Career Incentives, Land Allocation, and Local Government Debt in China's Mandarin Growth Model*

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Abstract

A Chinese city leader's promotion rewards local GDP growth. The exception is a hometown tie. When a newly appointed provincial superior shares the leader's birthplace, promotion no longer depends on growth at all. Exploiting the timing of these appointments as a plausibly exogenous relaxation of career incentives, we show that tied leaders shift land supply from industrial to residential use by six to seven percentage points, lower residential land prices by roughly a fifth, and curtail local government debt accumulation, on the order of a year of forgone borrowing. These estimates lie outside the distribution of a 500-draw randomization placebo, and they are not explained by preferential treatment from above or by anticipated promotions. The response is compositional rather than expansionary, because investment, infrastructure, and the total quantity of land supplied do not move. Tracing the reallocation into the real economy, the visible industrial engine the tournament rewards winds down, as foreign investment and industrial employment fall, while housing delivered to households appears to expand and output holds up, a recomposition toward construction rather than broad productive growth. These patterns are the leader-level footprint of China's Mandarin model of growth, in which career incentives steer local development toward the visible activity a promotion tournament can credit. Relaxing those incentives changes the composition of local growth, not its level.

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1 Introduction

China's four decades of growth is often linked to a "Mandarin" system in which local officials receive substantial economic discretion within a hierarchical state and face promotion incentives tied partly to local economic performance (Song and Xiong, 2026). One implication is that the career incentives of individual officials, not only fiscal or market fundamentals, shape economic decisions of the local officials, including the allocation of land across competing uses and the accumulation of local government debt. Establishing this relationship causally has been difficult because career incentives are not directly observed and are endogenous to the outcomes they affect.

We provide causal evidence that political career incentives shape local financial and real estate outcomes. Our empirical design exploits variation in the authority responsible for a city leader's promotion. In China's personnel hierarchy, most prefectural city leaders are evaluated by provincial superiors, with economic performance playing a central role in promotion incentives (Li and Zhou, 2005). These provincial superiors are appointed from above, at times and for reasons plausibly unrelated to any given city's land and housing markets. Building on prior work showing that social and political ties are relevant for political selection in China (Jia, Kudamatsu and Seim, 2015, Fisman et al., 2020, Chu et al., 2021), we use *hometown ties*, that is, when newly appointed superiors and incumbent city leaders share the same birthplace, as a source of variation in promotion pressure.

We show that a hometown tie relaxes GDP-based promotion pressure and thereby affects land allocation, local government borrowing, and land prices. City leaders with hometown ties shift the composition of planned land supply away from industrial and toward residential use by six to seven percentage points, and they slow local government debt accumulation, leaving the outstanding stock about 28% below its no-tie counterfactual in the year after the tie forms. Because local debt was compounding at roughly 36% a year over this period, this amounts to about one year of forgone new borrowing, the cumulative effect of slowed accumulation rather than a paydown of debt already incurred. The response is compositional rather than expansionary. We do not detect a change in the *total* quantity of land supplied, and aggregate investment and infrastructure are unchanged, so the tie works through the *mix* of land uses rather than the size of the resource base. This weighs against alternatives such as additional central transfers or a general loosening of fiscal discipline. Prices move accordingly, with residential land prices falling by about 19% and industrial land prices rising by about 15%, the latter a secondary result. House-price growth also slows, by roughly five percentage points per year, though we treat this last result as suggestive corroboration rather than as an independent estimate. A randomization-inference placebo that permutes superiors' birthplaces within province confirms the pattern is not an artifact of the appointment process. None of 500 placebo reassignments reproduces our land, price, or debt estimates.

The reallocation also propagates into the real economy, with a lag. In the two years after a tie forms, the city's industrial growth engine winds down. Foreign direct investment falls across every measure we observe, as do industrial employment and the urban population, while residential housing reaching households appears to expand and aggregate output, rather than contracting, holds up and if anything edges higher as the reallocation matures. This apparent paradox, that output rises precisely when the growth incentive is removed, resolves once growth is decomposed into the margins a superior can credit. What rises is concentrated in construction, the mechanical byproduct of the residential land the freed leader now releases, rather than the visible industrial effort the tournament rewards, which in fact winds down. A city-level decomposition of GDP by sector shows this directly, with construction value added accelerating after a tie even as the rewarded industrial margins fall. Career incentives thus shape not the *level* of local growth so much as its *composition*. They steer it toward the visible, attributable industrial activity the promotion tournament can credit to a leader, such as foreign investment, factories, and debt-financed buildout, and away from residential uses it cannot. We read this as the leader-level footprint of Song and Xiong's "Mandarin" account of Chinese growth (Song and Xiong, 2026), in which the cadre tournament shapes the character of local development. Relaxing it shifts that character toward residential use. Because the tournament steered the composition of activity rather than its scale, output need not fall when the steer is removed. We develop this interpretation, and are explicit about its limits, in Section 5.4. In particular, we do not measure productivity, and we cannot fully separate the recomposition from provincial favoritism toward a tied city.

The paper makes four main contributions. First and most centrally, it provides causal micro-evidence for the career-incentive mechanism that the macro literature on China's "Mandarin" system places at the center of how local economies are governed (Song and Xiong, 2026). That literature works largely through theory and aggregate accounting, whereas we supply the corresponding evidence at the individual-leader level, isolating an exogenous, within-leader relaxation of promotion pressure and tracing its effect on the allocation of land, the debt and prices tied to it, and, ultimately, the recomposition of the local real economy. In doing so we connect to a broad literature on political connections and economic outcomes, in cross-country data (Faccio, 2006), in developing countries (Khwaja and Mian, 2005, Claessens, Feijen and Laeven, 2008, Acemoglu, Hassan and Tahoun, 2018), and in developed economies (Goldman, Rocholl and So, 2009, Cooper, Gulen and Ovtchinnikov, 2010, Amore and Bennedsen, 2013, Akey, 2015, Acemoglu, Johnson, Kermani, Kwak and Mitton, 2016, Schoenherr, 2019). In China specifically, personal ties shape outcomes ranging from promotion (Ru, 2018) and trade credit (Kong, Pan, Tian and Zhang, 2020) to favorable treatment in elite scientific selection (Fisman, Shi, Wang and Xu, 2018), lenient government monitoring (Chu, Fisman, Tan and Wang, 2021), corporate tax breaks that fade as a leader's promotion incentives strengthen (Cheng, Cao, Xia, Xie and Zeng, 2023), and protection from anticorruption enforcement (Griffin, Liu and Shu, 2022). At the very apex of the system, where connections invite scrutiny, ties can instead carry a penalty in selection to the Politburo (Fisman, Shi, Wang and Wu, 2020), a contrast consistent with favoritism operating at the local tiers of the hierarchy that we study. Closest to our setting, Wang, Zhang and Zhou (2020) model city leaders as "city developers" and show that stronger career incentives push them to expand cities outward at low density to raise land revenue, while Henderson et al. (2022) structurally estimate the weight leaders place on growth relative to resident welfare that drives a wedge between industrial and residential land supply. These studies establish that career incentives shape the quantity, spatial form, and modeled preferences behind land development. We complement them along three lines. Our outcome is the industrial-versus-residential *composition* of land use rather than its quantity or density. Our variation is an exogenous, within-leader *relaxation* of the growth-promotion incentive, the hometown tie, rather than cross-sectional career-incentive proxies or a calibrated preference. And we follow the resulting shift through prices, debt, and the recomposition of the local real economy, which we read as the leader-level footprint of the "Mandarin" growth mechanism. Career incentives shape what local growth is made of, not only how much of it there is.

Second, this paper contributes to the literature on the demand and supply factors behind China's housing boom. Previous studies have scrutinized demand factors extensively, such as speculative investment, status competition, credit expansion, and urbanization, as well as fundamental factors such as population, wages, income, and construction costs (Wang and Zhang, 2014, Chow and Niu, 2015, Fang, Gu, Xiong and Zhou, 2016, Wu, Gyourko and Deng, 2016, Chen and Wen, 2017, Wei, Zhang and Liu, 2017, Chen and Zhang, 2023). Some research suggests that supply-side factors may have been overlooked in explaining China's housing boom (Glaeser, Huang, Ma and Shleifer, 2017, Wu, Gyourko and Deng, 2016).

However, although the Chinese Communist Party has a monopoly on the country's urban land supply, the impact of China's political institutions on its housing market is not fully understood. Cai, Henderson and Zhang (2013) and Chen and Kung (2019) document that corruption is an important consideration in primary market land auctions. Most closely related, Chang, Wang and Xiong (2026) show that local governments in China actively manage residential land and house prices, through supply controls, land purchases by their financing vehicles, and limits on new-home sales permits, generating a divergence between prices and transaction volumes. Their mechanism and ours are complementary. They study how local governments *intervene* in housing and land markets, whereas we identify how the *career incentives* of city leaders, and their relaxation through hometown ties, shape the composition of land supply, the accumulation of local debt, and house-price growth in the first place. A separate, public-finance literature emphasizes local governments' *fiscal* incentives as the prominent determinant of land supply, pointing to fiscal pressure and land-based financing (Gong, 2012, Li, Hong and Huang, 2013, Han and Kung, 2015, Liu and Xiong, 2020), and most rigorously to He et al. (2025), who show that the large residential-industrial price gap is substantially rationalized by the future tax revenues that industrial land generates. Closest to our own mechanism, Zheng and Shi (2011) document the very cross-subsidy at the heart of our setting, in which local governments depress industrial land prices to court investment while restricting and pricing up residential land. They trace it to the dual pressure of fiscal need and political performance, though their evidence is cross-sectional and, like ours, finds its transmission to housing prices statistically weak. We view our channel as complementary to these accounts. Holding the fiscal fundamentals they emphasize fixed, we isolate the distinct, political-career determinant of land allocation causally, using the hometown tie as exogenous within-leader variation a correlational design cannot supply. The career-incentive channel also builds on the literature on China's political tournament. Politicians are constantly ranked by their relative performance among their peers, the principle of *yardstick competition* (Shleifer, 1985, Maskin, Qian and Xu, 2000), for which Chen, Li and Zhou (2005) provide direct evidence from provincial-leader turnover. We show that evaluating city leaders on relative economic performance within provinces has important implications for land supplies and housing price growth.

Third, our findings speak to the rapidly growing literature on local government debt and land finance in China. A large body of work documents the scale and risks of local government borrowing, much of it intermediated by local government financing vehicles and collateralized by land (Ambrose, Deng and Wu, 2015, Song and Xiong, 2018, Chen, He and Liu, 2020), and links land-based revenue to local fiscal and political behavior (Chen and Kung, 2016, Li, Hong and Huang, 2013). A parallel strand debates the macroeconomic footprint of this borrowing, namely whether local spending and debt crowd private investment in or out (Su, 2023, Huang, Pagano and Panizza, 2020). The literature has emphasized fiscal-pressure and stimulus-driven accounts of the debt buildup (Bai, Hsieh and Song, 2016), and recent regional evidence shows that shortfalls against growth targets trigger additional land sales and borrowing (Chang, Wang and Xiong, 2025), while Song and Xiong (2026) argue that career incentives foster over-leverage at the aggregate level. We provide direct, leader-level causal evidence for the latter. Because debt-financed investment is itself an instrument for delivering the GDP growth on which promotion depends, a relaxation of promotion pressure through a hometown tie leads leaders to curtail borrowing. The resulting reduction in local debt is robust to our most demanding specification and lies decisively outside the placebo distribution. It points to political career incentives as an underappreciated driver of local government debt accumulation, complementary to the fiscal-capacity and stimulus channels emphasized in prior work.

Fourth, the paper connects to research on how governments compete for business activity. In our setting, cheap industrial land is the natural instrument of local governments' GDP competition. City governments effectively subsidize land for the production sector, much as U.S. states bid for large firms with subsidies whose gains accrue largely to the firms (Slattery, 2025). Our evidence implies a parallel transfer in China. The promotion tournament shifts land value from residents toward production, a transfer that falls on what is typically the dominant asset on household balance sheets in emerging economies, China included (Badarinza, Campbell and Ramadorai, 2016, Badarinza, Balasubramaniam and Ramadorai, 2019). The transfer may exceed our reduced-form estimates. Because government land interventions amplify the pass-through of local shocks to housing prices (Zhang, Fan and Mo, 2017), the price wedge we measure understates the full incidence on residents.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting, namely the promotion system that governs city leaders' careers and the urban land market they control. Section 3 organizes these institutions into the chain of predictions the analysis follows. Section 4 details our data on city and provincial leaders, land transactions, land and house prices, local government debt, and real-economy outcomes. Section 5 presents the results. We document the dependence of promotion on GDP growth and its disappearance once a hometown tie forms, the reallocation of land across uses and the accompanying movements in land prices and debt (with house-price growth as suggestive corroboration), the event-study dynamics, and the placebo, specificity, and heterogeneity checks. We then weigh two alternative interpretations against the evidence and, finally, trace the propagation of the reallocation into the real economy. Section 6 concludes.

2 Institutional Background

Two institutions shape how a Chinese city leader allocates land. One is the cadre evaluation system that governs his career. The other is the local government's monopoly over urban land. We describe each in turn.

2.1 Career Incentives in the Party Hierarchy

The Chinese Communist Party governs through a four-tier hierarchy running from the central Politburo down through provincial and prefecture committees to the counties (the *political pyramid* of Figure 10). The provincial and prefecture tiers are the ones that concern us. A prefecture's party secretary, hereafter the *city leader*, heads the city government and sets local policy, including land allocation. Party personnel are de jure elected by congresses, but in practice the committee one level up controls appointment, promotion, and demotion. A city leader's career rests with the provincial leadership (Li and Zhou, 2005).

What that leadership rewards, above all, is economic performance. Provincial superiors rank city leaders by relative GDP growth, a yardstick competition that has made local output the currency of political advancement (Chen, Li and Zhou, 2005, Song and Xiong, 2026). Evaluation is not purely mechanical, however. Personal ties enter alongside performance, and a connection to a higher-ranking official can outweigh a stronger professional record. Among such ties the shared-hometown bond holds a special place. The term *lǎoxiāng*, people from the same place whether or not they have ever met, denotes a recognized claim to mutual obligation and favor (Fisman et al., 2018, Ru, 2018). When a newly appointed provincial superior is a city leader's *lǎoxiāng*, advancement ceases to hinge on out-growing one's peers. That is the variation we exploit.

2.2 The Urban Land Market and the City Leader's Choice

All urban land in China belongs to the state, and since the 1998 Land Management Law local governments hold the right to lease it. A city government is therefore the monopoly supplier of land in its jurisdiction, selling multi-decade usufruct rights, use rather than ownership, to firms and developers (Liu and Xiong, 2020). How much rural land a city may convert to urban use is not its own decision. The Ministry of Land and Resources sets a national quota and allocates it down through the provinces to the cities (Wang, Zhang and Zhou, 2020, Liu and Xiong, 2020). That quota is in turn disciplined by a farmland-protection target. A binding national floor of roughly 1.8 billion mu of arable land, the farmland red line, requires each city to offset any conversion of farmland to urban use with comparable farmland created inside its own jurisdiction, which raises the cost of expanding the urban footprint and makes the supply of developable land a constrained choice rather than a free one (Yu, 2024). The city leader's effective choice variable is thus not the *quantity* of land but its *composition*, that is, how a largely fixed total is divided among industrial, residential, and commercial use, with residential and commercial parcels sold by public auction (Cai, Henderson and Zhang, 2013). Within the city, land use is planned by a land-reserve and allocation committee on which the senior city leaders hold the decisive voices (Wang, Zhang and Zhou, 2020), and because the city's consequential decisions are taken in the party committee headed by the secretary, the secretary's preferences dominate how land is allocated (Yao and Zhang, 2015). Figure 12 shows that industrial and residential uses absorb the bulk of supply over our sample period.

This composition decision embeds a sharp tension. Industrial land is kept deliberately cheap. Cities routinely grant it at or below cost to attract manufacturers, who bring the investment, employment, and visible output that the promotion tournament rewards. Residential land, scarce and far more valuable, is the principal source of land-sale revenue. Over 2004–2015 residential land prices rose several-fold while industrial prices stayed nearly flat (Liu and Xiong, 2020). Tilting supply toward industry therefore trades current land value for growth credentials, a trade a leader makes only so long as growth advances his career.

Land revenue, and the debt built upon it, is the second reason the allocation is politically charged. The 1994 tax-sharing reform left local governments with heavy spending obligations but a smaller share of tax revenue. It pushed them toward land sales, whose revenue grew rapidly over the period (Figure 11), and, after 2008, toward land-collateralized borrowing through local financing vehicles (Han and Kung, 2015, Liu and Xiong, 2020, Song and Xiong, 2018). Land-related revenue has grown to roughly a third of local budgets (Figure 13). This fiscal-incentive account of land allocation, developed most directly by He et al. (2025), is real, and we hold it fixed. Government revenue and expenditure enter every full-control specification, and the estimates are insensitive to them. Our subject is the distinct, time-varying *career*-incentive margin, namely what a city leader does with land and debt when, for him, the growth tournament is switched off.

3 Conceptual Framework and Hypotheses

The institutional setting just described suggests a simple way to organize the analysis. Consider a city leader who allocates the city's land across industrial, residential, and commercial uses and decides how much debt-financed investment to undertake. Two considerations bear on these choices. The first is *fiscal*. Residential land yields large upfront sale revenue, whereas industrial land yields modest upfront revenue but a stream of future tax payments from the firms it hosts, and borrowing finances the infrastructure that supports development (He et al., 2025). The second, our focus, is the leader's *career* incentive. Promotion in the cadre system rewards the measured economic performance of one's jurisdiction (Li and Zhou, 2005, Song and Xiong, 2026). Industrial production and debt-financed infrastructure register directly in GDP and investment, and are visibly attributable to the leader's own effort, in a way that diffuse, market-driven residential land appreciation is not. A leader who weights promotion heavily therefore has reason to tilt land toward industrial use and to borrow aggressively, beyond what the fiscal calculus alone would dictate. This is a multitasking logic in the sense of Holmström and Milgrom (1991). When promotion is scored on the achievements a superior can measure and credit, the leader tilts toward the creditable industrial margin and away from the less-creditable residential one. The premise has direct counterparts in the literature. Governments over-provide public goods whose outcomes evaluators can observe and attribute to official competence (Mani and Mukand, 2007). Within China's cadre system specifically, city spending tilts toward visible above-ground projects such as roads, bridges, and landscaping, and away from valuable but invisible below-ground infrastructure, precisely for promotion-eligible leaders and around evaluation windows (Wu and Zhou, 2018). And the promotion mechanism demonstrably rewards short-horizon, readily evaluable investments in construction over education and health (Persson and Zhuravskaya, 2016). We treat this *attributability* premise as an assumption grounded in the institution, and we return to its evidentiary status, and to what the promotion regressions can and cannot establish about it, in Section 5.4. The two motives are ordinarily entangled, and the career incentive is unobserved.

Our research design isolates the career incentive. When a city leader's superior is replaced by a newly appointed official who shares the leader's birthplace, a *hometown tie* forms, a salient and well-documented basis for favoritism among Chinese officials (Fisman et al., 2018, Ru, 2018). We show in Section 5 that such a tie sharply raises the city leader's promotion probability while severing the link between promotion and GDP. Because the superior's appointment is decided from above, at a time and for reasons unrelated to any individual city's land trajectory, a tie acts as a discrete, plausibly exogenous relaxation of the GDP-promotion incentive for an otherwise-comparable leader, holding the city's fiscal fundamentals fixed. This logic yields a connected chain of predictions, and the empirical analysis follows the chain rather than testing each link in isolation. The natural starting point is the first link. If a hometown tie genuinely relaxes the career incentive, it should raise the leader's promotion odds while breaking the dependence of promotion on GDP. With that established, the central behavioral prediction follows. A leader freed from growth pressure should rebalance land away from industrial and toward residential use. The *form* of this response is itself revealing. If career incentives change what leaders do with a given stock of resources, rather than how much they command, the reallocation should be compositional. The total quantity of land released, and the city's aggregate investment and immediate scale of activity, should be largely unchanged. That pattern would set the career channel apart from scale-based alternatives, such as additional central transfers or a general loosening of fiscal discipline, which would instead move totals. The reallocation should also leave a price signature, with residential land becoming cheaper and industrial land dearer. And because the same growth motive drives debt-financed investment, tied leaders should borrow less. Effects on house-price growth, a step further removed and mediated by the slow conversion of land into housing, should be present but more muted. The reallocation should also leave a footprint on the real economy itself, with a lag. The visible industrial engine the tournament rewards, foreign investment and industrial employment, should wind down, residential activity should expand, and, because the reallocation shifts the composition of activity rather than shrinking it, aggregate output should not contract, and may even edge up. Two further implications discipline the interpretation. The effects should arise only for ties that actually move promotion incentives, not for other social connections that leave the promotion-GDP link intact, and they should be strongest among leaders with the most to gain from the tournament. We develop these implications in the order in which they build the argument, letting each result motivate the next.

4 Data

In this section, we first detail the data collection process of Chinese politicians' biographical data, followed by procedures we employed to construct the measure of political promotion. We then explain city-level real estate, land supply, and other macroeconomic data from various sources and present relevant descriptive statistics.

4.1 Political Data

We combine two sources of data to construct our sample on Chinese politicians. Our main source of political data, the Chinese Political Elite Database (CPED), contains extensive demographic information for provincial/prefectural politicians.

Politicians in CPED include all city party secretaries and mayors between 2000 and 2015, all standing committee members between 2000 and 2012, all provincial party secretaries and governors between 1995 and 2015, and all other full and alternate CCP Central Committee members between 1997 and 2012. We manually extend CPED to 2015 to include all prefectural, provincial, and national leaders of interest.[1]

CPED records the start and end dates, workplaces, and political ranks of every job assignment in each politician's career, hand-collected from government websites, yearbooks, and other credible sources. We verify it against the Provincial and City Leader Database maintained by CSMAR and resolve any disparities manually against official government sources.

In short, we observe the universe of all city and province party committee leaders who assumed office between 2001 and 2015. For each leader, we obtain his/her biographical information and the entire career path, including educational background. There are 1,636 city-term observations between 2001 and 2015, which map to 1,302 Chinese politicians. Of these, 636 completed leader terms begin within our 2003–2015 analysis window, and 572 have measured GDP growth over the term. Dropping observations that are singletons within the fixed-effect cells, along with a handful of missing demographics, leaves the promotion estimation sample of 411 terms summarized in Table 1.

4.1.1 Promotion

The Chinese government is a highly unitary institution. Instead of elections, promotions of provincial and prefectural politicians are decided by government officials of higher political ranks. The power structure is the pyramid described in Section 2, namely the 25 Politburo members at the top, the provincial CCP committees below (13 members each, 34 provincial divisions in total), and the prefecture-level committees at the base (10–11 members each, 334 prefecture divisions in total). Leaders are generally promoted up this ladder level by level, and the promotion tournament is local. A city leader's performance is evaluated relative to other city leaders from the same province. We focus on city leaders in this paper and follow a simple rule of thumb to construct our promotion data, based on the explicit political hierarchy

[1] We focus on the period before 2015 because since 2016, the Party Central Committee led by Xi Jinping has designated "housing is for living, not for speculation" as a long-term national strategy, and as a result, various local policy restrictions have been implemented to curb housing prices.

in China[2] Based on the observed career paths of politicians in the data, we define a government official as promoted if he is appointed to a new position with a higher political rank within three months of the end of his term in office.

Fifty-six city leaders in our sample were dismissed during their political careers, commonly for corruption, which could introduce biases to our estimates Cai, Henderson and Zhang (2013), Fang, Wu, Zhang and Zhou (2022). We exclude these politicians from the main analysis.

4.1.2 Promotion for Local Communist Leaders

There has been an extensive literature on the Chinese government's personnel control. Li and Zhou (2005) find that for the turnover of provincial leaders, GDP performance, age, central connections, and years in office are among the most influential factors. At the city level, Yao and Zhang (2015) show that individual leaders matter for growth and that abler leaders are more likely to be promoted. Our promotion regressions condition on a similar set of characteristics, including education.

4.2 Real Estate Data

We obtain urban planning and land sales data aggregated at city level from China Real Estate Index System (CREIS) database maintained by China Index Academy. CREIS collected land transaction data from the Ministry of Land and Resources, and local Land Reserve Centers.

We restrict our sample period from 2003 since the marketization of urban land supply in China took off in that year Liu, Cao, Yan and Wang (2016). Information available in CREIS includes acreage, price and land use type for each parcel of land.

[2]The literature on the Chinese promotion system has not yet reached a consensus on the definition of promotion Tao, Su, Lu and Zhu (2010). The ambiguity stems largely from China's informal orders of precedence, which are not formally published and vary by person and period. The Chinese government does, however, maintain a regimented system of political ranks, which serves as a rough order of precedence in official protocol and which we adopt for our empirical exercise.

4.3 Macroeconomic Series

National Bureau of Statistics of China (NBS) maintains annual macroeconomic series at the city level. For each city, we collect population (measured by usual residence), government in-budget revenue and expenditure, total city-wide deposits, average wage, GDP, secondary- and tertiary-industry GDP, fixed investment, floor area sold for residential building, residential housing prices, and real estate investment by year during 2003–2015. Summary statistics for the land-market variables are reported in Table 2 and for the macro series in Table 3, and all price-related variables are normalized to a base year using the CPI published by NBS. To maintain a balanced sample, we keep cities observed for at least half of the sample period. The 195 cities included in this macro sample account for around 90% of all floor area sold nationwide. The land-market regressions draw on prefecture cities with CREIS land-transaction coverage, namely up to 191 cities after excluding the four direct-controlled municipalities, of which 179 enter the baseline land panel once dismissed leaders and missing outcomes are also excluded (Table 2, and pooling the municipalities back in is examined in Appendix Table 13). Sample sizes in the tables vary across outcomes with data availability.

For house prices, our primary measure is the constant-quality residential price index of Fang et al. (2016), who construct hedonic, repeat-sales-style indices from transaction-level data for roughly 120 prefecture cities. As Fang et al. (2016) document, the official NBS average-price series conflate genuine price changes with shifts in the size, quality, and geographic mix of transacted units, so a constant-quality index is needed to measure house-price growth accurately. We therefore use the Fang et al. (2016) index wherever it is available (through 2012) and splice on the NBS city-level residential price series for the remaining years of our sample, for which official coverage and quality have improved. House-price growth is the annual log change in this spliced index.

4.4 Local Government Debt

Our measure of local government debt is the city-level panel of total debt owed by local government financing vehicles constructed by Huang, Pagano and Panizza (2020). Municipalities in our sample period could not borrow from banks or issue bonds in their own name, so local borrowing ran through these vehicles, which the city capitalizes with transferred assets, usually land. The panel is assembled from the vehicles' balance sheets, which any vehicle applying to issue a bond must disclose for the current year and at least the three preceding years. It therefore captures total interest-bearing debt, namely short-term and long-term borrowing, notes payable, bonds outstanding, and other current liabilities, and not merely bond finance. Bank loans account for the bulk of this debt, with bonds rising from 6 percent of the total in 2006 to 21 percent in 2013 (Huang, Pagano and Panizza, 2020). Aggregated nationally, the series tracks the official National Audit Office figures to within five percent in 2012 and 2013 and matches their distribution across provinces, while remaining a conservative lower bound because vehicles that never tap the bond market are not observed. The panel covers 2006 through 2013, and coverage conditions on disclosure. A city-year enters only when some local vehicle's balance sheet is observed, and the debt regressions further control for the amount-weighted average coupon on the city's bond issues in that year, a summary measure of the price of credit the city faces, which is observed only in years with new issuance. The resulting regression sample contains 359 city-year observations, concentrated in the later years of the period as bond issuance, and with it disclosure, expanded. Our estimates therefore speak to the intensive margin of the debt stock conditional on observed borrowing by the city's vehicles. Uncovered city-years reflect the absence of disclosure rather than the absence of debt, which is why we do not impute zeros to them. The outcome in our regressions is the logarithm of the outstanding debt stock in a city-year. Debt is a stock with substantial inertia (the within-city-term first-order autocorrelation is 0.60), which motivates the lagged-tie timing convention discussed in Section 5.

4.5 Real-Economy Outcomes

Section 5.4 examines a set of real-economy outcomes drawn from CEIC's prefecture-level China statistical series, merged to our sample by province and city name, namely foreign direct investment (utilized value, contracted value, and the number of contracts), sectoral employment and output, non-agricultural population, and residential floor space sold. Within-window coverage varies by series, namely roughly 1,000 to 1,550 city-year observations for the headline outcomes in the event-study samples. Appendix Table 16 reports every outcome and transformation we examined, and per-outcome sample sizes for the confirmed set are in Table 11. The interpretation checks of Section 5.3.4 draw on two further sources. We proxy net fiscal transfers from above with the ratio of general-budget expenditure to own revenue, both already in the analysis panel, and we measure municipal-infrastructure investment by funding source, with central appropriations itemized separately, from the Urban Construction Statistical Yearbook for 2006 through 2015. For the sectoral decomposition of GDP we draw city value added by industry from provincial statistical yearbooks compiled by EPS China Data. For the real-economy decomposition of Section 5.4, these report the split of secondary-sector value added into industry and construction for far more cities, and earlier years, than the national series. The first-stage robustness of Section 5 uses the broader primary, secondary, and tertiary contributions, because the finer industry-construction split is too thinly observed at the leader-term frequency to enter the promotion regression.

5 Empirical Results

The analysis proceeds along the chain of reasoning set out in Section 3. We begin with its first link, namely whether a hometown tie genuinely relaxes a city leader's career incentive, because everything downstream depends on it. We then follow the leader's response, that is, how the relaxed incentive reshapes the allocation of land, whether that response operates through composition or scale, and how it surfaces in land and house prices and in local borrowing. We next subject the design to a battery of identification checks, namely a randomization-inference placebo, a specificity test against other social ties, and heterogeneity by promotion stakes, then weigh two alternative interpretations of the pattern, and, last, trace whether these political choices propagate into the real economy itself. At each step we let the evidence motivate the next question.

5.1 The Tie Decouples Promotion from Growth

Everything downstream rests on one premise, namely that a hometown tie genuinely alters the career incentive a city leader faces. We establish it directly. The promotion of city leaders is widely held to reward economic performance. If a hometown tie blunts that link, then once a tie is present the growth-promotion relationship should weaken or disappear. We therefore regress a leader's end-of-term promotion outcome on GDP performance, a hometown-tie indicator, and their interaction,

$$Y_{i,t} = \beta_0 + (\beta_1 + \beta_2 \mathit{Hometown\ Tie}_{i,t}) \times \text{GDP Growth}_{i,t} + \beta_3 X_{i,t} + \epsilon_{i,t} \tag{5.1}$$

where $Y_{i,t}$ is a promotion dummy for leader i whose term ends in year t, and each observation is one completed leader term. GDP growth is defined as the annualized GDP growth rate from when the leader took office until the term ended. Table 4 reports the estimates, and Figure 1 displays the relationship they capture. Promotion is strongly increasing in growth for unconnected leaders and essentially flat for tied leaders.

Hometown tie is an indicator variable that takes the value one if and only if the following scenario happens. During the city leader's term, the central government appoints a new provincial leader to the province in which the city leader works, and the newly appointed provincial leader has the same city of birth as the city leader.[3,4] As mentioned, city-level Chinese politicians' promotion decisions are made by higher-level communist officials in the same province. The appointments of new provincial leaders during a city leader's term are made confidentially by the central party committee, and are hence unexpected by the city leaders in office. To rule out the possibility that a provincial leader favors his hometown comrades by appointing them endogenously, we restrict the definition of a hometown tie to city leaders who had already assumed office when the tie was established (i.e., the provincial leader is appointed *after* the city leader assumed office). The indicator equals one in the year the tie forms and the following year, the timing convention used throughout the tables. Given the salience of the hometown bond among Chinese officials (Section 2), we conjecture that sharing a hometown with the provincial leader shifts a city leader's promotion prospects, and hence the career incentive he faces.

β_1 indicates the correlation between GDP growth and promotion outcome, in the absence of any hometown tie. β_2 captures the effect of hometown tie on the sensitivity of promotion to GDP growth. A positive β_2 indicates that the effect of GDP on promotion is enhanced in the presence of a hometown tie, whereas a negative estimate for β_2 means a hometown tie diminishes the effect of GDP growth on promotion.

All specifications include a comprehensive set of fixed effects to control for unobserved characteristics that might affect a leader's promotion probability beyond the GDP-promotion linkage. City fixed effects take out level differences across cities in the likelihood that their leaders are promoted. With province-year fixed effects, our estimates compare city leaders evaluated by the same provincial committee in the same year. We further include demo-

[3]Unlike in the U.S., the vast majority of city leaders are not local to the cities they govern. Between 2003 and 2015, only 5% of city leaders were born in the same city they managed. Only 0.5% of leaders had a hometown tie and were local.

[4]The four direct-controlled municipalities (Beijing, Shanghai, Tianjin, and Chongqing) have no provincial superior, namely their party secretaries hold provincial rank and are evaluated and promoted directly by the central leadership. For these four cities we define the hometown tie analogously at the central level, equal to one when a newly appointed Politburo member shares the municipality leader's birthplace. Because their promotion tournament operates at the central rather than provincial level, the baseline outcome samples exclude the four municipalities. Pooling them back in, with ties defined analogously against newly appointed Politburo members, leaves every result essentially unchanged (Appendix Table 13). The promotion estimation sample contains no municipality leader-terms, so the promotion column of that table is identical by construction.

graphic controls such as age and gender, which previous studies have argued are important factors in promotion decisions Wang, Zhang and Zhou (2020), and, to address the concern that the hometown tie might pick up leadership qualities specific to certain regions, birth-province fixed effects. Column (2) adds turnover fixed effects, comparing leaders who assumed their posts in the same year. Column (3) controls for the rank of the city leader, given that leaders of different ranks face different promotion odds, and column (4) further controls for whether the leader ever faced a disciplinary action. The coefficients of interest, namely those on GDP growth, the hometown tie, and their interaction, remain precisely estimated across all specifications.

Higher GDP growth is associated with a greater likelihood of being promoted. In the absence of a hometown tie, the estimated coefficient on annualized GDP growth (column 4) is sizeable. Across leader terms the standard deviation of annualized GDP growth is about 7 percentage points. Scaled by this standard deviation, a one-standard-deviation increase in growth raises a leader's promotion probability by about 12.7 percentage points, large relative to the 35% promotion rate of untied leaders, all else equal and absent a tie.

A hometown tie substantially reduces the importance of GDP performance in someone's evaluation for promotion. As the estimates in column (4) show, having a hometown tie cuts the sensitivity of promotion to GDP by 3.416. In fact, the sum $\beta_1 + \beta_2$ is not statistically distinguishable from zero. The null hypothesis that a hometown tie completely obviates the importance of GDP growth to a leader's promotion outcome is not rejected under a Wald test at the 0.1 significance level.

Conditional on performance and the fixed-effect comparisons, a hometown tie raises a leader's promotion odds. The estimate on the hometown-tie dummy is positive and statistically significant in every column, at the 5% level in columns (1) through (3) and at the 1% level in the full specification of column (4). Two clarifications guard against misreading these magnitudes. First, the main effect (0.625 in column 4) is the tie's effect evaluated at zero GDP growth. Because the interaction is negative, the tie raises promotion odds for any growth record below roughly 18% annualized growth (0.625/3.416), a threshold exceeded by about a quarter of sample growth records, with the boost largest for leaders with the weakest records. Averaged over the sample growth distribution, the marginal effect of a tie is +0.13 (s.e. 0.09), positive though imprecisely estimated. Second, the *unconditional* promotion rate is in fact lower for tied than untied leaders (0.25 versus 0.35, Table 1). The raw gap reflects composition rather than the tie's effect. A tie can only form when a new superior arrives mid-term, so tied terms are mechanically longer (4.8 versus 4.0 years), and longer terms are associated with worse promotion outcomes, while the regression's fixed effects compare leaders within the same province-year and cohort. Conditional on these comparisons, the tie helps. The literature on the promotion system for Chinese communist leaders still debates hometown favoritism (Kung and Zhou 2021, Fisman et al. 2018) versus faction control Francois, Trebbi and Xiao (2023). The estimates here support hometown favoritism among city-level party leaders.

Two facts emerge, and together they license the rest of the paper. Absent a tie, promotion is strongly increasing in GDP growth. Developing the local economy is the surest path to advancement. A hometown tie then eliminates that sensitivity. The growth-promotion link, so pronounced for unconnected leaders, is statistically indistinguishable from zero once a tie forms. A tie thus does exactly what our framework requires of it, namely it relaxes the GDP-promotion incentive facing an otherwise-comparable leader, without our ever having to observe that incentive directly. This sets up the question that occupies the rest of the paper. What does a leader do with the land and credit at his disposal once the pressure to manufacture measured growth is lifted?

Before turning to that question, we situate the first stage in a contested literature on Chinese political selection. Li and Zhou (2005) document a growth-promotion link for provincial leaders, while Shih, Adolph and Liu (2012) find little robust performance gradient for Central Committee members, and Jia, Kudamatsu and Seim (2015) find that connections and growth act as *complements* in provincial-leader promotion, the opposite interaction sign from ours. Three design differences plausibly account for the contrast. We study prefecture leaders inside the explicit within-province tournament, not provincial leaders evaluated at the center. Our connection is a tie to the leader's *direct evaluator* that forms mid-term through the timing of the superior's appointment, not a pre-existing factional alignment that may itself reflect selection. And our outcome is promotion at the end of the very term the tie interrupts. A tie to one's current evaluator substitutes for the performance statistic that evaluator would otherwise rely on, which is precisely what Table 4 measures, whereas the complementarity documented for factional ties operates at selection into higher office, where patrons elevate proteges who have also demonstrated competence. The two findings are not in conflict. They describe different margins of the same system. More directly, Chen and Kung (2016) show the growth-promotion link is itself relaxable, finding that land-revenue windfalls accruing to county leaders weaken the reward to GDP growth and shift promotion toward signaling and political connections. That conditionality is exactly what our design exploits, here through a cleaner, plausibly exogenous source of variation, a hometown tie to the leader's direct evaluator formed by the timing of the superior's appointment.

Because the object we estimate is the within-province-year *difference* in the growth-promotion slope between tied and untied leaders evaluated by the same provincial committee, rather than the average gradient itself, it is insulated from any level shift in that gradient. A decomposition by GDP component (Appendix Table 18) confirms that the gradient we estimate localizes on the secondary sector that the attributability logic of Section 3 predicts, rather than surviving as a diffuse aggregate, a localization that a merely spurious aggregate correlation would not produce.

5.2 The Leader's Policy Response: Land, Prices, and Debt

We now follow the leader's response, tracing the margins the framework flagged, namely the allocation of land, land prices, local debt, and house-price growth, as a hometown tie forms. The specification is deliberately spare. We regress each outcome on the hometown-tie status of city i in year t,

$$Y_{i,t} = \beta_0 + \beta_1 Hometown\ Tie_{i,t} + \beta_2 X_{i,t} + \epsilon_{i,t} \tag{5.2}$$

where $X_{i,t}$ collects the controls. The key coefficient is β_1 on the hometown tie.

All specifications include city-term and province-by-year fixed effects, and we then add (log) GDP, (log) resident population, and (log) government in-budget fiscal revenue and expenditure cumulatively. The tie coefficient barely moves at any step, which indicates that neither economic fundamentals nor fiscal conditions drive the results. For the land and debt outcomes, our preferred specification adds year-within-term fixed effects (labeled "Turnover FE" in the tables), which absorb the mechanical evolution of outcomes over a leader's tenure. The house-price specification, for reasons discussed below, omits them.[5]

Reverse causality would require the Politburo to select provincial appointees on the basis of a single city's land market, and to do so through the accident of a shared birthplace with that city's leader. Provincial appointments are made centrally, for province- and faction-level reasons, and precede the outcomes we study. Section 5.3.1 tests this exogeneity claim directly by permuting superiors' birthplaces. The remaining concern is anticipation. If land and housing markets moved in advance of an expected hometown connection, our estimates would understate the response. The event-study analysis below addresses this by displaying the dynamic timing directly, allowing the reader to verify that the movements are concentrated in the years following tie formation rather than preceding it.

5.2.1 Land Reallocates from Industrial to Residential Use

The most direct thing a leader controls is the land the city releases, so we begin there. If the career incentive tilts land toward industry, the use most directly tied to the GDP that promotion rewards, then relaxing it should tilt land back toward residential use. This is exactly what we find. The estimates for land shares (Table 7) and land prices (Table 8) are precise and robust.

Consistent with this, residential land supply, measured as the ratio to total land supply, increases with hometown tie, whereas industrial land supply decreases. The estimates are

[5] We have also estimated specifications that interact the hometown tie with the leader's year-within-term, allowing the response to depend on how far into the term the tie is active, since a tie late in a term may signal that a promotion decision is near. For every outcome the interaction terms are jointly insignificant and the implied average effect is essentially unchanged from the homogeneous estimate, so we report the more parsimonious specification.

notably stable across specifications. With no controls (column 1 of each panel), a hometown connection raises the residential share by 5.1 percentage points and lowers the industrial share by 5.9 percentage points. The coefficients barely move as the full control set is added (column 3 of each panel). This insensitivity to controls is reassuring, because the estimate does not depend on conditioning on covariates that are themselves plausibly affected by the political-incentive channel, so it is unlikely to be an artifact of "bad controls." In the preferred specification (column 4 of each panel), the effects are somewhat larger, the residential share rises by 6 percentage points and the industrial share falls by about 7 percentage points, and significant at the 5% level. The same pattern holds measuring shares by land area rather than building area (Appendix Table 21).

A natural next question, and one the fiscal literature has long pressed, is whether relaxed career pressure shows up in the land-sale *revenue* on which local budgets lean. A body of work on China's land finance holds that the growth incentives facing local leaders are part of what drives their reliance on land sales (Han and Kung, 2015, He et al., 2025, Liu and Xiong, 2020, Song and Xiong, 2018). To our knowledge the link has not been identified causally, and the hometown tie offers a way to probe it. Here our evidence is inconclusive. Table 9 regresses total land sales revenue, and revenue by use type, on the contemporaneous tie and its one- and two-year lags. Apart from a marginally significant one-year lag on total revenue ($p < 0.1$), no coefficient on total or residential revenue reaches significance, and the cumulative effects are insignificant throughout (total cumulative -0.19, joint $p = 0.41$). We read this as a limit of what our design can resolve rather than as evidence against the channel. Sales revenue is the product of price and quantity, is measured with considerable noise, and moves with a lag we cannot pin down precisely. The composition and pricing of land, which a leader controls more directly, respond clearly, whereas the downstream revenue is a margin better suited to data with finer transaction detail than ours. We return to the corresponding event-study dynamics, which are correspondingly noisy, in Section 5.2.4.

The effect is compositional rather than expansionary. A central feature of our findings is that the hometown tie reallocates land *across* use types while we do not detect a change in the *total* quantity of land a city makes available. Table 10 reports a regression of (log) total planned land supply on hometown tie. Across all specifications, the coefficient on tie is close to zero and statistically indistinguishable from it, at -0.050 with no controls beyond the fixed effects, -0.035 adding (log) GDP and (log) population, -0.035 with the full control set, and -0.035 in the preferred specification with year-within-term fixed effects, all with $|t| \leq 0.42$.

We read this as a point estimate near zero rather than as a precise null that rules out any change in total supply. The total-supply regression is estimated on essentially the same panel as the share regressions (516 versus 485 city-year observations, the difference reflecting outcome availability) and with the same two-way-clustered standard errors, and its point estimates sit close to zero, but the confidence interval is wide enough to admit moderate movements in the total. What is tightly estimated is the *compositional* shift, under the same specification and clustering. The total-supply estimate therefore tells us that the response operates mainly through the mix of land uses rather than through a clearly measured expansion of the overall allocation, and we test the scale-based alternatives it gestures toward, namely that tied leaders draw more transfers, credit, or political capacity to release land, directly and far more precisely in Section 5.3.4.

This compositional pattern carries three implications. First, it identifies the margin on which behavior moves most clearly, namely the relative shares of residential, industrial, and commercial land rather than the total quantity released. Second, it weighs against scale-based interpretations in which the tie mainly brings a large expansion of land resources, additional political capacity to release land, or a broad loosening of fiscal discipline, because such mechanisms would predict total supply to expand alongside the compositional shift, whereas the point estimate is near zero. We do not rest this conclusion on the total-supply estimate alone, since its interval is wide, but on the direct tests of the resource channels in Section 5.3.4. The same compositional reading holds for the broader real economy on impact. Contemporaneously the tie moves none of fixed-asset or real-estate investment, infrastructure (paved-road area), or secondary-industry output, while annual GDP growth is, if anything, marginally *higher* (+1.5 percentage points, significant at 5%, Appendix Table 15), a first hint of the delayed recomposition documented in Section 5.4. Tied leaders do not simply coast. They re-optimize the *composition* of land use and curtail debt while leaving the scale of other inputs, namely investment and infrastructure, unchanged. As we show in Section 5.4, output growth then edges up at a two-year horizon even though the resource base does not expand. Third, the compositional pattern justifies using land shares, rather than absolute quantities, as our headline outcomes. Shares are the margin on which behavior actually moves.

Table 8 then investigates how land prices respond when the proposed career incentive channel is shut down. Accompanying increased residential land supply and decreased industrial land supply, the unit price of residential land (per building area) falls while the unit price of industrial land rises. In the full-control specification (column 3 of each panel) the residential land price drops by about 23% (significant at 1%) and the industrial price rises by about 12% (significant at 10%). In the preferred specification (column 4 of each panel) the residential decline is about 19% (significant at 5%) and the industrial increase about 15% (significant at 10%). The residential-price decline is the more direct counterpart to the land-supply reallocation, and it is the price result the randomization placebo of Section 5.3.1 validates decisively. We treat the industrial-price increase as secondary, since it is significant only at the 10% level, is not separately placebo-tested, and is null in the event study below.

5.2.2 Tied Leaders Curtail Borrowing

The same growth imperative that tilts land toward industry also drives borrowing, because debt-financed infrastructure is among the most direct ways for a leader to manufacture the measured output that promotion rewards. Relaxing the career incentive should therefore slow the accumulation of debt, and it does. Because the level of debt is a stock with substantial inertia (the within-city-term AR(1) coefficient is 0.60), the contemporaneous tie cannot move it within a single year, and we therefore use the one-year-lagged tie. Columns (1) through (3) of Table 6 show that a hometown tie reduces the (log) level of local debt, with a coefficient near -0.32 (about a 28% reduction) that is stable as controls are progressively added, statistically significant at the 5% or 10% level. Adding year-within-term fixed effects to the full-control specification (column 5) leaves the result intact and sharpens it (a coefficient of -0.37, about a 31% reduction, significant at 5%), and it survives the placebo test of Section 5.3.1 decisively. As we discuss under economic magnitudes below, this is the cumulative consequence of slowed borrowing over the tie's life rather than a one-time repayment.

Column (4) reports a distributed-lag specification including the contemporaneous, one-year-lagged, and two-year-lagged tie. The contemporaneous tie has no detectable effect on debt (coefficient -0.13, statistically insignificant), consistent with debt's stock nature. The one-year-lagged tie produces about a 28% reduction in debt (significant at the 10% level in this specification), and the two-year-lagged tie a further reduction of about 15% (significant at the 5% level). The pattern is consistent with borrowing decisions taking a year to translate into debt-stock changes and the effect persisting into year two before attenuating.

The choice of lagged versus contemporaneous tie is supported by Appendix Table 12, which estimates the contemporaneous (L_0) and one-year-lagged (L_1) tie jointly for each main outcome. Most land-share and land-price outcomes load on the contemporaneous tie, namely the residential share at $L_0 = 0.054$ ($p < 0.05$) and the residential price at $L_0 = -0.22$ ($p < 0.01$), with the industrial share pointing the same way ($L_0 = -0.045$) though imprecisely. Commercial land share is the exception, in that it loads on the lag, with $L_1 = -0.040$ ($p < 0.01$) and L_0 statistically indistinguishable from zero (static commercial-land results are collected in Appendix Tables 22 and 23). This dynamic profile reconciles the contemporaneous static regressions in the main tables (which average over years and find commercial null) with the event-study evidence in Section 5.2.4 (which isolates the year-of-effect and finds commercial highly significant). Outcomes that accumulate over time, the debt level in particular, load on the lag rather than the contemporaneous tie, motivating the lagged-tie specification used for debt in Table 6. Finally, the borrowing cut is specific to the local government's own financing-vehicle debt rather than part of a broader credit contraction. Event studies of in-budget revenue and expenditure and of city-wide bank loan and deposit stocks around the tie show no statistically significant declines (untabulated, and if anything, city-wide loan growth ticks up). What the forgone borrowing would otherwise have financed, most plausibly off-budget, debt-financed investment by financing vehicles, is not separately observed in our data.

Economic magnitudes. Before turning to the housing market itself, it is worth pausing on the size of these effects, which are large relative to the variation they act on. Across city-years the residential land share averages about 50% (with a cross-sectional standard deviation of 17 percentage points) and the industrial share about 32%, measured as shares of planned building area. A hometown tie moves roughly six to seven percentage points between the two uses, on the order of two-fifths of a standard deviation of either share. The debt response is comparably sizable. Local government debt grew at an average of about 36% per year over our sample period, so the reduction that follows a tie, about 28% in the specifications without year-within-term effects and 31% in the preferred specification the placebo validates, is on the order of a full year of borrowing forgone, the cumulative effect of a leader slowing new borrowing rather than retiring debt already incurred. The channel is also not confined to a handful of cities. An active hometown tie is present in 8–9% of city-years (Table 2), so it operates across a meaningful slice of the sample rather than a few outliers. Together with the roughly 19% decline in residential land prices, the picture is of a politically-driven reallocation large enough to matter for aggregate land and credit outcomes, not a statistical curiosity.

5.2.3 House-Price Growth Slows, Suggestively

The final and most downstream link in the chain is the housing market itself. If a tie makes residential land more abundant and cheaper, housing should eventually become cheaper too. This is the hardest link to pin down, precisely because land takes years to convert into housing. Table 5 shows the estimated effect on a city's house-price growth rate. The specification uses the contemporaneous tie indicator (whose definition carries a one-year window) together with two lags of the dependent variable, since house-price growth is strongly autocorrelated. In columns (1) to (3), (log) GDP, (log) resident population, and government fiscal conditions are added gradually to check the robustness of the estimates. Throughout these specifications, the estimate for hometown tie remains similar in magnitude.

Estimates in Table 5 indicate that the hometown-tie channel is associated with slower house-price growth. Column (3) implies a decline of approximately 5.1 percentage points per year, economically meaningful relative to average house-price growth over the sample period. We emphasize, however, that this is the *least robust* of our findings, and we rest no causal claim on it. The coefficient attenuates toward zero and loses statistical significance once year-within-term fixed effects are added (column 4). It does not clear the randomization-inference placebo of Section 5.3.1. And its event-study counterpart shows no significant post-tie effect and a marginally significant deep lead. Part of the attenuation is mechanical, in that a slow, delayed price response is absorbed by a single contemporaneous coefficient that also nets out year-within-term variation, but the placebo points to a deeper issue, namely within-term mean reversion in price growth generates negative estimates even under random ties. We therefore treat the house-price result as suggestive corroboration of the land-supply and land-price results rather than as an independent causal estimate. The cleaner and more directly measured price evidence is the decline in residential *land* prices in Table 8, which clears the placebo decisively. The result is at least not an artifact of our splicing the constant-quality price index of Fang et al. (2016) (available through 2012) with the NBS city series thereafter. Restricting the sample to the constant-quality index alone (2003–2012) yields a similar estimate, if less precisely estimated on the smaller sample.

5.2.4 Dynamics and Pre-Trends

The static estimates establish the magnitudes. We now examine their timing, which also furnishes our first identification check. If a hometown tie causes the reallocation, the outcomes should move *after* the tie forms and not before, whereas a pre-existing trend would instead point to reverse causation or anticipation. To see this, we estimate an event study around the year a hometown tie first forms in a city. We measure event time at the *city* level, $\tau = t - t_c^*$, where t_c^* is the year of city c's first hometown tie. Measuring event time at the city level, rather than within a single leader's term, allows the city's pre-tie years, *including those served by the previous leader*, to populate the leads at $\tau = -2$ and $\tau = -3$, which a within-term clock leaves too sparse to identify. The cross-leader level differences this introduces (an incoming leader may operate the city at a persistently different level) could in principle contaminate the dynamic coefficients, and we neutralize them by including a separate city-term fixed effect for *each* leader, which absorbs leader-specific level shifts while the city-level event clock supplies the within-city event-time variation. The comparison group is city-term-years in the same province-year without an active tie.

Specifically, we estimate

$$Y_{i,t} = \sum_{\tau \neq -1} \beta_\tau \, \mathbb{1}\{\text{event time} = \tau\}_{i,t} + \alpha_i + \alpha_{p,t} + \alpha_s + \varepsilon_{i,t}$$

where i indexes city-term, t indexes year, the displayed event-time dummies cover $\tau = -3$ to $+2$ (with $\tau = -1$ omitted as the base), α_i is a city-term fixed effect (one per leader), $\alpha_{p,t}$ is a province-by-year fixed effect, and α_s is a year-within-term fixed effect. Following standard event-study practice we omit time-varying controls, which are partly endogenous to the political-incentive channel and would attenuate the dynamic coefficients. The full control set is retained in the main regression tables (Tables 5–8). The window $\tau \in [-3, +2]$ spans the range over which the city-level event clock yields enough observations on either side of the tie to identify the dynamics. We complement it with the lag-motivation analysis of Appendix Table 12, a formal timing-of-effect test on the full sample. Standard errors are two-way clustered at the city and province-year levels, matching the main tables, and in all figures the $\tau = -1$ base is zero by construction.

A subtle issue under the standard event-study definition is that the post-tie dummies pool observations regardless of whether the originating tie is still active. In our data, hometown ties are formed by the appointment of a province leader during a city leader's term and end whenever either leader rotates. Among our treated city-terms, 19% of $\tau = +1$ observations and 32% of $\tau = +2$ observations have already lost the tie. Under the standard intention-to-treat interpretation these reverted observations bias the dynamic coefficients toward zero. We therefore restrict the post-tie observations ($\tau \in \{0, +1, +2\}$) to those for which the tie is active in the year of measurement. Symmetrically, a pre-tie lead ($\tau < 0$) enters only if no tie is active that year, so that a previously-tied spell cannot masquerade as a clean pre-period. Each panel uses its full available data for the corresponding outcome under these restrictions. This active-tie restriction has an interpretive cost that the reader should bear in mind, because tie survival depends on neither leader having rotated, and the tie itself raises the city leader's promotion odds, so the post-tie coefficients are estimated on spells that survived, a treatment-on-the-treated-style object rather than the intention-to-treat average. This, together with the binned base described next, is why the dynamic coefficients are mechanically larger than the static regression estimates (the residential-share peak of roughly 22 percentage points at $\tau = +1$, versus 6 percentage points in Table 7). The static coefficient averages the tie's effect over all treated years, including spells that have reverted, while the event study isolates the peak response among spells still active. We therefore read the event study as evidence on *timing and pre-trends*, and rest all magnitude claims on the static regression estimates. The intention-to-treat version of the event study, with post-tie dummies assigned by event time alone, regardless of tie survival, shows the same shape with smaller magnitudes, as expected (residential share $+0.17$ at $\tau = +1$, debt -0.46 at $\tau = +2$), and flat leads throughout (joint lead $p \geq 0.48$, Appendix Figure 9). The land-market dynamics are also robust to a heterogeneity-robust estimator built for non-absorbing treatments (de Chaisemartin and D'Haultfœuille, 2026). Estimated around tie switches, the effect on the residential share is $+0.20$ (95% CI $[0.01, 0.39]$) and on the industrial share -0.11 ($[-0.21, -0.005]$) by the third treated year, with near-zero effects on impact, and the residential-price effect carries the baseline sign at every horizon (about -0.12), though the small number of switchers limits precision. For debt, that estimator's own placebo test rejects, so we rely on the conventional event study of Figure 5 for the debt dynamics (all untabulated).

A second subtle issue is that the omitted base ($\tau = -1$) implicitly includes both treated city-terms one year before tie formation and never-treated city-term-years at any calendar year. Lumping these populations into the same base would force the regression's baseline to be a mixture of two arguably different distributions, making the dropped-degree-of-freedom interpretation of $\tau = -1$ less clean. To address this, we adopt a binned event-study specification (Schmidheiny and Siegloch, 2023). We keep treated observations *outside* the displayed window, at $\tau < -3$ and $\tau > +2$, in the regression but assign them to their own binned dummies, which absorb their level differences from the never-treated base. This matters materially. Post-tie years beyond $\tau = +2$ are, for outcomes such as debt and the residential land share, well into the treated regime, so leaving them in the base would drag the baseline toward the treated mean and attenuate the displayed coefficients. The binned dummies are estimated but not displayed, and the figures show the $[-3, +2]$ window.

We organize the event study into four figures, one per outcome family. Figure 2 plots planned land shares for residential, industrial, and commercial land. Figure 3 plots the (log) sold price per building area for the three land types. Figure 4 plots (log) sold sales revenue. Figure 5 plots (log) local-government debt, and we explain below why no house-price-growth panel is displayed. Price and sales panels use the sold (auction-clearing) variables for consistency with the main regression tables, and the share panels use planned shares because they reflect the leader's listing decision before market matching.

In Figure 2, the planned residential share shows no pre-trend ($\beta_{-3} \approx 0$, $p = 0.99$, and $\beta_{-2} = 8$ percentage points, $p = 0.49$, the latter imprecisely estimated, with joint lead test $p = 0.76$) and rises sharply after the tie, by 22.2 percentage points at $\tau = +1$ ($p = 0.01$) and 19.5 at $\tau = +2$ ($p = 0.04$). The industrial share moves in the opposite direction post-tie (-10.7 percentage points at $\tau = +2$), but neither its post-tie decline ($p = 0.12$) nor its leads (around -9 percentage points, individually and jointly insignificant, joint $p = 0.77$) are precisely estimated, so the industrial panel is less informative about timing than the residential one, which carries the sharper evidence. The commercial share shows a marginal positive lead at $\tau = -3$ ($+0.098$, $p = 0.07$, with joint lead $p = 0.20$) and declines after the tie (-0.117 at $\tau = +1$, $p = 0.002$). The clean residential pre-period is the most direct visual support for the parallel-trends assumption underlying the paper's central land-reallocation result. In Figure 3, the (log) sold residential price is flat pre-tie (joint lead $p = 0.60$) and falls afterward, by 0.30 at $\tau = 0$ ($p = 0.07$) and 0.50 at $\tau = +2$ ($p = 0.03$). The commercial price likewise falls at $\tau = +2$ (-0.89, $p = 0.04$, city-clustered standard errors, as its two-way clustered variance is degenerate), while the industrial price is null throughout, a useful caveat to the industrial-price coefficient in Table 8. Figure 4 adds the sold-revenue dynamics, for total land sales and by type. Total revenue is flat through the year of the tie and softens at the two-year horizon (-0.46 at $\tau = +2$), a delayed profile that echoes the debt decline and is consistent with a land-finance machine that slows as borrowing is curtailed. We do not lean on this, treating it as suggestive at most, because its pre-tie leads are only borderline flat (joint $p = 0.13$), it does not clear the within-province placebo ($p \approx 0.10$, as with house-price growth), and the static distributed-lag regression, which averages over reverted post-tie spells, leaves total revenue indistinguishable from zero (Table 9). The by-type series are weaker still. Residential revenue trends down (-1.04 at $\tau = +2$) but carries a *pre*-tie lead of comparable size (-1.29 at $\tau = -3$), while industrial and commercial revenue are imprecise throughout. The by-type softening is the arithmetic of price times quantity, namely the sold residential price falls (-0.50 at $\tau = +2$) while sold quantity drifts down only noisily, so recorded revenue eases even as the planned residential share expands.

Figure 5 turns to (log) local government debt. The debt path shows positive but statistically insignificant leads ($\beta_{-3} = 0.12$, $p = 0.60$, and $\beta_{-2} = 0.20$, $p = 0.38$, with joint lead $p = 0.67$) and falls sharply after the tie, with -0.54 at $\tau = +1$ ($p = 0.08$) and -0.70 at $\tau = +2$ ($p = 0.03$). The pre-period point estimates drift upward within wide confidence intervals, and the post-tie reversal is sharp, mirroring the one-year-lagged estimate in Table 6.

We do not display an event-study panel for house-price growth. On the baseline sample its dynamic path shows no statistically significant post-tie effect (-0.049 at $\tau = +2$, $p = 0.20$) alongside a marginally negative deep lead (-0.101 at $\tau = -3$, $p = 0.07$). The joint lead test does not reject ($p = 0.18$), but the path's most prominent feature is a hump peaking at the omitted $\tau = -1$ base, namely the within-term mean-reversion pattern that the randomization placebo of Section 5.3.1 identifies, under which even random ties generate negative house-price-growth coefficients. This dynamic fragility reinforces our suggestive-only reading of the house-price evidence.

5.3 Identification Checks and Alternative Interpretations

We now subject the design to three sharper checks, namely a randomization-inference placebo that directly tests the exogeneity of tie formation, a specificity test against other social ties, and heterogeneity by the stakes a leader has in the promotion tournament, then close these checks by weighing two alternative interpretations of the pattern. Two preliminaries establish that the sample choices do not matter. First, nothing hinges on excluding the four direct-controlled municipalities (Beijing, Shanghai, Tianjin, and Chongqing) from the baseline. Their ties are defined at the central rather than provincial level, and Appendix Table 13 re-estimates the preferred specification of each main result pooling them back in, with every coefficient essentially unchanged in magnitude (the promotion sample contains no municipality leader-terms, so its column is identical by construction). Second, re-estimating the main outcomes *including* the 56 dismissed leaders leaves the land and debt results unchanged (Appendix Table 14), and only the suggestive house-price coefficient attenuates further.

5.3.1 Placebo Test: Permuting Superiors' Birthplaces

The identifying claim is that, conditional on our fixed effects, the formation of a hometown tie, namely a newly appointed provincial leader who happens to share a city leader's birthplace, is as good as random with respect to a city's land and housing trajectory. We subject this claim to a randomization-inference test that randomizes precisely the object we treat as exogenous, namely the superior's birthplace. We hold fixed the real appointment timing and province of every provincial leader and the real birthplaces of every city leader, and randomly permute provincial leaders' birthplaces among themselves *within province*. Each permutation is run through the identical tie-construction rule, and we re-estimate the full-control specification for each outcome. We repeat this 500 times to build a null distribution for the coefficient on hometown tie under fake assignments. Permuting within rather than across provinces is essential. City leaders and their provincial superiors are both disproportionately native to the province they serve, so a national reshuffle would mechanically collapse the rate of hometown matches and reject by construction. Permuting within province preserves each province's local-origin base rate exactly (the number of placebo ties matches the true count) and randomizes only *which* appointment, in *which* year, carries which hometown, namely the timing variation our design exploits. The permutations are run on the baseline sample under the preferred specification of each outcome. By construction, feeding the true birthplaces through the identical machinery reproduces the headline estimates of the main tables to the digit.

Figure 6 reports the result. For the four core outcomes the true-birthplace estimate (red dashed line) lies entirely outside the placebo distribution. None of the 500 within-province permutations produces a coefficient as large in magnitude as the real one for the residential land share (+0.060), the industrial land share (−0.071), the residential land price (−0.205), or local government debt (−0.373), giving placebo p-values below 0.002. The probability that a random reassignment of superiors' hometowns reproduces our reallocation, price, and debt results is therefore negligible. House-price *growth* is the exception. Its true coefficient (−0.051) sits inside the placebo distribution ($p = 0.14$), because even random within-province ties generate a mean coefficient of about −0.035, reflecting a within-term mean-reversion in price growth that the permutation does not break. We read this as one more reason to treat the house-price-growth reduced form as suggestive, consistent with the significant pre-tie lead in its event study, and to rest the paper's causal weight on the land-composition, land-price, and debt results, which the placebo validates decisively.

5.3.2 Specificity: It Is the Hometown Tie

A complementary concern is that our hometown-tie variable proxies for generic political connectedness, that is, that *any* social tie linking a city leader to a newly appointed superior would relax promotion pressure and move land and debt. It does not. We re-estimate every main outcome using two alternative social ties recorded in our data, a shared party school and a shared original (undergraduate) school. These are bona fide political connections in the Chinese setting, and indeed the party-school tie is *more* common in our sample than the hometown tie (194 versus 103 nonzero city-years), yet, as Appendix Table 19 reports, neither reproduces our pattern of results. The coefficients are small and statistically insignificant, with three marginal (10%) exceptions, two of the *opposite* sign to ours, and one (the original-school tie on debt, -0.13) of the same sign but a third of the hometown magnitude and, unlike the hometown tie, unaccompanied by any effect on the promotion-GDP link. Pooling all three ties into an "any-tie" indicator likewise produces no effect (untabulated). The reason is visible in the first stage. Only the hometown tie removes the sensitivity of promotion to GDP performance. The tie$\times$GDP-growth interaction in the promotion regression is -3.42 ($t = -2.6$) for hometown ties but statistically insignificant for both the party-school ($+0.90$, $t = 0.8$) and original-school (-0.75, $t = -0.5$) ties (reported in the note to Table 19). Because only the hometown tie relaxes the GDP-promotion incentive, only it propagates to land allocation, prices, and debt. This pattern is difficult to reconcile with any account in which our results reflect generic political connectedness, or a social tie that merely happens to coincide with the timing of appointments.

5.3.3 Heterogeneity: The Effect Tracks Promotion Stakes

If the channel operates through career incentives, the tie's effects should be concentrated among leaders who are genuinely competing for promotion, and muted for those effectively out of the running. We test this by interacting the hometown tie with an indicator for leaders who have passed the customary age ceiling for their rank, namely a start-of-term age above 55 for prefecture-rank officials, 60 for sub-provincial, and 65 for provincial, for whom further promotion is largely foreclosed. Appendix Table 20 is consistent with the prediction, most sharply for debt. The effect on every outcome is at least as large for high-stakes leaders as in the pooled sample, and the point estimates attenuate for out-of-tournament leaders. For the land shares and the land price, however, the interactions are imprecisely estimated, so the evidence there is directional rather than sharp. The debt effect is the clearest case. A reduction of roughly 0.47 log points for high-stakes leaders shrinks to an implied -0.10 for out-of-tournament leaders, with a marginally significant offsetting interaction ($+0.38$, $t = 1.9$). That the strongest response is governed by whether a leader can still be promoted supports the interpretation that the operative force is the GDP-promotion career incentive itself, rather than some fixed characteristic of cities that happen to receive tied leaders. This pattern echoes independent evidence that hometown favoritism in corporate tax treatment is itself stronger where a leader's promotion incentives are weaker (Cheng et al., 2023).

5.3.4 Alternative Interpretations

Two alternative interpretations of the evidence call for explicit treatment, and a third, complementary account of local-government price management deserves comment alongside them.

The first concerns the bundled nature of the treatment, in that a tie both raises promotion odds and severs the growth-promotion link, so the behavioral response could in principle reflect an *anticipated-promotion* channel, namely a leader who expects to depart soon invests less in long-gestation industrial projects, rather than relaxed growth pressure. Two observations weigh against this reading, though we cannot rule it out entirely. First, *among leaders still below the age ceiling*, the margin on which an anticipation story operates, the tie's effects do not vary with the remaining career horizon (the distance to the rank-specific retirement ceiling), which that story predicts they should (untabulated). The sharp attenuation documented above occurs at the eligibility margin itself, which both interpretations share. Second, and more telling, the real-economy evidence of Section 5.4 disciplines the interpretation. A leader merely coasting toward an expected promotion has no reason to expect the city's industrial engine to wind down even as output holds up, whereas that recomposition is what relaxing the career incentive itself predicts.

That complementary account concerns the active price management documented by Chang, Wang and Xiong (2026), who show that during the post-2020 downturn local governments propped up residential land and house prices through countercyclical land purchases by their financing vehicles and limits on new-home sales permits, producing a divergence between prices and transaction volumes. A reader might ask whether our lower residential prices and lower debt are simply a tied leader easing off such support rather than reallocating land. The two accounts operate on different objects. Their instruments manage the price *level* of residential land and housing, whereas our central result is the *composition* of primary land supply, with the residential share rising and the industrial share falling while the total quantity released does not move (Table 10). Supporting prices, whether by buying land or by rationing sales permits, does not shift the industrial-residential mix at a fixed total, so the reallocation at the heart of the paper is not a price-support artifact. Two further features point the same way. The *industrial* land price, if anything, rises as the residential price falls (a result we flagged above as secondary), an asymmetry that a supply reallocation predicts but that withdrawing residential price support does not. And the real-economy recomposition of Section 5.4, with foreign investment and industrial employment down, housing reaching households up, and output holding up, lies outside any pure price-management account. Where the two accounts meet is in treating land and land-backed debt as strategic levers. Chang, Wang and Xiong (2026) identify instruments through which local governments manage prices, while we identify the political-career incentive that governs how a leader allocates land and incurs debt in the first place, so we read the two as complementary rather than rival.

The second alternative is preferential treatment from above. A newly arrived superior who favors his hometown comrade might channel resources toward the tied city, through fiscal transfers, favorable project and credit allocation, or laxer oversight of borrowing, and the downstream outcomes could then reflect treatment from above rather than any change in the leader's own behavior. Each of these channels is directly testable, and each runs empty or in the wrong direction (Appendix Table 16, Panel F). The transfer test is the comprehensive one. Because cities in our period could not borrow within the general budget, expenditure in excess of own revenue is financed by transfers from above, and because every such transfer reaches the city through the province, the province's own funds together with the central funds the province sub-allocates, the ratio of expenditure to own revenue captures precisely the margin a provincial patron controls. The margin is wide, with expenditure running roughly 1.7 times own revenue over our period, yet the tie has a precisely estimated zero effect on this ratio at every lag. Project money corroborates this at the funding-source level. No source of municipal-infrastructure investment rises in tied city-years. The local-appropriation line, which bundles provincial project money with the city's own funds, is flat, and central appropriations, the one source the data itemize separately, do not rise either, carrying a negative point estimate at every horizon. Total bank loans, loan growth, and credit intensity are likewise unchanged, as are the total land supply the city makes available (Table 10) and fixed investment and infrastructure (Appendix Table 15). Laxer oversight of borrowing predicts more debt, whereas the debt stock falls by about 28 percent, and resource channeling predicts more industrial activity, whereas foreign investment and industrial employment growth decline (Section 5.4). Two further patterns are difficult for a treatment-from-above story to accommodate. The effects attenuate sharply once the leader ages past the promotion-eligibility ceiling (Section 5.3.3), although a patron's resources have no reason to follow the client's eligibility clock, and other social ties that plausibly carry generic favoritism, such as a shared alma mater, move none of the outcomes (Section 5.3.2). What survives is favoritism in the promotion rule itself, namely the patron promotes the tied leader with little regard to growth. That is not an alternative to our interpretation. It is the source of the incentive change whose behavioral consequences the paper traces.

5.4 From Policy Levers to the Real Economy: The Recomposition of Growth

The results so far concern the instruments a leader directly controls, namely the composition of land and the stock of debt. Whether these political choices leave a footprint on the *real* economy, or remain confined to the leader's policy levers, is a separate question, and we turn to it now. Because real activity responds to land allocation only after land is built upon, occupied, and put to use, we measure these outcomes with the same city-level event study and within-province placebo developed above, reading effects at the two-year horizon at which the slow conversion of land into activity should surface. The real-economy series are drawn from the CEIC prefecture panel described in Section 4. Table 11 collects the estimates and Figure 7 plots the dynamics over an extended window ($\tau \in [-5, +2]$, displaying deeper leads than the land-market figures precisely so the pre-periods can be inspected). Joint tests of the displayed leads fail to reject flat pre-trends for any outcome (p-values from 0.11 to 0.98, Table 11).

Three patterns emerge. The first is that the city's industrial growth engine winds down. Foreign direct investment, the most visible prize in the local growth tournament, falls on every measure we observe, namely utilized FDI by 0.35 log points at $\tau = +2$, contracted FDI by 0.58, and the number of FDI contracts by 0.16, clearing the within-province placebo at the 5% level for the first two measures and the 10% level for the third (placebo $p = 0.040$, 0.002, and 0.054). Because the $\tau = +2$ estimand for level outcomes carries a mild mechanical within-term drift, namely the placebo distribution is centered below zero rather than at zero, Table 11 reports each estimate net of that drift alongside the raw coefficient (utilized FDI -0.21 rather than -0.35, and contracted FDI -0.52). The net-of-drift figures are the magnitudes we would emphasize, and the randomization p-values account for the drift by construction. The industrial workforce contracts in step. Growth in secondary-sector employment falls (-0.099, placebo $p = 0.01$) and the city's non-agricultural population, its stock of urban, largely industrial workers, declines (-0.058, $p = 0.03$). One industrial series does not fit the pattern, namely industrial wastewater carries an opposite-signed point estimate (Appendix Table 16), which we flag rather than discard.

The second pattern is the mirror image on the household side. Residential floor space sold per capita, namely housing actually delivered to households, rises by 0.16 log points by $\tau = +2$. This is the welfare-relevant counterpart of the residential land reallocation, because housing is typically the dominant asset on household balance sheets in emerging economies, China included (Badarinza, Campbell and Ramadorai, 2016, Badarinza, Balasubramaniam and Ramadorai, 2019), so the supply of housing to households is where the land reallocation would first touch household welfare. It clears the placebo only marginally ($p = 0.09$), and the broader set of household-welfare outcomes we examined, namely disposable income, consumption, and fertility, a margin that housing access has been shown to move causally (van Doornik, Fazio, Ramadorai and Skrastins, 2025), does not move. We therefore read the household-side evidence as the weakest of the three and report the full battery of outcomes and transformations we examined, with their placebo p-values, in Appendix Table 16. The placebo p-values also survive a formal multiple-testing discipline, namely applying the Benjamini–

Hochberg false-discovery-rate correction across the full family of ten placebo-tested outcomes (q-values for the confirmed set in Table 11, and for the full family in Appendix Table 16), the contracted-FDI, employment-growth, and both output-growth results (GDP and secondary-industry) clear a 5% false-discovery rate and the remaining industrial-margin results clear 10%, while the household-side result just misses ($q = 0.11$), consistent with the grading of the evidence we have offered.

The third pattern is that aggregate output does not fall, and if anything it rises. The rise is a recomposition rather than a contradiction, because what grows is construction and the output it throws off, not the visible industrial effort the promotion tournament rewarded. GDP growth increases by 0.05 (placebo $p = 0.004$) and secondary-industry output growth by 0.07 ($p < 0.002$) by the second post-tie year. Unlike the level outcomes, these two growth-rate outcomes carry no mechanical drift, namely their placebo distributions are centered on zero, making these the cleanest of the real-economy estimates. The recomposition is thus not a contraction. The city produces at least as much, but a different mix, namely less foreign-investment-driven, labor-intensive industry, and more residential and construction activity. Two caveats discipline this reading. First, the natural reconciliation of rising secondary-industry *output* with falling secondary-sector *employment* is that construction, which sits inside secondary industry, expands as residential land is built out while labor-intensive manufacturing contracts. We can now show this directly. Using the provincial-yearbook decomposition of city GDP by industry (Section 4), which covers the secondary-sector split for far more cities and earlier years than the national series, secondary value-added growth rises at the two-year horizon (a coefficient of 0.060, placebo $p = 0.004$), and within it both construction value added (a coefficient of 0.20 on the 87 cities with the secondary split that enter the event study, placebo $p = 0.008$) and industry value added (0.055, placebo $p = 0.006$) grow, while tertiary value added is flat (Figure 8 and Appendix Table 16, Panel G). The construction that the residential build-out generates raises secondary value added even as labor-intensive manufacturing employment falls, which is precisely the reconciliation the diverging output and employment paths require. The construction channel is therefore shown, not merely inferred. Second, the scale of *inputs* does not expand, because investment and infrastructure are unchanged on impact (Appendix Table 15) and we detect no increase in total land supply (Table 10), so the output increase does not reflect a larger resource base, at least on impact. Third, and bearing most on interpretation, we cannot fully separate a more efficient use of the same resources from an alternative in which a tie leads the province to channel resources, namely construction projects, administrative attention, toward a favored city, raising its output and reducing its own borrowing without any genuine reallocation. Two features of the evidence sit awkwardly with a simple resource-favoritism story, namely the tied city's foreign investment, industrial employment, and urban population *fall* rather than rise, and its on-impact investment is flat while total land supply is not detectably higher, but a narrower channeling of construction resources would survive them. Nor does the relative comparison reflect discouraged competitors rather than a genuine tied-city response, because untied cities in the same province show no change in growth, foreign investment, or land composition when a province-mate becomes tied (Appendix Table 17). We therefore rest no welfare claim on the output increase, reading it cautiously as evidence that relaxing the career incentive recomposes local growth without contracting it. Whether local debt crowds private investment in or out is itself debated, because Huang, Pagano and Panizza (2020) find crowding-out, whereas other work finds local spending crowds private activity *in* through sizable multipliers (Su, 2023), so we do not lean on that channel. We note only that a cut in local financing-vehicle debt need not imply less private credit, consistent with the absence of any city-wide credit contraction in our data.

This pattern, namely output holding up even as the visible industrial engine winds down, fits the career logic of Section 3, and is the leader-level footprint of the "Mandarin" account. Our first stage shows promotion rewards GDP growth and that the tie severs the link, so one might expect growth to *slow* once the incentive is removed. But GDP is an *outcome* of the leader's choices, not a choice itself, and what a superior can credit to a leader's own effort is less the growth statistic than a visible, attributable achievement, namely a new factory, a signed FDI deal, a tract of industrial parkland. Residential land appreciation, by contrast, is diffuse and market-driven, and no official is promoted for it. A leader scored on attributable achievements therefore tilts toward the creditable industrial margin, namely cheap industrial land, FDI courting, debt-financed buildout. A tie that relaxes the incentive shifts that tilt back toward residential use, and because the tournament steered the *composition* of activity rather than its aggregate scale, output need not fall when the steer is removed. This is the multitasking logic of Holmström and Milgrom (1991), by which an agent reallocates effort toward the dimensions a principal can measure, and here it shapes *what* local growth is made of. Independent evidence is consistent with such a visible-achievement bias, namely the net productivity of China's politically driven infrastructure is estimated to be neutral or negative, with officials favoring visible, high-profile projects (Qian, Ru and Xiong, 2025, Robinson and Torvik, 2005). We are careful about what this does and does not establish. We do *not* claim the industrial land allocation is broadly inefficient, because He et al. (2025) show it is, on average, substantially rationalized by the future tax revenues industrial land generates, and the direction of our reallocation, namely industrial toward residential, coincides with the one they argue is value-enhancing in high-demand cities. What we add is the political-career *cause* of the reallocation, identified causally and read through the Mandarin model, and the evidence that it operates at the margin, within a leader's tenure, when promotion pressure is relaxed.

Read this way, the recomposition supplies leader-level evidence for the career-incentive mechanism at the heart of the aggregate "Mandarin" account (Song and Xiong, 2026), and sharpens it. Career incentives shape not the *level* of local growth so much as its *character*, steering economies toward visible, industry- and debt-heavy activity and away from residential use the tournament cannot credit. Two limits bound the interpretation. We do not measure productivity directly, so the output increase is at most *consistent with* a value-enhancing reallocation, not a welfare estimate. And although the leader-term sample is too small to separate industry from construction within the secondary sector in the promotion regression, decomposing the gradient does localize the rewarded margin in the secondary sector (Appendix Table 18), the same margin the real-economy reallocation implicates, so the first-stage reward function and the real-economy response point to the same place. This also resolves an apparent tension. The tie severs the reward to secondary-sector growth, yet secondary output *rises* afterward. The reconciliation is that the rewarded margin is the visible secondary-sector *effort* a superior can credit, namely foreign-investment courting and industrial employment, which is exactly what falls when the incentive is removed, whereas the secondary value added that rises is dominated by construction, the byproduct of the leader's land reallocation rather than of any creditable growth effort. Industry value added edges up as well, but modestly relative to construction, and as an output rather than as the effort margin the tournament scored. The component that grows is not the effort the tournament rewarded.

6 Conclusion

In this paper, we provide reduced-form causal evidence that the career incentives created by China's GDP-based promotion system are a first-order determinant of how local officials allocate land and how much debt their cities take on. Exploiting the plausibly exogenous timing of provincial and central leadership appointments, and the hometown ties they create, we show that when GDP-promotion pressure is relaxed, city leaders reallocate land from industrial to residential use, lower residential land prices, and curtail local government debt. This response is compositional rather than expansionary. We do not detect a change in the total quantity of land supplied, and direct tests of transfers, credit, and project allocation from above come back empty, so the channel we identify operates through the *composition* of land use rather than through an expansion of scale. House-price growth also slows in tied cities, though we treat this as suggestive corroboration rather than as an independent result. A randomization-inference placebo that permutes superiors' birthplaces confirms that the land-reallocation, land-price, and debt results are not artifacts of the appointment process. Tracing the channel one step further, into the real economy, we find that the reallocation recomposes local growth. The visible industrial engine the tournament rewards, namely foreign investment and industrial employment, winds down, residential housing reaching households appears to expand, and aggregate output does not fall and may even rise. Career incentives shape the *character* of local growth, not merely its level, steering officials toward the activity a promotion tournament can credit and away from uses it cannot.

These findings carry a clear policy implication, namely the criteria by which local officials are evaluated and promoted are themselves a lever over China's land, housing, and local-debt outcomes. Measures to contain the housing boom and the buildup of local government debt that focus only on credit conditions or demand-side instruments may miss a more fundamental driver, namely the political-incentive structure facing the very officials who supply land and incur debt. Recent policy analysis reaches a parallel conclusion, arguing that durably derisking China's real estate sector requires replacing the financing model that ties local governments to land revenue (Xiong, 2025). The lesson also travels beyond China. Wherever a bureaucracy scores its agents on the achievements it can measure and attribute, the composition of what those agents produce will tilt toward the measurable margin, and the gap between what is rewarded and what is valuable will leave footprints in asset markets and public balance sheets, as it has in China's land prices and local government debt.

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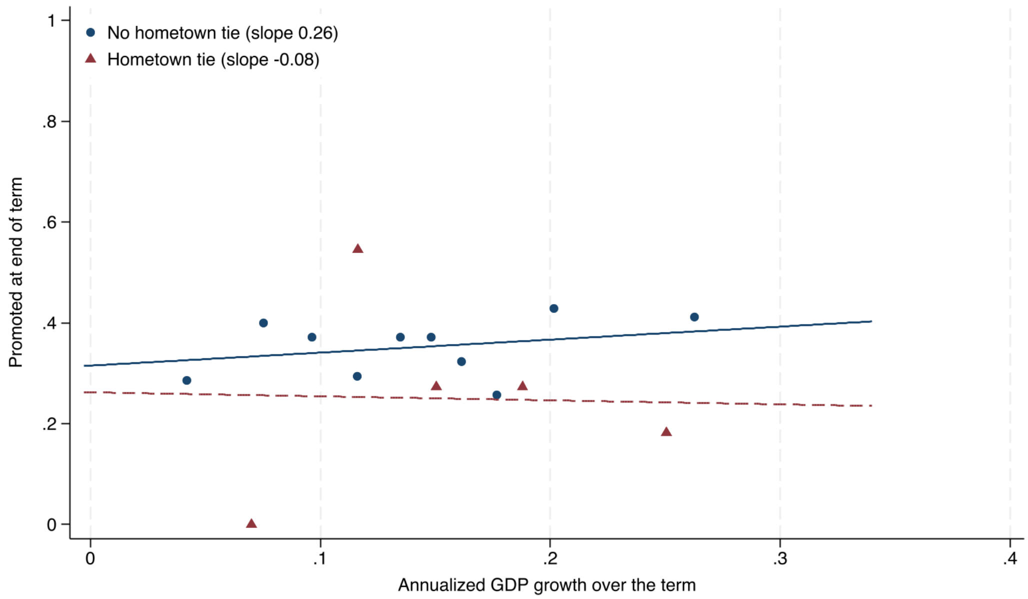
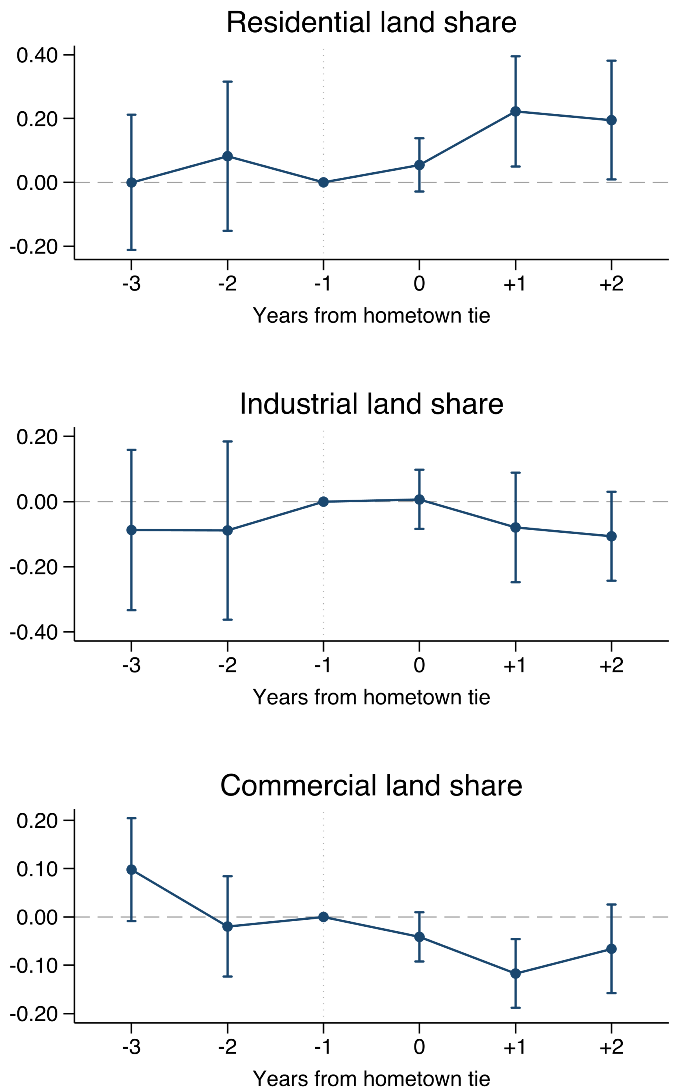
7 Figures

Figure 1: Promotion, GDP Growth, and the Hometown Tie



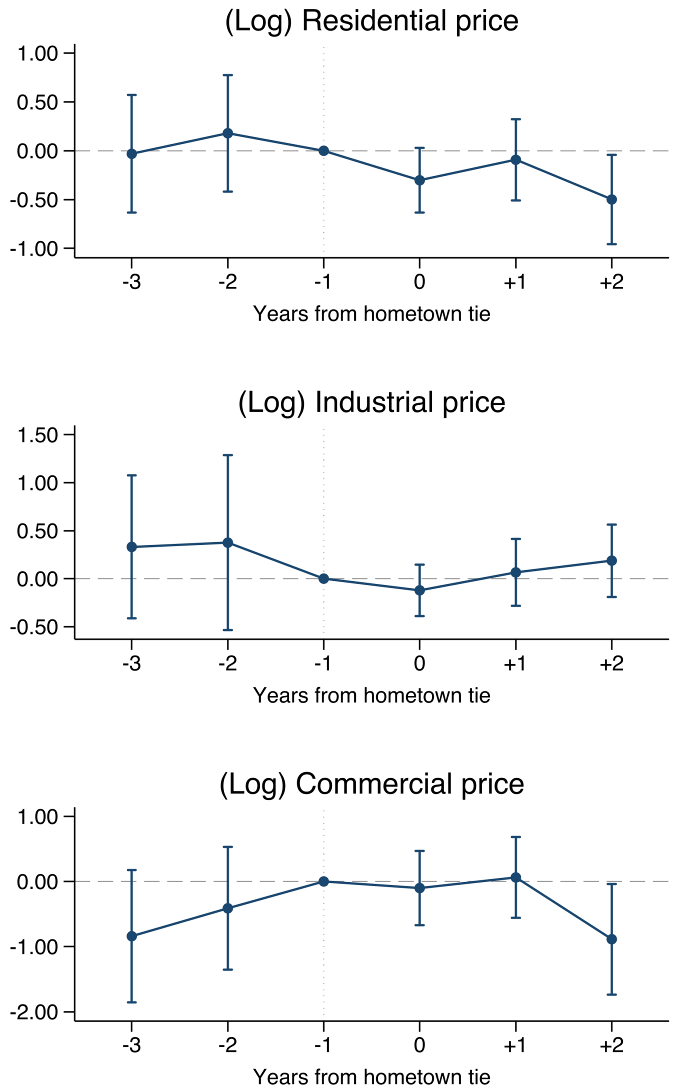
Note: Binned means of the end-of-term promotion indicator against annualized GDP growth over the term, separately for leader terms without (circles, ten bins) and with (triangles, five bins) a hometown tie, on the promotion estimation sample of Table 4 (growth trimmed at the 1st and 99th percentiles for display). Lines are unconditional linear fits estimated on the underlying terms. Promotion is increasing in growth for unconnected leaders and essentially flat for tied leaders. The raw slopes are attenuated relative to the within-province-year estimates of Table 4 by the composition differences discussed in the text.

Figure 2: Event Study: Planned Land Shares by Type



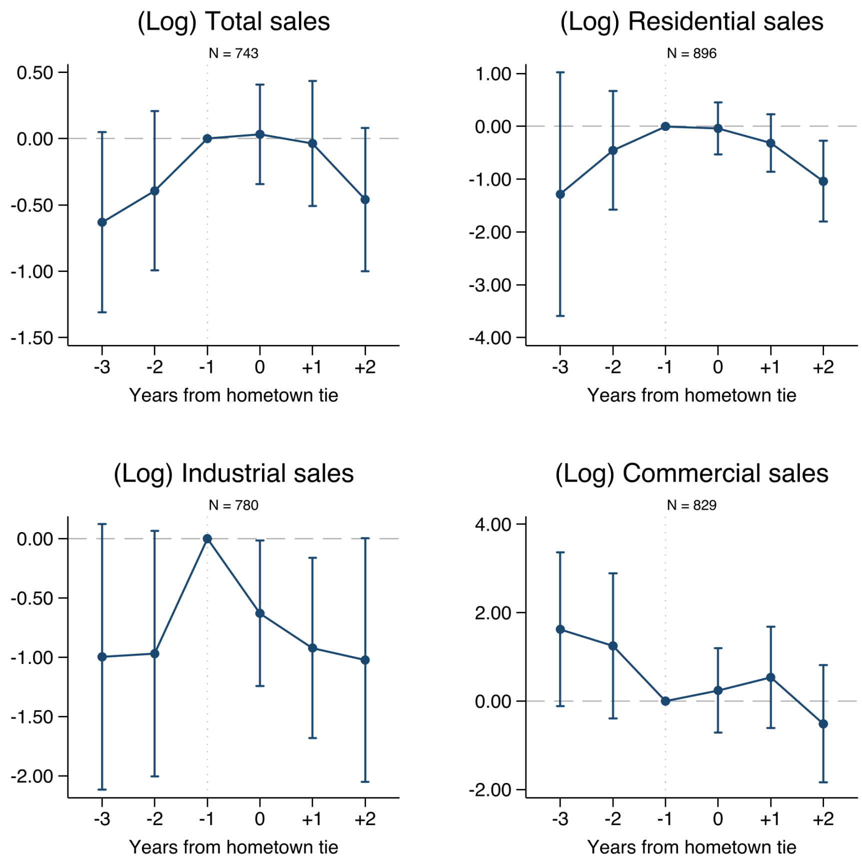
Note: Event-study coefficients β_τ from separate regressions of the planned land share on event-time dummies, where event time τ is years from a city's first hometown tie (measured at the city level, so a city's pre-tie years under the previous leader populate the leads). Panels correspond to residential, industrial, and commercial land shares. The omitted base is $\tau = -1$ (filled circle at zero, vertical dotted line). All specifications include a separate city-term fixed effect per leader (absorbing cross-leader level shifts), plus province-by-year and year-within-term fixed effects, with no time-varying controls. Post-tie observations are restricted to years in which the originating tie is still active. Pre-tie leads include only years with no active tie. Treated observations outside the $[-3, +2]$ window are absorbed into binned dummies (estimated, not displayed). Joint tests that the displayed leads are zero do not reject for any panel (residential $p = 0.76$, industrial $p = 0.77$, commercial $p = 0.20$). Standard errors are two-way clustered at the city and province-year levels. Bars are 95% confidence intervals. Source: CREIS.

Figure 3: Event Study: (Log) Sold Land Price by Type



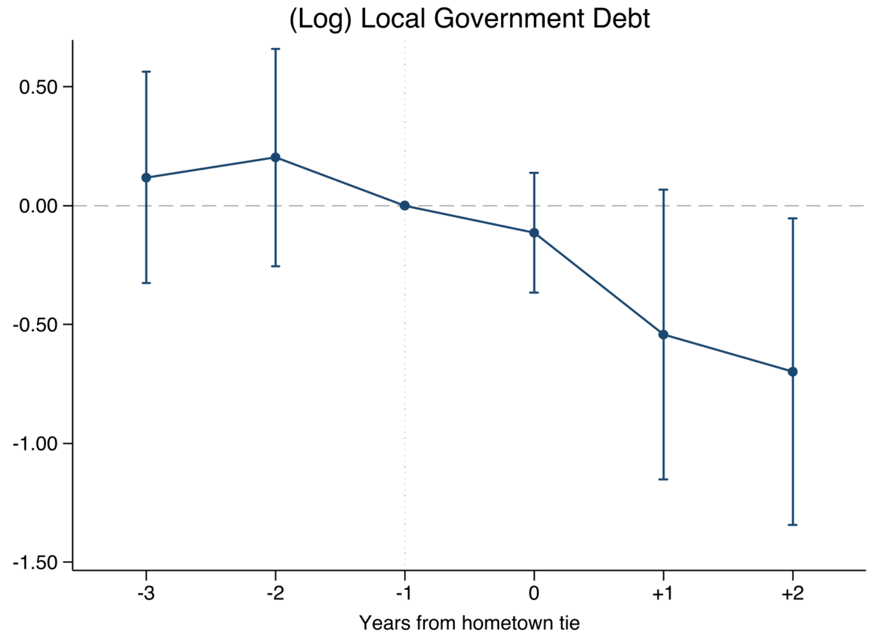
Note: Event-study coefficients for the log sold price per building area, by land type. Specifications, fixed effects, controls, and standard-error clustering match Figure 2, except that the commercial-price panel reports city-clustered standard errors (its two-way clustered variance estimate is degenerate). The omitted base is $\tau = -1$. Joint lead tests do not reject for any panel (residential $p = 0.60$, industrial $p = 0.68$, commercial $p = 0.16$). Each panel uses its full available data for the corresponding outcome. Source: CREIS.

Figure 4: Event Study: (Log) Land Sales Revenue, Total and by Type



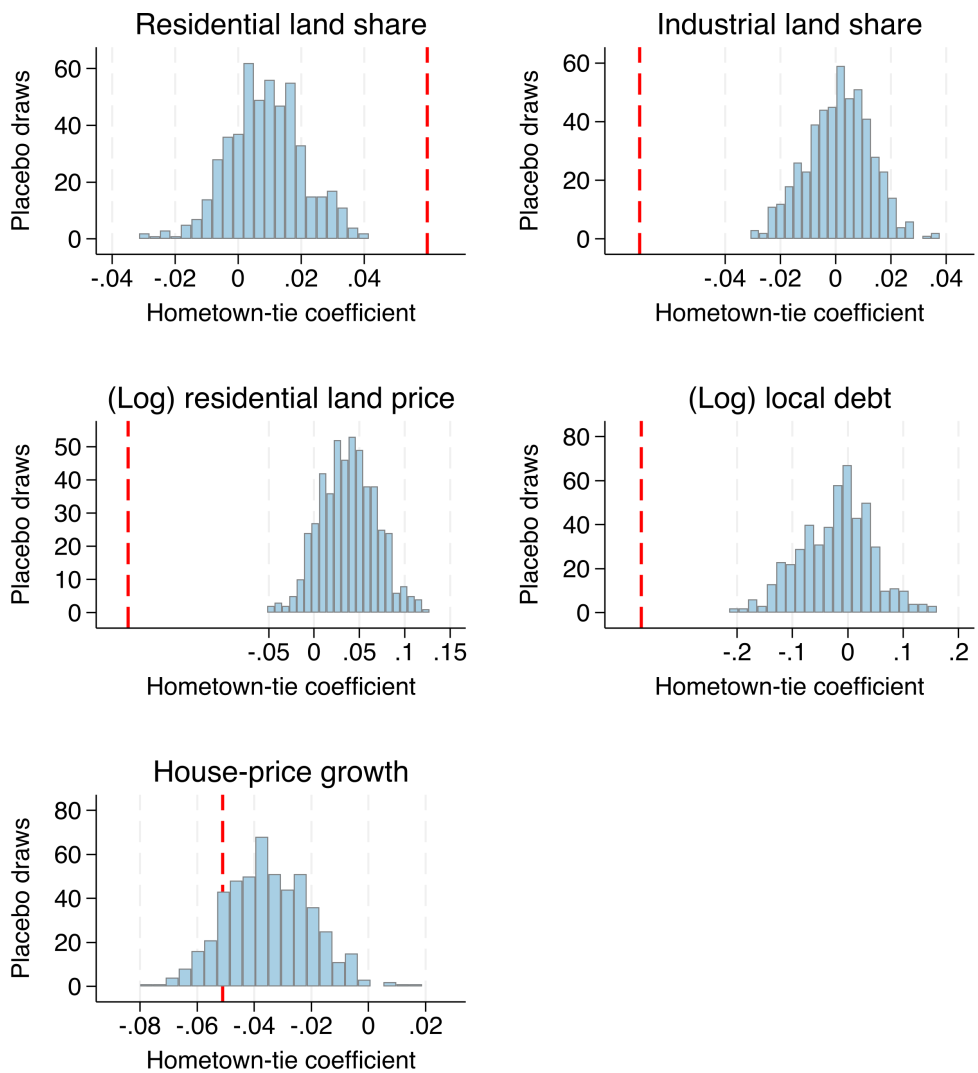
Note: Event-study coefficients for the log sold sales revenue (price $\times$ quantity), for total land sales (top-left panel) and by land type. Specifications, fixed effects, and standard-error clustering match Figure 2. The omitted base is $\tau = -1$. Total revenue softens at the two-year horizon (-0.46 at $\tau = +2$), a delayed profile that echoes local debt, but its pre-tie leads are only borderline flat (joint $p = 0.13$) and the estimate does not clear the within-province placebo ($p \approx 0.10$), so we read it as suggestive corroboration of the debt result rather than an independent finding, much as we treat house-price growth. The by-type series are weaker still, imprecise and, for residential, carrying a pre-tie lead comparable in size to the post-tie estimate (joint lead tests $p = 0.46$, 0.09, and 0.10 for residential, industrial, and commercial). All are in line with the inconclusive distributed-lag regressions of Table 9. Each panel uses its full available data for the corresponding outcome. Source: CREIS.

Figure 5: Event Study: Local Government Debt



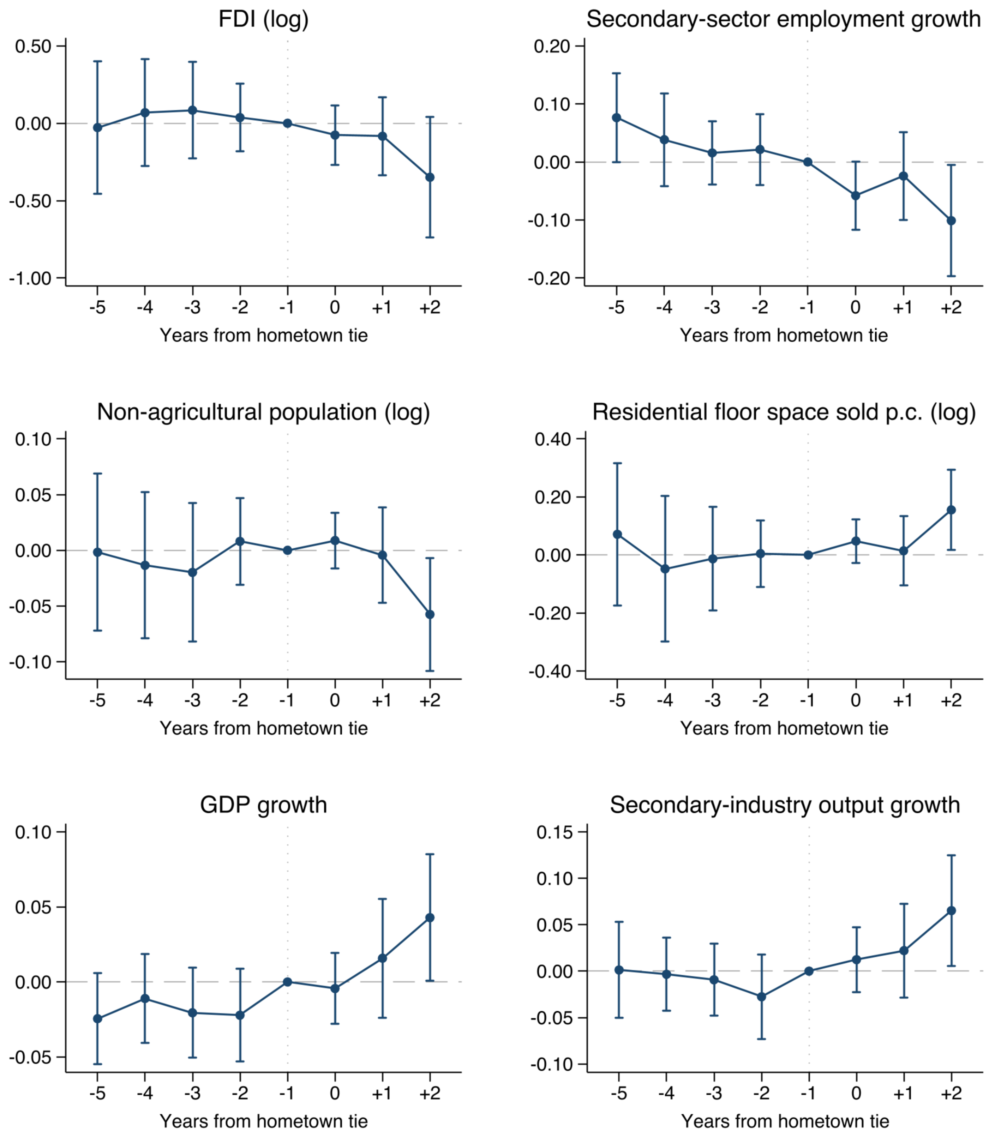
Note: Event-study coefficients for (log) local government debt around a city's first hometown tie. The omitted base is $\tau = -1$. Specifications match those in Figure 2, namely a city-term fixed effect per leader, province-by-year and year-within-term fixed effects, and no time-varying controls. Post-tie observations are restricted to active-tie years and pre-tie leads to no-tie years. Standard errors are two-way clustered at the city and province-year levels, with 95% confidence intervals shown. The joint test of the displayed leads does not reject ($p = 0.67$). Local debt is the local-government financing-vehicle debt panel of Huang, Pagano and Panizza (2020) (Section 4). The house-price-growth panel is omitted for the reasons given in the text.

Figure 6: Placebo Test: Permuting Provincial Leaders' Birthplaces Within Province



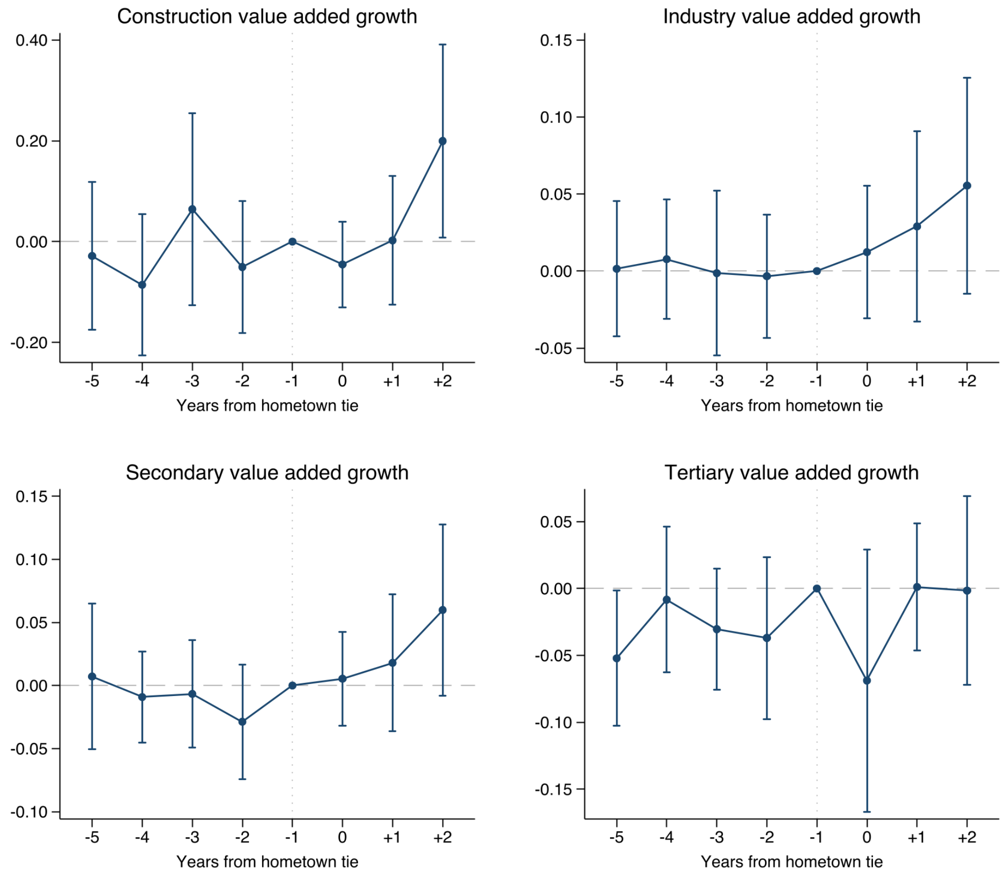
Note: Each panel plots the distribution of the estimated hometown-tie coefficient across 500 placebo samples in which provincial leaders' birthplaces are randomly permuted *within province*, holding fixed the real appointment timing and provinces of all leaders and the real birthplaces of all city leaders. Each placebo draw is run through the identical tie-construction rule and the identical preferred specification used for the corresponding outcome, namely city-term, province-by-year, and year-within-term fixed effects for the land and debt outcomes, and the lagged-dependent-variable specification without year-within-term fixed effects for house-price growth, matching the main tables. Standard errors are two-way clustered throughout. The red dashed line marks the estimate obtained under leaders' *true* birthplaces from the same specification and sample, and therefore equals the corresponding headline estimate of the main tables to the digit. Two-sided placebo p-values, namely the share of permutations with $|\hat{\beta}| \geq |\hat{\beta}^{\text{true}}|$, are below 0.002 for the residential land share, industrial land share, residential land price, and local debt (none of 500 permutations is as extreme), and 0.14 for house-price growth. The sample is the baseline sample of the main tables.

Figure 7: The Real Economy Recomposes Around the Hometown Tie



Note: Event-study coefficients around a city's first hometown tie for six real-economy outcomes, on the same city-level event clock and binned specification as Figure 2 (a separate city-term fixed effect per leader, province-by-year and year-within-term fixed effects, no time-varying controls, with post-tie observations restricted to active-tie years and pre-tie leads to no-tie years). The displayed window is $\tau \in [-5, +2]$ with $\tau = -1$ omitted as the base (vertical dotted line). The window extends two years deeper into the pre-period than the land-market figures so that the joint pre-trend tests of Table 11 can be inspected visually. *Top row*: utilized FDI (log, the panel titled "FDI") and secondary-sector employment growth. *Middle row*: non-agricultural population (log) and residential floor space sold per capita (log). *Bottom row*: GDP growth and secondary-industry output growth. The visible industrial engine (FDI, industrial employment, urban population) turns down after the tie, while residential sales and aggregate output turn up. Standard errors two-way clustered at the city and province-year levels, with 95% confidence intervals shown. Source: CEIC prefecture statistics merged to the analysis sample.

Figure 8: Decomposing the Post-Tie Output Rise by GDP Component



Note: Event-study coefficients for annual value-added growth by GDP component, on the same city-level event clock, binned specification, and fixed effects as Figure 7 (a separate city-term fixed effect per leader, province-by-year and year-within-term fixed effects), with $\tau = -1$ omitted as the base (vertical dotted line). The series are the secondary sector and its two components, construction and industry value added, together with tertiary value added, from the provincial-yearbook decomposition of city GDP described in Section 4. The post-tie rise in secondary value added is concentrated in construction, while industry edges up modestly and tertiary is flat, the coherent sign pattern behind the construction channel. Construction value added is observed for the fewest cities (87, against 151 to 162 for the other components), so its leads and confidence intervals are the widest. Bars are model-based 95% confidence intervals with standard errors two-way clustered at the city and province-year levels. Our preferred inference is the within-province placebo of Appendix Table 16, Panel G, under which the construction, industry, and secondary growth effects each clear the 1% level. Source: EPS provincial yearbooks merged to the analysis sample.

8 Tables

Table 1: Summary Statistics of City Party Secretaries, by Term

	No Hometown Tie			Hometown Tie		
	Mean	Std.	count	Mean	Std.	count
Promotion	0.35	0.48	354	0.25	0.43	57
Term Length	4.01	1.59	354	4.79	1.58	57
Term Start Age	49.97	3.56	354	49.58	4.11	57

Note: This table shows summary statistics for the 411 completed leader terms in the promotion estimation sample of Table 4 (city party secretaries in office between 2003 and 2015, collected from the China Political Elite Dataset. See Section 4 for the sample construction). The statistics are reported separately for terms with and without a hometown tie. Hometown tie equals one if the province leader is appointed after the city leader and shares the same hometown with the city leader this year or in the last year. Promotion equals one when the city party secretary is promoted to a higher political rank after his term as a city leader and zero otherwise. Term length is the total number of years during the term. Term start age is the age at which a city party secretary starts his term at the city.

Table 2: Summary Statistics of the Land-Market Variables

	Unit	N	Mean	Std. dev.	P25	Median	P75
Residential share of planned supply	share	630	0.50	0.17	0.39	0.51	0.61
Industrial share of planned supply	share	630	0.32	0.16	0.21	0.31	0.43
Commercial share of planned supply	share	630	0.16	0.10	0.09	0.14	0.21
Total planned supply (building area)	10k sq. m	630	1948	1618	808	1547	2576
Sold residential land price	RMB/sq. m	1057	1331	2826	490	757	1330
Sold industrial land price	RMB/sq. m	890	303	322	180	238	319
Hometown tie	0/1	1146	0.08	0.27	0.00	0.00	0.00

Note: Summary statistics on the baseline estimation panel: city-year observations of 179 prefecture cities, 2003–2015, excluding the four direct-controlled municipalities and the dismissed leaders (Section 5). Shares are planned building-area shares by use type. Total planned supply is in 10,000 square meters of building area. Prices are sold (auction) prices per square meter of building area, CPI-adjusted. Hometown tie is the treatment indicator. Sample sizes vary with outcome availability. Land data comes from China Real Estate Index System.

Table 3: Summary Statistics of Macro Variables, by City

	Housing Price	Fixed Investment	Real Estate Investment	GDP	ΔGDP	Average Wage
unit	RMB/sq m	RMB mn	RMB mn	RMB tn	RMB tn	RMB
count	195	195	195	195	195	195.00
mean	3750.41	105752.95	21748.16	0.1826	0.0168	31,342.38
std	2166.56	103832.76	33869.33	0.2245	0.0196	7,339.60
min	1657.70	9653.58	773.28	0.0146	0.0008	16,316.36
median	3017.30	66654.79	9738.30	0.1047	0.0097	29,810.69
max	16364.43	679984.85	258079.90	1.7508	0.1200	70,256.63

Table 3 (continued): Summary Statistics of Macro Variables, by City.

	Paved Road	Household Registration	Usual Residence	Govt. Revenue	Govt. Expenditure	Deposits
unit	sq m bn	Person th	Person th	RMB bn	RMB mn	RMB mn
count	195	195	186	195	195	195
mean	0.02	4,769.80	5,023.02	15.82	23,486.48	298,465.74
std	0.03	3,278.12	3,519.75	31.97	36,146.12	655,356.56
min	0.00	553.39	633.81	0.77	3,989.12	23,897.36
median	0.01	3,915.89	4,332.87	7.03	14,843.67	108,967.86
max	0.26	32,745.60	28,922.84	313.28	349,434.69	6,200,904.56

Note: This table shows summary statistics for city-level economic data for 195 cities in sample between 2004 and 2015, except usual-residence population, which is available for 186 of them. Data comes from China National Bureau of Statistics.

Table 4: Promotion, GDP, and Hometown Tie

	Dependent variable: end-of-term promotion			
	(1)	(2)	(3)	(4)
GDP Growth Rate*Hometown Tie	-3.397**	-3.800***	-3.374**	-3.416**
	(1.394)	(1.449)	(1.361)	(1.327)
Hometown Tie	0.529**	0.604**	0.574**	0.625***
	(0.229)	(0.247)	(0.235)	(0.227)
Annualized GDP Growth Rate	1.709***	2.209***	2.025***	1.810***
	(0.630)	(0.666)	(0.642)	(0.599)
Prov-Year FE	Y	Y	Y	Y
City FE	Y	Y	Y	Y
Turnover FE	N	Y	Y	Y
Gender FE	Y	Y	Y	Y
Age FE	Y	Y	Y	Y
Rank FE	N	N	Y	Y
Termination FE	N	N	N	Y
N	411	411	411	411
Adj. R^2	0.0991	0.0381	0.0705	0.0951

Note: This table shows estimates for the linear probability model on promotion, GDP growth, hometown tie, and their interactions. The estimation sample comprises 411 completed leader terms of city party secretaries in office between 2003 and 2015 (Section 4). Each observation is one completed prefecture party-secretary term. Hometown tie equals 1 if the province leader is appointed after the city leader and shares the same hometown with the city leader this year or in the last year. Standard errors are two-way clustered at city and province-year. All columns additionally absorb birth-province fixed effects (not displayed as a separate row). Political data comes from China Political Elite Dataset. City level economic data is from China National Bureau of Statistics. Standard errors in parentheses.

Table 5: Hometown Tie and House Price Growth Rate

	Dependent variable: house-price growth			
	(1)	(2)	(3)	(4)
Hometown Tie	-0.0559**	-0.0493**	-0.0514**	-0.0304
	(0.0213)	(0.0215)	(0.0226)	(0.0232)
L.pihpr	-0.394***	-0.408***	-0.397***	-0.401***
	(0.102)	(0.0991)	(0.0994)	(0.0954)
L2.pihpr	-0.235***	-0.250***	-0.248***	-0.257***
	(0.0610)	(0.0620)	(0.0631)	(0.0642)
(Log) GDP		-0.394**	-0.350	-0.362
		(0.193)	(0.221)	(0.224)
(Log) Population		0.443	0.432	0.396
		(0.574)	(0.537)	(0.562)
(Log) Fiscal Revenue			0.0793	0.114
			(0.111)	(0.109)
(Log) Fiscal Expenditure			-0.169	-0.152
			(0.119)	(0.120)
City-Term FE	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y
Turnover FE	N	N	N	Y
N	386	386	386	386
Adj. R^2	0.421	0.428	0.425	0.409

Note: This table shows OLS estimates for the effect of hometown tie on house price growth rate. Sample includes up to 191 prefecture-level cities in mainland China between 2003 and 2015 (the baseline excludes the four direct-controlled municipalities). Hometown tie equals 1 if the province leader is appointed after the city leader and shares the same hometown with the city leader this year or in the last year. Each observation is a city-year pair. Standard errors are two-way clustered at city and province-year. Political data comes from China Political Elite Dataset. Land data comes from China Real Estate Index System. City level economic data is from China National Bureau of Statistics. Standard errors in parentheses.

Table 6: Hometown Tie and Local Government Debt

	Dependent variable: (log) local government debt				
	(1)	(2)	(3)	(4)	(5)
L.Hometown Tie	-0.325*	-0.330**	-0.323*	-0.322*	-0.373**
	(0.164)	(0.164)	(0.167)	(0.178)	(0.162)
L2.Hometown Tie				-0.158**	
				(0.0617)	
Hometown Tie				-0.128	
				(0.135)	
Average Bond Coupon	0.0574**	0.0606**	0.0561*	0.0368	0.0480
	(0.0288)	(0.0292)	(0.0296)	(0.0276)	(0.0310)
(Log) GDP		-1.259	-1.482*	-1.256	-1.661*
		(0.843)	(0.887)	(0.903)	(0.891)
(Log) Population		-0.853***	-0.977**	-1.126**	-0.989**
		(0.323)	(0.386)	(0.434)	(0.460)
(Log) Fiscal Revenue			0.134	-0.0285	-0.120
			(0.390)	(0.489)	(0.333)
(Log) Fiscal Expenditure			0.339	0.381	0.402
			(0.289)	(0.518)	(0.263)
City-Term FE	Y	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y	Y
Turnover FE	N	N	N	N	Y
N	359	359	359	349	359
Adj. R^2	0.965	0.965	0.965	0.965	0.965

Note: This table shows OLS estimates for the effect of hometown tie on (the logarithm of) the level of local government outstanding debt. Columns (1)–(3) use the one-year-lagged hometown tie as the regressor of interest, with controls progressively added. Column (4) reports a distributed-lag specification with the contemporaneous, one-year-lagged, and two-year-lagged tie. Debt is a stock with substantial inertia (within-city-term AR(1) of 0.60). The contemporaneous tie cannot move the level within a year, while the lagged tie reduces it by roughly 28%, with the effect persisting into the second year. Column (5) adds year-within-term fixed effects to the full-control specification. Sample includes up to 191 prefecture-level cities in mainland China between 2003 and 2015 (the baseline excludes the four direct-controlled municipalities). Hometown tie equals 1 if the province leader is appointed after the city leader and shares the same hometown with the city leader this year or in the last year. Each observation is a city-year pair. Standard errors are two-way clustered at city and province-year. Political data comes from China Political Elite Dataset. Land data comes from China Real Estate Index System. City level economic data is from China National Bureau of Statistics. Standard errors in parentheses.

Table 7: Hometown Tie and Land Supply Ratio

Panel A. Residential land share	(1)	(2)	(3)	(4)
Hometown Tie	0.0513*	0.0472*	0.0464*	0.0600**
	(0.0290)	(0.0257)	(0.0258)	(0.0258)
(Log) GDP		0.378	0.316	0.366
		(0.329)	(0.346)	(0.359)
(Log) Population		-0.218	-0.250	-0.296
		(0.200)	(0.203)	(0.184)
(Log) Fiscal Revenue			0.0227	0.0476
			(0.137)	(0.130)
(Log) Fiscal Expenditure			0.107	0.108
			(0.122)	(0.125)
City-Term FE	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y
Turnover FE	N	N	N	Y
N	485	485	485	485
Adj. R^2	0.472	0.471	0.468	0.468

Panel B. Industrial land share	(1)	(2)	(3)	(4)
Hometown Tie	-0.0594*	-0.0530*	-0.0493*	-0.0710**
	(0.0320)	(0.0276)	(0.0287)	(0.0319)
(Log) GDP		-0.335	-0.210	-0.214
		(0.266)	(0.269)	(0.285)
(Log) Population		-0.367	-0.342	-0.324
		(0.249)	(0.245)	(0.252)
(Log) Fiscal Revenue			-0.182	-0.175
			(0.139)	(0.144)
(Log) Fiscal Expenditure			-0.0314	-0.0461
			(0.124)	(0.130)
City-Term FE	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y
Turnover FE	N	N	N	Y
N	485	485	485	485
Adj. R^2	0.502	0.508	0.509	0.501

Note: This table shows OLS estimates for the effect of hometown tie on the share of planned land supply by use type. Panel A reports the residential share and Panel B the industrial share, with controls added progressively across columns within each panel and column 4 the preferred specification. Sample includes up to 191 prefecture-level cities in mainland China between 2003 and 2015 (the baseline excludes the four direct-controlled municipalities). Hometown tie equals 1 if the province leader is appointed after the city leader and shares the same hometown with the city leader this year or in the last year. Each observation is a city-year pair. Standard errors are two-way clustered at city and province-year. Political data comes from China Political Elite Dataset. Land data comes from China Real Estate Index System. City level economic data is from China National Bureau of Statistics. Standard errors in parentheses.

Table 8: Hometown Tie and Unit Price of Land by Type

Panel A. (Log) residential land price	(1)	(2)	(3)	(4)
Hometown Tie	-0.258***	-0.263***	-0.266***	-0.205**
	(0.0955)	(0.0907)	(0.0930)	(0.104)
(Log) GDP		0.131	0.0400	-0.127
		(0.790)	(0.764)	(0.744)
(Log) Population		0.707	0.718	0.719
		(1.255)	(1.278)	(1.220)
(Log) Fiscal Revenue			0.177	0.286
			(0.456)	(0.457)
(Log) Fiscal Expenditure			-0.0727	-0.251
			(0.523)	(0.581)
City-Term FE	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y
Turnover FE	N	N	N	Y
N	506	506	506	506
$Adj.R^2$	0.860	0.860	0.858	0.858
Panel B. (Log) industrial land price	(1)	(2)	(3)	(4)
Hometown Tie	0.127*	0.119*	0.113*	0.141*
	(0.0716)	(0.0637)	(0.0644)	(0.0713)
(Log) GDP		0.803	0.524	0.458
		(0.581)	(0.562)	(0.560)
(Log) Population		-0.0740	-0.154	-0.104
		(0.660)	(0.676)	(0.722)
(Log) Fiscal Revenue			0.257	0.236
			(0.328)	(0.339)
(Log) Fiscal Expenditure			0.194	0.175
			(0.267)	(0.269)
City-Term FE	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y
Turnover FE	N	N	N	Y
N	506	506	506	506
$Adj.R^2$	0.688	0.686	0.685	0.678

Note: This table shows OLS estimates for the effect of hometown tie on the (log) sold price of residential land per unit building area (Panel A) and of industrial land (Panel B). Within each panel, columns add controls progressively, with column 4 the preferred specification. Sample includes up to 191 prefecture-level cities in mainland China between 2003 and 2015 (the baseline excludes the four direct-controlled municipalities). Hometown tie equals 1 if the province leader is appointed after the city leader and shares the same hometown with the city leader this year or in the last year. Each observation is a city-year pair. Standard errors are two-way clustered at city and province-year. In the no-control column of each panel, where the two-way clustered variance estimate is not positive semi-definite, city-clustered standard errors are reported (Cameron, Gelbach and Miller, 2011). Political data comes from China Political Elite Dataset. Land data comes from China Real Estate Index System. City level economic data is from China National Bureau of Statistics. Standard errors in parentheses.

Table 9: Hometown Tie and Land Sales Revenue

	(1) Total	(2) Residential	(3) Industrial	(4) Commercial
Hometown tie	-0.0983	0.0312	-0.104	0.0460
	(0.128)	(0.185)	(0.184)	(0.226)
one-year lag	-0.194*	-0.230	-0.113	-0.104
	(0.116)	(0.170)	(0.187)	(0.227)
two-year lag	0.105	0.182	0.112	0.104
	(0.122)	(0.144)	(0.196)	(0.230)
Cumulative effect	-0.187	-0.0165	-0.105	0.0458
Joint *p*-value	0.407	0.272	0.784	0.952
City-Term FE	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y
Turnover FE	Y	Y	Y	Y
N	562	609	576	593
R^2	0.955	0.928	0.870	0.895

Note: Distributed-lag estimates of (log) land sales revenue on the contemporaneous hometown tie (L_0) and its one- and two-year lags (L_1, L_2), entered jointly. Column (1) is *total* land sales revenue (residential, industrial, and commercial combined). Columns (2)–(4) decompose it by use type. "Cumulative effect" is the sum $L_0 + L_1 + L_2$. "Joint *p*-value" tests that the three lag coefficients are jointly zero. All specifications include the full control set together with city-term, province-year, and year-within-term fixed effects, with standard errors two-way clustered at city and province-year. Apart from a marginally significant one-year lag on total revenue ($p < 0.1$), no coefficient on total or residential revenue reaches significance, and the cumulative effects are insignificant throughout. We read the revenue margin as inconclusive rather than as a precisely estimated null (see text). Sample: 2003–2015, excluding the four direct-controlled municipalities and dismissed leaders. Each observation is a city-year pair. Land data come from the China Real Estate Index System. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Hometown Tie and Total Land Supply

	Dependent variable: (log) total planned land supply			
	(1)	(2)	(3)	(4)
Hometown Tie	-0.0501	-0.0350	-0.0345	-0.0347
	(0.119)	(0.124)	(0.128)	(0.129)
(Log) GDP		-0.882	-1.078	-1.134
		(0.808)	(0.813)	(0.762)
(Log) Population		-1.172	-1.350	-1.190
		(1.001)	(0.946)	(0.937)
(Log) Fiscal Revenue			-0.152	-0.225
			(0.428)	(0.423)
(Log) Fiscal Expenditure			0.627	0.740
			(0.485)	(0.513)
City-Term FE	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y
Turnover FE	N	N	N	Y
N	516	516	516	516
Adj. R^2	0.830	0.831	0.831	0.829

Note: This table shows OLS estimates for the effect of hometown tie on the logarithmic level of total land supply. Sample includes up to 191 prefecture-level cities in mainland China between 2003 and 2015 (the baseline excludes the four direct-controlled municipalities). Each observation is a city-year pair. Standard errors are two-way clustered at city and province-year. Political data comes from China Political Elite Dataset. Land data comes from China Real Estate Index System. City level economic data is from China National Bureau of Statistics. Standard errors in parentheses.

Table 11: The real economy recomposes: industrial engine down, residential and output up

Outcome	$\tau = +2$ effect (event study)	Net of drift	Pre-trend p-value	Placebo p-value	BH q-value	N
Panel A. The visible industrial engine contracts						
FDI, utilized (log)	−0.349	−0.21	0.98	0.040	0.067	1,543
FDI, contracted value (log)	−0.578	−0.52	0.11	0.002	0.010	1,030
FDI, number of contracts (log)	−0.163	−0.15	0.13	0.054	0.077	1,517
Secondary-sector employment growth	−0.099	−0.09	0.17	0.012	0.030	1,139
Non-agricultural population (log)	−0.058	−0.04	0.48	0.032	0.064	1,246
Panel B. Households and residential activity expand						
Residential floor space sold p.c. (log)	+0.156	+0.20	0.76	0.086	0.108	1,247
Panel C. Aggregate output rises (recomposition, not contraction)						
GDP growth	+0.050	+0.05	0.54	0.004	0.013	1,374
Secondary-industry output growth	+0.068	+0.06	0.51	< 0.002	0.010	1,370

Notes. Each row reports the $\tau = +2$ coefficient from the city-level event study of Section 5.2.4 around the city's first hometown tie, with province–year fixed effects, a separate city–term fixed effect per leader (absorbing cross-leader level shifts), and year-within-term fixed effects, and two-way clustering by province–year and city. "Net of drift" subtracts the mean of the outcome's 500-draw placebo distribution from the $\tau = +2$ estimate. The $\tau = +2$ estimand for *level* outcomes carries a mild mechanical within-term drift (placebo means of −0.134 for utilized FDI, −0.056 for contracted FDI, −0.045 for floor space per capita), while the *growth-rate* outcomes' placebo distributions are approximately centered on zero (means below 0.013 in absolute value). The pre-trend p-value is the joint test that the displayed leads ($\tau = -5, \ldots, -2$) are zero, and large values indicate flat pre-trends. The placebo p-value is a two-sided randomization-inference test, the share of 500 within-province permutations of superiors' birthplaces whose $\tau = +2$ coefficient is at least as large in absolute value as the estimate (the same procedure used for the core land, price, and debt results). Because it compares to the permutation distribution rather than to zero, it accounts for the drift by construction. The BH q-value applies the Benjamini–Hochberg false-discovery-rate correction to the placebo p-values across the full family of ten placebo-tested outcomes in Appendix Table 16.

Appendices

A Timing of the Hometown-Tie Effect

This appendix supports the timing choices made for each main outcome. For each outcome, we estimate a single regression including both the contemporaneous hometown tie (L_0) and the one-year-lagged tie (L_1), with the same controls and fixed-effect structure as the corresponding main table. The within-city-term first-order autocorrelation of the outcome is reported as a sluggishness gauge. Outcomes whose AR(1) is near zero are within-year decisions and load on L_0, while outcomes with substantial AR(1) load on L_1.

Table 12: Timing of the Hometown-Tie Effect Across Main Outcomes

Panel A. Planned land shares and local debt	Res. land share	Ind. land share	Com. land share	(Log) debt
Hometown Tie	0.0542**	-0.0445	-0.0127	-0.122
	(0.0268)	(0.0309)	(0.0173)	(0.137)
L.Hometown Tie	0.0277	0.0132	-0.0396***	-0.343*
	(0.0322)	(0.0328)	(0.0141)	(0.177)
AR(1) of outcome	-0.16	-0.13	0.00	0.60
City-Term FE	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y
N	456	456	456	359
Adj. R^2	0.467	0.511	0.289	0.965

Panel B. (Log) land prices and house-price growth	Res. land price	Ind. land price	Com. land price	HP growth
Hometown Tie	-0.215***	0.102	-0.171	-0.0543**
	(0.0824)	(0.0803)	(0.143)	(0.0215)
L.Hometown Tie	-0.00968	-0.0977	0.00459	-0.0464**
	(0.0660)	(0.0682)	(0.151)	(0.0215)
AR(1) of outcome	-0.01	0.00	-0.25	-0.24
City-Term FE	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y
N	765	678	710	386
Adj. R^2	0.824	0.611	0.575	0.425

Note: Each column estimates a single regression of the indicated outcome on the contemporaneous hometown tie (L_0, the row labeled Hometown Tie) and the one-year-lagged tie (L_1, the row labeled L.Hometown Tie), jointly. Panel A covers the planned land share by use type (residential, industrial, commercial) and (log) local government debt. Panel B covers the (log) sold land price per building area by use type and house-price growth. Land outcomes are from the CREIS land panel. House-price growth and (log) debt are from the house-price panel merged with the local-debt panel (Section 4). Controls include (log) GDP, (log) resident population, (log) government in-budget revenue, and (log) government in-budget expenditure. The HP regression additionally controls for L.pihpr and L_2.pihpr. The debt regression additionally controls for the average bond coupon. All specifications include city-term and province-year fixed effects, and standard errors (in parentheses) are two-way clustered at city and province-year. The AR(1) row reports the within-city-term first-order autocorrelation of the outcome as a sluggishness gauge.

B Robustness: Pooling the Direct-Controlled Municipalities and Including Dismissed Leaders

Table 13: Main Results Pooling the Four Direct-Controlled Municipalities

	(1) Promo.	(2) HP growth	(3) Debt	(4) Resid. share	(5) Indust. share	(6) Resid. price
GDP growth × tie	-3.416**					
	(1.327)					
Hometown tie	0.625***	-0.0437**		0.0706***	-0.0769***	-0.178*
	(0.227)	(0.0217)		(0.0261)	(0.0292)	(0.101)
Hometown tie (lag)			-0.376**			
			(0.155)			
N	411	413	378	503	503	522
Adj. R^2	0.0951	0.453	0.972	0.463	0.516	0.873

Note: This table re-estimates the preferred specification of each main result *pooling back in* the four direct-controlled municipalities (Beijing, Shanghai, Tianjin, and Chongqing), which the baseline sample excludes. Their ties are defined analogously at the central level (a newly appointed Politburo member sharing the leader's birthplace). Column (1) is the promotion linear-probability model, reporting the GDP-growth × hometown-tie interaction and the hometown-tie main effect. Columns (2)–(6) report the hometown-tie coefficient from the house-price-growth, (log) local-debt, residential land-share, industrial land-share, and (log) residential land-price regressions, respectively. The debt specification uses the one-year-lagged tie. Columns (2)–(6) include the full control set, city-term and province-year fixed effects, and two-way clustered standard errors, with samples harmonized as in the corresponding main tables (Tables 5, 6, 7, and 8). The land-share, land-price, and debt columns additionally include the year-within-term fixed effects of the preferred columns, while the house-price column includes two lags of the dependent variable and omits the within-term fixed effects, matching Table 5. Column (1) uses the specification of Table 4, column (4). The promotion estimation sample contains no leader-terms from the four municipalities, so this column is identical by construction. Every coefficient is essentially unchanged in magnitude relative to the baseline (the residential land price remains significant at the 10% level), confirming that nothing hinges on the treatment of the four municipalities. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

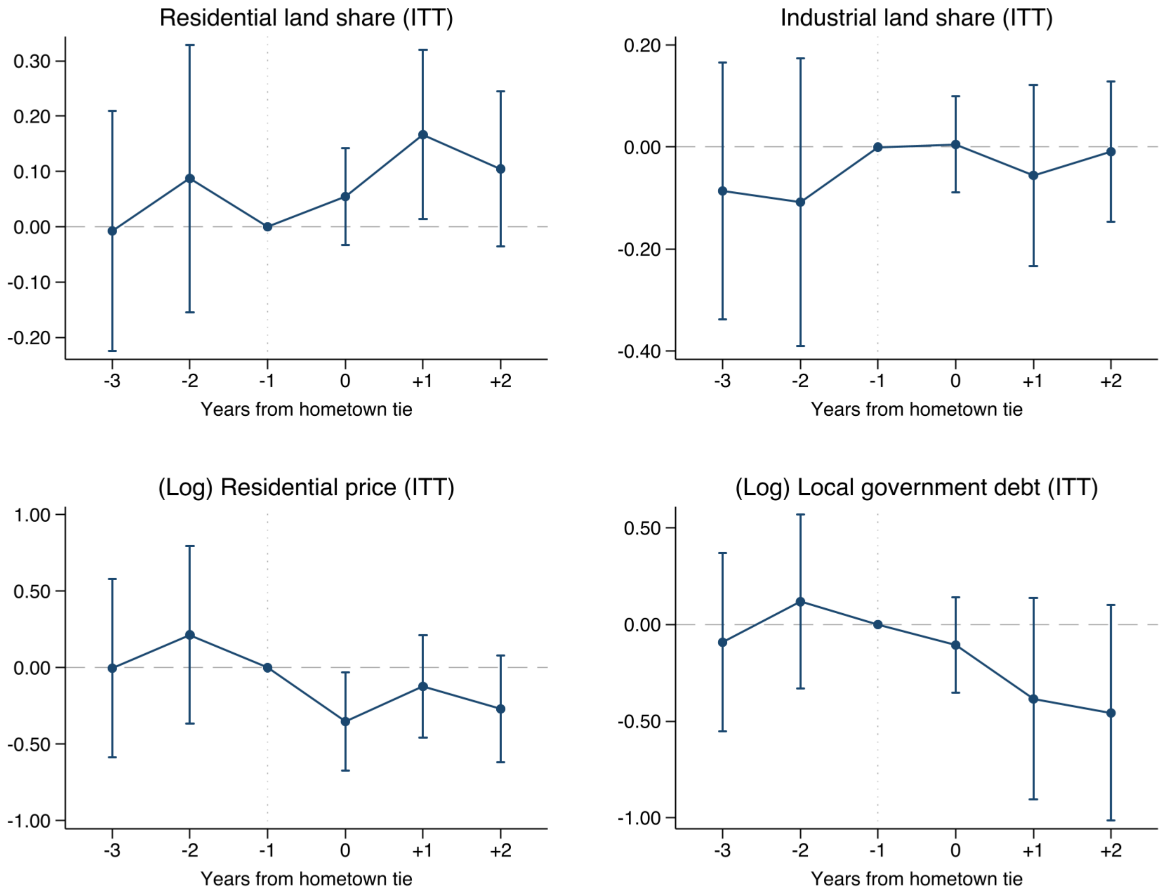
Table 14: Main Results Including the 56 Dismissed Leaders

	(1) HP Growth	(2) (Log) Debt	(3) Resid. Share	(4) Indust. Share	(5) (Log) Resid. Price
Hometown Tie	-0.0273		0.0668**	-0.0776**	-0.208**
	(0.0221)		(0.0256)	(0.0311)	(0.0927)
Hometown Tie (lagged)		-0.299**			
		(0.150)			
N	572	435	561	561	584
Adj. R^2	0.420	0.966	0.448	0.503	0.856

Note: This table re-estimates the preferred specification of each main outcome including the 56 city leaders dismissed during their careers (commonly for corruption), whom the baseline analysis excludes. Because investigation risk may itself correlate with political ties, this checks that the exclusion does not drive the results. Columns report the hometown-tie coefficient for house-price growth, (log) local debt (one-year-lagged tie), the residential and industrial land shares, and the (log) residential land price. Specifications, fixed effects, sample harmonization, and two-way clustering match the corresponding main tables. The land and debt results are essentially unchanged. The suggestive house-price coefficient attenuates. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C Intention-to-Treat Event Study

Figure 9: Event Study Without the Active-Tie Restriction (Intention to Treat)



Note: Intention-to-treat version of the event study of Section 5.2.4, in which post-tie dummies are assigned by event time alone, regardless of whether the originating tie is still active, and pre-tie leads enter regardless of tie status. The specification (a separate city-term fixed effect per leader, province-by-year and year-within-term fixed effects, binned out-of-window dummies, two-way clustering) is otherwise identical. Magnitudes are smaller than in the active-tie figures, as expected (the ITT averages over spells that have reverted), with the same shape and flat leads (joint lead p-values of 0.72, 0.74, 0.55, and 0.48 for the residential share, industrial share, residential price, and debt).

D No Effect on Aggregate Scale Outcomes

Table 15: Hometown Tie and Aggregate Real-Economy Outcomes

	(1) Δlog GDP	(2) (Log) Fixed Inv.	(3) (Log) RE Inv.	(4) (Log) Paved Road	(5) (Log) Sec. Industry
Hometown Tie	0.0154**	0.0213	0.0371	-0.00616	-0.0155
	(0.00640)	(0.0241)	(0.0341)	(0.0214)	(0.0141)
N	1365	1592	1589	1587	1592
Adj. R^2	0.558	0.992	0.978	0.975	0.996

Note: Each column regresses an aggregate real-economy outcome on the contemporaneous hometown tie, controlling for (log) resident population and (log) government in-budget revenue and expenditure, with city-term and province-year fixed effects. Standard errors are two-way clustered at city and province-year. The outcomes are annual GDP growth (Δlog GDP), (log) fixed-asset investment, (log) real-estate investment, (log) paved-road area, and (log) secondary-industry output. The hometown tie has no detectable effect on investment, infrastructure, or secondary-industry output. GDP growth is marginally *higher* (+1.5 percentage points, significant at the 5% level), foreshadowing the delayed recomposition documented in Section 5.4. The tie thus reallocates the *composition* of land use and curtails local debt without contracting the aggregate *scale* of investment or infrastructure. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

E Full Battery of Real-Economy Outcomes

The new real-economy results in Section 5.4 draw on a large set of candidate outcomes, so we report the full battery we examined, with pre-trend and placebo p-values, to make the multiple comparisons explicit. The discipline we impose is uniform, namely an outcome enters the confirmed set only if it is both pre-trend-clean and clears the within-province placebo. Table 16 lists every outcome and transformation, marking which survive and which are rejected, whether by a pre-trend violation, an opposite-signed estimate, or a failed placebo. We lead on the coherent sign pattern across the confirmed outcomes rather than on any single estimate.

Table 16: Full battery of real-economy outcomes examined

Outcome (form)	$\tau = +2$	Pre-trend p	Placebo p	Verdict
Panel A. Industrial / growth margin				
FDI, utilized (log)	-0.349	0.98	0.040 [q=0.07]	confirmed
FDI, contracted value (log)	-0.578	0.11	0.002 [q=0.01]	confirmed
FDI, number of contracts (log)	-0.163	0.13	0.054 [q=0.08]	confirmed
Secondary-sector employment growth	-0.099	0.17	0.012 [q=0.03]	confirmed
Non-agricultural population (log)	-0.058	0.48	0.032 [q=0.06]	confirmed
Secondary-industry GDP share	≈ 0	—	—	null
Secondary / manufacturing employment share	≈ 0	0.33	—	null
Industrial SO_2 emissions (log)	-0.305	0.003	—	fails pre-trend
Industrial SO_2 intensity (per GDP)	-0.279	0.00	—	fails pre-trend
Industrial wastewater (log)	$+0.186$	—	—	opposite sign
Panel B. Aggregate output				
GDP growth	$+0.050$	0.54	0.004 [q=0.01]	confirmed
Secondary-industry output growth	$+0.068$	0.51	< 0.002 [q=0.01]	confirmed
Panel C. Household / welfare margin				
Residential floor space sold p.c. (log)	$+0.156$	0.76	0.086 [q=0.11]	marginal
Residential floor space sold, level (log)	$+0.134$	0.86	0.168 [q=0.17]	fails placebo
Disposable income p.c. growth	$+0.026$	0.49	0.172 [q=0.17]	fails placebo
Disposable income / consumption p.c.	≈ 0	—	—	null
Retail sales (log)	≈ 0	—	—	null
Residential floor area p.c. (log)	≈ 0	—	—	null
Births / fertility rate	≈ 0	—	—	null
Number of households (log)	≈ 0	—	—	null
Panel D. Public goods / fiscal composition				
Education spending share	≈ 0	—	—	null
Health spending share / p.c.	-0.008	—	—	opposite sign
Social-security spending share	≈ 0	—	—	null
Enterprise-income / VAT tax share	-0.005	—	—	null at $+2$
Education, health physical provision p.c.	≈ 0	—	—	null
Panel E. Scale levels, revisited per capita and in growth				
Fixed-asset investment (p.c., growth)	≈ 0	—	—	null
Real-estate investment (p.c., growth)	≈ 0	—	—	null
Paved road (p.c., growth)	≈ 0	—	—	null
Secondary-industry output level (p.c.)	≈ 0	—	—	null
Panel F. Resources from above (alternative-mechanism checks)				
Net transfers from above (expenditure / own revenue, log)	≈ 0	0.80	—	null
Central appropriation received (any, linear probability)	-0.146	0.16	—	null
Central share of infrastructure funding	-0.051	0.47	—	null
Municipal infrastructure funding (log)	-0.352	0.07	—	null
Panel G. GDP by sector, value-added growth (EPS provincial-yearbook decomposition)				
Construction value-added growth	$+0.200$	0.20	0.008	confirmed
Industry value-added growth	$+0.055$	0.94	0.006	confirmed
Secondary value-added growth	$+0.060$	0.58	0.004	confirmed
Tertiary value-added growth	≈ 0	0.07	0.94	null

Notes. The full set of real-economy outcomes and transformations we examined, for transparency on the multiple comparisons behind Table 11. Each was run through the city-level event study of Section 5.2.4 and a joint pre-trend test. Outcomes that were pre-trend-clean and correctly signed were then put through the 500-draw within-province placebo (others show "—"). "≈ 0" denotes coefficients indistinguishable from zero at all horizons. Bracketed q-values apply a Benjamini–Hochberg false-discovery-rate correction across the ten placebo-tested outcomes. Confirmed outcomes clear the placebo with flat pre-trends, and we lead on the coherent sign pattern, not single estimates. Rejected candidates fail the pre-trend test (industrial SO_2) or the placebo (floor space sold in levels, disposable-income growth). Industrial wastewater and health spending are opposite-signed and were not placebo-tested. Panels A–E and the net-transfer ratio use CEIC prefecture statistics and the analysis panel. The net-transfer ratio (general-budget expenditure over own revenue) proxies transfers from above, as cities could not borrow on-budget in this period. Panel F (municipal-infrastructure funding by source, Urban Construction Statistical Yearbook) and Panel G (city GDP by industry, EPS provincial yearbooks, which carry the industry-construction split for many more cities and years than the national series) respectively test the preferential-treatment alternative of Section 5.3.4 and decompose the output response. Every Panel F estimate is negative or zero (opposite to resource favoritism). The Panel G construction, industry, and secondary growth results clear the placebo at 1%, so the construction channel is shown rather than inferred. The aggregate GDP-growth result is in Table 11.

Table 17: Untied Province-Mates Do Not Respond When a Province-Mate Becomes Tied

	(1) GDP growth	(2) FDI (log)	(3) Resid. share
Province-mate tied	−0.006	0.006	0.018
	(0.005)	(0.056)	(0.014)
one-year lag	−0.001	0.040	0.018
	(0.007)	(0.068)	(0.015)
two-year lag	0.001	−0.001	0.023
	(0.006)	(0.069)	(0.016)
Joint p-value	0.522	0.946	0.306
City FE	Y	Y	Y
Year FE	Y	Y	Y
N	1,178	1,165	517
R^2	0.460	0.916	0.637

Notes. A falsification of the resource-favoritism and discouraged-competitor readings of the output result in Section 5.4. The sample is untied city-years, and the regressor is an indicator for any province-mate being tied that year. Each column enters the contemporaneous indicator together with its first two annual lags, for GDP growth, foreign investment, and the residential land share respectively. Specifications absorb city and year fixed effects, so the coefficient is identified off variation in tied province-mates within a city over time, and standard errors are clustered by province, the level of treatment. The dependent variables are annual GDP growth, log utilized foreign investment, and the residential share of planned land supply. No coefficient is individually significant and the joint test of the tie and its lags fails to reject zero in every column, so when a city's province-mate becomes tied the untied city's own growth, investment, and land composition do not move. A province channeling resources to the tied city, or untied competitors slowing down in a zero-sum tournament, would both have moved these outcomes.

F Robustness of the First Stage to the Promotion-Coding Critique

A recent replication re-examines several prefecture-level promotion studies and reports that the unconditional growth-promotion gradient is sensitive to alternative promotion codings, control sets, and outcome definitions (Wiebe, 2024). Our identification does not rest on that unconditional gradient. The object we estimate is the *difference* in the growth-promotion slope between tied and untied leaders evaluated by the same provincial committee in the same year, which a level shift in the average gradient leaves intact. Two checks indicate that the gradient we do estimate is not a spurious aggregate. First, it is not an artifact of how growth is measured, in that the interaction is similar whether GDP growth is measured nominally, in real terms, or per capita. Second, decomposing the gradient by GDP component shows it is concentrated where the attributability logic of Section 3 predicts. Splitting aggregate growth into the additive contributions of the primary, secondary, and tertiary sectors (Table 18), only the secondary-sector contribution carries a positive promotion gradient, and the tie severs that contribution specifically, both on its own and in a joint specification, whereas the primary and tertiary contributions are neither robustly rewarded nor severed. A correlation that merely happened to survive in the aggregate would not localize on the visible industrial margin in this way. We cannot separate industry from construction within the secondary sector in the promotion regression, because construction is a small and volatile share of GDP, but the real-economy evidence of Section 5.4 speaks to that split directly.

Table 18: Decomposing the Promotion Gradient by GDP Component

GDP component	Gradient (one at a time)	× Hometown Tie (one at a time)	× Hometown Tie (joint)
Aggregate GDP growth	1.28**	−2.05*	—
	(0.57)	(1.06)	
Primary contribution	1.59	−9.99*	−8.78
	(2.94)	(5.70)	(6.41)
Secondary contribution	1.46*	−4.70***	−3.99**
	(0.83)	(1.43)	(1.78)
Tertiary contribution	2.58	−1.85	+1.91
	(2.26)	(4.67)	(4.85)

Notes. Each sector's *contribution* to aggregate annualized GDP growth is its change over the leader's term divided by the years elapsed and by GDP at the term's start year, so the primary, secondary, and tertiary contributions sum to aggregate GDP growth by construction. The dependent variable is the leader's end-of-term promotion, and the specification, fixed effects (province-year, city, and the demographic and rank controls), and two-way clustering match the first-stage regression of Table 4. The first column reports the promotion gradient on each contribution entered one at a time, the second the interaction of that contribution with the hometown tie entered one at a time, and the third the tie interaction when the primary, secondary, and tertiary contributions are entered jointly. Only the secondary contribution is both robustly rewarded (a positive gradient) and robustly severed by the tie, including in the joint specification. The primary and tertiary contributions are not robustly rewarded, and their tie interactions do not survive the joint specification. Industry and construction cannot be separated within the secondary sector here, because construction is a small and volatile share of GDP and is thinly observed across leader terms. Sample is completed leader terms, 2003–2015, excluding the four direct-controlled municipalities. $*$, $**$, $***$ denote significance at the 10%, 5%, and 1% levels.

G Specificity: Hometown Ties versus Other Social Ties

Table 19: The Effect Is Specific to Hometown Ties

	Resid. share	Indust. share	Resid. land price	(Log) debt	HP growth
Hometown tie	0.060**	-0.071**	-0.205**	-0.373**	-0.051**
	(0.025)	(0.032)	(0.104)	(0.162)	(0.023)
Party-school tie	-0.031	0.002	0.064	0.094*	0.034*
	(0.023)	(0.025)	(0.063)	(0.056)	(0.020)
Original-school tie	-0.020	-0.003	0.022	-0.133*	-0.034
	(0.020)	(0.031)	(0.113)	(0.076)	(0.049)

Note: Each cell reports the coefficient on a hometown, shared-party-school, or shared-original-school tie from the preferred specification for each outcome (full controls, city-term, province-year, and year-within-term fixed effects, with two-way clustered standard errors). Outcome samples are harmonized as in the corresponding main tables, so the hometown-tie row reproduces the headline point estimates exactly. The debt column uses the one-year-lagged tie. The house-price-growth column omits within-term fixed effects and includes two lags of the dependent variable, matching the main tables. The three tie types are equally valid social connections (and the party-school tie is in fact *more* common in the sample than the hometown tie), yet only the hometown tie produces the paper's pattern of results. The scattered marginal coefficients on the school ties are discussed in the text. Consistent with this, only the hometown tie removes the sensitivity of promotion to GDP performance in the first stage (the tie$\times$GDP-growth interaction is -3.42, $t = -2.6$, for hometown ties, and statistically insignificant for both the party-school tie, $+0.90$, $t = 0.8$, and the original-school tie, -0.75, $t = -0.5$). Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

H Heterogeneity by Promotion Stakes

Table 20: The Effect Is Concentrated Among Leaders Still in the Promotion Tournament

	Resid. share	Indust. share	Resid. land price	(Log) debt
Hometown tie	0.061**	-0.078**	-0.210**	-0.474**
	(0.029)	(0.036)	(0.105)	(0.206)
× out-of-tournament	-0.011	0.050	0.142	0.377*
	(0.060)	(0.036)	(0.190)	(0.196)
Implied: out-of-tournament	0.051	-0.027	-0.068	-0.097*
	(0.050)	(0.022)	(0.170)	(0.052)

Note: Each column adds to the preferred specification an interaction between the hometown tie and an indicator for leaders who are *out of the promotion tournament*, namely those past the customary age ceiling for their rank (start-of-term age above 55 for prefecture rank, above 60 for sub-provincial rank, and above 65 for provincial rank), for whom further promotion is effectively foreclosed. The first row reports the tie effect for leaders still in the tournament (high stakes). The second row the differential for out-of-tournament leaders. The third the implied effect for out-of-tournament leaders (the sum of the two). Consistent with the career-incentive mechanism, the effect on every outcome is concentrated among high-stakes leaders and attenuates toward zero for those out of the tournament, significantly so for local debt. The debt column uses the one-year-lagged tie. Controls, fixed effects (city-term, province-year, year-within-term), and two-way clustering match the preferred columns of the main tables. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

I Land Area vs. Building Area

Table 21: Hometown Tie and Residential Land Ratio (Land Area)

Panel A. Residential land-area share	(1)	(2)	(3)	(4)
Hometown Tie	0.0298	0.0295	0.0303	0.0406**
	(0.0259)	(0.0218)	(0.0216)	(0.0204)
(Log) GDP		0.0520	0.0295	0.0413
		(0.283)	(0.288)	(0.295)
(Log) Population		-0.0679	-0.107	-0.119
		(0.153)	(0.162)	(0.149)
(Log) Fiscal Revenue			-0.0675	-0.0539
			(0.114)	(0.120)
(Log) Fiscal Expenditure			0.143*	0.140
			(0.0858)	(0.0986)
City-Term FE	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y
Turnover FE	N	N	N	Y
N	516	516	516	516
Adj. R^2	0.498	0.492	0.491	0.481

Panel B. Industrial land-area share	(1)	(2)	(3)	(4)
Hometown Tie	-0.0594*	-0.0556**	-0.0529*	-0.0666**
	(0.0320)	(0.0268)	(0.0274)	(0.0300)
(Log) GDP		-0.175	-0.0617	-0.0673
		(0.315)	(0.304)	(0.324)
(Log) Population		-0.391	-0.373	-0.357
		(0.276)	(0.278)	(0.283)
(Log) Fiscal Revenue			-0.133	-0.128
			(0.143)	(0.153)
(Log) Fiscal Expenditure			-0.0479	-0.0634
			(0.111)	(0.121)
City-Term FE	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y
Turnover FE	N	N	N	Y
N	516	516	516	516
Adj. R^2	0.461	0.464	0.463	0.450

Note: This table repeats Table 7 measuring land shares by land area rather than building area. Panel A is the residential land-area share and Panel B the industrial, with controls added progressively across columns and column 4 the preferred specification. Sample, definitions, fixed effects, and two-way clustering are as in Table 7 (here covering 2004–2015). In the no-control column of each panel, where the two-way clustered variance estimate is not positive semi-definite, the city-clustered standard error is reported (Cameron, Gelbach and Miller, 2011). Standard errors in parentheses.

J Commercial Land

Table 22: Hometown Tie and Commercial Land: Share and Supply

Panel A. Commercial share of planned supply

	(1)	(2)	(3)	(4)
Hometown Tie	-0.000322	-0.000417	-0.00342	-0.000745
	(0.0213)	(0.0221)	(0.0231)	(0.0209)
(Log) GDP		-0.202	-0.262	-0.309
		(0.251)	(0.293)	(0.283)
(Log) Population		0.561**	0.572***	0.610***
		(0.231)	(0.214)	(0.231)
(Log) Fiscal Revenue			0.167	0.139
			(0.115)	(0.109)
(Log) Fiscal Expenditure			-0.0918	-0.0715
			(0.0566)	(0.0582)
City-Term FE	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y
Turnover FE	N	N	N	Y
N	485	485	485	485
Adj. R^2	0.264	0.300	0.301	0.313

Panel B. (Log) planned commercial supply

	(1)	(2)	(3)	(4)
Hometown Tie	-0.0694	-0.0559	-0.0760	-0.00652
	(0.149)	(0.151)	(0.157)	(0.147)
(Log) Total Supply	0.992***	0.996***	0.997***	0.998***
	(0.0914)	(0.0987)	(0.0988)	(0.102)
(Log) GDP		-2.162	-2.670	-2.978
		(1.703)	(1.942)	(1.900)
(Log) Population		3.139**	3.132***	3.357***
		(1.234)	(1.138)	(1.250)
(Log) Fiscal Revenue			1.068	0.880
			(0.692)	(0.709)
(Log) Fiscal Expenditure			-0.317	-0.157
			(0.414)	(0.450)
City-Term FE	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y
Turnover FE	N	N	N	Y
N	485	485	485	485
Adj. R^2	0.798	0.805	0.805	0.807

Note: OLS estimates for the effect of hometown tie on commercial land outcomes. Panel A: the commercial share of planned building-area supply. Panel B: the (log) level of planned commercial building-area supply, controlling for the (log) total land supply. In its no-control column (1), where the two-way clustered variance estimate is not positive semi-definite, city-clustered standard errors are reported (Cameron, Gelbach and Miller, 2011). The sample is the baseline panel (up to 191 prefecture cities, 2003–2015, excluding the four direct-controlled municipalities). Each observation is a city-year pair. Standard errors are two-way clustered at city and province-year, in parentheses. Land data: China Real Estate Index System. Political data: China Political Elite Dataset.

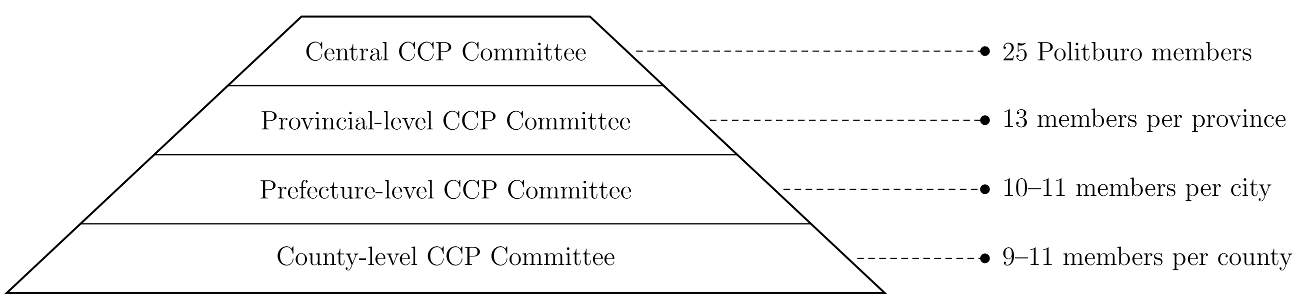
Table 23: Hometown Tie and Commercial Land Unit Price

	Dependent variable: (log) commercial land price			
	(1)	(2)	(3)	(4)
Hometown Tie	-0.0695	-0.0888	-0.106	-0.120
	(0.0951)	(0.0968)	(0.101)	(0.102)
(Log) GDP		1.829	1.141	1.168
		(1.175)	(1.216)	(1.168)
(Log) Population		-0.0669	-0.151	-0.151
		(0.431)	(0.427)	(0.385)
(Log) Fiscal Revenue			0.919	1.111
			(0.718)	(0.688)
(Log) Fiscal Expenditure			0.0695	-0.181
			(0.570)	(0.567)
City-Term FE	Y	Y	Y	Y
Prov-Year FE	Y	Y	Y	Y
Turnover FE	N	N	N	Y
N	501	501	501	501
$Adj.R^2$	0.677	0.678	0.679	0.682

Note: OLS estimates for the effect of hometown tie on the (log) sold price of commercial land per unit building area. Sample and inference as in Appendix Table 22.

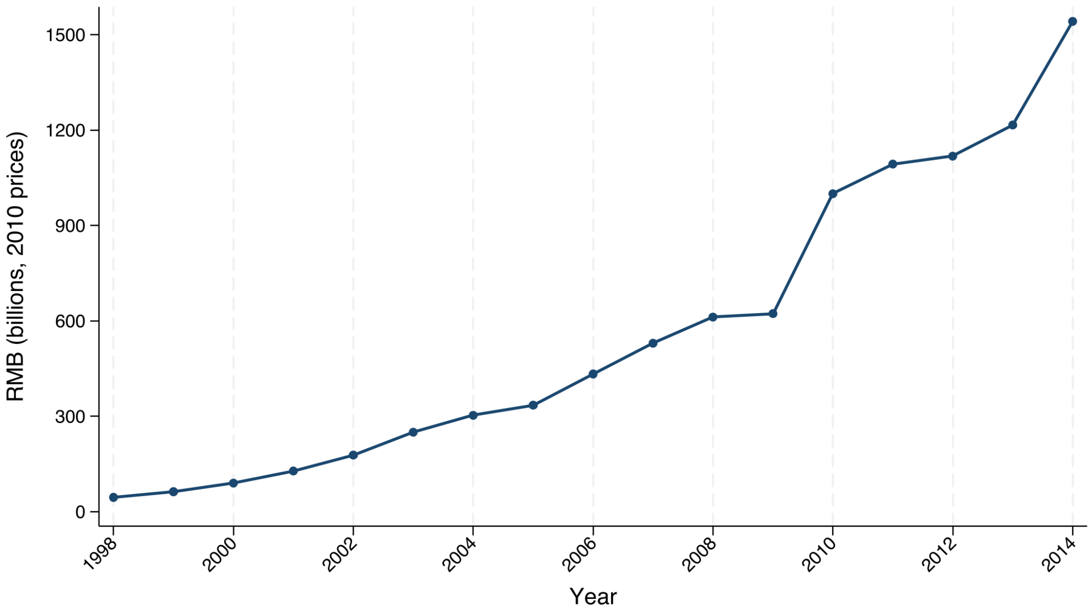
K Additional Descriptive and Institutional Figures

Figure 10: The Political Pyramid of the Communist Party of China



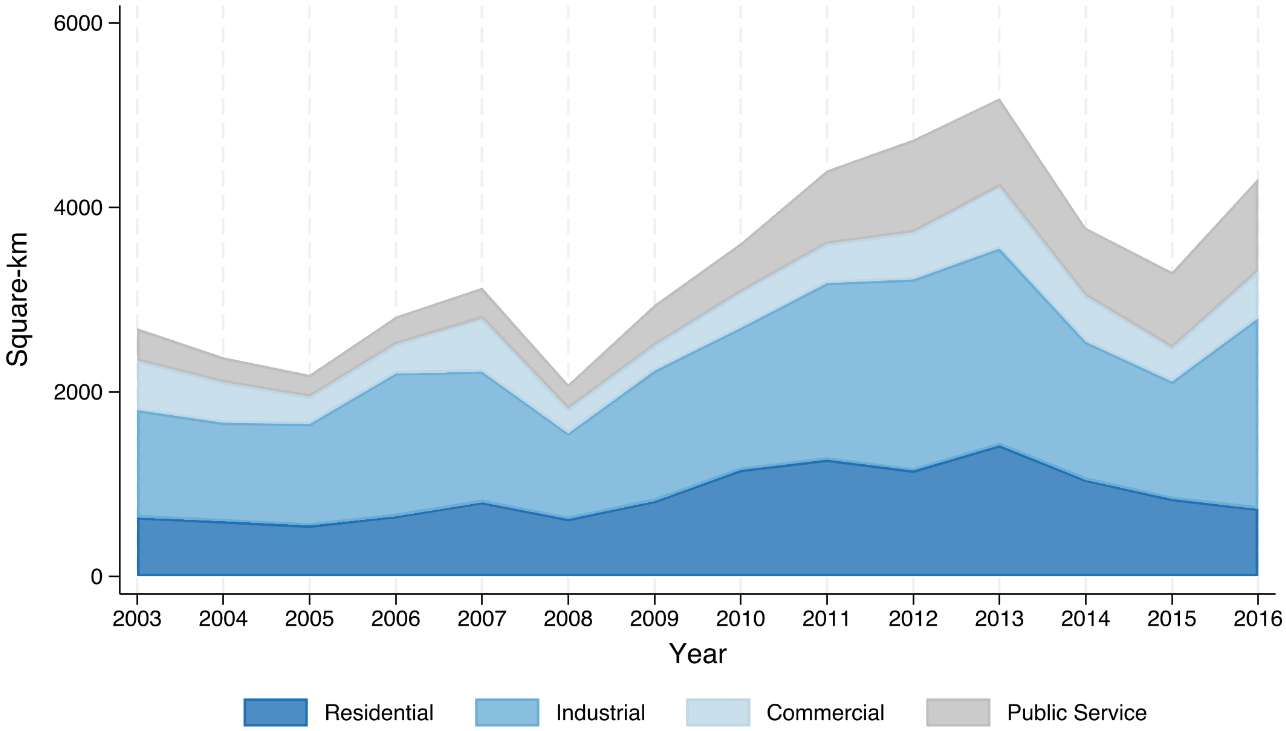
Note: This graph plots the organizational structure of the Communist Party of China.

Figure 11: Total Land Sales to Real Estate Developers (RMB Billions)



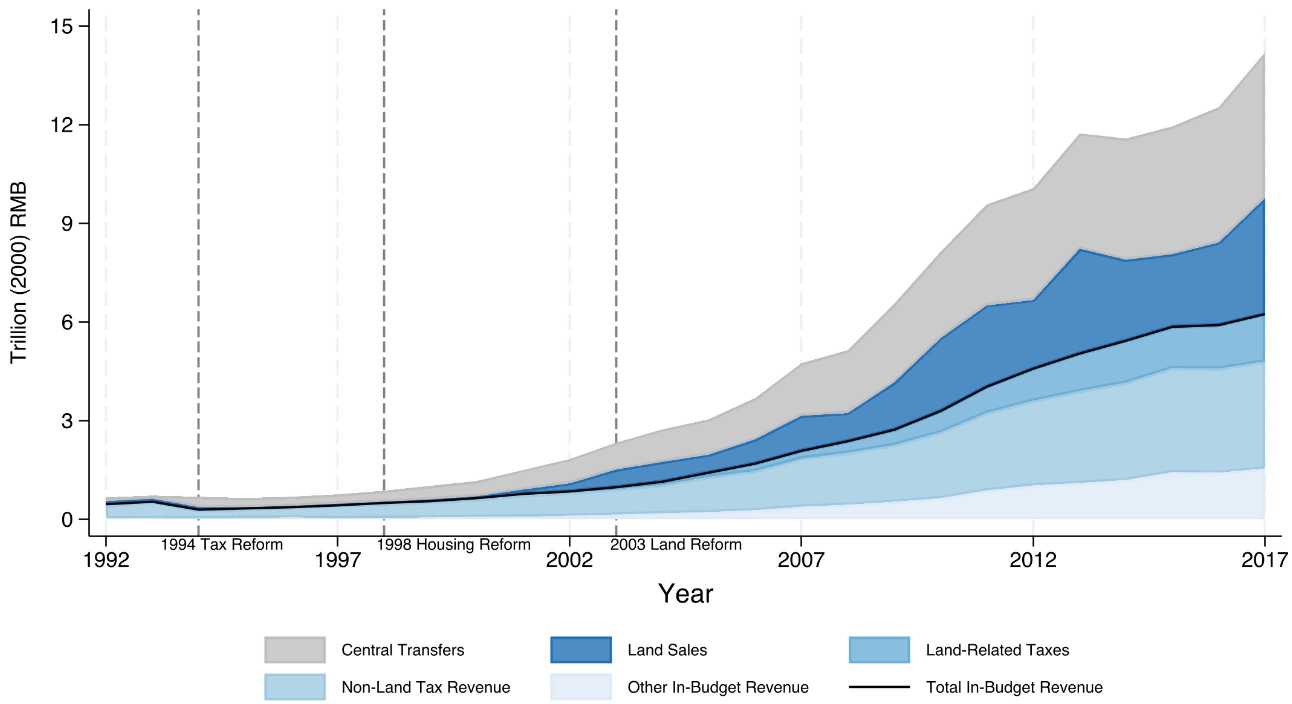
Note: This graph shows the annual level of total land sales revenue to real estate developers in the Chinese land market, in RMB billions. We collected data from China Statistics Yearbook (1999–2016). Prices are adjusted by CPI (2010=1).

Figure 12: Land Allocation by Use Type



Note: This graph plots the composition of land supplied between 2003 and 2016 in China. Omitted categories are land for water facilities, transportation, and special purpose. Data is collected by China National Bureau of Statistics.

Figure 13: Local Governments' Financial Structure



Note: This graph plots local Chinese governments' financial structure between 1992 and 2017. Land sales data is from Zhang (2009) for 1992–1999 and from Finance Yearbook of China for 2000–2017. Government budget data comes from China National Bureau of Statistics.