Connect to invest: Hometown ties, intercity capital flows, and allocative efficiency in China

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Abstract

This paper establishes a novel argument that social networks among local politicians reduce spatial frictions of corporate investment. We leverage the replacement of city officials and the resulting exogenous variations of hometown ties among city party secretaries to examine their impact on intercity capital flows in China. The results provide strong evidence that such connections significantly enhance capital flows between cities. These social bonds appear to effectively lower entry barriers for businesses and offer sustained support to connected firms without negatively impacting unconnected ones. Our research indicates that the increase in hometown-related investments does not displace non-hometown-related investments.

JEL classification: D25, D73, G11, L14

Keywords: hometown ties, capital flow, transaction costs, rent seeking, economic growth

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1 Introduction

Social networks exert a substantial influence on resource allocation (Jackson et al., 2017). Notably, the networks of influential elites, such as local politicians, can significantly affect the spatial distribution of resources and the resulting economic efficiency. However, due to data limitations, there is a lack of empirical evidence documenting these effects. In this study, we analyze a unique Chinese dataset of firm registrations to investigate the impact of social ties among city politicians on intercity capital flows within China. Our analysis sheds light on the nuanced role of politicians' social networks in reducing spatial frictions of corporate investment and improving allocative efficiency: they foster intercity investments and economic growth, but also give rise to rent-seeking opportunities and corruption.

In Chinese society, hometown ties, rooted in a shared sense of identity and trust, hold particular significance (Fei, 1947). We specifically investigate the hometown ties of city party secretaries, who occupy prominent positions as top city-level officials in China and often experience unexpected relocations and new appointments by higher-level governments. Political turnover may not be perfectly random, but the alignment of hometowns are arguably random, especially so across provinces, since it is highly implausible for the provincial leaders of two different provinces to intentionally appoint city party secretaries born in the same place. Such relocations and appointments offer exogenous changes in the hometown ties of politicians across different cities.[1]

Our paper exploits the setting of Chinese economy that features significant spatial frictions. On the one hand, the hukou system restricts labor spatial mobility. On the other hand, due to the complementarity of labor and capital and the huge institutional barrier of running business in a new environment, corporate investment flows are also subject to salient spatial frictions. Similar to the high-speed railway that effectively reduces such spatial frictions for corporate investment (Lin et al., 2023), we provide a novel argument that social networks among politicians in different localities also serve as lubricants to reduce spatial frictions of intercity corporate investment. In particular, social networks enable reciprocity among city leaders, who are willing to open the door mutually for corporate investment from connected peers.

We measure city-dyad capital flows using the capital registration data of new firms in destination cities where the owners hail from origin cities and supplement this measure using firm-to-firm equity investment data.[2] These data are sourced from

[1] In Table A.1 of the online Appendix, we demonstrate that major economic factors in either the origin or destination cities do not correlate with the presence of a common hometown tie among politicians from these cities. This observation supports the argument that variations in hometown ties are exogenous.

[2] An investment may be a case in which an investor in the origin city invests in a new firm in the destination city, or invests in an existing firm to be its new shareholder. We provide a detailed description in Section 3.

the universe of firm registration and changes in shareholding information recorded by the State Administration for Industry and Commerce (SAIC) of China. We employ a difference-in-differences approach to compare capital flows in city-dyads with and without such connections, while controlling for city-dyad fixed effects and year fixed effects to account for time-invariant heterogeneity and common year shocks.[3] To ensure the validity of our approach, we conduct event-study analyses that not only analyze changes in capital flows following the establishment of ties but also examine shifts after the severing of such ties, which confirm that our results are not driven by pre-existing trends.

We find a substantial 10% average increase in the total value of city-dyad investment flow when city party secretaries share a hometown. Hometown ties also positively impact firm registrations, leading to a 1% increase in the number of registrations and a 1% higher likelihood of firm registration between cities.[4] Conversely, when these ties are severed, city-dyad investment flows revert to mean levels. We find that the effects of hometown ties are much more significant compared to those of other types of social ties, such as college or workplace connections.

We find that the effects of hometown ties are more pronounced for cities in different provinces compared to those within the same province. This finding suggests that our results are unlikely driven by the coordinated appointments of party secretaries by higher-level governments, as it would be improbable for two different provinces to align on their choices of city party secretaries. Additionally, we mitigate the possibility of our results reflecting coordination within political factions, which capture political identity not social networks, in two ways. First, we control for the effects of four prominent political factions (the Communist Youth League of China (CYLC), Shanghai Gang, the Military, and the Princelings) following Francois et al. (2023). None of these factional linkages impact our results. Second, we conduct a more granular analysis of hometown ties by considering the birth cities of party secretaries and controlling for provincial hometown ties, which very much control for all birth-province-based factional linkages. This exercise again confirms the robustness of our findings. Moreover, our main results still hold after controlling for other connections and common background such as workplace ties and shared educational background, and high-dimensional fixed effects such as origin-year and destination-year fixed effects, which absorb all variations such as city-year-specific policies and high-speed railway connections. Thus, our results are not driven by all city-year-level factors such as coordinated policy choices and connections to road networks.

[3]We also control for other high-dimensional fixed effects in the regression for robustness checks, and the results are also robust.

[4]For robustness checks, we also employ the actual count of new firms as the dependent variable and conduct a Poisson Pseudo Maximum Likelihood (PPML) estimation and a negative binomial estimation, and the results are both qualitatively and quantitatively similar.

To understand the underlying mechanisms of the effects observed, we investigate the potential role of lowered barriers to entry as a result of hometown ties between local politicians and their impact on firm registrations. We first employ the provincial barrier to entry index developed by Brandt et al. (2020) to examine whether the effects of hometown ties are stronger in destination cities located in provinces with higher barriers to entry. Our results confirm our hypothesis. We then analyze the city-level share of state-owned enterprises (SOEs). A higher SOE share may indicate greater local protection and consequently higher barriers to entry for private firms. Our findings align with our hypothesis. We also find that our results are predominantly attributable to small and private firms, and, consistently, we provide evidence that firms from connected cities enjoy lower land acquisition prices, enabling them to acquire larger plots of land compared to those firms from nonconnected cities.

In addition to the potential impact of being connected on lowering barriers to entry, it is important to consider the ongoing support that firms from connected cities may receive. To explore this aspect, we analyze the effects of hometown ties on existing firms, allowing us to control for firm fixed effects and assess the impact of these ties on the same firm when it becomes connected due to political turnover. Connected firms refer to those whose legal representatives come from connected cities in a given year.[5] Our findings indicate that when hometown ties are formed, connected firms receive greater government subsidies than nonconnected firms. Furthermore, we consistently observe improvements in various aspects of firm performance, including sales, employment, fixed assets, total factor productivity (TFP), and patent applications. When such a connection is severed due to the relocation of officials, however, we observe a decline in the performance of previously connected firms. Despite the increased capital flow between connected cities and the improved performance of connected firms, we find no evidence of crowding-out effects on nonconnected firms. In fact, we do find that hometown connection increases the total amount of investments received in a city. This finding implies that the overall welfare impact of hometown ties may be positive.

To assess the robustness of our findings and further investigate the causal nature of hometown ties, we conduct placebo tests focusing on the period following the 2012 Chinese anti-corruption campaign. This campaign marked a significant turning point in terms of China's efforts to deter and punish corruption, with its primary objective being the deterrence of corrupt practices. However, it is important to note that the campaign also aimed to discourage any transfer of benefits through coalitions among politicians, irrespective of their corrupt intentions. Our placebo tests reveal that the effects of hometown ties on city-dyad investment become nonexistent in the post-

[5]Connected cities refer to cities where the party secretaries share a common hometown (thus are connected). In this sense, both party secretaries are linked to the connected firms.

campaign period. This finding aligns with the prediction of the rent-seeking mechanism.[6]

To gain further insights into the underlying incentives driving the increases in bilateral investments facilitated by hometown ties among city party secretaries, we explore two plausible motivations of local officials. First, we investigate the possibility that heightened investments contribute to local gross domestic product (GDP) growth, thereby increasing the probability of officials' promotion within the political hierarchy. Second, we delve into the potential involvement of rent-seeking practices amidst these investments.

We first present evidence indicating that the influence of hometown ties on bilateral investment is more pronounced between city pairs that have greater incentives for official promotion and between those with higher instances of corrupt officials. To further examine these motivations, we analyze the correlations between connected investment share in the destination city (reflecting investment proportions of cities with hometown ties) and three outcome variables: GDP growth, chance of promotion, and corruption investigation. Our findings indicate a positive correlation between connected investment share and both GDP growth and chance of promotion. We also observe a positive correlation between connected investment share and corruption investigation probabilities among city party secretaries. These results suggest that officials benefit from connected investments for both career prospects and personal gains. However, we find that while stronger political promotion incentives enhance the positive effects of connected investments on economic growth, corruption involvement undermines the growth benefits of hometown ties. This finding highlights the dual nature of hometown ties, which can lower transaction costs and foster economic growth while simultaneously enabling rent-seeking activities, ultimately diminishing the growth benefits of such connections.

Finally, we use a quantitative trade model to conduct a welfare analysis in Appendix A. The model is a static firm entry model a la Melitz (2003) and is adapted from Shi (2022a) and Liu et al. (2022). We establish a connection between hometown connections among city leaders and the entry cost, which, in turn, affects firms' entry decisions. It uses the total amount of new investments as a sufficient statistic for welfare and implies that hometown connections increase welfare by 6.89%. Lowering entry barriers and sustained support contribute by 65.6% and 34.4% of the welfare gain, respectively. Since these two mechanisms are the major ones we detect in our

[6]Consistently, our findings indicate that, prior to the campaign, connected firms acquired land at lower prices and secured larger parcels compared to their non-connected counterparts. However, these differences vanished in the post-campaign period. Additionally, we observed that the proportion of investments from connected firms positively influenced local economic growth, increased the odds of promotions for officials, and correlated with a higher incidence of corruption investigations. Yet, these effects were not observed in the post-campaign period.

empirical analysis, we assume that these two are the ones at play, and we decompose the welfare gain into these two sources.

Our paper first speaks to the large literature on the determinants of corporate investment. Our distinct contribution is to focus on a context with substantial intranational spatial frictions and the role of social networks. In this sense, our paper complements the work of Shi et al. (2021) and Lin et al. (2023), who find that political connections and transportation infrastructure can facilitate intercity capital flows in China. Moreover, most of the papers on cross-regional corporate investment focus on the international level (Stulz, 1981; Lucas, 1990; Gourinchas and Jeanne, 2013), and, thus, our paper enriches the small but growing field of the determinants and implications of intranational cross-regional corporate investment flows.

Our study also contributes to the comprehensive body of literature that investigates the implications of political connections. Numerous studies have documented the beneficial aspects of political connections to firms (Khwaja and Mian, 2005; Faccio et al., 2006; Cohen et al., 2008; Claessens et al., 2008; Amore and Bennedsen, 2013). However, recent literature has also shed light on the distortive effects these connections can have (Duchin and Sosyura, 2012; Bertrand et al., 2018; Haselmann et al., 2018; Schoenherr, 2019). Our study deviates from the majority of these works by examining the horizontal connections among politicians, as opposed to the more frequently explored vertical connections between politicians and firms.[7] While the influence of horizontal social ties on resource allocation is widely recognized (Greif, 1993; Rauch and Trindade, 2002; Olson, 2008; Burchardi and Hassan, 2013; Rehbein et al., 2020), empirical evidence specifically regarding these ties among politicians remains limited. This is primarily due to the difficulty in observing outcome variables such as inter-regional investment and trade.

Our study hones in on a notable social tie in Chinese culture – the hometown tie among politicians – and examines its implications for inter-city capital flows.[8] While investment is a pivotal driver of economic growth in China (Bai et al., 2006) and nearly half of these investments occur across cities in China, there remains a gap in our understanding of the factors determining these intercity capital flows. Our analysis reveals that social ties among city politicians significantly stimulate investments, thereby boosting economic growth. However, these same ties can also foster corruption. Consequently, our findings not only identify these social ties as a crucial factor propelling China's economic growth, but also underscore the dual existence of both beneficial

[7]A few studies examine the effects of horitonal connections. For example, Chen et al. (2021) show that personal connections between executives play a key role in supplier selection. Cohen et al. (2008) document that social networks between mutual fund managers and corporate board members matter for funds' investment decisions. But our work is one of the few that examine the horizonal connections among politicians.

[8]In another study that examines the effects of the hometown ties, Chu et al. (2021) find that these ties reduce the quality of government monitoring in China.

and detrimental aspects within this social network.

This study also aligns with research conducted by scholars examining the elite network in China, including, but not limited to, works by Shih (2007), Chen and Hong (2020), Fisman et al. (2020), Bai et al. (2022), and Francois et al. (2023).[9] However, diverging from the majority of preceding research which emphasizes vertical ties between politicians or between politicians and firms, our study highlights the significance of horizontal connections among Chinese politicians of equivalent rank. For instance, Jiang and Zhang (2020) delve into vertical patron-client networks within the Communist Party of China (CPC), analyzing the politics of fiscal transfers in China. Shi et al. (2021) probe the co-movement of firms instigated by the transfers of patron politicians, while Nian and Wang (2023) scrutinize the implications of firm-politician connections for land purchases and usage efficiency. In contrast, we focus on the connections among city politicians linked by shared hometown ties, and examine their influence on capital flows between cities.

The remainder of this paper is organized as follows. Section 2 introduces the institutional background. In Section 3, we discuss our data sources and the measurement of our main variables. Section 4 presents our empirical strategies, and Section 5 provides the results. Section 6 concludes the paper.

2 Institutional Background

2.1 Political appointments of local officials

The Chinese government consists of five layers: central, provincial, city, county, and township. A party secretary is the highest-ranking leader at each level of government. The Politburo Standing Committee of the Communist Party of China has ultimate decision-making power over the appointments of politicians at the provincial level (Jia et al., 2015); similarly, the appointments of local officials are controlled by the party committee at the next highest level (Guo, 2009). Although a few positions at the city level (i.e., the party secretary of Guangzhou) are appointed by the central government, most are directly controlled by the corresponding provincial governments (Xu, 2011). The promotion and demotion of local officials are generally heavily influenced by their past performance, but the process is often opaque, which prevents market participants and political observers from predicting political turnover (An et al., 2016).

[9] A related study by Shi (2022b) documents hometown favoritism in China. However, our paper diverges in focus as it does not examine hometown favoritism, and our findings remain robust even when controlling for hometown influences. Another study of relevance is that by Jiang and Mei (2020), which differs from ours as it considers the effects of a single rotating politician rather than two interconnected officials.

Political turnover may not be perfectly random, but the alignment of hometowns are arguably random, especially so across provinces. Such relocations and appointments offer exogenous changes in the hometown ties of politicians across different cities.

In our sample, the average tenure of a city party secretary is 4.3 years, with a 26% probability of promotion after tenure; approximately half of all city party secretaries were born outside of the province of their posting. City party secretaries command substantial power over local economic affairs and have autonomy in undertaking any lawful measure to promote growth (Li and Zhou, 2005; Xu, 2011). The performance-based evaluation system also sets an implicit age limit for each rank; thus, officials are incentivized to seek promotion as rapidly as possible to avoid premature and permanent career stagnation. For instance, the level directly above the city party secretary is the vice-provincial (or deputy minister) level, and the age of ineligibility for promotion to this level is 57 years (Kou and Tsai, 2014; Yu et al., 2016). This age constraint also incentivizes local politicians to improve local economic performance, which includes attracting investment from localities with which the politicians share social ties.

2.2 Anti-corruption campaign

After the 18th National Congress of the Communist Party of China (CPC) in 2012, President Xi Jinping initiated a far-reaching anti-corruption campaign targeting both "tigers" (i.e., high-ranking officials) and "flies" (i.e., local officials). The campaign has generally been regarded as highly effective in terms of curbing corruption (Qian and Wen, 2015; Lan and Li, 2018; Hao et al., 2020; Ding et al., 2020; Zhang, 2023). This campaign targeted not only corrupt dealings between government officials and private businesses but also coalitions among politicians, which are often based on social ties. A commentary in the People's Daily, the CPC's main newspaper, stressed that "*Corruption and factionalism are conjoined evils*" and that "*Some cliques of officials are in essence parasitic relationships that consist of transferring interests, and turning public power into private goods*" (People's Daily, 2015/01/05; New York Times, 2015/01/06). Anecdotal evidence shows that the campaign has, to a great extent, disrupted those corrupt coalitions that may have existed widely prior to the campaign. Below is an excerpt from a commentary published on November 06, 2014, on the CPC news:[10]

"The Central Inspection Team noted the clique culture in the officialdom of Guangxi, Sichuan, Jiangsu, and Hebei... in these cliques, people no longer regard each other as comrades but think of each other as brothers instead. They are no longer fellows who are emotionally attached but turn into corrupt gangs. Their ties have become a way of transferring benefits and are a root of corruption."

[10] http://cpc.people.com.cn/pinglun/n/2014/1106/c241220-25989189.html

It is important to emphasize that the campaign also aimed to discourage any transfer of benefits through coalitions among politicians, irrespective of their corrupt intentions. Although the transfer of benefits through social ties may still be occurring since the start of the campaign in 2012, possibly in less visible ways, the costs of doing so have substantially increased. Therefore, we can compare the effects of hometown ties on intercity capital flow before and after the start of the campaign to shed light on the nature of such ties.

2.3 Spatial frictions in China

Labor and capital flows in China are subject to spatial frictions—barriers and inefficiencies that hinder the optimal allocation and movement of these resources across different regions. These frictions have significant implications for economic growth, development, and inequality. For example, the household registration system (hukou) ties social benefits such as education, healthcare, and housing to one's place of birth. Migrant workers often lack access to these benefits in urban areas where they work, discouraging permanent migration and contributing to labor market segmentation. Due to the complementarity of labor and capital, corporate investment flows are also restricted across regions. Moreover, local governments may implement protectionist policies to favor local businesses and industries, creating barriers to the free flow of capital across regions. This can result in inefficiencies and misallocation of resources. However, in this paper, we argue that social networks among local leaders may alleviate spatial frictions for interregional corporate investment flows. We especially provide evidence that such networks can promote economic growth, but at the same time give rise to rent-seeking and corruption.

3 Data and Measurement

The sample of our empirical analysis covers 2000-2019. We obtain data on local officials from the China Center of Economic Research (CCER) at Peking University. These data contain the curricula vitae of the party secretaries and mayors of all cities in China. For clarity, we focus on the hometown ties of city party secretaries in our main text and include mayors when conducting robustness checks. Our main explanatory variable is a dummy variable that indicates whether the party secretaries of a pair of cities share a hometown province in a given year. We also use age, promotion, corruption, and other personal and career information of the officials contained in this dataset. In our sample, 4.8% of the city-dyad observations in the sample share a common hometown. In Table A.2 of the online appendix, we present more detailed statistics on the share of observations by the number of years in which two party secretaries share a common hometown. On average, approximately 10% of the city-dyads have ever shared a hometown. About 2% of the observations have party secretaries in two cities who have shared a hometown for more than five years.

We compute city-dyad investment flows using Chinese firm registration data provided by the SAIC database, which provides registry information for the universe of formal firms in China (approximately 20 million firms), covering each firm's location, establishment year, exit year (if any), amount of registry capital, and legal representative origins. According to China's *Company Law*, each company must have a legal representative who executes functions and holds power on behalf of the company.[11] The representative is typically the chairperson of the board of directors, executive director (if there is no board of directors), or general manager. The origin of the legal representative is defined by the first four digits of his or her national ID number, which remains unchanged throughout his or her lifetime. These four digits represent the person's city of residence when he or she obtains a national ID for the first time before reaching 18 years of age.[12]

We construct three variables to measure capital flows. First, we calculate the value of the aggregate registry capital of all firms established in a city that have legal representatives from another city during a given year. Second, we use the number of registrations of firms in a city that have legal representatives from another city during a given year. Third, we generate a dummy variable indicating whether the investment flow is strictly positive, which represents the probability of investment. The entire sample consists of 292 cities and 84,972 city-dyads. The dataset does not include data on the four centrally administered municipalities of China, namely, Beijing, Tianjin, Shanghai, and Chongqing. Every city pair is observed in two instances: once for investments originating from city A and destined for city B, and a second time for investments from city B directed towards city A.

We supplement this measure by incorporating data on equity investments between firms, sourced from the changes in shareholding information recorded in the SAIC database. The registered locations of the respective firms are used to establish the points of origin and destination. Utilizing the previously mentioned method, we cal-

[11] If a firm has more than one legal representative, then we account for each as a separate capital flow. For instance, if company A in Shanghai is represented by Wang from Beijing and Zhang from Wuhan, we recognize two distinct flows: one from Beijing to Shanghai and another from Wuhan to Shanghai, of equal value. Nevertheless, our main findings remain solid even when we modify our approach to count only one representative per firm. In such cases, we would randomly choose between representatives—for example, between Wang and Zhang—yet this selection does not significantly alter the outcome of our analysis.

[12] Technically, the location of residence associated with the national ID may not be perfectly the same as the location of residence when the investor is investing. However, on the one hand, information on the detailed location of business activities of the investors of the universe of firms in China is nonexistent; on the other hand, due to the hukou system, interregional labor mobility in China is rather low, and thus it is more likely that the locality of national ID is the one where the business activities occurred.

culate the aggregate value of equity transfers, the frequency of these transactions, and introduce a binary indicator that signifies the occurrence of a positive equity flow, all at the level of city pairs and by year.

We present the summary statistics in Table 1. As shown in Panel A, the mean of the log value of total capital flow is 1.397 (with a mean absolute value of 47.5 million RMB). The mean of the log value of firm registrations is 0.164 (with the mean absolute value being 17.4). In addition, 22.7% of the observations in the sample have strictly positive investment flows. The mean share of investments from officials with hometown ties is 22.1%. In our sample, approximately 42.1% of firm investment is made outside the city and 26.8% is made outside the province.

To investigate the barriers to entry channel, we employ data on the provincial firm barriers to entry index in 2004, sourced from Brandt et al. (2020). As land acquisition is a major entry cost for firms, we also utilize land transaction data from the Ministry of Land and Resources, which provides comprehensive information on land transactions in China. This dataset includes details such as the unit price of acquired land, size of the land acquired, and intended purpose of the land use.

We utilize multiple data sources to investigate the ongoing support received by connected firms. Our analysis relies on the Annual Survey of Industrial Firms (ASIF), a panel dataset covering all industrial SOEs and non-SOEs with annual sales exceeding 5 million RMB in China. We concentrate on the period from 2000 to 2011. In line with existing literature, we have excluded data from 2010, as the ASIF data for that year is deemed unreliable. For firm total factor productivity (TFP), our attention is directed to the period from 2000 to 2007, since the ASIF dataset does not provide reports on value added beyond 2007. We ascertain the origin of each firm in the ASIF by matching the ASIF data with that of the State Administration for Industry and Commerce (SAIC). This is achieved through Structured Query Language (SQL) techniques, which incorporate regular expressions and fuzzy matching for firm names, in conjunction with precise matches for location and sector. This process results in approximately 81% of observations in the ASIF data being matched. A firm is classified as connected if the party secretaries of its place of origin and current location share a common hometown. We examine the impact of these hometown connections on various firm performance indicators, including sales, employment, fixed assets, value-added taxes (VATs), corporate income taxes (CITs), and subsidies. Additionally, we calculate firm-level TFP following the approach outlined by Ackerberg et al. (2015). We also analyze patent filing data obtained from China's State Intellectual Property Office (SIPO). The SIPO dataset provides information on the names of patent assignees and their patent applications. We determine the place of origin for each patent assignee by linking the SIPO data with those of the SAIC. Similar to the registration data matching process, we employ the SQL approach utilizing regular expressions and fuzzy matching for firm names, along with exact matches for location and sector. The matching rate between the two datasets is 92.8%. Our main city-level control variables include city GDP and population, which are obtained from the Chinese City Statistical Yearbooks.

4 Empirical Strategy

Our main specification examines how dyadic intercity capital flows react to the hometown ties of city party secretaries. We use the following difference-in-differences strategy:

$$y_{ijt} = \alpha \; HometownTie_{ijt} + X_{ijt}\beta + \lambda_{ij} + \gamma_t + u_{ijt}, \tag{1}$$

where the dependent variable y_{ijt} is the log value of total new firm registry capital flowing from city i to city j in year t. We also examine the log number of new firm registrations and whether any new firms from city i register in city j in year t as our dependent variables. Our main explanatory variable $HometownTie_{ijt}$ is an indicator variable that is equal to 1 if the party secretaries of cities i and j in year t share a hometown tie and 0 otherwise. X_{ijt} is a vector of control variables that include the log value of per capita real GDP and the log of populations of both the origin and the destination city. λ_{ij} denotes city-dyad fixed effects, which are included to remove time-invariant heterogeneity across different city pairs; γ_t denotes year fixed effects, which are included to help remove any effects related to common year shocks. u_{ijt} is an error term that captures all other unobservables that influence intercity capital flows. As our main variation is at the city-dyad level, we cluster the standard errors at this level. For robustness checks, we also cluster the standard errors twoway at the origin city and destination city level, at the province-dyad level, and at different radiuses levels to address spatial correlation concerns (Conley, 1999).

Our main parameter of interest in equation (1) is α, which identifies the average causal effect of the hometown ties of party secretaries on capital flow between two cities. The main identification threat to our specification is that the appointments of city party secretaries are nonrandom. It is possible that the appointments of two local officials are influenced by factors that directly affect intercity investment. We address this concern in three ways. First, given the importance of the position of city party secretaries, it is highly unlikely that a secretary's hometown would be a determinant of such an appointment. Second, we study the capital flow between cities that are located in two different provinces. As discussed in the background section, city party secretaries are appointed at the provincial level. It is unlikely that two provinces would coordinate on their respective choices of party secretaries in their respective provinces. Third, we conduct event studies to determine whether cities with and without connected officials have different pretrends for our main outcome variables. We use the following flexible specification that allows for the effects to differ over time:

$$y_{ijt} = \sum_{\tau \neq -1} \alpha_\tau \; HometownTie_{ijt} \times \rho_{ij,t-\tau} + X_{ijt}\beta + \lambda_{ij} + \gamma_t + u_{ijt}, \tag{2}$$

where all variables are the same as in equation (1) except that we replace the main explanatory variable with a set of interaction terms $HometownTie_{ijt} \times \rho_{ij,t-\tau}$. $\rho_{ij,t-\tau}$ is a dummy indicating whether $t - \tau$ is the first year that cities i and j shared a hometown connection (if $\tau < 0$) and party secretaries remain the same from $t - \tau$ to t (if $\tau > 0$). To ensure clarity, we concentrate on the subset of observations where the party secretaries with hometown ties remain in their positions following political turnover. This approach allows us to analyze the enduring effects of hometown ties, denoted as α_τ. We specifically test whether the coefficients α_τ for $\tau < 0$ are nonsignificant, utilizing the year immediately preceding the replacement of party secretaries who share a hometown tie as the reference year.

In subplots (a), (b), and (c) of Figure 1, we plot the coefficients of $-3 \leq \tau \leq 3$, $\tau \leq -4$, and $\tau \geq 4$, for log registered capital, log number of firm registrations, and the probability of having any positive investment, respectively. We observe that the effects of hometown ties between party secretaries in two cities not only persist but also intensify over time.

We then introduce a new dummy variable, denoted as $TieSeverance_{ijt}$, which equals 1 after the hometown tie is severed and 0 before that. We replace $HometownTie_{ijt}$ with this new dummy variable in our event study. By examining the sample of observations where the hometown-connected party secretaries remain in position prior to political turnover, we focus on the effects following the severance of hometown ties. We anticipate these effects to be opposite to those of $HometownTie_{ijt}$. Subplots (d), (e), and (f) of Figure 1 display the coefficients for log registered capital, log number of firm registrations, and the probability of positive investment, respectively. Consistent with our expectation, we find that tie severance has a negative impact.

To capture overall effects, we conduct a third analysis. In this exercise, we retain all observations and replace the main explanatory variable with $HometownTieEver_{ijt}$ which is equal to 1 when two cities have ever been connected prior to year t and 0 otherwise. For example, if for one pair of cities, hometown ties of party secretaries were formed in 2005 but severed in 2008, then we still define $HometownTieEver_{ijt}$ as 1 in all years after 2008 for this city pair. This approach allows us to incorporate the effects of tie severance. In subplots (g), (h), and (i) of Figure 1, we depict the coefficients for log registered capital, log number of firm registrations, and the probability of having any positive investment, respectively. The comparison between the results using different samples reveals that the effects vanish once the hometown tie is severed.

In light of the recent discussion in the literature on staggered difference-in-differences methods, in the online appendix, we also follow De Chaisemartin and d'Haultfoeuille (2020) to plot the estimates in Figure A.1 and follow Borusyak et al. (2022) to plot the estimates in Figure A.2 for robustness checks. We find that our results remain highly consistent.

5 Results

5.1 Baseline results

Table 2 presents the results of the baseline specification using data from period 2000-2011. In columns (1) to (3), we focus on the log value of total newly registered firm capital between city pairs in a given year. Moving on to columns (4) to (6), we examine the log number of firm registrations, while columns (7) to (9) study the probability of having any positive amount of capital registration. Our regressions initially include city-dyad fixed effects and year fixed effects to control for unobserved heterogeneity and time-specific factors. We then introduce the log of per capita real GDP and the log of populations for both the origin and destination cities to account for economic and demographic factors. To address concerns related to political turnover, we include a set of dummy variables indicating the year of the city party secretaries' tenure, thereby removing any effects associated with such turnover. As our main variation is at the city-dyad level, we first cluster standard errors at this level and report the results in parentheses.

We are interested in the estimated coefficients of $HometownTie_{ijt}$, as they capture the causal impact of hometown ties between city party secretaries on intercity capital flow. Our findings indicate that these hometown ties have a significant and positive effect on city-dyad investment, with an approximate increase of 10%. We also observe a 1% increase in firm registrations and a 1% higher probability of receiving any investment from the connected city due to these hometown ties. These results remain highly robust even after including additional controls.

Furthermore, we address potential spatial correlation in capital flows across cities to mitigate any overestimation of significance levels. We first twoway cluster standard errors at the origin city and destination city levels. Then, following Conley (1999), we account for spatial correlations by considering capital investment within province-dyads, within a 200 kilometer radius, within a 300 kilometer radius, and within a 500 kilometer radius. We report the corresponding standard errors in brackets, which further confirm the robustness of our results.

Throughout the paper, we use the log (1+x) transformation. In Table A.3, we demonstrate the robustness of our results to alternative forms of log transformations, namely log (0.01+x), log(0.001+x), and the inverse hyperbolic sine transformation. For further robustness checks, in Table 3, we follw Chen and Roth (2024) to employ the actual count of new firms as the dependent variable and conduct a Poisson Pseudo Maximum Likelihood (PPML) estimation and a negative binomial estimation, and the results are both qualitatively and quantitatively similar. On average, a hometown connection results in an increase of 3-5 in firm registrations.

5.2 Robustness checks

We conduct a number of robustness checks on our results. First, our study focuses solely on hometown ties, setting aside other forms of social ties, which may lead to bias. To address this concern, we include two additional major forms of social ties, namely, common workplaces and common colleges, in our analysis as robustness checks. Table 4 presents the results of these checks. In columns (1) to (3), we respectively examine the effects of college ties, workplace ties, and the combined effects of college ties, workplace ties, and hometown ties on city-dyad capital flows. We find that the effects of college and workplace ties are much smaller compared to those of hometown ties, indicating that hometown ties have a more significant impact on intercity capital flows. The magnitudes of the effects of hometown ties remain robust, after including these additional controls. Moving on to columns (4) to (9), we investigate the effects on firm registrations and the probability of firm entry. The findings in these columns are consistent with the previous results, further supporting the robustness of the effects of hometown ties.

Second, there is a concern that hometown ties might be a proxy for broader political connections or factional affiliations, potentially reflecting factional cooperation and competition rather than the ties themselves. To address this concern, we explicitly control for the effects of political factions in China, following Francois et al. (2023), who identify the CYLC, Shanghai Gang, Military, and Princelings as the four prominent factions. In this exercise, we include a dummy variable that is equal to 1 if the two party secretaries in the city pair belong to the same faction. Additionally, we examine both the main effects of factions and their interactive effects with hometown ties. The results are presented in Table 5. We find that none of the prominent factions significantly influence bilateral capital flows among Chinese cities. This finding suggests that the observed effects are driven primarily by hometown ties between party secretaries, rather than by broader factional dynamics.

Third, we conduct additional robustness checks in Table 6 to further strengthen our analysis. Columns (1) to (4) focus on city-dyad capital flows. In column (1), we introduce a set of party secretary characteristics as control variables, including a gender dummy, education-level dummies, ethnicity dummy, and age of the party secretary, to account for their potential influence. This approach helps ensure that the observed effects are not driven by these characteristics of individual party secretaries. In column (2), we include origin-year fixed effects and destination-year fixed effects to remove any effects associated with political turnovers or investment shocks in either the origin or destination cities. By doing so, we isolate the specific effects of hometown ties from those of general political changes or local economic shocks within cities. Column (3) takes a different approach by replacing party secretary characteristics with party-secretary fixed effects. Thus, the analysis exploits the rotations of the same individuals across different cities. Column (4) goes one step further by incorporating both horizontal and vertical factional linkages. The horizontal factional linkage is a dummy variable that indicates whether the two city party secretaries belong to the same faction, either in the CYLC, Shanghai Gang, Military, or Princelings. The vertical factional linkage is a dummy variable that is equal to 1 if the party secretary of either the origin or destination city shares the same political faction as the provincial party secretary or chief of the respective province. This approach helps account for potential factional dynamics and their interactions with hometown ties. Columns (5) to (8) repeat the same exercise for log firm entries, examining the robustness of our findings regarding firm registrations. Columns (9) to (12) replicate the analysis for the probability of firm entry, further ensuring the robustness of our results.

Fourth, in the main text, we consider the birth city of firms' legal representatives as the origin city of capital flows. One might be concerned that business owners may not stay in their birth place. To mitigate the concern, we follow Lin et al. (2023) to examine the effects on firm-to-firm investment flows across Chinese cities. An investment occurs when a firm contributes capital to another firm and thereby becoming its shareholder. We have the registered location of two firms and can therefore clearly define the origin and destination cities. We follow our main specification and present the results in Table 7. We find the effects to be highly consistent with our main findings.

Fifth, in Table 8, we adopt a more granular approach to studying the effects of hometown ties while controlling for birth-province-based political factions. We refine the definition of hometown ties by considering two party secretaries as sharing a hometown if they were born in the same city, rather than in the same province. Odd-numbered columns follow the specification in equation (1) but replace $HometownTie_{ijt}$ with $CityHometownTie_{ijt}$. This allows us to examine the effects of hometown ties at the city level. Even-numbered columns go a step further by additionally controlling for $HometownTie_{ijt}$. This helps us assess the specific contributions of city-level hometown ties beyond the effects captured by provincial hometown ties. The results show that the effects of hometown ties remain strong even when considering hometown ties at this more granular level.[13]

[13]Since our investment data is at the city level, we use the city-level connections as the most granular level of hometown ties.

Sixth, to address the concerns about the potential influence of hometown favoritism (Shi, 2022b) on our results, we incorporate dummy variables indicating whether the party secretary of either city governs the other's hometown. We present the results in Table A.4. The results demonstrate that the effects of hometown ties remain robust and significant even after accounting for the presence of hometown favoritism. This finding suggests that the impact of hometown ties on intercity capital flow is distinct from the effects of hometown favoritism.

Seventh, we extend our analysis to include hometown ties between city mayors and between mayors and party secretaries. We examine both scenarios: including all hometown ties across different local officials and excluding observations with other local officials' ties. The results, presented in Table A.5, confirm the robustness of our main findings. These additional dimensions of hometown ties provide further evidence of the significance of social networks and personal connections in driving intercity capital flows.[14]

Finally, we aggregate hometown ties and investment at the destination city level. The explanatory variable is the number of hometown ties, while the outcome variables include the total amount of capital investment in a city and the total number of firm registrations in a city. We present the results in columns (1) and (2) of Table A.7. Our results remain highly robust at the city level.

5.3 Mechanisms

5.3.1 Barriers to entry

The existing literature on social ties has extensively studied their impact on reducing transaction costs (Burchardi and Hassan, 2013; Rehbein et al., 2020), primarily through mechanisms such as information sharing and decreasing the barriers to entry. Building on this literature, we investigate this channel in two ways. First, we analyze the effects on capital flows separately for private firms and state-owned enterprises in China, considering the higher entry barriers faced by the former. In Table 9, columns (1) and (2) present our findings, which reveal that the observed effects primarily stem from the capital flow of private firms. This result supports the notion that hometown ties play a crucial role in mitigating entry barriers for private firms, facilitating their capital investment across cities. The null result of SOE is consistent with the fact that investment decisions of state firms in China are less influenced by investment oppor-

[14]For another robustness check, we also control for the hometown fixed effects of the two party secretaries of both the origin and destination cities. It addresses the concern that people from certain provinces behave differently compared to others. The results are reported in Table A.6 and are highly robust.

tunities (Chen et al., 2011).[15]

In columns (3) and (4), we examine the intercity capital flow within a province and across provinces, respectively. Our findings indicate that the effects of hometown ties are more pronounced in interprovincial city-dyad investments. This result provides additional support for the hypothesis that hometown ties contribute to lowering entry barriers if there exists more information asymmetry for cross-province investments. Moreover, the results in column (2) demonstrate that our findings are not driven by the coordinated appointments of party secretaries, as it is unlikely that different provinces will coordinate on the selection of city party secretaries. While we acknowledge the potential for competition among connected officials within the same province, which may reduce bilateral investment and explain our results, the evidence of capital flow within provinces nonetheless confirms that the promotion of intercity capital flows by hometown ties is strong.

Columns (5) and (6) analyze capital flow within city pairs, categorized by distance: below and above the sample median. Results consistently show stronger effects of hometown ties in city pairs with greater distances. This suggests that hometown ties may reduce entry barriers, as there can be higher information asymmetry between more distant cities. Our results support the story that connected city pairs implement more friendly policies for investment from each other. Distant investments incur a large cost and a higher entry barrier, and, thus, are more responsive to such policies.[16]

In columns (7) to (9) of our analysis, we focus on the number of firm registrations categorized by size: large, medium-sized, and small firms. Large firms represent the top 10% of firms in terms of registry capital (greater than 10 million RMB), medium-sized firms encompass the top 10% to 50% of firms (registry capital between 0.1 million and 10 million RMB), and small firms represent the bottom 50% of firms (registry capital below 0.1 million RMB).[17] Given that larger firms typically have more resources and information advantages compared with smaller firms to overcome the entry barrier, we expect the effects of social ties to be stronger for small firms. Again, our results support the story that connected city pairs implement more friendly policies for investment from each other. Larger firms are relatively insensitive to such policies but smaller firms are. Our results do not support the story in which city leaders bring certain specific large firms to enter their jurisdictions, but they establish business-friendly

[15]Different SOEs are associated with different levels of the local or the central government. For city governments, they only have control over SOEs below the city level, which are relatively smaller in size and economic significance. Moreover, it is institutionally infeasible for two city party secretaries to coordinate to initiate intercity investment to create a new SOE, especially across provinces.

[16]Frictions of intercity investment also increase with the distance between the origin and the destination cities. Thus, a more salient effect between more distant cities indicates that social networks among city party secretaries reduce spatial frictions of investments.

[17]We also try different cutoffs for the definition of large, medium-sized, and small firms, and the results are still qualitatively similar.

policies to all firms from the connected city in general. This is, again, built on reciprocity.

One important barrier to entry could be land acquisition. In columns (10) and (11) of our analysis, we explicitly study the effects of hometown ties on firms' land acquisition prices and acquired land area to show direct evidence on the mechanism. By utilizing data from the Ministry of Land and Resources, we compare entering firms from connected cities with those from nonconnected cities, while controlling for city-dyad fixed effects and land usage dummies. Our findings indicate that hometown ties have a significant impact on reducing land acquisition prices for connected firms entering the market. This finding suggests that firms with hometown ties benefit from lower costs when acquiring land for their operations. Moreover, we observe a positive effect of hometown ties on the land acquisition area, indicating that connected firms tend to acquire larger pieces of land compared to their nonconnected counterparts. These results align with our hypothesis of hometown ties lowering entry barriers.

We further investigate the entry barrier channel by utilizing the provincial firm entry barrier index developed by Brandt et al. (2020). We use the entry barrier index in 2004 for our analysis, which is calculated using the 2004 Chinese Industrial Census data and a Hopenhayn (1992) model. We divide provinces into two categories: those with high-entry barriers and those with low-entry barriers.[18] This division allows us to examine the effects of hometown ties based on the investment destination city's location within these different barrier categories.

In columns (1) to (3) of panel A of Table 10, we focus on observations where the investment destination city is situated in a province with a high level of entry barriers. Here, we find that hometown ties have a significant effect on intercity capital flow, indicating that the ties play a crucial role in facilitating investment in areas with higher entry barriers. Conversely, in columns (4) to (6), we shift our attention to observations where the investment destination city is located in a province with a low level of entry barriers. In this case, we do not observe significant effects of hometown ties on intercity capital flow, suggesting that the impact of hometown ties is more pronounced in areas where entry barriers are more substantial.

In our earlier analyses, we show that these effects are driven by the investment of private firms, which suggests that the prevalence of SOEs relative to non-state firms can be used as a proxy for entry barrier against private firms. In panel B of our analysis, we divide the observations based on the SOE share in the investment destination city. The SOE share is calculated based on the firm registration data recorded by the SAIC in each city in 2000. In columns (7) to (8) of panel B, we focus on those observations where the investment destination city has an SOE share that is higher than the national median. Here, we find robust and positive effects of hometown ties on

[18]The cutoff is sample median value, but the results are also robust to alternative cutoff values.

intercity capital flow. Conversely, in columns (10) to (12) of panel B, we focus on observations where the investment destination city has an SOE share below the national median. In this case, we do not find significant effects of hometown ties on intercity capital flow. These results suggest that hometown ties play a significant role in promoting investment in cities where SOEs have a larger presence, which aligns with hypothesis concerning barriers to entry.

5.3.2 Crowding-out effects

One immediate question arising from the fact that hometown ties lower entry barriers for connected investment is whether these investments would crowd out other investments. If this is not the case, then hometown ties and the related connected investment could actually promote economic efficiency. The notable effects observed among small firms suggest that crowding-out effects are likely to be minimal.

To directly test for potential crowding-out effects, we investigate whether the volume of investments tied to hometown-city dyads influences other investments originating from different cities to the destination city. We employ the following strategy:

$$y_{ijt} = \alpha \; ConnectedInvestment_{jt} + X_{ijt}\beta + \lambda_{ij} + \gamma_t + u_{ijt}, \tag{3}$$

where $ConnectedInvestment_{jt}$ represents the total investment received by city j from all cities with which it shares hometown connections in year t. We then retain only those city pairs that are not connected. The dependent variable, y_{ijt}, encompasses the non-connected investments from city i to city j in year t. This exercise allows us to examine whether connected investment in a destination city may result in the crowding out of other investments.

We subsequently substitute the primary explanatory variable with one that signifies the total investment originating from city i to all its connected cities in year t. This alteration allows us to investigate whether the connected investment from an origin city could potentially crowd out investments intended for other cities.

In Table 11, we present the results. In column (1), we focus on the observations where two cities do not share hometown ties, while keeping the observation unit as city-dyad-year. The main explanatory variable is the log investment from hometown-tied cities, which we find to be uncorrelated with investments from alternative origins. Similarly, in column (2), we keep the same observations but replace the explanatory variable with the log investment to hometown-tied cities. We find that this variable is also uncorrelated with investments to alternative destinations.[19]

It is plausible that an uptick in investment from one city to another could coincide

[19]Since the amount of connected investment is an endogenous object, we do not make causal claims here but only estimate correlations.

with a decrease in investment within the originating city, and vice versa. This raises questions about whether observed increases in bilateral investment genuinely affect capital allocation. To address this, we explore the impact of hometown connections on locally registered capital, which symbolizes the total investments made by investors from the origin city within that same city. The results are displayed in columns (3) and (4) of the table, and the analysis is carried out at the city-year level. In column (3), we find no correlation between the presence of other cities where party secretaries share a common hometown and the log value of the local registered capital. This suggests that there is no decrease in registered capital within a city by its own residents when such connections exist. Further, in column (4), we investigate if the number of hometown ties a city shares with other cities correlates with the log value of total locally registered capital. Once again, we find no significant correlation. These findings collectively suggest that the extent of hometown ties does not influence the overall level of investments made by investors from the origin city within their own city.

In order to delve deeper into the potential crowding-out effects and the impact of hometown ties on capital allocation, we analyze the correlations between hometown connections and the total capital directed towards a city in a given year. Our analysis uncovers strong positive correlations both between the hometown tie dummy variable and the total capital inflow to a city, and between the number of hometown ties and the same metric. These findings imply that the existence of hometown ties, as well as the quantity of such connections, are associated with increased levels of capital inflow.[20]

Given these findings, it is improbable that the increases in bilateral investments stimulated by hometown ties have overshadowed other investments. The positive correlations suggest that hometown ties do indeed play a role in shaping capital allocation and contribute to the overall surge in registered capital. Finally, we employ a quantitative trade framework to evaluate the welfare effects in Appendix A. The framework uses the total amount of investment as a sufficient statistic for welfare. The non-existence of crowding-out effects and the monotonic increase in total investment implies a higher level of welfare and allocative efficiency.

5.3.3 Continuing support

Another potential mechanism is that the hometown ties of local politicians may help connected firms receive continuing support. Connected firms refer to those whose legal representatives come from connected cities in a given year. If lasting support is anticipated, a firm would be more likely to establish operations in a city when a connection is formed. We then analyze firm performance based on whether firms

[20]The estimate presented in column (4) differs from the one reported in column (1) of Table A.7 due to the inclusion of additional control variables in the analysis.

become connected as a result of political turnover in the city. To conduct this analysis, we require panel data on firms, allowing us to track their performance over time, as well as information on firms' origins. To accomplish this, we merge the firm data from the ASIF, which provides panel data on industrial firms, with the firm registration data from the SAIC database.[21] This matching process is performed using fuzzy firm names and exact location and industry. We are able to successfully match 81% of the observations in the ASIF dataset.

By combining these datasets, we can examine the performance differences between firms that become connected due to political turnover in the city and those that do not have such connections. This analysis helps illuminate whether hometown ties provide enduring support to the connected firms and contribute to their performance. We use the following difference-in-differences specification to examine firm performance:

$$y_{fijt} = \alpha \ HometownTie_{fijt} + X_{ijt}\beta + \lambda_f + \lambda_{ij} + \gamma_t + u_{ijt}, \tag{4}$$

where y_{fijt} represents the performance of firm f, which originates from city i and operates in city j in year t. For each firm f in each year t, we know whether cities i and j are connected through party secretaries' hometown ties. If they are connected, then $HometownTie_{fijt}$ is equal to 1; otherwise, it is equal to 0. Our performance measures include the logarithm of firm sales, the logarithm of firm employment, and the logarithm of firm fixed assets. For these measures, we have data for all years from 2000 to 2011 except for 2010. To capture changes in firm productivity, we compute firm-level TFP following the approach outlined in Ackerberg et al. (2015). For TFP, however, we are able to compute firm TFP only from 2000 to 2007, as the ASIF dataset does not report firm value added from 2008 onward. Additionally, we study the effects on whether firms file patents using firm patent data and whether firms de-register using SAIC data.

To explicitly examine the policy support received by connected firms, we include variables such as firm remitted VAT, CIT, and government subsidies received by the firm. To account for individual firm characteristics, we control for firm fixed effects. Moreover, to capture the specific dynamics of each city dyad, we incorporate city-dyad fixed effects. In essence, we analyze how these outcome variables change within the same firm when it becomes connected as a result of political turnover at the city level.

In Panel A of Table 12, we present the results of our analysis. Our findings reveal that hometown ties have a significant positive impact on firm performance, as evidenced by increases in firm sales, employment, and fixed assets. Moreover, connected firms receive higher government subsidies, indicating the provision of policy support to these firms. We also find that connected firms' TFP increases and that they are more

[21] Firms included in the ASIF database are a subset of those included in the SAIC database.

likely to file a patent. Consistently, we find that connected firms are less likely to exit or de-register than are nonconnected firms.

To address concerns regarding pretrends, we conduct event studies to examine the effects of both the formation and severance of connections. These event studies allow us to assess whether the observed changes in firm performance are specifically attributable to the establishment or termination of hometown ties. In subplots (a) to (f) of Figure 2, we present the results of the event studies for various firm outcomes, including firm sales, employment, fixed assets, subsidies, TFP, and patent-filing probability. In subplots (g) to (l) of the figure, we show the dynamic effects when the ties are severed. Our findings indicate that there are no pretrends leading up to the formation or the severance of connections. Instead, we observe that the effects on firm performance begin precisely at the time when the connection is formed or severed, suggesting a direct causal relationship between hometown ties and firm outcomes.

To investigate the possibility of crowding-out effects on nonconnected firms, we conduct a placebo analysis focusing on the performance of firms that are not directly connected by hometown ties. For each firm f in each year t, we know whether there exists any other firm that originated in city i and operates in city j that is connected through party secretaries' hometown ties. We redefine $HometownTie_{fijt}$ to be 1 if there is any other firm that is connected and 0 otherwise. We remove the connected-firm observations from our sample and compare the performance of the remaining firms before and after the existence of firms with hometown ties between the two cities.

The results of this analysis are presented in panel B of Table 12. Our analysis indicates that the establishment of hometown ties has no significant effects on these firm performance or government policy outcome variables for nonconnected firms. These results suggest that the improved performance observed among the connected firms unlikely comes at the expense of the performance of nonconnected firms.

5.3.4 Politicians' incentives

Officials who share hometown ties naturally have closer relationships and superior access to information, which can facilitate and promote bilateral investment between their respective cities. This information-sharing mechanism could be one of the driving forces behind the observed positive effects. However, this mechanism is less testable, as we do not have direct observations of the information being exchanged among officials. Nor do we observe the entering firms' quality relative to those that chose not to enter in the connected cities.

Nonetheless, another testable explanation is that local politicians, at least, may have incentives to actively promote and facilitate bilateral investment between their cities. By doing so, they could reap personal benefits, gain political advantages, or engage in other forms of rent-seeking activities resulting from this increased investment activity.

First, we investigate the possibility of a transfer of benefits between connected officials by examining the effects of hometown ties on intercity capital flow following the initiation of the anti-corruption campaign in China in 2012. This campaign aimed to target both individual instances of corruption and any transfer of benefits through political coalitions, irrespective of corrupt intentions.

If the effects of hometown ties continue even after the campaign, it suggests that their influence extends beyond the personal interests of politicians. This could indicate the presence of other forms of support or information sharing between individuals connected through shared hometowns.

Table 13 presents the results for the period from 2012 to 2019. In contrast to the findings before the anti-corruption campaign discussed in the main text, we observe that hometown ties no longer have a significant impact on city-dyad capital flows. This finding suggests that the personal incentives of politicians, likely involving benefit transfers, may be an important cause of the capital flow in the pre-campaign period. Likewise, in Table A.10, we find no effects of hometown ties on the land acquisition price or area of the entering connected firms.[22] While we make conservative argument to attribute the difference between pre-2012 and post-2012 pattern completely to the anti-corruption campaign, we are assured that such a comparison, together with all other evidence, is at least consistent with the story of corruption and rent-seeking enabled by hometown networks.

Subsequently, we delve into two specific forms of personal incentives for local officials: political promotion and corruption. We investigate the relationship between connected investment and local economic growth. Higher levels of investment can boost economic performance, which, in turn, could enhance the chances of an official's promotion. Next, we analyze the correlation between connected investment and the likelihood of a corruption investigation. Investments tied to hometown connections could potentially be more susceptible to corruption opportunities.

Table 14 reports the results. Panel A of the table presents the OLS results. In columns (1) to (2), we examine the effects of hometown-tied investment on city GDP growth. The main explanatory variable is "share of connected investment," which is defined as the amount of a city's received investment from connected cities as a share of total investment received. We expect that hometown ties will positively influence economic performance through connected investment. The results confirm this expectation, showing strong and positive correlations between connected investment shares and city GDP growth in the pre-campaign period, but not in the post-campaign

[22]In Table A.11, we include both pre- and post-campaign periods in one analysis and use a triple difference strategy to test different effects in these two periods.

period.[23]

Columns (3) to (4) focus on the promotion probability of party secretaries at the end of their terms. We find strong and positive correlations between connected investment shares and the probability of promotion in the pre-campaign period, but not in the post-campaign period. Columns (5) to (6) further examine the probability of city party secretaries being prosecuted for corruption by the end of 2021. For party secretaries who assumed their positions before 2012, we observe a positive correlation between connected investment shares and the probability of prosecution for corruption. However, for party secretaries who assumed their positions after 2012, such effects are no longer present.[24]

In Panel B of our analysis, we employ an instrumental variable approach to further examine the effects of hometown ties on investments. We use the presence of a hometown tie between a city party secretary and a party secretary in any other Chinese city as the instrumental variable. By conducting two-stage-least-squares (2SLS) regressions, we find that our results are highly consistent and robust, supporting a causal interpretation. These findings suggest that personal incentives may play a significant role in shaping the effects of hometown ties on investments.

5.4 Further discussion

5.4.1 Symmetry of effects

The influence of local politicians' hometown ties on firm investment may or may not exhibit symmetry. And whether the investments are made based on reciprocal principle is beyond the scope of this study. It is important to note that even if the investment amounts between origin and destination cities are equal, as long as these investments are driven by reduced entry barriers, they should still positively impact economic growth.[25] Also, the findings presented in Table 11 indicate an increase in total investment within the destination city, which negates the notion that investment symmetry would result in zero net investment.

We conduct a statistical analysis to show some evidence on whether the effects are driven by one-direction investments between a set of special cities. For each of the 84,972 city pairs, we randomly choose an origin city and a destination city for

[23]Similarly, we examine the correlations between the number of hometown ties, capital investment, and firm registrations at the destination city level in the post-campaign period. We present the results in columns (3) and (4) of Table A.7. In contrast to the results in columns (1) and (2) for the pre-campaign period, no correlations exist in the post period.

[24]In Table A.8 and Table A.9, we first present evidence indicating that the influence of hometown ties on bilateral investment is more pronounced between city pairs that have greater incentives for official promotion and between those with higher instances of corrupt officials.

[25]More importantly, our analysis suggests that hometown ties increase the absolute amount of capital investment and new firms, thus contributing to economic growth.

this study. Each selection generates 509,832 observations, which is half the number of observations in our primary results that consider each city pair twice. We conduct regression for each sample selection and repeat this process 500 times. We then plot histograms of the estimates in Figure A.3 for log firm capital, log firm registration, and the likelihood of any firm registration. The distribution of these estimates aligns closely with our primary result estimates, suggesting that the effects of hometown ties are statistically symmetric. It should be noted that the outcomes presented are purely statistical. However, the impact of these hometown connections might differ depending on the attributes of the destination cities. This is a subject we will explore in more detail in the following subsections.[26]

5.4.2 Investment destination

In the previous analyses, we have demonstrated that hometown ties of local officials have positive effects on total local capital investment and local GDP growth. However, concerns may arise regarding the potential misallocation of resources across different cities and distortions in the aggregate economic efficiency. Specifically, the lower entry barriers and ongoing support to connected firms may lead to investments in locations that are not ideal for certain industries or without favorable factors of production, resulting in inefficiencies, even when such resource allocation does not entail crowding-out effects. While a comprehensive analysis of the impact of hometown ties on aggregate economic efficiency is beyond the scope of this study, we present evidence below to address the concern of potential distortions. Specifically, we focus on the role of hometown ties of local officials in facilitating a better match between firms and their destinations, suggesting that the impact of these ties is unlikely to result in significant distortions in resource allocation.

To examine the investment patterns of connected investors in relation to the sectors of the destination city, we analyze whether these investors make more investments in sectors where the destination city has a comparative advantage. We define a city as having a comparative advantage in a sector if its output share in that sector, based on the 2-digit Chinese Industry Classification, is above the national median. Conversely, if the output share is below the national median, the city is considered to have a comparative disadvantage in that sector. The comparive (dis)advantage is a revealed one in the sense that a sector with a larger (smaller) volumn reveals itself with comparative (dis)advantage. This captures both the geographic concentration of the sector in the city and potential advantages, such as lower production costs or significant external spillovers. To conduct this analysis, we split firm registrations based on whether the

[26] To be more specific, the symmetry of effects also relies on whether the origin city and the destination city have similar or symmetric characteristics.

investment is made in a city-sector-year cell where the destination city's output share in the sector is above or below the national median. The output shares for each city in each 2-digit sector are computed using data from the ASIF in 2000. We then aggregate the firm investments by city-dyad-year cells for further analysis.

We present the results in Table 15. In columns (1) to (3), we focus on the effects of hometown ties on investments in sectors where the destination cities have a comparative advantage. On the other hand, columns (4) to (6) examine the effects of hometown ties on investments in sectors where the destination cities do not have a comparative advantage. Our findings indicate that the effects of hometown ties are significant and positive only in destination cities with sectors that have a comparative advantage.

Next, we investigate how the effects of hometown ties vary with two factors of production in the destination cities: road density and skill-labor intensity. Road density is measured as the ratio of paved road area to the total area of the destination city in 2000, while skill-labor intensity is calculated using data from the 2000 Chinese population census, representing the ratio of college-educated individuals to the total population.

Table 16 presents the results. We find that the effects of hometown ties are stronger in destination cities with higher road density or higher skill-labor intensity. These findings indicate that firms are more inclined to invest in locations that offer favorable factors of production. Thus, the results imply that social networks may improve allocative efficiency, by guiding investments to more productive locations and raising the matching efficiency between investments and locations.

Overall, our evidence suggests that hometown ties unlikely lead to investments in suboptimal locations. On the contrary, the findings more align with the notion that hometown ties sustain a higher level of economic efficiency by attracting investments in more appealing locations.

5.4.3 Heterogeneous growth implications

The findings presented in Table 14 demonstrate that although connected investment may entice corruption, its overall impact remains positive for economic growth. However, it is crucial to investigate the conditions under which hometown ties exhibit positive growth effects and those under which they exhibit negative effects. To explore this heterogeneity, we shift our focus back to the political incentives of politicians and corrupt practices, examining their implications at both the firm and city levels. By doing so, we aim to gain a deeper understanding of the factors that influence the diverse effects of hometown ties on economic outcomes.

To investigate the impact of political incentives on the effects of hometown ties, we utilize the ages of city party secretaries as a proxy for their career advancement prospects. In the Chinese political system, there is an implicit age limit for each rank, and city party secretaries below the age of 57 years have a stronger incentive to promote investment to enhance their chances of promotion (Kou and Tsai, 2014; Yu et al., 2016).[27] We examine the heterogeneous effects of hometown ties by comparing the outcomes when the city party secretary is below 57 years old (indicating stronger political incentives) versus when the city party secretary is 57 years old or older (indicating weaker political incentives).

We also explore the influence of corruption. Although we cannot directly observe corruption incentives, we utilize data on the corruption investigations of city party secretaries until 2021. It enables us to compare the effects of hometown ties on growth between corrupt city party secretaries and those who have not been implicated in corruption cases. By examining these dimensions of heterogeneity, we aim to gain insights into the conditions under which hometown ties have varying effects on economic growth.

Table 17 presents the results of our analysis. We first focus on entering firms in each year in each city. Notice that here we want to understand how the different incentives of politicians alter the mix of entering firms. In columns (1) and (2), we examine the effects of hometown ties on whether a firm files a patent. The dependent variable is 1 if the entering firm has ever filed a patent after entry (until the last year of our patent dataset, 2019) and 0 otherwise. We compare firms that enter from connected cities with those from nonconnected cities and explore how these effects vary depending on whether the city's party secretary has strong political promotion incentives or has been implicated in corruption activities by 2021. Our findings indicate that political incentives amplify the positive effects of hometown ties on entering firms patent filing behaviors, while corruption undermines these effects.

Similarly, in columns (3) and (4), we focus on the TFP growth of firms within the first 2 years of entry. Once again, we focus on entering firms in each year in each city. The results reinforce the same pattern, highlighting that political incentives enhance the positive impact of hometown ties on firm TFP growth, either through selection or lasting support, while corruption weakens these effects.

Column (5) of the table further examines the relationship between shares of connected investment and GDP growth of a city. We find that the positive correlation between investment share and GDP growth differs by political promotion incentives of the city party secretary and the presence of corruption. Specifically, strong promotion incentives amplify the positive effect of hometown ties on GDP growth, while corruption weakens this effect.

Columns (6) to (7) of the table examine the effects of hometown ties on patent

[27] City party secretaries older than 59 are restrained by formal institutions not having any chances of promotion. However, they de facto lose their chances of promotion two years before the age of 59.

growth and trademark growth, which serve as alternative measures of economic growth. The results consistently demonstrate that promotion incentives have a positive impact on growth, while corruption has a negative impact. These findings reinforce the notion that hometown ties may facilitate corruption, as indicated in the earlier results in Table 14, thereby diminishing the growth benefits associated with these connections.[28]

Lastly, we use a quantitative trade model to conduct a welfare analysis. We present the full model in Section A of the online appendix. The model is a static firm entry model a la Melitz (2003) and is adapted from Shi (2022a) and Liu et al. (2022). We establish a connection between hometown connections among city leaders and the entry cost, which, in turn, affects firms' entry decisions. It uses the total amount of new investments as a sufficient statistic for welfare and implies that hometown connections increase welfare by 6.89%. Lowering entry barriers and sustained support contribute by 65.6% and 34.4% of the welfare gain, respectively.[29]

6 Conclusion

In this study, we examine the impact of social networks on capital allocation and economic efficiency, focusing on the role of hometown ties among Chinese local officials. By utilizing unique firm registration data and leveraging the relocation patterns of these officials, we uncover substantial effects of hometown ties on intercity capital flow, surpassing the influence of college ties or workplace ties. Our findings reveal that hometown ties lead to a 10% increase in city-dyad investments and a 1% rise in firm registrations.[30]

We investigate two possible mechanisms underlying these effects. First, hometown ties may lower the barriers to entry of connected firms, enabling them to access resources and opportunities more easily. Second, hometown ties may provide lasting support and advantages to connected firms through anticipated political backing. We find no evidence of crowding-out effects, further supporting the notion that hometown ties contribute to positive economic outcomes. Consistently, our analysis demonstrates a positive correlation between investments from connected cities and the GDP growth of Chinese cities.

[28]We use two-stage-least-squares estimation to provide a conservative argument on causality. The results are qualitatively similar using OLS estimation.

[29] Since these two mechanisms are the major ones we detect in our empirical analysis, we assume that these two are the ones at play, and we decompose the welfare gain into these two sources.

[30]Using a PPML and a negative binomial estimation, we find that hometown ties increase bilateral investment flows by approximately 12% of the standard deviation.

While prior literature has presented both positive and negative effects of social networks, our research provides a more nuanced understanding of these networks. By comparing the effects before and after the anti-corruption campaign, and by exploring the heterogeneous effects that vary by corruption practices, we show that these networks can enable rent-seeking activities among local officials that ultimately undermine growth. By unraveling the complex interplay among social ties, rent-seeking activities, and economic efficiency, we also shed light on the mechanisms that shape resource allocation within a social network. This nuanced perspective opens up avenues for future research to delve deeper into the underlying mechanisms through which social networks influence economic outcomes and explore potential policy implications regarding the harnessing of the positive aspects of these networks while mitigating the negative ones.

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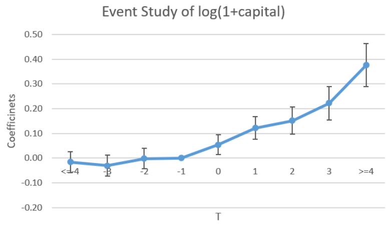
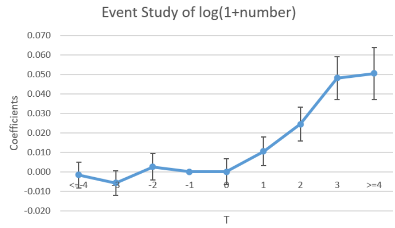
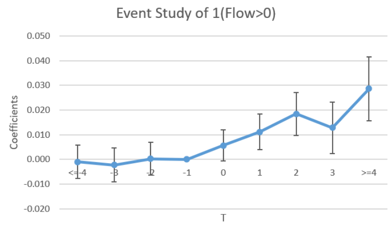


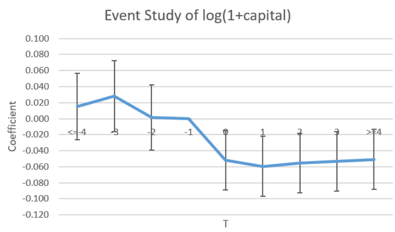
(a) Capital, connection formed



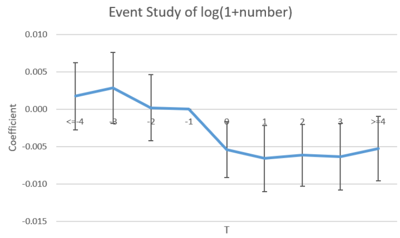
(b) Registries, connection formed



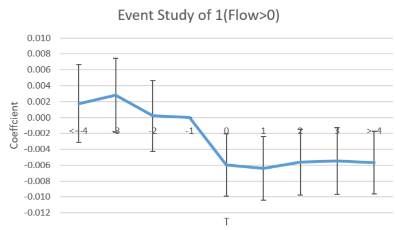
(c) 𝟙(Flow>0), connection formed



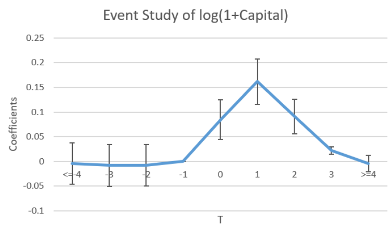
(d) Capital, connection severed



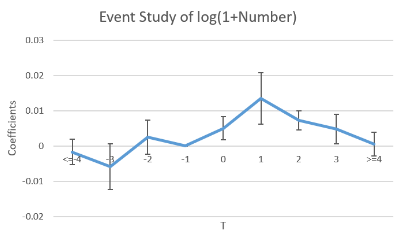
(e) Registries, connection severed



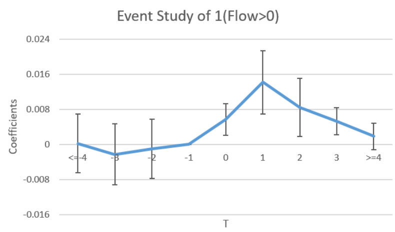
(f) 𝟙(Flow>0), connection severed



(g) Capital, overall effects



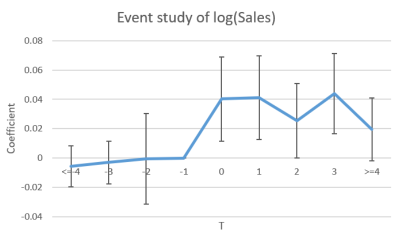
(h) Registries, overall effects



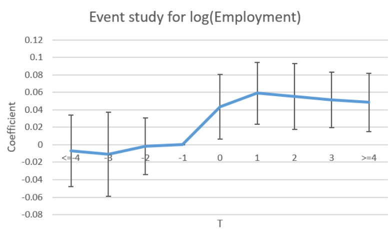
(i) 𝟙(Flow>0), overall effects

Figure 1: Event Studies on Connection Formation and Severance

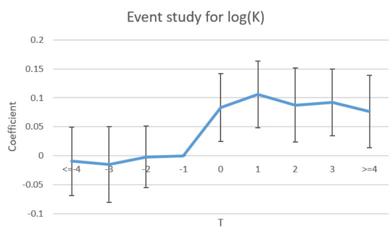
Notes: The figure illustrates the dynamic effects of the treatment (connection formation and severance) on the log value of newly registered total firm capital, log number of newly registered firms, and probability of new firm entry between city dyads. The observations are at the city-dyad-year level, and the sample covers 84,972 city dyads from 2000 to 2011. The figure presents three exercises separately. In the first exercise (sub-figures a, b, c), we examine the effects of hometown ties between two cities when the hometown-connected party secretaries remain in their positions. In the second exercise (sub-figures d, e, f), we analyze the effects when one of the hometown-connected party secretaries leaves office, resulting in the severance of the connection. For clarity, we only include observations where the two cities in the control group never formed a connection, and we start counting the observations from the year when the connection was formed if it existed. In the third exercise (sub-figures g, h, i), we consider the effects of hometown ties as long as two party secretaries in two cities shared a common hometown at any time before the time of observation. The horizontal axis represents the years since the city dyad experienced the treatment, with 0 indicating the first year of treatment. The vertical axis represents the regression coefficients for the dynamic effects, obtained using the specification shown in equation (2). The capped spikes indicate the 95% confidence interval, with standard errors clustered at the city-dyad level. Each estimated effect is compared to the base year, which is one year prior to the treatment and standardized to 0.



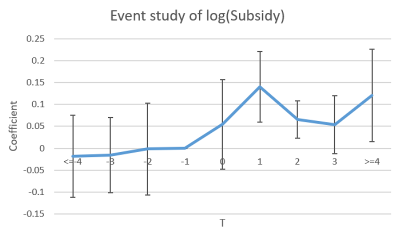
(a) Sales, connection formed



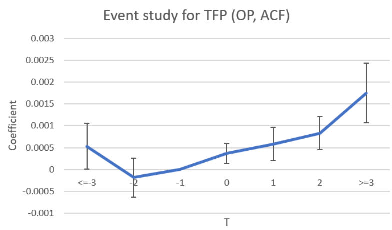
(b) Employment, connection formed



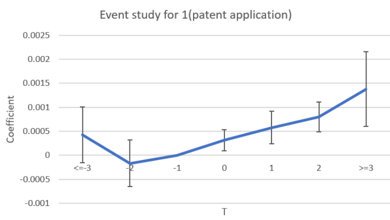
(c) Fixed asset, connection formed



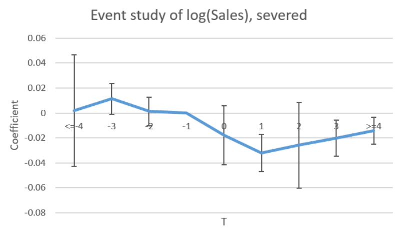
(d) Subsidy, connection formed



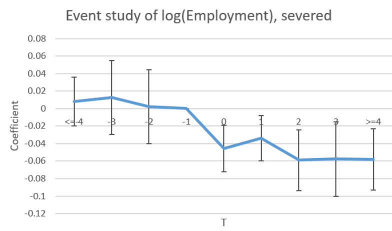
(e) TFP, connection formed



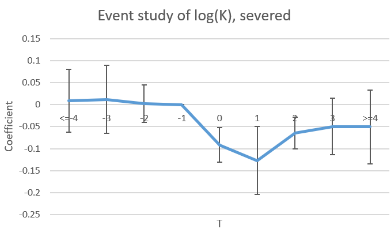
(f) Patent, connection formed



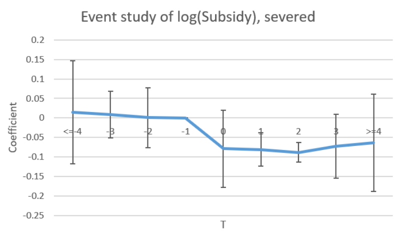
(g) Sales, connection severed



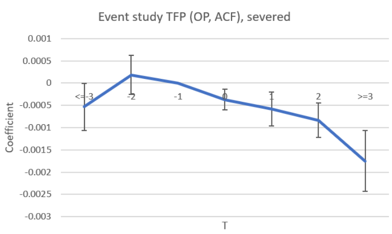
(h) Employment, connection severed



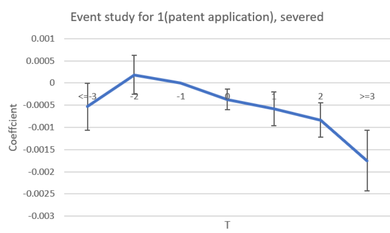
(i) Fixed asset, connection severed



(j) Subsidy, connection severed



(k) TFP, connection severed



(l) Patent, connection severed

Figure 2: Event Studies on Existing Connected Firms' Outcome Variables

Notes: The figure illustrates the dynamic effects of the treatment (connection formation and severance) on various firm outcomes, including firm sales, employment, fixed assets, government subsidy, total factor productivity (TFP), and patent filing. The outcome variables are measured in log values, except for patents, which is a binary variable indicating whether a firm files a patent in a given year. The observations are at the firm-year level, spanning from 2000 to 2011 (excluding 2000), except for firm TFP. When we examine the dynamic effects on connected firms' TFP when connections are formed and when they are severed, we focus on the firm-year level data from 2000 to 2007. The horizontal axis represents the years since the city dyad experienced the treatment, with 0 representing the first year of the treatment. The vertical axis shows the regression coefficients for the dynamic effects. The capped spikes indicate the 95% confidence interval, with standard errors clustered at the city-dyad level. Each estimated effect is compared to the base year, which is one year prior to the treatment and standardized to 0.

Table 1: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Panel A: City dyad data set					
Capital (1 million RMB)	1,048,575	47.546	78.103	0	8,886,110
Number	1,048,575	17.446	42.508	0	22,026
log(1+Capital)	1,048,575	1.397	2.809	0	16.261
log(1+Number)	1,048,575	0.164	0.619	0	10.444
1(Flow>0)	1,048,575	0.227	0.419	0	1
Hometown tie	1,048,575	0.048	0.214	0	1
Panel B: Land transaction data set					
1og(Unit land price)	153,294	5.317	1.736	-0.150	14.914
log(Area)	153,296	0.671	0.805	0	6.909
Panel C: Firm level data set					
Sales (1,000 RMB)	3,096,979	142480.200	1377059.000	-32799	4.77E+08
Employment	3,068,537	286.856	1290.297	0	569670
Capital (1,000 RMB)	3,091,853	27927.120	774500.600	-2E+06	1.00E+09
VAT (1,000 RMB)	3,054,296	4823.957	76210.830	-2E+06	2.62E+07
CIT (1,000 RMB)	2,081,414	2390.829	128097.900	-43188	4.77E+07
Subsidy (1,000 RMB)	2,160,873	310.990	8528.734	-2E+06	4811285
log(TFP)	3,043,677	-0.001	1.107	-13.268	9.268
1(Patent)	2,907,001	0.025	0.156	0	1
1(Exit)	3,098,032	0.146	0.353	0	1
Panel D: City data set					
GDP growth	5,624	0.122	0.093	-1.068	1.385
Share of connected investment	4,392	0.211	0.234	0	1
Panel E: Official data set					
1(Promotion)	5,624	0.166	0.372	0	1
1(Corrupt)	697	0.099	0.299	0	1

Table 2: The Effects of Hometown Ties on Firm Capital Flow (2000-2011)

Dep. var.:	Log firm capital			Log firm registries			1(Flow>0)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Hometown Tie	0.0990***	0.0970***	0.0821***	0.0118***	0.0118***	0.00933***	0.00933***	0.0119***	0.00965***
	(0.0125)	(0.0125)	(0.0130)	(0.00238)	(0.00238)	(0.00248)	(0.00194)	(0.00194)	(0.00201)
Twoway cluster at origin and destination	[0.0191]***	[0.0190]***	[0.0198]***	[0.00248]***	[0.00247]***	[0.00254]***	[0.00202]***	[0.00202]***	[0.00206]***
Clustered at province-dyad	[0.0189]***	[0.0189]***	[0.0195]***	[0.00245]***	[0.00245]***	[0.00252]***	[0.00200]***	[0.00200]***	[0.00204]***
Spatially correlated, 200km	[0.0184]***	[0.0183]***	[0.0190]***	[0.00251]***	[0.00250]***	[0.00255]***	[0.00202]***	[0.00203]***	[0.00207]***
Spatially correlated, 300km	[0.0190]***	[0.0190]***	[0.0199]***	[0.00257]***	[0.00257]***	[0.00260]***	[0.00205]***	[0.00205]***	[0.00209]***
Spatially correlated, 500km	[0.0193]***	[0.0193]***	[0.0203]***	[0.00261]***	[0.00261]***	[0.00263]***	[0.00208]***	[0.00209]***	[0.00211]***
City dyad FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y	N	Y	Y
Tenure FE	N	N	Y	N	N	Y	N	N	Y
Observations	1,019,664	1,019,664	977,550	1,019,664	1,019,664	977,550	1,019,664	1,019,664	977,550
R-squared	0.064	0.065	0.067	0.050	0.051	0.053	0.050	0.051	0.052
Number of city dyads	84,972	84,972	81,510	84,972	84,972	81,510	84,972	84,972	81,510

Notes: The analysis is conducted at the city-dyad-year level using a sample of 84,972 city dyads covering the period from 2000 to 2011. All columns include controls for city-dyad fixed effects and year fixed effects. Additionally, the controls include the logarithm of per capita real GDP and the logarithm of population for both the origin and destination cities. Tenure fixed effects include a set of dummy variables that indicate the number of years that each of the party secretaries has been in position. We first cluster standard errors at the city-dyad level as this is the level of variation of our analysis, and we report them in the parentheses. We then cluster standard errors twoway at the origin city and destination city levels. Next, we consider spatial correlations by examining capital investment within province-dyads, within a 200 kilometer radius, within a 300 kilometer radius, and within a 500 kilometer radius, following the approach outlined by Conley (1999). We report the corresponding standard errors in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 3: Alternative Empirical Specification

Dep. var.	Firm registries		
	(1)	(2)	(3)
Specification:	OLS	PPML	Negative binomial
Hometown Tie	3.145***	5.327***	4.461***
	(0.422)	(1.325)	(0.770)
City dyad FE	Y	Y	Y
Year FE	Y	Y	Y
Controls	Y	Y	Y
Observations	1,019,664	1,019,664	977,550
R-squared	0.064	0.065	0.067
Number of city dyads	84,972	84,972	84,972

Notes: The analysis examines the effects of hometown tie on the number of firm registrations using alternative specifications. Column (1) uses ordinary least squares. Column (2) uses Poisson Pseudo Maximum Likelihood (PPML) regressions. Column (3) uses a negative binomial regression. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 4: Robustness Check: Effects of Other Forms of Social Ties

Dep. var.:	Log firm capital			Log firm registration			$\mathbb{1}$(Flow>0)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
College Tie	0.0210*		0.0219*	0.0042**		0.0031	0.0021		0.0012
	(0.0130)		(0.0141)	(0.0024)		(0.0031)	(0.0020)		(0.0024)
Workplace Tie		0.0119	0.0100		0.0022	0.0020		0.0012	0.0015
		(0.0101)	(0.0122)		(0.0015)	(0.0017)		(0.0011)	(0.0013)
Hometown Tie			0.110***			0.0119***			0.0213***
			(0.0034)			(0.0034)			(0.0041)
City-dyad FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664
R-squared	0.066	0.065	0.066	0.051	0.051	0.051	0.051	0.051	0.051
Number of city dyads	84,972	84,972	84,972	84,972	84,972	84,972	84,972	84,972	84,972

Notes: The sample consists of 84,972 city dyads from 2000 to 2011, and the observation is at the city-dyad-year level. A college tie between two officials is defined when they graduated from the same college, while a workplace tie is established if they have shared work experience in the same government agency. In all columns, we include city-dyad and year fixed effects. Control variables comprise the log per capita real GDP and log population of both the origin and destination cities. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 5: Controlling for Four Prominent Factions and Groups Within the CCP (Francois et al., 2023)

	(1) Log firm capital	(2) Log firm registries	(3) 𝟙(Flow>0)
Hometown Tie	0.1130***	0.0124***	0.0221***
	(0.0032)	(0.0033)	(0.0044)
Hometown Tie × CYLC	0.0013	-0.0069	-0.0042
	(0.0114)	(0.0070)	(0.0051)
Hometown Tie × Military	0.0003	0.0010	0.0016
	(0.0002)	(0.0009)	(0.0022)
Hometown Tie × Shanghai Gang	-0.0012	-0.0008	-0.0057
	(0.0007)	(0.0008)	(0.0072)
Hometown Tie × Princelings	0.0103	0.0022	0.0013
	(0.0121)	(0.0019)	(0.0014)
CYLC	0.0003	0.0004	-0.0006
	(0.0007)	(0.0003)	(0.0011)
Military	-0.0055	0.0227	0.0020
	(0.0051)	(0.0163)	(0.0017)
Shanghai Gang	-0.0038	-0.0015	0.0005
	(0.0036)	(0.0023)	(0.0005)
Princelings	0.0006	0.0000	0.0002
	(0.0006)	(0.0008)	(0.0031)
City dyad FE	Y	Y	Y
Year FE	Y	Y	Y
Controls	Y	Y	Y
Observations	1,019,664	1,019,664	1,019,664
R-squared	0.065	0.051	0.051
Number of city dyads	84,972	84,972	84,972

Notes: Following Francois, Trebbi, and Xiao (2023), we incorporate control variables to capture the influence of four major factions within the Chinese Communist Party (CCP): CYLC (Communist Youth League of China, associated with General Secretary Hu Jintao), Shanghai Gang (mainly affiliated with Jiang Zemin and benefiting from Shanghai's political significance), Princelings (children of high-ranking party officials and revolutionary veterans during Mao's era), and Military (politicians with a military background). The sample consists of 84,972 city dyads from 2000 to 2011, and the observation is at the city-dyad-year level. All columns include city-dyad and year fixed effects. Control variables encompass the log per capita real GDP and log population of both the origin and destination cities. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 6: Robustness Check: Controlling For More Fixed Effects

Dep. var.:	Log firm capital				Log firm registries				$\mathbb{1}$(Flow>0)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Hometown Tie	0.0860***	0.0714***	0.0534**	0.0538**	0.0103***	0.0084***	0.0048***	0.0048**	0.0104***	0.0084***	0.0047*	0.0047**
	(0.0144)	(0.0125)	(0.0232)	(0.0218)	(0.0025)	(0.0035)	(0.0026)	(0.0025)	(0.0026)	(0.0030)	(0.0027)	(0.0021)
City dyad FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Tenure FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Party secretary controls	Y	Y	N	N	Y	Y	N	N	Y	Y	N	N
Origin-year FE	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Destination-year FE	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Party secretary FE	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y
Horizontal factional linkage	N	N	N	Y	N	N	N	Y	N	N	N	Y
Vertical factional linkage	N	N	N	Y	N	N	N	Y	N	N	N	Y
Observations	977,550	977,550	977,550	977,550	977,550	977,550	977,550	977,550	977,550	977,550	977,550	977,550
R-squared	0.071	0.092	0.110	0.113	0.061	0.076	0.099	0.104	0.069	0.085	0.102	0.105
Number of city dyads	81,510	81,510	81,510	81,510	81,510	81,510	81,510	81,510	81,510	81,510	81,510	81,510

Notes: The analysis is conducted at the city-dyad-year level, using a sample of 84,972 city dyads spanning the period from 2000 to 2011. All columns include city-dyad and year fixed effects to control for specific city and year factors. Tenure fixed effects are also incorporated, using dummy variables to capture the number of years each of the party secretaries has been in office. Control variables consist of log per capita real GDP and log population for both the origin and destination cities. Party secretary characteristics, including gender, education level, ethnicity, and age, are included as additional controls. To account for political and economic shocks affecting investments, origin-year fixed effects and destination fixed effects are also included. The horizontal factional linkage variable is a dummy that indicates whether the two city party secretaries belong to the same faction (CYLC, Shanghai Gang, Military, or Princelings). The vertical factional linkage variable is a dummy that is equal to 1 if either the origin or destination party secretary shares the same faction as the provincial party secretary or chief of the respective province. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 7: Robustness Check: Firm-to-firm Investment Flows

	Firm-to-firm investment flows as in Lin et al. (2023)		
	Log investment value	Log investment number	𝟙(Investment flow>0)
	(1)	(2)	(3)
Hometown Tie	0.0731***	0.0440**	0.0321**
	(0.0189)	(0.0213)	(0.0155)
City dyad FE	Y	Y	Y
Year FE	Y	Y	Y
Controls	Y	Y	Y
Observations	1,019,664	1,019,664	1,019,664
R-squared	0.064	0.051	0.052
Number of city dyads	84,972	84,972	84,972

Notes: The analysis is conducted at the city-dyad-year level, using a sample of 84,972 city dyads in the period of 2000 to 2011. We follow Lin et al. (2023) to measure firm-to-firm investment flows for each city pair in each year. An investment occurs when a firm contributes capital to another firm and thereby becoming its shareholder. We use the registered location of the two firms to define the origin and destination city. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 8: Robustness Check: City Hometown Ties

	Log firm capital		Log firm registration		𝟙(Flow>0)	
	(1)	(2)	(3)	(4)	(5)	(6)
City Hometown Tie	0.138***	0.119***	0.0196***	0.0137***	0.0119***	0.0102***
	(0.0229)	(0.0244)	(0.0033)	(0.0037)	(0.0019)	(0.0020)
Hometown Tie	N	Y	N	Y	N	Y
City dyad FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Observations	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664
R-squared	0.045	0.048	0.039	0.041	0.040	0.043
Number of city dyads	84,972	84,972	84,972	84,972	84,972	84,972

Notes: The analysis is conducted at the city-dyad-year level, using a sample of 84,972 city dyads in the period of 2000 to 2011. The explanatory variable City Hometown Tie takes a value of 1 if the party secretaries of two cities share a common city hometown. The control variable Hometown Tie is a dummy variable indicating whether the two party secretaries share a common province hometown. All columns include city-dyad and year fixed effects to account for specific city-pair and year factors. Control variables consist of log per capita real GDP and log population for both the origin and destination cities. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 9: Examining the Entry Barrier Channel

Dep. var.:	Log firm capital						Log firm registries			Log land price	Log land area
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Sample:	SOE	Private	Intra-province	Inter-province	Close cities	Far cities	Large	Medium	Small	All	All
Hometown Tie	-0.0026	0.1020***	0.0553**	0.0912***	0.0724***	0.1050***	0.0003	0.0010*	0.0046**	-0.1270***	0.1320***
	(0.0058)	(0.0125)	(0.0264)	(0.0138)	(0.0211)	(0.0157)	(0.0002)	(0.0006)	(0.0022)	(0.0219)	(0.0069)
Equality test:			p = 0.068		p = 0.079						
City dyad FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Land usage dummies	N	N	N	N	N	N	N	N	N	Y	Y
Observations	1,019,664	1,019,664	41,736	977,928	432,048	587,616	1,019,664	1,019,664	1,019,664	141,495	141,497
R-squared	0.000	0.070	0.185	0.061	0.084	0.051	0.000	0.003	0.045	0.054	0.193
Number of city dyads	84,972	84,972	3,478	81,494	36,004	48,968	84,972	84,972	84,972	NA	NA

Notes: In columns (1) to (9), the analysis is conducted at the city-dyad-year level, using a sample spanning the period from 2000 to 2011. The analyses in columns (10) and (11) are conducted at the firm-year level in the period from 2000 to 2011. Column (1) analyzes the total capital flow of entering firms that register as state-owned enterprises in the city dyad. Column (2) focuses on the total capital flow of entering firms that register as private firms in the city dyad. Columns (3) and (4) examine the capital flow within city pairs that are within the same province and those across different provinces, respectively. Columns (5) and (6) in the analysis focus on the capital flow within city pairs based on their distance, specifically distinguishing between city pairs with distances below the sample median and those above the sample median. Columns (7) to (9) consider different firm sizes, with large firms having registry capital greater than 10 million, medium firms with registry capital between 0.1 million and 10 million, and small firms with registry capital lower than 0.1 million. Columns (10) and (11) investigate the land acquisition price and acquired area by the connected firms entering the city dyad. All columns include city-dyad and year fixed effects. Controls consist of log per capita real GDP and log population for both the origin and destination cities. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 10: Further Examining the Entry Barrier Channel

Panel A: by Investment Destination Province Entry Barrier

	Entry barrier>=sample median			Entry barrier<sample median		
	(1)	(2)	(3)	(4)	(5)	(6)
	Log firm capital	Log firm registries	$\mathbb{1}$(Flow>0)	Log firm capital	Log firm registries	$\mathbb{1}$(Flow>0)
Hometown Tie	0.1160***	0.0214**	0.0324***	0.0554	0.0064	0.0061
	(0.0584)	(0.0097)	(0.0090)	(0.0566)	(0.0071)	(0.0083)
Equality test:				p = 0.012	p = 0.033	p = 0.046
(b/w high- and low-barrier provinces)						
Observations	481,440	481,441	481,442	538,224	538,224	538,224
R-squared	0.065	0.050	0.051	0.065	0.050	0.051
Number of City Dyads	40,120	40,120	40,120	47,200	47,200	47,200

Panel B: by Investment Destination City SOE share

	SOE share>=sample median			SOE share<sample median		
	(7)	(8)	(9)	(10)	(11)	(12)
	Log firm capital	Log firm registries	$\mathbb{1}$(Flow>0)	Log firm capital	Log firm registries	$\mathbb{1}$(Flow>0)
Hometown Tie	0.1320***	0.0202**	0.0177**	0.0953	-0.0004	0.0112
	(0.0581)	(0.0086)	(0.0090)	(0.0579)	(0.0089)	(0.0086)
Equality test:				p = 0.017	p = 0.020	p = 0.031
(b/w high- and low-SOE cities)						
Observations	667,680	667,680	667,680	351,982	351,982	351,982
R-squared	0.034	0.027	0.020	0.024	0.026	0.016
Number of City Dyads	55,640	55,640	55,640	47,200	47,200	47,200

Notes: The observation is at the city-dyad-year level. All columns include city-dyad fixed effects and year fixed effects, controlling for the log per capita real GDP and log population of both the origin and destination cities. In Panel A, the sample is divided based on the destination province's entry barrier in 2004, as determined by Brandt et al. (2020). The entry barrier was calculated using the 2004 Chinese Industrial Census data and a Hopenhayn (1992) model. In Panel B, the observations are divided based on the SOE share in the investment destination city. The SOE share is calculated using the firm registration data recorded by SAIC in each city in the year 2000. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 11: Crowding-out Effects of Connected Investment

Dep. var.:	Log firm capital		Log local registered capital		Log all capital	
	(1)	(2)	(3)	(4)	(5)	(6)
Log investment from hometown-tied cities	0.0021					
	(0.0115)					
Log investment to hometown-tied cities		-0.0034				
		(0.0043)				
Have any hometown ties			0.0034		0.0258***	
			(0.0050)		(0.0050)	
Number of hometown ties				0.0058		0.0122***
				(0.0052)		(0.0025)
City dyad FE	Y	Y	N	N	N	N
City FE	N	N	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Sample	City-dyad-year		City-year		City-year	
Observations	970,720	970,720	3,504	3,504	3,504	3,504
R-squared	0.065	0.065	0.335	0.320	0.383	0.383

Notes: In columns (1) and (2), the observation is at the city-dyad-year level. We include city-dyad and year fixed effects and exclude investments between connected cities in our sample. In columns (3) to (6), the observation is at the city-year level, so we control for city and year fixed effects. "Log local registered capital" refers to the total amount of capital registered by new businesses in a city where the legal representative is also from the same city. "Log all capital" refers to the total amount of capital registered by new businesses in a city. Controls include log per capita real GDP and log population of both the origin and destination cities. Standard errors, clustered at the city-dyad level, are reported in parentheses in columns (1) and (2). Standard errors, clustered at the city level, are reported in parentheses in columns (3) to (6). ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 12: Effects of the Hometown Tie on Existing Firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Effects of Connection on Connected Firms									
	Log sales	Log emp	Log capital	Log VAT	Log CIT	Log subsidy	Log TFP	$\mathbb{1}$(Patent)	$\mathbb{1}$(Exit)
Hometown Tie	0.0325**	0.0438*	0.0646**	0.0122	0.0226	0.0959***	0.0343***	0.0060**	-0.0044***
	(0.0158)	(0.0255)	(0.0285)	(0.0138)	(0.0357)	(0.0128)	(0.0025)	(0.0025)	(0.0010)
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3,071,688	3,005,891	3,013,047	3,000,569	2,014,796	2,174,566	1,310,549	3,519,826	3,519,826
R-squared	0.0285	0.0290	0.0264	0.000	0.107	0.111	0.000	0.0405	0.0559
Panel B: Placebo Effects of Connection on Nonconnected Existing Firms									
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	Log sales	Log emp	Log capital	Log VAT	Log CIT	Log subsidy	Log TFP	$\mathbb{1}$(Patent)	$\mathbb{1}$(Exit)
Hometown Tie	0.0145	-0.0444	0.0075	-0.0304	0.0341	0.0244	-0.0151	0.0011	-0.0004
	(0.121)	(0.0667)	(0.0112)	(0.0863)	(0.0321)	(0.0421)	(0.0176)	(0.0017)	(0.0004)
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	2,825,952	2,765,419	2,772,003	2,760,523	1,853,612	2,000,600	1,205,705	3,124,657	3,124,657
R-squared	0.0281	0.0295	0.0260	0.000	0.096	0.102	0.000	0.0313	0.0664

Notes: The observation is at the firm-year level. Panel A focuses on the effects on connected firms, and panel B focuses on the effects on the nonconnected firms. In columns (1) to (6) and columns (10) to (15), we utilize matched data between the ASIF and the SAIC for the years 2000 to 2011 (excluding 2010). Due to many firms reporting 0 or missing values for corporate income tax (CIT) and government subsidies, we have fewer observations for these columns when using log values as the dependent variables. For firm TFP in columns (7) and (16), we only use the matched data between the ASIF and the SAIC from 2000 to 2007 because the ASIF data do not report firm value-added after 2007, which is essential in computing firm productivity using the ACF method. Columns (8) and (17) focus on a dummy variable indicating whether a firm files a patent in the observed year. We utilize the matched data between firm patent applications and the SAIC, creating a panel data where the value is 0 if a firm does not file a patent. Columns (9) and (18) examine whether a firm exits in the SAIC sample, i.e., the firm deregisters. We create a panel data based on the SAIC deregistration database, where the value is 0 if a firm does not exit in a year. In all columns, we control for firm fixed effects and year fixed effects, as well as log per capita real GDP and log population of both the origin and destination cities. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 13: Placebo Effects of Hometown Ties on Firm Capital Flow (2012-2019)

	(1)	(2)	(3)
Dep.var.:	Log firm capital	Log firm registries	𝟙(Flow>0)
Hometown Tie	0.0236	0.00395	0.00140
	(0.0240)	(0.00443)	(0.00233)
Twoway cluster at origin and destination	[0.0244]	[0.00754]	[0.00508]
Clustered at province-dyad	[0.0242]	[0.00753]	[0.00507]
Spatially correlated, 200km	[0.0245]	[0.00754]	[0.00509]
Spatially correlated, 300km	[0.0247]	[0.00757]	[0.00511]
Spatially correlated, 500km	[0.0249]	[0.00758]	[0.00513]
City dyad FE	Y	Y	Y
Year FE	Y	Y	Y
Controls	Y	Y	Y
Observations	679,776	679,776	679,776
R-squared	0.075	0.105	0.038
Number of city dyads	84,972	84,972	84,972

Notes: The observation is at the city-dyad-year level, and the sample covers 84,972 city dyads from 2012 to 2019. We first cluster standard errors at the city-dyad level and report them in the parentheses. We then cluster standard errors twoway at the origin city and destination city levels. To address spatial correlations, we follow Conley (1999) and consider capital investment within province-dyads as well as within different distance thresholds: 200 kilometers, 300 kilometers, and 500 kilometers. By including these spatial measures, we account for potential spatial dependencies in capital investment. The corresponding standard errors, reported in brackets, provide additional confirmation of the robustness of our results. In all columns, we include city-dyad and year fixed effects, as well as controls for log per capita real GDP and log population of both the origin and destination cities. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 14: The Effects of Capital Flow on GDP Growth, Promotion, and Corruption

Dep. var.:	GDP growth		𝟙(Promotion when term ends)		𝟙(Corruption prosecution up till 2021)	
Data period	Pre-2012	2012-2019	Pre-2012	Post-2012	Pre-2012	2012-2017
Panel A: OLS regressions	(1)	(2)	(3)	(4)	(5)	(6)
Share of connected investment	0.0194***	-0.0023	0.177***	0.0504	0.225***	-0.0230
	(0.0074)	(0.0162)	(0.0323)	(0.0731)	(0.0730)	(0.0246)
Panel B: 2SLS regressions	(7)	(8)	(9)	(10)	(11)	(12)
Share of connected investment	0.0312***	0.0208	0.397***	0.0230	0.490***	-0.0301
	(0.00511)	(0.0211)	(0.0563)	(0.0140)	(0.0312)	(0.0335)
First-stage F	22.873	12.119	22.873	12.119	22.873	12.119
City FE	Y	Y	Y	Y	N/A	N/A
Year FE	Y	Y	Y	Y	Y	Y
Control Variables	Y	Y	Y	Y	Y	Y
Observations	3,504	2,349	3,043	1,435	654	669
R-squared	0.243	0.339	0.067	0.045	0.065	0.076

Notes: Panel A presents the results obtained using ordinary least squares (OLS) estimation, while Panel B displays the results obtained using two-stage least squares (2SLS) estimation. In Panel A, the variable "Share of connected investment" represents the proportion of intercity investments originating from cities where the party secretaries share the same hometown. In Panel B, we employ the presence of a hometown tie between a city party secretary and a party secretary in another Chinese city as the instrumental variable in the 2SLS estimation. This helps address potential endogeneity issues and obtain consistent estimates. In all columns, we include city fixed effects and year fixed effects to account for time-invariant city-specific factors and year-specific factors that may influence the outcomes of interest. Additionally, we control for log per capita real GDP and log population of the destination city as additional control variables. Standard errors, clustered at the city level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 15: The Heterogeneous Effects of Hometown Ties by Destination Sector

	(1)	(2)	(3)	(4)	(5)	(6)
Sample aggregated by:	Destination sector output share > median			Destination sector output share < median		
Dep.var.:	Log firm capital	Log firm registries	$\mathbb{1}$(Flow>0)	Log firm capital	Log firm registries	$\mathbb{1}$(Flow>0)
Hometown Tie	0.109***	0.0228***	0.0225***	0.0316	0.00731	0.00553
	(0.0313)	(0.00413)	(0.00733)	(0.0442)	(0.0222)	(0.00641)
City-dyad FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Control Variables	Y	Y	Y	Y	Y	Y
Observations	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664
R-squared	0.065	0.050	0.051	0.053	0.042	0.039
Number of City Dyads	84,972	84,972	84,972	84,972	84,972	84,972

Notes: We first split all firm registrations based on whether the firm investment is made in the destination city-sector-year cell, where the destination city's output share in the 2-digit Chinese Industry Classification sector in 2000 is above or below the national median. We compute the output shares for each city in each 2-digit sector using the data from the ASIF in 2000. We then aggregate the firm investments by city-dyad-year cells. The first three columns present the aggregate investment in these cells for investments made in sectors of destination cities where the sector's output share exceeds the national median. The last three columns show the aggregate investment in these cells for investments made in sectors of destination cities where the sector's output share is below the national median. City-dyad fixed effects and year fixed effects are included in all columns to account for time-invariant city-specific factors and year-specific factors that may influence the outcomes of interest. Additionally, log per capita real GDP and log population of both the origin and destination cities are included as additional control variables. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 16: The Heterogeneous Effects of Hometown Ties by Road and Skill Labor Densities of the Destination City

	(1)	(2)	(3)	(4)	(5)	(6)
	Log firm capital		Log firm registries		$\mathbb{1}$(Flow>0)	
Hometown Tie	0.125***	0.135***	0.0137***	0.0159***	0.0166***	0.0155***
	(0.0179)	(0.0225)	(0.00225)	(0.00221)	(0.00338)	(0.00351)
Hometown Tie × Road density	0.0310***		0.0216**		0.0197***	
	(0.0151)		(0.0114)		(0.0038)	
Hometown Tie × Skilled labor density		0.0366***		0.0179**		0.0179***
		(0.0188)		(0.0083)		(0.0045)
City-dyad FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Control Variables	Y	Y	Y	Y	Y	Y
Observations	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664
R-squared	0.065	0.065	0.050	0.050	0.051	0.051
Number of City Dyads	84,972	84,972	84,972	84,972	84,972	84,972

Notes: Road density is calculated as the ratio of paved road area to the total area of the destination city in 2000. Skill-labor density is calculated using the 2000 Chinese population census data and represents the ratio of college-educated individuals to the total population. In all columns, we include city-dyad fixed effects and year fixed effects to account for time-invariant city-specific factors and year-specific factors that may influence the outcomes of interest. Additionally, we control for log per capita real GDP and log population of both the origin and the destination cities as additional control variables. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 17: The Implications on Growth

	Firm-level outcomes				City-level outcomes (2SLS)		
Dep. var.:	$\mathbb{1}$(Patent application)		TFP growth 2000-2007 (OP ACF)		GDP growth	Patent growth	Trademark growth
Sample:	2000-2011		2000-2007			Pre-2012	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Hometown Tie	0.0010***	0.0011***	0.0022***	0.0027***			
	(0.0003)	(0.0003)	(0.0004)	(0.0003)			
Hometown Tie × Promotion incentives	0.0012**	0.0010***	0.0017***	0.0010***			
	(0.0004)	(0.0002)	(0.0002)	(0.0001)			
Hometown Tie × Corruption	-0.0007***	-0.0064***	-0.0016***	-0.0009***			
	(0.0002)	(0.0011)	(0.0012)	(0.0001)			
Share of connected investment					0.0207***	0.0459***	0.0481***
					(0.0034)	(0.0066)	(0.0051)
Share of connected investment × Promotion incentives					0.0218***	0.0380***	0.0258***
					(0.0034)	(0.0091)	(0.0049)
Share of connected investment × Corruption					-0.0146**	-0.0396***	-0.0376**
					(0.0068)	(0.0077)	(0.0133)
Sector FE	N	Y	N	Y	N	N	N
City-dyad FE	Y	Y	Y	Y	N	N	N
City FE	N	N	N	N	Y	Y	Y
Enter Year FE	Y	Y	Y	Y	-	-	Y
Year FE	-	-	-	-	Y	Y	
Control Variables	Y	Y	Y	Y	Y	Y	Y
Observations	910,258	910,258	461,973	461,973	3,504	3,504	3,504
First-stage F	-	-	-	-	22.873	22.873	22.873
R-squared	0.065	0.066	0.057	0.059	0.243	0.377	0.305

Notes: In all columns, we include the variable $\mathbb{1}(\textit{Promotionincentives})$, which is a dummy variable that takes the value of 1 if both party secretaries' ages are below 57 at the time of the observation. This variable captures the presence of promotion incentives for party secretaries. Additionally, we include the variable Corruption, which is a dummy variable that takes the value of 1 if either party secretary has been investigated for corruption by the year 2021. This variable helps capture the potential influence of corruption on the outcomes of interest. The variable "Share of connected investment" represents the proportions of intercity investments originating from cities where the party secretaries share the same hometown. In columns (1) to (4), we control for city-dyad fixed effects to account for time-invariant city-dyad-specific factors that may affect the outcomes. Standard errors are clustered at the city-dyad level, and the clustered standard errors are reported in parentheses. In columns (5) to (7), we utilize the presence of a hometown tie between a city party secretary and a party secretary in another Chinese city as the instrumental variable. This instrumental variable helps address potential endogeneity issues. We also control for city fixed effects and report the clustered standard errors at the city level in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Online Appendix

A Welfare implication

In this section, we introduce a simple quantitative trade model to conduct a welfare analysis. The model is a static firm entry model a la Melitz (2003) and is adapted from Shi (2022a) and Liu et al. (2022). We establish a connection between hometown connections among city leaders and the entry cost, which, in turn, affects firms' entry decisions. The key insights of the model is that capital flows into a city can serve as a sufficient statistic for welfare.

A.1.1 Setup

Our model begins with a single location that encounters a unit mass of non-local entrants. While the model can be easily expanded to encompass multiple locations, such as cities, both the theoretical insights and the quantitative implications remain unaffected by this expansion. Therefore, to simplify analysis and to fix the idea, we focus on the case of a single location. In the appendix available only upon request, we also provide a multi-regional version of this quantitative model. We assume that a representative local resident's preference is captured by the following utility function:

$$U = \int_{z \in Z} [u(q(z)) - p(q(z))q(z)] f(z) dz, \tag{A.1}$$

where Z is the set of all potential firm entrants, z indexes these firms. Each firm's index also represents its marginal production cost, which is determined by a probability density function $f(z) = \frac{dF(z)}{dz}$. In the model with multiple locations, we can also adopt the same framework to illustrate household behavior, with the only exception that a subscript of location should be exploited. The utility derived from consumption is given by $u(q) = \frac{e}{e-1} q^{\frac{e-1}{e}}$, where e represents the demand elasticity. Equation (A.1) implies the following solution to the resident's maximization problem:

$$q^*(p) = \arg\max_{q} u(q) - pq = p^{-e}. \tag{A.2}$$

For tractability, we define the cumulative distribution function of firms' marginal costs as $F(z) = z^r$. The constant marginal cost of production for each firm is represented by τz, where $\tau \geq 1$ captures the effects of hometown connections on production costs. Based on our empirical results that hometown connections reduce the cost of business, we stipulate that τ is smaller if entrants enjoy the benefits of connections. Apart from production costs, f is the fixed entry cost that entrants have to pay to start a business.

We employ the monopolistic competition framework, in which firms maximize potential expected profit:

$$\Pi = \int_0^1 \max\{\pi(z) - f, 0\} dF(z), \tag{A.3}$$

where firms compare the profit for entry ($\pi(z) - f$) and no entry (0), the integral indicates that firms are making decisions based on expected profits. The profit of entry is given by

$$\pi(z) = \max_p (p - \tau z) q^*(p). \tag{A.4}$$

In the model with multiple locations, we also introduce a Gumbel-distributed location-specific parameter that enters the profit function. On top of that, however, the main results below still stand. Given the standard Melitz (2003) structure, there exists a cutoff point z, below which it is profitable for firms to enter the market. This cutoff point is defined by the condition $\pi(\bar{z}) = f$. We can then define an equilibrium as follows.

Definition. An equilibrium consists of a tuple $\{p(z), q(z), \pi(z)\}$, such that (i) firms enter if and only if $\pi(z) \geq f$, (ii) $\pi(z)$ and $p(z)$ solve equation (A.4), and (iii) $q(z)$ solves equation (A.2).

We can then derive the welfare implications. The first-order derivative of consumer surplus with respect to the cost parameter is

$$\begin{aligned} -\frac{d \log U}{d \log \tau} &= -\frac{1}{\int_0^{\bar{z}} u(z) f(z) dz} \{ \int_0^{\bar{z}} \frac{du(z)}{d \log \tau} f(z) dz + \frac{d \int_0^{\bar{z}} u(z) f(z) dz}{d\bar{z}} \frac{d\bar{z}}{d \log \tau} \} \\ &= (e - 1) + (r - e + 1) \\ &= r. \end{aligned} \tag{A.5}$$

Following Liu et al. (2022), e and r are two key elasticities that determine the responses of consumer surplus to the cost index. e dictates how consumer surplus responds to production costs and prices among the entrants that have already entered. r dictates how the surplus responds to the extensive margin of new entrants. As equation (A.5) indicates, the net effect is exactly r. We denote the measure of new entrants by $M = F(\bar{z})$. Thus, we have the following lemma.

Lemma. $-\frac{d \log U}{d \log \tau} = -\frac{d \log \mu}{d \log \tau} = r$, *where* μ *indexes the amount of firm entrances.*

Proof: Given our model, we have the following

$$p(z) = \frac{e}{e-1}\tau z; q(z) = (\frac{e}{e-1}\tau z)^{-e}; \pi(z) = e^{-1}(\frac{e}{e-1}\tau z)^{1-e}. \tag{A.6}$$

Thus, from the definition of $\bar{z}$, we have

$$\bar{z} = \frac{e-1}{e}(ef)^{\frac{1}{1-e}} \tag{A.7}$$

Thus, we have

$$U = \int_0^{\bar{z}} (u(z) - p(z)q(z))f(z)dz = A \times \tau^{-r}, \tag{A.8}$$

where A is a constant. In addition,

$$\mu = F(\bar{z}) = B \times \tau^{-r}, \tag{A.9}$$

where B is a constant. Thus, we can easily derive the derivative of $\log U$ and $\log \mu$ with respect to $\log \tau$, which are both $-r$.

Also, note that this lemma still holds for the model with multiple locations. This equivalence is the reason why we use this simplified version for quantitative and welfare analysis.

A.1.2 Welfare analysis

In the quantitative exercise, we express the logarithm of the marginal cost of production, $\log \tau$, as a function of hometown connections. It is parameterized as $\log \tau = a_0 + a_1\text{Connection}$, where Connection represents the number of hometown connections a firm has, and a_0 and a_1 are parameters that govern the relationship between connections and marginal cost.

To capture more nuanced effects, the model is extended to include an interaction term: $\log \tau = b_0 + b_1\text{Connection} + b_2\text{Connection} \times \text{Channel}$. Here, b_0, b_1, and b_2 are additional parameters. The variable Channel signifies the specific ways through which hometown connections might facilitate new firm entries, including (1) reducing entry barriers and (2) providing ongoing support.

We can then have the following results.

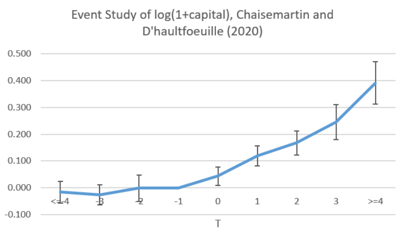
$$-\frac{d\log U}{dConnection} = -\frac{d\log U}{d\log\tau}\frac{d\log\tau}{dConnection} = r \times a_1;$$
$$-\frac{d^2\log U}{dConnectiondChannel} = -\frac{d\log U}{d\log\tau}\frac{d^2\log\tau}{dConnectiondChannel} = r \times b_2. \tag{A.10}$$

Equation (A.10) posits that the elasticity r measures the partial effects of a reduction in the cost index on new investments. The term $r \times a_1$ quantifies the impact of hometown connections on these investments, while $r \times b_2$ assesses the effect of specific channels through which these connections operate. These parameters can be well-identified by the difference-in-differences regressions described below.

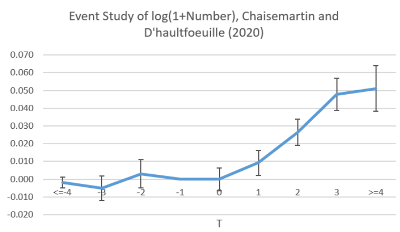
According to the reduced-form estimates presented in Table A.7, the estimates correspond to the parameter $r \times a_1$, Thus, an additional hometown connection increases welfare by either 6.89% or 5.10%, depending on whether new entrants are measured by capital or number. Furthermore, based on the estimates from Tables 10 and 12, the estimates correspond to $r \times b_2$ of different channels, and the contribution to welfare from the channel of reducing entry barriers is 65.6%, while the contribution of the channel of continued support accounts for 34.4%.[31] Since these two mechanisms are the major ones we detect in our empirical analysis, we assume that these two are the ones at play, and we decompose the welfare gain into these two sources.

[31] The contribution of continued support corresponds to the effect of hometown connections on firms' revenue, which also equals $r \times a_1$. Thus, the contributions are calculated by comparing the welfare increase when an entry barrier is reduced from the top 75% to the bottom 25%, which is a change of 0.062 log points, to the welfare increase from continued support, which is 0.0325 log points. The percentage contribution of each channel is thus determined by dividing each channel's welfare change by the total welfare change and converting it to a percentage: $0.062/(0.062 + 0.0325) \times 100$.

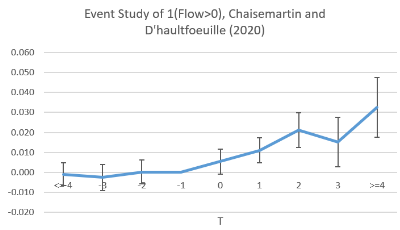
B Figures and Tables



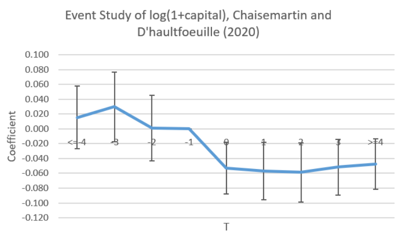
(a) Capital, connection formed



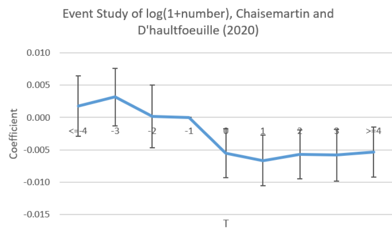
(b) Registries, connection formed



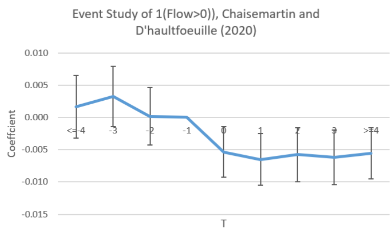
(c) 𝟙(Flow>0), connection formed



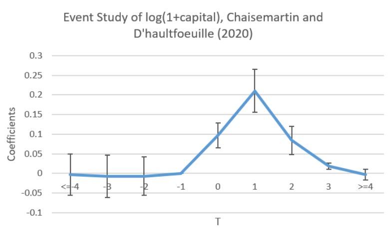
(d) Capital, connection severed



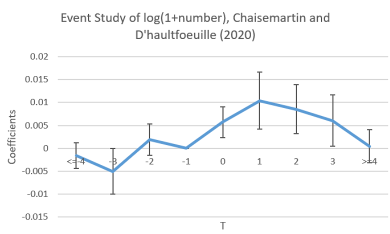
(e) Registries, connection severed



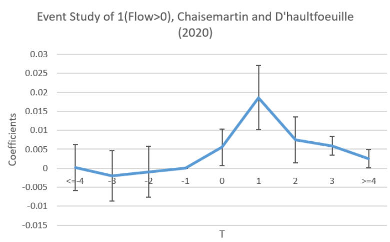
(f) 𝟙(Flow>0), connection severed



(g) Capital, overall effects



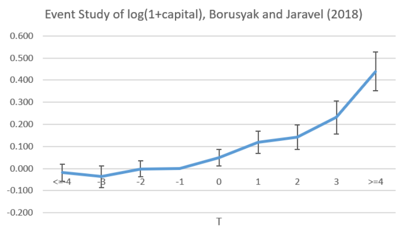
(h) Registries, overall effects

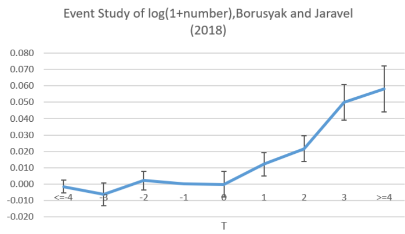


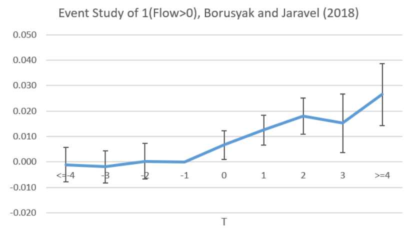
(i) 𝟙(Flow>0), overall effects

Figure A.1: Event Studies Following De Chaisemartin and d'Haultfoeuille (2020)

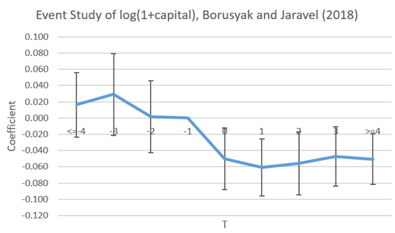
Notes: The figure follows the methods developed by De Chaisemartin and d'Haultfoeuille (2020) to conduct the event study. It illustrates the dynamic effects of the treatment (connection formation and severance) on the log value of newly registered total firm capital, log number of newly registered firms, and probability of new firm entry between city dyads. The observations are at the city-dyad-year level, and the sample covers 84,972 city dyads from 2000 to 2011. It presents three exercises separately. In the first exercise (sub-figures a, b, c), we examine the effects of hometown ties between two cities when the hometown-connected party secretaries remain in their positions. In the second exercise (sub-figures d, e, f), we analyze the effects when one of the hometown-connected party secretaries leaves office, resulting in the severance of the connection. For clarity, we only include observations where the two cities in the control group never formed a connection, and we start counting the observations from the year when the connection was formed if it existed. In the third exercise (sub-figures g, h, i), we consider the effects of hometown ties as long as two party secretaries in two cities shared a common hometown at any time prior to the year of observation. The horizontal axis represents the years since the city dyad experienced the treatment, with 0 indicating the first year of treatment. The vertical axis represents the regression coefficients for the dynamic effects, obtained using the specification shown in equation (2). The capped spikes indicate the 95% confidence interval, with standard errors clustered at the city-dyad level. Each estimated effect is compared to the base year, which is one year prior to the treatment and standardized to 0.

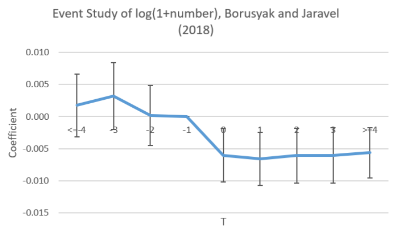


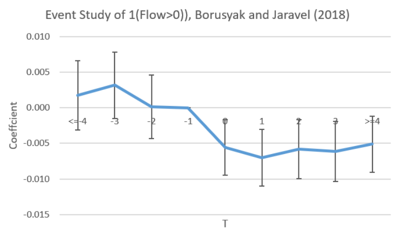




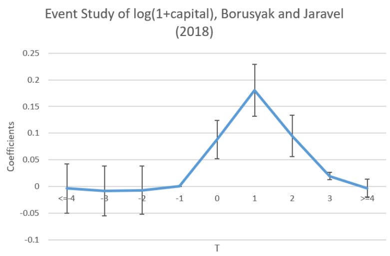
(a) Capital, connection formed (b) Registries, connection formed (c) $\mathbb{1}$(Flow>0), connection formed

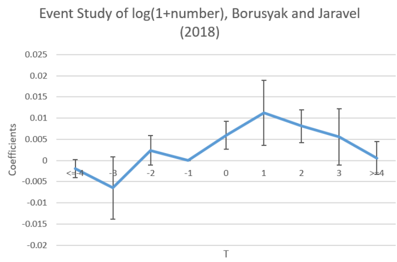


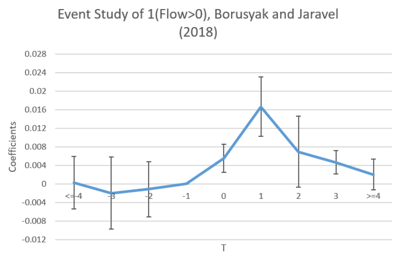




(d) Capital, connection severed (e) Registries, connection severed (f) $\mathbb{1}$(Flow>0), connection severed



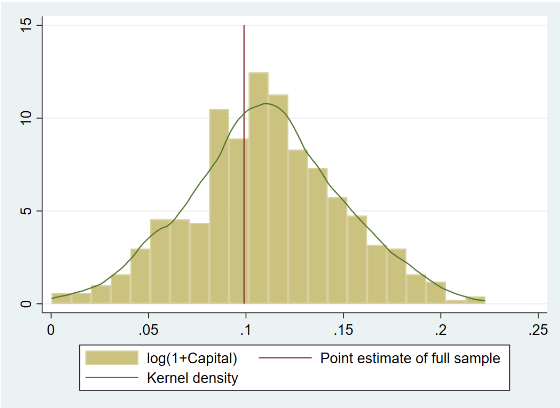




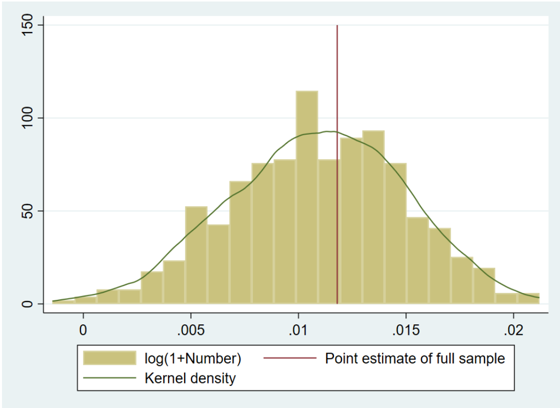
(g) Capital, overall effects (h) Registries, overall effects (i) $\mathbb{1}$(Flow>0), overall effects

Figure A.2: Event Studies Following Borusyak et al. (2022)

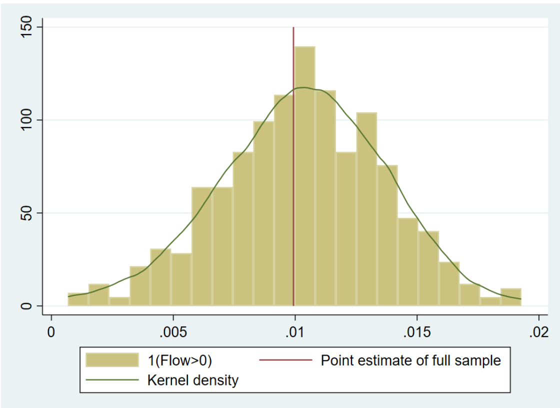
Notes: The figure follows the methods developed by Borusyak, Jaravel, and Spiess (2022) to conduct the event study. It illustrates the dynamic effects of the treatment (connection formation and severance) on the log value of newly registered total firm capital, log number of newly registered firms, and probability of new firm entry between city dyads. The observations are at the city-dyad-year level, and the sample covers 84,972 city dyads from 2000 to 2011. It presents three exercises separately. In the first exercise (sub-figures a, b, c), we examine the effects of hometown ties between two cities when the hometown-connected party secretaries remain in their positions. In the second exercise (sub-figures d, e, f), we analyze the effects when one of the hometown-connected party secretaries leaves office, resulting in the severance of the connection. For clarity, we only include observations where the two cities in the control group never formed a connection, and we start counting the observations from the year when the connection was formed if it existed. In the third exercise (sub-figures g, h, i), we consider the effects of hometown ties as long as two party secretaries in two cities shared a common hometown at any time prior to the year of observation. The horizontal axis represents the years since the city dyad experienced the treatment, with 0 indicating the first year of treatment. The vertical axis represents the regression coefficients for the dynamic effects, obtained using the specification shown in equation (2). The capped spikes indicate the 95% confidence interval, with standard errors clustered at the city-dyad level. Each estimated effect is compared to the base year, which is one year prior to the treatment and standardized to 0.



(a) Firm Capital



(b) Number of Firm Registrations



(c) Investment Probability

Figure A.3: Distribution of Estimated Effects Using Random Samples that Contain Origin and Destination Cities Only Once

Notes: In each of the 84,972 city pairs, we randomly choose a city of origin and a destination city to carry out this study. Each selection yields 509,832 observations, which is half the number of observations seen in our main results that include each city pair twice. We perform regression for each sample selection and repeat the process 500 times. We then graph histograms of the estimates, specifically for log firm capital, log firm registration, and the probability of any firm registration. Red vertical line indicates our estimate in the main results. The distribution of these estimates gravitates around our primary result estimates.

Table A.1: Determinants of Common Hometown Tie

Dep. var.:	Hometown Tie			
	(1)	(2)	(3)	(4)
Log value of population (origin city)	-0.00132			
	(0.00143)			
Log value of population (destination city)	-0.00132			
	(0.00143)			
Log value of GDP per capita (origin city)	-9.01e-05			
	(0.00221)			
Log value of GDP per capita (destination city)	-9.01e-05			
	(0.00221)			
Log value of number of firms (origin city)		0.00101		
		(0.00354)		
Log value of number of firms (destination city)		0.00101		
		(0.00354)		
Log value of number of workers (origin city)		0.00935		
		(0.00575)		
Log value of number of workers (destination city)		0.00935		
		(0.00575)		
Share of workers in primary industry (origin city)			0.000274	
			(0.000170)	
Share of workers in primary industry (destination city)			8.54e-05	
			(0.000103)	
Share of workers in secondary industry (origin city)			0.000274	
			(0.000170)	
Share of workers in secondary industry (destination city)			8.54e-05	
			(0.000103)	
Share of GDP in primary industry (origin city)				0.000107
				(0.000300)
Share of GDP in primary industry (destination city)				-0.000168
				(0.000214)
Share of GDP in secondary industry (origin city)				0.000107
				(0.000300)
Share of GDP in secondary industry (destination city)				-0.000168
				(0.000214)
City dyad FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	762,912	611,568	901,086	908,306
R-squared	0.491	0.504	0.435	0.434

Notes: The analysis is conducted at the city-dyad-year level using a sample of 84,972 city dyads covering the period from 2000 to 2011. All columns include controls for city-dyad fixed effects and year fixed effects. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A.2: Hometown sharing statistics

	Share (%)
Panel A: Years of hometown sharing	
0	89.34
1	2.68
2	2.03
3	2.21
4	1.63
5	0.53
6	0.58
7	1.01
Panel B: City dyads ever have hometown sharing	
	10.70

Table A.3: Alternative Logarithmic Forms

	(1) log(0.01+Capital)	(2) log(0.01+Number)	(3) log(0.0001+Capital)	(4) log(0.0001+Number)	(5) arcsinh(Capital)	(6) arcsinh(Number)
Hometown Tie	0.0918***	0.0112***	0.0935***	0.0114***	0.101***	0.0109***
	(0.0134)	(0.00221)	(0.0246)	(0.00191)	(0.00240)	(0.00166)
City dyad FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Observations	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664
R-squared	0.064	0.051	0.064	0.051	0.064	0.051
Number of city dyads	84,972	84,972	84,972	84,972	84,972	84,972

Notes: The analysis is conducted at the city-dyad-year level using a sample of 84,972 city dyads covering the period from 2000 to 2011. All columns include controls for city-dyad fixed effects and year fixed effects. Additionally, the controls include the logarithm of per capita real GDP and the logarithm of population for both the origin and destination cities. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A.4: Robustness Check: Controlling for Hometown Postings

Dep. var.:	Log firm capital	Log firm registration	$\mathbb{1}$(Flow>0)
	(1)	(2)	(3)
Hometown Tie	0.0919***	0.0120***	0.0112***
	(0.0126)	(0.00240)	(0.00195)
City dyad FE	Y	Y	Y
Year FE	Y	Y	Y
Controls	Y	Y	Y
PS at hometown, origin	Y	Y	Y
PS at hometown, destination	Y	Y	Y
Observations	1,019,664	1,019,664	1,019,664
R-squared	0.065	0.051	0.051
Number of city dyad	84,972	84,972	84,972

Notes: The analysis is conducted at the city-dyad-year level using a sample of 84,972 city dyads covering the period from 2000 to 2011. All columns include city-dyad and year fixed effects to account for time and cross-city variations. The controls used in the analysis include log per capita real GDP and log population of both the origin and destination cities. We include dummy variables to control for the party secretary (PS) working in the other's hometown city. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A.5: Robustness Check: Alternative Measures of Hometown Ties

Dep. var.:	Log firm capital			Log firm registration			$\mathbb{1}$(Flow>0)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Hometown Tie, PS	0.0793***		0.0613***	0.0103***		0.0117***	0.00934***		0.0121***
	(0.0127)		(0.0042)	(0.00244)		(0.00239)	(0.00198)		(0.00195)
Hometown Tie, Mayor	0.0563***			0.0118***			0.00525***		
	(0.0126)			(0.00237)			(0.00195)		
Hometown Tie, PS and Mayor	0.313***			0.0326***			0.0436***		
	(0.0147)			(0.00239)			(0.00244)		
Hometown Tie, All		0.476***			0.0798***			0.0658***	
		(0.0457)			(0.00273)			(0.00908)	
City-dyad FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,019,664	1,019,664	963,716	1,019,664	1,019,664	963,716	1,019,664	1,019,664	963,716
R-squared	0.065	0.065	0.062	0.051	0.051	0.051	0.051	0.051	0.051
Number of city dyads	84,972	84,972	84,696	84,972	84,972	84,696	84,972	84,972	84,696

Notes: The sample consists of 84,972 city dyads observed from 2000 to 2011. The variables $\mathbb{1}(HometownTie, PS)$, $\mathbb{1}(HometownTie, Mayor)$, $\mathbb{1}(HometownTie, PSandMayor)$, and $\mathbb{1}(HometownTie, All)$ are dummy variables indicating different types of hometown ties between party secretaries (PS) and mayors in the city dyads. Specifically, $\mathbb{1}(HometownTie, PS)$ is 1 if only the party secretaries share a common hometown, $\mathbb{1}(HometownTie, Mayor)$ is 1 if only the mayors share a common hometown, $\mathbb{1}(HometownTie, PSandMayor)$ is 1 if only the mayor in one city shares a hometown with the party secretary in the other city, and $\mathbb{1}(HometownTie, All)$ is 1 if there is any hometown tie between the mayors or party secretaries in the city dyads. In columns (3), (6), and (9), the analysis focuses on city dyads where the mayors and the mayor and the party secretary were not born in the same province. This restriction helps isolate the effects of hometown ties of mayors. All columns include city-dyad and year fixed effects to account for time and cross-city variations. The controls used in the analysis include log per capita real GDP and log population of both the origin and destination cities. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A.6: Robustness Check: Controlling for Hometown Province FE

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	Log firm capital	Log firm registration	$\mathbb{1}$(Flow>0)	Log firm capital	Log firm registration	$\mathbb{1}$(Flow>0)
City Hometown Tie	0.0812***	0.00714***	0.00841***			
	(0.0206)	(0.00199)	(0.00207)			
Province Hometown Tie				0.0794***	0.00656***	0.00660***
				(0.0288)	(0.00144)	(0.00233)
City dyad FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Hometown province FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Observations	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664	1,019,664
R-squared	0.077	0.058	0.066	0.077	0.058	0.066
Number of city dyads	84,972	84,972	84,972	84,972	84,972	84,972

Notes: In this exercise, we examine the effects of hometown ties – at the birth city level in columns (1) to (3) and at the birth province level in columns (4) to (6) – on our main outcome variables, controlling for the hometown province fixed effects of the party secretaries of both cities. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A.7: City level results

	(1)	(2)	(3)	(4)
Time period:	2000-2011		2012-2019	
Dep. var.:	log(Capital)	log(Number)	log(Capital)	log(Number)
Number of Hometown Ties	0.0689***	0.0510***	0.00443	0.00361
	(0.0135)	(0.0124)	(0.00507)	(0.00377)
City FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	3,504	3,504	2,336	2,336
R-squared	0.231	0.169	0.209	0.157
Number of cities	292	292	292	292

Notes: The observation is at the city-dyad-year level. All columns include city-dyad fixed effects and year fixed effects. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A.8: Promotion Incentives Strengthen the Effects of Hometown Ties on Bilateral Investment

Dep. var.:	Log firm capital			
	(1)	(2)	(3)	(4)
	Origin w/ incentive	Origin w/o incentive	Destination w/ incentive	Destination w/o incentive
Hometown Tie	0.0693***	0.00516	0.0795***	0.0283
	(0.0144)	(0.0313)	(0.0129)	(0.0805)
City dyad FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
Observations	950,281	69,383	991,437	28,227
R-squared	0.062	0.123	0.065	0.101
Number of city dyads	79,775	9,665	83,226	6,402

Notes: The dependent variable is the logarithm of total registration capital. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A.9: Corruption Strengthens the Effects of Hometown Ties on Bilateral Investment

Dep. var.:	Log firm capital		
	(1)	(2)	(3)
Hometown Tie	0.0679***	0.0752***	0.0711***
	(0.0181)	(0.0128)	(0.0129)
Hometown Tie × 1(More corrupt cases/pop in destination)	0.0467*		
	(0.0247)		
Hometown Tie × 1(Corrupt official in destination)		0.254***	
		(0.0463)	
Hometown Tie × 1(Corrupt official in origin)			0.300***
			(0.0439)
City dyad FE	Y	Y	Y
Year FE	Y	Y	Y
Controls	Y	Y	Y
Observations	977,928	1,019,664	1,019,664
R-squared	0.061	0.065	0.065
Number of city dyads	81,494	84,972	84,972

Notes: The observation is at the city-dyad-year level. All columns include city-dyad fixed effects and year fixed effects. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A.10: The Effects on Firm Land Acquisition in the Post-Campaign Period

Sample period:	2012-2017	
	Firm Land Acquisition	
	(1)	(2)
Dep. var.:	log(Unit Land Price)	log(Land Area)
Hometown Tie	0.0229	0.0110
	(0.0204)	(0.0239)
Controls	Y	Y
Land Usage FE	Y	Y
Year FE	Y	Y
City-dyad FE	Y	Y
Observations	84,250	84,241
R-squared	0.0092	0.0094

Notes: Our analysis examines the entering firms during the period from 2012 to 2017. We control city-dyad fixed effects, year fixed effects, and land usage fixed effects. Controls include the log registry capital of the firm, and log per capita GDP and log population of the city. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A.11: Include both Pre- and Post-campaign Periods

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	Log firm capital	Log firm registration	$\mathbb{1}$(Flow>0)	Log firm capital	Log firm registration	$\mathbb{1}$(Flow>0)
Time period:	2000-2019					
Hometown Tie	0.0668**	0.00790*	0.00599*	0.134***	0.0125***	0.0114***
	(0.0288)	(0.00442)	(0.00301)	(0.0323)	(0.00226)	(0.00299)
Hometown Tie × Post 2012				-0.0680**	-0.00496*	-0.00499**
				(0.0322)	(0.00281)	(0.00213)
City dyad FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Observations	1,699,440	1,699,440	1,699,440	1,699,440	1,699,440	1,699,440
R-squared	0.066	0.048	0.055	0.066	0.048	0.055
Number of city dyads	84,972	84,972	84,972	84,972	84,972	84,972

Notes: The sample covers a period of 2000 to 2019, which includes both pre- and post-campaign periods. The controls used in the analysis include log per capita real GDP and log population of both the origin and destination cities. Standard errors, clustered at the city-dyad level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.