

Moving Officials, Moving Access: The Distributive Effects of Bureaucratic Rotation

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Abstract

How does bureaucratic rotation shape the political allocation of public resources? We propose a theory of *portable political connections*: officials whose careers depend on implementing policy favor firms from earlier postings when they know little about local alternatives. We examine public procurement as a form of public resource allocation, combining career histories, contracts, bids, and firm records for 335 Chinese cities (2013–2020). Firms from incoming leaders’ prior work cities gain contracts but lose most of this advantage after connections end. Connected winners later obtain more business and enter more external markets. Mechanism analysis shows that longer origin-city tenure strengthens preferential access, whereas prior destination service weakens it, suggesting that information asymmetries drive officials’ reliance on familiar firms. Connected awards carry higher prices and poorer performance, indicating potential corruption. Rotation can therefore extend favoritism in public resource allocation across jurisdictions, creating commercial advantages for connected firms at public expense.

Keywords: Bureaucratic rotation, China, political-business relations, political connections, public procurement.

Word count: 9,978.

Introduction

Governments rotate officials across jurisdictions to reward performance, maintain control, and prevent entrenched local alliances (Abbink 2004; Hassan 2017; Khan et al. 2019; Landry 2008). Transfers relocate authority but preserve business relationships that help officials implement policy when local firms are unfamiliar (Hou and Li 2023; Shi et al. 2021; Toral 2024). Whether rotation limits favoritism therefore depends on how officials use these relationships in new posts.

Political connections shape which firms receive public resources and on what terms. Connected firms are common across countries (Faccio 2006) and win more government contracts and bailouts (Boas et al. 2014; Faccio et al. 2006; Goldman et al. 2013). Favoritism toward them can also weaken competition and raise purchasing costs (Baránek and Titl 2024; Brugués et al. 2024; Dahlström et al. 2021; Lehne et al. 2018; Schoenherr 2019; Tkachenko and Esaulov 2020). Rotation changes where officials exercise authority over public resources, but their earlier business ties can persist. How do these ties shape allocation after officials move?

Public resources can also shape recipients' later market opportunities. Research on mobile connections documents investment, land access, and trade between officials' former and current jurisdictions (Jiang and Mei 2020; Li et al. 2022; Nian and Wang 2023; Shi et al. 2021), and public contracts provide revenue and experience that support later growth (Ferraz et al. 2021; Hvide and Meling 2023). Do connected recipients expand beyond the official's jurisdiction, how long do their advantages last, and what costs do public buyers bear?

We argue that officials carry *portable political connections*: relationships with and knowledge of firms acquired in earlier postings. Ordinary policy cooperation can create these ties and reveal which firms are reliable (Evans 1995; Macchiavello and Morjaria 2015; Toral 2024). After reassignment, officials need dependable partners but know little

about unfamiliar firms’ capabilities or willingness to cooperate (Jiang 2018; Laffont and Tirole 1993). They therefore have incentives to use their authority over public resources to favor familiar partners (Li et al. 2022; Shi et al. 2021; Szucs 2024), which we call *rotation-based favoritism*. Access should weaken after departure as authority changes hands. Longer origin-city tenure should strengthen it by deepening knowledge of earlier partners, whereas prior destination service should weaken it by reducing uncertainty about local alternatives (Hou and Li 2023). Both predictions follow from the same information asymmetry.

Public resources can finance recipients’ expansion and provide reusable experience, although their commercial advantage should narrow as competitors acquire comparable capabilities. When officials’ preference for dependable cooperation displaces better-value alternatives, favoritism can weaken competition and reduce the value public agencies receive.

We examine these arguments with Chinese municipal leadership rotations and public procurement, combining leadership careers, goods contracts, disclosed bids, and firm records for 335 cities in 2013–2020. The setting suits our argument because provincial authorities rotate city leaders frequently, partly to limit lasting local power, and city leaders influence a large procurement market. A difference-in-differences (DiD) design compares procurement from connected and unconnected origins supplying the same destination around appointment, and a counterfactual estimator evaluates access after departure. We then follow firms that won or lost their first connected bid outside their home city into external markets. Comparisons of the same recorded product models show what agencies pay and receive.

Our analysis yields four main findings. First, origin firms gain preferential access after a leader arrives, worth about 0.55 million renminbi (RMB) in additional contracts per connected origin–destination pair each month, and this advantage falls by about four-fifths in the six months after the connection ends. Second, initial winners later obtain more

business and enter more external markets. Their annual probability of winning an external contract rises by 3.30 percentage points, although their advantage in new contracts narrows. Third, longer origin tenure strengthens access and prior destination service weakens it, consistent with the information asymmetry mechanism. Fourth, connected awards carry prices 3.83 per cent higher for comparable product models, with poorer performance and weaker competition, indicating potential corruption and waste in public resource allocation. The price premium alone implies about RMB 730 million in additional expenditure a year in the connected price sample.

Our study makes three contributions. First, we show that personnel institutions have distributive consequences for state–business relations. Research explains how bureaucratic organization shapes officials’ incentives (Charron et al. 2017; Dahlström et al. 2012; Grindle 2012; Pepinsky et al. 2017; Xu 2018) and how appointments and transfers serve political control (Agnihotri et al. 2026; Hassan 2017; Iyer and Mani 2012; Reuter and Robertson 2012). Staff rotation is a standard remedy for capture (Abbink 2004), yet it can relocate favoritism across jurisdictions. Building on work on local capture, learning, and embeddedness (Bardhan and Mookherjee 2000; Bhavnani and Lee 2018; Hou and Li 2023; Persson and Zhuravskaya 2016; Tkachenko and Esaulov 2020), we explain how reassignment changes which firms receive resources through officials’ accumulated relationships and knowledge. Officials’ geographic experience thus shapes how personnel reform redistributes public resources. Second, we show how political allocation shapes expansion beyond connected jurisdictions. Mobile connections give firms opportunities where officials move (Nian and Wang 2023; Shi et al. 2021), and public contracts support growth and commercial innovation in Brazil, Norway, and China (Beraja et al. 2023a; Beraja et al. 2023b; Ferraz et al. 2021; Hvide and Meling 2023). Bid records let us follow connected winners and losers across all 335 cities, so we can show how political selection distributes these opportunities beyond officials’ networks and accelerates entry into markets where other agencies make purchasing decisions. Third, we weigh recipients’ commercial gains

against what public agencies pay and receive. Extending research on connected purchasing and wasteful public spending (Bandiera et al. 2009; Baránek and Titl 2024; Brugués et al. 2024; Lehne et al. 2018; Schoenherr 2019), we examine prices, performance, and competition together. The 3.83 per cent premium is comparable to the 6 per cent overpricing of connected Czech firms (Baránek and Titl 2024) but arises under rotation designed to prevent local capture. The same contract can help a firm gain experience and reach new buyers while the awarding agency pays more for poorer performance.

The Distributive Effects of Bureaucratic Rotation

Officials rely on firms to carry out projects, provide public goods and services, and invest in policy initiatives, and firms seek public finance, assets, and business opportunities in return (Shleifer and Vishny 1994). Where career advancement rewards implementation, officials value responsive partners (Evans 1995; Hassan 2020; Jiang 2018). Our argument concerns officials with authority over public resources and firms that can operate across jurisdictions. Rotation relocates this authority, while officials retain earlier knowledge and relationships.

Political Relationships and Resource Allocation. *Portable political connections* grow out of ordinary cooperation. Implementing a project lets an official observe a firm’s capabilities, reliability, and responses to unforeseen demands, and the firm learns the official’s priorities, which eases later coordination. Repeated exchange generates knowledge and expectations of continued cooperation that remain useful after reassignment (Macchiavello and Morjaria 2015; Toral 2024).

Rotation creates an information asymmetry between familiar firms and local alternatives. An incoming official has worked directly with some earlier partners but knows less about destination firms, and formal qualifications only partly reveal a firm’s capabilities and willingness to accommodate changing demands (Laffont and Tirole 1993). We as-

sume officials must implement policy before learning fully about these alternatives. Local suppliers may offer lower coordination costs and better local knowledge, but uncertainty about their cooperation makes familiar outsiders more predictable partners for officials who must implement policy quickly (Hou and Li 2023; Shi et al. 2021). Economic actors facing uncertainty likewise favor partners from past exchanges (Granovetter 1985; Podolny 1994).

Officials can turn this preference into allocations directly or through agencies that assess applications and set terms. Managers who depend on officials for budgets or career rewards have incentives to accommodate preferred firms (Brierley 2020; Romero 2025), and eligibility requirements and evaluation criteria give agencies room to favor these firms when deciding who qualifies and whose application succeeds (Laffont and Tirole 1991; Lu and Wang 2023; Schoenherr 2019). Agency autonomy and enforceable standards constrain this discretion (Bersch and Fukuyama 2023; Charron et al. 2017; Fazekas et al. 2023). Familiarity becomes *rotation-based favoritism* when it changes who receives public resources.

Officials' knowledge of firms is concentrated in their prior work jurisdictions, where familiarity with agencies and business communities also helps them assess other firms or receive recommendations about them. Because these firms can operate in the new destination without relocating (Nian and Wang 2023), rotation can shift resources toward earlier partners.

Hypothesis 1 *After a leader takes office in a destination jurisdiction, firms from the leader's prior work city experience a greater increase in access to public resources than otherwise comparable firms from unconnected cities.*

Political Privilege and Competitive Advantage. Preferential access can change firms' competitive positions (Fisman 2001; Rijkers et al. 2017). Public resources can finance production and build experience useful for later business, and other buyers may then se-

lect recipients for the capabilities acquired through the allocation. Political selection can therefore shape opportunities beyond the official's jurisdiction.

Such expansion requires reusable benefits. Revenue supports production, completed projects demonstrate reliability, and experience improves delivery. Initial government sales and buyer relationships can generate growth, learning, and new customers (Alfaro-Ureña et al. 2022; Ferraz et al. 2021; Hvide and Meling 2023), and reusable data and technology support commercial applications (Beraja et al. 2023a; Beraja et al. 2023b). These assets help firms build buyer relationships and overcome barriers to entering other markets (Cai and Szeidl 2018; Johanson and Vahlne 2009; McMillan and Woodruff 1999).

Although recipients retain the resources they acquire, their advantage in new business can narrow. We assume that competitors obtain comparable resources and experience through later transactions, which makes recipients' capabilities less distinctive. An initial allocation can thus accelerate entry while recipients' later flow of new business converges toward that of unsuccessful applicants.

Hypothesis 2 *Among firms from a leader's prior work city seeking an initial allocation in the new jurisdiction, recipients subsequently obtain more business and enter more markets outside connected jurisdictions than unsuccessful applicants. Their advantage in new business narrows as competitors acquire similar resources.*

Political Position and Rotation-Based Favoritism. The resources and experience that support external expansion can outlast political support. Continued priority in the connected jurisdiction, however, depends on the official's authority over allocation. After departure, the official still knows earlier partners, but agencies respond to a successor's priorities. Managers who seek budgets and career rewards have incentives to accommodate the new leadership's preferred firms, and successors can redirect agencies' priorities, budgets, and appointments toward those partners (Akhtari et al. 2022). Turnover therefore reduces the value of ties to former incumbents (Blanes i Vidal et al. 2012; Goldman et al. 2013; Shi et al. 2021). After Ukraine's 2004 Orange Revolution, for example, firms in

regions that had backed the new president gained productivity relative to firms in regions that had backed his rival, especially in sectors relying on government contracts (Earle and Gehlbach 2015).

The decline may be gradual because approved projects and commitments made under the former leadership can continue. Agencies may also retain partners whose performance they know, and organizational relationships can survive individual departures (Rogan 2014). The speed of decline therefore depends on how much access rests on the departing official and how much has become part of agency routines. Firms relying on that official must rebuild support under the new leadership (Zhu and Zhang 2017). Access sustained by the official should decline as earlier commitments are fulfilled, provided no serving official maintains the connection.

Hypothesis 3 *After the connected leader leaves and no serving leader maintains the connection, firms from the leader's prior work city lose their advantage in new allocations as commitments made under the former leadership are fulfilled.*

Geographic Experience and Preferential Access. Geographic experience shapes the information officials bring to allocation. Longer origin-city tenure gives officials more chances to observe firms across projects and disruptions (Macchiavello and Morjaria 2015), which widens earlier partners' informational advantage. Relationships accumulated over longer service therefore remain more useful after reassignment (Nian and Wang 2023) and strengthen preferences for origin firms.

Prior destination service narrows that advantage by providing knowledge of local firms and agencies. An official who previously worked there begins the new posting knowing how local firms perform and which agencies have cooperated well with them, so dependable local partners are easier to identify. Holding origin tenure constant, familiarity with these alternatives reduces uncertainty (Hou and Li 2023) and reliance on origin firms. The two predictions thus follow from one information asymmetry, because service in each jurisdiction improves knowledge of different partners.

Experience also creates relationships and obligations (Coviello and Gagliarducci 2017; Persson and Zhuravskaya 2016; Tkachenko and Esaulov 2020; Xu et al. 2023). During later service, learning about destination firms should reduce information-driven reliance on earlier partners (Hou and Li 2023), although continued cooperation can renew those partners' informational advantage and reciprocal commitments. Our predictions therefore concern familiarity at appointment, while continued access depends on how learning and relationships evolve.

Hypothesis 4 *The advantage in access to public resources associated with a leader's prior work city is greater with longer service in that city and smaller with previous service in the destination.*

Private Gains and Public Costs. Familiarity can improve allocation by identifying reliable firms (Coviello et al. 2018). Our public-cost prediction also assumes that career rewards weigh visible implementation and responsive cooperation more heavily than the economic performance of each allocation. Officials may then prefer predictable partners even when alternatives offer comparable performance at lower cost, because a partner's willingness to accommodate an official's demands carries career value even when the agency pays more or receives poorer service.

Discretion lets agencies shield preferred firms from competitive pressure (Romero 2025; Szucs 2024), which weakens their incentives to offer better terms. Expected political support can also sustain market positions while discouraging capability development (Akcigit et al. 2023). An allocation can therefore support a firm's expansion elsewhere while giving the awarding agency less value.

Public agencies lose resources when familiar firms deliver less for the same expenditure or acquire public assets on terms yielding less revenue. Prices, performance, and competition make these losses measurable in public contracts (Baránek and Titl 2024; Brugués et al. 2024; Lehne et al. 2018; Schoenherr 2019). Such losses can arise through ordinary

administrative decisions (Bandiera et al. 2009) and leave fewer resources for other purposes. Cooperation supplies the information that motivates preferential selection, while officials' priorities and weakened competition can turn that selection into public losses.

Hypothesis 5 *Public allocations to firms connected through officials' prior postings carry higher costs without compensating improvements in performance.*

Managing and Rotating City Leaders in China

China rotates municipal leaders to broaden their experience, fill vacancies, and limit lasting local power. Provincial Party authorities control prefecture-level appointments, with organization departments proposing candidates and consulting both jurisdictions before provincial committees decide collectively. The 2006 exchange regulations favor within-province transfers during leadership renewal (CPC Central Committee 2006), and transfers can advance careers without an immediate increase in rank (Jia and Xu 2020; Landry 2008).

Mayors direct departments, budgets, and public services. Municipal people's congresses elect them, and their standing committees designate acting mayors between sessions (National People's Congress 2015). Party secretaries chair the committees that decide major policy and personnel matters (CPC Central Committee 2015). Both influence public allocations, so firms have reasons to cooperate with them on land, finance, and policy initiatives (Bai et al. 2020; Hou 2019). Careers span several cities and sometimes earlier destination posts, allowing us to separate origin relationships from destination familiarity.

Our study period contains 1,543 appointments, 778 of mayors and 765 of party secretaries (Table 1). A promotion is recorded for 66.2 per cent of them, and 34.5 per cent of incoming officials had previously served in the destination. Appointments peak at 313 in

2016 (Figure 1), during the concentrated renewal of local leadership teams ahead of the 2017 Party Congress (Liu 2016).

Table 1: Promotion and Prior Destination Service

Prior destination service	Promotion at appointment		Total
	Horizontal	Promoted	
No	340	671	1,011
Yes	182	350	532
Total	522	1,021	1,543

Notes: Counts cover appointments in 2013–2020 (778 mayors, 765 party secretaries). Prior destination service includes any earlier municipal post there. Horizontal appointments have no recorded promotion. Promoted appointments have a recorded promotion. Initial incumbents and appointments following gaps in the career record are excluded.

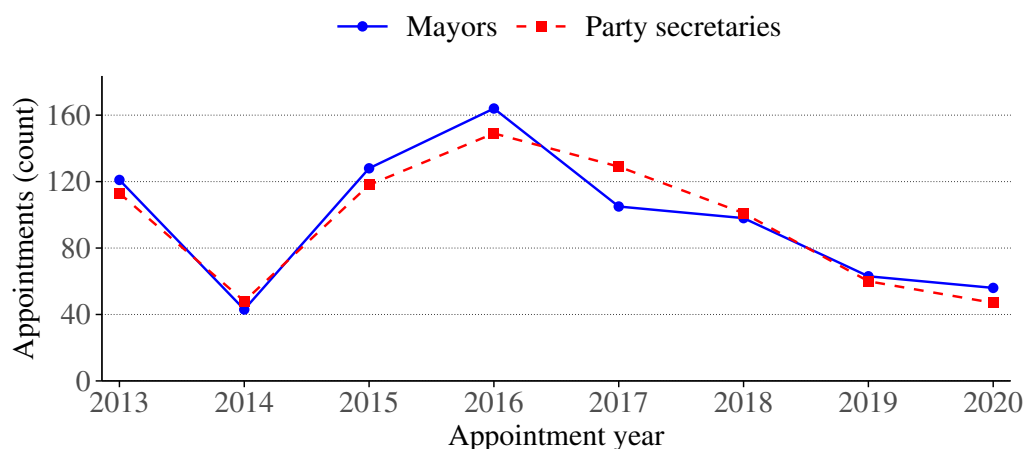


Figure 1: Annual City-Leader Appointments, 2013–2020

Note: Each appointment to a leadership position is counted separately. Initial incumbents and spells first recorded after gaps are excluded.

Government Procurement and City Leadership

Government procurement gives municipal leaders influence over a large market. National purchases reached RMB 3.70 trillion in 2020, 92.3 per cent of it by subnational units (Ministry of Finance 2021). Municipal departments and public institutions buy catalog goods through designated agencies and other goods directly or through appointed agents (National People’s Congress 2014, arts. 2, 7, 18–19). Buyers specify needs, solicit offers, select suppliers under lowest-evaluated-price or comprehensive-scoring rules (State Council

2015, art. 34), and inspect delivery before payment. Supplier selection determines which firms gain business, while contract terms and delivery determine what the public receives.

City leaders shape these purchases through budgets, coordination, and personnel. Shanghai’s 2018 measures, for example, assign procurement policy to a government-appointed committee and supervision to the finance department (Shanghai Municipal People’s Government 2018). Leaders can introduce familiar firms or encourage eligibility and evaluation rules that favor them, despite regulations requiring independent assessment.¹ Agencies thus provide channels for preferential treatment (Gong and Zhou 2015; Lu and Wang 2023; Romero 2025; Tang et al. 2025). Figure A2 summarizes how leadership changes affect supplier selection, purchasing costs, and recipients’ later opportunities.

Research Design

Data

We combine leadership careers, procurement records, firm registrations, and a socioeconomic panel for 335 cities in 2013–2020. Dated biographies identify each leader’s prior work cities, service duration, and earlier posts in the destination.

The procurement records contain about a million goods contracts awarded by municipal buyers, identifying buyers, suppliers, amounts, and procedures. Bid records list participants, quotations, and winners. Product models, quantities, and unit prices permit comparisons of equivalent goods, and acceptance scores and delays measure performance. A

¹The 2015 regulations require independent assessment, normally by randomly selected experts, and awards that follow the recommended ranking (State Council 2015, arts. 39–43). The procurement law prohibits regional barriers and the unlawful designation of suppliers (National People’s Congress 2014, arts. 5, 64).

single-bidder award discloses one participant, single-source is a procurement procedure, and winner-only notices report only the recipient (Table A1). The fields follow the disclosure formats of the China Government Procurement Platform (<https://www.ccgp.gov.cn/>) and Biaozhaozhao (<https://www.biaozhaozhao.com/>). Firm identifiers match transactions to registry information corresponding to Qichacha (<https://www.qcc.com/>) on location, age, legal form, capital, and insured employment, and a supplier’s registered city defines its origin. The socioeconomic city panel supplies gross domestic product (GDP), population, revenue, and expenditure. Appendix A documents definitions and source records (Figure A1) and describes the sample (Tables A2, A3, and A4).

Figures A3 and A4 map 1,885 distinct within-province ties between prior work cities and leadership destinations and 334 cross-province ties. Consistent with personnel rules, portable ties concentrate within provinces but also reach other provincial administrations.

Empirical Strategy

We define connections from leaders’ dated careers. An origin $o \neq d$ is connected to destination d in month t , $C_{odt} = 1$, when either the serving mayor or party secretary previously worked in o , and a firm’s registered city determines its connection status. Every prior work city counts while either leader maintains the tie, and the connection ends when neither does.² An arrival event e is the first recorded appointment connecting the two cities, dated G_e , with relative month $k = t - G_e$.

To test Hypothesis 1, we compare monthly procurement from connected and unconnected origins supplying the same destination, including the destination’s own suppliers, from twelve months before to twenty-four months after arrival. Awards are aggregated by

²If a leader serves in A, then B, and then C, both A–C and B–C are connected during service in C. A move from mayor to party secretary preserves these ties. Office-specific arrival estimates group events by the office that first created the connection.

the supplier’s registered city, with zeros for months without awards, and comparison cities remain unconnected throughout this window. The sample contains 1,135 arrival events in 2014–2018. The two-year follow-up gives incoming leaders time to learn about local suppliers (Hou and Li 2023), and research on political budget cycles shows that spending and firm benefits vary over a posting (Chen and Zhang 2021; Guo 2009). Follow-up continues through later endings or renewals, so the estimates capture the procurement path that begins with arrival. Events receive equal weight, and within each event the connected origin and the unconnected origins as a group receive equal weight.

We use the inverse hyperbolic sine (asinh) transformation. Monthly outcomes are $Y = \text{asinh}(N)$ for award count and $Y = \text{asinh}(V/10,000)$ for value in RMB, which retain zeros and limit the influence of large purchases. Subtracting each origin’s outcome in the month before arrival gives $Z_{ejk} = Y_{ejk} - Y_{ej,-1}$ for origin j in event e . With Connection_{ej} identifying the newly connected origin and $\text{Post}_{ek} = 1\{1 \leq k \leq 24\}$, the average model uses months 1–24:

$$Z_{ejk} = (\beta_A + \delta' \Delta \mathbf{X}_e)(\text{Connection}_{ej} \times \text{Post}_{ek}) + \lambda_{ek} + \varepsilon_{ejk}. \quad (1)$$

Differencing removes stable bilateral differences, and event-by-relative-month fixed effects (FE), λ_{ek} , absorb monthly conditions shared by origins supplying the destination. In the most parsimonious models, $\delta = 0$, and β_A measures the average connected–unconnected difference in procurement changes. Because economic size and fiscal conditions can affect suppliers’ capacity to expand, adjusted models let the advantage vary with $\Delta \mathbf{X}_e$, the connected origin’s $\log(1 + \text{GDP})$, $\log(1 + \text{Population})$, $\log(1 + \text{Revenue})$, and fiscal self-reliance (revenue/expenditure) in the year before appointment minus the unconnected origins’ averages. In these models, β_A measures the advantage when the connected origin’s economic and fiscal characteristics equal the comparison cities’ average.

Standard errors are clustered by connected origin and destination, allowing dependence across events that share either city. Identification requires that, without the new connection, procurement from connected and unconnected origins would change in parallel, conditional on measured economic and fiscal gaps in adjusted models. The FE absorb destination-wide purchasing increases, but demand shifts favoring goods from the leader's former city would enter the estimated advantage. Procurement must also not respond to anticipated appointments, and comparison cities must carry no continuing effects of earlier connections (Imai and Kim 2021; Imai et al. 2023).

We also test whether connected firms are more likely to win among participants seeking the same contract, using 3,650,350 disclosed bids by 144,400 firms, including local suppliers. A linear probability model includes connection status at award, a local-firm indicator, and firm and contract FE. Contract FE hold the purchasing opportunity constant and firm FE absorb stable capabilities, so the connection coefficient compares connected and unconnected outsiders. Standard errors are clustered by destination and firm home city. A favoritism interpretation requires that remaining differences in bid quality be unrelated to connection status.

To test Hypothesis 2, we compare firms that won ($W_f = 1$) or lost ($W_f = 0$) their first recorded connected nonlocal bid, a common starting point for firms seeking business in connected markets. Firms must have bid in 2013–2014 and made this bid in 2015–2018, which ensures earlier activity and at least two later years to capture the predicted early expansion. We follow 20,598 firms annually from 2013 to 2020, including years without awards.

External markets exclude the firm's home city and every destination connected to that city through leaders' careers at any time in 2013–2020. Applying these exclusions throughout the panel keeps the definition fixed as leaders move, so the outcomes measure growth outside the recorded career network. Outcomes are annual asinh award value and count, the probability of any award, the number of distinct buying cities, entry into cities where

the firm receives its first recorded award, and average award value, which measures order size.

Let c_f be the year of firm f 's first connected nonlocal bid. We estimate

$$Y_{fy} = \beta_F W_f 1\{y > c_f\} + \gamma_F W_f 1\{y = c_f\} + \alpha_f + \lambda_{o(f),y} + \psi_{c_f,y} + \epsilon_{fy}. \quad (2)$$

The coefficient β_F compares initial recipients' later external growth with that of losers, net of their pre-bid difference, and γ_F separates the bid year from later full years. Firm FE, α_f , absorb stable capabilities. Home-city-by-year FE, $\lambda_{o(f),y}$, absorb changing conditions in firms' home markets, and bid-cohort-by-year FE, $\psi_{c_f,y}$, absorb changes shared by firms first bidding in connected markets in the same year. Firms remain in their initial winner or loser group after later awards, so later business counts toward the growth path that follows the initial decision.

Standard errors are clustered by home city. Identification requires that, without the initial award, winners and losers would grow similarly in external markets, conditional on the FE, and that initial awards leave losers' growth unaffected. Because firms already growing faster could be more likely to win, we test for pre-bid differences and also compare winners and losers of the same contests.

For Hypothesis 3, month 0 is the first month in which neither serving leader maintains the tie. We retain 508 endings with procurement records for the six preceding months and eighteen consecutive unconnected months (0–17). This common follow-up shows whether earlier commitments and agency relationships sustain access after departure.

The fixed-effects counterfactual estimator (FEct) predicts procurement from formerly connected origins without the connection (Liu et al. 2024). It fits city-pair and destination-month FE to unconnected observations from eligible pairs and from origins never connected to the same destinations, capturing stable bilateral trade and monthly purchasing changes shared across origins supplying a destination. Because earlier commitments may

sustain purchases after departure, we exclude months 0–6 from fitting so that these purchases do not determine the unconnected baseline.

For ending event e and relative month k , let Y_{ek} be the transformed value or count of awards and $\hat{Y}_{ek}^0$ its predicted level without the connection. We calculate the remaining advantage and its early change as

$$\begin{aligned}\hat{g}_{ek} &= Y_{ek} - \hat{Y}_{ek}^0, \\ \hat{\Delta} &= \bar{\hat{g}}_{1:6} - \bar{\hat{g}}_{-6:-1}.\end{aligned}\tag{3}$$

A positive $\hat{g}_{ek}$ indicates procurement above the predicted unconnected level, and overbars denote means across events and the specified months. The contrast $\hat{\Delta}$ thus compares the first six months after the ending with the six months before it, with negative values indicating contraction. Gaps in months 12–17 show whether access persists beyond the initial transition.

Standard errors come from 499 destination-city bootstrap draws. The model assumes that unconnected procurement reflects stable bilateral levels and destination–month changes, with residual bilateral shocks unrelated to connection histories, and that fitting observations are free of continuing effects of the connection.

For Hypothesis 4, origin tenure proxies knowledge of earlier firms, and prior destination service proxies local familiarity. The sample contains 1,142 arrivals with both measures known before appointment. Origin tenure enters in years centered at its mean, $\tilde{E}_e^O = E_e^O - \bar{E}^O$ with $\bar{E}^O \approx 8.573$, and E_e^D indicates any earlier municipal post in the destination, including lower offices. With $A_{ek} = 1\{k = 0\}$ marking the appointment month, we estimate

$$\begin{aligned}Y_{eodt} &= (\beta_E + \gamma_O \tilde{E}_e^O + \gamma_D E_e^D)(\text{Connection}_{eo} \times \text{Post}_{ek}) \\ &\quad + (\theta_0 + \theta_O \tilde{E}_e^O + \theta_D E_e^D)(\text{Connection}_{eo} \times A_{ek}) + \alpha_{eod} + \lambda_{ek} + \nu_{ot} + \varepsilon_{eodt}.\end{aligned}\tag{4}$$

The coefficient β_E measures the advantage at mean origin tenure without destination experience, and γ_O and γ_D , estimated jointly, show how it varies with each type of experience while holding the other constant. The θ terms separate the appointment month, and follow-up is defined as in the arrival analysis.

Event–city-pair FE, α_{eod} , absorb stable trade differences, event-by-relative-month FE, λ_{ek} , absorb shared purchasing changes in the destination, and origin–calendar-month FE, ν_{ot} , absorb supply conditions. Standard errors are clustered by connected origin and destination. Identification requires parallel trends at each experience level.

For Hypothesis 5, we compare connected and unconnected purchases of the same recorded product model in the same month, following studies of prices paid for identical goods in Italy and Russia (Bandiera et al. 2009; Best et al. 2023). Eligible awards by prefectural buyers have identified suppliers and products, positive prices and quantities, and known connection status. Each outcome uses available observations in product–month cells containing both connected and unconnected awards, which limits differences in the goods purchased and their market prices.

For award c , we estimate

$$y_c = \beta_P C_{o(c),d(c),t(c)} + \gamma_Q \log Q_c + \pi_{p(c)} + \alpha_{f(c)} + \mu_{m(c),t(c)} + \lambda_{d(c),t(c)} + \nu_{o(c),t(c)} + \epsilon_c. \quad (5)$$

Outcomes are the log unit price, the acceptance score (0–100), and the delay in days. Firm FE absorb stable supplier differences, product-model–month FE hold goods and purchasing periods constant, and destination–month and origin–month FE absorb geographic conditions. Log quantity and procedure indicators hold purchase volume and procurement method constant, so β_P measures the difference associated with connection status after adjustment. Standard errors are clustered by destination and origin city. Unobserved product specifications, purchasing needs, and other factors affecting both selection and performance must be unrelated to connection status.

Results

Rotation and Public Contract Allocation

Table 2 shows procurement shifting toward leaders' prior work cities after arrival, supporting Hypothesis 1. Panels A–B report asinh value and count. Columns (1)–(2) pool offices, (3)–(4) cover mayors, and (5)–(6) cover party secretaries, with prior economic and fiscal gaps added in even-numbered columns. Connection $\times$ Post is positive for both offices, with and without adjustment ($p < 0.01$).³

Table 2: First Entry and Procurement from Prior Work Cities

	Pooled		Mayors		Party secretaries	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Contract value (asinh)</i>						
Connection $\times$ Post	0.693** (0.060)	0.673** (0.058)	0.722** (0.085)	0.685** (0.083)	0.662** (0.082)	0.663** (0.081)
<i>Panel B. Contract count (asinh)</i>						
Connection $\times$ Post	0.169** (0.015)	0.164** (0.014)	0.179** (0.020)	0.169** (0.019)	0.159** (0.019)	0.160** (0.019)
Control variables	No	Yes	No	Yes	No	Yes
Event $\times$ relative month FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment differencing	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,900,088	8,900,088	4,579,368	4,579,368	4,320,720	4,320,720

Notes: Connection identifies an origin city where an incoming leader previously worked. Comparison origins remain unconnected. Post covers months 1–24 after arrival, with month -1 as the reference. Value is asinh(RMB/10,000) and count is asinh(awards). Economic controls interact Connection $\times$ Post with prior-year GDP, population, revenue, and fiscal-self-reliance gaps, as specified in Table B1. Events and months receive equal weight. Observations are event–origin–months. Standard errors cluster by destination and treated-origin city. * $p < 0.05$, ** $p < 0.01$.

Column (2) of Table B2 implies, in original units, RMB 0.55 million and 0.28 additional contracts per connected origin–destination pair each month, with prior economic and fiscal gaps held at zero, or RMB 6.6 million and 3.4 contracts a year. The value sums the destination's orders from suppliers registered in the connected origin and, because the comparison is relative, includes orders redirected from competing origins. Such redistribu-

³Table B1 reports the economic and fiscal coefficients, and Appendix B provides full and original-unit estimates.

tion toward the leader's prior work city helps explain increased trade between politically connected places (Jiang and Mei 2020).

Figure 2 shows the shift in the most parsimonious arrival model. Joint pre-arrival tests detect no differential trend in pooled value ($p = 0.877$) or count ($p = 0.918$), with similar results for each office. The advantage builds gradually and remains positive through year 2, consistent with agencies incorporating an incoming leader's preferred suppliers into later purchasing decisions.

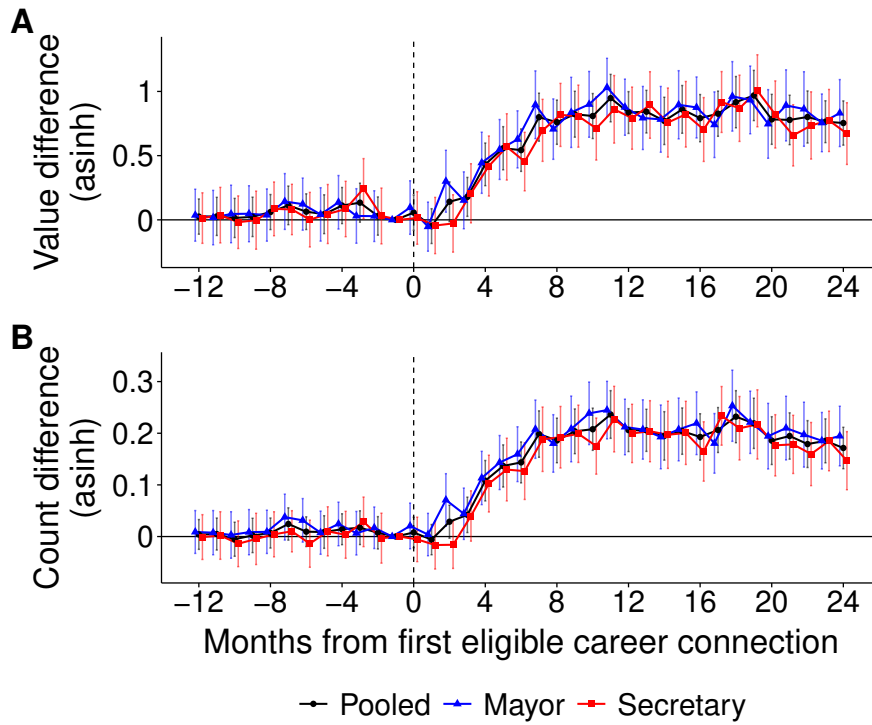


Figure 2: Procurement around Leadership Arrival

Note: Monthly differences between connected and unconnected origins, with equal event weights and no economic controls. Event–relative-month FE and pre-arrival differencing follow Table 2, columns (1), (3), and (5). Value is $\text{asinh}(\text{RMB}/10,000)$, and count is $\text{asinh}(\text{awards})$. Month 0 is the first arrival and month -1 is the reference. Bars show point-wise 95% confidence intervals with standard errors clustered by destination and treated-origin city.

Table D4 shows that connected firms win more often among bidders for the same contract. Column (1) uses all disclosed bids, (2) excludes local firms, and (3) keeps contests with both connected and unconnected bidders. In these mixed contests, connected out-

siders are 36.9 percentage points more likely to win than unconnected outsiders and 33.6 percentage points more likely than local bidders ($p < 0.001$).⁴ This advantage within contracts points to preferential supplier selection behind the geographic shift in Table 2.

Firm Expansion beyond Connected Markets

Table 3 shows greater external business following initial connected awards, supporting Hypothesis 2. Panels A–C report annual asinh value and count and distinct buying cities outside home and career-connected markets, and Award recipient $\times$ Post compares initial winners’ and losers’ later changes. Column (1) pools ties through either or both offices, and (2)–(3) cover ties only through mayors or only through party secretaries.

Panels A–B of Table 3 show positive value and count differences for both offices ($p < 0.01$). In original units, recipients gain 4.5 additional external contracts per 100 firms a year and RMB 56,700 per firm, although the value estimate is imprecise ($p = 0.196$, Table B4). Panel C shows 4.4 additional buying cities per 100 recipients a year. Orders and geographic reach thus provide the clearest evidence of expansion.

Figure 3 shows that the count advantage is largest in years 1–2 and approaches zero in years 3–5. Pre-bid tests detect no divergence in value or count ($p = 0.840$ and $p = 0.542$, Table B5), including capability-specific trends (Table D9). Although fewer cohorts reach later years, the narrowing also appears within the same 2015–2016 cohorts (Table B6).

Table B3 separates external participation, new-city entry, and average award size in columns (1)–(3). Recipients’ annual probability of any external award rises by 3.30 percentage points, and they enter 4.4 additional new cities per 100 firms a year, while their average award in years with an award is RMB 0.238 million smaller ($p = 0.035$). The narrowing advantage concerns additional orders in a given year. Cities already entered remain part of recipients’ market histories, so convergence in new orders can coexist with

⁴We include local firms to assess whether career connections offer an advantage comparable to being registered in the buyer’s city.

Table 3: Firm Expansion beyond Connected Markets

	Pooled (1)	Mayors (2)	Party secretaries (3)
<i>Panel A. Total award value (asinh)</i>			
Award recipient $\times$ Post	0.176** (0.032)	0.207** (0.040)	0.145** (0.046)
Award recipient $\times$ Initial-bid year	0.085* (0.037)	0.139* (0.054)	0.033 (0.057)
<i>Panel B. Award count (asinh)</i>			
Award recipient $\times$ Post	0.035** (0.007)	0.040** (0.009)	0.030** (0.010)
Award recipient $\times$ Initial-bid year	0.015 (0.008)	0.023* (0.011)	0.008 (0.013)
<i>Panel C. Annual purchasing cities served (count)</i>			
Award recipient $\times$ Post	0.044** (0.009)	0.041* (0.017)	0.050** (0.018)
Award recipient $\times$ Initial-bid year	0.016 (0.011)	0.018 (0.020)	0.014 (0.021)
Firm FE	Yes	Yes	Yes
Year FE	No	No	No
Home city $\times$ year FE	Yes	Yes	Yes
Bid cohort $\times$ year FE	Yes	Yes	Yes
Control variables	No	No	No
Observations	164,784	77,272	61,320

Notes: Recipients won their first connected nonlocal bid in 2015–2018. Comparison firms lost their first such bid. All firms participated in bidding in 2013–2014. Post compares subsequent years with pre-bid years. Outcomes exclude home cities and cities connected through officials' careers. Value is $\text{asinh}(\text{RMB}/10,000)$, and count is $\text{asinh}(\text{awards})$. Panel C counts distinct purchasing cities each year. Observations are firm-years, including years without awards. Position-specific samples exclude dual connections. Standard errors cluster by firm home city. * $p < 0.05$, ** $p < 0.01$.

wider commercial reach. Smaller awards and broader entry suggest that experience and delivery records help recipients reach additional buyers through incremental business, consistent with research on demand and buyer relationships (Alfaro-Ureña et al. 2022; Ferraz et al. 2021; Hvide and Meling 2023).

Column (5) of Table C7 compares winners and losers within 270 shared initial contests involving 552 firms, holding the buyer, purchase, and timing of the initial opportunity fixed, as in comparisons of procurement winners and losers in Brazil and Norway (Ferraz et al. 2021; Hvide and Meling 2023). With initial-contest–year FE and equal total weight for winners and losers in each contest, recipients' external value and count remain higher ($p < 0.01$).

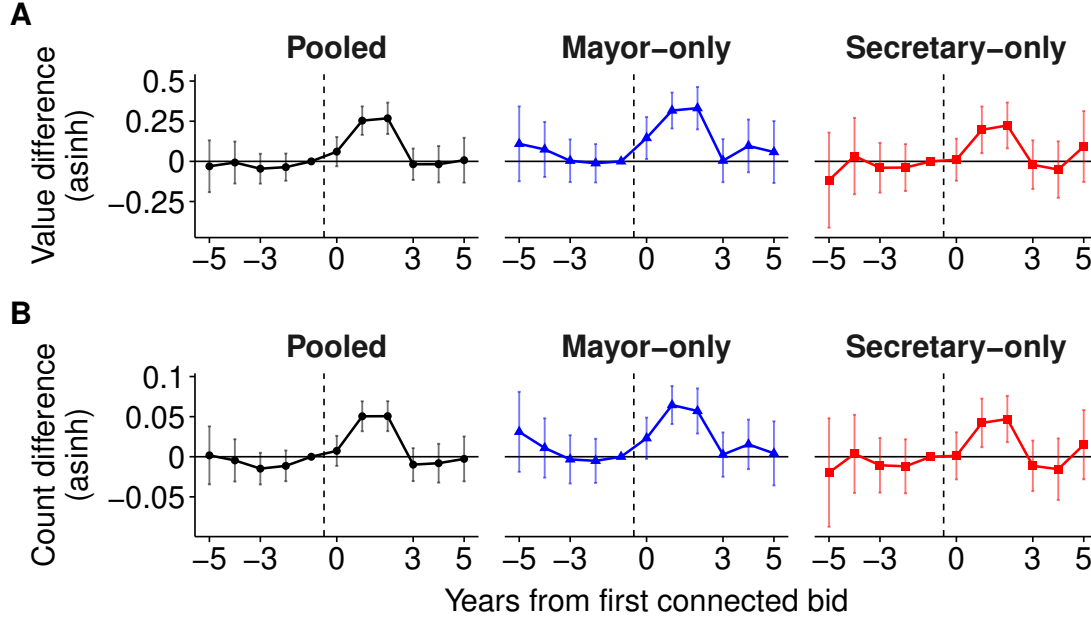


Figure 3: Business in Other Markets around the Initial Connected Bid

Note: Annual differences between recipients and unsuccessful connected bidders in business outside home and career-connected cities. Outcomes, sample restrictions, and FE follow Table 3, Panels A–B, columns (1)–(3). Value is $\text{asinh}(\text{RMB}/10,000)$, and count is $\text{asinh}(\text{awards})$. Year 0 is the initial bid and year -1 is the reference. Later years contain fewer cohorts. Bars show pointwise 95% confidence intervals with standard errors clustered by home city.

Tables D2 and D3 examine whether expansion extends to private firms and whether an initial award matters more for firms with little procurement experience. Private recipients enter five additional new cities per 100 firms a year ($p < 0.001$). The increase is 4.6 among firms with at most two earlier awards and 2.5 among more active firms, but the difference is imprecise (Holm-adjusted $p = 0.489$). Private firms clearly expand, whereas larger benefits for less experienced firms remain suggestive.

Table C8 tests whether expansion depends on capital-city buyers or on heavily represented contests and home cities. Columns (2)–(3) separate initial noncapital and capital buyers, and (4)–(5) give contests or home cities equal weight. Recipients add 4.1 and 5.5 buying cities per 100 firms a year after noncapital and capital bids. Both groups expand geographically, although the capital-group count estimate is less precise. Ordinary municipal buyers thus also support expansion.

Political Authority and Procurement Access

Figure 4 shows procurement advantages contracting after connections end, supporting Hypothesis 3. The pooled asinh value gap falls from 0.858 before the ending to 0.187 in months 1–6 after it, and the count gap from 0.225 to 0.046 (Table B7), declines of 78 per cent and 80 per cent ($p < 0.01$). Both offices show sharp losses of access. A positive gap remains in the early months, consistent with approved commitments and agency relationships carrying forward decisions made under the departing leadership.

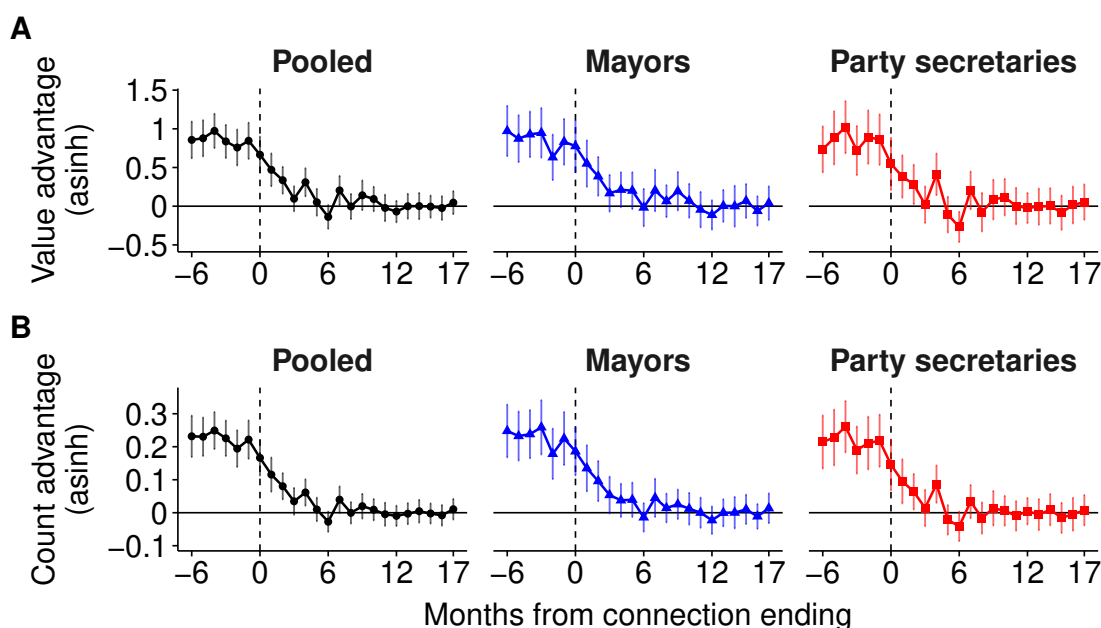


Figure 4: Procurement Advantage around Leadership Departure

Note: Actual minus predicted procurement after a connection ends, estimated with FEct in the R package fect (Liu et al. 2024). The counterfactual uses city-pair and destination-month FE. Six-month averages are in Table B7, columns (1)–(3). Value is $\text{asinh}(\text{RMB}/10,000)$, and count is $\text{asinh}(\text{awards})$. Month 0 is the first unconnected month. Bars show pointwise 95% confidence intervals from destination-city bootstrap resamples with counterfactual predictions held fixed.

Column (1) of Table C10 compares before–after changes in procurement for formerly connected and never-connected origins in original units. In the first six months after the ending, monthly procurement from formerly connected origins falls by RMB 0.626 million and 0.335 contracts relative to never-connected origins, about RMB 3.8 million and 2.0

contracts per origin–destination pair over six months. The arrival and departure analyses use different events, but both show allocations moving with current political authority.

Column (2) of Table C11 assesses later convergence using a baseline fitted without the eighteen post-ending months. Gaps in months 12–17 are -0.001 for value ($p = 0.973$) and approximately zero for count ($p = 0.991$). Continued early purchases and later convergence fit the fulfillment of earlier commitments as successors redirect new allocations, consistent with preferential access that depends on incumbent authority (Blanes i Vidal et al. 2012; Goldman et al. 2013; Shi et al. 2021). External expansion (Table 3) can thus coexist with the loss of purchasing priority in the official’s former jurisdiction.

Career Experience and Information Uncertainty

Table 4 shows stronger access with longer origin tenure and weaker access with prior destination service, supporting Hypothesis 4. Panels A–B report asinh value and count, and $\text{Connection} \times \text{Post}$ interacts with both experience measures (Table B8). Columns (1)–(2) pool offices, (3)–(4) cover mayors, and (5)–(6) cover party secretaries, with destination experience added in even-numbered columns.

Column (2) of Table 4 shows that every five additional years of origin tenure are associated with an advantage larger by 0.108 pre-arrival standard deviations in value and 0.142 in count ($p < 0.01$). Repeated projects show officials how firms perform under different demands and whether they cooperate when difficulties arise, so longer experience can increase confidence in earlier partners, alongside accumulated relationships (Nian and Wang 2023).

Table 4 also shows the opposite association for prior destination service, with advantages smaller by 0.362 standard deviations in value and 0.468 in count ($p < 0.01$). At eight years of origin tenure, predicted advantages are 0.431 and 0.532 standard deviations without destination experience but 0.069 and 0.065 with it. Both offices show these opposing relationships, without detectable experience-specific pretrends (Table B9). The pattern

Table 4: Prior Experience and Allocation Advantage

	Pooled		Mayors		Party secretaries	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Total contract value (asinh)</i>						
Connection $\times$ Post	0.630** (0.023)	0.864** (0.029)	0.652** (0.028)	0.840** (0.033)	0.588** (0.029)	0.877** (0.040)
Connection $\times$ Post $\times$ origin experience	0.043** (0.005)	0.042** (0.004)	0.044** (0.006)	0.042** (0.006)	0.045** (0.006)	0.044** (0.006)
Connection $\times$ Post $\times$ destination experience		-0.707** (0.047)		-0.684** (0.065)		-0.728** (0.065)
<i>Panel B. Contract count (asinh)</i>						
Connection $\times$ Post	0.159** (0.006)	0.221** (0.008)	0.161** (0.007)	0.210** (0.008)	0.149** (0.007)	0.228** (0.010)
Connection $\times$ Post $\times$ origin experience	0.012** (0.001)	0.011** (0.001)	0.012** (0.002)	0.012** (0.002)	0.012** (0.002)	0.012** (0.001)
Connection $\times$ Post $\times$ destination experience		-0.188** (0.012)		-0.178** (0.015)		-0.197** (0.016)
Control variables	No	No	No	No	No	No
Event $\times$ origin $\times$ destination FE	Yes	Yes	Yes	Yes	Yes	Yes
Event $\times$ relative month FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin $\times$ month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,805,588	13,805,588	7,108,255	7,108,255	6,697,333	6,697,333

Notes: Post compares months 1–24 with months –12 to –1. Years of service in the origin are centered at 8.573. Destination service indicates prior work in the destination (0/1). Value is asinh(RMB/10,000), and count is asinh(awards). Appointment-month interactions appear in Table B8. Standard errors cluster by destination and treated-origin city. * $p < 0.05$, ** $p < 0.01$.

supports the information asymmetry mechanism, in which origin tenure builds knowledge of earlier partners and destination service reduces uncertainty about local alternatives. It extends the argument of Hou and Li (2023) about predictable cooperation by showing how geographic knowledge shapes who benefits, and local relationships can reinforce the shift (Bhavnani and Lee 2018; Persson and Zhuravskaya 2016).

Table C13 distinguishes having destination experience from accumulating more years there. Column (2) replaces the indicator with years of earlier destination service, and column (3) includes both. Years of service have a negative association when entered alone, but in the joint model only the indicator remains precisely negative. Arriving with some local knowledge is thus associated with weaker reliance on origin firms, whereas the association with further years of experience is imprecise.

Table D5 tests whether documented earlier cooperation predicts stronger access, separating connected bidders with and without home-city awards during the leader’s earlier

service there. Adjusted columns add prior business and tenure. Both groups win more often than unconnected outsiders ($p < 0.05$), but their difference depends on how awards in the first and last months of service are classified.⁵ Recorded awards document one route to familiarity, which can also arise from unrecorded dealings.

Table D6 asks whether access weakens as officials spend more time in the destination, separating follow-up years in the adjusted arrival model for leaders serving twenty-four months without interruption. If initial uncertainty alone sustained reliance on earlier partners, learning about local alternatives should reduce it. Origin advantages increase from year 1 to year 2 ($p < 0.001$), including when no other appointment occurs. Continued cooperation can renew knowledge of earlier partners and reciprocal commitments, so the experience estimates support an initial information advantage that renewed relationships help sustain.

Government Purchasing Costs

Table 5 shows higher prices, poorer performance, and weaker competition for connected awards, supporting Hypothesis 5. In columns (1)–(3), connected prices are 3.83 per cent higher, scores 0.83 points lower on a 100-point scale, and delays 0.21 days longer ($p < 0.01$, Table B10).

Table B11 converts the premium into additional public expenditure. Goods costing RMB 1 million at comparable unconnected prices cost about RMB 38,300 more at connected prices. The 83,732 connected awards in the price sample average RMB 19.77 billion a year, and removing the common estimated premium from this fixed basket implies RMB 730 million in additional annual expenditure, or RMB 5.84 billion during 2013–2020. The calculation holds goods and quantities fixed and assumes that the conditional price difference applies across connected purchases in the sample. Higher prices absorb

⁵Career histories define the connection. Earlier awards record cooperation with public buyers in the prior work city.

Table 5: Connected Awards and Government Purchasing Costs

Dependent variable Units	Log unit price log(RMB) (1)	Acceptance score 0–100 (2)	Delivery delay Days (3)	Bidders Count (4)	One bidder Indicator (5)
Connection	0.038** (0.001)	−0.829** (0.020)	0.214** (0.032)	−0.150** (0.029)	0.023** (0.006)
Quantity control variable	Yes	Yes	Yes	No	No
Procedure control variables	Yes	Yes	Yes	No	No
Firm FE	Yes	Yes	Yes	Yes	Yes
Product model × month FE	Yes	Yes	Yes	Yes	Yes
Destination × month FE	Yes	Yes	Yes	Yes	Yes
Origin × month FE	Yes	Yes	Yes	Yes	Yes
Observations	488,300	470,772	464,848	488,300	488,300

Notes: Connection identifies suppliers registered in a serving leader’s prior work city. Product-model-month cells contain connected and unconnected awards. Quantity enters in logs. Competitive consultation is the reference procedure. Negative delays indicate early delivery. Bidder counts use disclosed participants. Stars in (4)–(5) use Holm-adjusted p -values. Standard errors cluster by destination and origin city. * $p < 0.05$, ** $p < 0.01$.

purchasing capacity, leaving less for other services. A net welfare calculation would also require monetary values for performance and firms’ costs and profits.

Columns (4)–(5) of Table 5 keep the FE but allow purchase size and procedures to differ, capturing how competition is organized around connected awards. These awards disclose 0.15 fewer bidders and are 2.27 percentage points more likely to have a single bidder (Holm-adjusted $p < 0.001$), consistent with reduced pressure on favored suppliers (Bosio et al. 2022; Szucs 2024). Single bidding is a widely used cross-national indicator of corruption risk in procurement (Fazekas and Kocsis 2020), so weaker competition reinforces the price evidence of potential corruption. Familiar cooperation can improve procurement (Coviello et al. 2018), but measured performance here offers little compensation for higher expenditure (Bandiera et al. 2009).

Tables C14 and C15 test whether product comparability or capital-city purchases explain the cost differences. Adding city-pair FE or restricting goods to office consumables (columns (2)–(3) of the first table) leaves premiums of 3.3 per cent and 5.0 per cent with poorer performance. The second table excludes capital-city buyers in column (2), also excludes capital-city origins in (3), and equalizes buyer weights in (4). Noncapital buy-

ers pay 3.8 per cent more and give scores 0.80 points lower. Also excluding capital-city suppliers leaves a 3.4 per cent premium and scores 0.69 points lower, although the delay difference becomes imprecise. Public costs thus extend to ordinary municipal buyers and readily comparable goods.

Robustness

Table C3 tests whether outcome transformations or economic adjustment drive the arrival advantage. Columns (1)–(3) estimate adjusted advantages in original units, $\log(1 + y)$, and asinh , (4) omits economic controls, and (5) adds origin–calendar-month FE. The advantage remains positive throughout. Table C4 assesses the influence of capital-city buyers and heavily represented destinations, and Table C17 removes local suppliers, who benefit from proximity to the buyer, from the comparison. Allocation and experience estimates change little.

Table C5 asks whether interruptions or later appointments account for the arrival advantage. Estimates remain positive when follow-up ends at an interruption, when connections must last two years, or when later appointments are excluded, although these restrictions select more stable leadership histories. Table C1 tracks whether a tie is active in each month of the 2013–2020 panel, and active ties are associated with greater procurement under city-pair, origin–month, and destination–month FE (Nian and Wang 2023). Appendix C explains these treatment definitions and assumptions.

Table C2 checks whether procurement moves with ties as they begin and end across full connection histories. The intertemporal DiD estimator (de Chaisemartin and D’Haultfœuille 2026) compares pairs whose ties change with origins that retain their initial status. Entry estimates are positive and value declines after exit, but full-history pre-entry tests reject parallel trends ($p < 0.001$), as do tests for persistent histories (Table C5). These comparisons use different histories from the main first-arrival sample.

Table C6 assesses how much the main arrival result depends on parallel trends (Rambachan and Roth 2023). The confidence bounds include zero if, without a connection, the connected–unconnected procurement gap could change after arrival by half the largest pre-arrival change. Insignificant pre-arrival tests therefore offer limited protection against later differences in procurement trends (Roth 2022).

Columns (1)–(2) of Table C7 test outcome scales. Award count rises in original units and $\log(1 + y)$, whereas original-unit value is imprecise. Columns (3)–(4) use firms with known prior award count, market breadth, and age, and (4) interacts these characteristics with year. Value and count estimates decline by 15 per cent and 18 per cent but remain positive, suggesting that prior capabilities explain part of recipients’ growth. Growth also remains positive with equal contest and home-city weights (Table C8), and both checks keep the main FE.

Table C9 follows the same 2015–2016 cohorts with constant weights in years 1–2 and 3–4, so the same firms contribute to both comparisons. Cohort-specific estimates use the estimator of Sun and Abraham (2021), with the main FE in column (1) and home-city–bid-cohort–year FE added in (2). In both, later recipient–loser differences approach zero, showing convergence within these cohorts.

Tables C10 and C11 test whether the departure result depends on how unconnected procurement is estimated. Direct comparisons use alternative scales with event–city-pair and event–relative-month FE, adding origin–month FE in column (4). Counterfactual models keep city-pair and destination–month FE but exclude the eighteen post-ending months from fitting. The contraction persists when follow-up purchases play no role in the predicted baseline.

Table C12 tests whether city conditions or extreme tenure values explain the experience associations. Column (2) adds economic adjustment, and (3) restricts origin tenure to two through fourteen years, both with the main experience FE. Longer origin tenure and prior destination service retain opposing associations.

Table C16 tests whether connected awards differ in reporting the fields needed for the price and performance analyses. Panel A describes availability before outcome restrictions, and Panel B estimates connection differences with destination–month, origin–month, and goods-category–month FE. The differences are small and imprecise, reducing concern that connection status determines which awards enter these analyses.

Alternative Explanations

Table D1 examines whether officials simply prefer dependable state-owned firms (Hou and Li 2023). It interacts connection status with ownership in winning models with firm and contract FE for all bids and mixed contests (columns (1)–(2)), and columns (3)–(4) exclude state-owned participants and use private-only contests. The state-owned–private difference is imprecise, whereas connection advantages persist at 36.9 and 40.0 percentage points ($p < 0.001$). Career access thus extends to private firms, consistent with Table D2.⁶

Panel A of Table D11 assesses whether leaders carry policy priorities that create new demand for goods from their former cities (Lu 2024). It separates purchases announced before the official took office, interacting connection and local status with announcement timing in winning models with firm and contract FE. All awards occur after arrival during uninterrupted connections. Connected advantages are 20.9 percentage points for earlier announcements ($p < 0.01$) and 34.81 percentage points for later ones ($p < 0.001$), and their difference is imprecise ($p = 0.070$). Access in contests announced before arrival reduces concern that newly introduced demand alone explains allocation.

Table D10 assesses whether appointments coincide with economic conditions that affect procurement. Annual appointment models with city and year FE and lagged GDP, population, revenue, and fiscal self-reliance, adding provincial debt in column (2), detect

⁶Registry classifications are recorded at one point in time. Legal forms and foreign-investment indicators define mutually exclusive ownership groups (Appendix D).

no joint association ($p = 0.229$ and $p = 0.207$), reducing concern about selection on measured conditions.

Panel B of Table D11 tests whether connected firms win by offering lower prices. It examines lowest-evaluated-price contests with both connection groups and positive quotations from every bidder, using models of quotation/budget and winning with firm and contract FE. Connected quotations are 1.81 percentage points lower as a share of the contract budget ($p < 0.001$).⁷ Lower offers help explain these wins, and because budgets differ, lower bids within a contest can coexist with higher unit prices across comparable purchases.

Columns (4)–(5) of Table D4 ask whether connected advantages depend on evaluation rules or procedures, interacting connection and local status with comprehensive scoring and open tendering, with firm and contract FE and with lowest-evaluated-price selection and other competitive procedures as references. Advantages persist across rules, with imprecise interactions (open tendering $p = 0.530$), consistent with eligibility and evaluation criteria accommodating preferences within formal competition (Lu and Wang 2023; Romero 2025).

Table D12 tests whether favoritism concentrates among leaders receiving a promotion (Jiang 2018). Adjusted arrival models that distinguish appointments with and without rank advancement show positive advantages in both groups for each office, with imprecise within-office differences, so preferential access also accompanies appointments without promotion.⁸

Tables D7 and D8 test whether hometown affinity explains career-based access (Hodler and Raschky 2014; Persson and Zhuravskaya 2016; Xu et al. 2023). The first excludes

⁷Arithmetic corrections and policy deductions can change evaluated prices (Ministry of Finance 2017, art. 54). Here every winner submits the unique lowest nominal quotation.

⁸Jiang (2018) identifies the provincial secretary serving when a city leader was first promoted. Our measure records rank advancement at the municipal appointment.

origins matching municipal leaders' hometowns and estimates the difference associated with hometown overlap, and the second excludes appointees sharing a hometown with a serving provincial secretary or governor and tests that difference. Advantages change little and the interactions are imprecise, supporting the role of prior work relationships beyond native-place ties.

Conclusion

Rotation can disrupt local alliances while carrying officials' familiar partners into new jurisdictions. This challenges the expectation that rotation limits favoritism by separating officials from accumulated relationships (Abbink 2004). Our theory explains how ordinary cooperation creates knowledge that remains useful after reassignment. Officials uncertain about local alternatives favor dependable earlier partners and use their authority to make familiarity matter for allocation. Rotation can therefore extend preferential access, while the resources favored firms acquire support commercial expansion beyond the official's jurisdiction.

Using career histories, about a million goods contracts, bids, and firm records for 335 Chinese cities in 2013–2020, we find support for all five hypotheses. Access rises after appointment and declines after connections end. Initial recipients expand externally while their advantage in new contracts narrows. Longer origin tenure strengthens access, and prior destination service weakens it. Connected purchases have higher prices, poorer performance, and weaker competition, and the price premium implies RMB 730 million in additional annual expenditure in the connected price sample.

These findings show that personnel institutions have distributive consequences for state–business relations. Extending research on mobile connections (Nian and Wang 2023), our theory explains how knowledge acquired in different places shapes who benefits. The opposing experience associations support the information asymmetry mech-

anism, while the growth of access during uninterrupted service suggests that continued cooperation and reciprocal commitments keep earlier partners attractive. Rotation-based favoritism thus rests on both initial uncertainty and relationships renewed under current authority.

Combining firms' career connections, bids, and later awards lets us follow political access from selection to external expansion (Ferraz et al. 2021). Following recipients outside career-connected cities shows how wider market reach coexists with a narrowing flow of additional orders, and comparing purchases of the same product models sets this expansion against wasteful public spending (Bandiera et al. 2009). Monetizing quality and delivery and measuring firms' production costs would turn the expenditure benchmark into a fuller welfare assessment.

The argument should travel wherever officials move, influence allocations, and can work with earlier partners across jurisdictions, and it predicts stronger favoritism when local capabilities are difficult to assess. Politically driven transfers of Indian civil servants and Kenyan provincial administrators (Hassan 2017; Iyer and Mani 2012) offer natural settings for testing it. Public credit and business licensing provide further tests, given connections' documented influence on lending and on entry into sectors that require authorization (Haselmann et al. 2018; Khwaja and Mian 2005; Rijkers et al. 2017). Following credit administrators or licensing officials across postings could reveal whether earlier clients gain access, whether local experience weakens it, and whether recipients expand elsewhere. Because commercial expansion requires reusable resources, experience, or credentials, tight selection rules and benefits confined to one location should limit these effects.

Can credible information about local firms reduce incoming officials' reliance on earlier partners? Randomly providing standardized performance records at appointment would test this implication, and records of direct cooperation could clarify how knowledge and reciprocal obligations evolve. These tests matter because rotation changes both authority and the information available to decision makers. The distributive consequences

of personnel reform depend on how governments manage the relationships and knowledge officials carry into public resource allocation.

AI Usage Statement

The author used artificial intelligence (AI) through the OpenAI Codex application (GPT-6 and GPT-6.1), available at <https://openai.com/codex/>, in October 2026 to assist with writing and checking statistical-analysis code, manuscript drafting and language revision, literature searches, reference checks, and L^AT_EX preparation. The author reviewed the AI-assisted code and outputs, takes full responsibility for the manuscript's scientific content, and confirms that the paper adheres to the journal's authorship policy.

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Competing Interests

The author declares none.

Ethical Standards

The study involved no recruitment or experimental intervention with human participants.

Data Availability Statement

The data, analysis code, codebook, replication instructions, and statistical outputs will be deposited in the journal's Dataverse upon acceptance.

Supplementary Material

The accompanying appendix contains data documentation, full estimates, robustness checks, and tests of alternative explanations.

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Moving Officials, Moving Access:

The Distributive Effects of Bureaucratic Rotation

Supplemental Information

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Appendix A Data and Measurement

Monetary values use renminbi (RMB), and economic measures include gross domestic product (GDP). Descriptive tables report standard deviations (SD) alongside means and ranges. Procurement totals use the inverse hyperbolic sine (asinh) transformation.

(a) Procurement search portal



中国政府采购网

中国政府购买服务信息平台

www.ccgp.gov.cn

首页

政采法规

购买服务

监督检查

信息公告

GPA专栏

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政策法规

搜索平台 采购公告

搜标题

搜全文

类型

所有类型

公开招标

询价公告

竞争性谈判

单一来源

资格预审

邀请公告

中标公告

更正公告

其他公告

竞争性磋商

成交公告

终止公告

▼ 更多项目

类别

所有类别

中央公告

地方公告

品目

所有品目

货物类

工程类

服务类

时间

指定时间

2022-02-15

2022-02-22

以

查询

取消

近3月

近半年

查询2013年之前的公告信息，请点击这里。

采购人

项目编号

地区

代理机构

Figure A1: Examples of Procurement and Leadership Records

Note: The China Government Procurement Platform’s search interface displays notice type, procurement category, dates, buyer, region, and agency. The screenshot illustrates the record format. Its search dates fall outside the study period.

(b) Procurement award notice

福建省亿达工程咨询有限公司关于三明市梅列区列西小学多媒体系统设备采购中标公告

2013年04月23日 16:14 来源: 中国政府采购网 【打印】

1、采购项目编号: FJYD2013-SM1021#

2、采购项目名称: 多媒体系统设备采购 → **Project Name**

3、采购单位名称: 三明市梅列区列西小学 → **Buyer**

采购单位地址: 福建省三明市梅列区列西富华新村12幢 → **Buyer Address**

联系人: 郑先生 联系方式: 0598-8231596

4、招标代理机构名称: 福建省亿达工程咨询有限公司 → **Agency Name**

招标代理机构地址: 三明市梅列区梅岭新村51幢102室

联系人: 刘小姐、吴小姐 联系方式: 0598-8221299

5、公开招标公告发布日期: 2013年4月1日

6、发布媒介名称: **Bidding Price**

三明市政府采购网 (<http://www.smzfcg.gov.cn/>)

三明市公共资源交易中心网 (<http://www.smggzy.com/>)

Provider Name

7、开标地点: 三明市公共资源交易中心第三交易厅

8、开标时间: 2012年4月22日下午15:00 (北京时间) → **Bidding date** **Provider Address**

9、中标情况: 经评标委员会评审后确定三明市梅列区列西小学多媒体系统设备采购项目中标供应商为福建恩特计算机网络工程有限公司, 具体情况如下:

项目名称	中标数量	简要技术指标	采购单位委托价(万元)	中标金额(万元)	节约金额(万元)	节约率	中标单位	地址	合同履行日期
多媒体系统设备采购	1批	详见第三章招标内容及要求	10.56	10.448	0.112	1.06%	福建恩特计算机网络工程有限公司	三明市三明饭店综合楼二楼	按业主要求

10、评审专家名单: 林向东、黄合根、罗建平、杨振毅、肖守伦

投标供应商对中标有异议的, 请在中标公告发布之日起七个工作日内, 以书面的形式向招标代理机构或采购单位提出质疑。

特此公告。

Figure A1: Examples of Procurement and Leadership Records (continued)

Note: The original notice identifies the project, buyer, agency, supplier, addresses, award amount, and dates. English labels identify these fields. Dates are reproduced as printed in the source, including its opening-date discrepancy.

(c) Official biography

王智慧 简历



王智慧，男，汉族，1968年8月生，河南商人，中共党员，清华大学人文社会科学学院科学技术哲学专业，研究生文化程度，哲学硕士学位。

1986.09—1990.07山东大学科社系科学社会主义专业学习
1990.07—1991.08河南省委政策研究室干部（其间：
1990.08—1991.08在河南省孟县谷旦乡锻炼）
1991.08—1998.05历任河南省委政策研究室政治体制改革处科员、社会处副主任干事、综合处主任干事
1998.05—2001.05历任河南省委政策研究室综合处助理调研员、科教党建处副处长
2001.05—2006.09河南省委防范和处理邪教问题领导小组办公室（省政府防范和处理邪教问题办公室）调研处处长（其间：
2002.03—2002.06在河南省委党校第25期处长进修班学习；
2000.09—2002.07在清华大学人文社会科学学院科学技术哲学专业学习，获哲学硕士学位；
2004.06—2004.12赴美国参加高级管理人才培训班学习；
2005.09—2006.01在河南省委党校第37期青干班学习；
2002.11—2004.11挂职任邓州市委副书记、副市长）
2006.09—2009.02河南省委政法委员会副巡视员
2009.02—2010.06河南省委防范和处理邪教问题领导小组办公室（省政府防范和处理邪教问题办公室）专职副主任（副厅级）
2010.06—2011.08河南省委防范和处理邪教问题领导小组办公室（省政府防范和处理邪教问题办公室）专职副主任（副厅级）兼省委宣传部副部长
2011.08—2014.02河南省南阳市常委、政法委书记
2014.02—2014.03河南省南阳市常委、政法委书记、市纪委书记
2014.03—2015.11河南省南阳市常委、市纪委书记
2015.11—2016.10河南省南阳市委副书记
2016.10—2017.10河南省南阳市委副书记、统战部部长
2017.10—2018.11河南省南阳市委副书记
2018.11—2021.06河南省委全面深化改革委员会办公室主任
2021.06—2021.07河南省南阳市委副书记、提名为市人民政府市长候选人
2021.07—河南省南阳市委副书记，市人民政府代市长、市长、党组书记

（简历来源：南阳政府网站）

B: provincial Govt

A: Local promotion to mayor from a lower position

Figure A1: Examples of Procurement and Leadership Records (continued)

Note: Wang Zhihui's biography from the Nanyang government website shows dated provincial and municipal posts. The original includes later appointments and illustrates how careers are coded. The analysis uses service histories within 2013–2020.

Table A1: Measurement Definitions and Analysis Samples

Measure	Operationalization/Proxy	Example
Supplier origin and market	Registered city identifies supplier origin. The purchasing city identifies its procurement market. Firm identifiers link bids and awards. Operations may span cities.	An A-registered supplier wins in B: origin A, market B.
Career connection	Earlier service in origin $o \neq d$ by either mayor or Party secretary currently in destination d gives $C_{odt} = 1$. All earlier city postings count. Municipal position changes preserve ties.	Prior service in A and B, current office in C: both A–C and B–C connected.
Prior local procurement	Supplier has an origin-city award during the serving leader’s prior source service. Histories are frozen before arrival and fully observed. Firms predate service. Interior-month checks exclude service boundaries.	Recorded earlier local award: prior supplier = 1.
Connection timing	First activation starts entry. Connections end when neither leader maintains them. Reconnection never resets entry.	Arrival month: $k = 0$. Next month: $k = 1$.
Appointments	Position counts exclude initial incumbents and post-gap spells. Annual indicator records any newly observed appointment after analysis begins.	Two position appointments. Annual indicator = 1.
Procurement value and count	Value totals RMB amounts. Count totals awards. Baseline outcomes are $\text{asinh}(V/10,000)$ and $\text{asinh}(N)$, including zeros.	No awards: both outcomes = 0.
External business and reach	Exclude home and all cities connected at any time in 2013–2020. Count distinct annual purchasing cities and cities without earlier recorded supplier awards. Any award is binary. Mean award value is V/N in positive-award years.	A later-connected C remains excluded from A’s external business.
Price and delivery	Unit price is RMB per quantity unit and enters in logs. Acceptance scores range 0–100. Delay is actual minus scheduled days.	Delay = -2 : delivery two days early.
Bidding and competition	Bids identify firm–contract pairs. Winning = 1 for recipients and = 0 for unsuccessful participants. Single-bidder awards disclose one participant. Winner-only notices list only the recipient.	One disclosed participant: single bidder = 1.

Continued on next page

Table A1: Measurement Definitions and Analysis Samples (continued)

Measure	Operationalization/Proxy	Example
Announcement timing	New announcement equals 1 after the arrival month and 0 before. Arrival-month notices are excluded. Sample restrictions follow Table D11.	Notice after arrival month: new announcement = 1.
Bid quotation	Nominal RMB quotation divided by the contract budget. Lowest-price checks require positive quotes from every bidder.	Quote/budget = 90/100 = 0.9.
Purchasing controls	Log quantity and procedure indicators. Local bidders share the buyer city. Scoring equals 1 for comprehensive scoring and 0 for lowest-evaluated-price selection.	Buyer and firm share a city: local bidder = 1.
Economic and fiscal controls	Prior-year $\log(1 + \text{GDP})$, $\log(1 + \text{Population})$, $\log(1 + \text{Revenue})$, and revenue/expenditure. Arrival gaps: connected city minus the mean of unconnected cities supplying the same destination. Appointment models also use $\log(1 + \text{Provincial debt})$.	Revenue/expenditure = 50/100 = 0.5.
Career experience	Origin years centered at 8.573. Prior destination municipal service: indicator or total years (zero if none). Measures fixed at entry.	Two prior destination years: indicator = 1, duration = 2.
Prior firm capabilities	2013–2014 count and value enter as centered $\log(1 + N)$ and $\log(1 + V/10,000)$. Prior markets count distinct purchasing cities. Firm age is measured the year before the initial bid.	Earlier awards in one city: one prior market.
Native-place ties	City-identifiable hometowns define origin overlap and ties to provincial leaders. Provincial ties require all three hometowns known.	All hometowns known. Leader–governor overlap: tie = 1.
Arrival sample	Complete months –12 to 24. Controls remain unconnected. Exclude initially active and connections with gaps in recorded coverage. Follow-up continues after departure.	Departure in year one retains year-two follow-up.

Continued on next page

Table A1: Measurement Definitions and Analysis Samples (continued)

Measure	Operationalization/Proxy	Example
Departure sample	Main departure comparisons require six preceding months of procurement observations and eighteen consecutive unconnected months beginning at month 0. Direct before–after checks also require a connection throughout the six preceding months.	Reconnection within 18 months: ending excluded.
Firm panel	Initial connected nonlocal bids in 2015–2018 after participation in 2013–2014. Annual follow-up: 2013–2020, including zero-award years.	No award in a year: value and count = 0.
Purchasing sample	Goods require firm/model identification, positive price/quantity, known connection, prefectural-tier buyers, and complete item details and outcomes. Model–month cells require both groups. Separate brands and units are unavailable.	Only connected awards in a model–month cell: excluded.

Notes: Monetary controls use RMB 100 million. Population uses 10,000 persons. Capital excludes directly administered municipalities.

Table A2: Leadership, City Conditions, and Firm Outcomes

Variable	Observations	Mean	SD	Minimum	Maximum
<i>Experience at entry</i>					
Origin service (years)	1,142	8.573	5.397	0.5	23
Destination service (0/1)	1,142	0.327	0.469	0	1
Destination service (years)	1,142	1.109	1.865	0	6
<i>City conditions and appointments</i>					
GDP (RMB billion)	2,678	243.7	370.8	2.89	3,870
Population (million)	2,659	4.11	3.282	0.103	34.16
Revenue (RMB billion)	2,679	23.13	53.835	0.086	716.5
Fiscal self-reliance	2,679	0.412	0.247	0.013	5.082
Provincial debt (RMB billion)	220	581.4	336.6	3.1	1,723
New appointment (0/1)	2,345	0.413	0.493	0	1
<i>Native-place indicators</i>					
Origin–hometown match	1,083	0.096	0.295	0	1
Provincial hometown tie	951	0.025	0.157	0	1
<i>Monthly connected-origin procurement</i>					
Contract count	41,995	0.397	1.033	0	45
Value (RMB million)	41,995	0.754	3.604	0	205.5
Active connection (0/1)	41,995	0.601	0.490	0	1
<i>Arrival-event controls and weights</i>					
$\Delta \log(1 + \text{GDP})$	1,135	0.070	1.021	-3.514	2.889
$\Delta \log(1 + \text{Population})$	1,135	0.032	0.805	-3.269	1.583
$\Delta \log(1 + \text{Revenue})$	1,135	0.06	1.093	-3.805	3.521
Fiscal self-reliance gap	1,135	0.007	0.237	-0.417	1.09
Prior 12-month buyer awards	1,135	369.6	494.9	3	3,028
<i>Initial bidders and prior capabilities</i>					
Award recipient (0/1)	20,598	0.581	0.493	0	1
Prior award count	20,598	2.882	4.423	0	121
Prior value (RMB million)	20,598	4.181	9.716	0	261.4
Prior markets served	20,598	1.554	1.973	0	62
Firm age (years)	20,278	7.89	5.915	0	65
<i>Annual external-market outcomes</i>					
Contract count	164,784	0.423	1.082	0	88
Value (RMB million)	164,784	0.760	3.625	0	177
Purchasing cities served	164,784	0.392	0.955	0	53
Any award (0/1)	164,784	0.253	0.435	0	1
New purchasing cities	144,186	0.309	0.763	0	20
Mean award value (RMB million)	41,737	1.871	4.631	0.001	154.9

Notes: Observations count nonmissing values. City conditions come from the annual socioeconomic panel, debt is measured annually for each province, and arrival characteristics are measured for each event. Monthly purchases cover months –12 to 24. Firm characteristics precede the initial bid. Annual outcomes include years without awards. New-market measures begin in 2014, and mean award value uses years with positive awards. Hometown indicators require known hometowns.

Table A3: Purchasing and Bidding Characteristics

Variable	Observations	Mean	SD	Minimum	Maximum
<i>Recorded goods awards</i>					
Award value (RMB million)	962,756	1.813	4.88	2.6×10^{-4}	712.3
Career connection (0/1)	962,756	0.105	0.306	0	1
Log unit price	925,119	7.576	2.955	0.438	13.784
Log quantity	925,119	5.576	3.174	0	16.257
Disclosed bidders	962,756	3.792	2.402	1	31
Single bidder (0/1)	962,756	0.145	0.353	0	1
<i>Delivery outcomes</i>					
Acceptance score	938,168	94.398	3.056	63	100
Delivery delay (days)	928,777	1.346	4.333	-4	46
<i>Disclosed bids</i>					
Winning bid (0/1)	3,650,350	0.264	0.441	0	1
Career connection (0/1)	3,650,350	0.043	0.203	0	1
Local bidder (0/1)	3,650,350	0.516	0.500	0	1
Scoring rule (0/1)	3,650,350	0.464	0.499	0	1
<i>Targeted bidding checks</i>					
New announcement (0/1)	103,157	0.995	0.069	0	1
Quote/budget	296,246	0.872	0.076	0.519	1
Lowest raw quote (0/1)	296,246	0.215	0.411	0	1
<i>Record completeness</i>					
Item data available (0/1)	962,756	0.947	0.225	0	1
Both outcome fields (0/1)	962,756	0.940	0.237	0	1
<i>Procurement-procedure indicators (0/1)</i>					
Open tender	962,756	0.600	0.49	0	1
Competitive negotiation	962,756	0.141	0.348	0	1
Price inquiry	962,756	0.111	0.315	0	1
Competitive consultation	962,756	0.106	0.308	0	1
Single source	962,756	0.035	0.184	0	1
Invitation tender	962,756	0.006	0.077	0	1

Notes: Counts cover awards or disclosed bids with nonmissing values before regression restrictions, except the targeted checks, which use the estimation samples in Table D11. Prices are in RMB per recorded quantity unit before taking logs. Higher acceptance scores indicate better performance. Negative delays indicate early delivery. Procedure and completeness indicators are binary.

The goods awards span 15 recorded product categories. Specialized equipment accounts for the largest shares of contracts and value (Table A4), making detailed product comparisons important for the price analysis.

Table A4: Goods Categories in Municipal Procurement

Goods category	Awards	Awards (%)	Value (%)
Specialized equipment	369,594	38.39	48.54
Other goods	131,831	13.69	11.38
General equipment	74,522	7.74	10.64
General-purpose equipment	102,537	10.65	7.56
Office consumables	84,731	8.80	4.94
Pharmaceuticals	26,662	2.77	3.86
Other classified goods	37,569	3.90	3.81
Agricultural products	27,880	2.90	2.18
Furniture	23,665	2.46	1.39
Vehicles	16,963	1.76	1.29
General materials	12,302	1.28	1.06
Books and archives	13,615	1.41	1.05
Food and beverages	11,813	1.23	0.96
Specialized materials	18,655	1.94	0.80
Related office supplies	10,417	1.08	0.55
Total	962,756	100.00	100.00

Notes: All 962,756 goods awards are classified once using the recorded product category. Value shares use final award amounts in RMB.

The firm registry covers 235,169 firms, and procurement records identify 122,499 purchasers. Their locations are standardized using the same 335-city dictionary.

Of 144,400 firms with disclosed bids, 64,907 have a recorded connected nonlocal bid. Of these, 32,509 make their first such bid during 2015–2018. Among them, 20,598 also bid during 2013–2014, providing the earlier observations needed to compare growth before and after the initial decision. The panel contains 164,784 firm-years, including 123,047 with no award outside home and career-connected cities. Firms without a 2013–2014 award receive zero prior procurement activity. The capability-adjusted comparison uses 20,278 firms with known registration years, estimating baseline and adjusted models on this same sample (Table C7). Models without age adjustment retain all 20,598 firms.

Table A1 defines eligibility and measurement for each empirical design. Arrival comparisons use the 1,135 events in the 1,142-event core frame with the required economic and fiscal measures for the year before appointment (Table A2). Each purchasing-performance model includes awards with recorded values for the outcome being estimated.

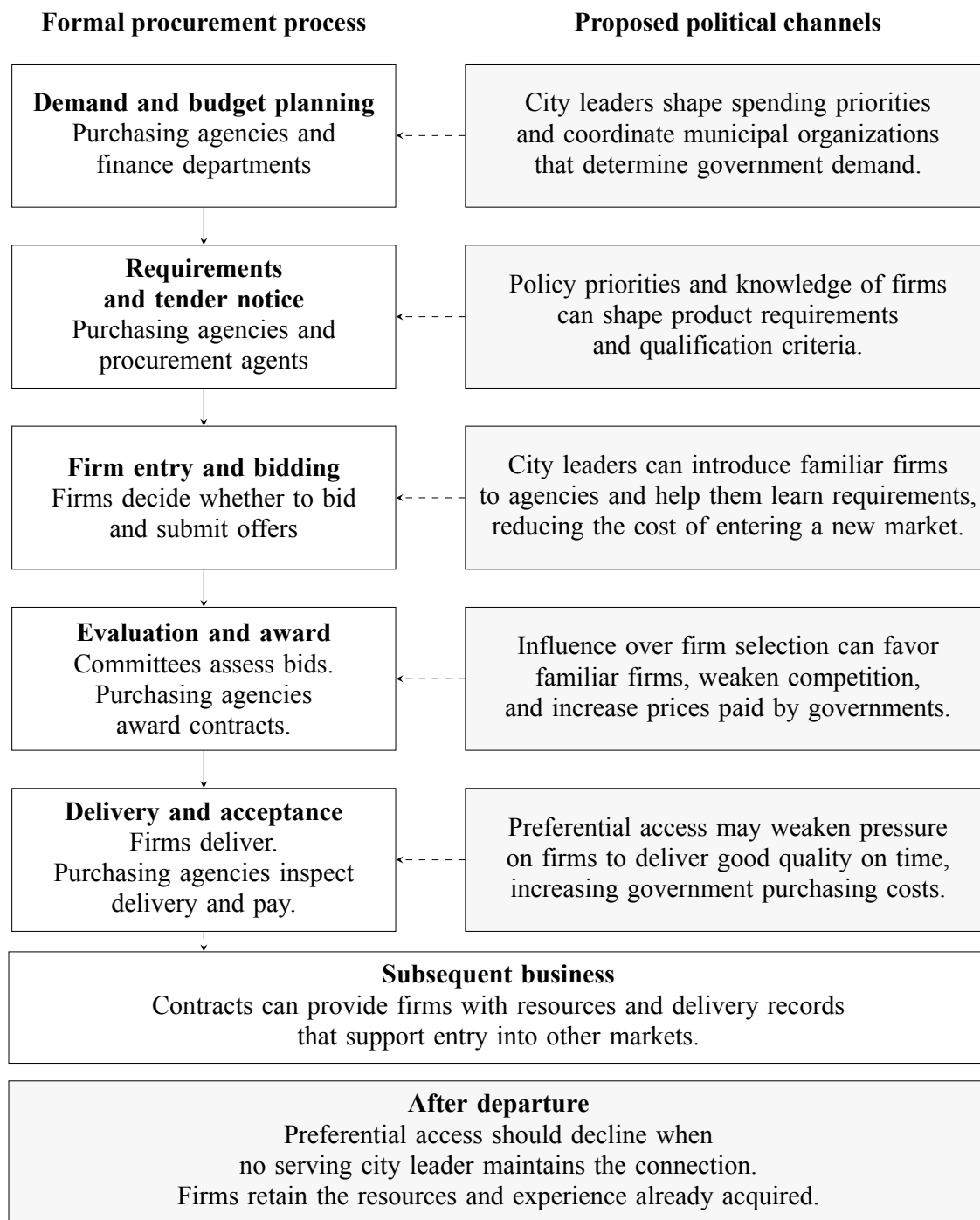


Figure A2: City Leaders and the Government Procurement Process

Note: Solid arrows show the formal procurement sequence. Dashed arrows show the proposed political channels and commercial consequences.

Figures A3 and A4 show 1,885 within-province links in blue and 334 cross-province links in red, counting each directed city pair once.

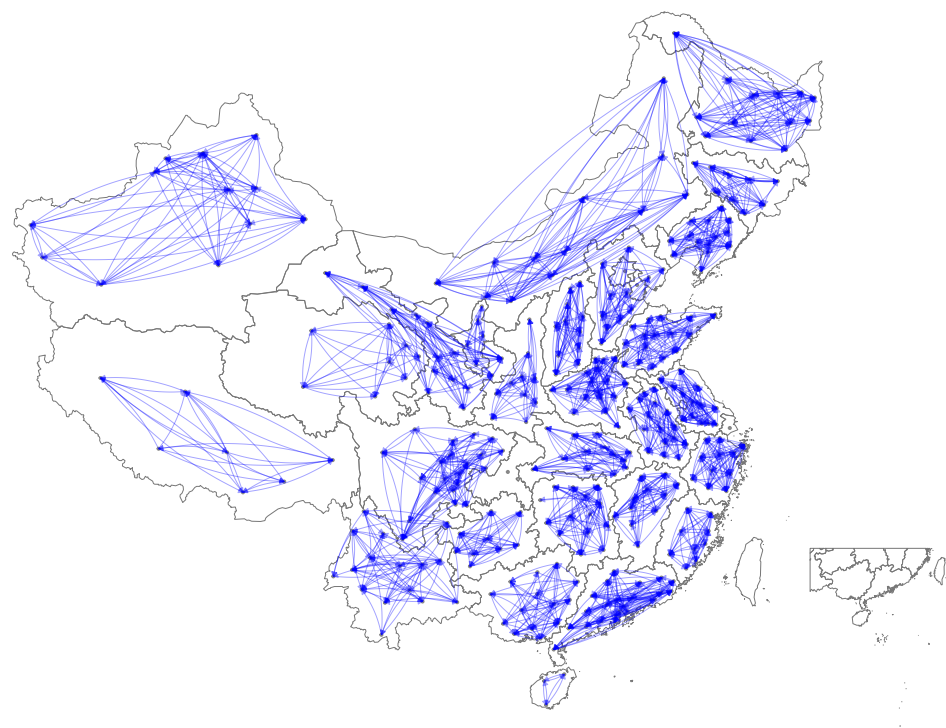


Figure A3: Prior-Work-City Links within Provinces

Notes: Arrows run from prior work cities to subsequent leadership cities. Only links whose endpoints are mapped are shown. Repeated links are counted once.



Figure A4: Prior-Work-City Links across Provinces

Notes: Arrows run from prior work cities to subsequent leadership cities. Only links whose endpoints are mapped are shown. Repeated links are counted once.

Appendix B Supplementary Estimates

The supplementary models use the fixed effects (FE) defined in the main manuscript. The arrival and firm-expansion estimates implement difference-in-differences (DiD) comparisons, and the departure estimates use the fixed-effects counterfactual estimator (FEct).

Table B1: First-Entry Estimates with Economic and Fiscal Control Variables

	Pooled		Mayors		Party secretaries	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Award value (asinh)</i>						
Connection $\times$ Post	0.693** (0.060)	0.673** (0.058)	0.722** (0.085)	0.685** (0.083)	0.662** (0.082)	0.663** (0.081)
Connection $\times$ Post $\times$ log(1 + GDP) gap		0.361 (0.251)		0.384 (0.340)		0.308 (0.297)
Connection $\times$ Post $\times$ log(1 + Population) gap		−0.195 (0.181)		−0.374 (0.209)		−0.010 (0.242)
Connection $\times$ Post $\times$ log(1 + Revenue) gap		0.094 (0.267)		0.229 (0.323)		−0.029 (0.419)
Connection $\times$ Post $\times$ fiscal self-reliance gap		−0.770 (0.785)		−1.226 (0.977)		−0.277 (1.212)
<i>Award count (asinh)</i>						
Connection $\times$ Post	0.169** (0.015)	0.164** (0.014)	0.179** (0.020)	0.169** (0.019)	0.159** (0.019)	0.160** (0.019)
Connection $\times$ Post $\times$ log(1 + GDP) gap		0.093 (0.057)		0.121 (0.074)		0.050 (0.070)
Connection $\times$ Post $\times$ log(1 + Population) gap		−0.052 (0.044)		−0.113* (0.051)		0.012 (0.058)
Connection $\times$ Post $\times$ log(1 + Revenue) gap		0.026 (0.058)		0.053 (0.068)		0.008 (0.092)
Connection $\times$ Post $\times$ fiscal self-reliance gap		−0.196 (0.194)		−0.370 (0.257)		−0.002 (0.270)
Control variables	No	Yes	No	Yes	No	Yes
Event $\times$ relative month FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment differencing	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,900,088	8,900,088	4,579,368	4,579,368	4,320,720	4,320,720

Notes: Outcomes, timing, and weights follow Table 2. Prior-year gaps subtract the means of unconnected cities supplying the same destination from connected-city values for log(1+GDP), log(1+population), log(1+revenue), and revenue/expenditure. Adjusted connection effects are evaluated at zero gaps. Standard errors cluster by destination and treated-origin city. * $p < 0.05$, ** $p < 0.01$.

Table B2: First-Entry Estimates in Original Procurement Units

	Pooled		Mayors		Party secretaries	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Award value (RMB million)</i>						
Connection $\times$ Post	0.567** (0.075)	0.550** (0.074)	0.507** (0.113)	0.475** (0.106)	0.631** (0.088)	0.633** (0.089)
<i>Award count (count)</i>						
Connection $\times$ Post	0.289** (0.026)	0.280** (0.025)	0.295** (0.034)	0.277** (0.031)	0.282** (0.033)	0.285** (0.032)
Control variables	No	Yes	No	Yes	No	Yes
Event $\times$ relative month FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment differencing	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,900,088	8,900,088	4,579,368	4,579,368	4,320,720	4,320,720

Notes: Timing, control gaps, and weights follow Table B1. Control-variable coefficients are omitted. Standard errors in parentheses cluster by destination and treated-origin city. * $p < 0.05$, ** $p < 0.01$.

Table B3: Firm Business Gains beyond Connected Markets

Dependent variable	Any award	New markets	Average award value
Units	Indicator	Count	RMB million
	(1)	(2)	(3)
Award recipient $\times$ Post	0.033** (0.006)	0.044** (0.008)	−0.238* (0.112)
Award recipient $\times$ Initial-bid year	0.016* (0.007)	0.008 (0.010)	0.001 (0.180)
Control variables	No	No	No
Firm FE	Yes	Yes	Yes
Home city $\times$ year FE	Yes	Yes	Yes
Bid cohort $\times$ year FE	Yes	Yes	Yes
Observations	164,784	144,186	41,737

Notes: Sample and treatment definitions follow Table 3. New markets have no earlier award to the firm. Column (2) begins in 2014. Column (3) retains positive-award years. Standard errors in parentheses cluster by firm home city. * $p < 0.05$, ** $p < 0.01$.

Table B4: Firm Expansion in Procurement Units

	Pooled (1)	Mayors (2)	Party secretaries (3)
<i>Total award value (RMB million)</i>			
Award recipient $\times$ Post	0.057 (0.044)	0.050 (0.062)	0.080 (0.068)
Award recipient $\times$ Initial-bid year	0.042 (0.052)	0.120 (0.074)	−0.009 (0.078)
<i>Award count</i>			
Award recipient $\times$ Post	0.045** (0.010)	0.041* (0.020)	0.051* (0.021)
Award recipient $\times$ Initial-bid year	0.012 (0.013)	0.016 (0.023)	0.012 (0.025)
<i>Markets served</i>			
Award recipient $\times$ Post	0.044** (0.009)	0.041* (0.017)	0.050** (0.018)
Award recipient $\times$ Initial-bid year	0.016 (0.011)	0.018 (0.020)	0.014 (0.021)
Firm FE	Yes	Yes	Yes
Home city $\times$ year FE	Yes	Yes	Yes
Bid cohort $\times$ year FE	Yes	Yes	Yes
Control variables	No	No	No
Observations	164,784	77,272	61,320

Notes: Sample and treatment definitions follow Table 3. Standard errors in parentheses cluster by firm home city. * $p < 0.05$, ** $p < 0.01$.

Table B5: Joint Pre-Bid Tests for Firm Expansion

	All city leaders	Mayors	Party secretaries	Both positions
Award value (asinh) joint pre-bid p	0.840	0.769	0.768	0.707
Award count (asinh) joint pre-bid p	0.542	0.634	0.832	0.652
Control variables	No	No	No	No
Firm FE	Yes	Yes	Yes	Yes
Home city $\times$ year FE	Yes	Yes	Yes	Yes
Bid cohort $\times$ year FE	Yes	Yes	Yes	Yes
Observations	164,784	77,272	61,320	26,192

Notes: Sample and outcomes follow Table 3. Recipient-by-year interactions replace the two recipient-period interactions. Joint tests cover years −5 to −2, with year −1 as the reference. All cohorts contribute through year 2. Only earlier cohorts contribute to years 3–5. Standard errors cluster by firm home city.

Table B6: Business Differences over Common Early and Late Follow-Up

	All city leaders	Mayors	Party secretaries
<i>Award value (asinh)</i>			
Mean difference, years 1–2	0.212	0.298	0.184
Mean difference, years 3–4	-0.035	0.000	0.013
Late minus early difference	-0.247	-0.297	-0.172
95% confidence interval	[-0.334, -0.160]	[-0.417, -0.178]	[-0.332, -0.012]
<i>Award count (asinh)</i>			
Mean difference, years 1–2	0.044	0.061	0.050
Mean difference, years 3–4	-0.010	0.003	0.002
Late minus early difference	-0.054	-0.059	-0.048
95% confidence interval	[-0.071, -0.037]	[-0.080, -0.038]	[-0.084, -0.012]
Control variables	No	No	No
Firm FE	Yes	Yes	Yes
Home city $\times$ year FE	Yes	Yes	Yes
Bid cohort $\times$ year FE	Yes	Yes	Yes
Observations	86,877	39,641	31,759

Notes: Definitions follow Table 3. Only 2015–2016 bid cohorts are retained. The same firms enter relative years –2 through 4, with year –1 as the reference. Inference clusters by firm home city.

Table B7: Allocation Changes after the Official Leaves

	Pooled (1)	Mayors (2)	Party secretaries (3)
<i>Award value (asinh)</i>			
Months -6 to -1	0.858** (0.066)	0.864** (0.087)	0.852** (0.099)
Months 1 to 6	0.187** (0.042)	0.250** (0.057)	0.120* (0.056)
Post minus pre	-0.671** (0.071)	-0.614** (0.094)	-0.731** (0.107)
Months 12 to 17	-0.008 (0.020)	-0.010 (0.031)	-0.007 (0.028)
<i>Award count (asinh)</i>			
Months -6 to -1	0.225** (0.018)	0.230** (0.024)	0.220** (0.026)
Months 1 to 6	0.046** (0.010)	0.058** (0.013)	0.033* (0.013)
Post minus pre	-0.180** (0.018)	-0.172** (0.024)	-0.188** (0.025)
Months 12 to 17	-0.001 (0.004)	-0.002 (0.006)	-0.001 (0.006)
Control variables	No	No	No
Counterfactual origin $\times$ destination FE	Yes	Yes	Yes
Counterfactual destination $\times$ month FE	Yes	Yes	Yes
Observations	508	262	246

Notes: Entries report mean differences between actual and predicted procurement. Month 0 begins eighteen consecutive unconnected months. Model fitting excludes connected observations and months 0–6. Value uses $\text{asinh}(\text{RMB}/10,000)$. Count uses $\text{asinh}(\text{count})$. Observations are origin–destination departure windows. Parentheses contain standard errors from 499 destination-city bootstrap resamples, holding fitted counterfactual predictions fixed. * $p < 0.05$, ** $p < 0.01$.

Table B8: Prior Experience and Allocation Advantage

	Pooled		Mayors		Party secretaries	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Total contract value (asinh)</i>						
Connection $\times$ Post	0.630** (0.023)	0.864** (0.029)	0.652** (0.028)	0.840** (0.033)	0.588** (0.029)	0.877** (0.040)
Connection $\times$ Post $\times$ origin experience	0.043** (0.005)	0.042** (0.004)	0.044** (0.006)	0.042** (0.006)	0.045** (0.006)	0.044** (0.006)
Connection $\times$ Post $\times$ destination experience		-0.707** (0.047)		-0.684** (0.065)		-0.728** (0.065)
Connection $\times$ Month 0	0.000 (0.051)	-0.018 (0.061)	0.002 (0.072)	-0.007 (0.082)	-0.006 (0.073)	0.028 (0.090)
Connection $\times$ Month 0 $\times$ origin experience	-0.003 (0.008)	-0.003 (0.008)	-0.012 (0.013)	-0.012 (0.013)	0.010 (0.011)	0.009 (0.011)
Connection $\times$ Month 0 $\times$ destination experience		0.058 (0.114)		0.032 (0.169)		-0.075 (0.147)
<i>Contract count (asinh)</i>						
Connection $\times$ Post	0.159** (0.006)	0.221** (0.008)	0.161** (0.007)	0.210** (0.008)	0.149** (0.007)	0.228** (0.010)
Connection $\times$ Post $\times$ origin experience	0.012** (0.001)	0.011** (0.001)	0.012** (0.002)	0.012** (0.002)	0.012** (0.002)	0.012** (0.001)
Connection $\times$ Post $\times$ destination experience		-0.188** (0.012)		-0.178** (0.015)		-0.197** (0.016)
Connection $\times$ Month 0	-0.004 (0.011)	0.002 (0.013)	-0.007 (0.015)	0.002 (0.017)	-0.003 (0.015)	0.007 (0.018)
Connection $\times$ Month 0 $\times$ origin experience	-0.001 (0.002)	-0.001 (0.002)	-0.002 (0.003)	-0.002 (0.003)	0.001 (0.003)	0.001 (0.003)
Connection $\times$ Month 0 $\times$ destination experience		-0.015 (0.022)		-0.035 (0.028)		-0.025 (0.029)
Control variables	No	No	No	No	No	No
Event $\times$ origin $\times$ destination FE	Yes	Yes	Yes	Yes	Yes	Yes
Event $\times$ relative month FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin $\times$ month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,805,588	13,805,588	7,108,255	7,108,255	6,697,333	6,697,333

Notes: Sample and measures follow Table 4. Month 0 is the appointment month. Event-pair, event-month, and origin-calendar-month FE follow equation 4. Standardized outcomes use connected-origin pre-arrival SD (pooled: 1.951 for value, 0.403 for count). Standard errors cluster by destination and treated-origin city. * $p < 0.05$, ** $p < 0.01$.

Table B9: Pre-entry Dynamics by Prior Experience

	Pooled		Mayors		Party secretaries	
	Value	Count	Value	Count	Value	Count
Origin experience (11 terms)	0.262	0.167	0.133	0.302	0.362	0.309
<i>p</i> value	0.992	0.999	1.000	0.985	0.969	0.984
Destination experience (11 terms)	0.128	0.689	0.206	0.350	0.229	0.475
<i>p</i> value	1.000	0.749	0.997	0.973	0.995	0.918
Denominator degrees of freedom	316	316	287	287	275	275
Observations	13,805,588	13,805,588	7,108,255	7,108,255	6,697,333	6,697,333
Control variables	No	No	No	No	No	No
Event $\times$ origin $\times$ destination FE	Yes	Yes	Yes	Yes	Yes	Yes
Event $\times$ relative month FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin $\times$ month FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Definitions, sample, and weights follow Table 4. Joint Wald F tests cover eleven connection-by-month interactions with experience in months -12 through -2 , with month -1 as the reference. Tests are unadjusted. Inference clusters by destination and treated-origin city.

Table B10: Connected Awards and Government Purchasing Costs

Dependent variable	Log unit price	Acceptance score	Delivery delay	Bidders	One bidder
Units	log(RMB) (1)	0–100 (2)	Days (3)	Count (4)	Indicator (5)
Connection	0.038** (0.001)	−0.829** (0.020)	0.214** (0.032)	−0.150** (0.029)	0.023** (0.006)
Log quantity	−0.006** (3×10^{-4})	−0.051** (0.005)	0.007 (0.006)		
Competitive negotiation	−0.035** (0.001)	−0.005 (0.025)	0.035 (0.039)		
Invited tender	−0.015** (0.004)	0.018 (0.065)	0.005 (0.115)		
Open tender	−0.056** (0.001)	−0.009 (0.020)	0.021 (0.030)		
Price inquiry	−0.127** (0.001)	−0.006 (0.023)	0.005 (0.041)		
Single source	0.089** (0.002)	−0.013 (0.037)	−0.046 (0.057)		
Observations	488,300	470,772	464,848	488,300	488,300
Quantity control variable	Yes	Yes	Yes	No	No
Procedure control variables	Yes	Yes	Yes	No	No
Firm FE	Yes	Yes	Yes	Yes	Yes
Product model $\times$ month FE	Yes	Yes	Yes	Yes	Yes
Destination $\times$ month FE	Yes	Yes	Yes	Yes	Yes
Origin $\times$ month FE	Yes	Yes	Yes	Yes	Yes

Notes: Definitions and samples follow Table 5. A causal interpretation requires comparable product specifications and delivery obligations, with connection status unrelated to unobserved determinants of purchasing outcomes within these award comparisons, including factors affecting award selection. Competitive consultation is the reference procedure. Connection stars in columns (4)–(5) use Holm adjustment across the two competition outcomes. Standard errors in parentheses cluster by destination and origin city. * $p < 0.05$, ** $p < 0.01$.

Table B11: Connected Purchasing Expenditure and a Fixed-Basket Cost Benchmark

Year	Connected awards (1)	Recorded expenditure (RMB billion) (2)	Additional expenditure (RMB million) (3)
2013	10,255	14.87	549.0
2014	9,280	15.21	561.4
2015	10,619	17.56	648.0
2016	11,428	20.73	765.3
2017	10,615	20.79	767.4
2018	10,420	20.80	767.7
2019	11,131	22.52	831.2
2020	9,984	25.70	948.6
Annual average	10,466	19.77	729.8
Total	83,732	158.19	5,838.6

Notes: Column (1) counts connected awards in the unit-price estimation sample of Table 5, column (1). Column (2) sums recorded unit price times quantity, $S_y = \sum_{c \in y, C_c=1} p_c Q_c$, expressed in RMB billion. Column (3) applies $S_y(1 - e^{-\hat{\beta}_P})$, where $\hat{\beta}_P = 0.0376073$ is the conditional log-price coefficient. This coefficient comes from a model controlling for log quantity and procedure with firm, product-model-month, destination-month, and origin-month FE. The benchmark removes the same proportional premium from all connected goods while holding the basket, suppliers, and purchasing arrangements fixed. It values the observed price association under that assumption. Acceptance and delay differences are reported in their own units in the main table. Counts in the annual-average row are rounded to the nearest award.

Appendix C Robustness

The main arrival analysis follows the first connection for twenty-four months. Here we follow active connections and changes in connection status throughout January 2013–December 2020. Following Nian and Wang (2023), $D_{odt} = 1$ while either serving mayor or party secretary in destination d has prior service in origin o . The connection ends when both links cease and can reactivate under a later appointment. Pairs connected in January 2013 enter with $D_{odt} = 1$ and may subsequently lose or regain the connection. The balanced panel contains 111,890 nonlocal origin–destination pairs and 10,741,440 pair-months, including months with no awards. For the same monthly asinh outcomes, we

estimate

$$Y_{odt} = \beta_S D_{odt} + \alpha_{od} + \lambda_{ot} + \mu_{dt} + \varepsilon_{odt}. \quad (\text{C.1})$$

City-pair effects absorb stable bilateral differences, while origin–calendar-month and destination–calendar-month effects account for supply and demand changes. We weight observations equally and cluster standard errors by origin and destination. Table C1 applies union, mayor, and party-secretary definitions to the same full panel. A causal interpretation requires parallel trends in procurement without a connection after these adjustments, the same additive connection effect across pairs and months, no anticipation, no effects persisting after the connection ends, and no spillovers to comparison origins.

We then use de Chaisemartin and D’Haultfœuille (2026)’s intertemporal DiD estimator to allow heterogeneous effects and subsequent treatment changes. Let g denote a city pair, and let F_g be its first change from the January 2013 state. Entries start at zero and exits start at one. At horizon $h = 1, \dots, 6$, the pair-specific comparison is

$$\hat{\Delta}_{gh} = Y_{g,F_g+h-1} - Y_{g,F_g-1} - \sum_{j \in C_{gh}} w_{jgh} (Y_{j,F_g+h-1} - Y_{j,F_g-1}). \quad (\text{C.2})$$

Comparison pairs supply the same destination, begin with the same connection status, and retain that status through the target month. Their weights sum to one. We estimate entries and exits separately, giving each pair that changes status equal weight and retaining its later reversals. At each horizon, the estimate compares procurement after the initial and any later changes with procurement if the initial status had continued. Negative exit coefficients therefore indicate contraction.

Identification requires no anticipation, parallel trends if the initial connection status continued within destination and initial-status groups, and no spillovers between comparison origins. Standard errors cluster by destination. Table C2 reports relative months 0, 2, and 5, corresponding to horizons 1, 3, and 6. Comparison pairs become ineligible as they change status. Never-connected origins are pooled into destination–month means of the trans-

formed outcomes, weighted by the number of origins represented. Each ever-connected pair remains separate with unit weight. Observations count the changes in outcomes for pairs changing status and eligible comparison pairs at each horizon, after pooling. Six placebo estimates before the first change assess parallel trends.

Pre-entry placebo tests reject parallel trends for both outcomes ($p < 0.001$). The pre-exit tests yield $p = 0.364$ for value and $p = 0.358$ for count. Entry estimates are positive. After exit, value declines at month 5 ($p = 0.022$), while the count estimate is imprecise ($p = 0.216$). The pre-entry differences qualify the causal interpretation of the positive entry estimates. The exit comparison shows no detectable pre-event divergence.

Table C1: Current Connections and Monthly Procurement

	(1)	(2)	(3)
Contract value (asinh)	0.491** (0.023)	0.430** (0.027)	0.394** (0.026)
Contract count (asinh)	0.122** (0.006)	0.109** (0.007)	0.097** (0.008)
Connection definition	Either leader	Mayor	Party secretary
Observations	10,741,440	10,741,440	10,741,440

Notes: The balanced 2013–2020 panel covers 111,890 nonlocal city pairs, including months without procurement. City-pair months receive equal weight. Entries report coefficients on D_{odt} in equation C.1. All columns include city-pair, origin-month, and destination-month FE. Value is in RMB 10,000 before transformation. Standard errors cluster by origin and destination. * $p < 0.05$, ** $p < 0.01$.

Table C2: Procurement Changes after Connection Starts and Ends

	Value (asinh)		Count (asinh)	
	Entering (1)	Exiting (2)	Entering (3)	Exiting (4)
Switch month	0.109** (0.002)	0.091 (0.186)	0.018** (0.001)	0.097 (0.050)
Observations	5,026	709	5,026	709
Switching pairs	1,526	342	1,526	342
Month 2	0.169** (0.003)	0.018 (0.173)	0.033** (0.001)	−0.002 (0.050)
Observations	4,898	676	4,898	676
Switching pairs	1,520	329	1,520	329
Month 5	0.524** (0.003)	−0.477* (0.208)	0.127** (0.001)	−0.062 (0.050)
Observations	4,794	658	4,794	658
Switching pairs	1,508	322	1,508	322

Notes: Estimates follow equation C.2, using the R package DIDmultiplegtDYN. Never-connected origins are pooled into destination–month means, weighted by the number of origins. Ever-connected pairs remain separate. Observations count the resulting comparison changes. Exit estimates report changes in actual procurement. Value is in RMB 10,000 before transformation. Standard errors cluster by destination. * $p < 0.05$, ** $p < 0.01$.

Table C3: Arrival Estimates: Outcomes and Specifications

	(1)	(2)	(3)	(4)	(5)
<i>Panel A. Monthly contract value</i>					
Connection × Post	0.550** (0.074)	0.596** (0.051)	0.673** (0.058)	0.693** (0.060)	0.689** (0.036)
Observations	8,900,088	8,900,088	8,900,088	8,900,088	8,900,088
<i>Panel B. Monthly contract count</i>					
Connection × Post	0.280** (0.025)	0.127** (0.011)	0.164** (0.014)	0.169** (0.015)	0.168** (0.009)
Observations	8,900,088	8,900,088	8,900,088	8,900,088	8,900,088
Outcome scale	Raw	$\log(1 + y)$	asinh	asinh	asinh
Control variables	Yes	Yes	Yes	No	Yes
Event × relative month FE	Yes	Yes	Yes	Yes	Yes
Origin × calendar month FE	No	No	No	No	Yes

Notes: Models follow Table B1 and use the same sample with complete economic data. Raw value is RMB million. Transformed value uses RMB 10,000. Month −1 is the reference. Economic-control coefficients are omitted. Standard errors in parentheses cluster by destination and treated-origin cities. * $p < 0.05$, ** $p < 0.01$.

Table C4: Arrival Estimates across Provincial-Capital Samples and Weights

	All destinations (1)	Noncapital destination (2)	Capital destination (3)	Both noncapital (4)	All destinations (5)	All destinations (6)
<i>Panel A. Contract value (asinh)</i>						
Connection $\times$ Post	0.673** (0.058)	0.652** (0.058)	0.778** (0.208)	0.625** (0.057)	0.689** (0.065)	0.848** (0.129)
<i>Panel B. Contract count (asinh)</i>						
Connection $\times$ Post	0.164** (0.014)	0.155** (0.014)	0.227** (0.047)	0.149** (0.014)	0.168** (0.015)	0.224** (0.030)
Observations	8,900,088	8,108,160	791,928	7,131,072	8,900,088	8,900,088
Economic controls	Yes	Yes	Yes	Yes	Yes	Yes
Event $\times$ relative month FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment differencing	Yes	Yes	Yes	Yes	Yes	Yes
Event weights	Equal	Equal	Equal	Equal	Destination	Prior count

Notes: Definitions follow Table B1. Capitals are the 27 provincial and autonomous-region capitals. Column (4) excludes capital destinations and origins. Columns (5)–(6) weight events by inverse destination frequency or prior-year destination award count. Comparison origins receive the event weight. Standard errors cluster by destination and treated-origin city. * $p < 0.05$, ** $p < 0.01$.

Table C5: Arrival Estimates: Connection Histories and Group-Time Comparisons

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Monthly contract value (asinh)</i>						
Connection $\times$ Post	0.703** (0.059)	0.710** (0.071)	0.763** (0.099)	0.862** (0.109)	0.589** (0.080)	0.589** (0.080)
Observations	7,880,161	6,114,720	3,570,120	2,209,104	115,168	283,008
<i>Panel B. Monthly contract count (asinh)</i>						
Connection $\times$ Post	0.173** (0.014)	0.178** (0.017)	0.192** (0.021)	0.204** (0.026)	0.150** (0.018)	0.150** (0.018)
Observations	7,880,161	6,114,720	3,570,120	2,209,104	115,168	283,008
Selection	All arrivals	Active 24 months	Active through 2020	One appointment	Active through 2020	Active through 2020
Follow-up	To first end	Months 1–24	Months 1–24	Months 1–24	Quarters 1–8	Quarters 1–8
Economic controls	Yes	Yes	Yes	Yes	No	No
Event $\times$ period FE	Yes	Yes	Yes	Yes	Yes	No
Destination indicators	No	No	No	No	No	Yes

Notes: Monthly outcomes follow Table B1. Column (1) ends follow-up for both groups at the first connection interruption. Samples with one appointment exclude later arrivals. Columns (5)–(6) compare persistent ties with never-connected origins by quarter. Column (6) applies Callaway and Sant’Anna (2021) to origin-province means weighted by the number of origins. Full-history pretrend tests reject ($p < 0.001$). Standard errors cluster by destination and treated origin in (1)–(4), and by destination in (5). Column (6) uses a destination multiplier bootstrap. * $p < 0.05$, ** $p < 0.01$.

Table C6: Sensitivity to Differential Procurement Trends

$\bar{M}$	Award value (asinh)		Award count (asinh)	
	Lower bound	Upper bound	Lower bound	Upper bound
0	0.577	0.808	0.141	0.198
0.5	-0.985	2.362	-0.134	0.478
1	-2.635	4.012	-0.438	0.783
Joint pre-arrival p	0.877		0.918	
Input Observations	40,860		40,860	

Notes: The 95% bounds apply the relative-magnitude restriction of Rambachan and Roth (2023): later changes in differential trends are at most $\bar{M}$ times the largest pre-arrival change. The target is the average for months 1–24 relative to month -1 . Event-month difference estimates provide the full covariance matrix. Estimates omit economic controls. Standard errors cluster by destination and treated-origin city.

Table C7: Firm Expansion: Outcomes, Prior Capabilities, and Initial Opportunities

	(1)	(2)	(3)	(4)	(5)
<i>Panel A. Annual external award value</i>					
Award recipient $\times$ Post	0.057 (0.044)	0.154** (0.029)	0.180** (0.032)	0.152** (0.029)	0.518** (0.198)
Award recipient $\times$ Initial-bid year	0.042 (0.052)	0.074* (0.033)	0.090* (0.036)	0.070 (0.036)	0.425 (0.256)
Observations	164,784	164,784	162,224	162,224	4,416
<i>Panel B. Annual external award count</i>					
Award recipient $\times$ Post	0.045** (0.010)	0.027** (0.005)	0.035** (0.007)	0.029** (0.006)	0.131** (0.043)
Award recipient $\times$ Initial-bid year	0.012 (0.013)	0.011 (0.006)	0.016* (0.008)	0.012 (0.008)	0.115* (0.057)
Observations	164,784	164,784	162,224	162,224	4,416
Outcome scale	Raw	$\log(1 + y)$	asinh	asinh	asinh
Control variables	No	No	No	Yes	No
Firm FE	Yes	Yes	Yes	Yes	Yes
Home city $\times$ year FE	Yes	Yes	Yes	Yes	Yes
Bid cohort $\times$ year FE	Yes	Yes	Yes	Yes	No
Initial contest $\times$ year FE	No	No	No	No	Yes
Balanced contest weights	No	No	No	No	Yes

Notes: External markets follow Table 3. Raw value is RMB million. Columns (3)–(4) use the same complete cases. Column (4) interacts prior awards, market breadth, and firm age with year. In (5), each of 270 initial contests gives its winner weight one and its unsuccessful bidders a combined weight of one (552 firms). Standard errors cluster by home city and, in (5), by contest. * $p < 0.05$, ** $p < 0.01$.

Table C8: Firm Expansion by Initial Procurement City and Weighting

	All firms (1)	Noncapital initial city (2)	Capital initial city (3)	All firms (4)	All firms (5)
<i>Panel A. Annual external award value (asinh)</i>					
Award recipient $\times$ Post	0.176** (0.032)	0.165** (0.035)	0.185* (0.091)	0.174** (0.033)	0.133* (0.063)
Award recipient $\times$ Initial-bid year	0.085* (0.037)	0.081 (0.043)	0.135 (0.109)	0.079* (0.037)	0.086 (0.077)
<i>Panel B. Annual external award count (asinh)</i>					
Award recipient $\times$ Post	0.035** (0.007)	0.033** (0.007)	0.035 (0.019)	0.034** (0.007)	0.030* (0.014)
Award recipient $\times$ Initial-bid year	0.015 (0.008)	0.013 (0.009)	0.021 (0.027)	0.013 (0.008)	0.003 (0.017)
<i>Panel C. Annual purchasing cities served (count)</i>					
Award recipient $\times$ Post	0.044** (0.009)	0.041** (0.010)	0.055* (0.026)	0.044** (0.009)	0.046* (0.020)
Award recipient $\times$ Initial-bid year	0.016 (0.011)	0.015 (0.012)	0.014 (0.040)	0.015 (0.011)	0.006 (0.023)
Weights	Equal firms	Equal firms	Equal firms	Equal initial contract groups	Equal home cities
Firm FE	Yes	Yes	Yes	Yes	Yes
Home city $\times$ year FE	Yes	Yes	Yes	Yes	Yes
Bid cohort $\times$ year FE	Yes	Yes	Yes	Yes	Yes
Control variables	No	No	No	No	No
Observations	164,784	136,984	25,464	164,784	164,784

Notes: Outcomes and timing follow Table 3. Capital-city restrictions concern initial bid destinations. Columns (2)–(3) also exclude directly administered municipalities. Columns (4)–(5) reweight the full sample by inverse eligible-firm counts per initial contract or home city, fixed across years. Standard errors cluster by firm home city. * $p < 0.05$, ** $p < 0.01$.

Table C9: Cohort-Specific Firm Expansion and Subsequent Convergence

	(1)	(2)
<i>Panel A. Annual external award value (asinh)</i>		
Recipient difference: initial-bid year	0.062 (0.047)	0.094 (0.056)
Recipient difference: subsequent years	0.123** (0.041)	0.131** (0.045)
2015–2016 recipients: years 1–2	0.237** (0.054)	0.228** (0.057)
2015–2016 recipients: years 3–4	–0.018 (0.055)	–0.032 (0.056)
2015–2016 recipients: late minus early	–0.254** (0.045)	–0.259** (0.047)
<i>Panel B. Annual external award count (asinh)</i>		
Recipient difference: initial-bid year	0.007 (0.010)	0.015 (0.011)
Recipient difference: subsequent years	0.021* (0.009)	0.024* (0.010)
2015–2016 recipients: years 1–2	0.046** (0.011)	0.047** (0.012)
2015–2016 recipients: years 3–4	–0.009 (0.012)	–0.010 (0.013)
2015–2016 recipients: late minus early	–0.055** (0.009)	–0.057** (0.009)
<i>Panel C. Annual distinct external purchasing cities</i>		
Recipient difference: initial-bid year	0.012 (0.013)	0.022 (0.015)
Recipient difference: subsequent years	0.031* (0.012)	0.038** (0.014)
2015–2016 recipients: years 1–2	0.060** (0.016)	0.063** (0.017)
2015–2016 recipients: years 3–4	–0.003 (0.018)	–0.004 (0.019)
2015–2016 recipients: late minus early	–0.063** (0.013)	–0.067** (0.014)
Control variables	No	No
Firm FE	Yes	Yes
Home city × year FE	Yes	No
Home city × bid cohort × year FE	No	Yes
Bid cohort × year FE	Yes	Yes
Observations	164,784	164,784

Notes: Estimates using Sun and Abraham (2021) retain initial recipient status. Unsuccessful first bidders remain in the comparison group after later awards. Year –1 is the reference. Year 0 enters separately. Early and late differences use the same 2015–2016 cohorts and weights. Standard errors use the home-city-clustered covariance matrix. * $p < 0.05$, ** $p < 0.01$.

Table C10: Procurement Changes after Connections End

	(1)	(2)	(3)	(4)
<i>Panel A. Monthly contract value</i>				
Ended connection × Month 0	0.039 (0.202)	−0.171 (0.106)	−0.199 (0.119)	−0.204 (0.107)
Ended connection × Months 1–6	−0.626** (0.110)	−0.598** (0.067)	−0.675** (0.075)	−0.690** (0.057)
Ended connection × Months 7–11	−0.621** (0.111)	−0.695** (0.071)	−0.789** (0.081)	−0.789** (0.057)
Ended connection × Months 12–17	−0.759** (0.110)	−0.783** (0.068)	−0.883** (0.076)	−0.887** (0.043)
Observations	3,952,200	3,952,200	3,952,200	3,952,200
<i>Panel B. Monthly contract count</i>				
Ended connection × Month 0	−0.137** (0.049)	−0.047* (0.020)	−0.060* (0.026)	−0.063** (0.024)
Ended connection × Months 1–6	−0.335** (0.038)	−0.140** (0.014)	−0.181** (0.018)	−0.183** (0.014)
Ended connection × Months 7–11	−0.389** (0.041)	−0.167** (0.016)	−0.215** (0.020)	−0.213** (0.013)
Ended connection × Months 12–17	−0.412** (0.042)	−0.178** (0.015)	−0.230** (0.019)	−0.227** (0.010)
Observations	3,952,200	3,952,200	3,952,200	3,952,200
Outcome scale	Raw	$\log(1 + y)$	asinh	asinh
Control variables	No	No	No	No
Event × city-pair FE	Yes	Yes	Yes	Yes
Event × relative month FE	Yes	Yes	Yes	Yes
Origin × calendar month FE	No	No	No	Yes

Notes: Stacked DiD compares ending connections with never-connected origins supplying the same destination. Events require six connected months before departure and eighteen unconnected months after departure. The preceding six months form the reference period. Raw value is RMB million. Transformed value uses RMB 10,000. Events and comparison groups receive equal weight. Standard errors cluster by destination and departure-origin city. * $p < 0.05$, ** $p < 0.01$.

Table C11: Departure Estimates under Alternative Training Exclusions

	(1)	(2)
Award value, pre months -6 to -1	0.858** (0.066)	0.866** (0.066)
Award value, post months 1–6	0.187** (0.042)	0.195** (0.043)
Award value, post minus pre	−0.671** (0.071)	−0.671** (0.071)
Award value, late months 12–17	−0.008 (0.020)	−0.001 (0.022)
Award count, pre months -6 to -1	0.225** (0.018)	0.227** (0.019)
Award count, post months 1–6	0.046** (0.010)	0.047** (0.010)
Award count, post minus pre	−0.180** (0.018)	−0.180** (0.018)
Award count, late months 12–17	−0.001 (0.004)	1×10^{-4} (0.005)
Control variables	No	No
Counterfactual origin $\times$ destination FE	Yes	Yes
Counterfactual destination $\times$ month FE	Yes	Yes
Observations	508	508

Notes: Outcome, timing, and sample-size definitions follow Table B7. Both columns exclude connected observations from counterfactual fitting. Columns (1) and (2) also exclude months 0–6 and 0–17, respectively. Standard errors in parentheses use 499 destination-city bootstrap draws, holding fitted counterfactual predictions fixed. * $p < 0.05$, ** $p < 0.01$.

Table C12: Career Experience: Economic Controls and Duration Support

	(1)	(2)	(3)
<i>Panel A. Monthly contract value (asinh)</i>			
Connection × Post	0.865** (0.030)	0.848** (0.028)	0.814** (0.030)
Connection × Post × Origin years	0.042** (0.005)	0.041** (0.005)	0.038** (0.006)
Connection × Post × Destination service	−0.706** (0.048)	−0.681** (0.047)	−0.652** (0.053)
Connection × Appointment month	−0.013 (0.061)	−0.025 (0.063)	−0.036 (0.073)
Connection × Appointment month × Origin years	−0.002 (0.008)	−0.003 (0.008)	0.021 (0.016)
Connection × Appointment month × Destination service	0.056 (0.114)	0.076 (0.116)	0.110 (0.133)
Observations	13,720,969	13,720,969	10,904,973
<i>Panel B. Monthly contract count (asinh)</i>			
Connection × Post	0.221** (0.008)	0.217** (0.007)	0.209** (0.008)
Connection × Post × Origin years	0.011** (0.001)	0.011** (0.001)	0.010** (0.002)
Connection × Post × Destination service	−0.189** (0.012)	−0.181** (0.012)	−0.176** (0.013)
Connection × Appointment month	0.003 (0.013)	0.001 (0.013)	−0.010 (0.015)
Connection × Appointment month × Origin years	−0.001 (0.002)	−0.001 (0.002)	0.004 (0.003)
Connection × Appointment month × Destination service	−0.016 (0.022)	−0.014 (0.022)	−0.003 (0.025)
Observations	13,720,969	13,720,969	10,904,973
Control variables	No	Yes	Yes
Event × city-pair FE	Yes	Yes	Yes
Event × relative month FE	Yes	Yes	Yes
Origin × calendar month FE	Yes	Yes	Yes
Origin-service range	0.5–23 years	0.5–23 years	2–14 years

Notes: Measures follow Table B8. Origin years are centered at 8.573. Destination service is binary. Columns (1)–(2) use the same complete cases. Columns (2)–(3) add interactions with economic gaps for postarrival and appointment months. Column (3) retains the common 10th–90th percentile range of origin service. Standard errors cluster by destination and treated-origin city. * $p < 0.05$, ** $p < 0.01$.

Table C13: Career Experience: Presence and Duration of Prior Destination Service

	(1)	(2)	(3)
<i>Panel A. Monthly contract value (asinh)</i>			
Connection × Post	0.864** (0.029)	0.808** (0.028)	0.865** (0.029)
Connection × Post × Origin years	0.042** (0.004)	0.042** (0.004)	0.042** (0.004)
Connection × Post × Destination indicator	−0.707** (0.047)		−0.645** (0.082)
Connection × Post × Destination years		−0.157** (0.013)	−0.018 (0.021)
Connection × Appointment month	−0.018 (0.061)	0.000 (0.059)	−0.017 (0.061)
Connection × Appointment month × Origin years	−0.003 (0.008)	−0.003 (0.008)	−0.003 (0.008)
Connection × Appointment month × Destination indicator	0.058 (0.114)		0.195 (0.230)
Connection × Appointment month × Destination years		0.001 (0.029)	−0.040 (0.058)
Observations	13,805,588	13,805,588	13,805,588
<i>Panel B. Monthly contract count (asinh)</i>			
Connection × Post	0.221** (0.008)	0.206** (0.008)	0.221** (0.008)
Connection × Post × Origin years	0.011** (0.001)	0.011** (0.001)	0.011** (0.001)
Connection × Post × Destination indicator	−0.188** (0.012)		−0.169** (0.018)
Connection × Post × Destination years		−0.042** (0.003)	−0.006 (0.005)
Connection × Appointment month	0.002 (0.013)	0.005 (0.013)	0.002 (0.013)
Connection × Appointment month × Origin years	−0.001 (0.002)	−0.001 (0.002)	−0.001 (0.002)
Connection × Appointment month × Destination indicator	−0.015 (0.022)		0.032 (0.047)
Connection × Appointment month × Destination years		−0.007 (0.005)	−0.014 (0.011)
Observations	13,805,588	13,805,588	13,805,588
Control variables	No	No	No
Event × city-pair FE	Yes	Yes	Yes
Event × relative month FE	Yes	Yes	Yes
Origin × calendar month FE	Yes	Yes	Yes

Notes: All columns use the 1,142 events in Table B8. Origin years are centered at 8.573. The destination indicator records prior municipal service there. Destination years sum the duration of that service, with zero for none. Other definitions and weights follow that table. Standard errors in parentheses cluster by destination and treated-origin cities. * $p < 0.05$, ** $p < 0.01$.

Table C14: Recorded Purchasing Outcomes under Stricter Comparability

	(1)	(2)	(3)
<i>Panel A. Unit price (log RMB per recorded quantity)</i>			
Connection	0.038** (0.001)	0.033** (0.003)	0.049** (0.012)
Observations	488,300	488,300	39,117
<i>Panel B. Acceptance score (0–100)</i>			
Connection	−0.829** (0.020)	−0.811** (0.048)	−0.417** (0.149)
Observations	470,772	470,772	37,647
<i>Panel C. Delivery delay (days)</i>			
Connection	0.214** (0.032)	0.213** (0.075)	1.511** (0.363)
Observations	464,848	464,848	37,136
Quantity control	Yes	Yes	Yes
Procedure controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Destination × month FE	Yes	Yes	Yes
Origin × month FE	Yes	Yes	Yes
Product × month FE	Yes	Yes	Yes
Supplier–buyer city-pair FE	No	Yes	No

Notes: Connection is the explanatory variable. All columns include log quantity and procedure controls, firm FE, product–model–month FE, and destination–month and origin–month FE, as in Table 5. Column (2) adds supplier–buyer city-pair FE. Column (3) covers office consumables. Product–month cells retain both connection groups. Standard errors cluster by destination and supplier–origin city. * $p < 0.05$, ** $p < 0.01$.

We also examine narrower comparisons within product–destination–quarter groups and product–month–procedure–quantity groups. After absorbing supplier and geographic time effects and removing observations without comparison support, the remaining comparisons contain no variation in connection status. Table C14 reports the three specifications in which its coefficient can be estimated.

Table C15: Purchasing Comparisons across City Samples and Weights

	Baseline (1)	Noncapital buyers (2)	Both cities noncapital (3)	Equal buyer weights (4)
<i>Panel A. Unit price (log RMB)</i>				
Connection	0.038** (0.001)	0.037** (0.002)	0.033** (0.003)	0.036** (0.001)
Observations	488,300	357,771	130,668	488,300
<i>Panel B. Acceptance score (0–100)</i>				
Connection	−0.829** (0.020)	−0.797** (0.030)	−0.685** (0.068)	−0.819** (0.026)
Observations	470,772	344,780	125,236	470,772
<i>Panel C. Delivery delay (days)</i>				
Connection	0.214** (0.032)	0.187** (0.044)	0.151 (0.102)	0.214** (0.032)
Observations	464,848	340,322	123,383	464,848
Log quantity control	Yes	Yes	Yes	Yes
Procedure controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Product model × month FE	Yes	Yes	Yes	Yes
Destination × month FE	Yes	Yes	Yes	Yes
Origin × month FE	Yes	Yes	Yes	Yes

Notes: Connection is the explanatory variable. Log quantity, procedure controls, and all FE follow Table 5. Capitals are the 27 provincial and autonomous-region capitals. Municipalities may remain supplier origins. Each restricted product-model-month cell retains both connection groups. Column (4) gives each destination equal total weight. Standard errors cluster by destination and origin city. * $p < 0.05$, ** $p < 0.01$.

Table C16: Connection Status and Availability of Purchasing Fields

<i>A. Availability among all recorded awards (%)</i>		
	Unconnected	Connected
Item data available	94.68	94.56
Both performance fields available	94.03	93.92
Observations	861,800	100,956
<i>B. Connection and availability in prefectural destinations</i>		
Dependent variable	Item data	Both performance fields
Connection (pp)	0.091 (0.101)	−0.122 (0.079)
<i>p</i>	0.368	0.121
Holm <i>p</i>	0.368	0.242
Observations	920,409	920,409
Control variables	No	No
Destination × month FE	Yes	Yes
Origin × month FE	Yes	Yes
Category × month FE	Yes	Yes

Notes: Item fields are available when firm and model identifiers and all item details are present, and price and quantity are positive. Performance fields are available when acceptance score and delivery delay are present. Panel A covers recorded awards. Panel B reports linear-probability estimates in percentage points (pp) for prefectural buyers. Holm adjustments cover both tests. Standard errors cluster by destination and origin city.

Table C17 excludes local comparison origins while retaining the same events as each main analysis. Panel A keeps all 1,135 arrival events with economic controls and recomputes comparison weights and prior-year economic gaps. Panel B keeps all 1,142 experience events and renormalizes comparison weights. Removing local suppliers scarcely changes the arrival estimates or either experience association. Longer origin service is still associated with a larger advantage, and prior destination service with a smaller one.

Table C17: Local Suppliers and the Comparison Group

Comparison origins	Value (asinh)		Count (asinh)	
	All	Nonlocal	All	Nonlocal
<i>Panel A. Arrival advantage</i>				
Connection $\times$ Post	0.673** (0.058)	0.674** (0.058)	0.164** (0.014)	0.164** (0.014)
Observations	8,900,088	8,872,848	8,900,088	8,872,848
Within R^2	0.0494	0.0496	0.0593	0.0601
Economic controls	Yes	Yes	Yes	Yes
Event–relative-month FE	Yes	Yes	Yes	Yes
<i>Panel B. Experience and the arrival advantage</i>				
Connection $\times$ Post	0.864** (0.029)	0.864** (0.029)	0.221** (0.008)	0.221** (0.008)
Connection $\times$ Post $\times$ origin experience	0.042** (0.004)	0.042** (0.004)	0.011** (0.001)	0.011** (0.001)
Connection $\times$ Post $\times$ destination experience	−0.707** (0.047)	−0.707** (0.047)	−0.188** (0.012)	−0.188** (0.012)
Connection $\times$ Month 0	−0.018 (0.061)	−0.018 (0.061)	0.002 (0.013)	0.001 (0.013)
Connection $\times$ Month 0 $\times$ origin experience	−0.003 (0.008)	−0.003 (0.008)	−0.001 (0.002)	−0.001 (0.002)
Connection $\times$ Month 0 $\times$ destination experience	0.058 (0.114)	0.059 (0.114)	−0.015 (0.022)	−0.015 (0.022)
Observations	13,805,588	13,763,334	13,805,588	13,763,334
Within R^2	0.0216	0.0217	0.0287	0.0291
Economic controls	No	No	No	No
Event–city-pair FE	Yes	Yes	Yes	Yes
Event–relative-month FE	Yes	Yes	Yes	Yes
Origin–calendar-month FE	Yes	Yes	Yes	Yes

Notes: Each panel uses the same events in its two comparison samples. All includes local and unconnected nonlocal origins. Nonlocal removes local origins and adjusts the remaining comparison weights. Panel A retains all 1,135 events with economic controls. Prior-year economic gaps are recalculated for each sample and interacted with connection and Post as in equation 1. Outcomes are differenced from month -1 . Panel B retains all 1,142 experience events and uses months -12 through 24. Origin years are centered at 8.573. Destination experience indicates prior service (0/1). Post covers months 1–24. Month 0 enters separately. Event FE absorb connection and experience main effects and the required lower-order interactions. Standard errors cluster by destination and treated-origin city. Within R^2 is unadjusted after absorbing FE. * $p < 0.05$, ** $p < 0.01$.

Appendix D Alternative Explanations

We examine winning and expansion by recorded supplier ownership and prior procurement activity. Supplier types come from legal-form wording, while foreign investment is a separate registry indicator. To form mutually exclusive groups, indicated foreign in-

vestment takes precedence, followed by indicated state-owned enterprise (SOE) status, recorded private ownership, and a residual group containing unclassified ownership and public institutions. The registry reports ownership at one point in time and provides no dated histories of ownership changes.

Table D1 reports the winning model for all 3,650,350 disclosed bids by 144,400 firms in column (1), including single-source and single-bidder awards. Column (2) uses every contest containing both connected and unconnected bidders. Firm effects absorb group main effects, and connection and local-firm interactions permit group-specific advantages. Columns (3) and (4) retain, respectively, contests without an indicated state-owned bidder and contests containing only recorded private suppliers. Both retain every competitor. Across all disclosed bids, the indicated state-owned-minus-private difference is imprecise ($p = 0.117$), while the foreign-invested-minus-private difference is -9.20 percentage points (Holm-adjusted $p = 0.018$). In contests containing both connected and unconnected bidders, that foreign-invested difference is -5.04 percentage points ($p = 0.109$). Excluding entire contests with an indicated state-owned bidder leaves a 36.89 percentage points connection advantage ($p < 0.001$). In recorded-private-only contests the advantage is 39.95 percentage points ($p < 0.001$). The residual-group differences are -6.82 percentage points across all bids and -4.99 percentage points in contests containing both connected and unconnected bidders (Holm-adjusted $p = 0.021$ and $p = 0.068$, respectively). This group combines institutions with unclassified ownership. All these comparisons concern winning among disclosed bidders.

Table D1: Supplier Classifications and the Connection Advantage in Winning

Contests	(1) All disclosed	(2) Mixed	(3) SOE-free	(4) Private
Connection	0.472** (0.032)	0.374** (0.022)	0.369** (0.022)	0.400** (0.021)
Connection $\times$ State-owned	0.038 (0.025)	0.037 (0.023)		
Connection $\times$ Foreign-invested	-0.092** (0.033)	-0.050 (0.031)		
Connection $\times$ Other	-0.068* (0.026)	-0.050* (0.022)		
Local firm	0.112** (0.010)	0.035** (0.009)	0.033** (0.009)	0.050** (0.008)
Local firm $\times$ State-owned	0.033** (0.007)	0.007 (0.012)		
Local firm $\times$ Foreign-invested	-0.038** (0.013)	-0.013 (0.017)		
Local firm $\times$ Other	-0.024** (0.009)	-0.017* (0.009)		
Observations	3,650,350	617,412	567,855	281,871
Within R^2	0.0370	0.1144	0.1141	0.1270
Firm FE	Yes	Yes	Yes	Yes
Contract FE	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes

Notes: The dependent variable is winning (0/1). Control variables comprise the local-firm indicator and its displayed ownership interactions. Connection identifies career-connected outsiders. Winning probabilities are proportions. Column (1) retains every disclosed bid, including single-bidder awards. Columns (2)–(4) contain connected and unconnected bidders. Private suppliers are the reference in (1)–(2). Firm FE absorb ownership main effects. Other combines other ownership forms and public institutions. Column (3) excludes every contest with an indicated SOE bidder; (4) retains contests in which all bidders are classified as private. All competitors in the selected contests are retained. Standard errors cluster by destination and firm home city. Within R^2 is unadjusted after absorbing FE. * $p < 0.05$, ** $p < 0.01$.

Table D2 assesses whether expansion extends beyond state-owned suppliers. It repeats the analysis after excluding suppliers with an indicated state-owned legal form and among suppliers classified as private. Recipient status, the initial-bid-year term, FE, external-market definition, and zero-award years follow the main analysis. Among private suppliers, subsequent award count increases by 0.0365 asinh units ($p < 0.001$), and newly served cities increase by 0.0496 annually ($p < 0.001$). All six outcome–sample comparisons have Holm-adjusted pre-bid test $p = 1.000$.

Table D2: Firm Expansion across Recorded Supplier Classifications

Suppliers	(1) All	(2) No indicated SOE	(3) Recorded private
<i>Panel A. Total award value (asinh)</i>			
Recipient $\times$ Post	0.176** (0.032)	0.177** (0.033)	0.192** (0.033)
Recipient $\times$ Initial year	0.085* (0.037)	0.084* (0.037)	0.098* (0.038)
Observations	164,784	162,360	137,768
Within R^2	0.0003	0.0003	0.0004
<i>Panel B. Award count (asinh)</i>			
Recipient $\times$ Post	0.035** (0.007)	0.035** (0.007)	0.036** (0.008)
Recipient $\times$ Initial year	0.015 (0.008)	0.014 (0.008)	0.016* (0.008)
Observations	164,784	162,360	137,768
Within R^2	0.0003	0.0003	0.0003
<i>Panel C. New purchasing cities</i>			
Recipient $\times$ Post	0.044** (0.008)	0.044** (0.008)	0.050** (0.012)
Recipient $\times$ Initial year	0.008 (0.010)	0.008 (0.010)	0.013 (0.011)
Observations	144,186	142,065	120,547
Within R^2	0.0003	0.0003	0.0003
Firm FE	Yes	Yes	Yes
Home city $\times$ year FE	Yes	Yes	Yes
Bid cohort $\times$ year FE	Yes	Yes	Yes
Control variables	No	No	No

Notes: Recipients won their first connected nonlocal bid. Post excludes the initial year. External outcomes exclude home cities and all markets with a recorded career connection during 2013–2020. Years without awards are retained. Column (2) retains suppliers without an indicated SOE legal form, including those with other ownership forms. Column (3) uses private legal forms without a foreign-investment indication. New-city outcomes begin in 2014. Standard errors cluster by firm home city. Within R^2 is unadjusted after absorbing FE. * $p < 0.05$, ** $p < 0.01$.

Table D3 asks whether an initial award matters more for firms with limited procurement experience. Awards during 2013–2014 precede every initial connected bid in the 2015–2018 cohorts. Firms with at most the pooled median of two earlier awards form the lower-activity group. The joint model interacts both recipient timing terms with higher activity and permits group-specific home-city–year and bid-cohort–year FE. Direct higher-minus-lower differences are imprecise in all three outcomes (Holm-adjusted $p = 0.489$ for each).

Higher-activity award-count and new-city estimates are positive but less precise. Group-specific and differential pre-bid lead tests yield no rejection after adjustment.

Table D3: Firm Expansion and Prior Procurement Activity

	(1) ≤ 2	(2) > 2	(3) Both groups
Prior awards			
<i>Panel A. Total award value (asinh)</i>			
Recipient $\times$ Post	0.161** (0.028)	0.105 (0.059)	0.161** (0.028)
Recipient $\times$ Initial year	0.054 (0.041)	0.063 (0.062)	0.054 (0.041)
Recipient $\times$ Post $\times$ Higher activity			−0.056 (0.065)
Recipient $\times$ Initial year $\times$ Higher activity			0.009 (0.070)
Observations	104,304	60,480	164,784
Within R^2	0.0003	0.0001	0.0002
<i>Panel B. Award count (asinh)</i>			
Recipient $\times$ Post	0.033** (0.005)	0.014 (0.012)	0.033** (0.005)
Recipient $\times$ Initial year	0.009 (0.009)	0.005 (0.013)	0.009 (0.009)
Recipient $\times$ Post $\times$ Higher activity			−0.018 (0.013)
Recipient $\times$ Initial year $\times$ Higher activity			−0.004 (0.015)
Observations	104,304	60,480	164,784
Within R^2	0.0003	0.0000	0.0002
<i>Panel C. New purchasing cities</i>			
Recipient $\times$ Post	0.046** (0.007)	0.025 (0.017)	0.046** (0.007)
Recipient $\times$ Initial year	0.009 (0.012)	−0.007 (0.020)	0.009 (0.012)
Recipient $\times$ Post $\times$ Higher activity			−0.021 (0.018)
Recipient $\times$ Initial year $\times$ Higher activity			−0.016 (0.024)
Observations	91,266	52,920	144,186
Within R^2	0.0004	0.0001	0.0002
Firm FE	Yes	Yes	Yes
Home city $\times$ year FE	Yes	Yes	By activity group
Bid cohort $\times$ year FE	Yes	Yes	By activity group
Control variables	No	No	No

Notes: Prior activity counts all awards in 2013–2014, before initial connected bids in 2015–2018. The pooled median is two awards. Higher activity means more than that median. Column (3) allows separate home-city-year and cohort-year effects for each activity group, absorbing its interactions with period main effects. Outcomes and recipient assignment follow Table D2. Standard errors cluster by firm home city. Within R^2 is unadjusted after absorbing FE. * $p < 0.05$, ** $p < 0.01$.

Table D4: Career Connections and Award Allocation

	(1)	(2)	(3)	(4)	(5)
<i>Winning the award</i>					
Connection	0.463** (0.033)	0.386** (0.028)	0.369** (0.022)	0.371** (0.022)	0.371** (0.022)
Local firm	0.109** (0.011)		0.033** (0.009)	0.033** (0.009)	0.033** (0.009)
Connection $\times$ score rule				-0.004 (0.003)	
Local firm $\times$ score rule				0.001 (0.002)	
Connection $\times$ Open tender					-0.004 (0.007)
Local firm $\times$ Open tender					1×10^{-4} (0.004)
Observations	3,650,350	1,766,521	617,412	617,412	617,412
Control variables	Yes	No	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Contract FE	Yes	Yes	Yes	Yes	Yes
Nonlocal firms only	No	Yes	No	No	No
Mixed contests only	No	No	Yes	Yes	Yes
Score-rule interactions	No	No	No	Yes	No
Procurement-method interactions	No	No	No	No	Yes

Notes: The dependent variable is winning (0/1). Connection is measured at award. Control variables comprise the local-firm indicator and its displayed interactions. Local firms are registered in the buyer city. Columns (3)–(5) use contests with connected and unconnected bidders. Column (4) compares comprehensive scoring with lowest evaluated price. Column (5) compares open tender with invited tendering, negotiation, consultation, and inquiry. Contract FE absorb procedure main effects. Single-source awards are excluded from (5). Coefficients are proportions. Standard errors cluster by destination and firm home city. * $p < 0.05$, ** $p < 0.01$.

Table D4 compares winning among disclosed bidders, with firm and contract FE. Connection and Local firm share unconnected nonlocal bidders as their reference. Subtracting the Local firm coefficient from the Connection coefficient gives an advantage for connected outsiders of 35.41 percentage points (standard error = 2.80) across all disclosed bids and 33.57 percentage points (standard error = 1.82) in contests containing both connected and unconnected bidders (both $p < 0.001$). Standard errors use the clustered covariance of the two coefficients. These comparisons concern winning among disclosed participants.

Table D5 asks whether prior cooperation distinguishes firms' access within career-connected origins. Eligible contests contain both connection groups and share one appointment month across the leaders whose careers create those connections. Awards occur afterward, fixing when earlier business is measured across bidders. The adjusted within-

contract model is

$$W_{fc} = \beta C_{fc} + \delta C_{fc} B_{fc} + \theta L_{fc} + \rho H_{fc} + \eta C_{fc} (H_{fc} - \bar{H}_C) + \kappa C_{fc} (S_{fc} - \bar{S}_C) + \alpha_f + \lambda_c + \varepsilon_{fc}. \quad (\text{D.1})$$

W_{fc} indicates winning, C_{fc} is the current career connection, B_{fc} indicates at least one home-city award during a serving leader's earlier service there, and L_{fc} marks local firms. H_{fc} is the asinh count of home-city awards from 2013 to the month before the shared appointment. S_{fc} sums each leader's years of earlier origin service and averages across leaders when both provide the connection. Adjusting for these histories distinguishes prior cooperation from firms' broader procurement activity and officials' tenure. Connected-bid means are $\bar{H}_C = 1.585$ and $\bar{S}_C = 1.899$. Columns (2)–(3) evaluate group advantages at these means, so δ directly compares connected groups. Column (1) omits these controls, setting $\rho = \eta = \kappa = 0$. Firm and contract FE hold stable firm attributes and the current purchase constant.

Table D5: Prior Local Procurement and Connected Bidding

	Basic	Adjusted	Interior months
Connected, no prior local award	0.149* (0.060)	0.133* (0.055)	0.100 (0.057)
Connected, prior local award	0.106 (0.057)	0.170* (0.067)	0.201** (0.066)
Difference between connected groups	−0.043 (0.036)	0.037 (0.041)	0.101** (0.035)
<i>p</i> : difference	0.238	0.369	0.005
Observations	32,194	32,194	32,194
Contracts	6,434	6,434	6,434
Firm and contract FE	Yes	Yes	Yes
Local-firm indicator	Yes	Yes	Yes
Prior award and service controls	No	Yes	Yes
Service boundary months included	Yes	Yes	No

Notes: The outcome is winning (0/1). All columns include Connection, its interaction with earlier local awards, and Local firm, with firm and contract FE. Columns (2)–(3) also include earlier home-city procurement and its connection interaction, plus the connection interaction with origin service. Winning probabilities are proportions. Group estimates compare connected bidders with unconnected nonlocal bidders. Prior local awards were made in the supplier's origin during the serving leader's earlier service there. The measure is fixed before arrival in the destination. All relevant service spells are fully covered in 2013–2020, and connected firms registered before those spells began. Contracts are awarded after the leaders' shared appointment month, and every bidder is retained. There are 3,690 connected bids with and 2,919 without prior local awards under the full-month definition. Adjusted group estimates use common history and service means. Column (3) excludes service-start and service-end months from the history measure. Standard errors cluster by destination and firm origin. * $p < 0.05$, ** $p < 0.01$.

Table D6: Origin Advantages under Continuing Leadership

	(1)	(2)	(3)	(4)
	Same leader remains		No other appointments	
	Value	Count	Value	Count
Months 1–6	0.232** (0.068)	0.063** (0.015)	0.404** (0.104)	0.092** (0.023)
Months 7–12	0.791** (0.075)	0.201** (0.018)	0.874** (0.116)	0.219** (0.028)
Months 13–18	0.850** (0.083)	0.215** (0.020)	1.022** (0.127)	0.247** (0.030)
Months 19–24	0.967** (0.091)	0.232** (0.022)	1.148** (0.145)	0.260** (0.034)
Year 2 – year 1	0.397** (0.047)	0.091** (0.012)	0.446** (0.075)	0.098** (0.017)
<i>p</i> : year difference	< 0.001	< 0.001	< 0.001	< 0.001
Observations	18,696	18,696	6,744	6,744
Origin–destination events	779	779	281	281
Initial appointments	715	715	254	254

Notes: The leader who established each first connection remains in office through month 24. Columns (3)–(4) also require one appointment in the arrival month and no other mayor or secretary appointment during follow-up. Outcomes subtract changes for fixed unconnected comparison origins from treated-origin changes since month -1 , using $\text{asinh}(V/10,000)$ and $\text{asinh}(N)$. Columns (1) and (3) measure transformed value, and (2) and (4) measure transformed count. Differencing and comparison weights implement the event–relative-month FE in the arrival design. Events receive equal weight. Prior-year economic gaps have separate coefficients for each period. Year 2 – year 1 compares months 13–24 with months 1–12 using the full covariance matrix. Standard errors cluster by treated-origin and destination city. Samples require the leader to remain in office. * $p < 0.05$, ** $p < 0.01$.

Table D7: Arrival Estimates and Hometown Overlap

	Full (1)	Known home (2)	Exclude overlap (3)	Interaction (4)	Both leaders (5)
<i>Award value (asinh)</i>					
Connection × Post	0.673** (0.058)	0.664** (0.060)	0.663** (0.062)	0.663** (0.062)	0.682** (0.063)
Connection × Post × hometown overlap				0.015 (0.186)	
<i>Award count (asinh)</i>					
Connection × Post	0.164** (0.014)	0.163** (0.015)	0.164** (0.015)	0.164** (0.015)	0.169** (0.015)
Connection × Post × hometown overlap				−0.012 (0.044)	
Control variables	Yes	Yes	Yes	Yes	Yes
Event × relative month FE	Yes	Yes	Yes	Yes	Yes
Pre-treatment differencing	Yes	Yes	Yes	Yes	Yes
Observations	8,900,088	8,492,616	7,677,120	8,492,616	7,042,536

Notes: Hometown is the recorded native-place city. Columns (2)–(4) require the appointed leader's hometown. Column (3) excludes matching origins and (4) compares them with nonmatching origins. Column (5) requires both leaders' hometowns and excludes either match. Other definitions follow Table B1. Standard errors cluster by destination and treated-origin city. * $p < 0.05$, ** $p < 0.01$.

Table D8: Arrival Estimates and Native-Place Ties with Provincial Leaders

	Known ties (1)	Exclude ties (2)	Interaction (3)
<i>Award value (asinh)</i>			
Connection × Post	0.653** (0.064)	0.657** (0.064)	0.659** (0.064)
Connection × Post × shared native place			−0.236 (0.370)
<i>Award count (asinh)</i>			
Connection × Post	0.163** (0.015)	0.165** (0.015)	0.166** (0.015)
Connection × Post × shared native place			−0.095 (0.089)
Control variables	Yes	Yes	Yes
Event × relative month FE	Yes	Yes	Yes
Pre-treatment differencing	Yes	Yes	Yes
Observations	7,458,024	7,269,672	7,458,024

Notes: A tie means the appointed leader and a serving provincial Party secretary or governor share a recorded hometown city. All three hometowns must be known. Column (2) excludes ties. Column (3) uses no-tie events as the reference. Other definitions follow Table B1. Standard errors cluster by destination and treated-origin city. * $p < 0.05$, ** $p < 0.01$.

Table D9: Pre-Bid Growth Tests by Prior Firm Capability

Prior capability	<i>F</i>	<i>p</i>	Holm <i>p</i>	Observations
<i>Award value (asinh)</i>				
log(1 + Award count)	1.507	0.223	0.893	66,926
log(1 + Award value)	1.767	0.172	0.862	66,926
Purchasing cities served	4.529	0.011	0.092	66,926
Firm age (years)	0.147	0.864	1.000	65,859
<i>Award count (asinh)</i>				
log(1 + Award count)	2.141	0.119	0.725	66,926
log(1 + Award value)	1.358	0.259	0.893	66,926
Purchasing cities served	2.283	0.104	0.725	66,926
Firm age (years)	0.398	0.672	1.000	65,859
Firm FE			Yes	
Home city × year FE			Yes	
Bid cohort × year FE			Yes	

Notes: Pre-bid firm-years follow Table 3. Each model jointly tests the recipient time slope and its interaction with a prior capability, controlling for the capability time slope. Time is measured relative to year -1 . Capabilities are measured by 2013–2014 awards, purchasing cities, and pre-bid firm age. Value is logged after scaling by RMB 10,000. Holm adjustment covers eight tests. Standard errors cluster by firm home city.

Table D10: Prior Economic Conditions and City-Leader Appointments

Dependent variable	New appointment (binary)	
	(1)	(2)
log(1 + Revenue)	0.044 (0.075)	0.054 (0.092)
Fiscal self-reliance	0.074 (0.049)	0.082* (0.040)
log(1 + GDP)	-4×10^{-4} (0.096)	0.030 (0.116)
log(1 + Population)	-0.230 (0.162)	-0.113 (0.218)
log(1 + Provincial debt)		0.027 (0.089)
Joint-test <i>p</i>	0.229	0.207
Observations	2,321	2,005
Additional control variables	No	No
City FE	Yes	Yes
Year FE	Yes	Yes

Notes: Linear-probability models predict whether a city receives a new mayoral or party-secretary appointment in each year from 2014 to 2020, using preceding-year conditions. Monetary variables use RMB 100 million. Population uses 10,000 persons. Fiscal self-reliance is revenue/expenditure. Joint tests cover all displayed predictors. Standard errors cluster by city. * $p < 0.05$, ** $p < 0.01$.

Table D11: Announcement Timing and Within-Award Bid Quotations*Panel A: Winning by announcement timing*

	Before arrival	After arrival	After minus before
Connection advantage	0.209** (0.080)	0.348** (0.020)	0.139 (0.076)
Local-firm advantage	0.001 (0.050)	0.061** (0.009)	0.059 (0.051)
Observations		103,157	

Panel B: Lowest-evaluated-price contests

	Quote/budget	Winning
Connection	-0.018** (0.001)	0.366** (0.021)
Local firm	-0.003** (0.000)	0.036** (0.008)
Observations	296,246	296,246
Firm FE		Yes
Contract FE		Yes

Notes: Panel A compares awards announced before and after arrival that were made after arrival under uninterrupted connections. Arrival-month notices are excluded (96 earlier, 20,635 later). The three columns are contrasts from one winning-bid model with firm and contract FE, Connection, Local firm, and their interactions with announcement timing. The test of the connection difference gives $p = 0.070$. Panel B uses contests with connected and unconnected bidders and positive quotes from every disclosed bidder. Quote/budget divides nominal RMB bids by the contract budget. All 63,699 winners submit the unique lowest raw quote. Both Panel B models include Connection, Local firm, firm FE, and contract FE. Winning estimates omit quote controls. Nominal quotes may differ from evaluated prices under Ministry of Finance (2017, art. 54). Standard errors cluster by destination and firm home city. * $p < 0.05$, ** $p < 0.01$.

Table D12: Arrival Estimates by Recorded Promotion and Leader Position

	Mayors		Party secretaries	
	Horizontal (1)	Promoted (2)	Horizontal (3)	Promoted (4)
<i>Contract value (asinh)</i>				
Connection $\times$ Post	0.588** (0.133)	0.740** (0.102)	0.703** (0.157)	0.635** (0.100)
Promotion difference p	0.355		0.727	
<i>Contract count (asinh)</i>				
Connection $\times$ Post	0.145** (0.030)	0.183** (0.023)	0.171** (0.037)	0.153** (0.022)
Promotion difference p	0.306		0.675	
Control variables	Yes	Yes	Yes	Yes
Event $\times$ relative month FE	Yes	Yes	Yes	Yes
Events	203	381	182	369
Observations	1,591,776	2,987,592	1,426,656	2,894,064

Notes: Promotion is matched to appointment position and start month. Horizontal appointments have no recorded rank increase. Each column estimates the adjusted model in Table 2 within the stated group. Difference p -values compare promoted and horizontal appointments within each position. Standard errors cluster by destination and treated-origin city. * $p < 0.05$, ** $p < 0.01$.

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