Subnational Leaders and Economic Growth

Evidence from Chinese Cities*

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Abstract

This paper studies the role of subnational leaders in economic growth using city-leader pair data collected from 241 cities in China's 18 provinces for the period 1994-2008. A unique feature of China's institutional arrangements is that local leaders are often moved from one city to another. This allows us to compare leaders across cities. Two alternative specifications are adopted and both find that leaders matter for local economic growth. Further exploration shows that our sample does not suffer from biased attrition and our estimates of the leader effects are robust to transitory shocks and are not affected by nonrandom moves of leaders. Using the leaders' personal effects estimated from our test, we find that more capable leaders are more likely to get promoted although their chances are nuanced by their ages. We also find that local economic growth is not a good predictor for promotion after leaders' personal effects are controlled.

Keywords: local leaders, economic performance, promotion tournament

JEL Classification: H11, M51, O53, P26

1 Introduction

Empirical studies in the literature have found that national leaders play an important role in economic growth. For example, Glaeser, La Porta, Lopez-de-Silanes, and Shleifer (2004) find that poor countries get out of poverty through good policies, often pursued by dictators, and subsequently improve their political institutions. Using the sudden death of a leader as an exogenous shock, Jones and Olken (2005) find that the change of leaders has a significant impact on a country's economic growth. These findings can be contrasted with the thesis that institutions are the more fundamental cause for economic growth (e.g., North and Thomas,

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1973; Acemoglu, Johnson, and Robinson, 2006). This paper extends the above literature to studying subnational leaders using data collected on 241 cities from 18 Chinese provinces for the period 1994-2008. The advantage of studying subnational leaders is that these leaders face the same national institutional settings so their role can be isolated from the role of national institutions. As a result, such a study can provide more decisive evidence for the question whether leaders matter for economic growth.

In most countries, though, subnational leaders do not serve in more than one locality, making comparisons among them impossible because their observed performances are results of the combination of their own abilities and local conditions. In this regard, China provides a unique opportunity for the study of subnational leaders. In the country, local leaders are engaged in a promotion tournament in which they compete with each other for promotion (Li and Zhou, 2005; Xu, 2011). In the process, a large number of them are periodically shuffled between localities; those movers "connect" leaders serving in different localities in a way that researchers are made able to compare all the connected leaders. Tracing the leaders who served in more than one city, we construct "connected" subsamples of cities that had leaders moving between them. With the largest of those subsamples, we are able to estimate leaders' relative contributions (personal effects) to local economic growth and compare them across cities. This is an improvement to the Jones and Olken test that only accounts for within-locality variations.

Complementary to studying the contribution of subnational leaders to economic growth, we also explore the linkage between leaders' personal effects to their chances of promotion. We keep in mind two purposes of doing this. One is to check the consistency of our first-step results. To the extent that the Chinese hierarchy — one that is modeled on the Soviet *nomenklatura* — promotes local leaders based on their personal abilities, we should find a positive correlation between leaders' personal effects and their chances of promotion. The other, and more substantial purpose, is to find out whether the Chinese *nomenklatura* really promotes talents. The existing studies (eg., Li and Zhou, 2005; Xu, Wang, and Shu, 2007; Wang, Xu and Li, 2009) have studied provincial leaders. In contrast, we study city leaders and have more observations so potentially we can provide more reliable results. In addition, the promotion of provincial leaders to the central government is often influenced by political factors (Opper and Brehm, 2007) whereas the promotion of city leaders to the provincial offices is less likely so. Lastly, the existing studies use the average growth rate of a leader's tenure to predict his promotion. However, the growth rate may not entirely reflect a leader's own capabilities which presumably are the factor the government's organizational department looks at. We are able to infer leaders' personal effects from local economic growth rates and use them to predict their promotion; therefore, we provide a finer test for the relationship between leader performance and promotion.

Methodologically, the construction of the connected samples is critical for us to identify

leaders' personal effects. Unlike in a two-way fixed-effect model where the two sets of fixed effects are orthogonal to each other, we are dealing with a three-way fixed-effect model where the leader's personal effect and the city fixed effect share the same dimension while they are both orthogonal to time. [1] As a result, like in the analysis of firm-employee matched data (e.g., Abowd and Kramarz, 2006), the leader's personal effect cannot be separated from the city fixed effect if no leader moves between cities. In a connected sample, we can use the movers as a vehicle to obtain a complete order of the connected leaders' personal effects although we still cannot estimate the absolute value of each leader's personal effect *per se*.

We face several challenges in our identification strategy. One is endogenous sample composition and endogenous switches. Like Jones and Olken's test, our test also relies on detecting the variation among leaders. If the entry and attrition of leaders in our sample follow particular patterns — e.g., stronger leaders are more likely to appear in more recent years, and weaker leaders tend to be more likely to leave the sample — then the variation among leaders can be systematically altered. In the same vein, the variation among leaders may also be inflated or deflated if leaders follow a systematic pattern to switch from one city to another. Jones and Olken only compare leaders within the same country, but still face the issues of endogenous sample composition and changes of leaders. They deal with the second issue by a novel approach of using the sudden death of a leader to ensure an exogenous switch, and deal with the sample composition issue by limiting the comparison within a relatively short period of time before and after a leader's sudden death. The approach we adopt is to follow the labor economists studying employee-firm matched data to conduct various tests to ensure that entry and attrition are sufficiently random and endogenous switches do not seriously impede our results.

A more serious challenge is to separate leaders' personal effects from transitory shocks in the cities. In essence, the way we adopt to infer leaders' personal effects is a variant of decomposition of variance. Specifically, a leader's personal effect stems from the city economic performance of his tenure period if he only served in one city, and is the average of the economic performances of his tenure periods in different cities if he has served more than one city. While the average of the city-level variations is absorbed by the city fixed effect and some of the transitory shocks are absorbed by the control variables we are going to add, it is still possible that our estimates of the leader effects just pick up city-specific and transitory shocks. The strategy we adopt to deal with this issue is to show that the order of the leader effects, which is what we can identify anyway, is not subject to the influence of transitory shocks.

The rest of the paper is organized as follows. Section 2 gives a description of the sources and structure of the data. Section 3 deals with the methodological issues. We first lay out

[1] In our empirical analysis, we study both the party secretary and the mayor as leaders in a city. Assuming that both are essential and not substitutable in contributing to local growth, we can treat each of them separately as if growth were contributed by one leader only. See more discussions in Section 3.

the benchmark econometric specification and propose an alternative specification. Then we describe the problem of indeterminacy implied by the two specifications and show how our connected sample allows us to overcome the problem. We also discuss how endogenous sample composition, endogenous switches, and transitory shocks would affect our test results and proposes methods to test them. Section 4 presents our main test results and performs additional tests on sample composition, endogenous switches, and transitory shocks to make sure that they do not impede our main test results. In addition, we carry out a study on the heterogeneous effects of leaders' personal abilities on the growth of different sectors. Section 5 analyzes the relationship between leaders' personal abilities and their promotion. We adopt both the linear probability model and the multinomial logit model to do the analysis and compare leaders at different time points. In addition, we compare the promotion of party secretaries and mayors. Section 6 concludes the paper.

2 The Data

China has a highly decentralized fiscal system despite its one-party political system (Che, Qian, and Weingast, 2005; Xu, 2011). [2] There are five levels of government in the country: the central, provincial, city, county/district, and township. Each level of government has its own independent budgets and independent or shared revenue sources. At the each level, two officials assume the highest offices. One is the secretary of the local communist party committee, and the other is the head of the executive branch (e.g., governor at the provincial level and mayor at the city level). In theory, the party secretary is elected by the local party congress and the executive officer is elected by the local People's Congress, the legislative body; in reality, most of them are appointed by the organizational department of the Communist Party in the government one level higher. As described in Li and Zhou (2005) and Xu (2011), the process of moving up in the government/party hierarchy can be best characterized by a tournament. That is, lower-level (e.g., county) government officials compete with each other, and those picked by the organizational department enter another round of elimination game at a higher-level (e.g., city), so on so forth, until one reaches the highest level, the standing committee (in recent times, consisted of nine people) of the political bureau in the Party's central committee. As such, the competition becomes more and more intense when one moves up the hierarchy.

While political lineages are an important determining factor in the promotion tournament, personal abilities are as equally important, especially at the lower level. Although the central government has been trying to include more goals into the list of evaluation, economic growth has become the dominant goal for most officials. From the perspective of the multitasking

[2] Xu (2011) calls it a regionally decentralized authoritarian (RDA) regime. He provides a comprehensive survey for this regime, explaining its historical roots, its function, and its implications for economic reform and growth in China.

theory, this is hardly surprising because economic growth is much easier to measure than other goals. The literature has documented both systematic and anecdotal evidence for the importance of economic growth in the promotion tournament; [3] some authors (e.g., Xu, 2011) attribute China's high economic growth rates to competition among local leaders. On the other hand, higher growth rates of the local economy serve as a good predictor for a leader's chances of promotion (Li and Zhou, 2005). Presumably, the organizational department is using the record of economic growth in a leader's tenure to gauge his personal abilities.

In this paper, we study the party secretaries and mayors in the cities. There are four kinds of cities: the four provincial cities (Beijing, Shanghai, Tianjin and Chongqing), sub-provincial cities (provincial capitals and one or two other big cities in the province), prefectural cities, and county cities. The four provincial cities are clearly outliers, and the number of county cities is numerous. So we study sub-provincial and prefectural cities. They are more homogenous in terms of size and the rank of government officials. There are a total of 333 such cities in the country.

By law, the mayor is the executive officer of the municipal government; at the same time, the law also says that the mayor is under the guidance of the city communist party committee to which the party secretary is the head. In most cities, the party secretary clearly is the number-one person because important decisions are made in the party committee. However, his power is checked by the mayor because in theory the executive orders should be delivered through the mayor. In the end, the party secretary and the mayor share power in a city. A division of labor emerging from the reality is that the party secretary is in charge of the personnel and other political duties such as maintaining social stability while the mayor is in charge of the daily operation of the government for which economic growth is a top priority. To the extent that the mayor has to rely on the bureaucracy to manage the economy, his contribution to local economic growth is tied to the party secretary's efforts to select more capable subordinates. In reality, the interaction between the party secretary and the mayor takes many forms and the pattern of their contributions to local economic growth cannot be readily parameterized. In our empirical study, we will take a simple approach by treating them as making separate contributions to local economic growth. [4]

The period covered by our study is 1994-2008. This period was chosen primarily because of the availability of data. It is difficult to get data on city leaders before 1994, and data beyond

[3] The authors' own interviews in the summer of 2011 found that city and county governments used score boards to evaluate subordinate government officials, every month in some cases. The goals on top of the list were ubiquitously related to economic growth, such as the growth of tax revenues, the progress of road building, conversion of village land to industrial parks, etc., as well as GDP figures themselves. The officials who were ranked at the bottom faced the risk of being sacked (in one county, it was made clear that a leader who ranked as the last one twice in a row would be removed from his/her position).

[4] Including both the secretaries and mayors in our analysis can substantially increase the sample size of our largest connected sample. On the other hand, though, this causes an identification problem. We will deal with it in the next section.

2008 were not made public when the major body of data was collected. The year 1994 was chosen as the starting year also because China started a new revenue-sharing system in that year. Before that year, the central government shared revenue with provincial governments based on negotiation; since that year, revenue has been shared under preset rules, similar to the federal system adopted in the United States.

Information of the party secretaries and mayors was collected from *The China Yearbook of Municipalities*, provincial yearbooks and reports from the mass media. We then match the leaders to annual macroeconomic data collected from provincial yearbooks by the following rules [5]:

1. Each city-year observation is matched with one secretary and one mayor.

2. If there was a turnover within a year, we take the leader who stayed for over 6 months in that year.

3. If there were multiple turnovers in a year and no leader stayed for over 6 months, we take the leader with the longest stay in that year.

Due to the limitation of data sources, we were able to collect a complete name list for 1,671 leaders in 241 cities of 18 provinces for the whole period 1994 to 2008. For more detailed personal information such as age, previous experience, and promotion, we were only able to get data for the period 2001 to 2008. We will call the 15-year sample "the long sample", and the 8-year sample "the short sample" henceforth. Table A1 in the appendix lists the name of cities in our dataset; Figure A1 then maps them in a Chinese map.

Figures 1 and 2, using the long sample, compare our sample cities' average per-capita GDP and nominal growth rates with the respective national figures in the period 1994-2008. The shaded cities are our sample cities, and the dark shaded cities are those in our largest connected sample.[6] Because most of our sample cities are located in the east side of the country and those in the largest connected sample are more so, we see that the average income is higher in our sample cities than the national average. However, their average growth rates have been about the same as the national average.

[Figures 1 and 2 inserted here.]

Among all the 1,671 leaders in the long sample, 1,425 served in only one city, 219 had one switch, 23 had two switches, and the remaining four had three switches (Table A2). We call those who served in more than one city in our sample period "the movers". The total number of movers was 246, or about 15% of the total number of leaders. Figure 3 shows the histogram

[5] These rules are similar to those applied in Li and Zhou (2005)

[6] The composition of connected samples will be described in detail in the next section.

of all leaders' tenures in one city. The average was 3.8 years, lower than the designated tenure of five years, also lower than the average tenure of the provincial leaders during the period 1978-2005 which was almost four years (Wang and Xu, 2008). The median tenure was even shorter, only three years. [7]

[Figure 3 inserted here.]

In a dataset like ours, attrition is unavoidable. Table A3 presents the distribution of the number of years a leader appears in our sample. Half of the leaders appear for less than three years while only one quarter appears for more than five years. There are generally three ways for a leader to leave our sample: promotion to the provincial or central government, being moved to a city not covered by our sample, and retirement. We follow Li and Zhou (2005) to define promotion in the following way:

- From a mayor to a party secretary in any city.
- From an ordinary city to a mayor/party secretary in a sub-provincial city.
- From any city to a post in the central government or to the provincial government as party secretary, governor, vice secretary or vice governor.
- From an ordinary city to the head of a department in the provincial government.

Same as in Li and Zhou (2005), we treat being moved to the city or provincial legislative bodies (People's Congress and People's Political Consultation Conference) as retirement in addition to regular retirement because this kind of moves is almost a certain sign for a leader to lose real power in the Chinese system.

Using the short sample, we can get a sense of the distribution of attrition. Among the 1191 leader-term pairs in the short sample, 465 of them (39.0%) ended with the leaders' getting promoted while 184 (15.5%) ended with retirement. Among the 586 leaders in the short sample, 390 (70%) left our sample before 2008. While the determination of each of these three ways of attrition is not likely to be random, what is pertinent to our study is whether the group of attrition as a whole is systematically different from the group staying in our sample. If it is not, then attrition can be treated as random draws from the existing sample.

[7] There are several reasons why the tenure is so low. One is to limit the chances of corruption; another is to prevent local leaders from building their own power bases; the most important, though, is the promotion tournament itself — to move from the bottom to the very top, it would take a person several decades, so his tenure at one post has to be short enough to allow that happen before he gets to the retirement age.

3 The Econometric Specification and Identification

3.1 Econometric Specifications

Our main purpose is to compare leaders by their personal abilities. Obviously there is no direct way for us to do that, especially when we do not have information on their education levels and career backgrounds for most of the time. Similar to Jones and Olken (2005), we rely on comparing the economic growth rates in their tenure periods to infer their relative abilities. To be sure, what we will get may not be their real abilities. Thereafter, we will use the term "personal effects" to describe what we aim at obtaining. To begin with, we note that the economic growth rate of a city in a particular year is related to four unobserved factors in addition to observed covariates, namely, the year fixed effect, the city fixed effect, and the party secretary and mayor's personal effects. The year fixed effect is orthogonal to the other three effects and can be identified by including the year dummies in a panel regression with the growth rate as the dependent variable. On the other hand, the city fixed effect and the two personal effects share the same dimension of data and cannot be readily identified. Our aim is to find a strategy to disentangle these three effects. For that, we start with a discussion of the relationship between the party secretary and the mayor's personal effects.

As we pointed out in the last section, the party secretary and the mayor share power in a city. With no *prior* knowledge on how they interact with each other — there would be tremendous variations across cities even if we did have the knowledge — any specification that requires them to complement or substitute with each other would yield biased estimates. [8] In our baseline specification, therefore, we treat them as independent draws from a common distribution; [9] in other words, the party secretary and mayor contribute to our sample as two different observations. With this in mind, our baseline specification is the following three-way fixed-effect model:

$$y_{jt} = X_{jt}\beta + \theta_i + \psi_j + \gamma_t + \epsilon_{ijt} \tag{1}$$

where y_{jt} is the real GDP growth rate of city j in year t, X_{jt} is a set of time-varying controls, θ_i is the personal (fixed) effect of leader i (either a party secretary or a mayor) of city j in year t, ψ_j is city j's fixed effect, γ_t is the fixed effect for year t, and ϵ_{jt} is the random disturbance. In X_{jt} we include per capita GDP and population of city j in the starting year of leader i's

[8] For example, a sensible way to handle the two effects seems to assume that they are additive. However, this will lead to two problems. One is that it implies that the mayor and the party secretary substitute with each other, which is a strong assumption hard to be substantiated. The other is that it will make our identification more difficult because we need to disentangle three effects, the city fixed effect, the party secretary effect and the mayor effect, that share the same dimension of data. While there are ways to conduct the identification, the size of each connected sample will be small and the results will not be reliable.

[9] More precisely, we treat each pair of a leader and a city's annual growth rate as an independent draw from a common distribution.

tenure, and the provincial GDP deflator.[10] Under the maintained assumption of exogeneity

$$E(\epsilon_{ijt}|X_{jt}, \theta_i, \psi_j, \gamma_t) = 0$$

equation (1) is a revised version of the regular growth equation.

Note that by using equation (1), the GDP growth rate, as well as the right-hand-side variables other than the personal effect, appears twice in the dataset: one for the party secretary and the other for the mayor. In effect, we are stacking together the data of two separate regressions for party secretaries and mayors. The gain of the stacking is that we get one common set of estimates for the other right-hand-side variables.

Note also that equation (1) is built on the idea of decomposition of variance. We rely on the different tenures of leaders having served in the same city to attribute that city's economic growth to these leaders' personal contributions. In particular, the paired party secretary and mayor are assumed to make separate contributions. Therefore, if a party secretary and a mayor had their tenures perfectly coincided (i.e., they either worked in exactly the same years in the same city or moved together to another city), their estimated personal effects would be the same. Fortunately, we do not have any pair of such party secretaries and mayors.

Instead of assuming that mayors and party secretaries are independently drawn from the same distribution, we can also assume that the pair of mayor and party secretary working in a city in the same year is drawn from a bivariate distribution so we estimate the following system of equations:

$$\begin{aligned} y_{jt} &= X_{jt}\beta + \theta_i + \psi_j + \gamma_t + \epsilon_{jt} \\ y_{jt} &= X_{jt}\beta' + \theta_{i'} + \psi_j + \gamma_t + \epsilon'_{jt} \end{aligned} \tag{2}$$

where i is the index for mayors and i' is the index for party secretaries. When the year and city fixed effects are imposed to be equal in the two equations, the identification will be the same as in equation (1). Though, the number of cases is reduced by half.

3.2 Identification

In most panel-data analysis, researchers focus on the coefficients of covariates and add fixed effects only as controls to eliminate unobservable within-group-invariant factors. In this paper, we care about the fixed effects themselves. However, we have three sets of fixed effects to estimate while the data of economic performance only have two dimensions, i.e., city-leader pair and the calendar year, so there is indeterminacy between the city and leader fixed effects. Following the literature of firm-employee matched data in labor economics (Abowd et al, 1999; Abowd et al, 2002; Abowd and Kramarz, 2006; Bertrand and Schoar, 2003; Cornelissen, 2008),

[10] We could find the city GDP deflators only after 2000; we use the provincial deflator for all years for the sake of consistency. We have tried replacing the provincial deflator with the city deflator for years since 2001 onward, and the result is not changed much.

we would like to stress the indeterminacy in two aspects. First, not all leader and city fixed effects can be identified; only those leaders and cities "connected" by leaders switching between cities can be identified. Second, even within a connected group, only the difference of the leader or city fixed effects is meaningful while their magnitudes can be arbitrary.

Figure 4 provides a simple illustration with only one leader in each city. In the figure there are three cities and six leaders. Leaders 1 and 2 only served in city A, leader 3 served in both city A and city B, leader 4 only served in city B, and leaders 5 and 6 only served in city C. City 1 and city 2 are connected by leader 3 who served both cities. Because of that, all the four leaders having served in the two cities are also connected. We then call cities A and B and leaders 1 to 4 a connected group. In contrast, city C has not a leader switching to the other two cities, nor does it have a leader coming from the other two cities. So city C and leaders 5 and 6 form a separate group.

[Figure 4 inserted here.]

Because cities and leaders share the same dimension in the space determining economic performance, normally we can only identify the sum of the leader fixed effect and the city fixed effect $\omega_{i,j} = \theta_i + \psi_j$. As a result, the fixed effect of city C ψ_C cannot be separated from the fixed effects of leaders 5 and 6, θ_5 and θ_6. So ψ_C cannot be identified. However, the difference of θ_5 and θ_6 can be identified because it is equal to $\omega_{6,C} - \omega_{5,C}$.

In the connected group, we can do more using the connection created by the "switcher" leader 3. Firstly, subtracting $\omega_{3,A}$ from $\omega_{3,B}$ we get the difference between city A and city B's fixed effects $\psi_B - \psi_A$. Then subtracting $\omega_{3,A}$ from $\omega_{1,A}$ and $\omega_{2,A}$ we get the difference between leaders 1 and 3 and the difference between leaders 2 and 3, respectively. Finally, subtracting $\omega_{3,B}$ from $\omega_{4,B}$ we get the difference between leader 4 and leader 3. With that, we can finally compare all the four connected leaders. However, the values of θ_i and ψ_j are not unique. For example, we can add 1 to ψ_j for all j and subtract 1 from θ_i for all i and leave $\omega_{i,j} = \theta_i + \psi_j$ unchanged.

The study of both the mayor and the party secretary in a city complicates the identification problem. In this regard, our two specifications help us out. By both specifications, we can treat a pair of party secretary and mayor working in the same city in the same year as if they were working in two separate albeit identical cities. Because they share the same city fixed effect, they can be treated as if working in the same city, so the size of the connected group increases by an exponential order. In the meantime, because they are treated and estimated separately, their $\omega_{i,j}$ have different values despite sharing the same city fixed effect as a component, we can separate their personal effects from each other.

In conclusion, we can identify the differences of fixed effects between leaders as well as between cities within a connected group and can identify the differences of fixed effects between

leaders having served in the same city. In this sense, moving local officials from city to city increases the size of the connected group, which makes it feasible to compare personal qualities within a larger amount of cities. As long as there is one switcher during the sample period, we can compare the fixed effects between the cities as well as between all leaders who served in those cities, even if they did not move at all. This may be a key reason why the central government in China keeps moving officials across cities and provinces.

In our long sample of 1,671 leaders, the 246 movers allow us to identify 20 connected groups plus 27 isolated cities and 191 isolated leaders. Table A4 provides detail information on the groups. As one can see, among the connected groups, many groups are small. But group 5 is sufficiently large for our analytical purpose. This group consists of 85 cities, 558 leaders (among which 101 are movers) and 2,242 leader-year pairs (observations); that is, it accounts for about one third of the whole sample. Thereafter we will confine our analysis to this group and simply call it "the connected sample". In Figure A1 the connected sample of cities is painted in a darker color contrasting to other sample cities in a lighter color.

Based on the connected sample, we can estimate equation (1) and the system of equations (2) to test whether leaders matter. As noted above, we can only identify the differences between cities and between leaders. But to save notation, we set the mean of θ_i's to 0 and still use θ_i and ψ_j to denote those differences. Then for equation (1) an F test on the joint significance of θ_i suffices for our test, and for the system of equations (2), we can rely on the χ^2 test for $\theta_i = \theta_i' = 0$. Both tests share the same idea as Jones and Olken's, i.e., they rely on the variation among leaders to answer the questions whether leaders matter. However, the Jones and Olken test is a stronger test than our tests. This is primarily because the Jones and Olken test, if it were to be applied to our case, would only consider within-city variations, but our tests consider both within-city and between-city variations so a rejection of the null by the Jones and Olken test definitely implies a rejection of the null by our tests. Conversely, if the Jones and Olken test accepts the null, it does not mean that leaders do not matter, as Jones and Olken themselves have noticed, because leaders in different cities may perform differently. However, an acceptance of the null by our tests does not mean that leaders do not matter either. This is because we can only estimate the differences among leaders and cannot estimate the absolute values of their personal effects. That is, an acceptance of the null can imply that leaders are equally capable. Because of the dimensional limitation involving leaders and cities, this is by far the best one can achieve. Our improvements on Jones and Olken's test thus are two folds. One is that our test has a smaller probability of making the Type II error than Jones and Olken's, and the other is that we use more information and an acceptance of the null is a more decisive indicator that leaders are all the same and do not matter for local economic growth in the sense that shuffling them around does not have any effect.

To see how our tests would differ from that in Jones and Olken (2005), we also compose a

similar χ^2-test using only within-city variations. Our data have multiple turnovers of leaders in a single city so the PRE and $POST$ in their paper are not clearly defined in our case. But consider a city A with 3 consecutive leaders with fixed effects $\theta_{1,A}$, $\theta_{2,A}$ and $\theta_{3,A}$. In the first turnover, $POST-PRE = \theta_{2,A}-\theta_{1,A}$; while in the second turnover, $POST-PRE = \theta_{3,A}-\theta_{2,A}$. So the parallel test is $\theta_{2,A} = \theta_{1,A}$ and $\theta_{3,A} = \theta_{2,A}$, which can be reduced to $\theta_{i,A} = 0$ for all i since the mean of θ_i's can be set to zero.

In practice, we can estimate equation (1) using the whole long sample and then conduct the following test. First, denote $\bar{\theta_j}$ as the average leader personal effect in city j and $\phi_{i,j} = \theta_i - \bar{\theta_j}$ as leader i's relative personal effect in city j. Note that $\phi_{i,j}$ can be also calculated by demeaning $\omega_{i,j}$ within cities. We then execute a χ^2-test with $H_0' : \phi_{i,j} = 0$ for all i and j. If we reject the null, then we can conclude that leaders differ even within a city, as done by Jones and Olken; if we accept the null, then we conclude that leaders do not outperform each other within a city.

3.3 Other Econometric and Measurement Issues

Our test may be hampered by several kinds of issues involving our sample and measurement. One is nonrandom entry and attrition, which affect the composition of our sample; another is endogenous moves of leaders who are already in our sample; and the third is transitory shocks. All of them may bias our estimates of the leaders' personal effects.

The entry of leaders becomes an issue if it changes the composition of our sample. This is particularly important if there exists a cohort effect. For example, if more recent leaders were more capable than their predecessors, the variation in our sample increases. However, this issue is lessened when one realizes that leaders' capabilities should be factored in when the question is asked whether leaders matter. Nevertheless, we will check whether there are significant cohort effects in the estimated leaders' personal effects.

Nonrandom attrition can be a more serious issue for us. However, because attrition involves three exits, i.e., retirement, moving to a city not covered by our sample, and promotion, that are unlikely to require the same level of leaders' personal abilities, nonrandom attrition may not be a serious issue in our sample since most leaders finally leave the sample as time goes by while the rest are subject to right censoring in the year 2008. We will conduct a test that checks the correlation between attrition and the residual from equation (1) to further legitimize our analysis.

Following the labor economics literature (e.g., Abowd et al., 1999), we need to be concerned about endogenous switches of leaders from one city to another in addition to the conventional orthogonal condition. While endogenous switches can take many forms, we are more worried about those that inflate or deflate the variation among the leaders because the variation is the key to our test. In this case, the complementarity effect is the form of endogeneity that we need to pay more attention.

The complementarity effect arises when the match between leaders and cities enhances or reduces leaders' personal abilities. Our test relies on detecting the variation among leaders, but the complementarity effect may inflate or deflate the variation. There are several cases in which this effect can do that. First, an originally mediocre leader may improve his ability when he is placed in a city with significant challenges. For example, a leader in an already prosperous city may only show average ability, but when he is moved to a less prosperous city, he may register a remarkable performance record. Second, experience in certain types of city can enhance a leader's ability, so when he is moved to another city, he performs better than the average leader. For example, experience in a more open city may enhance a leader's ability to boost international trade, so when he is moved to a less open city, that city begins to export more. In both cases, the variation among leaders is enlarged due to the complementarity effect. Then third, it is intuitive to understand that reverse movements of case 1 and case 2 can deflate the variation among the leaders.

In addition to endogenous sample composition and switches, we also need to worry about city-specific transitory shocks. In essence, we identify a leader's personal effect by the specific effect of his tenure in a city if he never moved or the average specific effects of his tenures in multiple cities if he has moved. In a sense, this is also how Jones and Olken (2005) measure the leader effect in their paper. But we have the added problem of comparing leaders across the borders, so city-specific transitory shocks can inflate or deflate our estimates of the leader effects. There were three rounds of economic booms and recessions during the 15 years of our sample and they were not uniform across regions. As a result, leaders of different periods and different cities may benefit or suffer from the boom or recession, respectively. The provincial GDP deflator is meant to capture the regional and temporal variations of those booms and recessions; together with the city and year fixed effects it can absorb much of the transitory shocks. However, by the specification in equation (1), which we believe is the best for our purposes, our measure of the leader personal effects is bound to pick up some of the city-specific transitory shocks even with those controls. The key is not to get rid of them, but to show that they do not affect the relative ranking of leaders, which is what we can identify anyway. For that, it suffices to show that the transitory shocks are random with respect to leaders. It turns out that the transitory shocks and endogenous switches are related to each other when we try to deal with them. We will then examine them with three tests.

In the first test, we test whether moves affect leaders' personal effects. Even if moves are not random — for example, better leaders are selected to move — they do not affect our estimates if they do not increase or decrease movers' personal effects. A similar argument can also be made for transitory shocks. Movers, as they are called by the name, worked in at least two cities, so they should receive different shocks in their careers, and their personal effects should then increase or decrease due to their moves if those shocks are not random. Putting

the two arguments together, we estimate a variant of equation (1) as follows:

$$y_{ijt} = X_{ijt}\beta + \theta_i + \alpha_i \text{Move}_i + \psi_j + \gamma_t + \epsilon_{ijt} \tag{3}$$

where $move_i$ is a dummy variable indicating the years a mover served beyond his first city; that is, it is the subset of a mover's personal dummy excluding his tenure in his first city. As a result, θ_i is the personal effect of leader i in the first city and α_i is the added value of his switches. [11] Note that we can only estimate α_i for movers. An F-test of the joint significance of α_i for them provides a decisive conclusion on whether switches, endogenous or exogenous, and transitory shocks affect our measure of individual leaders' personal effects.

In the second test, we examine the difference between the pair of party secretary and mayor having served in the same city for a period of time. To support our identification strategy, our first test needs to accept the null. However, this acceptance could be incidental, i.e., a result driven by unknown and unsubstantial factors. A comparison between the partner party secretary and mayor can then serve as a placebo test. They shared the shocks equally in the years when they worked together. But since their tenures were different, we are able to estimate separate personal effects for them. However, if the null of our first test is rejected because of incidental factors, the pair of personal effects we estimate for the partner party secretary and mayor may not be different although in theory they have to. To provide an answer, we execute a pairwise comparison for each city-year cell between the personal effects of the party secretary and the mayor with the null $H_0 : \theta_{PS,jt} = \theta_{Mayor,jt}$ for all city j and year t. Under the condition that the null of our first test is accepted, a rejection of this null indicates that party secretaries are different from mayors, even if they have worked together, not because they have moved, but because they are different in abilities. Put in another way, this shows that the common shocks that a party secretary and his partner mayor received in a particular city do not have persistent effects on them.

Finally in the third test, we keep only movers in the connected sample and re-estimate equation (1). The connectivity is built on movers; as long as movers remain in the sample, the connections will not change, so we can retain the same connected cities as before. If moves have been proven not to affect leaders' personal effects in our first test, we do not need to worry about the endogenous composition of this smaller sample. Movers worked in more than one city, so they were supposedly subject to the transitory shocks in different cities. Therefore, unless luck was systematically in favor or against a particular leader, one has to conclude that the effects of city-specific transitory shocks are averaged out for the movers and a comparison among them reveals their relative contributions to economic growth.

[11] As Table A2 shows, less than 2% of the leaders served more than two cities. We treat those leaders as having had one switch.

4 Leaders and Economic Growth: Empirical Results

4.1 The Jones and Olken Test

We first conduct the Jones and Olken test. We do this twice, one using the whole sample and the other using the connected sample. For the whole sample, the χ^2 statistic is 0.126 and the p-value is 1.000 under 2,042 degrees of freedom. As for the connected sample, the statistic is as small as 1.74×10^{-5}, and the p-value is 1.000 under 676 degrees of freedom. Thus we accept the null hypothesis with a large margin and conclude that within-city variations are not sufficiently large. However, we may make the Type II error if we conclude that leaders do not matter for local economic growth because leaders' personal effects may vary between cities.

4.2 Tests Based on the Benchmark Specification

We conduct the F-test by estimating equation (1) using the connected long sample of 558 leaders and a total of 2,242 observations of leader-year pairs. Following the solution in Cornelissen (2008), we impose the following zero-mean constraint in our estimation

$$\sum \theta_i = 0$$

By imposing this constraint, it is straightforward to apply the standard F-test that θ_i's are all zero. The resulted F-static is F(557, 1,583)=1.61, and the p-value is smaller than 0.001. That is, the null hypothesis that leaders do not matter is rejected with a large margin. We also conduct two more F-tests for mayors' personal effects to be jointly zero and for party secretaries' personal effects to be jointly zero. The resulted F-statics are 1.45 and 1.76, respectively, whose p-value are both less than 0.001.

The estimates for population and initial per-capita GDP level both return negative and significant coefficients. The result of the initial per-capita GDP fits into the story of convergence, and the other result implies a certain burden of a larger population. The estimated coefficient for the GDP deflator is also negative and significant, showing that inflation is bad for economic growth.

The role of leaders can also be shown by an analysis of variance. Table 1 shows the share of variance of real GDP growth that the city, year, and personal dummies respectively explain in the connected sample. The city dummies alone explain only 5% of the total variation, the year dummies explain almost 18%, and the personal dummies explain 27%. If we have correctly measured the personal effects, this shows that leaders have played a significant role in explaining local economic growth.

[Table 1 inserted here.]

In Figure 5, the filled line is the kernel density of the estimated individual leader effects. Table 2 provides summary statistics of the distribution of all leaders. The standard deviation

is relatively small and the kurtosis is large, but the distribution is skewed left, indicating that there is a group of leaders with relatively low personal abilities. Figure 6 then separates the party secretaries and the mayors. The distribution of the mayors slightly dominates the party secretaries' although the gap is not statistically significant.

[Figures 5 and 6 and Table 2 inserted here.]

4.3 Tests Based on the Alternative Specification

We estimate the system of equations (2) by imposing the restriction that the city and year fixed effects are respectively equal to each other in the two equations. [12] Then we conduct two separate tests. One is that the mayors have the same personal effects, and the other is that the party secretaries have the same personal effects. The first test returns a χ^2 (338) = 308.20, which implies that the null can be accepted by a margin of 87.6%. That is, the mayors are not distinguishable from each other. The second test returns a $\chi^2(285) = 316.58$, which implies that the null can be rejected at the 9.6% significance level. That is, the party secretaries are sufficiently diverse among themselves. Adding the mayors and party secretaries together, we conclude that leaders are sufficiently different because a subset of them, the party secretaries, are sufficiently different.

For a comparison with the benchmark specification, we present in Figure 5 the kernel density of the estimated personal effects of mayors and party secretaries added together (the dotted line). Compared with what we get under the benchmark specification, the new distribution is much more concentrated. Fortunately, our tests presented above still show that leaders are sufficiently different from each other. Figure 7 then shows the kernel densities of the mayors and party secretaries separately. Although there are differences, the two distributions are broadly similar.

[Figure 7 inserted here.]

4.4 Tests of Sample Composition

We conduct our robustness tests using the results from the benchmark specification. We first explore whether there are significant cohort effects among the estimated leader effects. Figure 8 shows the average personal effect of leaders entering our sample in the same year during the period 1995-2008.[13] Leaders entering in the earlier years and later years have higher personal effects than those entering in the years in between. While more research is needed to explain

[12] There were cases in which mayors were moved to another city to become the party secretary. Those leaders appear in both equations, one as mayor and the other as party secretary. We impose that they have the same personal effect in the two equations.

[13] The year 1994 is subject to left censoring so its data are not shown.

this pattern, the relevant message coming out of the figure is that there is not a linear trend so we do not need to worry about systematic biases in the composition of our sample.

[Figure 8 inserted here.]

To check the randomness of attrition, we compose a dummy dropout_{it} indicating that leader i leaves the sample in year t. We check the correlation between dropout_{it} and the residual from equation (1) ϵ_{ijt} and find that the correlation coefficient is -0.133 with a standard error of 0.17. That is, attrition is orthogonal to the error term.

4.5 Endogenous Moves and Transitory shocks

As a first step to check endogenous switches, we first see if movers are significantly different from non-movers at the mean. For that, we regress the estimated leader effects on a dummy indicating movers and a constant. This returns a coefficient for the dummy of 0.010 with a standard error of 0.00369. That is, movers on average are more capable than non-movers. However, this can be caused by the higher abilities of the movers relative to the non-movers, not necessarily a result that switches improve movers' abilities. To settle the issue, we need to wait for the results of equation (3). After equation (3) is estimated, we conduct an F-test for the joint significance of α_i's for the movers, which returns F(101,1,547)=0.67 and a p-value of 0.95. Therefore, we conclude that switches do not affect individual leaders' personal effects. To show the randomness of term terminations and switches, we create two dummies indicating these two events. By regression the dummies on the residual produced by equation (1), we find that neither terminations nor switches is predictable by the residual. That is, they are orthogonal to the error term.

Secondly, we run the test of pair-wise comparison of the party secretary and the mayor. Under the null $H_0 : \theta_{PS,jt} = \theta_{Mayor,jt}$ for all city j and year t, the t value is -2.519. With 1100 degrees of freedom, the p-value is 0.012, which implies that the personal effects in each pair are different at the mean.

Thirdly, we re-estimate leaders' personal effects using only the sample of movers. The F-value for $H_0 : \theta_i = 0$ for all i is F(100,495)=1.5 with p-value equal to 0.003. That is, leaders having served more than one city are still sufficiently different. Under the condition that the transitory shocks they received in different cities were random across the board and thus are averaged out, the differences among the movers reflect their varying contributions to the local economy.

With those test results, we conclude that endogenous sample composition and endogenous switches do not affect our main results, and our measure of the leader personal effect does not merely capture transitory shocks. This last conclusion is greatly facilitated by the inclusion of the movers in our sample. There is not *a prior* reason to believe that any mover should be

subject to systematic good or bad shocks throughout his career in different cities. [14] Our first two tests show that transitory shocks are random, so the differences among the movers reflect their relative ranking of contribution to economic growth.

The randomness of entry and attrition could be a result of the relatively large size of our connected sample. The inability of switches to improve individual leaders' personal effects could be explained by the promotion procedure. As we showed in Section 2, the leaders in our sample had a very short tenure in each city. The downside of a short tenure is obvious: it encourages leaders' short-sighted behavior. One of the explanations is that short tenures are used to prevent leaders from forging personal networks for corruption and accumulation of political power. If that is the case, it is better for the central/provincial authorities to randomly shuffle city leaders. Another explanation is related to promotion. The central/provincial authorities want to promote leaders with higher personal abilities. But they have to face the same challenge that we face to identify leaders' personal effects. Shuffling leaders among cities thus can be a strategy for them to identify city leaders' true abilities, just like how we conduct our identification using the connected sample. With this in mind, one realizes that even a random shuffling is sufficient to allow the central/provincial authorities to conduct a complete comparison of city leaders serving in the "connected" cities. However, we do find that movers on average have higher abilities than non-movers and these higher abilities are not caused by the move itself. This leads us to two conclusions regarding the promotion process. First, the central/provincial authorities can identify better leaders; and second, they move better performing leaders more often to show a kind of favoritism to them — in reality, moving from a city to another is often seen as a prelude for a leader to get promoted, a perception often confirmed.

4.6 Sectoral Effects

As a final note on the role of leaders on local economic growth, we study their heterogeneous effects across the primary, secondary and tertiary sectors. In the literature (e.g., Xu et al., 2007), a switch of provincial leaders is found to be associated with different rates of sectoral growth; the secondary sector seems to benefit more. The secondary sector accounts for about half of the Chinese economy; its share is even higher in localities that are more open to international trade and thus are experiencing faster rates of economic growth. In addition, businesses in the secondary sector are more mobile than those in the other two sectors, so local leaders have to spend more efforts competing with each other in this sector. It is therefore of interests to see whether more capable leaders are associated with higher growth rates of this sector.

[14] There were altogether 118 moves of leaders between cities in the connected sample, among which 81 cases were moves from a richer city to a poorer one and 37 were the reverse. The correlation between the average real GDP growth rates of the first city and the last city a leader served is 0.11 and statistically insignificant.

We use the personal effects estimated from our benchmark specification to conduct our regressions. Because these effects are relative measures of leaders' personal abilities, we transform them into percentile rankings (called "leader ranking" thereafter) in the interval [0, 1]. This will facilitate the interpretation of results. In addition, it is also consistent with the promotion tournament in which only relative rankings matter for promotion. [15] Table 3 reports two sets of results. Regressions (1)-(3) study the growth rates of the three sectors, and regressions (4)-(6) study their shares in the local GDP. It is clear that the leader ranking has a larger effect on the growth of the secondary sector than on the other two sectors, and in accordance, it increases the share of the secondary sector and reduces the shares of the other two sectors.

[Table 3 here]

5 Personal Effects and Promotion

5.1 Methodological Issues

As we pointed out in the introduction, we have two purposes in mind to study how personal effects affect promotion. One is to test the sensitivity of our estimates of the personal effects, and the other is to improve on the literature of the promotion tournament. Li and Zhou (2005) find that the average GDP growth rate of a provincial leader's tenure is a good predictor for his probability of promotion and retirement. However, Opper and Brehm (2007) and Wang and Xu (2008) provide different results. Opper and Brehm (2007) define an index of political connections and find that it is a strong predictor for the promotion of provincial leaders while the local growth rate has no predictor power. Yet Wang and Xu (2008)'s results seem to reject the political connection story. While they find that provincial party secretaries and governors who are later promoted to the central government do not significantly outperform others, they also find that the provincial leaders who come from and then go back to the central government even underperform the average. The controversy may have a lot to do with directly using the GDP growth rate as the predictor. The GDP growth rate may not be a good indicator for a leader's personal ability because it is highly correlated with local conditions, some of which change over time and cannot be accounted for by the provincial fixed effect. One of the regularities concerning the promotion is that almost all the members of the standing committee of the political bureau, the center of the CCP leadership, have been either directly promoted from or have worked in the few most advanced provinces as well as Beijing and Shanghai. Because those localities enjoy preferred economic policies during various periods, Li and Zhou (2005)'s significant results may well reflect some peculiarities than general rules linking promotion to performance. One component of Opper and Brehm

[15] We have also tried using untransformed personal effects in our current and subsequent regressions and found similar results.

(2007)'s index of political connection is whether a leader has worked in provinces that a member of the standing committee has worked for. So their results may suffer the same problem of incidental correlation. [16] Lastly, ministry-level officials in the central government may be sent to provinces only for them to gain local experiences, which could lead to Wang and Xu (2008)'s findings.

Our data and identification strategy allow us to improve the existing literature. On the one hand, the personal effects estimated from equation (1) reflect leaders' own contributions to cities' economic performance; on the other hand, the promotion of city leaders is less subjected to political considerations. We only have data of promotion after 2001 and can only estimate the personal effects of leaders in the connected sample. So our estimation of the relationship between personal effects and promotion is performed for the period 2001-2008 using the connected sample. This allows us to examine 211 leaders. As we did in the study of sectoral effects, we use the leader ranking to do the study.

At any point of time, there are three possible states for a leader: promotion, retirement, and staying as a city leader (including moving to another city). We adopt two econometric methods to conduct our study. One is to use the linear probability model (LPM) to make two comparisons, one between promotion and the other two options together, and the other between promotion and staying alone. The LPM has the advantage of being able to provide straightforward and unconditional predictions based on each estimate alone. Our two comparisons are meant to see how retires affect our results. When a leader approaches the age of 60, the chances of promotion diminishes quickly even if he is capable. So including retirees in the comparison may cause a downward bias on the estimate of leader ranking. However, since we define moving to a non-executive job also as retirement, excluding all the retirees also forces us to compare those promoted with a younger comparison group of leaders not promoted. To the extent that younger leaders have the age advantage to get promoted, our estimates may also be biased downward if we exclude the retirees. The net outcome depends on which effect is stronger. The results of our two comparisons can be examined against each other to find which the case it is.

The other estimation method we adopt is the multinomial logit model (MLM). [17] The MLM has the advantage of treating all the three states simultaneously and thus avoiding potential biases in the estimates of the LPM due to the failure of accounting for the correlation between states. However, it does not provide direct unconditional predictions. [18]

[16] Another component of their index is whether a leader went to the same university that a member of the standing committee went to. Because most of the top leaders graduated from the few top universities, this can also cause an incidental correlation.

[17] Li and Zhou (2005) apply the ordered Probit model. The MLM provides more flexibility.

[18] It is noteworthy that both LPM and MLM do not account for the time dependency of promotion. In this regard, the competing risks model is a more appropriate method. However, the difficulty is to decide the time path of the competing risks. A person's tenure as a city leader seems to be a natural choice because being a city leader longer increases both the risks of promotion and retirement, but age can also be an equally qualified

For both the LPM and MLM, there is an issue regarding the time point we adopt to do the comparison. Because a leader's ranking is time-invariant in our sample, it seems that a sensible time point for comparison is when a leader leaves our sample. We call this comparison "career-based" comparison. The drawback of this comparison is that it may ignore the heterogeneities across time and by doing this we also lose promotions and switches between cities that are both in the sample. [19] Alternatively, we can compare leaders when they leave a city. We call this comparison "term-based" comparison. [20] This comparison will allow us to account for more heterogeneities across time as well as promotions within the sample. However, it is not so obvious why the leaving year should be chosen as the time point for comparison — a leader faces the risks of promotion, moving to another city, and, to a lesser extent, retirement each year and the leaving year is just the year one of the risks is realized. Viewing against this, a third approach is thus just to make the comparison each year. This has the advantage to overcome the issues encountered by the first two approaches. However, there is no variation in the leader ranking for an individual so its estimate may have a larger standard error. In practice, we will present results for all three kinds of comparison and see how they will differ from each other.

Note that there is a censoring issue with the leaders who remained in our sample by 2008. Neither the LPM nor the MLM can deal with this issue. In our analysis, we simply drop the 2008 data of those leaders. This means that when career is used as the observation unit, we are making comparison among leaders who eventually left our sample, i.e., those who were promoted, retired, or moved to a city outside our sample. Our tests in the last section have shown that this subsample is not statistically different from the whole sample, so our comparison is equivalent to a comparison using the whole sample. In accordance, we will make comparison among leaders who had finished their current terms, i.e., those who were promoted, retired, and moved to another city, when term is used as the observation unit.

One final remark is about the spatial scope of comparison. While there have been leaders moving to cities out of their own provinces, it has been more often that city leaders are promoted or shuffled within the same province. Among the 101 movers in the long connected sample, only 15 served in two different provinces; among the 148 leaders who enjoyed promotion, only 6 were promoted outside the province. That is, competition among leaders is mostly restricted within the same province. This suggests that we add the provincial dummies in our regression analyses. We could also use the city dummies, but that confines the scope of comparison to

candidate because the risks of promotion and retirement can follow significant patterns as one's age increases. As a result, we do not adopt the competing risks model. We add age, city tenure, and year dummies in both the LPM and MLM to control time dependency.

[19] Promotions within the sample include moves from mayor to party secretary and from an ordinary city to a sub-provincial city.

[20] Here "term" is not the official term of five years, but refers to a leader's spell in a specific city for a specific position (i.e., secretary or mayor).

an individual city, which is too restrictive.

In both the LPM and the MLM, in addition to the leader ranking, provincial dummies, and year dummies, we control the following variables: leader age, age squared, number of years since a person became a city leader (city tenure), and a dummy variable indicating whether a leader worked in the provincial government or not (provincial experience). To compare with the results of existing literature, we will also add the growth rate of per-capita GDP in some of the regressions. Age could be the most important factor in addition to ability determining a leader's chances of promotion; the promotion tournament constantly eliminates people so being young can be a big advantage if one wants to move upward in the bureaucratic hierarchy. However, age also means experience. So promotion can be positively correlated with age up to a certain level. Therefore, we set a quadratic form of age in the regression, as in Li and Zhou (2005). Also for the reason of the promotion tournament, starting serving as a city leader earlier increases a person's chances of promotion. Lastly, provincial experience is meant to capture the influence of political affiliation. Having worked in the provincial government should increase a person's chances of getting promoted if political connection is important. However, it could also be the case that coming down from the provincial government to work in a city is a kind of exile for a person because it means that he cannot get promoted in the provincial government.

When annual data are used, the control variables are measured by year. When term is used as the observation unit, except the GDP growth rate which takes the average in a term, all the control variables are measured for the year when a leader ended his current term. Lastly, when the whole career is used as the observation unit, the GDP growth rate takes the average of a leader's whole career and the other control variables are measured for the year when a leader leaved our sample.

5.2 Results of the LPM

Table 4 presents the results of four regressions of the LPM using annual data. Regressions (1) and (2) compare the promoted leaders with all the rest and the stayers, respectively, and include all the control variables except the growth rate of per-capita GDP. The leader ranking is weakly statistically significant in regression (1) and is significant at the 10% significance level. The two regressions show that the leader of the highest ranking is about 12% more likely to get promoted than the leader of the lowest ranking. In the short sample, about 18% of the leaders got promoted each year when we consider the annual regression. Therefore, the effects shown by these two comparisons are meaningful in real terms.

[Table 4 here]

Among the control variables, age is shown to have a significant and concave effect on promotion. In both regressions, chances of promotion increase with age until one reaches the average age of the sample, 50 years old, and then begin to decline. The finding agrees with the competition in the government hierarchy. If one can rise steadily in the hierarchy, he is normally promoted by one rank up in every three to five years. This means that it takes 20 to 30 years for a person to go beyond a city leader. Assuming a person begins a governmental job in his early 20s, 40 - 50 years old then is the crucial period for him to move beyond a city post provided he has not been eliminated in the tournament.

City tenure is highly significant in both regressions. One more year as a city leader increases a person's chances of promotion by 3.4 and 5.0 percentage points by the results of the two regressions, respectively. Provincial experience has positive but statistically highly insignificant coefficients. So on average coming down from a provincial post is neither an exile nor a prelude for promotion.

Regressions (3) and (4) modify regressions (1) and (2) by replacing leader ranking by the growth rate of GDP in each regression. While the results of the control variables remain largely intact, the coefficient of the GDP growth rate is positive but statistically highly insignificant in both regressions. Regressions (5) and (6) then add the leader ranking back. The GDP growth rate is still insignificant, but the effect of leader ranking is weakened. Now, not only it is marginally significant when promoted leaders are compared with the rest of the leaders, but it is also barely significant at the 10% significance level when promoted leaders are compared with those stayed. As we pointed a while ago, the statistical significance of leader ranking is likely to be reduced when annual data are used. Nevertheless, we can still conclude that the leader ranking is a better predictor than the GDP growth rate for a leader's chances of promotion. From this perspective, we provide a finer test than Li and Zhou (2005).

[Table 5 here]

Table 5 presents the LPM results by individual leaders' terms, repeating all the six specifications of Table 4. Now we have stronger results. Leader ranking is statistically significant in all the regressions that include it. The economic significance implied by the estimates is remarkable: the leader of the highest ranking is 30% more likely to get promoted than the leader of the lowest ranking. The GDP growth rate, again, does not return any significant result. We have also repeated the six specifications of Table 4 using career-based data. However, leader ranking does not return any significant result. One plausible explanation is that the career-based regressions fail to capture the temporal heterogeneities whose impacts are entangled with the impact of leader ranking. For example, our MLM results presented below show that the effect of leader ranking depends crucially on a leader's age.

5.3 Results of the MLM

The MLM may be more sensitive than the LPM to the variation of data because of its non-linearity. So when annual data are used, it does not return any significant results for leader ranking, which does not change for a leader through the years. The term and career-based results, presented in Table 6 and Table 7, respectively, are more promising. We treat staying as the reference option and study how the leader ranking and other variables affect leaders' chances of promotion and retirement. Again, the specifications reported in Table 4 are repeated except that we deal with three exits simultaneously with MLM. The leader ranking is shown to significantly increase one's chances of promotion, but does not affect one's chances of retirement. The concavity of age affecting promotion is preserved, but the coefficients are only marginally statistically significant. As before, the growth rate of per-capita GDP does not return any significant result. In fact, the coefficients of leader ranking slightly increase when the GDP growth rate is added.

[Tables 6 and 7 here]

Because the LPM and the MLM both return significant results when comparisons are made by term, it seems that term-based data provide a nice balance of temporal and cross-sectional variations. [21] Therefore, we use the term-based results to make further explorations offered by the MLM. The coefficient of leader ranking is 2.0 when the GDP growth rate is not added in the regression. There are two ways to use this number to gauge the contribution of ranking to a leader's probability of getting promoted. One way is to look at the odds ratio between promotion and staying. In this regard, the odd of the leader ranked the highest to get promoted would be more than two times than his odd of staying as a city leader. Another way is to compare the probabilities of promotion of leaders with different rankings. Table 8 does that by quartile while assuming other variables at their means. The effects of leader ranking are smaller than those predicted by the term-based LPM. For example, the leader of the highest ranking is only 4% more likely to get promoted than the leader of the lowest ranking whereas by the term-based LPM the gap between those two leaders is 30%. This large difference may be caused by the fact that unconditional predictions can be made with the estimates of the LPM whereas only conditional predictions are possible with the estimates of the MLM.

[Table 8 here] [Figure 9 here]

Among the conditioning factors, age by far is the most important. Figure 9 shows the distribution of probabilities of the three exit events along age at the end of a term. It is clear that the probability of promotion is sensitive to age. It increases with age until 53 years old

[21] This may also be caused by random term termination, as we have shown in Section 4.5.

and then declines. This may also affect the impact of leader ranking. Using the results of the term-based regression and holding other variables constant at their means, Figure 10 then plots the gap of the probabilities of promotion for the leaders ranked at 0% and 100% along the age at the end of a term. A clear pattern emerges from the figure: among leaders younger than 52 years old, ranking does not distinguish them in a significant way, but among leaders older than 52, ranking plays a more and more important role as a leader becomes older. At the age 52, the gap between the lowest and the highest ranked leaders is only 2.2%, but by the age 58 and 59, this increases to 29% (it declines to 20% at the age 60). The average gap between age 53 and 60 is 16.4% whereas it is only 1.6% for age 42 to 52.

[Figure 10 here]

In summary, the term-based LPM and MLM provide qualitatively similar results for the role of ranking in determining a leader's probability of promotion. It seems that the term-based data provide a good mixture of temporal heterogeneities and matches between leader ranking and leaders' career achievements. The LPM with annual data and the MLM with career-based data also return significant results. The term-based LPM provides an unconditional prediction that the highest ranked leader has a lead of 30% over the lowest ranked leader to get promoted, but the term-based MLM shows that the impact of leader ranking increases with age and reaches almost the same level as the LPM prediction when a leader gets to 58 and 59 years old.

In the Chinese promotion tournament, younger leaders have overwhelming advantages over older leaders. The organizational department has a so-called "Leader Reserves Program" that identifies promising young cadres and places them in leadership position in their early ages in the hope that they would one day become provincial and national leaders. Young leaders in our sample might fall into that category; that is, they were prescreened before they became city leaders so their performances might not be as important as for other leaders. In contrast, older leaders were either not prescreened or left over from the reserves program and thus had to rely on performance to single them out. In addition, facing the hard constraint of retirement by the age 60, the competition for promotion can be fiercer among this group of leaders than among younger leaders.

5.4 Party Secretaries vs. Mayors

In the previous section, we have studied the relation between leaders' rankings and their probabilities of promotion assuming the party secretaries and mayors are homogeneous in this relation. However, because their duties are different, their promotion patterns may also differ with each other. Therefore, it is worth examining the possible heterogeneities between these two groups of leaders by studying their promotion separately. We only present the results of

the LPM for annual and term data. Because some mayors became party secretaries, a career-based study is not feasible. In addition, preliminary exploration has found that the MLM returns similar results.

[Table 9 here]

Table 9 presents the results. There is a surprisingly large difference between party secretaries and mayors. Leader ranking is not significant in any regression for party secretaries, but is significant in all the four regressions for mayors. In addition, the effects for the mayors are similar to those obtained for all the leaders using term data, shown in Table 5.

To make sense the contrast between the party secretary and the mayor, it would help to consider the roles that the two leaders assume in a city. As we pointed out before, the party secretary is supposed to control the personnel and to make broad decisions, while the mayor takes more responsibilities of detailed policy decision and execution for the local economy. Therefore, the party's organizational department may evaluate mayors more closely against local economic growth while taking other factors into account when it evaluates party secretaries.

6 Concluding Remarks

In this paper we study the role of subnational leaders in local economic growth and their promotion using a unique dataset collected on city leaders from 18 Chinese provinces. With the city leaders facing homogenous national institutional settings, we are able to isolate leaders' personal abilities from institutional factors that may confound the cross-country studies aiming at answering the question whether institutional or human factors contribute to economic growth. We also improve the literature on the relationship between leader performance and their promotion in China. Using a leader's ranking of estimated contribution as the predictor for his promotion, we obtain finer results than those offered by the existing studies. In particular, we find that the estimated ranking of contribution is a stronger predictor than a city's economic growth, but its effect is sensitive to a leader's age.

In addition to confirming the thesis that leaders matter for economic growth, our study also sheds lights on the promotion tournament in the Chinese political hierarchy. First, we show both theoretically and empirically that shuffling is a way to order the leaders. The purpose of shuffling is not so much about testing leaders in different cities, but to make cities connected so their leaders can be compared. As a result, the number of shuffling does not need to be large (in our connected sample, only 18.6% were shuffled). Second, age is a pivotal factor determining a leader's chances of promotion. A person's chances diminish quickly as he gets older and personal abilities become a stronger factor determining a person's chances of promotion. Third, the competition among leaders has heterogeneous effects on different

sectors; more capable leaders put more efforts on the secondary sector because businesses in that sector are more mobile than in the two other sectors. This has implications for China's internal and external imbalance problems. One of distinctive features of the Chinese economy is that the manufacturing sector takes a much larger share in GDP than in other countries. But manufacturing is more capital intensive than services and agriculture, so a larger manufacturing sector contributes to larger domestic savings (Li, Liu, and Wang, 2009; Chen and Yao, 2011). Therefore, our results show that China's imbalance problems are linked to the promotion tournament, an inherent part of China's political system.

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Figure 1: Sample and National Per-capita GDP

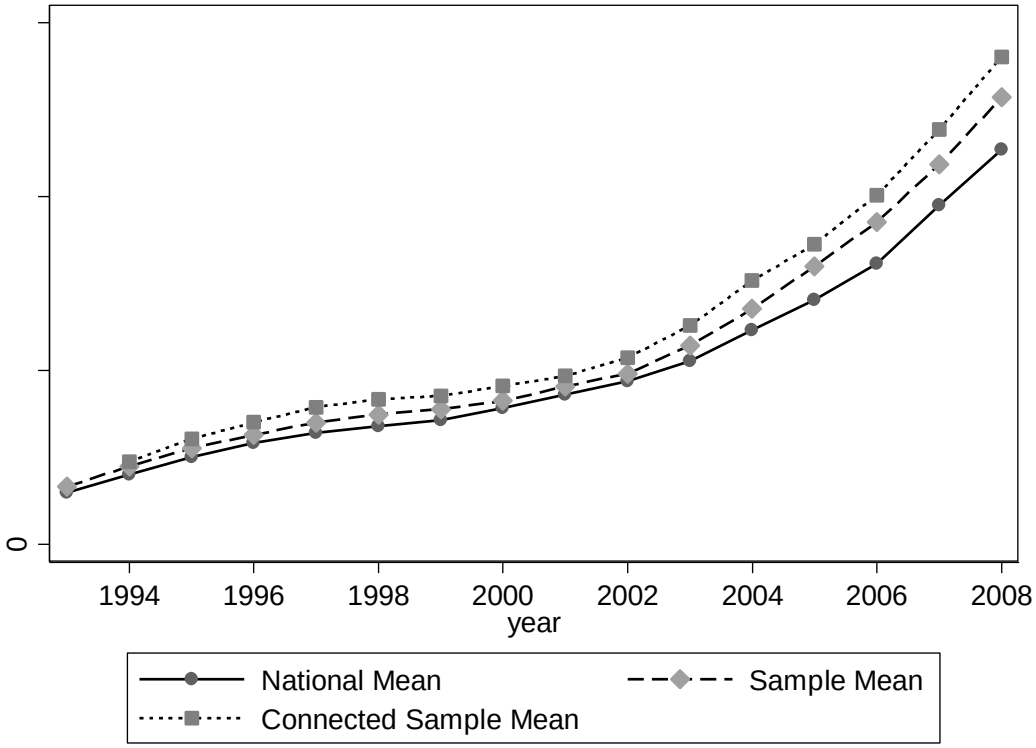


Figure 2: Nominal Growth Rates of Per-capita GDP

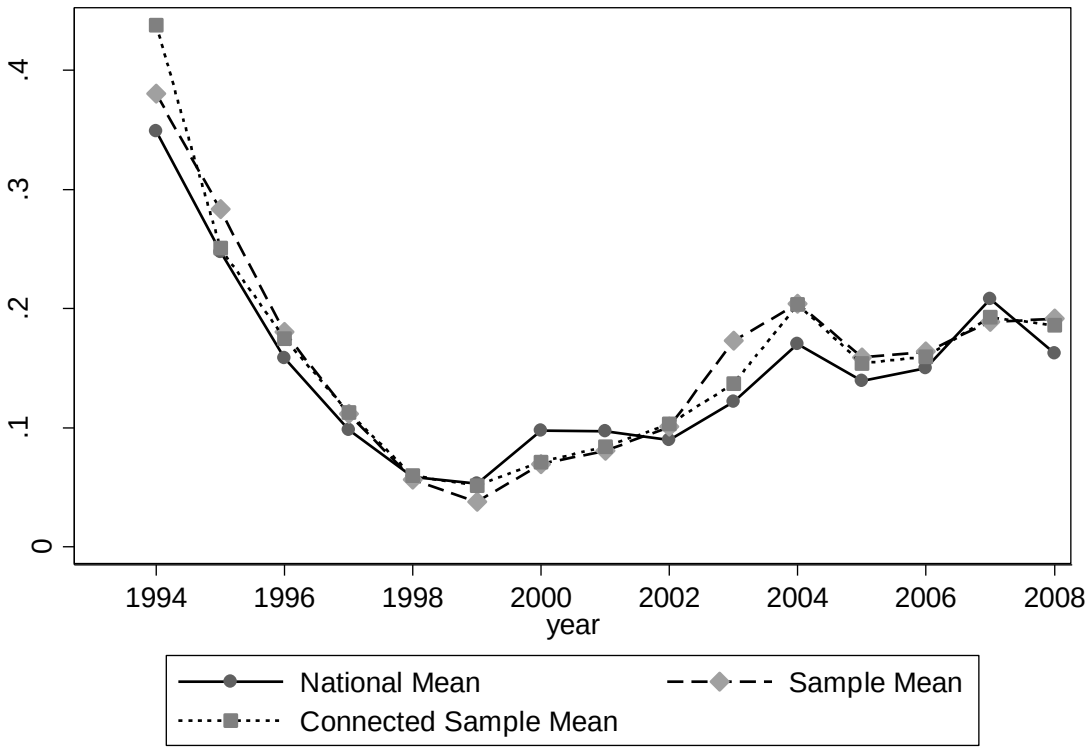


Figure 3: Histogram of Leaders' Tenures

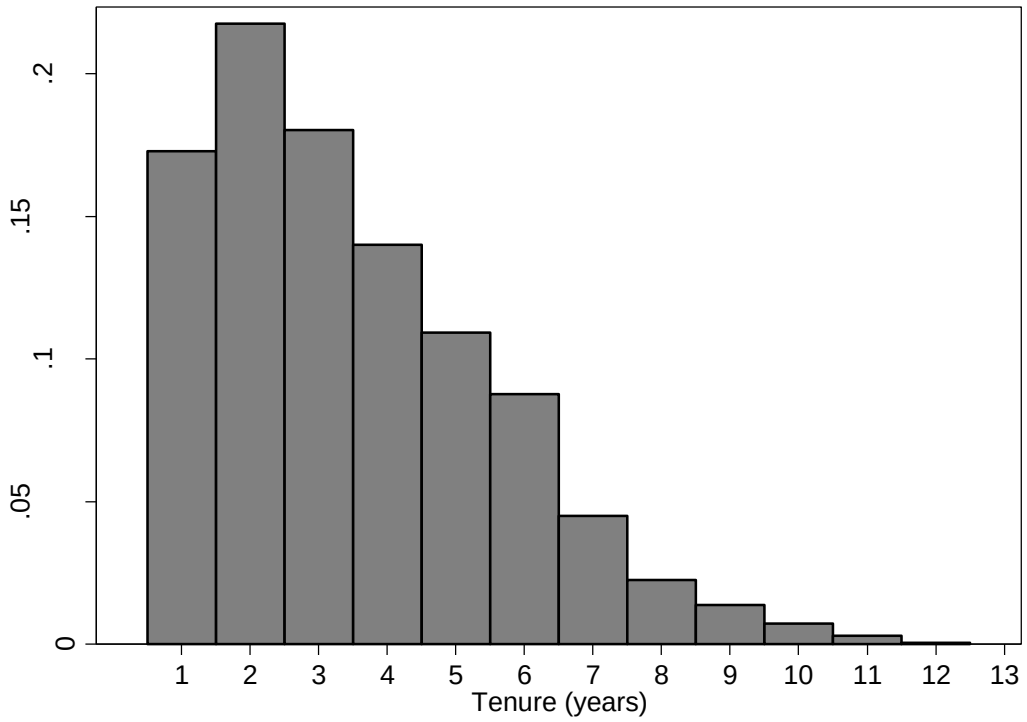
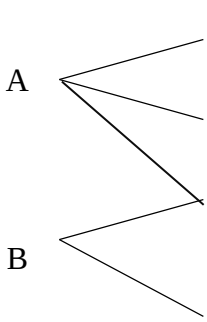


Figure 4: An Illustration

City	Leader	Group
A	1	1
A	2	1
A	3	1
B	3	1
B	4	1
C	5	2
C	6	2



City	Leader	Identifiable parameters
A	1	$\omega_{1,A}$
A	2	$\omega_{2,A}$
A	3	$\omega_{3,A}$
B	3	$\omega_{3,B}$
B	4	$\omega_{4,B}$
C	5	$\omega_{5,C}$
C	6	$\omega_{6,C}$

Figure 5: Kernel Density of Individual Leader Effects

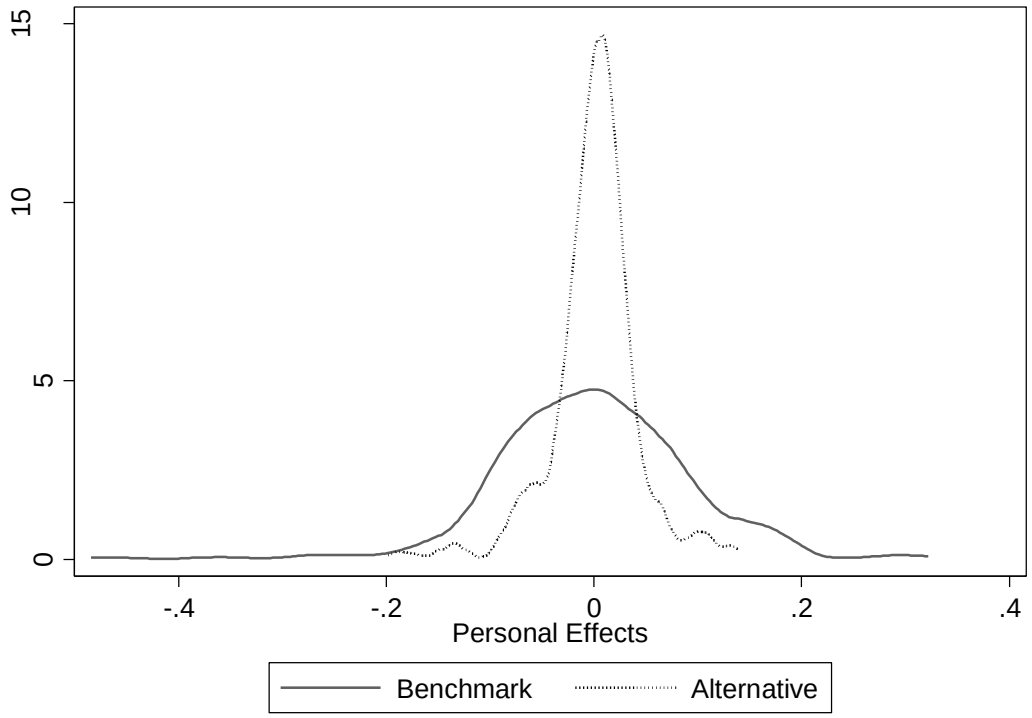


Figure 6: Kernel Densities by Type of Leaders: Benchmark Specification

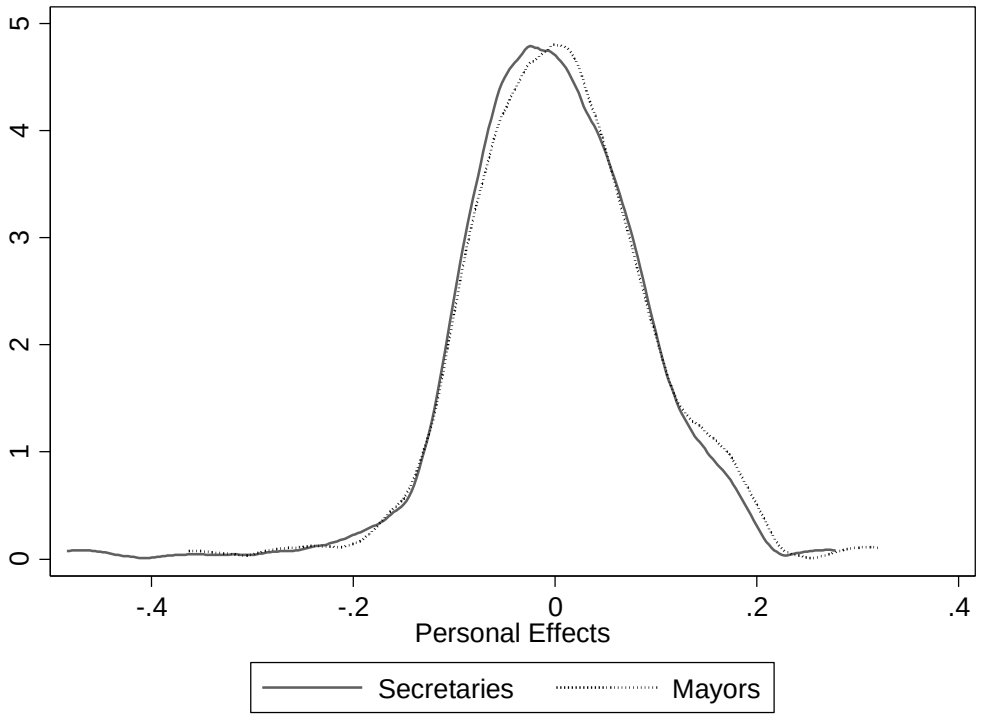


Figure 7: Kernel Densities by Type of Leaders: Alternative Specification

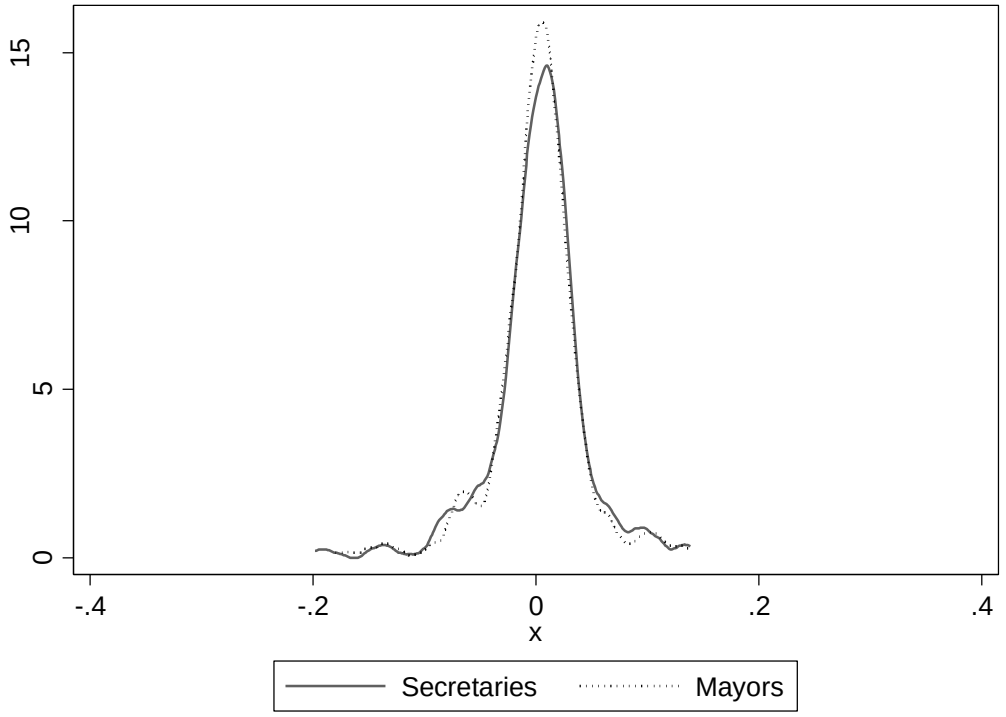


Figure 8: Personal Effects by Cohort

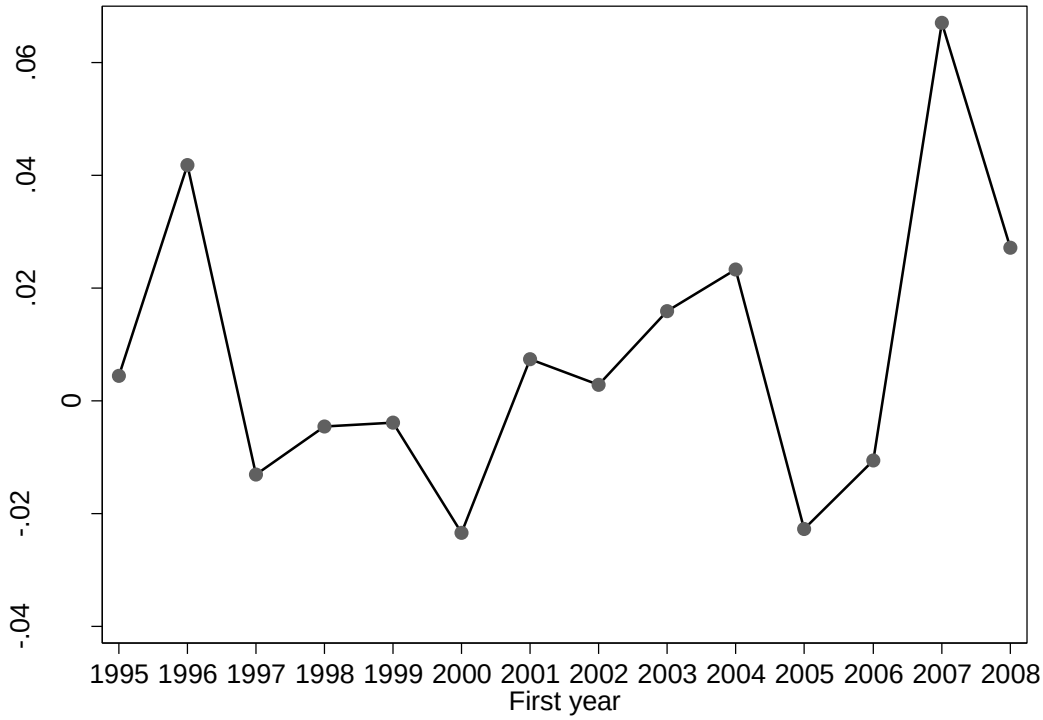
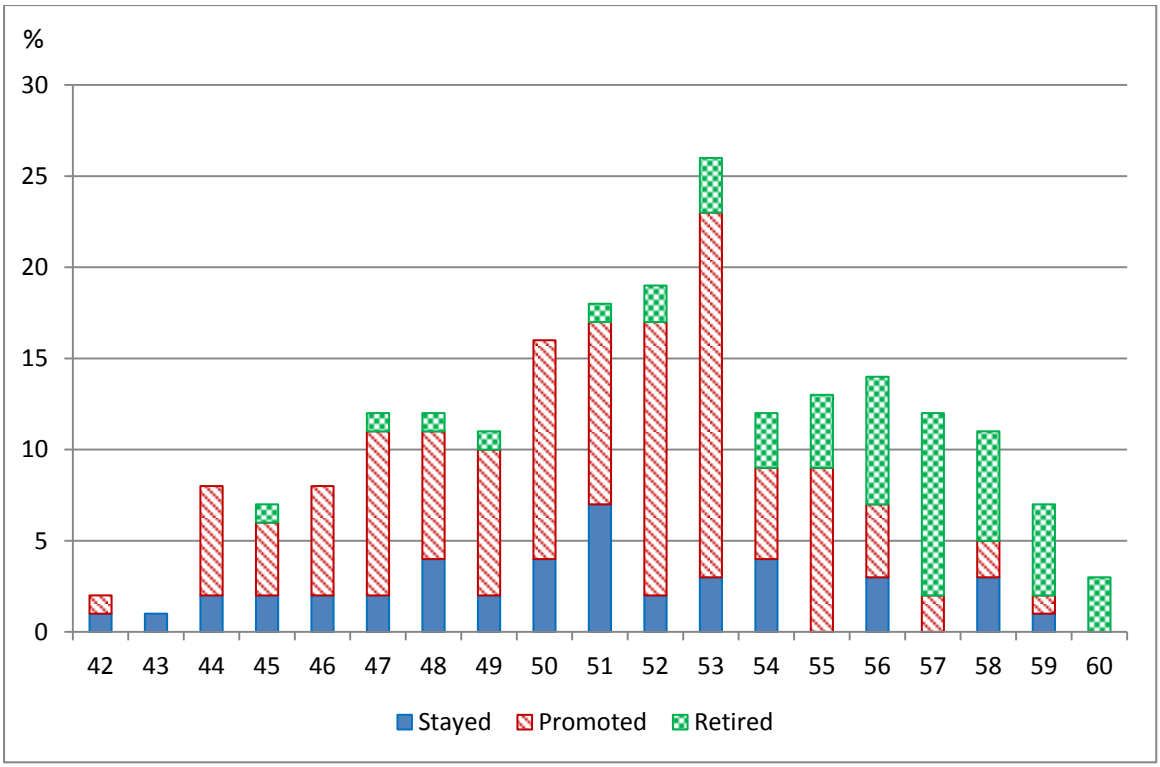
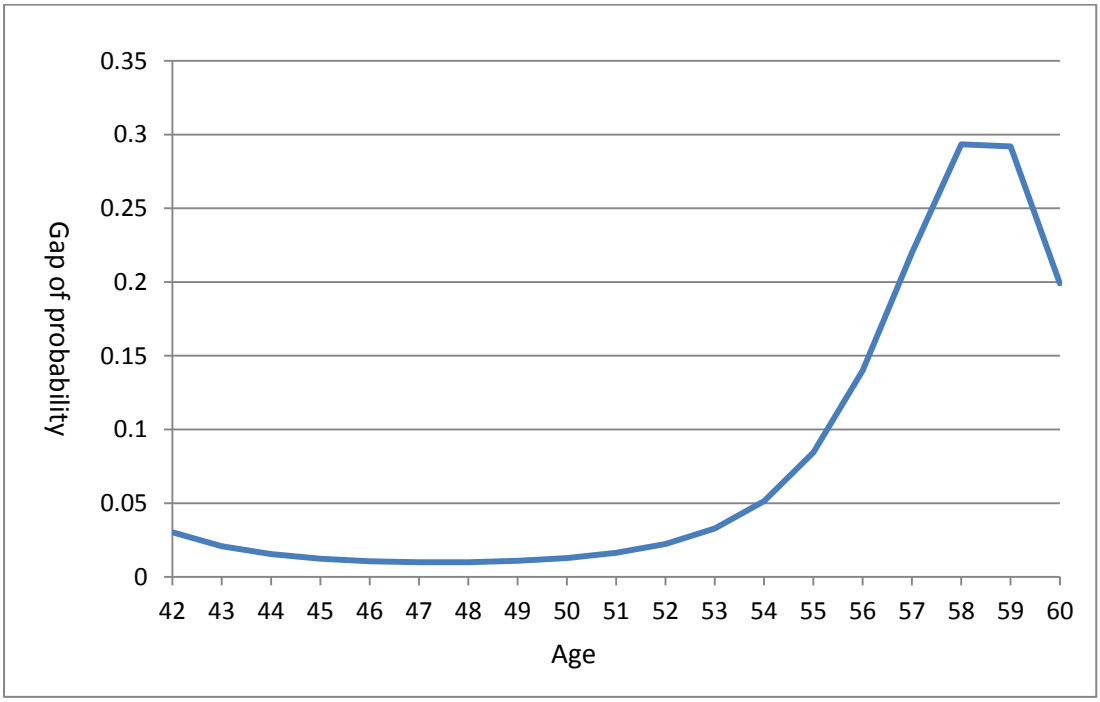


Figure 9: Distribution of Leaders at the End of a Term



Notes: Statistics are for 212 uncensored terms in the short sample.

Figure 10: The Gap of Promotion between Leaders Ranked 0% and 100%



Notes: The figure is drawn using the term-based MLM results holding other variables constant at their means.

Table 1: Decomposition of Variance

	(1)	(2)	(3)
City Dummies	Y	Y	Y
Year Dummies	N	Y	Y
Leader Dummies	N	N	Y
R^2	0.0496	0.2362	0.5088
Observations	2,242	2,242	2,242

Table 2: Summary Statistics of Estimated Leader Effects

Observations	Mean	Median	Std. Dev.	Skewness	Kurtosis
558	-6.84e-4	1.08e-3	0.0922	-0.428	6.194

Table 3: Sectoral Effects

	Sectoral Growth Rate			Sectoral Share in GDP		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Primary	Secondary	Tertiary	Primary	Secondary	Tertiary
Leader ranking	0.0634***	0.238***	0.135***	-0.0200***	0.0493***	-0.0296***
	(0.0139)	(0.0150)	(0.0141)	(0.00438)	(0.00548)	(0.00421)
Constant	0.0928***	-0.0431**	0.334***	0.112***	0.538***	0.350***
	(0.0202)	(0.0219)	(0.0205)	(0.00641)	(0.00802)	(0.00616)
City Dummy	Y	Y	Y	Y	Y	Y
Year Dummy	Y	Y	Y	Y	Y	Y
Observations	2,185	2,185	2,185	2,201	2,201	2,201
R-squared	0.263	0.394	0.347	0.939	0.916	0.849

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Results of the LPM with Annual Data

VARIABLES	(1) Promotion vs. the rest	(2) Promotion vs. stayers	(3) Promotion vs. the rest	(4) Promotion vs. stayers	(5) Promotion vs. the rest	(6) Promotion vs. stayers
Leader ranking	0.113	0.127*			0.113	0.126
	(0.0741)	(0.0767)			(0.0753)	(0.0780)
Age	0.244***	0.188**	0.244***	0.188**	0.244***	0.188**
	(0.0695)	(0.0760)	(0.0696)	(0.0762)	(0.0696)	(0.0761)
Age-sq	-0.00247***	-0.00188**	-0.00247***	-0.00189**	-0.00247***	-0.00188**
	(0.000691)	(0.000761)	(0.000692)	(0.000763)	(0.000691)	(0.000762)
City tenure	0.0341**	0.0499***	0.0333**	0.0494***	0.0341**	0.0499***
	(0.0159)	(0.0168)	(0.0159)	(0.0168)	(0.0159)	(0.0168)
Provincial Experience	0.0324	0.0383	0.0314	0.0377	0.0324	0.0382
	(0.0327)	(0.0337)	(0.0328)	(0.0338)	(0.0327)	(0.0338)
Real GDP growth rate			0.0654	0.0940	-0.000409	0.0215
			(0.245)	(0.248)	(0.249)	(0.252)
Constant	-5.775***	-4.431**	-5.729***	-4.406**	-5.775***	-4.434**
	(1.742)	(1.892)	(1.745)	(1.897)	(1.744)	(1.894)
Province Dummy	Y	Y	Y	Y	Y	Y
Year Dummy	Y	Y	Y	Y	Y	Y
Observations	691	646	691	646	691	646
R-squared	0.091	0.143	0.091	0.143	0.091	0.143

Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Results of the LPM by Term

VARIABLES	(1) Promotion vs. the rest	(2) Promotion vs. stayers	(3) Promotion vs. the rest	(4) Promotion vs. stayers	(5) Promotion vs. the rest	(6) Promotion vs. stayers
Leader ranking	0.303**	0.317*			0.297*	0.312*
	(0.152)	(0.165)			(0.161)	(0.174)
Age	0.741***	0.406**	0.742***	0.415**	0.740***	0.406**
	(0.146)	(0.178)	(0.147)	(0.180)	(0.146)	(0.179)
Age-sq	-0.00745***	-0.00393**	-0.00747***	-0.00402**	-0.00745***	-0.00394**
	(0.00143)	(0.00178)	(0.00144)	(0.00180)	(0.00143)	(0.00179)
City tenure	0.0290	0.0825**	0.0275	0.0832**	0.0291	0.0825**
	(0.0301)	(0.0347)	(0.0303)	(0.0351)	(0.0302)	(0.0348)
Provincial Experience	0.00851	0.0193	0.00879	0.0246	0.00846	0.0193
	(0.0658)	(0.0708)	(0.0663)	(0.0714)	(0.0660)	(0.0710)
Real GDP growth rate			0.636	0.616	0.104	0.0764
			(0.827)	(0.922)	(0.871)	(0.965)
Constant	-17.55***	-9.629**	-17.56***	-9.823**	-17.55***	-9.655**
	(3.697)	(4.446)	(3.726)	(4.499)	(3.706)	(4.471)
Province Dummy	Y	Y	Y	Y	Y	Y
Year Dummy	Y	Y	Y	Y	Y	Y
Observations	236	189	236	189	236	189
R-squared	0.240	0.207	0.228	0.192	0.240	0.208

Standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1..

Table 6: Results of the MLM by Term

VARIABLES	(1a) Promotion	(1b) Retirement	(2a) Promotion	(2b) Retirement	(3a) Promotion	(3b) Retirement
Leader Ranking	2.046**	0.793			2.070**	0.631
	(0.885)	(1.235)			(0.937)	(1.295)
Age	1.487	-2.058	1.521	-2.011	1.499	-2.059
	(1.009)	(1.360)	(0.981)	(1.339)	(1.010)	(1.352)
Age_sq	-0.0140	0.0235*	-0.0144	0.0230*	-0.0141	0.0235*
	(0.0100)	(0.0132)	(0.00975)	(0.0129)	(0.0100)	(0.0131)
City tenure	0.153	0.0642	0.129	0.0519	0.150	0.0648
	(0.182)	(0.209)	(0.180)	(0.209)	(0.182)	(0.210)
Provincial Experience	0.310	-0.217	0.312	-0.197	0.309	-0.216
	(0.372)	(0.559)	(0.370)	(0.560)	(0.372)	(0.561)
GDP growth rate			3.470	3.441	-0.194	2.797
			(4.507)	(6.611)	(4.884)	(7.132)
Constant	-36.90	43.48	-37.48	42.14	-37.17	43.21
	(25.22)	(34.88)	(24.50)	(34.40)	(25.23)	(34.68)
Province dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	239	239	239	239	239	239

Standard errors are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table 7: Results of the MLM by Career

VARIABLES	(1a) Promotion	(1b) Retirement	(2a) Promotion	(2b) Retirement	(3a) Promotion	(3b) Retirement
Leader Ranking	2.720*	1.000			3.471**	1.106
	(1.461)	(1.609)			(1.549)	(1.619)
Age	2.129	-1.208	1.913	-1.380	2.397	-1.116
	(1.638)	(1.810)	(1.565)	(1.764)	(1.636)	(1.797)
Age_sq	-0.0221	0.0140	-0.0199	0.0158	-0.0247	0.0132
	(0.0160)	(0.0174)	(0.0153)	(0.0170)	(0.0160)	(0.0173)
City tenure	0.270	0.0925	0.212	0.0673	0.246	0.102
	(0.225)	(0.223)	(0.217)	(0.220)	(0.227)	(0.225)
Provincial Experience	-0.502	-1.007	-0.505	-0.973	-0.441	-0.967
	(0.551)	(0.619)	(0.551)	(0.621)	(0.559)	(0.624)
GDP growth rate			-3.308	-1.978	-10.78	-2.567
			(7.024)	(7.873)	(8.035)	(8.356)
Constant	-31.85	41.24	-28.14	43.43	-40.05	36.61
	(3,034)	(3,034)	(944.8)	(945.0)	(921.6)	(921.8)
Province dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	193	193	193	193	193	193

Standard errors are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table 8: The Effects of Leader Ranking on Probability of Promotion (Results Based on 1(a) of Table 6)

	0%	25%	50%	75%	100%
0%	0				
25%	0.015	0			
50%	0.027	0.011	0		
75%	0.035	0.020	0.008	0	
100%	0.042	0.026	0.0149	0.006	0

Table 9: The LPM for Party Secretaries and Mayors Separately

	Party Secretary				Mayors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Annual	Annual	Term	Term	Annual	Annual	Term	Term
VARIABLES	Promotion vs. rest	Promotion vs. stayers	Promotion vs. rest	Promotion vs. stayers	Promotion vs. rest	Promotion vs. stayers	Promotion vs. rest	Promotion vs. stayers
Leader ranking	0.140	0.138	0.125	-0.152	0.203*	0.212*	0.397*	0.442*
	(0.103)	(0.108)	(0.196)	(0.197)	(0.109)	(0.110)	(0.226)	(0.233)
Age	0.196	0.0923	0.794***	0.371	0.390***	0.340***	0.616***	0.314
	(0.124)	(0.139)	(0.254)	(0.266)	(0.0864)	(0.0911)	(0.197)	(0.229)
Age-sq	-0.00199	-0.000924	-0.00816***	-0.00373	-0.00396***	-0.00346***	-0.00610***	-0.00295
	(0.00122)	(0.00138)	(0.00245)	(0.00261)	(0.000871)	(0.000924)	(0.00196)	(0.00232)
City tenure	0.0759***	0.0854***	0.0868*	0.0616	0.0820***	0.113***	-0.0123	0.0719
	(0.0279)	(0.0295)	(0.0446)	(0.0440)	(0.0251)	(0.0269)	(0.0552)	(0.0646)
Provincial experience	0.0586	0.0582	0.0116	-0.0532	0.0425	0.0530	0.0408	0.0677
	(0.0481)	(0.0507)	(0.0848)	(0.0846)	(0.0483)	(0.0483)	(0.104)	(0.105)
Constant	-4.556	-2.037	-18.35***	-8.138	-9.427***	-8.231***	-14.94***	-7.856
	(3.132)	(3.495)	(6.516)	(6.745)	(2.141)	(2.242)	(4.936)	(5.649)
Province dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	390	357	123	90	301	289	113	101
R-squared	0.087	0.149	0.433	0.352	0.233	0.281	0.324	0.384

Standard errors are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Figure A1: Map of Cities in the Sample

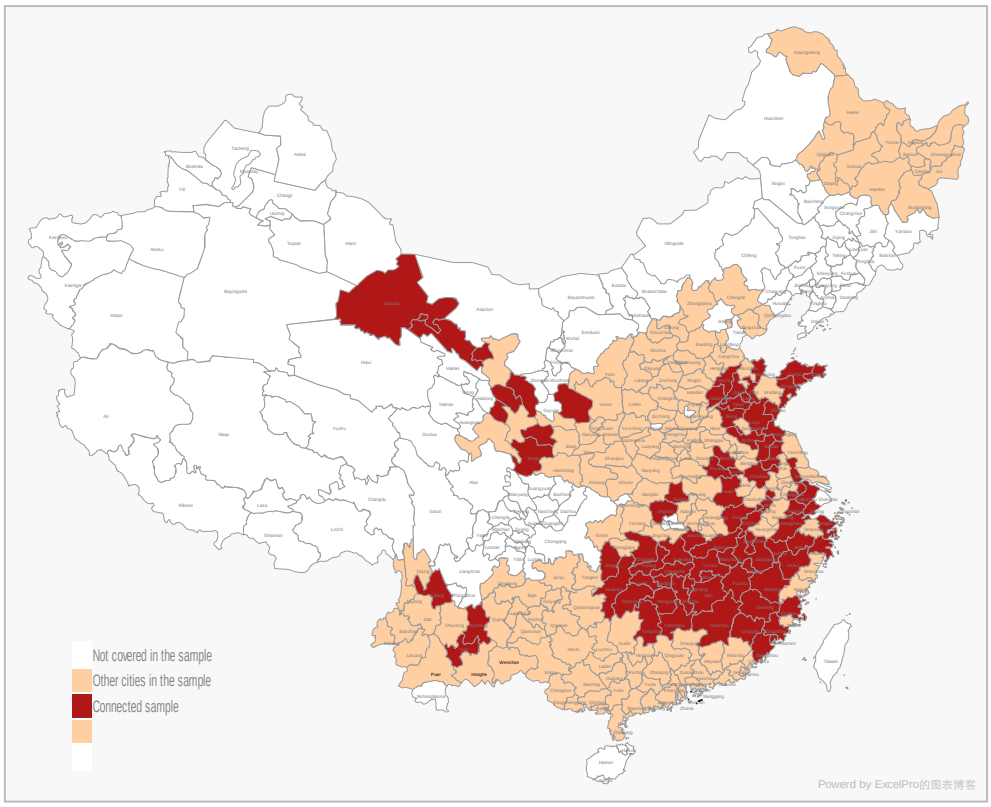


Table A1: Cities Covered in the Study

Province	City	Province	City	Province	City
Anhui	Anqing*	Guizhou	Qianxinan	Jiangsu	Xuzhou*
Anhui	Bengbu	Guizhou	Tongren	Jiangsu	Yancheng
Anhui	bozhou*	Guizhou	Zunyi	Jiangsu	Yangzhou
Anhui	Chaohu	Heibei	Baoding	Jiangsu	Zhenjiang
Anhui	Chizhou*	Heibei	Cangzhou	Jiangxi	Fuzhou*
Anhui	Chuzhou*	Heibei	Chengde	Jiangxi	Ganzhou*
Anhui	Fuyang*	Heibei	Handan	Jiangxi	Jian*
Anhui	Hefei	Heibei	Hengshui	Jiangxi	Jingdezhen*
Anhui	Huaibei	Heibei	Langfang	Jiangxi	Jiujiang*
Anhui	Huainan*	Heibei	Qinhuangdao	Jiangxi	Nanchang*
Anhui	Huangshan	Heibei	Shijiazhuang	Jiangxi	Pingxiang*
Anhui	Liuan*	Heibei	Tangshan	Jiangxi	Shangrao*
Anhui	Maanshan*	Heibei	Xingtai	Jiangxi	Xinyu*
Anhui	Suzhou	Heibei	Zhangjiakou	Jiangxi	Yichun*
Anhui	Tongling*	Heilongjiang	Daqing	Jiangxi	Yingtan*
Anhui	Wuhu*	Heilongjiang	Daxinganling	Shaanxi	Ankang
Anhui	Xuancheng	Heilongjiang	Haerbin	Shaanxi	Baoji
Fujian	Fuzhou*	Heilongjiang	Hegang	Shaanxi	Hanzhong
Fujian	Longyan*	Heilongjiang	Heihe	Shaanxi	Shangluo
Fujian	Nanping*	Heilongjiang	Jiamusi	Shaanxi	Tongchuan
Fujian	Ningde	Heilongjiang	Jixi	Shaanxi	Weinan
Fujian	Putian	Heilongjiang	Mudanjiang	Shaanxi	Xian
Fujian	Quanzhou*	Heilongjiang	Qiqihaer	Shaanxi	Xianyang
Fujian	Sanming*	Heilongjiang	Qitaihe	Shaanxi	Yanan
Fujian	Xiamen*	Heilongjiang	Shuangyashan	Shaanxi	Yulin
Fujian	Zhangzhou*	Heilongjiang	Suihua	Shandong	Binzhou
Gansu	Baiyin*	Heilongjiang	Yichun	Shandong	Dezhou*
Gansu	Dingxi	Henan	Anyang	Shandong	Dongying*
Gansu	Gannan	Henan	Jiaozuo	Shandong	Heze
Gansu	Jiayuguan*	Henan	Kaifeng	Shandong	Jinan*
Gansu	Jinchang*	Henan	Hebi	Shandong	Jining*
Gansu	Jiuquan*	Henan	Luohe	Shandong	Laiwu*
Gansu	lanzhou*	Henan	Luoyang	Shandong	Liaocheng*
Gansu	Linxia*	Henan	Nanyang	Shandong	Linyi*
Gansu	longnan*	Henan	Pingdingshan	Shandong	Qingdao*
Gansu	Pingliang	Henan	Puyang	Shandong	Rizhao*
Gansu	Qingyang*	Henan	Sanmenxia	Shandong	Taian*
Gansu	Tianshui*	Henan	Shangqiu	Shandong	Weifang
Gansu	Wuwei	Henan	Xinxiang	Shandong	Weihai*
Gansu	Zhangye*	Henan	Xinyang	Shandong	Yantai*
Guangdong	Chaozhou	Henan	Xuchang	Shandong	Zaozhuang
Guangdong	Dongwan	Henan	Zhengzhou	Shandong	Zibo*
Guangdong	Fuoshan	Henan	Zhoukou	Shanxi	Changzhi
Guangdong	Guangzhou	Henan	Zhumadian	Shanxi	Datong
Guangdong	Heyuan	Hubei	Enshi	Shanxi	Jincheng
Guangdong	Huizhou	Hubei	Ezhou	Shanxi	Jinzhong
Guangdong	Jiangmen	Hubei	Huanggang	Shanxi	Linfen
Guangdong	Jieyang	Hubei	Huangshi	Shanxi	Lvliang

Guangdong	Maoming	Hubei	Jingmen*	Shanxi	Shuozhou
Guangdong	Meizhou	Hubei	Jingzhou	Shanxi	Taiyuan
Guangdong	Qingyuan	Hubei	Shiyan	Shanxi	Xinzhou
Guangdong	Shantou	Hubei	Suizhou*	Shanxi	Yangquan
Guangdong	Shanwei	Hubei	Wuhan	Shanxi	Yuncheng
Guangdong	Shaoguan	Hubei	Xiangfan	Yunnan	Baoshan
Guangdong	Shenzhen	Hubei	Xianning*	Yunnan	Chuxiong
Guangdong	Yangjiang	Hubei	Xiaogan	Yunnan	Dali
Guangdong	Yunfu	Hubei	Yichang	Yunnan	Dehong
Guangdong	Zhanjiang	Hunan	Changde*	Yunnan	Diqing
Guangdong	Zhaoqing	Hunan	Changsha*	Yunnan	Honghe
Guangdong	Zhongshan	Hunan	Chenzhou*	Yunnan	Kunming*
Guangdong	Zhuhai	Hunan	Hengyang*	Yunnan	Lijiang*
Guangxi	Baise	Hunan	Huaihua*	Yunnan	Lincang
Guangxi	Beihai	Hunan	Loudi*	Yunnan	Nujiang
Guangxi	Chongzuo	Hunan	Shaoyang*	Yunnan	Puer
Guangxi	Fangchenggang	Hunan	Xiangtan*	Yunnan	Qujing
Guangxi	Guigang	Hunan	xiangxi*	Yunnan	Shaotong
Guangxi	Guilin	Hunan	Yiyang*	Yunnan	Wenshan
Guangxi	Hechi	Hunan	Yongzhou*	Yunnan	Xishuangbanna
Guangxi	Hezhou	Hunan	Yueyang*	Yunnan	Yuxi*
Guangxi	Laibin	Hunan	Zhangjiajie	Zhejiang	Hangzhou*
Guangxi	Liuzhou	Hunan	Zhuzhou*	Zhejiang	Huzhou*
Guangxi	Nanning	Jiangsu	Changzhou	Zhejiang	Jiaxing*
Guangxi	Qinzhou	Jiangsu	Huaian*	Zhejiang	Jinhua*
Guangxi	Wuzhou	Jiangsu	Lianyungang*	Zhejiang	Lishui*
Guangxi	Yulin	Jiangsu	Nanjing	Zhejiang	Ningbo*
Guizhou	Anshun	Jiangsu	Nantong	Zhejiang	Quzhou*
Guizhou	Bijie	Jiangsu	Suqian*	Zhejiang	Shaoxing
Guizhou	Guiyang	Jiangsu	Suzhou*	Zhejiang	Taizhou*
Guizhou	Liupanshui	Jiangsu	TaizhouJS*	Zhejiang	Wenzhou
Guizhou	Qiandongnan	Jiangsu	Wuxi*	Zhejiang	Zhoushan
Guizhou	Qiannan				

*: in the connected sample

Table A2: Number of Cities Served by Leaders

Number of cities served	Freq.	Percent	Cum.
1	1,425	85.28	85.28
2	219	13.11	98.38
3	23	1.38	99.76
4	4	0.24	100
Total	1,671	100	

Table A3: Number of Years Leaders Appear in the Sample

Number of years	Freq.	Percent	Cum.
1	283	16.94	16.94
2	288	17.24	34.17
3	257	15.38	49.55
4	230	13.76	63.32
5	157	9.4	72.71
6	184	11.01	83.72
7	108	6.46	90.19
8	73	4.37	94.55
9	43	2.57	97.13
10	25	1.5	98.62
11	14	0.84	99.46
12	7	0.42	99.88
13	1	0.06	99.94
14	1	0.06	100
Total	1,671	100	

Table A4: Grouping

Group No.	Observations*	Leaders	Movers	Cities
Isolated observations**	750	191	0	27
1	837	205	36	28
2	321	88	16	11
3	441	113	20	16
4	60	13	1	2
5	2,242	558	101	85
6	60	14	2	2
7	116	29	4	5
8	60	15	1	2
9	56	13	1	2
10	520	136	25	18
11	420	89	13	14
12	60	16	1	2
13	270	65	12	9
14	105	27	3	4
15	84	22	3	3
16	54	14	1	2
17	52	11	1	2
18	48	13	1	2
19	72	21	2	3
20	53	18	1	2
Total	6,681	1,671	245	241

Notes: *: Observations are defined as leader-year pairs.

**: Isolated observations are the cities that are not connected with any other city.