# Fiscal Incentives, Political Tenure, and Capital Misallocation in China *

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*Preliminary Draft*

**Abstract**

Correcting capital misallocation requires local politicians with both the capacity and the incentive to act. Using more than one million manufacturing firm-years matched to mayoral records, I find that misallocation falls as a mayor's tenure lengthens, but only in cities that rely on their own revenues; where local budgets depend on transfers, tenure brings little improvement. I develop a spatial model with interprovincial trade and costly migration in which tenure builds a mayor's capacity to alter capital allocation, while fiscal incentives determine the distortion the mayor seeks to implement. Changing fiscal incentives raises manufacturing TFP by up to 0.46 percent; trade spreads the gains to every province, and deeper market integration raises the cost of the distortion, making fiscal reform and integration complements. Short tenure is costly only when politicians have the incentive to use accumulated control.

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# 1 Introduction

Prefecture mayors in China formally serve five-year terms, yet complete only 2.8 years in office on average before promotion, lateral transfer, or retirement ends the spell.[1] While in office, a mayor exercises substantial administrative control over local capital allocation: the assignment of land, the coordination of state-bank lending, subsidies and tax treatment, and the restructuring of municipally owned firms. Existing work disagrees on whether longer tenure improves government: it can raise politician effort (Dal Bó and Rossi, 2011) or deepen the collusion that repeated interaction with the same contractors makes possible (Coviello and Gagliarducci, 2017), and China's own cadre-management system, built on rotation, promotion tournaments, and personnel control (Li and Zhou, 2005; Xu, 2011; Jia et al., 2015), is explicitly designed to limit how long an official can entrench local relationships. This paper asks a sharper question: does short political tenure constrain the correction of capital misallocation, and does its effect depend on the fiscal incentives facing the politician?

Chinese prefectures differ sharply in how they pay for local government. The 1994 tax-sharing reform recentralized revenue collection while leaving expenditure mandates local, so some cities finance most spending from their own current revenue while others depend heavily on transfers from above.[2] The distinction matters economically, not merely administratively. A mayor whose budget tracks local output internalizes the productivity consequences of who holds capital; a mayor financed by transfers does not, and in a system where visible local prosperity can reduce next year's allocation, the marginal fiscal return to reallocating capital away from favoured firms can be zero or negative. This is the logic long emphasized in work on revenue retention and the soft budget constraint under Chinese-style federalism (Qian and Roland, 1998; Jin et al., 2005; Han and Kung, 2015): what a local government does with the instruments it controls depends on what it is allowed to keep.

I combine the Annual Survey of Industrial Firms for 2000–2007, covering 1.76 million manufacturing firm-years, with the tenure records of prefecture mayors and party secretaries and with fiscal dependence measured before the sample period begins. Misallocation, measured as the dispersion of firms' marginal revenue products around the efficient allocation (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009), declines as a mayor's tenure lengthens, but only in cities that finance their own budgets; in transfer-dependent

[1] Within the paper's 2000–2007 data period, 316 completed mayoral spells average 2.8 years (median 3.0); only 10 percent reach the fifth year of a term. Party secretaries show the same pace over the same period (238 completed spells, 2.8 years, 10 percent reaching year five).

[2] Dependence is the 1995–99 average share of expenditure financed by transfers, measured before the sample period opens and split at the median; the measure correlates at 0.66 with its 2000–07 realization, so it captures a persistent feature of a city's fiscal position rather than a transitory shock.

cities the same additional year in office buys no improvement, and the relationship weakens smoothly as fiscal dependence rises rather than jumping at an institutional threshold. Neither tenure nor fiscal dependence is experimentally assigned, so what follows documents conditional patterns that discipline a model of political constraints rather than identify the causal effect of either institution in isolation.

The adjustment is concentrated on the ownership margin. State-linked firms hold capital at a marginal revenue product roughly 0.49 log points below private firms', and as a self-financing mayor's tenure lengthens, private firms' returns fall toward the state sector's rather than the reverse. The same pattern shows up together in subsidy rates, borrowing costs, and the pace of ownership conversion, not as separate mechanisms but as one wedge read off every price the local budget can see. Throughout the paper, favouritism means this wedge: the implicit capital-price advantage a city government extends to state-linked firms, whatever mix of credit, subsidy, tax, and land instruments carries it. The improvement does not accumulate indefinitely: around each mayoral transition the ownership gap widens sharply and then narrows again over the new mayor's term, a sawtooth consistent with an incoming mayor inheriting the status quo and gradually acquiring the capacity to change it.

These facts point to two structural objects governing realized favouritism. Tenure determines how much control a mayor has accumulated over inherited allocation arrangements, a capacity margin I denote $\mathcal{K}(a)$. Fiscal incentives determine the allocation toward which that accumulated control is used, a target I denote $\chi^*(\phi)$: the wedge a mayor would choose given full command of the allocation machinery. Realized favouritism blends the inherited default and the target in proportion to how much control tenure has delivered, so fiscal dependence changes the destination, tenure changes how far a mayor can travel toward it, and turnover resets the city toward the point it started from.

I embed this political block in a multi-region spatial model with internal trade and costly migration in the tradition of Tombe and Zhu (2019), and use it to compare reforms to the two margins. Removing political rotation entirely, a ceiling that grants a mayor permanent control, raises manufacturing TFP by 0.24 percent; changing the fiscal incentive instead, so that every city faces the fiscal cost of its own favouritism, raises TFP by 0.46 percent, roughly twice as much.[3] Tenure changes the speed at which capital allocation moves; fiscal incentives change the target it moves toward. In this setting, the target matters more than the speed.

The paper's first contribution is to political economy. Work on tenure and government

[3]Delivering the statutory five-year term under observed incentives raises TFP by 0.07 percent and welfare by 0.03; the fiscal-incentive reform's cycle-average gain is 0.46 percent, or 0.15 as a flow average solved year by year along the term. The exercise changes an incentive-indexed wedge, not the transfer system itself: transfers and their public-goods value are held fixed, and no government budget re-clears.

performance finds effects that run in both directions (Dal Bó and Rossi, 2011; Coviello and Gagliarducci, 2017), and studies of Chinese cadre rotation have emphasized its role in political selection and inter-regional competition (Li and Zhou, 2005; Jia et al., 2015; Jiang and Mei, 2020). The results here suggest a reason the sign can differ across settings: tenure is a capacity, not an incentive, and its consequences for policy depend on whether the official gains from using that capacity well.

The second contribution links two literatures that have developed largely apart. Research on fiscal federalism studies how revenue retention and the threat of a soft budget constraint shape local incentives (Qian and Roland, 1998; Jin et al., 2005; Han and Kung, 2015); research on misallocation typically treats firm-level wedges as a static object to be measured and priced (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009), with a China literature that traces such wedges to cheap capital and preferential treatment for state-linked firms (Song et al., 2011; Brandt et al., 2012; Bai et al., 2020; Li et al., 2022). I give one such wedge a political law of motion tied to an official's tenure and fiscal incentive, then carry the estimated evolution into a spatial equilibrium that prices how local distortions propagate through trade and migration (Fan, 2019; Lu et al., 2024). The result is a setting in which the size of a reform's local political benefit, not merely its administrative reach, determines its aggregate consequence.

The paper is organized as follows. Section 2 describes the institutional background. Section 3 presents the data, empirical strategy, and main results. Section 4 develops the structural model, and Section 5 calibrates it. Section 6 introduces the political block, and Section 7 presents the counterfactuals. Section 8 concludes.

# 2 Institutional Background: Two Margins of Capital Allocation

## 2.1 The Mayor and the Party Secretary

Each Chinese prefecture is led by two officials: the party secretary, who ranks first and has primary responsibility for personnel and political matters (including, through the party's cadre system, the managers of state-linked firms) and the mayor, who heads the government and has formal responsibility for land, economic administration, and the municipal bureaus that implement local policy. These portfolios overlap in practice, so office is not an exclusion restriction. The government side's allocative power runs through individually negotiated deals.[4] Both are appointed by the province, not elected, and both

[4] Bai et al. (2020) describe a prefecture whose mayor and vice-mayors personally brokered the arrangements of some two hundred favoured firms.

are subject to the cadre rotation system, under which the formal term of office is five years.

The formal term is not the effective one. In the prefecture leader records used throughout this paper, restricted to the 2000–2007 period this paper analyzes, 316 completed mayoral spells average 2.8 years and the median is 3.0; only 10 percent of mayors are still in office in the fifth year of a term (Figure 1, panel (a)). Party secretaries show the same pace over the same period (238 completed spells, 2.8 years on average, 10 percent reaching year five). Turnover is initiated from above (promotion, lateral transfer, or retirement), and mid-term replacement is the norm rather than the exception. This gap between the designed and the delivered term is the institutional fact the paper's political block is built around: if control over the allocation machinery takes most of a term to accumulate, the effective term determines how much of that accumulation is ever used.

## 2.2 Fiscal Dependence after the 1994 Tax-Sharing Reform

The 1994 tax-sharing reform recentralised revenue while leaving expenditure mandates local, and it did so unevenly: some prefectures finance most of their spending from own current revenue, others depend on transfers from above. The split has a clear geography: the coast largely pays for itself while the interior is financed from above (Figure 1, panel (b)). I measure dependence as the 1995–99 average share of expenditure financed by transfers (the residual of total over own current revenue in the budget accounts) and split cities at the median (Figure A1). Two properties make the measure usable. It is predetermined: the window closes before the outcome period opens. And it is persistent: the 1995–99 measure correlates at 0.66 with its 2000–07 realisation across 288 cities, so it describes a structural feature of a city's fiscal position rather than a transitory shock. The economic content is the mayor's stake: a self-financing mayor's discretionary budget rises with the local tax base, while a transfer-financed mayor's budget is insulated from local efficiency, and visible prosperity can reduce next year's transfers, so the marginal fiscal return to reform can be negative.

## 2.3 Instruments of Local Capital Allocation

The mayor's office allocates capital through instruments that are administrative rather than legislative: land is supplied by negotiated assignment or auction at the city's discretion; bank lending is coordinated through the municipal branches of the state banks; subsidies, tax treatment, and the restructuring or privatisation of municipally owned firms are decided within the government the mayor heads. The party secretary has a different formal portfolio but can influence these decisions through the party hierarchy. The office comparison in Section 3 is therefore a joint-office empirical contrast, not a placebo or

exclusion restriction. Three of these instruments are observable in the firm accounts (subsidies, borrowing costs, and ownership conversion), and Section 3.5 shows each moving with the mayor's tenure in the direction the fiscal incentive predicts. The cheap money is visible as a price, but the on-book discount is far too small to account for the full return gap on its own, consistent with most of the subsidy running off the income statement.[5]

These institutions yield a simple prediction. Tenure should improve capital allocation only when the mayor's fiscal position rewards the use of accumulated control. The next section tests that prediction and identifies the firm-level margin on which it operates.

# 3 Reduced Form Results

This section follows the causal chain in the data. It first establishes that misallocation falls with mayoral tenure and that fiscal dependence switches off that decline. It then shows that the adjustment runs through the state–private capital-allocation margin and that turnover resets it. The final subsection maps these facts into the two objects disciplined in the structural model: the fiscal target wedge and tenure-dependent administrative control.

## 3.1 Data

Table 1 summarizes the three data layers.

**Manufacturing firms.** The Annual Survey of Industrial Firms, 2000–2007, unbalanced: 1.76 million usable firm-years covering 540,298 enterprises, with revenue, cost of sales, the wage bill, employment, net fixed assets, subsidy income, and the composition of paid-in capital by ownership type, at the two-digit industry level.

**City leaders.** Prefecture mayors and party secretaries with personal identifiers, ages, education and appointment dates, giving each city-year two tenure clocks. Tenure is capped at five years, the nominal term length.

**Fiscal dependence.** From the 1993–2007 prefecture fiscal statistical volumes, dependence is the 1995–99 average share of expenditure financed from transfers rather than own current revenue, measured prior to every outcome window for 274 cities and split at the median.

[5]Over 2000–2007, majority state firms paid 2.9 cents of interest per yuan of capital against 3.2 for private firms, a 9.5 percent discount, with modestly higher direct subsidy rates.

## 3.2 Measuring Misallocation

For each firm-year I compute revenue productivity as

$$\ln \mathrm{TFPR}_{it} = \ln VA_{it} - \alpha_s \ln K_{it} - (1 - \alpha_s) \ln W_{it}, \tag{1}$$

demeaned within industry × year, with $\alpha_s$ recovered from industry wage-bill shares under the Hsieh and Klenow (2009) adjustment. The outcome is the absolute deviation: the firm's own distance from the efficient allocation. Throughout, the return gap and the policy instruments are measured objects; reading the gap as an implicit subsidy is an interpretation, supported by the observed price levels of Section 2, the covariation of the gap with observed credit advantages in Section 3.5, and the accounting of Section 6; and the step from dispersion to output loss follows Hsieh and Klenow (2009) under stated assumptions.

## 3.3 Empirical Strategy

Every specification includes mayor tenure, its interaction with predetermined transfer dependence, and the stated controls. Appendix D collects the party-secretary results.

$$\widetilde{\left|\ln \mathrm{TFPR}_{it}\right|} = \beta_M t_{ct}^M + \gamma_M t_{ct}^M \times \mathrm{Hi}_c + X_{ct}'\delta + \phi_i + \lambda_t + \varepsilon_{it} \tag{2}$$

The baseline regresses the outcome on the mayor's tenure and its interaction with predetermined transfer dependence, with firm and year fixed effects, mayor controls, and the party secretary's tenure clock, its dependence interaction, and age as controls: both offices share every city-year, so one clock is not interpretable without holding the other fixed. Common city-year exposure makes the secretary a useful placebo below, but overlapping authority prevents the office comparison from serving as an exclusion restriction. The two fiscal regimes differ in baseline economic structure, as a predetermined moderator should be expected to (Figure A2, Appendix E); the design absorbs these level differences with firm fixed effects. The tenure process itself is nearly identical across regimes: completed spell lengths, the share of spells reaching year four, and annual turnover rates are indistinguishable, and mayors in dependent cities are appointed less than a year younger (Table A14), a descriptive comparability check rather than an identification claim.

### 3.4 A Baseline Fact: Misallocation Falls with Tenure Only in Self-Financing Cities

Majority state-owned firms operate at a mean log marginal revenue product of capital 0.49 log points below private firms: they hold 1.6 times the capital per worker and produce roughly a third as much output per unit of capital (Figure 2). This gap is the distortion whose political determination the paper studies, and the baseline asks what happens to it as a mayor's term unfolds.

With both offices' tenure clocks and characteristics controlled, dispersion in value added per worker falls by 0.0073 log points per mayoral tenure year in self-financing cities (s.e. 0.0025), with a $+0.0069$ dependence interaction (s.e. 0.0027): the gradient is a self-financing-city phenomenon and is absent where transfers pay. A tenure year is close to one within-firm standard deviation of the clock, and the decline is two and a half percent of a within-firm standard deviation of the outcome, seven percent over a delivered term. The TFPR and TFPR-squared outcomes show the same pattern (Table 2, Figure 3). The decline is reallocation rather than contraction: city-level output is unchanged and the firm-level fall in value added is a one-time shift, not a trend (Table A2). Robustness across seventeen alternative measures and the full fixed-effect ladder is collected in Appendix C. Against the worry that anything cyclical in city politics would put a gradient on any leader's clock, the party secretary's clock, controlled throughout, shows no erosion, and the mayor-minus-secretary difference-in-difference across fiscal regimes is $+0.013$ (s.e. 0.005; Table A12, Appendix D): the gradient belongs to the office whose budget is exposed to the cost of favouritism.

### 3.5 The Ownership Mechanism

If the fiscal reading is right, the tenure gradient must live on the ownership margin: dependence can discipline only a distortion the budget pays for, and the instruments with budget lines (subsidies, credit, taxes) transfer to state-linked firms. It does, on ownership measured before the mayor's arrival (Table 3). Wedges interacted with initial productivity show no gradient, and the firm-age and entry–exit checks are flat. The distortion that moves is the output wedge rather than the capital–labour ratio, consistent with capital being reallocated across ownership types rather than re-optimised within firms.

Table 3 estimates the mechanism as a fully saturated triple interaction. Each column interacts the mayor's clock with transfer dependence and with the firm's ownership share, carrying every lower-order term; the time-invariant terms are absorbed by the firm fixed effect.[6] Collective and legal-person capital are grouped with individual private capital:

[6]Ownership shares are measured in the firm's first observed year, before the incumbent mayor's arrival,

township, village and urban collectives in this period were run as private businesses, legal-person capital is the stake held by other enterprises, and firms in which either dominates behave like the rest of the non-state economy rather than like state firms (Table A9). Foreign, Hong Kong, Macau and Taiwan capital form the third dimension. Two coefficients are displayed per column, the fiscal interaction of the mayor's clock and the triple; the ownership-specific tenure terms and every other lower-order interaction are carried in the specification but not displayed.

The test is the triple: does the fiscal regime switch off the ownership-specific tenure gradient? On the private dimension it does, at $+0.021$ (s.e. 0.010): where transfers pay, the private-specific gradient that operates where the city pays is erased rather than attenuated. The state and foreign triples are opposite-signed and imprecise. The pattern is unchanged under sector $\times$ year fixed effects, and the six-category counterpart in Table A9 shows the same contrast group by group.

The price adjustment is accompanied by movement in capital itself. Estimating the same specification with the firm's capital stock on the left, each tenure year raises a fully private firm's capital by 0.025 log points where the city finances itself, and by 0.019 measured against the city's own manufacturing capital stock; in transfer-dependent cities the city-relative response is 0.003 (Table A10). The ownership-linked capital wedge narrows with mayoral tenure where cities bear their own fiscal consequences, and this is accompanied by capital growth among more private-intensive firms. The regime interaction is estimated less precisely on quantities than on prices, so the paper reads the capital response as corroboration of the price mechanism rather than as independent identification, and it does not establish a citywide shift in the ownership composition of capital.

The same wedge is visible at the city level, where entry, exit and restructuring enter the average: the state-minus-private gap closes at $+0.026$ per tenure year in self-financing cities (s.e. 0.008) and *re-widens* at $-0.016$ in dependent ones (s.e. 0.009), a triple difference of $-0.042$, consistent with favouritism being replenished through entry and restructuring as incumbents' advantage decays; Brandt et al. (2026) document exactly this entry instrument in the hands of local cadres, and the pattern corresponds to the upward drift in Figure 6, panel (b). The city-level gap, the composition-inclusive object, is the moment the model of Section 6 calibrates on. Three further checks discipline the reading: development moderators (coastal, initial GDP, promotion competition) leave the city-level dependence interaction unchanged ($+0.026$ against $+0.028$ with all three); inside mayor-spell fixed effects the differential keeps its sign at two thirds of its magnitude ($+0.017$), which mayor selection would not produce; and a one-standard-deviation stronger ex-ante promotion incentive steepens the erosion by $-0.084$ log points per year where the index is coded.

so restructuring during a mayor's own term cannot mechanically move a firm across the ownership margin.

Because favoured *private* firms sit among the controls, the ownership contrast is a lower bound on favouritism (Bai et al., 2020), though least so in this period (Bai et al., 2021).

These are not separate mechanisms but the same wedge read off the prices the budget can see. Among private firms, TFPR dispersion falls by 0.0066 log points per tenure year where the city pays and the +0.0065 dependence interaction offsets it almost exactly (Table A8); the fiscal interaction survives a joint race against development moderators (+0.0041). On the instrument side, direct subsidies to state firms decline by 0.0005 of value added per tenure year where the city pays and rise where transfers do; the state sector's borrowing cost is flat where the city pays and falls by a net −0.0010 per unit of capital where transfers do; and the privatisation hazard falls by 0.0053 per year where the city pays and rises by 0.0051 on net where transfers do, against a base rate of 0.159 (Table A8, panel B). Where favouritism is refunded, the state sector is subsidised, financed cheaply and sold down; where the mayor bears the cost, it is none of the three. Effective tax rates and the land-price and land-area gradients are flat, and these regressions document associations with candidate instruments rather than an exhaustive reconstruction of the policy wedge.

## 3.6 The Fiscal Dose

The moderator is a dose, not a label. Replacing the median split with the continuous transfer share, the tenure gradient declines continuously in dependence (interaction +0.031, s.e. 0.012) and crosses zero at a transfer share of 0.60 (s.e. 0.045; Figure 4). Beyond the crossing the point estimate turns positive (+0.0029 in the most dependent quintile, against −0.0090 in the least): where the bill is refunded, tenure if anything extends favouritism. The smooth decline is what a fiscal-incentive target predicts and what a binary institutional difference between city types would not.

## 3.7 Dynamics: The Term Profile and the Turnover Reset

The two results above say where the wedge moves; the dynamics say how fast, and they discipline the model's accumulation-of-control object $\mathcal{K}(a)$. Figure 5 reports event-time associations in the city-year state-minus-private gap around mayor transitions, with city and year fixed effects and the outgoing mayor's final year omitted. In self-financing cities the gap is 8–11 log points more negative in years zero through two; the coefficients decay from year three and are back within two log points of the pre-transition level by year five, completing the cycle just as the statutory term would end. Transfer-dependent cities show no comparable pattern. The magnitudes are consistent with a novice reset, but recurrent,

non-random transitions and possible contemporaneous policy changes preclude a causal turnover interpretation.

Mayors observed in a second city provide a selected exception: their first-year gap is 0.16 log points narrower and their subsequent gradient differs from first-time mayors', consistent with portable experience, but the comparison rests on 30 matched pairs. Section 6.3 therefore treats $\chi^* = 0.326$ as a conditional point within the identified set $[0.204, 0.373]$, and the counterfactuals are solved at both ends of the set. Nationally, the value-added-weighted gap narrowed from $-0.58$ to $-0.26$ over 2000–07, with a mechanical accounting assigning 0.29 log points to continuing-spell years and 0.05 to transitions; this is not a causal decomposition, since continuing years also load national SOE reform and composition change.

The declining profile is consistent with accumulation of control, but career incentives and selective retention remain alternative explanations. Table A11 probes them: appointment-age and expected-term splits show no promotion-horizon heterogeneity, while the spell-length split conditions on endogenous retention and is read accordingly. I therefore use the Weibull tenure profile as a disciplined calibration, not as a selection-free causal law of motion.

## 3.8 From Evidence to the Two Structural Margins

Two margins bound what the mechanism does and does not move, with details in Appendix C. Entry does not respond: private entrants arrive capital-starved (an MRPK premium of $+0.13$ over state entrants), but entry rates, entrant composition, and the entrants' capital disadvantage all show zero tenure gradient, so the evidence supports focusing on reallocation among existing firms, without establishing that a fixed firm set is innocuous in every counterfactual. Employment reallocates without contracting: state firms shed labour as the wedge closes ($-1.7$ percent per tenure year), city manufacturing employment and the wage bill are flat, and state firms' stability advantage does not erode: favouritism buys the composition of employment, not its level.

The evidence therefore carries one message: mayoral tenure reduces an ownership-linked capital wedge only where local governments face the fiscal consequences of favouritism, and turnover resets the adjustment. It supports modelling the political distortion as an ownership-linked capital wedge whose evolution depends jointly on mayoral tenure and fiscal incentives, and it disciplines two structural margins rather than a collection of separate mechanisms. Fiscal dependence determines the target wedge, $\chi^*(\phi)$: the direction in which the mayor wants to move the distortion. The tenure profile, turnover reset, and experienced-mayor comparison determine the accumulation of control,

$\mathcal{K}(a)$: how quickly the mayor can move toward that target. The model now maps the resulting wedge into provincial productivity and general equilibrium.

# 4 Model

The empirical evidence identifies a change in the state–private capital-allocation wedge. This section maps that wedge into provincial manufacturing efficiency and then into aggregate outcomes through trade, factor prices, housing, and migration. The economic block takes the favouritism parameter $\chi_n$ as given. Section 6 then closes the causal chain by separating the fiscal target $\chi^*(\phi)$ from tenure-dependent control $\mathcal{K}(a)$. The spatial structure follows Tombe and Zhu (2019) and Eaton and Kortum (2002).

## 4.1 Environment

There are $N$ domestic regions, indexed by $n$, and a rest-of-world (ROW) region; in the calibration the regions are China's 30 provinces. Each region produces agriculture, manufacturing, and services, $j \in \{ag, mf, sv\}$. Domestic migration choices span $2N$ states: agricultural and non-agricultural work in each region. ROW participates in goods trade but not in migration. A subscript $ni$ denotes a shipment from origin $i$ to destination $n$; $o$ and $d$ denote registration-origin and employment- destination states, respectively. Firms enter the model only through the ownership-wedge block: no free entry, variety, or agglomeration, so the counterfactuals price reallocation, not scale.[7]

## 4.2 Households, Housing, and Location Choice

Let $g \in \{ag, na\}$ index labor type. Agricultural labor supplies agriculture; manufacturing and services share the provincial non-agricultural labor market and therefore a common wage $w_{na,n}$. Conditional on working in state $d = (n, g)$, a worker consumes a Cobb–Douglas aggregate of three goods and housing:

$$U_{gn} = C_n^{\alpha} S_n^{1-\alpha}, \qquad C_n = \prod_j (C_n^j)^{\psi^j}, \qquad \alpha = 0.87, \quad \sum_j \psi^j = 1. \tag{3}$$

The corresponding goods price is $P_{C,n} = \prod_j (P_n^j)^{\psi^j}$ and the housing share is $1 - \alpha = 0.13$. Let $F_n$ denote production-factor income: labor earnings plus locally owned capital and

[7] Favouritism also operates through barriers to entry (Brandt et al., 2026), a margin captured here only in reduced form (Section 3.5); the counterfactuals price the capital-price instrument alone, one more reason the reported gains are a floor.

production-land income, the latter distributed equally across current residents. Housing rent is rebated locally, so total household income is $I_n = F_n/\alpha$ and goods demand is $\psi^j F_n$. If $f_{gn}$ is per-worker production-factor income (the type-specific wage plus the local nonlabor dividend), destination real income is

$$V_{gn} = \frac{f_{gn}/\alpha}{P_{C,n}^{\alpha} p_{H,n}^{1-\alpha}}. \tag{4}$$

The constant $1/\alpha$ cancels from every hat. Housing structures $\bar{S}_n^H$ are fixed, so the housing-rent hat equals the hat of $F_n$ rather than a mechanically imposed population hat.

## 4.3 Production and Trade

Sectoral output is a CES composite, with elasticity $\sigma$, of a continuum of varieties. A variety with productivity $\varphi$ uses labor, capital, production land, and intermediate goods in the nested Cobb–Douglas structure

$$y_n^j(\varphi) = \varphi(\Lambda_n^j)^{\nu^j} \left[(K_n^j)^{\zeta^j}(L_n^j)^{1-\zeta^j}\right]^{\beta^j} (H_n^j)^{\eta^j} \prod_k (M_n^{kj})^{\varsigma^{kj}}, \tag{5}$$

where $\beta^j + \eta^j = \nu^j$ and $\beta^j + \eta^j + \sum_k \varsigma^{kj} = 1$. Here $\nu^j$ is the value-added share of gross output, $\beta^j$ is the share of the capital–labor composite, $\eta^j$ is the production-land share, and $\zeta^j$ is only the capital share *inside* the capital–labor composite. In particular, $\zeta^j$ is neither the non-agricultural value-added share nor the exponent on the entire value-added bundle.

Let $g(ag) = ag$ and $g(mf) = g(sv) = na$. The unit cost of production is

$$c_n^j \propto (\Lambda_n^j)^{-\nu^j} (w_{g(j),n})^{\beta^j(1-\zeta^j)} (r_{K,n}^j)^{\beta^j\zeta^j} (r_{T,n}^j)^{\eta^j} \prod_k (P_n^k)^{\varsigma^{kj}}. \tag{6}$$

Domestic sectoral capital $\bar{K}_n^j$ and production land $\bar{H}_n^j$ are fixed locally in these short-run counterfactuals and earn separate rents $r_{K,n}^j$ and $r_{T,n}^j$. Agricultural and non-agricultural employment is endogenous.

Shipping a unit from $i$ to $n$ requires $\tau_{ni}^j$ units. With Fréchet productivity of shape $\theta$, Eaton–Kortum expenditure shares and price indices are

$$\pi_{ni}^j = \frac{T_i^j(\tau_{ni}^j c_i^j)^{-\theta}}{\sum_m T_m^j(\tau_{nm}^j c_m^j)^{-\theta}}, \tag{7}$$

$$P_n^j = \gamma \left[\sum_i T_i^j(\tau_{ni}^j c_i^j)^{-\theta}\right]^{-1/\theta}. \tag{8}$$

Revenues and expenditures are linked through the measured three-sector input–output network by the goods-market condition in Section A.3.

## 4.4 Observed Migration and Migration Costs

The baseline location object is the observed $60 \times 60$ matrix $M_{2000} = (m_{od,2000})$ from the 2000 population-census replication files of Tombe and Zhu (2019); $M_{2005}$ provides the corresponding later matrix. Both include stayers and are row stochastic. Within province, agricultural states precede non-agricultural states. Let $L_{d,2000}$ be observed destination employment. Registered-worker stocks $\bar{L}_o$ are recovered once from

$$L_{d,2000} = \sum_o m_{od,2000} \bar{L}_o. \tag{9}$$

All recovered stocks are positive and reconstruct observed employment exactly.

For a real-income hat $\widehat{V}_d$ and bilateral migration-cost hat $\widehat{\tau}_{od}$, the counterfactual choice shares are

$$m'_{od} = \frac{m_{od,2000}(\widehat{V}_d / \widehat{\tau}_{od})^\kappa}{\sum_q m_{oq,2000}(\widehat{V}_q / \widehat{\tau}_{oq})^\kappa}, \qquad \kappa = 1.5, \tag{10}$$

and destination employment is

$$L'_d = \sum_o m'_{od} \bar{L}_o. \tag{11}$$

The observed matrices and real income per worker also identify bilateral migration-cost changes, by inverting equation (10) origin by origin (Appendix C.1). The recovered 2000 costs behave like costs rather than residuals: with origin fixed effects, they rise with capital-to-capital distance with an elasticity of 0.92 (s.e. 0.05), carry a large premium at the provincial (hukou) boundary and a separate agricultural–non-agricultural switching premium, and these observables explain half of their variation, the validation practice of Bryan and Morten (2019). CF1, CF3, CF4, and the term-length experiment set $\widehat{\tau}_{od} = 1$; the recovered cost hats enter only the separate 2000–05 migration-cost experiment.

## 4.5 From Ownership Wedges to Regional Efficiency

Within region $n$'s manufacturing sector, firms differ in physical productivity $A_\iota$ and state capital share $s_\iota \in [0, 1]$, both observed. Firm $\iota$ rents capital at

$$\rho_\iota \;=\; \rho \, \exp\!\left(-\chi_n \, s_\iota\right), \qquad \chi_n \in \mathbb{R}, \tag{12}$$

so $\chi_n$ is the private-minus-state log MRPK gap (equivalently, the log capital-price advantage of a fully state-owned firm relative to a fully private one). The empirical support includes Qinghai's negative value; the calibration therefore treats $\chi_n$ as real-valued rather than clipping it. Cost minimisation under CES demand gives (Appendix A), up to industry–year constants,

$$\ln \text{MRPK}_\iota = -\chi_n s_\iota + \epsilon_\iota, \qquad \ln \text{TFPR}_\iota = -\zeta \chi_n s_\iota + u_\iota, \tag{13}$$

with $(\epsilon, u) \perp s$. The first equation identifies $\chi_n$ as the private-minus-state gap in mean log MRPK.

**Remark 1** (Ownership-based, not productivity-based, wedges)**.** *Tying the wedge to ownership alone is an evidence-based restriction: wedges interacted with firms'* initial productivity *show no tenure gradient in Section 3.5, while the fiscal regime switches the tenure gradient specifically on the ownership margin (Tables 3 and A9). Those moments do not support a specification $\tau^K(A_\iota)$ as the source of the reported tenure gradient.*

Aggregating under the log-normal approximation of Hsieh and Klenow (2009),

$$\ln \Lambda_n = -\frac{\sigma}{2} \text{Var}_n(\ln \text{TFPR}) = -\frac{\sigma}{2}\left[\zeta^2 \chi_n^2 \text{Var}_n(s) + \text{Var}_n(u)\right]. \tag{14}$$

**Proposition 1** (Aggregate cost of the ownership wedge)**.** *(i) $\ln \Lambda_n$ is quadratic in $\chi_n$, is maximized at zero, and falls with $|\chi_n|$; (ii) the loss scales with $\text{Var}_n(s)$: a uniformly state or uniformly private region loses nothing because a common wedge distorts no relative margin; (iii) $\text{Var}_n(u)$ is policy-invariant and drops out of all counterfactual changes.*

**The formula against the full firm distribution.** Equation (14) uses two moments of the firm distribution. Recomputing the ownership term exactly, firm by firm, removing the component $\zeta \chi_n (s_\iota - \bar{s}_n)$ of each input-price wedge and reallocating the composite input within the province at $\sigma = 3$, gives a value-added-weighted national loss of 0.99 percent against the parametric 0.77 and raises the CF4 welfare cell from 0.33 to 0.44; correcting the exact calculation for transitory measurement error by firm averaging raises them further, to 1.27 percent and 0.58. Error masks rather than creates the ownership gains. The province-level correspondence is noisier (correlation 0.24 uncorrected, 0.44 corrected), because the exact calculation is dominated by second-best interactions with residual wedges that the two-moment formula deliberately excludes; the parametric mapping is therefore retained as the baseline and read as conservative in the aggregate rather than province by province. The gap itself is not a size artifact: in the firm-level regression of log MRPK on the state

capital share, conditioning on log employment and the average wage removes about a tenth of the coefficient ($-0.87$ against $-0.97$, industry demeaned, 2002).

Goods, labor, capital and production land, housing, and the migration consistency that supplies employment close the model; Appendix A.3 states the five sets of conditions.

## 4.6 Equilibrium Given Policy

Given trade fundamentals, the observed migration matrix, registered-worker stocks, locally fixed capital, production land, and housing, and policies $\{\chi_n\}$, an equilibrium consists of goods prices and trade shares $\{P_n^j, \pi_{ni}^j\}$, the two domestic wages $\{w_{ag,n}, w_{na,n}\}$, sectoral capital and production-land rents $\{r_{K,n}^j, r_{T,n}^j\}$, housing rents $\{p_{H,n}\}$, bilateral migration shares and destination employment $\{m_{od}, L_d\}$, and sectoral revenues $\{R_n^j\}$ such that (i) households and location choice satisfy equations (3)–(4) and (10)–(11); (ii) production, pricing, and trade satisfy equations (6)–(8), with favouritism entering through the ownership-efficiency mapping of Section 4.5; and (iii) the goods, labor, capital, production-land, and housing markets clear, equations (A6)–(A11).

## 4.7 Solution in Changes

Counterfactuals are solved in relative changes, $\hat{x} = x'/x$, following Dekle et al. (2007); Appendix A states the full updating system. The algorithm alternates the hat analogues of the cost, price, and trade-share equations (6)–(8) with the market-clearing conditions of Section A.3, real incomes, and the migration system (10)–(11), with the employment-weighted geometric mean of the 60 domestic wage hats as numeraire, until all residuals fall below $10^{-8}$.

The reported manufacturing-TFP hat is the baseline-value-added-weighted geometric mean of provincial manufacturing-efficiency hats,

$$\widehat{TFP}_{mf} = \prod_{n=1}^{30} (\widehat{\Lambda}_n^{mf})^{\omega_n^{mf}}, \qquad \omega_n^{mf} = \frac{\nu^{mf} R_{n0}^{mf}}{\sum_i \nu^{mf} R_{i0}^{mf}}. \tag{15}$$

Production-side real GDP aggregates deflated revenue hats $\widehat{R}_n^j / \widehat{c}_n^j$ with the analogous baseline value-added weights across all domestic province–sector cells.

For migration-active welfare, the origin-specific inclusive-value hat is

$$\widehat{W}_o = \left[ \sum_d m_{od,2000} (\widehat{V}_d / \widehat{\tau}_{od})^{\kappa} \right]^{1/\kappa}, \qquad \widehat{W} = \prod_o \widehat{W}_o^{\bar{L}_o / \sum_q \bar{L}_q}. \tag{16}$$

The migration-off column instead holds $m'_{od} = m_{od,2000}$ and aggregates destination real-income hats using baseline flows. It is reported only as a channel decomposition using the identical production and trade block. Both measures exclude the mayor's political payoff, public goods, and government budgets; they are conditional consumption-equivalent accounting objects, not complete social welfare.

For an experiment moving favouritism from $\chi_n$ to $\chi'_n$,

$$\ln \hat{\Lambda}_n^{mf} = -\frac{\sigma}{2}(\zeta^{mf})^2 \mathrm{Var}_n(s) \left[(\chi'_n)^2 - \chi_n^2\right]. \tag{17}$$

# 5 Calibration

This section calibrates the economic block that prices a change in favouritism. The main text records the inputs needed to interpret the counterfactuals; Table 4 summarizes the full set of production, trade, migration, and political parameters. Section 6 then uses the empirical tenure and fiscal patterns to determine which wedge changes are politically attainable.

## 5.1 Favouritism and Ownership Dispersion

$\chi_n$ is the private-minus-state gap in mean log MRPK, computed from the firm microdata and aggregated to provinces with value-added weights; $\mathrm{Var}_n(s)$ is computed directly. The pooled gap is 0.49 log points. The dependence measure is based on 1995–99 city fiscal accounts; provinces are classified as self-financing or transfer-dependent at the median.

**Measurement.** Three standard objections to MRPK-based wedges are checked directly. Differential tax burdens do not masquerade as the wedge: recomputed on after-tax returns, the province wedges are unchanged (correlation 0.998, median ratio 1.00). Transitory measurement error (Bils et al., 2021) does not carry it either: a within-firm autocovariance decomposition attributes only 13 percent of MRPK dispersion to the transitory component that classical error can inhabit (33 percent is firm-permanent, 54 percent persistent with root 0.70), and the ownership gap is unchanged on firm-averaged data (0.40 against 0.38 annual). Nor is it adjustment-cost dynamics (David and Venkateswaran, 2019): the gap is a firm-permanent object, which a standing ownership policy, not lumpy investment, produces; and the identifying object is in any case the *within-firm cyclical movement* of the gap with the tenure clock, which none of these frictions generates. Cross-province capital reallocation (Bau and Matray, 2023) is outside the model: provincial capital stocks are fixed, and the counterfactuals price the within-province ownership margin only.

## 5.2 Trade, Production, and Migration Inputs

Intermediate shares $\varsigma^{jk}$, value-added shares $\nu^j$, and final- demand shares $\psi^j$ come from the 2002 interprovincial input–output table. Production-land shares $\eta^j$ follow Tombe and Zhu (2019); hence $\beta^j = \nu^j - \eta^j$. The measured values are $(\nu^{ag}, \nu^{mf}, \nu^{sv}) = (0.59, 0.27, 0.48)$, $(\beta^{ag}, \beta^{mf}, \beta^{sv}) = (0.31, 0.24, 0.38)$, and $(\eta^{ag}, \eta^{mf}, \eta^{sv}) = (0.28, 0.03, 0.10)$. Household final-use shares are $(0.13, 0.31, 0.56)$. Bilateral trade matrices cover all 30 domestic provinces plus ROW in agriculture, manufacturing, and services; national expenditure-weighted home shares are 0.86, 0.62, and 0.91.

The migration block uses the 2000 and 2005 census matrices distributed with Tombe and Zhu (2019), with observed migrant shares of 0.174 and 0.257. The matrices and observed income identify the bilateral cost hats, and registered stocks come from equation (9); the migration-fit moments in Table 5 are therefore calibration reproductions, not independent checks.

## 5.3 Structural Parameters

The trade elasticity is $\theta = 4$. The capital share within each capital– labor composite is $\zeta^j = 0.5$; in manufacturing this implies gross-output shares 0.119 labor, 0.119 capital, 0.030 production land, and 0.732 intermediates. The firm-level substitution elasticity is $\sigma = 3$ and the migration elasticity is $\kappa = 1.5$. Migration sensitivity uses exactly $\{1.0, 1.5, 2.0, 3.0\}$; housing's expenditure share is 0.13.

**Internal estimates of the borrowed elasticities.** The headline gains scale with $\sigma\zeta^2$, and each borrowed value has an internal counterpart in the paper's own data. Measured from 2002 ASIF factor payments under the build's convention (industry capital share $1 - 1.5\times$ the median wage-bill share of value added, value-added weighted), the composite capital share is 0.51, indistinguishable from the calibrated 0.5. The value-added-weighted median markup, 1.26, implies $\sigma = 4.9$; carrying both internal estimates through the loss formula would scale the headline gains up by a factor of about 1.7, so $\sigma = 3$ is the conservative choice and is retained. The migration elasticity is estimated by gravity on the same censuses: the log odds of migrating on destination income, with origin-period fixed effects, distance and boundary controls, and a neighbor-income instrument, give $\kappa = 1.25$ (s.e. 0.13), inside the $[1.19, 1.61]$ range of Tombe and Zhu (2019) and with the baseline 1.5 inside the confidence interval; the untargeted 2005 migrant share does not identify $\kappa$ (it moves 1.1 points over $\kappa \in [1.0, 2.5]$), and headline welfare is flat in $\kappa$ (Table A4). The baseline is retained as an estimate-consistent value.

# 6 Endogenous Favouritism

The model so far prices $\chi_n$ without explaining its political evolution. Favouritism here is the wedge itself: the implicit capital-price advantage the city government extends to state-linked firms through the instruments of Section 2; endogenous means the mayor chooses how much of it to maintain. This section adds the two structural margins identified in the data. Fiscal incentives determine the target wedge, $\chi^*(\phi)$: what the mayor would choose with full administrative control. Tenure determines realised movement toward that target through $\mathcal{K}(a)$. Ownership and fiscal dependence discipline the target; the within-term profile, turnover reset, and experienced mayors discipline the accumulation of control.

## 6.1 Fiscal Incentives Set the Target Wedge

The mayor values the budget he commands and a political return to supporting the state sector: patronage, employment stability, the goodwill of superiors, or a share of favoured firms' rents, the special-deal margin documented by Bai et al. (2020). With $\phi_n \in (0,1)$ the own-revenue share of the budget, $t$ the effective local tax rate, $Y_n(\chi) = \bar{Y}_n \Lambda_n(\chi)$ local value added, and $C_n(\chi)$ the implicit capital-subsidy bill of favouritism (measured below), of which the budget ultimately bears a share,

$$U_n(\chi) = \phi_n \big[ t Y_n(\chi) - C_n(\chi) \big] + (1 - \phi_n) G(\chi) : \tag{18}$$

the mayor internalises the share $\phi_n$ of both the tax base and the bill, which is the formal statement of "whose budget is exposed to favouritism." This objective is used consistently in the closed form below, the concave-benefit calibration of Section 6.3, and the derivations in Appendix A. Fiscal dependence is a predetermined moderator of the marginal local budget return; equation (18) is a calibration device, not a claim that $\phi_n$ is experimentally assigned.

The wedge has a fiscal price. Priced as the capital subsidy the model says it is, favouritism costs $C_n(\chi) = \sum_\iota \rho_n K_\iota \big(1 - e^{-\chi_n s_\iota}\big)$ per year: in 2002 this implicit bill has a median of 6.9 percent of manufacturing value added across self-financing provinces (6.0 among transfer-dependent ones), roughly twice a province's own current budgetary revenue (Table A3). Two facts about the bill discipline the objective. First, the on-book instruments (direct subsidies, the interest-rate advantage, the tax advantage) cover typically less than a tenth of it: the wedge is carried off the income statement, through directed credit and implicit guarantees. The observable levers in Table A8 move accordingly. They move the right way and by economically meaningful fractions of their own base rates: state firms receive subsidies worth 2.8 percent of value added against 1.2 percent for private

firms, and the state-firm subsidy rate falls with mayoral tenure in self-financing cities by about two percent of that base per year. But they are far too small to carry the aggregate. Subsidies do distort: a firm's own subsidy rate lowers its marginal revenue product of capital ($-1.05$, s.e. 0.04, within firm), and across cities the state-minus-private subsidy gap tracks the ownership wedge ($+1.18$, s.e. 0.44). Yet multiplying the tenure gradient in subsidies by that distortion accounts for only about a fiftieth of the measured within-term closing of the gap. The dynamic arithmetic and the static accounting agree: the instrument that moves the wedge is overwhelmingly off the books. Second, the efficiency cost is fiscally invisible to the mayor: at the calibration point the output term $t\,Y_n'(\chi)$ is half of one percent of the marginal subsidy bill $C_n'(\chi)$. What varies with fiscal dependence is therefore not the incentive to fix misallocation but whose budget is exposed to the cost of favouritism: a self-financing mayor bears it in his own budget, while the equalisation system refunds it to a transfer-dependent one. The dose–response in Figure 4 is this statement in the microdata: the erosion rate declines continuously in the transfer share and vanishes where the refund is largest, crossing zero at a transfer share of 0.60 (s.e. 0.045). In the independent design of Brandt et al. (2026), richer own fiscal revenue likewise predicts lower barriers to private entry: the entry-margin analog of the same fiscal discipline.

The closed-form expression used for the political mapping requires additional conditions. If $G(\chi) = g\chi$ is linear, the local approximation $\Lambda_n(\chi) \simeq 1$ is valid around the calibration point, the marginal subsidy bill is locally constant at $\bar{c}_n \equiv C_n'(\bar{\chi}_n)$, and the problem has a unique interior solution satisfying the second-order condition, then

$$\chi^*(\phi_n) = \frac{\left[(1-\phi_n)g/\phi_n - \bar{c}_n\right]_+}{t\bar{Y}_n\sigma\zeta^2\mathrm{Var}_n(s)}, \qquad \frac{\partial\chi^*}{\partial\phi_n} < 0 \text{ on the interior region.} \tag{19}$$

The derivative is a local comparative static under these restrictions. No global monotonicity result is asserted for an unrestricted $G$ or away from the local $\Lambda \simeq 1$ approximation.

**Proposition 2** (Preferred favouritism under the calibrated approximation)**.** *Under linear $G$, local $\Lambda \simeq 1$, a locally constant marginal subsidy bill, existence of a unique interior solution, and the second-order condition, equation* (19) *holds and preferred favouritism is locally decreasing in $\phi_n$, holding the remaining calibration objects fixed.*

## 6.2 Tenure Determines Adjustment toward the Target

The control technology depends on tenure $a$ and observed prior-office type $e \in \{nov, exp\}$. For novice mayors, the headline specification is the minimum-distance Weibull

$$\mathcal{K}(a, nov) = 1 - \exp[-(a/s)^p], \qquad (s, p) = (3.68, 9.09). \tag{20}$$

The exponential $1 - e^{-\lambda a}$ is retained only as an extrapolation and functional-form sensitivity. For an experienced mayor observed in a second city, the maintained type mapping sets $\mathcal{K}(a, exp) \approx 1$ from the first observed year. Realised favouritism is

$$\boxed{\chi_{nt} = [1 - \mathcal{K}(a_{nt}, e_n)]\bar{\chi}_n + \mathcal{K}(a_{nt}, e_n)\chi^*(\phi_n).} \tag{21}$$

The technology has an institutional reading: favouritism runs through individually negotiated, relationship-specific deals (Bai et al., 2020), so an incoming mayor inherits neither the relationships nor the apparatus.

The type mapping is disciplined by associations, not random assignment. In the transition event study, the average private-minus-state gap moves back toward its default around novice successions. Experienced mayors have a smaller first-year gap and no subsequent gradient, but the experienced sample is selected and contains only 30 matched pairs. The maintained $\mathcal{K}(a, exp) \approx 1$ interpretation is therefore conditional rather than a direct revelation of type or preferred policy.

**Definition 1** (Block-recursive political accounting)**.** *Given tenure $a_{nt}$, observed type $e_n$, and fiscal moderator $\phi_n$, equation (21) maps these objects into $\chi_{nt}$, which then enters the competitive accounting equilibrium. Local governments do not choose wedges while internalising trade, competitors' responses, transfers, or a government budget constraint; this is not a Nash policy equilibrium of the type estimated by Lu et al. (2024).*

**Proposition 3** (The novice tenure profile)**.** *Conditional on novice type and equation (21),*

$$\frac{\partial \chi_{nt}}{\partial a_{nt}} = \mathcal{K}_a(a, nov)[\chi^*(\phi_n) - \bar{\chi}_n], \qquad \mathcal{K}_a(a, nov) > 0. \tag{22}$$

*Within this type, tenure is sufficient for the modelled control share conditional on $(\bar{\chi}_n, \chi^*(\phi_n), s, p)$. Neither sufficiency nor the implied monotonic path is asserted after pooling novice and experienced types or conditioning on endogenous spell survival.*

## 6.3 Calibration of the Political Block

Table 6 reports the city-year private-minus-state MRPK gap on mayor tenure and its interaction with predetermined transfer dependence, with city and year fixed effects. These coefficients discipline the within-term profile but are interpreted as conditional associations. The novice headline uses the Weibull profile in equation (20); the exponential profile is an extrapolation sensitivity rather than the baseline law of motion. The default $\bar{\chi}_n$ is the first-year private-minus-state gap. Figure 7 plots the calibrated policy rule and the

within-term paths it implies: at the delivered 2.8-year term the Weibull path has captured almost none of the attainable erosion.

The preferred favouritism $\chi^*_{self}$ is set-identified, and two independent moments locate it. The tenure gradient identifies only the product $\lambda(\bar{\chi} - \chi^*)$, so the profile alone bounds $\chi^*_{self}$ between the exponential extrapolation and the Weibull saturation value (0.373). The experienced-mayor coefficient supplies a second, level-based estimate under the maintained $\mathcal{K}(a, exp) \approx 1$ mapping: $\chi^*_{self} = 0.326$ with a 95 percent interval of $[0.204, 0.448]$, within one standard error of the Weibull value obtained from the unrelated shape moment. Intersecting the two yields the working set $[0.204, 0.373]$ with the point 0.326. The estimate is conditional on selection into a second appointment and rests on 30 matched pairs, which is why the set rather than the point is propagated: the quantitative solver reports CF1 at both ends of the set alongside the point. The transfer–own-revenue elasticity, 0.299 (0.020), is reserved as an untargeted fiscal-panel check rather than used to fit the tenure profile.

The two anchors also identify the political benefit. With $G(\chi) = g\,\chi^{1-\gamma}/(1-\gamma)$, the mayor's first-order condition must hold at $(\chi^*, \phi) = (0.326, 0.62)$ for the self-financing group and at $(0.35, 0.50)$ for the transfer-dependent group, whose flat profile makes the default the preferred point; the marginal subsidy bill $C'(\chi^*)$ is measured from the 2002 firm data. The two conditions give $\gamma = 5.4$, strongly diminishing political returns, and the policy rule $\chi^*(\phi)$ plotted in Figure 7. The calibration is exactly identified and conditional: it requires $\chi^*_{self} < \bar{\chi}_{dep} = 0.35$, which holds at the point and the lower end of the identified set but fails at the Weibull upper end (0.373), where no concave benefit rationalizes both anchors. One implication is free of $\gamma$ (Lemma 1, Appendix A): any concave $G$ through the anchors implies $\chi^*(1) = 0$, so full fiscal retention removes the preferred wedge entirely: the anchor used by CF3.

**Remark 2** (The bill need not be on-budget)**.** *The calibration does not require the measured bill to be a literal municipal cash outlay. Suppose the budget ultimately bears only a constant fraction $\alpha \in (0, 1]$ of $C_n(\chi)$, the remainder carried off-book by banks or implicit guarantees. Because $Y'_n(\chi) < 0$, the internalised marginal cost becomes $m^\alpha_n(\chi) = \alpha C'_n(\chi) + t|Y'_n(\chi)| \geq \alpha\,\underline{m} > 0$ for every $\alpha > 0$, so Lemma 1 and the CF3 corner $\chi^*(1) = 0$ survive under any positive fiscal exposure. Writing $m^\alpha_n = \lambda_\alpha m_n$ with $\lambda_\alpha = \alpha + (1-\alpha)\,t|Y'_n|/m_n$ common across the two calibration groups, $\lambda_\alpha$ cancels from the ratio that identifies $\gamma$ and from the first-order condition at every $\phi$ once $g$ is refit: $\gamma$, the policy rule $\chi^*(\phi)$, the retention ladder, and CF3 are unchanged, and only the level of $g$, which no result uses separately, rescales. With $t|Y'_n|$ about half a percent of $C'_n$, a group-specific $\alpha$ would move $\gamma$ by under 0.1. The invariance is a property of this concave-G block; the linear-G approximation of Proposition 2 scales with $\alpha$ and is not used quantitatively. What $\alpha < 1$ does change is the descriptive pricing of the bill in Table A3, which is labelled an accounting magnitude for this reason.*

**Model implications.** The political block is fit on mayor–firm moments only. The opposite secretary gradient confirms the office contrast and reveals the incompleteness of the one-office abstraction; it is not a formal prediction of a model that contains a secretary's objective. Industrial land-price and land-area gradients are flat. Table A7 reports these failures and nulls rather than relabelling them as support.

# 7 Quantitative Analysis

## 7.1 Mapping the Two Margins into Counterfactuals

The counterfactuals compare the two structural margins directly. CF1 and CF2 change how much control tenure delivers. CF3 changes the target toward which that control is used. CF4 removes the measured wedge and provides an efficiency bound. Freezing $\chi$ nests the standard exogenous-wedge misallocation model: that model prices the baseline dispersion and CF4 identically, but CF1–CF3 are not defined in it, because their policy levers are the political objects themselves. $\mathcal{K}(a)$ is the control margin, the speed at which allocations move; $\chi^*(\phi)$ is the incentive margin, the target they move toward. The ranking below is arithmetic on those two margins. All headline counterfactuals jointly solve trade, factor prices, housing, and the 60-state migration system, holding policy migration costs at their 2000 level ($\widehat{\tau}_{od} = 1$). Every scenario is also solved with migration off using the same production and trade block; that column isolates the migration channel and is not a competing baseline. Wedge moments cover 26 of 30 provinces.[8] Aggregation to provinces is itself conservative: the mayor is a prefecture office, and computing the loss formula prefecture by prefecture and value-added aggregating gives a loss 1.9 times the province-level one, because within-province dispersion of prefecture wedges is large (median within-province standard deviation 0.52). Small prefecture cells inflate estimated dispersion, so the factor overstates, but the direction is unambiguous; together with the four uncovered provinces, the reported gains are a floor with respect to spatial aggregation. A prefecture-level equilibrium is not attempted: interregional trade data below the province level do not exist publicly, the same constraint Lu et al. (2024) face.

**CF1 (mayors never rotated).** Among covered, shock-eligible, self-financing provinces only, the wedge falls by the absolute improvement $\bar{\chi} - \chi^*$, subject to a zero lower bound; dependent and uncovered provinces receive no wedge shock. Because $\chi^*$ is set-identified, CF1 is solved at the point (0.326) and at both ends of the set (0.204, 0.373), and reported as a point with brackets. CF1 is a ceiling on the time margin rather than a policy proposal: the

[8] Beijing, Tianjin, Shanghai, and Hainan remain in every goods, factor, and migration market but receive a unit manufacturing-efficiency hat because complete wedge moments are unavailable.

model's only tenure channel is accumulated control, so permanent tenure can only help.[9]

**CF2 (delivering a five-year term).** Average control under observed completed spells is compared with the person-year average of $\mathcal{K}(a, nov)$ over ages one through five. This changes the wedge only in eligible self-financing provinces and does not replace the observed spell distribution with a deterministic mean spell. A *synchronized* variant prices the election-calendar version of the same reform: appointments cluster on the five-year party-congress calendar, so the natural fixed-term institution is a common cycle in which every eligible mayor serves years one through five together, with wedge $\chi_n - [\mathcal{K}(a) - \kappa_{obs}](\bar{\chi} - \chi^*)$ at common age $a$; five equilibria, one per cycle year, price the calendar.

**CF3 (full fiscal retention).** In all 26 covered provinces, $\chi'_n = (1 - \bar{\kappa})\chi_n = 0.59\chi_n$, where $\bar{\kappa} = 0.41$ is the average control a mayor commands over a term under the fitted Weibull accumulation technology. The experiment gives every mayor the self-financing incentive: under any concave political benefit through the calibration anchors, full retention sets the preferred wedge to zero, $\chi^*(1) = 0$ (Section 6.3); rotation continues as observed, so each term erodes the fraction $\bar{\kappa}$ of its inherited wedge. The transformation is applied algebraically, including Qinghai's negative measured wedge. The experiment changes the retention *incentive* only. Stated precisely, CF3 is a retention-indexed wedge-reduction scenario, not a reform of the transfer system: $\phi_n$ is an average own-revenue share rather than a marginal retention rate, and no government budget re-clears, a deliberate contrast with fiscal-space models in which transfer-financed spending clears in equilibrium (Fajgelbaum et al., 2019; Henkel et al., 2021). The public-goods value of transfers is held fixed by design rather than assumed away.[10]

**CF4 (production-efficiency bound).** The measured ownership wedge is set to zero in all 26 covered provinces. The result is an efficiency bound under the maintained pure-waste interpretation, not a complete evaluation of state-sector objectives.

**Policy experiments on the calibrated rule.** CF3 uses only the $\gamma$-free corner of the political calibration. Two further experiments use its interior and are reported in Figure 9. The *retention ladder* imposes a uniform floor $\bar{\phi}$ on every province's own-revenue share, $\phi'_n = \max(\phi_n, \bar{\phi})$: each mayor re-optimizes along the calibrated rule $\chi^*(\phi)$ of Figure 7, and each term cycle erodes the fraction $\bar{\kappa} = 0.41$ of the inherited wedge toward the new preferred point, $\chi'_n = \chi_n[(1 - \bar{\kappa}) + \bar{\kappa}\,\chi^*(\phi'_n)/\chi^*(\phi_n)]$. The transformation is the identity where the floor does not bind and reproduces CF3 exactly at $\bar{\phi} = 1$. The *time–incentive matrix* crosses the term reform with the retention reform. Under observed rotation, average

[9] The procurement-collusion costs of long tenure documented by Coviello and Gagliarducci (2017), and the entrenchment that the rotation system exists to prevent, would lower CF1, reinforcing the ranking below.

[10] Giving preferences a separable factor $\Gamma_n^\omega$ in transfer-financed spending multiplies every welfare hat by $\widehat{\Gamma}_n^\omega$; because all reported counterfactuals hold transfer levels fixed, $\widehat{\Gamma}_n = 1$ and the factor drops out for any $\omega$. A counterfactual that cut transfer *levels* would need a calibrated $\omega$, and is not run.

control per term cycle is the person-year value $\kappa_{obs} = 0.16$, computed on completed spells within the paper's 2000–2007 data period, rather than the five-year value 0.41, so full retention under observed rotation applies $\chi'_n = 0.84\chi_n$ in covered provinces; the remaining three cells are the baseline, CF2, and CF3. The cross-partial of the matrix prices the complementarity of the two margins.

**Invariance.** Every counterfactual holds fixed the trade and technology fundamentals, migration costs, ownership dispersion $\text{Var}_n(s)$, the default wedges $\bar{\chi}_n$, the control technology $\mathcal{K}$, and the political benefit $G$ (the invariance discipline of Redding, 2016). The strongest of these assumptions is the invariance of $G$ to the fiscal reform it prices: $G$ is calibrated from behavior under the current transfer system, and a reform that changed what superiors reward would move it. The exposure is limited at the full-retention corner, which the $\gamma$-free Lemma pins down from concavity alone; the interior of the retention ladder leans on the calibrated $\gamma$ and should be read with that caveat.

**Historical and migration-cost exercises.** The 2002–07 wedge history is reported for repeated cross sections and, as the preferred historical accounting, a stable endpoint cohort that fixes firm identity, province, two-digit industry, and 2002 ownership group while holding cohort ownership dispersion fixed. Endpoint wedge moments exist for all 30 provinces (including the 62,441-firm stable cohort), so the history uses a full-30 mask, distinct from the policy scenarios' 26-province mask. Migration costs stay at their 2000 level; recovered 2000–05 cost hats enter only a separate migration-cost experiment.

## 7.2 Fiscal Incentives Dominate Tenure

The counterfactuals stage a contest between the two institutions that ration reform. CF2 prices the feasible time reform: delivering the statutory term. CF1 prices the time margin's ceiling: permanent tenure, credited with full control and none of the entrenchment costs the rotation system exists to prevent. CF3 prices the incentive margin: the reason that equalisation removes. CF4 bounds what any institutional repair could deliver.

Table 5 and Figure 8 report the headline results; throughout, "welfare" is the consumption-equivalent change for initial residents with the migration block active. Never rotating mayors (CF1) raises manufacturing TFP by 0.24 percent (the identified set for $\chi^*$ spans $[0.17, 0.36]$) and welfare by 0.10 $[0.07, 0.15]$. Full fiscal retention (CF3) yields 0.46 and 0.21 at the cycle-average wedge (0.15 as a flow average along the term cycle, developed below), with its own parameter band: the term-cycle control $\bar{\kappa}$ is estimated at 0.41 (s.e. 0.09), and re-solving at $\bar{\kappa} \pm 1$ standard error moves the welfare cell across $[0.18, 0.25]$ (sampling intervals for every cell are in Table A6). The efficiency bound (CF4) gives 0.71 and 0.33;

delivering the statutory five-year term, 0.05 and 0.02. Four provinces without wedge moments receive no shock, so these are national outcomes for a shock covering 26 of 30 provinces. One ranking is strict: CF3 exceeds CF1 at *every* point of the identified set, roughly two to one at the point estimate, so the equalisation system's allocative cost, not the rotation system's, is the larger institutional price. Credited with permanent tenure and none of the entrenchment costs rotation exists to prevent, the time channel still delivers half of what retention does, and the two reforms work on different cities: rotation reform only where mayors already pay for favouritism, fiscal reform on what every mayor wants. In accounting terms, the mechanism is first order for its own objects: the turnover cycle moves a city's wedge by 32 to 44 percent of its within-city standard deviation each term, and the incentive margin explains 65 percent of the priced aggregate cost of favouritism (CF3 over CF4). What it does not explain are cross-city *levels* of the default wedge, which load on history rather than fiscal regime (a regime dummy explains under 3 percent of the between-city variance), and the secular trend, which the historical accounting below assigns mainly to privatisation.

The calibrated rule prices the interior between these poles (Figure 9), and two results follow. First, the incentive reform is back-loaded. Raising every province's own-revenue share to a floor of 0.85, which binds in 25 of the 26 covered provinces, delivers a welfare gain of 0.04 percent, less than a fifth of full retention's 0.21; a floor of 0.95 still delivers only 0.08. The strongly diminishing political returns that rationalize the two anchors ($\gamma = 5.4$) imply that mayors surrender most of the preferred wedge only once they internalize nearly the entire bill, so the allocative dividend of equalisation reform arrives mostly at the end of the ladder. Interior points inherit the calibrated rule, which the data find too steep between the anchors (Section 7.3); both endpoints are anchored. Second, the time and incentive margins are complements. Delivering the statutory five-year term is worth $+0.03$ percent of welfare under observed incentives (CF2) but $+0.12$ under full retention, over four times as much, because retention enlarges what a term's accumulated control is worth: the clock erodes toward a preferred wedge of zero in all 26 covered provinces instead of toward 0.326 in the self-financing ones. In the other direction, full retention is worth $+0.10$ under observed rotation and $+0.19$ under delivered five-year terms. The cross-partial, $+0.09$, is the quantitative content of the paper's opening contrast: the clock matters where, and only where, the mayor pays.

**The time path of the gains.** The counterfactual cells are steady-state comparisons, but the law of motion behind them implies its own transition. Under full retention each term starts at the inherited default and erodes along the fitted Weibull, so the wedge path repeats every cycle: the sawtooth is the steady state. Solved year by year, gains arrive

late (zero in year one, 0.09 in year three, 0.33 in the wedge-free year five, which coincides with CF4), and their cycle average is 0.15 percent against the published 0.21: gains are concave in control, so the cell that prices the cycle-average wedge overstates the flow average, which is the dynamic object. Two conclusions survive intact: the fiscal reform still dominates the time ceiling (0.15 against CF1's 0.10), and there is no long transition to wait through, since the cycle repeats from the first term and within-term discounting at five percent costs seven percent of the flow value. The exercise is a sequence of static equilibria, not a dynamic spatial equilibrium in the sense of Kleinman et al. (2023); the shortcut is bounded directly, since re-solving the deepest cycle year without any migration response moves its welfare cell by 0.02 basis points, and capital accumulation, not worker foresight, is the margin a genuinely dynamic extension would add.

**Synchronized terms.** The election-calendar variant makes the whole economy move through the term cycle together, producing a macro favouritism cycle: welfare sits below baseline in years one through three ($-0.02$ at age one, when the fresh synchronized cohort commands less control than today's incumbent mix), peaks at $+0.08$ in year five, and averages $+0.024$ against the staggered $+0.027$ ($+0.021$ discounted, since the peak arrives last). Gains are concave in control, so a common calendar trades away the phase-averaging that staggered rotation provides: synchronization is slightly worse than staggered delivery, and the calendar cannot rescue the time margin.

**Sensitivity to the accumulation technology.** The Weibull shape concentrates control in years four and five, and late-tenure cells carry survival selection, so the $2\times2$ is re-solved under the smooth gradient-implied exponential technology ($\lambda_e = 0.16$). Control then accrues early and the cells become $+0.01$, $+0.17$, and $+0.20$: the CF3 $>$ CF1 ranking, the sign of the complementarity, and its ratio (3.9 against 4.1) all survive; what the shape governs is the absolute size of the time margin, which shrinks further when control accrues early.

The migration channel is economically negligible for these experiments (Table A5): welfare moves by less than 0.03 basis points relative to migration off in every scenario, and varying the migration elasticity over $\{1.0, 1.5, 2.0, 3.0\}$ leaves headline welfare unchanged to two decimals. Trade reallocation is the spatial margin that matters, contributing about a tenth of the total gain (0.044 of 0.329 points under CF4). The reason is identifiable rather than assumed: location choice obeys an envelope property, and the reform moves cross-state real incomes by less than three-quarters of a percent, leaving migrants little to arbitrage. The same block delivers a 7.6 percent welfare gain when the measured 2000–05 migration-cost declines are fed in directly (Appendix C.1), so the ranking of channels

reflects this experiment, not a muted migration block.

The preferred stable-cohort 2002–07 history yields a 0.27 percent manufacturing-TFP gain and 0.11 percent of welfare; repeated cross sections imply 1.48 and 0.59, so the compositional change of the privatisation wave, outside the political mechanism, carries most of the decade's measured convergence. Feeding the measured 2002–07 drift in favouritism and ownership dispersion through the model likewise explains essentially none of the cross-province variation in manufacturing growth ($R^2 = 0.01$): the channel is a within-city allocative distortion, not a driver of regional growth differentials.

The incidence of the fiscal reform is local first and shared second. Gains concentrate where favouritism falls: province welfare tracks the own efficiency shock nearly one for one (correlation 0.98), from 0.61 percent in Guangxi to near zero in the unshocked municipalities (Figure A4, Appendix E). Two forces temper the concentration. Housing rents rise with local incomes and absorb roughly the housing share of the local gain, and cheaper manufactures spread through the trade network, handing even entirely unshocked provinces about 0.05 percent. Every province gains, 0.22 on average in self-financing and 0.17 in transfer-dependent provinces. On the allocative margin the reform has no regional losers; the distributional conflict it would face concerns the transfers themselves, whose redistributive function the model holds fixed.

Internal trade integration and the fiscal reform are complements. Between the 2002 and 2007 interprovincial tables the national manufacturing home share fell from 0.62 to 0.59 (from 0.86 to 0.77 in agriculture and 0.91 to 0.85 in services). Solving the same wedge counterfactuals on the 2007 economy, with identical 2002-measured shocks and the same migration block, raises the welfare gain of every scenario by 10 to 13 percent: CF3 rises from 0.21 to 0.24 percent and CF4 from 0.33 to 0.36. Five years of measured integration made favouritism about a tenth more costly. As goods markets integrate, the same local distortion destroys more national welfare, so the case for the fiscal reform appreciates over time.

## 7.3 Calibration Fit and Model Diagnostics

The political block is calibrated to the within-term tenure profile in self-financing cities, the experienced-mayor level, and the two fiscal-regime anchors; these moments discipline the accumulation of control $\mathcal{K}(a)$ and the target wedge $\chi^*(\phi)$. The turnover pattern around successions and the continuous fiscal gradient are not used to fit these objects and therefore provide independent diagnostics of the resulting law of motion. The trade and migration blocks are calibrated separately from the input–output tables and census matrices of Section 5. Figure 6 organises the model–data comparison by evidentiary status. Panel (a)

is the calibration fit itself, three parameters against four profile coefficients, and is reported as such; what it does establish is the timing, since data and fitted path alike put only 14 percent of the five-year closing before year three. The flat dependent-city prediction is partially targeted and is not drawn: it follows from the anchor choice that the flat profile of Figure 3 motivates, and the year-by-year path is not fitted.

The main validation claim comes from panel (b), the turnover reset, which enters no calibration objective. Because the average spell lasts 2.8 years while the event window spans nine, most city-years lie inside the windows of two or more different transitions at once, and the overlapping dummies attenuate every recurrent-design coefficient toward zero—by more in a simulated panel, which is balanced, than in the data. The comparison therefore keeps only city-years belonging to exactly one transition's window, the identical restriction on both series, and reports event years $-3$ through $+2$; beyond $+2$ fewer than twenty clean empirical cells exist in the 2000–2007 panel. A synthetic city panel generated from the calibrated block alone and run through this design reproduces the reset: at succession the empirical gap re-widens by 11.0 log points against 13.6 simulated, average depth over event years zero through two is 15.5 log points in the data against 15.8 in the model, neither series shows a significant pre-trend, and every simulated coefficient lies within one city-clustered standard error of its empirical counterpart. A second untargeted check is the continuous policy rule, calibrated to two group anchors only: the estimated dose slope is $+0.106$ (s.e. 0.051) against a linearized prediction of $+0.22$, the anchor-point gradients match closely ($-0.022$ estimated at the self-financing mean share against $-0.026$ predicted; $-0.009$ at the dependent mean against a predicted zero), and the estimated zero crossing, 0.58, lies above the predicted 0.50.

Two limits are equally explicit. The isolated-transition restriction means the exercise validates the depth and two-year persistence of the reset, not the full recovery: with 2.8-year spells, the 2000–2007 panel contains too few undisturbed post-succession runs to test the model's implied return to baseline near year five. And the model's dependent-city path is flat by construction while the data drift toward a wider gap, $+0.074$ by year five: the law of motion caps the dependent wedge at its default and cannot represent tenure extending favouritism where the bill is refunded. The direction of that miss suggests the maintained model may be conservative along this margin, but the exact quantitative implication is not identified by this exercise. The rule between the anchors is flatter in the data than the calibration implies, a caveat carried forward to the interior of the retention ladder in Section 7.2.

**Estimation against calibration.** Estimating the technology by simulated method of moments on the eight recurrent-design sawtooth coefficients pushes the amplitude to

the full default gap (0.48 against the profile fit's 0.11; J of 15.5 on 5 degrees of freedom)—largely an artifact of the overlapping-window attenuation those coefficients inherit, which shrinks the simulated profile more than the empirical one and is compensated by a larger amplitude—yet the policy-relevant functional barely moves: the implied term-cycle control is 0.44 against the calibrated 0.41, and CF3 re-solved at the SMM value gives welfare $+0.22$ against $+0.21$. The headline cell does not depend on which moment set disciplines the technology. A fit-to-changes check in the spirit of Caliendo and Parro (2015) bounds the trade block's scope: measured 2002–07 wedge changes move provincial manufacturing home shares by 4 percent of their mean absolute observed change; the decade's trade reallocation belongs to trade costs and productivity, which the integration experiment of Section C.2 prices separately.

The migration block reproduces the census: the inversion matches the 2000 migrant share (0.17) and the observed decline in migration costs, fits the bilateral 2000–05 share changes with $R^2 = 0.91$ (Figure A3; a calibration input), and implies an aggregate 2005 migrant share of 23.2 percent against 25.7 observed and a non-agricultural employment-change fit of $R^2 = 0.75$ (calibration implications). Two moments are genuinely untargeted. The transfer–own-revenue elasticity (0.299) is never used in fitting the tenure profile. And the party secretary, who shares every city-year but not the allocation instruments, shows no favouritism erosion, exactly the pattern a mechanism located in the mayor's office requires; the model's one-office design is a deliberate abstraction, not an omission the data rejects. City-level land-price gradients are flat and are reported as nulls (Table A7, Appendix E).

## 7.4 Uncertainty and Limitations

A 99-draw city-cluster bootstrap (resampling cities, recomputing the wedge moments, and re-solving the full equilibrium each draw) delivers tight percentile intervals around the headline counterfactuals (Table A6, Appendix E); the intervals capture sampling variation in the wedge moments, holding the migration and trade calibration fixed. Structural-parameter sensitivity rescales rather than reorders the conclusions: gains scale approximately linearly in the wedge-mapping term $\sigma\zeta^2$, are insensitive to the trade elasticity, and CF3 exceeds CF1 in every cell (Table A4, Appendix E).

Several limitations remain. The four uncovered wedge locations are held at unit efficiency rather than extrapolated. Capital, production land, and housing structures are fixed locally, and baseline accounting residuals remain fixed relative to the ROW nominal scale: the paper prices the within-province ownership margin of a fixed provincial capital stock, not cross-region capital reallocation (Bau and Matray, 2023). The political block

models the mayor only; the opposite secretary gradient requires a two-office extension. The fit evidence should also be graded honestly: the migration-block statistics are calibration reproductions, the political block's diagnostics compare the model with moments not used in calibration, and none is a causal model test in the sense of Adão et al. (2025), which would require an exogenous policy change whose general-equilibrium footprint the model must predict; the 2002 income-tax sharing reform is underpowered for that purpose in this window, and such a test is the natural next step. Finally, a true government-policy reform would require local budgets, taxes, transfers, public spending, and strategic cross-region policy responses. The exercises here identify conditional consequences of measured wedge paths.

# 8 Conclusion

China assigns its prefecture mayors five-year terms and delivers about three. This paper began with the question that gap raises: does the constant rotation of local officials leave capital misallocated because no mayor stays long enough to fix it? The data give a conditional answer. Where a city pays for its own government, the return gap between state-linked and private firms narrows year after year as the mayor's control over the allocation machinery accumulates, and each succession hands the allocation back to a newcomer who starts over. Where the budget arrives from above, the same years in office change almost nothing. The clock matters where, and only where, the mayor pays the bill.

The model translates this pattern into two objects: capacity, which time in office builds, and the target, which fiscal incentives set. Comparing reforms of the two margins gives the paper's central result. Making every term run its statutory five years is worth less than a tenth of a percent of manufacturing TFP. Abolishing rotation altogether, a deliberately generous ceiling, is worth a quarter of a percent. Confronting every city with the fiscal cost of its own favouritism is worth nearly half a percent, roughly twice the ceiling of the time margin. Time in office is cheap to grant and buys little on its own; the incentive is what is expensive, and what pays.

These magnitudes are conditional. Neither tenure nor fiscal dependence is randomly assigned, and the counterfactuals hold transfers and public spending fixed, so the exercise prices the allocation margin of fiscal incentives rather than a full reform of China's transfer system.

The lesson for institutional design follows. Longer terms give politicians the capacity to change what they inherit; they do not decide what politicians will do with it. If capital is misallocated because the officials who allocate it bear none of the cost, the remedy is not a longer clock but a budget that makes efficiency pay.

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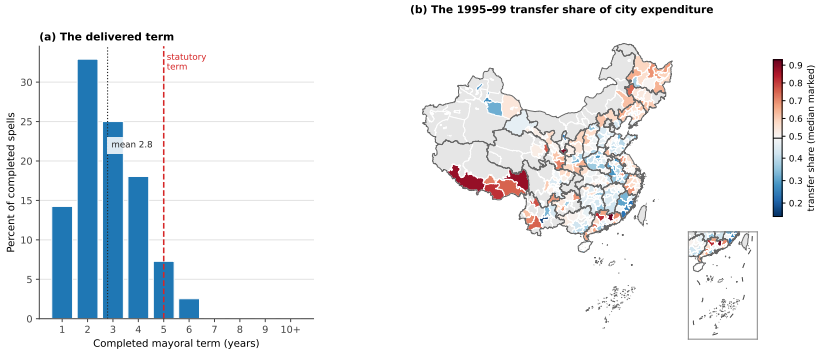
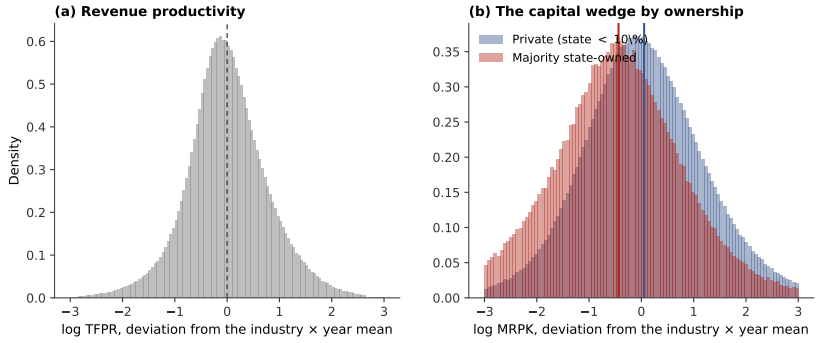
# Figures

Figure 1: The Two Rationed Ingredients: Delivered Terms and Fiscal Dependence



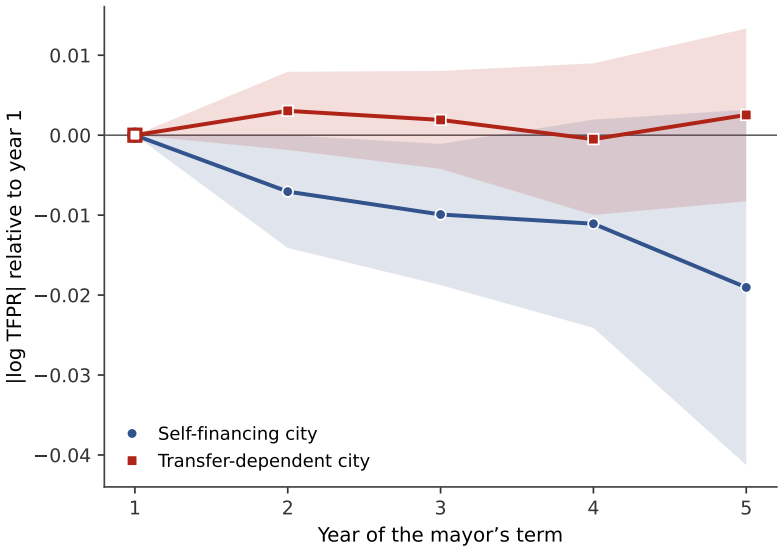

*Notes*: Panel (a) plots the distribution of completed mayoral terms over the 316 interior spells observed within the paper's 2000–2007 data period: spells observed from their first year and concluded within that window (the leader records themselves span 1995–2018, but only 2000–2007 matches the firm panel this paper analyzes). The mean is 2.8 years and the median 3.0; 10 percent of completed spells reach the statutory five-year term (dashed line). Panel (b) maps the moderator: each prefecture's 1995–99 share of budgetary expenditure financed by transfers rather than own current revenue, from the prefecture fiscal statistical volumes. Blue prefectures are self-financing (below the median share of 0.48, marked on the colour scale) and red prefectures transfer-dependent; grey areas are outside the estimation sample. The inset shows the South China Sea. Coastal prefectures largely finance themselves; the interior depends on transfers.

Figure 2: The Distribution of Revenue Productivity and the Capital Wedge



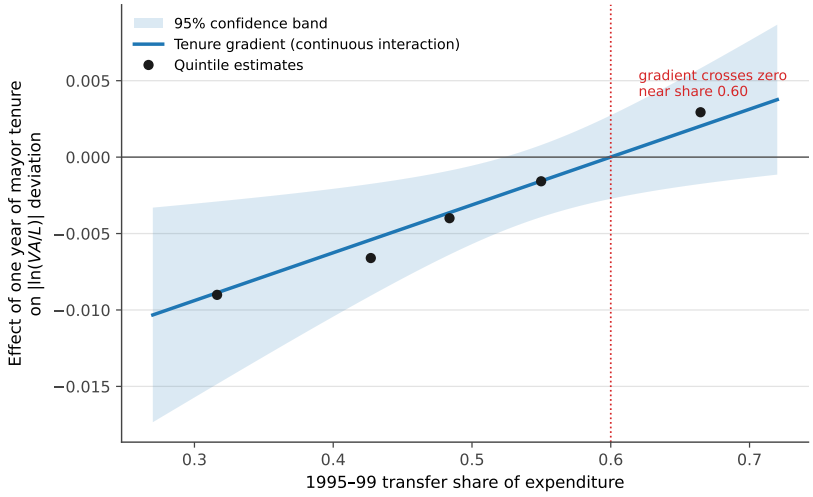

*Notes*: This figure describes the raw dispersion the paper studies, using all usable firm-years of the Annual Survey of Industrial Firms, 2000–2007. Panel (a) plots the density of log revenue productivity (ln TFPR, value added over factor shares) demeaned within industry × year; the pooled standard deviation is 0.766. Panel (b) plots the density of the log marginal revenue product of capital ($\ln \text{MRPK} = \ln VA/K$, demeaned within industry × year) separately for majority state-owned firms and for firms with a state capital share below 10%, with group means marked by vertical lines. State-linked firms sit 0.49 log points below private firms: they hold 1.6 times the capital per worker and produce roughly a third as much output per unit of capital, the capital-price advantage whose political determination the paper studies.

Figure 3: Misallocation over the Mayoral Term, by Fiscal Dependence



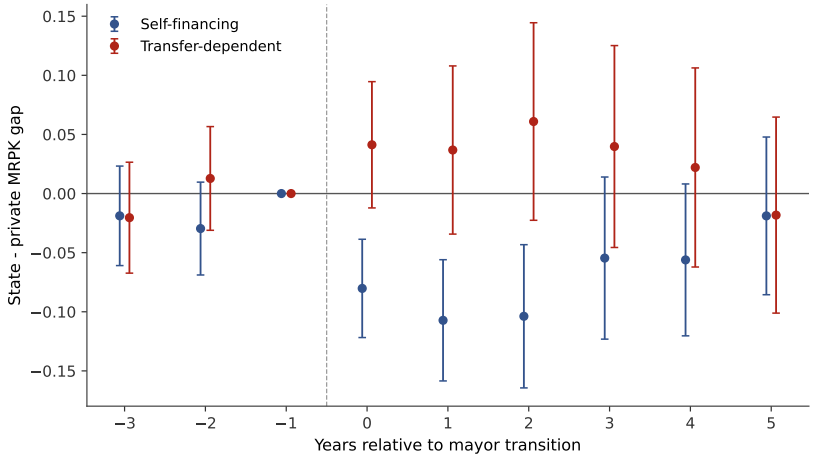

*Notes*: This figure shows the tenure profile of misallocation by fiscal regime. Each point is the coefficient on a mayor-tenure-year indicator (year one omitted) in a regression of the absolute deviation of log revenue productivity ($\ln$ TFPR demeaned within industry $\times$ year) on the indicators, estimated separately for self-financing and transfer-dependent cities on the Annual Survey of Industrial Firms, 2000–2007, with firm and year fixed effects and mayor age, age squared and education as controls. Standard errors are clustered at the city level; vertical bars show 95% confidence intervals. Dispersion falls monotonically across the term in self-financing cities and is flat in transfer-dependent ones.

Figure 4: The Tenure Gradient Declines Continuously in Transfer Dependence



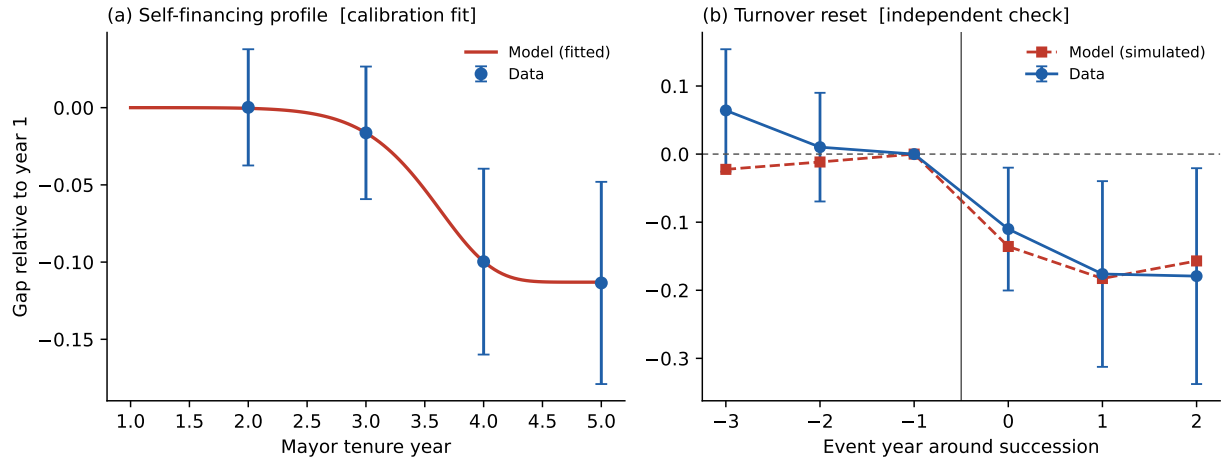

*Notes*: This figure replaces the binary fiscal split with the continuous moderator. The solid line is the mayor-tenure gradient of the baseline dispersion outcome (the absolute deviation of log value added per worker from its industry × year mean) implied by the continuous interaction of tenure with the centred 1995–99 transfer share of city expenditure (+0.031, s.e. 0.012), in the baseline specification: firm and year fixed effects and both offices' tenure and characteristic controls, standard errors clustered by city, $N = 1{,}045{,}078$ firm-years. The shaded region is the delta-method 95% confidence band; dots are the quintile point estimates from the same specification, shown as data anchors. The gradient declines continuously in dependence and crosses zero near a transfer share of 0.6 (vertical dotted line).

Figure 5: The Sawtooth: Turnover Resets What the Term Eroded



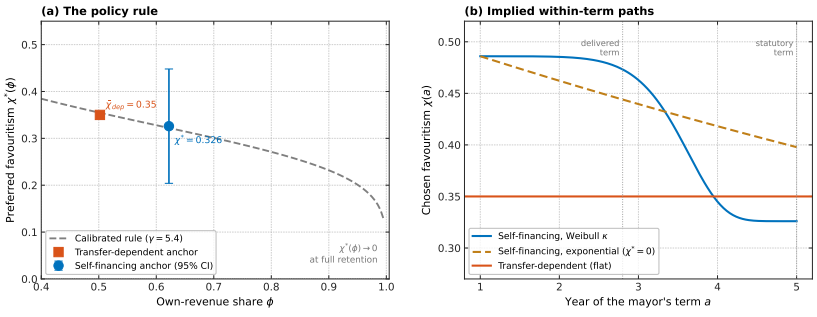

*Notes*: Event study of the turnover reset: the city-year state-minus-private mean log MRPK gap (industry × year demeaned; cells with at least 5 state and 10 private firms) on event-time dummies around mayor transitions, base $k = -1$, with city and year fixed effects; bars are 95% intervals clustered by city. A more negative gap is wider favouritism. These are event-time associations around recurrent, non-random turnovers.

Figure 6: The Law of Motion: Calibration Fit and Model Diagnostics



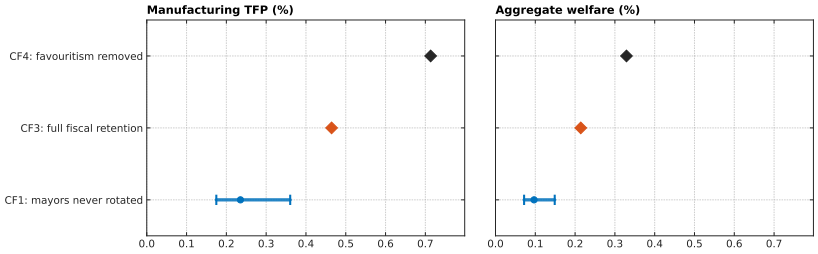

*Notes*: The outcome in panel (a) is the private-minus-state MRPK gap, so a decline is favouritism eroding; panel (b) uses the state-minus-private orientation of the turnover panel, so a more negative value is the gap re-widening at succession. The two panels have different evidentiary status. Panel (a) is a *calibration fit*, not a test of the model: the Weibull $(A, s, p)$ is minimum-distance fitted to exactly these four profile coefficients, so the near-exact fit is three parameters against four moments; the fitted amplitude 0.113 implies $\chi^* = 0.373$, the upper end of the identified set, while the counterfactuals use $\chi^* = 0.326$ from the independent experienced-mayor moment. Panel (b) is an *independent check*: the reset regression is not used in calibration, and the model line is the calibrated block (at the deployed $\chi^* = 0.326$) simulated through the paper's own event-study design. Both series keep only city-years inside a single transition's window (with 2.8-year average spells, overlapping windows otherwise attenuate the recurrent-design coefficients, the simulated more than the empirical) and are shown through event year +2, beyond which fewer than twenty clean empirical cells exist; bars are 95% intervals clustered by city, and the simulated profile is averaged over 40 replications.

Figure 7: The Policy Rule and Its Implied Within-Term Paths



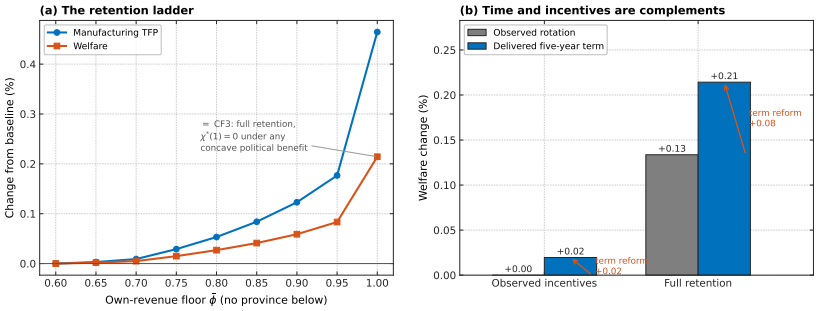

*Notes*: Panel (a): preferred favouritism $\chi^*$ against the own-revenue share $\phi$; the square and circle are the transfer-dependent (0.35 at $\phi = 0.50$) and self-financing (0.326 at $\phi = 0.62$, with its 95% interval) anchors, and the dashed curve is the rule implied by $G(\chi) = g\chi^{1-\gamma}/(1-\gamma)$ with $\gamma = 5.4$, reaching $\chi^* = 0$ at full retention (the CF3 anchor). Panel (b): the novice within-term path under the Weibull control technology ($s = 3.68$, $p = 9.09$), against the exponential family; vertical lines mark the delivered (2.8-year) and statutory terms.

Figure 8: Counterfactual Scenarios




*Notes*: The CF1 bar spans the identified set for $\chi^* \in [0.204, 0.373]$ with the circle at the point estimate ($\chi^* = 0.326$); diamonds mark CF3 (full fiscal retention, $\chi'_n = 0.59\chi_n$ in covered provinces) and CF4 (covered wedges set to zero), which are invariant to the set. The left panel reports manufacturing TFP, the right panel the welfare of initial residents, both in percent, from the three-sector spatial model with the migration block active. The four provinces without wedge moments receive no shock. The bar is parameter uncertainty, not sampling noise: it re-solves CF1 at every value of the partially identified $\chi^*$; CF3's analogous band, from the estimated accumulation technology ($\bar{\kappa} \pm 1$ s.e.), spans $[0.18, 0.25]$ percent of welfare and is reported in the text; sampling intervals for all scenarios come from the city-cluster bootstrap in Table A6, Appendix E.

Figure 9: Incentive-Based Policy Counterfactuals




*Notes*: Panel (a): welfare and manufacturing TFP gains from a uniform floor $\bar{\phi}$ on every province's own-revenue share; each mayor re-optimizes along the calibrated rule of Figure 7 and each term cycle erodes the fraction $\bar{\kappa} = 0.41$ of the inherited wedge toward the new preferred point. The $\bar{\phi} = 1$ endpoint coincides with CF3 (Lemma 1). Panel (b): welfare in the four cells crossing the term rule with the retention incentive; under observed rotation full retention applies $\chi'_n = 0.84\chi_n$ ($\kappa_{obs} = 0.16$). All equilibria use the 2002 baseline with trade and migration active.

# Tables

Table 1: Summary Statistics

| | Mean | SD | Median | N |
|---|---|---|---|---|
| *Panel A: Firm-year estimation panel (ASIF, 2000–2007)* | | | | |
| $\lvert \ln(VA/L) \rvert$ deviation | 0.618 | 0.484 | 0.504 | 1,045,078 |
| $\lvert \ln \text{TFPR} \rvert$ deviation | 0.567 | 0.478 | 0.442 | 1,056,916 |
| ln MRPK (industry × year demeaned) | -0.011 | 1.145 | -0.044 | 1,056,916 |
| State capital share | 0.084 | 0.266 | 0.000 | 1,056,916 |
| ln value added | 8.424 | 1.309 | 8.284 | 1,056,916 |
| ln capital stock | 8.493 | 1.649 | 8.384 | 1,056,916 |
| ln employment | 4.825 | 1.111 | 4.727 | 1,056,916 |
| *Panel B: City-year gap panel (turnover event study)* | | | | |
| Private − state MRPK gap | 0.352 | 0.413 | 0.383 | 1,820 |
| State firms per cell | 66.658 | 57.907 | 49.000 | 1,820 |
| Private firms per cell | 706.166 | 1153.638 | 315.000 | 1,820 |
| Transfer-dependent (share) | 0.460 | 0.499 | 0.000 | 1,820 |
| *Panel C: Leader panel, estimation cities (2000–2007)* | | | | |
| Mayor tenure (years) | 2.483 | 1.519 | 2.000 | 1,816 |
| Party secretary tenure (years) | 2.792 | 1.674 | 2.000 | 1,816 |
| Mayor age | 49.428 | 4.194 | 50.000 | 1,759 |
| Secretary age | 51.267 | 4.024 | 51.000 | 1,816 |
| Fiscal-dependence measure (1995–99) | -0.126 | 0.920 | -0.139 | 1,816 |

*Notes:* Panel A describes the firm-year estimation samples of the baseline regressions: Annual Survey of Industrial Firms manufacturing observations, 2000–2007, after the sample construction of Section 3.1 (positive value added, capital, and wage bill; industry × year demeaning; 0.5/99.5 percent trims) and the regression-sample restrictions (non-missing leader controls, singleton groups dropped), so each row's observation count equals the corresponding regression sample in Tables 2 and 3. Deviations are absolute deviations from industry × year means. Panel B describes the city-year ownership-gap panel of the turnover event study (Figure 5): cells require at least five state and ten private firms and a matched leader record. Panel C describes the prefecture leader panel over the outcome window for the cities in the estimation sample; tenure is counted in years in office and capped at five; the fiscal-dependence measure is the 1995–99 pre-period construction of Section 3.1.

Table 2: Misallocation and Leadership Tenure

| | (1) VA/worker | (2) Rev/worker | (3) MRPL | (4) TFPR | (5) $TFPR^2$ |
|---|---|---|---|---|---|
| *Panel A. Firm and year fixed effects* | | | | | |
| Mayor tenure | -0.0073*** | -0.0042* | -0.0052** | -0.0059** | -0.0110** |
| | (0.0025) | (0.0023) | (0.0026) | (0.0025) | (0.0050) |
| × Transfer-dependent | 0.0069** | 0.0030 | 0.0048* | 0.0058** | 0.0118** |
| | (0.0027) | (0.0031) | (0.0027) | (0.0027) | (0.0052) |
| Observations | 1,045,078 | 1,045,384 | 1,045,079 | 1,056,916 | 1,056,916 |
| *Panel B. Firm and sector × year fixed effects* | | | | | |
| Mayor tenure | -0.0070*** | -0.0038* | -0.0047* | -0.0056** | -0.0105** |
| | (0.0025) | (0.0023) | (0.0025) | (0.0025) | (0.0050) |
| × Transfer-dependent | 0.0067** | 0.0025 | 0.0046* | 0.0056** | 0.0114** |
| | (0.0027) | (0.0031) | (0.0027) | (0.0027) | (0.0052) |
| Observations | 1,045,076 | 1,045,382 | 1,045,076 | 1,056,914 | 1,056,914 |

*Notes:* This table reports the baseline effect of leadership tenure on misallocation. The outcome in each column is the absolute deviation of the indicated log productivity measure from its industry × year mean: column (1) value added per worker, column (2) revenue per worker, column (3) the marginal revenue product of labour, column (4) revenue productivity $\ln \text{TFPR} = \ln VA - \alpha_s \ln K - (1-\alpha_s) \ln W$, and column (5) its square; a negative coefficient means firms move toward the efficient allocation. The analysis uses the Annual Survey of Industrial Firms merged to prefecture leader records, 2000–2007. Mayor and party-secretary tenure are counted in years in office, capped at five; Transfer-dependent equals one for cities whose 1995–99 average share of expenditure financed by transfers rather than own current revenue lies above the median. All regressions control for the party secretary's tenure clock and age alongside the mayor's age, age squared, and education. Standard errors (in parentheses) are clustered at the city level. Panel A includes firm and year fixed effects; Panel B replaces year with sector × year fixed effects. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Mechanism: Ownership and the Capital Wedge

| | (1) State | (2) Private | (3) Foreign |
|---|---|---|---|
| *Panel A: firm and year fixed effects* | | | |
| Mayor tenure × Transfer-dependent | +0.0030 | −0.0117 | +0.0065 |
| | (0.0093) | (0.0084) | (0.0087) |
| Ownership share × Mayor tenure × Transfer-dependent | −0.0132 | +0.0206** | −0.0169 |
| | (0.0104) | (0.0098) | (0.0110) |
| Observations | 1,056,916 | 1,056,916 | 1,056,916 |
| R-squared | 0.754 | 0.754 | 0.754 |
| *Panel B: firm and sector × year fixed effects* | | | |
| Mayor tenure × Transfer-dependent | +0.0027 | −0.0132 | +0.0067 |
| | (0.0093) | (0.0081) | (0.0087) |
| Ownership share × Mayor tenure × Transfer-dependent | −0.0121 | +0.0223** | −0.0196* |
| | (0.0104) | (0.0095) | (0.0109) |
| Observations | 1,056,914 | 1,056,914 | 1,056,914 |
| R-squared | 0.758 | 0.758 | 0.758 |
| Controls | Yes | Yes | Yes |

*Notes:* This table locates the mechanism in ownership. The outcome is the log marginal revenue product of capital, $\ln \text{MRPK} = \ln VA/K$. Each column is one full-sample regression interacting the mayor's clock with transfer dependence and with the ownership share named in the column head, with every lower-order term; time-invariant terms are absorbed by the firm fixed effect. Shares are of paid-in capital in the firm's first observed year; private pools collective, individual and legal-person capital, foreign pools Hong Kong, Macau and Taiwan with foreign, and each pooling is checked category by category in Table A9. The secretary's clock enters with the same interaction set and is not displayed; the ownership-specific tenure terms and every other lower-order interaction are likewise included but not displayed. The coefficient of interest is the triple in the second row. Panels as in Table 2. Sample, tenure and dependence definitions, controls, inference, and significance stars as in Table 2.

Table 4: Key Parameters and Political Calibration

| Parameter | Value | Description | Source |
|---|---|---|---|
| *Economic block* | | | |
| $\theta$ | 4 | Trade elasticity | Tombe and Zhu (2019) |
| $\sigma$ | 3 | Elasticity of substitution across firms | Hsieh and Klenow (2009) |
| $\zeta$ | 0.50 | Capital share in the capital–labour composite | Data: ASIF cost shares |
| $\kappa$ | 1.5 | Migration elasticity | Tombe and Zhu (2019) |
| *Ownership wedge* | | | |
| $\chi_n$ | 0.49 (pooled) | Private–state MRPK gap | Data: ASIF |
| $\text{Var}_n(s)$ | 0.14 (pooled) | Dispersion of ownership shares | Data: ASIF |
| *Political block* | | | |
| $\bar{\chi}_n$ | (0.49, 0.35) self-fin./dep. | Default wedge | Data: ASIF × leader records |
| $(s, p)$ | (3.68, 9.09) | Control accumulation (Weibull scale, shape) | Estimated from tenure profile |
| $\chi^*_{self}$ | 0.326 | Target wedge; identified set $[0.204, 0.373]$ | Experienced-mayor moment |
| $\phi_n$ | (0.62, 0.50) self-fin./dep. | Own-revenue share of budget | Fiscal accounts, 1995–99 |

*Notes:* The table reports the parameters most important for the wedge-to-efficiency mapping and the political law of motion. Detailed input–output, trade, migration, and accounting inputs are reported in Table A1.

Table 5: Counterfactual Accounting Results

| Scenario | Manufacturing TFP (%) | Welfare (%) | Real GDP (%) |
|---|---|---|---|
| CF1: mayors never rotated | +0.24 [+0.17, +0.36] | +0.10 [+0.07, +0.15] | +0.09 [+0.07, +0.13] |
| CF2: delivering the statutory five-year term | +0.07 | +0.03 | +0.02 |
| CF3: full fiscal retention | +0.46 | +0.21 | +0.17 |
| CF4: efficiency bound | +0.71 | +0.33 | +0.27 |

*Notes:* All three sectors trade on measured 2002 bilateral matrices, with employment endogenous through the migration block (validated in Appendix E). The manufacturing efficiency shock enters gross unit cost at the measured value-added share $\nu^{mf} = 0.268$. CF1 lowers the wedge by $\bar{\chi} - \chi^*$ in covered, eligible, self-financing provinces only; because $\chi^*$ is set-identified, its row reports the point ($\chi^* = 0.326$) with brackets spanning the identified set $[0.204, 0.373]$. CF3 applies $\chi_n' = (1 - \bar{\kappa})\chi_n = 0.59\chi_n$ in every covered province, with $\bar{\kappa} = 0.41$ the Weibull average control over a term; CF4 sets covered wedges to zero. Beijing, Tianjin, Shanghai, and Hainan lack wedge moments and receive no shock. Welfare is the consumption-equivalent change for initial residents, with the migration block active and excludes political benefits, public goods, and government-budget changes.

Table 6: Estimation of the Political Block

| VARIABLES | (1) $\chi_{ct}$ | (2) $\chi_{ct}$ |
|---|---|---|
| Mayor tenure | -0.0242*** | -0.0274*** |
| | (0.0072) | (0.0077) |
| × Transfer-dependent | 0.0405*** | 0.0397*** |
| | (0.0107) | (0.0114) |
| Mayor controls | – | Yes |
| City FE | Yes | Yes |
| Year FE | Yes | Yes |
| Observations | 1,631 | 1,524 |
| Cities | 264 | 262 |
| R-squared | 0.716 | 0.736 |

*Notes:* Calibration regression of the political block, in the estimation-inside-calibration format of Lu et al. (2024). The dependent variable is the city-year favouritism wedge $\chi_{ct}$ (mean 0.353), which under the model's wedge specification is the favouritism parameter itself, so the mayor-tenure coefficient is $\partial\chi/\partial a$ in self-financing cities. Column (1): city and year fixed effects; column (2) adds mayor controls. Inference and stars as in Table 2.

The appendix is organized as follows. Appendix A contains derivations and the solution algorithm; Appendix B details data construction and measurement; Appendix C collects additional results and robustness checks; Appendix D reports the party-secretary results; and Appendix E collects all appendix figures and tables, figures first.

# A Derivations

## A.1 From the Firm Problem to Regional Efficiency

Within region $n$'s manufacturing sector, firm $\iota$ with physical productivity $A_\iota$ produces $y_\iota = A_\iota k_\iota^{\zeta} l_\iota^{1-\zeta}$, faces CES demand with elasticity $\sigma$, and rents capital at $\rho_\iota = \rho\, e^{-\chi_n s_\iota}$ and labor at the common wage $w$. Cost minimisation equates $\rho_\iota k_\iota/(w l_\iota)$ to $\zeta/(1-\zeta)$, so the marginal revenue product of capital is proportional to the firm's capital price,

$$\mathrm{MRPK}_\iota = \frac{\sigma-1}{\sigma}\,\zeta\,\frac{p_\iota y_\iota}{k_\iota} \;\propto\; \rho_\iota, \qquad \ln \mathrm{MRPK}_\iota = -\chi_n s_\iota + \mathrm{const} + \epsilon_\iota, \tag{A1}$$

with $\epsilon_\iota$ orthogonal to $s_\iota$. Revenue productivity is proportional to marginal cost, $\mathrm{TFPR}_\iota \propto \rho_\iota^{\zeta} w^{1-\zeta}$, hence

$$\ln \mathrm{TFPR}_\iota = -\zeta\chi_n s_\iota + u_\iota. \tag{A2}$$

Under the joint log-normality maintained by Hsieh and Klenow (2009), aggregating the CES composite gives, up to a policy-invariant constant,

$$\ln \Lambda_n = -\frac{\sigma}{2}\,\mathrm{Var}_n(\ln \mathrm{TFPR}) = -\frac{\sigma}{2}\Big[\zeta^2\chi_n^2\,\mathrm{Var}_n(s) + \mathrm{Var}_n(u)\Big], \tag{A3}$$

using $(\epsilon, u) \perp s$. Proposition 1 follows by inspection: $\ln\Lambda_n$ is a downward parabola in $\chi_n$ maximised at zero, the loss scales with $\mathrm{Var}_n(s)$, and the policy-invariant $\mathrm{Var}_n(u)$ cancels from every counterfactual hat, leaving equation (17).

## A.2 The Mayor's Problem

**Linear benefit.** With the objective of equation (18), $U_n(\chi) = \phi_n[tY_n(\chi) - C_n(\chi)] + (1-\phi_n)g\chi$, $Y_n(\chi) = \bar{Y}_n\Lambda_n(\chi)$, the local approximation $\Lambda_n \simeq 1$, and the marginal bill locally constant at $\bar{c}_n = C_n'(\bar{\chi}_n)$,

$$U_n'(\chi) = -\phi_n\big[t\bar{Y}_n\,\sigma\zeta^2\mathrm{Var}_n(s)\,\chi + \bar{c}_n\big] + (1-\phi_n)g, \qquad U_n''(\chi) = -\phi_n t\bar{Y}_n\,\sigma\zeta^2\mathrm{Var}_n(s) < 0,$$

so the optimum is unique, interior whenever $(1-\phi_n)g/\phi_n > \bar{c}_n$ and at the corner $\chi^* = 0$ otherwise; equation (19) follows, and $\partial\chi^*/\partial\phi_n < 0$ on the interior region from $(1-\phi_n)/\phi_n$

decreasing in $\phi_n$. This proves Proposition 2.

**Concave benefit.** The quantitative block uses $U_n(\chi) = \phi_n\big[tY_n(\chi) - C_n(\chi)\big] + (1-\phi_n)G(\chi)$, where $C_n(\chi) = \rho_n \sum_\iota K_\iota\big(1 - e^{-\chi s_\iota}\big)$ is the subsidy bill. Write $m_n(\chi) \equiv C_n'(\chi) - tY_n'(\chi)$ for the marginal internalised cost; on $[0, \bar{\chi}]$,

$$m_n(\chi) \;\geq\; \rho_n e^{-\bar{\chi}} \sum_\iota K_\iota s_\iota \;\equiv\; \underline{m} \;>\; 0.$$

An interior optimum satisfies

$$(1-\phi)\, G'(\chi^*) = \phi\, m_n(\chi^*). \tag{A4}$$

**Lemma 1** (Full retention removes the preferred wedge)**.** *Let $G$ be differentiable and concave on $(0,\infty)$ and rationalise interior optima at the two calibration anchors. Then $\chi^*(\phi) \to 0$ as $\phi \to 1$.*

*Proof.* By equation (A4), $G'(\chi^*(\phi)) = \frac{\phi}{1-\phi}\, m_n(\chi^*(\phi)) \geq \frac{\phi}{1-\phi}\, \underline{m} \to \infty$ as $\phi \to 1$. Concavity makes $G'$ nonincreasing, so $G'$ can diverge only as $\chi \to 0^+$. If instead $G'(0^+) < \infty$, then for $\phi$ close enough to one no interior solution exists and the optimum is the corner $\chi^* = 0$. In either case $\chi^*(\phi) \to 0$. □

**Exact identification of $(g, \gamma)$.** With $G(\chi) = g\,\chi^{1-\gamma}/(1-\gamma)$, imposing equation (A4) at the anchors $(\chi_s^*, \phi_s) = (0.326,\, 0.622)$ and $(\chi_d^*, \phi_d) = (0.350,\, 0.502)$ solves in closed form,

$$\gamma = \frac{\ln\!\Big[\dfrac{\phi_s\, m_s/(1-\phi_s)}{\phi_d\, m_d/(1-\phi_d)}\Big]}{\ln\!\big(\chi_d^*/\chi_s^*\big)}, \qquad g = \frac{\phi_s}{1-\phi_s}\, m_s\, \big(\chi_s^*\big)^{\gamma}, \tag{A5}$$

where $m_s$ and $m_d$ are the group marginal bills measured from the 2002 firm data. Concavity ($\gamma > 0$) requires $\chi_s^* < \chi_d^*$, which holds at the point and the lower end of the identified set and fails at the Weibull upper end, as reported in Section 6.3.

## A.3 Market Clearing Conditions

Five sets of conditions close the model: goods, labor, capital and production land, housing, and the migration consistency that supplies employment. Production-factor income in province $n$ collects the factor payments of its residents,

$$F_n = \sum_g w_{g,n} L_{(n,g)} + \sum_j \big(\beta^j \zeta^j + \eta^j\big) R_n^j, \tag{A6}$$

with locally owned capital and production-land income distributed equally across current residents (Section 4.2).

*Goods.* Sectoral revenue equals the trade-share-weighted sum of destination expenditures, and expenditure retains the measured three-sector input–output network:

$$R_i^j = \sum_n \pi_{ni}^j X_n^j, \qquad X_n^j = \psi^j F_n + \sum_k \varsigma^{jk} R_n^k + D_n^j. \tag{A7}$$

The calibration transfer is defined cell by cell as

$$D_n^j \equiv X_n^j - \psi^j F_n - \sum_k \varsigma^{jk} R_n^k. \tag{A8}$$

It reconciles the spatial trade table with factor income and the measured input–output shares; its all-sector national sum is zero, and it is held fixed (relative to the ROW numeraire) in counterfactuals. These residuals are quantitatively nontrivial (median absolute $D_n^j / X_n^j$ ratios are 0.23, 0.10, and 0.13 in agriculture, manufacturing, and services), so reported counterfactuals are conditional on this closure.

*Labor.* Agricultural labor clears against agricultural revenue, and the shared nonagricultural market clears against manufacturing and services jointly:

$$w_{ag,n} L_{(n,ag)} = \beta^{ag}(1-\zeta^{ag}) R_n^{ag}, \qquad w_{na,n} L_{(n,na)} = \sum_{j \in \{mf,sv\}} \beta^j (1-\zeta^j) R_n^j, \tag{A9}$$

with employment $L_{(n,g)}$ supplied by the migration system (11).

*Capital and production land.* Because $\bar{K}_n^j$ and $\bar{H}_n^j$ are fixed locally, their rents absorb sectoral revenue one for one:

$$r_{K,n}^j \bar{K}_n^j = \beta^j \zeta^j R_n^j, \qquad r_{T,n}^j \bar{H}_n^j = \eta^j R_n^j. \tag{A10}$$

*Housing.* Housing structures $\bar{S}_n^H$ are fixed and rents are rebated locally, so total income is $I_n = F_n/\alpha$ (Section 4.2) and the housing market clears at

$$p_{H,n} \bar{S}_n^H = (1-\alpha) F_n / \alpha. \tag{A11}$$

*Rest of the world.* ROW enters the goods condition (A7) as an origin and destination but not the migration system; it has exogenous common primary-factor price hats and perfectly elastic factor quantities, so its quantities absorb revenue changes.

## A.4 Equilibrium in Changes

Counterfactuals are solved in relative changes $\hat{x} = x'/x$ (Dekle et al., 2007). Because housing rent is rebated locally, total income satisfies $I_n = F_n + p_{H,n}\bar{S}_n^H$ with $p_{H,n}\bar{S}_n^H = (1-\alpha)I_n$, so $I_n = F_n/\alpha$ and goods demand is $\alpha\psi^j I_n = \psi^j F_n$; the constant $1/\alpha$ cancels from every hat. Given shocks $\{\hat{\Lambda}_n^{mf}, \hat{\tau}_{od}\}$, each iteration maps guesses of the factor-price hats into updated quantities:

1. *Costs, prices, and trade shares.*

$$\hat{c}_n^j = (\hat{\Lambda}_n^j)^{-\nu^j}(\hat{w}_{g(j),n})^{\beta^j(1-\zeta^j)}(\hat{r}_{K,n}^j)^{\beta^j\zeta^j}(\hat{r}_{T,n}^j)^{\eta^j}\prod_k(\hat{P}_n^k)^{\varsigma^{kj}}, \tag{A12}$$

$$\hat{P}_n^j = \Big[\sum_i \pi_{ni}^j(\hat{c}_i^j)^{-\theta}\Big]^{-1/\theta}, \qquad \pi_{ni}^{j\prime} = \pi_{ni}^j\left(\hat{c}_i^j/\hat{P}_n^j\right)^{-\theta}. \tag{A13}$$

2. *Goods markets.* Revenues solve $R_i^{j\prime} = \sum_n \pi_{ni}^{j\prime}X_n^{j\prime}$ with $X_n^{j\prime} = \psi^j F_n' + \sum_k \varsigma^{jk}R_n^{k\prime} + D_n^j$, the accounting transfers $D_n^j$ held fixed relative to the ROW numeraire.

3. *Factor payments.* With sectoral capital and production land fixed locally, $\hat{r}_{K,n}^j = \hat{r}_{T,n}^j = \hat{R}_n^j$; wage hats divide the type-relevant labor payments by endogenous employment, agricultural labor clearing at $w_{ag,n}$ and manufacturing and services jointly at $w_{na,n}$; ROW factor quantities are perfectly elastic at a common exogenous factor-price hat.

4. *Incomes, housing, and real incomes.* $F_n'$ collects labor earnings and local capital and land income; $\hat{p}_{H,n} = \hat{F}_n$ because structures are fixed; destination real-income hats follow from equation (4).

5. *Migration.* Choice shares and destination employment update by equations (10)–(11).

6. *Normalisation and convergence.* The employment-weighted geometric mean of the 60 domestic wage hats is the numeraire and also scales the ROW factor-price hat; all positive objects are damped, and when required a homotopy scales the shocks from zero to their requested values. Convergence requires goods, labor-payment, migration-share, destination-employment, and population residuals below $10^{-8}$.

Reported aggregates follow Section 4.7; production-side real GDP aggregates $\hat{R}_n^j/\hat{c}_n^j$ with baseline value-added weights $\omega_n^j = \nu^j R_{n0}^j/\sum_{i,k}\nu^k R_{i0}^k$.

# B Data and Measurement

## B.1 Firm Data

The firm data are the Annual Survey of Industrial Firms, 2000–2007: 1,755,478 firm-years and 540,298 firms after cleaning, manufacturing two-digit industries 13–43. Value added is main-business revenue minus the cost of sales net of the wage bill; capital is the stock of fixed assets; the wage bill is the annual payroll; observations require all three to be positive. The ownership share $s_\iota$ is state paid-in capital over total paid-in capital (state, collective, legal-person, individual, Hong Kong–Macao–Taiwan, and foreign components). The mechanism interactions of Table 3 fix ownership shares at the firm's first observed year, so restructuring during a mayor's own term cannot drive the interaction. Direct subsidies, interest expense, and income tax are the corresponding income-statement items, expressed relative to value added or the capital stock and bounded to rule out reporting errors.

## B.2 Productivity and Dispersion Outcomes

The industry capital share is $a_s = 1 - 1.5 \times \text{median}_s(W/VA)$, bounded to $[0.2, 0.8]$; TFPR is $\ln VA - a_s \ln K - (1 - a_s) \ln W$, demeaned within industry $\times$ year. Dispersion outcomes are absolute deviations (or squares) of demeaned objects, trimmed at the 0.5 and 99.5 percentiles; the headline outcome is the absolute deviation of log value added per worker.

## B.3 Wedges and the Gap Panels

The favouritism parameter $\chi_n$ is the value-added-weighted private-minus-state gap in mean log MRPK, aggregated to provinces; $\text{Var}_n(s)$ is computed directly. Two city-year gap panels appear in the paper: the calibration panel (private $s_\iota < 0.1$, state $s_\iota > 0.5$, at least ten firms each, private-minus-state orientation) and the turnover panel of Figure 5 (at least five state and ten private firms, state-minus-private orientation).

## B.4 Fiscal Dependence and Leaders

Transfer dependence is the 1995–99 average share of budgetary expenditure financed by transfers rather than own current revenue, from the prefecture fiscal statistical volumes; cities are classified at the median share of 0.48, and the continuous share enters Figures 1 and 4. The leader panel covers 1995–2018; tenure clocks are counted in years in office, capped at five in the regressions, and both offices' clocks, their dependence interactions, and leader ages (plus the mayor's education) enter as controls throughout.

# C Additional Results

## C.1 The 2000–05 Migration-Cost Experiment

Feeding the recovered 2000–05 migration-cost changes through the model, with no wedge shock, raises welfare by 7.6 percent. This is the model's valuation of the observed hukou liberalisation of the early 2000s, in the spirit of Tombe and Zhu (2019), and it is an order of magnitude larger than any of the political counterfactuals. It is reported here rather than with the headline results because it concerns a different policy margin: nothing in it is attributable to mayoral tenure or the fiscal system, and placing it beside effects of 0.1–0.3 percent invites a comparison the paper does not make.

The recovered costs invert the migration system: for year $t$, the inverse income-net cost shifter is

$$\xi_{od,t} = \left(\frac{m_{od,t}}{m_{oo,t}}\right)^{1/\kappa} \left(\frac{V_{d,t}}{V_{o,t}}\right)^{-1}, \tag{A14}$$

normalized bilaterally and year by year using the origin's stay share. The effective inverse cost additionally multiplies the origin fixed-factor rebate of the Tombe–Zhu replication; effective costs are its reciprocal.

## C.2 Access to the Rest of the World

The rest-of-world region stays in every goods and factor market, so each counterfactual prices the external margin jointly with internal reallocation. That margin barely responds. The largest change in any bilateral expenditure share is 0.52 percentage points under CF4 (0.34 under CF3, 0.18 under CF1); interprovincial manufacturing trade moves by less than 0.02 percent, and manufacturing exports by less than half a percent. Favouritism is a within-city allocative distortion, and removing it reallocates capital across firms rather than activity across borders: trade contributes to the gains through the internal terms of trade (Table A5), not through access to external demand. These are conditional implications of the measured rest-of-world home shares and fixed aggregate deficits, with perfectly elastic rest-of-world factors, not independent checks of the model.

## C.3 Robustness

The result survives seventeen alternative constructions of the misallocation measure, including a fully non-parametric rank measure and a within-firm moving average that addresses the measurement-error critique of Bils et al. (2021) (Figure A5), and is stable across the fixed-effect ladder from city-year to firm level (Figure A6).

### C.4 The Entry Margin

If favouritism also deterred private entry, the general-equilibrium numbers would be a lower bound. The entry margin shows a clear level fact and a clear null. The level fact: private entrants arrive capital-starved, with an MRPK premium of $+0.13$ (s.e. 0.06) over state entrants in the same industry and year. The null: nothing about entry responds to the mayor's tenure. Entry rates (mean 0.30 per year in this high-growth period), the private entry rate, the private share of entrants, and the entrants' capital disadvantage itself all show flat gradients in self-financing cities, plausibly because entry conditions are set by slower-moving institutions than a single mayoral term.

### C.5 What Favouritism Buys: Employment

At the firm level, state firms shed labour as the wedge closes (state $\times$ tenure on log employment is $-0.017$, s.e. 0.005, in self-financing cities), so favouritism was propping up state payrolls. But the city-year total is flat: log city manufacturing employment moves $+0.004$ (s.e. 0.007) per tenure year, and the city wage bill likewise, so the labour leaving state firms is reabsorbed by the expanding private sector within the same city, mirroring the value-added reallocation result. Nor is stability eroding: state firms are 4.6 percentage points less volatile in employment growth than private firms, and that advantage does not shrink over tenure ($-0.002$, s.e. 0.002). The caveat is that city-year nets say nothing about transition costs for the workers who switch employers.

### C.6 Transmission across Successions

Regressing the successor's first-year state-minus-private gap on the predecessor's last-year gap (within city) gives $\rho = 0.59$ (s.e. 0.06) in self-financing cities. This is not inheritance of reform: typed by successor, novice successions carry, if anything, a higher coefficient (0.67) than experienced ones (0.39, on 30 pairs), so the cross-sectional transmission mostly reflects serially correlated city-level defaults $\bar{\chi}_n$. The mean reset, the sawtooth of Figure 5, is the object the law of motion speaks to, and it is full-sized.

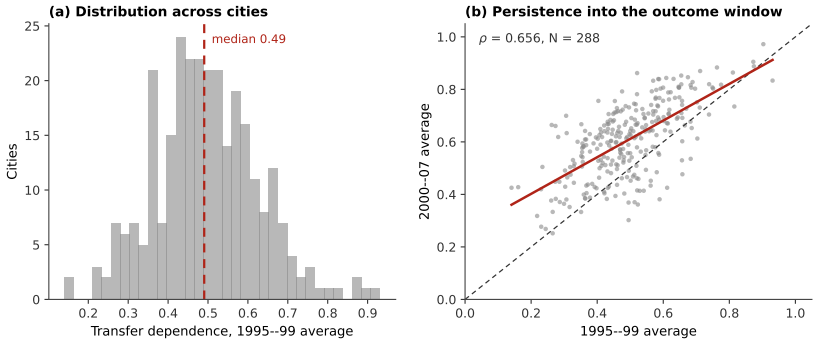
## D Party Secretary Results

The party secretary outranks the mayor and has a different formal portfolio, while retaining influence through the party hierarchy. This section collects the secretary results in one place. The opposite signs are what the incidence of favouritism suggests, because the two offices sit on opposite sides of its ledger. Favouritism charges its cost, on and off the

books, against the government side of the ledger: the mayor heads the government, so the fiscal exposure is his; the secretary heads the party, whose personnel system staffs the state firms that collect it. Control accumulated in the exposed office erodes the wedge; control accumulated in the collecting office, if anything, protects it. The ledger reading is an interpretation rather than a separately tested claim, and the estimates are not a formal prediction of a model with no secretary objective. Table A12 reports the joint-office contrast; Table A13 collects the secretary-only coefficients.

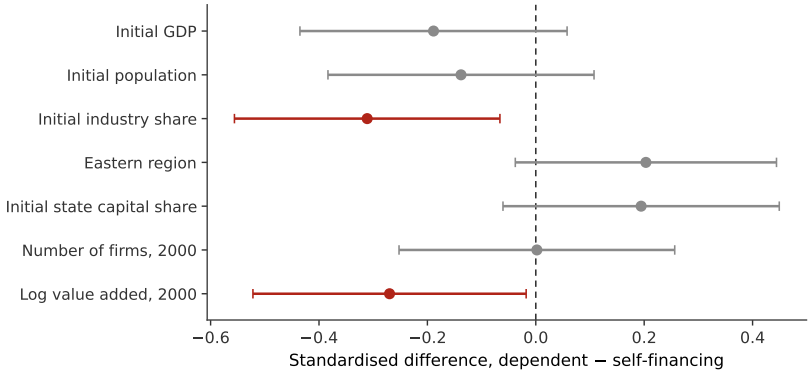
# E Additional Figures and Tables

Figure A1: Fiscal Dependence Across Cities and its Persistence




*Notes*: This figure describes the fiscal-dependence measure. Panel (a) plots the distribution across 288 prefectures of the 1995–99 average share of budgetary expenditure financed by transfers rather than own current revenue, from the prefecture fiscal statistical volumes; the dashed line marks the median at which cities are classified self-financing or transfer-dependent. Panel (b) plots the same 1995–99 measure against its realisation over the 2000–07 outcome window, with the 45° line and a fitted line; the correlation is 0.656, so pre-period dependence describes a persistent structural feature of a city rather than a transitory snapshot, and the classification predates every outcome year used in the paper.

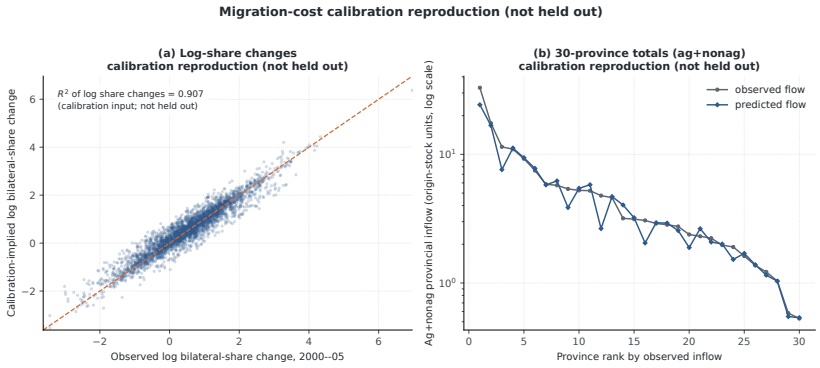
Figure A2: Baseline Characteristics by Fiscal Dependence




*Notes*: This figure describes how transfer-dependent and self-financing cities differ on observables measured at the start of the outcome window (2000). Bars show the standardised difference between the two groups with 95% intervals. Transfer-dependent and self-financing cities differ systematically in baseline economic structure: dependent cities are less industrial and smaller in manufacturing value added. These level differences are expected, since fiscal dependence is a predetermined structural feature of a city rather than a randomized treatment; the empirical design does not require balance across fiscal regimes, and time-invariant level differences are absorbed by the firm fixed effects used throughout. Mayor characteristics and tenure processes by regime are reported in Table A14.

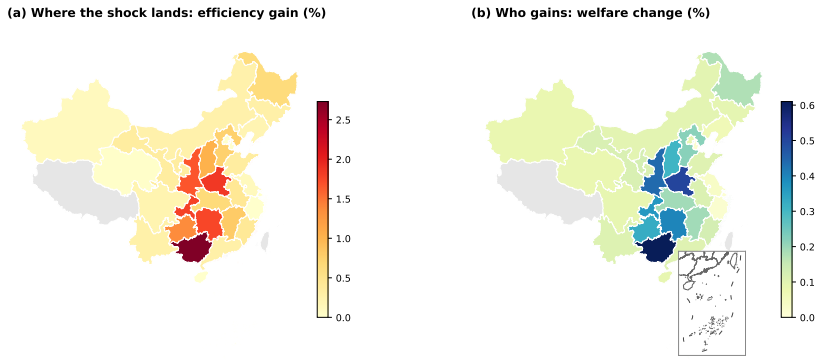
This appendix collects the descriptive, diagnostic, and robustness exhibits referenced in the text.

Figure A3: Migration Calibration: Observed and Model-Implied Bilateral Changes



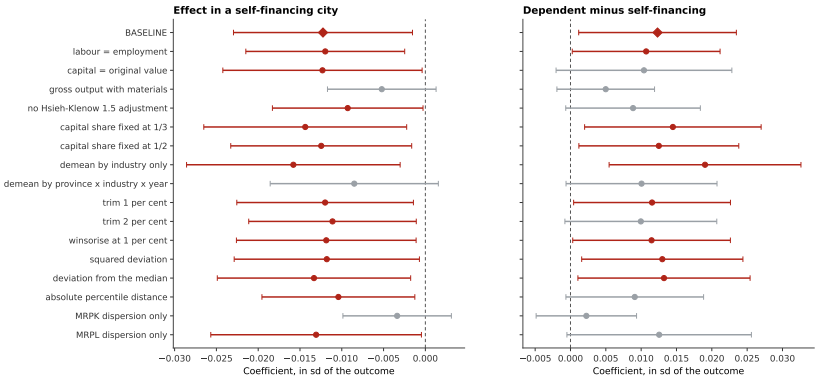

*Notes*: The figure compares observed and model-implied bilateral log share changes between the 2000 and 2005 census matrices. The same two matrices and real-income changes identify bilateral migration-cost hats, so this is a calibration reproduction rather than an independent check. The unweighted bilateral $R^2$ is 0.907.

Figure A4: Who Gains from Full Fiscal Retention



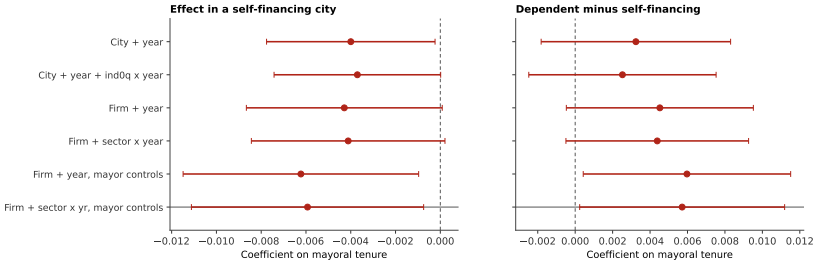

*Notes*: Panel (a) maps the CF3 manufacturing efficiency shock, $100 \times \ln \hat{\Lambda}_n$; panel (b) the province welfare gain, migration active. Gains are positive everywhere and track the shock nearly one for one (correlation 0.98); unshocked provinces still gain about 0.05 percent through cheaper traded manufactures. Grey provinces are outside the model. The model prices the allocative margin only; the redistributive function of transfers is held fixed.

## Figure A5: Robustness to How Misallocation Is Measured




*Notes*: This figure shows the robustness of the baseline tenure gradient to how misallocation is measured. Each point is the coefficient on mayor tenure in self-financing cities from the baseline specification (firm and year fixed effects, both offices' tenure clocks, and leader controls, clustered by city, Annual Survey of Industrial Firms 2000–2007), estimated for seventeen alternative constructions of the outcome: alternative production-function elasticities, trimming rules, demeaning schemes, a fully non-parametric within-cell rank measure, and squared rather than absolute deviations. Coefficients are standardised by each outcome's dispersion for comparability; bars show 95% confidence intervals.

## Figure A6: The Effect Across the Fixed-effect Ladder




*Notes*: This figure traces the same coefficient (mayor tenure in self-financing cities, dispersion outcome) across progressively more demanding specifications, from city-year aggregates with city and year fixed effects to the firm-level specification with firm and sector × year fixed effects, all controlling for both offices' tenure clocks and leader characteristics, with standard errors clustered by city. The coefficient is stable across this fixed-effect ladder, but this does not eliminate endogenous spell survival or other time-varying selection.

## Table A1: Detailed Calibration Inputs

| Object | Value | Description | Source |
|---|---|---|---|
| *Spatial production and trade* | | | |
| $\nu^j$ | (0.59, 0.27, 0.48) | Value-added shares of gross output | 2002 IO table |
| $\beta^j$ | (0.31, 0.24, 0.38) | Composite-factor shares (ag, mf, sv) | 2002 IO table $-\ \eta^j$ |
| $\eta^j$ | (0.28, 0.03, 0.10) | Land/fixed-factor shares | Tombe and Zhu (2019) |
| $\varsigma^{jk}$ | (0.16, 0.19, 0.06)<br>(0.06, 0.53, 0.14)<br>(0.01, 0.28, 0.23) | Intermediate input shares<br>(rows/columns: ag, mf, sv) | 2002 IO table |
| $\psi^j$ | (0.13, 0.31, 0.56) | Final-demand shares (ag, mf, sv) | 2002 IO table |
| $\alpha$ | 0.87 | Goods expenditure share | Tombe and Zhu (2019) |
| $1-\alpha$ | 0.13 | Housing expenditure share | Tombe and Zhu (2019) |
| $\pi^j_{nn}$ | (0.86, 0.62, 0.91) | National expenditure-weighted home trade shares | 2002 IO table |
| $N$ | 30 | Provinces in the domestic geography | Census/IO harmonization |
| *Migration block* | | | |
| $\mu_{od,t}$ | $60 \times 60$ | Origin-conditional bilateral shares | 2000/2005 census replication |
| $\Pr(o \neq d)$, 2000 | 0.1740 | Observed migrant share | 2000 census replication |
| $\Pr(o \neq d)$, 2005 | 0.2568 | Observed migrant share | 2005 census replication |
| $\kappa$ grid | {1.0, 1.5, 2.0, 3.0} | Migration-elasticity sensitivity grid | Sensitivity analysis |
| $L_{n\ell}$ | endogenous | 60-state province–labor allocation | Spatial-equilibrium closure |
| *Political block, auxiliary* | | | |
| $\overline{-\partial\chi/\partial a}$ | 0.026 per year | Within-term decline of the wedge | Data: ASIF $\times$ leader records |

*Notes:* Baseline data objects behind Table 4. Intermediate, value-added and final-demand shares are measured from the 2002 interprovincial input–output table. The capital share $\zeta$ is within the capital–labour composite: in manufacturing, the gross-output shares are 0.119 capital, 0.119 labour, 0.030 fixed factor and 0.732 intermediates. Observed 2000 and 2005 census matrices provide row-stochastic bilateral shares across 60 province–labor states; registered-worker stocks are recovered from the 2000 matrix and destination employment.

## Table A2: Levels: Reallocation Rather Than Contraction

| | (1)<br>Value added | (2)<br>VA/worker | (3)<br>Capital | (4)<br>Employment |
|---|---|---|---|---|
| Mayor tenure | -0.0151** | -0.0137* | -0.0051 | -0.0014 |
| | (0.0075) | (0.0071) | (0.0050) | (0.0028) |
| $\times$ Transfer-dependent | 0.0081 | 0.0076 | 0.0073 | 0.0005 |
| | (0.0095) | (0.0094) | (0.0058) | (0.0030) |
| Observations | 1,056,916 | 1,056,916 | 1,056,916 | 1,056,916 |

*Notes:* This table separates reallocation from contraction. Outcomes are the logs of firm value added, value added per worker, the capital stock, and employment: levels rather than dispersions, demeaned by the fixed effects only, so the coefficients read as within-firm growth effects of an additional tenure year. Sample, tenure and dependence definitions, controls, inference, and significance stars as in Table 2. City-level total manufacturing value added is unchanged across the term (+0.001 per year), so the firm-level movements reflect reallocation across firms. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## Table A3: The Price of Favouritism

| Province | $\bar{\chi}_n$ | Implied subsidy (% of mf VA) | Implied subsidy (% of own revenue) | On-book coverage (%) | Regime |
|---|---|---|---|---|---|
| Henan | 0.85 | 12.4 | 353 | 10 | Self-fin. |
| Guangxi | 0.74 | 14.8 | 237 | 12 | Dependent |
| Hunan | 0.72 | 12.8 | 345 | -2 | Self-fin. |
| Shaanxi | 0.58 | 13.1 | 349 | 0 | Self-fin. |
| Chongqing | 0.57 | 5.4 | – | -21 | Self-fin. |
| Fujian | 0.54 | 3.6 | 105 | -12 | Self-fin. |
| Guangdong | 0.53 | 2.4 | 81 | -21 | Self-fin. |
| Shandong | 0.51 | 6.0 | 202 | 0 | Self-fin. |
| Guizhou | 0.51 | 10.6 | 292 | 7 | Dependent |
| Hebei | 0.50 | 9.1 | 312 | 5 | Self-fin. |
| Shanxi | 0.50 | 9.6 | 251 | 7 | Dependent |
| Hubei | 0.46 | 9.1 | 392 | 11 | Dependent |
| Jiangxi | 0.43 | 11.0 | 226 | 7 | Dependent |
| Anhui | 0.43 | 7.9 | 212 | 27 | Self-fin. |
| Jiangsu | 0.40 | 2.7 | 114 | -5 | Dependent |
| Heilongjiang | 0.37 | 6.9 | 145 | -5 | Self-fin. |
| Ningxia | 0.31 | 7.4 | 239 | -16 | Self-fin. |
| Sichuan | 0.30 | 5.0 | 166 | 4 | Dependent |
| Liaoning | 0.29 | 6.2 | 147 | 19 | Dependent |
| Gansu | 0.28 | 6.0 | 171 | 30 | Dependent |
| Inner Mongolia | 0.27 | 4.7 | 82 | 85 | Dependent |
| Jilin | 0.26 | 4.4 | 212 | 3 | Dependent |
| Yunnan | 0.24 | 5.6 | 229 | -54 | Self-fin. |
| Xinjiang | 0.14 | 3.0 | 69 | 19 | Self-fin. |
| Zhejiang | 0.13 | 0.7 | 29 | 16 | Dependent |
| Qinghai | -0.07 | -1.7 | -55 | – | Dependent |
| Self-financing median | 0.51 | 6.9 | 221 | -2 | |
| Dependent median | 0.30 | 6.0 | 171 | 9 | |

*Notes:* This table prices each province's measured ownership wedge as the implicit annual capital subsidy $C_n(\bar{\chi}) = \sum_\iota \rho_n K_\iota (1 - e^{-\bar{\chi}_n s_\iota})$, using the 2002 manufacturing Annual Survey of Industrial Firms and the model's capital income share (0.119/0.268 of value added). Columns report the bill as a share of manufacturing value added and of the province's own current budgetary revenue (2002 prefecture fiscal accounts), and the share of the bill visible on the income statement: direct subsidies, the private-minus-state interest-rate gap applied to state capital, and the private-minus-state income-tax gap applied to state value added. On-book coverage is noisy and typically below a tenth: the wedge is carried off-book through directed credit and implicit guarantees. Chongqing's 2002 fiscal accounts are unavailable. Entries are accounting magnitudes, not estimates with standard errors.

## Table A4: Sensitivity to Structural Parameters

| | CF1 | | CF3 | | CF4 | |
|---|---|---|---|---|---|---|
| | TFP | Welfare | TFP | Welfare | TFP | Welfare |
| baseline ($\sigma = 3$, $\theta = 4$) | 0.24 | 0.10 | 0.46 | 0.21 | 0.71 | 0.33 |
| $\sigma = 2$ | 0.16 | 0.06 | 0.31 | 0.14 | 0.47 | 0.22 |
| $\sigma = 5$ | 0.39 | 0.16 | 0.78 | 0.36 | 1.19 | 0.55 |
| $\theta = 2.5$ | 0.24 | 0.10 | 0.46 | 0.21 | 0.71 | 0.33 |
| $\theta = 6$ | 0.24 | 0.10 | 0.46 | 0.22 | 0.71 | 0.33 |

*Notes:* Each row re-solves CF1 (at the point $\chi^* = 0.326$), CF3, and CF4 in the full model with migration active, varying one structural parameter around the baseline. Entries are percent changes in manufacturing TFP and in the welfare of initial residents. The wedge-to-efficiency mapping depends on $\sigma$ and $\zeta$ only through $\sigma\zeta^2$, so the $\sigma$ rows also cover proportional variation in $\zeta$; gains scale approximately linearly in $\sigma\zeta^2$ and are insensitive to the trade elasticity $\theta$. The ranking CF3 > CF1 holds in every cell.

Table A5: Trade and Migration Channel Decomposition

| | Direct | Trade | Migration | Interaction | Total |
|---|---|---|---|---|---|
| CF1: no rotation | +0.087 | +0.010 | -0.000 | +0.000 | +0.097 |
| CF3: full fiscal retention | +0.186 | +0.028 | -0.001 | +0.001 | +0.214 |
| CF4: efficiency bound | +0.285 | +0.044 | -0.002 | +0.001 | +0.329 |

*Notes:* The four cells cross Eaton–Kortum trade reallocation on/off with migration on/off. Direct is the cell with neither margin active; Trade and Migration turn on one margin at a time; Interaction is the joint residual. Entries are welfare percentage points and sum exactly to the total welfare change.

Table A6: City-Cluster Bootstrap of the Joint-Equilibrium Counterfactuals

*Panel A. TFP and welfare changes (%)*

| | TFP point | TFP 95% CI | Welfare on point | On 95% CI | Welfare off point | Off 95% CI |
|---|---|---|---|---|---|---|
| CF1 | +0.235 | [+0.199, +0.254] | +0.097 | [+0.083, +0.101] | +0.097 | [+0.083, +0.101] |
| CF3 | +0.464 | [+0.404, +0.505] | +0.214 | [+0.186, +0.233] | +0.214 | [+0.186, +0.233] |
| CF4 | +0.713 | [+0.621, +0.775] | +0.329 | [+0.286, +0.358] | +0.329 | [+0.286, +0.358] |

*Panel B. Migration contribution (basis points)*

| | Point | 95% CI |
|---|---|---|
| CF1 | +0.0025 | [-0.0013, +0.0095] |
| CF3 | -0.0144 | [-0.0177, -0.0028] |
| CF4 | -0.0200 | [-0.0248, -0.0026] |

*Notes:* The table is generated by the full 99-draw run with seed 20260828. Each draw resamples cities within province, recomputes the wedge moments, and resolves the complete trade, factor-price, housing, and migration equilibrium separately with migration active and with bilateral shares held at baseline. The observed migration matrices and trade/input–output blocks remain fixed. No legacy interval is inserted by hand.

Table A7: Model Implications and Calibration Checks

| Moment (not used in calibration) | Estimate | Standard error | Assessment |
|---|---|---|---|
| 2007 manufacturing-share fit: slope | -2.93 | 5.85 | Low predictive fit ($R^2 = 0.013$) |
| Secretary tenure gradient | +0.0049 | 0.0025 | Contradicts zero prediction |
| Secretary tenure $\times$ dependence | -0.0065 | 0.0029 | Opposite office response |
| Industrial land price, mayor tenure | +0.0081 | 0.0081 | No tenure gradient |
| Industrial land area, mayor tenure | +0.0319 | 0.0302 | No tenure gradient |

*Notes:* These moments were not used to fit the mayoral tenure profile. The opposite-signed secretary gradient confirms the office contrast and reveals the incompleteness of the one-office abstraction, while the land outcomes are flat. The table reports failures and nulls explicitly rather than counting them as support; migration-matrix reproductions are excluded because they are calibration inputs.

## Table A8: Where the Effect Lives, and the Observable Instruments

| | Mayor tenure (self-financing) | × transfer-dependent | Observations | R-squared |
|---|---|---|---|---|
| *Panel A: dispersion, \|log TFPR deviation\|* | | | | |
| All private firms | −0.0066** (0.0029) | +0.0065** (0.0031) | 963,777 | 0.589 |
| dependent-group total | −0.0001 (0.0013) | | | |
| Size interaction (small × tenure, self-fin.) | −0.0002 (0.0026) | | 464,567 | 0.583 |
| State firms | −0.0030 (0.0023) | +0.0034 (0.0032) | 82,036 | 0.698 |
| *Panel B: observable instruments (state × tenure)* | | | | |
| Subsidies / value added | −0.0005* (0.0003) | +0.0011*** (0.0004) | 1,056,916 | 0.640 |
| Interest expense / capital | +0.0003 (0.0003) | −0.0013** (0.0006) | 1,056,916 | 0.621 |
| Income tax / value added | +0.0005 (0.0003) | −0.0004 (0.0004) | 1,056,916 | 0.613 |
| P(privatised \| state at $t-1$)† | −0.0053* (0.0031) | +0.0104** (0.0049) | 62,514 | 0.035 |
| Firm FE, Year FE | Yes (industry and year FE for privatisation) | | | |
| Leader controls | Yes | | | |

*Notes:* Panel A: the absolute deviation of ln TFPR from its industry × year mean on the mayor clock, for private firms, the size interaction, and state firms. Panel B: the indicated instruments on the state share × tenure interaction, and the privatisation hazard on firms majority state in the previous year (industry and year fixed effects; base rate 0.159 per year). Sample, tenure and dependence definitions, controls, inference, and significance stars as in Table 2.

## Table A9: The Ownership Mechanism, by Raw Ownership Category

| | (1) State | (2) Collective | (3) Private | (4) Legal person | (5) Foreign | (6) HK, Macau, Taiwan |
|---|---|---|---|---|---|---|
| *Panel A: firm and year fixed effects* | | | | | | |
| Mayor tenure × Transfer-dependent | −0.0083 | +0.0163** | +0.0087 | +0.0051 | +0.0007 | −0.0015 |
| | (0.0062) | (0.0079) | (0.0111) | (0.0096) | (0.0091) | (0.0110) |
| Mayor tenure | −0.0003 | −0.0191*** | −0.0155* | −0.0132** | −0.0145* | −0.0100 |
| | (0.0044) | (0.0052) | (0.0090) | (0.0067) | (0.0082) | (0.0065) |
| Observations | 101,817 | 116,561 | 452,217 | 218,796 | 67,073 | 83,337 |
| R-squared | 0.786 | 0.741 | 0.739 | 0.738 | 0.782 | 0.772 |
| *Panel B: firm and sector × year fixed effects* | | | | | | |
| Mayor tenure × Transfer-dependent | −0.0084 | +0.0176** | +0.0086 | +0.0051 | −0.0011 | −0.0009 |
| | (0.0060) | (0.0081) | (0.0109) | (0.0094) | (0.0108) | (0.0109) |
| Mayor tenure | +0.0011 | −0.0192*** | −0.0145 | −0.0119* | −0.0136 | −0.0100* |
| | (0.0042) | (0.0057) | (0.0089) | (0.0064) | (0.0088) | (0.0061) |
| Observations | 101,813 | 116,550 | 452,207 | 218,791 | 67,060 | 83,335 |
| R-squared | 0.792 | 0.747 | 0.744 | 0.743 | 0.788 | 0.779 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* This table is the within-group counterpart of Table 3 and takes apart the groups it pools. The outcome is ln MRPK, the sample split is on shares of paid-in capital in the firm's first observed year, and the same specification is estimated inside each of the six raw categories; the two mayor rows are reported, the secretary rows being as in Table 3. Collective and legal-person firms behave like private ones and Hong Kong, Macau and Taiwan capital like foreign, which is what licenses the pooling; the legal-person gradient of −0.013 sits between private and foreign and nowhere near the state column's zero. Panels follow Table 2. Sample, tenure and dependence definitions, controls, inference, and significance stars as in Table 2.

## Table A10: The Quantity Side: Private Ownership and Firm Capital

| | (1) ln capital | (2) ln capital, city-relative |
|---|---|---|
| *Panel A: firm and year fixed effects* | | |
| **Private share × Mayor tenure × Transfer-dependent** | −0.0152 | −0.0154 |
| | (0.0110) | (0.0128) |
| Private share × Mayor tenure | +0.0252*** | +0.0186* |
| | (0.0082) | (0.0100) |
| Mayor tenure | −0.0218*** | −0.0259** |
| | (0.0079) | (0.0125) |
| Mayor tenure × Transfer-dependent | +0.0162* | +0.0210 |
| | (0.0089) | (0.0171) |
| Private share × Mayor tenure, transfer-dependent | +0.0100 | +0.0031 |
| | (0.0075) | (0.0080) |
| Observations | 1,056,916 | 1,056,916 |
| R-squared | 0.915 | 0.934 |
| *Panel B: firm and sector × year fixed effects* | | |
| **Private share × Mayor tenure × Transfer-dependent** | −0.0161 | −0.0157 |
| | (0.0106) | (0.0127) |
| Private share × Mayor tenure | +0.0247*** | +0.0182* |
| | (0.0078) | (0.0098) |
| Mayor tenure | −0.0221*** | −0.0265** |
| | (0.0074) | (0.0123) |
| Mayor tenure × Transfer-dependent | +0.0162* | +0.0211 |
| | (0.0086) | (0.0170) |
| Private share × Mayor tenure, transfer-dependent | +0.0086 | +0.0025 |
| | (0.0072) | (0.0081) |
| Observations | 1,056,914 | 1,056,914 |
| R-squared | 0.915 | 0.934 |
| Controls | Yes | Yes |

*Notes:* The quantity counterpart of Table 3: the same saturated private-share specification with capital on the left. Column (1) is log firm capital; column (2) subtracts log total city manufacturing capital, the firm's position within a roughly fixed local stock. The last row is the implied transfer-dependent gradient. The regime interaction is less precise than its price-side counterpart, and the paper reads this table as corroboration rather than identification. Sample, tenure and dependence definitions, controls, inference, and significance stars as in Table 2.

## Table A11: Selection, Horizons, and the Tenure Profile

| | State × tenure (reference group) | Differential for named group | Observations | R-squared |
|---|---|---|---|---|
| Pooled (headline) | +0.0197*** (0.0051) | | 518,374 | 0.751 |
| *T1: career horizons — appointment age* | | | | |
| Old appointees (above-median age) | +0.0159* (0.0081) | +0.0080 (0.0131) | 518,374 | 0.751 |
| *T2: career horizons — expected term from city rotation history* | | | | |
| Short expected term (below median) | +0.0179*** (0.0062) | +0.0077 (0.0125) | 518,374 | 0.751 |
| *T3: survivorship — spell length observed ex post* | | | | |
| Within spells lasting 4+ years | +0.0192*** (0.0068) | | 331,020 | 0.768 |
| Within spells shorter than 4 years | −0.0253** (0.0113) | | 154,650 | 0.807 |
| Firm FE, Year FE | | Yes | | |
| Leader controls | | Yes | | |

*Notes:* This table probes accumulation, career-horizon, and survivorship interpretations; it does not separate them causally. Coefficients are on the state share × mayor tenure interaction in self-financing cities (ln MRPK, industry × year demeaned; firm and year fixed effects; $N = 591{,}809$). T1 splits by appointment age, T2 by expected term length, T3 by ex-post spell length, an endogenous retention outcome ($N = 392{,}438$ and 199,371). Significance: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A12: The Joint-Office Mayor–Secretary Contrast

| | $\lvert \ln(VA/L) \rvert$ |
|---|---|
| Mayor | -0.0073*** |
| | (0.0025) |
| Secretary | 0.0049** |
| | (0.0025) |
| **Mayor − Secretary** | -0.0123*** |
| | (0.0045) |
| Mayor × dependent | 0.0069** |
| | (0.0027) |
| Secretary × dependent | -0.0065** |
| | (0.0029) |
| **Difference-in-difference** | 0.0134*** |
| | (0.0050) |
| Observations | 1,045,078 |
| Cities | 257 |

*Notes:* This table reports the joint-office contrast specification; the triple interaction is a heterogeneity pattern, not a source of quasi-experimental identification. The outcome is the absolute deviation of log value added per worker from its industry × year mean. Rows report the mayor and party-secretary tenure gradients, their within-city difference, the interactions with transfer dependence, and the office contrast across fiscal regimes. Both offices share every city-year but have different formal portfolios; because their authority overlaps, the comparison is not an exclusion restriction. Sample, tenure and dependence definitions, controls, inference, and significance stars as in Table 2.

Table A13: Party Secretary Tenure and Misallocation

| | (1) VA/worker | (2) Rev/worker | (3) MRPL | (4) TFPR | (5) $TFPR^2$ |
|---|---|---|---|---|---|
| *Panel A. Dispersion, firm and year fixed effects* | | | | | |
| Party secretary tenure | 0.0049** | 0.0023 | 0.0035 | 0.0050* | 0.0086* |
| | (0.0025) | (0.0022) | (0.0026) | (0.0025) | (0.0049) |
| × Transfer-dependent | -0.0065** | -0.0028 | -0.0068** | -0.0066** | -0.0131** |
| | (0.0029) | (0.0031) | (0.0033) | (0.0029) | (0.0055) |
| Observations | 1,045,078 | 1,045,384 | 1,045,079 | 1,056,916 | 1,056,916 |
| *Panel B. Dispersion, firm and sector × year* | | | | | |
| Party secretary tenure | 0.0047** | 0.0023 | 0.0032 | 0.0048* | 0.0082* |
| | (0.0023) | (0.0023) | (0.0025) | (0.0025) | (0.0048) |
| × Transfer-dependent | -0.0059** | -0.0025 | -0.0060* | -0.0065** | -0.0128** |
| | (0.0027) | (0.0031) | (0.0032) | (0.0029) | (0.0054) |
| Observations | 1,045,076 | 1,045,382 | 1,045,076 | 1,056,914 | 1,056,914 |

*Notes:* This table collects secretary-only dispersion coefficients. Their opposite-signed pattern confirms the office contrast: the office without the allocation instruments shows no favouritism erosion, motivating a two-office extension; it is not labelled a placebo or positive validation. Outcomes and inference otherwise follow Table 2.

Table A14: Mayor Characteristics and Tenure Processes by Fiscal Regime

| | (1) Self-financing | (2) Transfer-dependent | (3) Difference |
|---|---|---|---|
| Mayor age at appointment | 48.6 | 47.8 | $-0.8^{**}$ |
| | | | (0.3) |
| Mayor education (years) | 17.9 | 18.1 | 0.2 |
| | | | (0.2) |
| Completed spell length (years) | 2.83 | 2.80 | $-0.02$ |
| | | | (0.15) |
| Share of spells reaching year 4 | 0.290 | 0.289 | 0.000 |
| | | | (0.055) |
| Annual turnover rate | 0.316 | 0.325 | 0.010 |
| | | | (0.013) |

*Notes:* Descriptive comparison of mayors and the tenure process across fiscal regimes, 2000–2007: age and education are measured in the appointment year; spell statistics use completed interior spells; the turnover rate is the share of city-years with a new mayor. Column (3) reports the mean difference with standard errors, in parentheses, clustered by city. Completed spell lengths, the share of spells reaching year four, and turnover rates are indistinguishable across regimes, so the tenure clocks being compared run at the same pace; mayors in transfer-dependent cities are appointed slightly younger. This is a descriptive comparability check, not an identification claim. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.