				
Above-median human capital	6.372***	(1.616)	10.816	58.9%
Below-median human capital	8.860***	(2.191)	5.302	167.1%

Notes: The table reports baseline CSDID estimates from the further analyses. All specifications include PLFS fixed effects and year fixed effects. Panel A decomposes local patenting into patenting by incumbent firms and patenting by post-entry entrants. Panel B decomposes local patenting by inventor type, distinguishing pre-existing inventors from newly appearing inventors. Panel C reports heterogeneity by host-region human capital, proxied by the city-level share of college students in the population. $Avg(Y^{pre})$ denotes the pre-treatment mean of the dependent variable. ATT/pre-mean is computed as the ATT divided by the pre-treatment mean and is not reported when the pre-treatment mean is mechanically zero. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Detailed CSDID aggregations, specifications with additional controls, road-control specifications, and event-study plots are reported in the Supplementary Appendix.

per year filed by post-entry firms in the same area, indicating that entry rises and that the entering firms are more innovative on average. I also rule out direct patent-level collaboration as the primary driver. Patents jointly filed with the focal PLFS account for only 1.16 per cent of local-firm patents, and joint patents with the parent listed firm account for an additional 0.5 per cent, suggesting that the effects operate beyond formal co-invention. The Supplementary Appendix shows that these results are robust to alternative CSDID aggregations, additional controls, and event-study specifications.

This decomposition helps distinguish spillover-driven growth from two alternative interpretations. First, PLFS entry induces incumbents to patent more, consistent with competition-induced innovation or local knowledge spillovers (Aghion et al., 2005). The entrant margin further shows that PLFS entry changes the composition of local firm entry: Post-entry firms in treated neighborhoods are more innovative on average. Taken together, these results suggest that the baseline innovation gains are not merely a reshuffling of activity across nearby locations. They reflect both incumbent upgrading and the entry of innovative firms, pointing to a localized growth response rather than pure displacement.

5.2 Existing versus new inventors

To better understand the channels of local innovation spillovers, I conduct an inventor-level decomposition. I separate inventors into (1) *pre-existing inventors* who appear in the local patent record before PLFS entry and (2) *new inventors* who first appear afterward. Inventors are identified by name; because the analysis is restricted to a 2.5 km radius around each PLFS, the scope is sufficiently narrow to substantially limit measurement error from homonymous inventors.

The key idea is that pre-existing inventors proxy the *pre-entry local technology trajectory* in the immediate area, while new inventors capture innovation along a *post-entry trajectory* that emerges after the subsidiary's arrival. Using patent counts by inventor type as outcomes, I assign zeros to new inventors in pre-entry periods by construction. Table 4, Panel B, reports the baseline CSDID estimates. Patenting by pre-existing inventors, which proxies for established local innovation trajectories, declines sharply after PLFS entry. By contrast, patenting by newly appearing inventors, which proxies for new technological directions, rises substantially. Event-study estimates in the Supplementary Appendix show no systematic pre-trends and reveal a similar post-entry divergence. This pattern is consistent with a local creative-destruction dynamic in which PLFS entry redirects local innovation away from incumbent trajectories and toward new ones.

A potential concern is name-based misclassification. As shown in Supplementary Appendix A, the matching procedure, if anything, is more likely to over-assign patents to pre-existing inventors and under-assign to new inventors. This implies that any homonym bias would attenuate the divergence, so the estimated contrast is conservative.

The literature highlights inventor mobility as a key mechanism through which large firms transmit knowledge to local firms (Matray 2021). I therefore directly test this channel by matching inventor identities between PLFS patents and local-firm patents. Only 0.2 per cent of local-firm patents are attributable to inventors previously observed patenting for PLFSs, which makes direct inventor turnover an unlikely first-order mechanism in this setting. This is consistent with subsidiaries in underdeveloped areas drawing primarily from the same local labor market as incumbent firms. Taken together, the evidence points to demonstration effects and supply-chain pass-through as more plausible drivers of the observed spillovers (Chu, Tian, and Wang 2019).

5.3 Heterogeneity with respect to hosting regions

I next examine whether the effects vary with host-region initial conditions. Human capital is a natural margin of heterogeneity. If local skills primarily operate as absorptive capacity, spillovers should be larger in high-human-capital places, as suggested by parts of the FDI literature. For example, Gong (2023) finds that innovation by US multinationals is associated with larger productivity gains for nearby domestic firms, especially among firms with higher-wage workers and stronger innovation capacity. At the same time, high-human-capital places may already be on steeper innovation trajectories, leaving less room for additional gains from PLFS entry, whereas lower-human-capital places may have greater scope for improvement (Abebe, McMillan, and Serafinelli 2022). Domestic mega-firm spillovers may also differ from foreign-technology transfer because language and organizational barriers are less central.

I therefore split host cities by the city-level share of college students in the population and estimate treatment effects separately for above- and below-median cities. As shown in Table 4 Panel C, PLFS entry has larger effects in below-median human-capital cities. In levels, it increases local patenting by 8.86 patents per year within 2.5 km in below-median cities, about 40 per cent larger than the 6.37-patent increase in above-median cities. The contrast is sharper in relative terms because baseline patenting is lower in below-median cities: the implied relative increase is 167 per cent in below-median cities, compared with 59 per cent in above-median cities. This pattern is more consistent with greater scope for improvement in initially less innovative places than with a simple absorptive-capacity interpretation.

The results are robust to adding city- and parent-firm controls and to alternative CSDID aggregation choices. They are also robust to an alternative human-capital measure that combines college and vocational students per million residents. Detailed estimates and event-study plots are reported in the Supplementary Appendix.

This heterogeneity also helps interpret external validity. The below-median human-capital host cities in my sample are comparable to many developing-country settings. During the sample period, their average college-student count is 67.63 per 10,000 residents, which corresponds to the eleventh percentile among 218 countries in World Bank data; in levels, the average falls around the twentieth percentile of the global distribution.[10]

10. Tertiary enrollment data are from World Bank Education Statistics (https://datatopics.worldbank.org/education/home), and population data are from the World Bank Population Estimates and Projections database (https://databank.worldbank.org/source/population-estimates-and-projections). The Supplementary Appendix plots the cross-country distributions and indicates China's position.

6. Conclusions

This article studies local economic and innovation spillovers from the entry of subsidiaries of domestically large firms, proxied by publicly listed parents. Subsidiary entry generates sizable local patenting gains and increases in nighttime lights, with effects that attenuate sharply with distance and concentrate in technologies close to the parent firm's patent portfolio. A battery of checks reduces concerns about policy confounding at fine spatial scales: Estimates are similar after excluding overlaps with SEZs and remain stable when accounting for post-entry road expansion and implicit zoning advantages proxied by SOE ownership. PLFS entry raises innovation among pre-existing local firms, consistent with competition-induced innovation. It also draws in more innovative entrants. Together, these two margins suggest a localized growth response rather than a purely relocation-based interpretation. At the inventor level, PLFS entry is accompanied by a compositional shift in the local inventor pool, consistent with a creative-destruction mechanism. This shift is not explained by inventor mobility, pointing instead to channels such as demonstration effects and supply-chain pass-through. Heterogeneity analyses further show larger spillovers in lower-human-capital host regions, supporting external validity for underdeveloped settings.

These findings inform the evaluation of place-based policies aimed at attracting investment. By isolating the effect of a single subsidiary entry, this article provides a building block for interpreting the impacts of broader policy packages in which multiple instruments are bundled and individual components are otherwise hard to separate.

More broadly, the results speak to the design of place-based strategies. Traditional "big-push" approaches spread resources across many projects in targeted regions and can deliver uneven returns. For example, China's Third Front program generated long-run gains but also exhibited diminishing returns in less developed areas (Fan and Zou 2021). In contrast, my evidence points to a more targeted approach that prioritizes attracting subsidiaries of large, capable firms and aligns public inputs with their expansion. This "firm-led, government-follow" strategy may improve allocative efficiency and yield more durable development effects than broadly dispersed subsidies.

It is important, however, to interpret these estimates as local effects rather than as a full general-equilibrium welfare assessment. The firm-level decomposition provides suggestive evidence of localized growth: incumbent firms innovate more, and new entrants are more innovation-intensive. Nevertheless, the current research design cannot fully decompose net new activity from within-city spatial reallocation. Nor does it measure possible reorganization effects across broader regions that may arise when PLFS entry changes the spatial distribution of economic activity. These aggregate and spatial-equilibrium implications remain important directions for future research.

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Supplementary material

Supplementary materials available at *Journal of Economic Geography* online.

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