Leadership Rotations and the Convergence of Subnational Economic Policies in China: Evidence from Provincial Government Work Reports

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Leadership Rotations and the Convergence of Subnational Economic Policies in China: Evidence from Provincial Government Work Reports

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Abstract

This article examines how China's subnational leadership rotations have affected local governments' policy choices. Utilizing the annual government work reports that outline provincial policy priorities, I find that shuffled leaders choose similar economic policies across different provinces, and this translates into similarities in several policy outcomes. I then show that such policy isomorphism has been driven mainly by convergence in which topics these reports cover, rather than by convergence in how the provincial leaders discuss a given policy issue. However, according to the event study estimates, this economic policy similarity may disappear soon after the shuffled leader leaves office, thus implying that the policy convergence might be transitory. One plausible explanation for these findings is that leadership rotations often indicate that the central government favors the policies implemented by these shuffled leaders, so they tend to replicate some of these policies after moving to the destination province. (*JEL* H11, H70).

Keywords: Leadership Rotation, Policy Diffusion, Regional Integration, Government Work Report, Chinese Political Economy

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1. Introduction

China's steady and sustained effort in economic decentralization has mobilized its subnational governments to advocate local economic growth, which is one of the important reasons for the country's remarkable economic success over the past four decades (Xu, 2011).[1] However, it has become a big challenge for the central government to maintain its political centralization while continuing decentralizing the economy.[2] To overcome this challenge, one particular institutional arrangement is to frequently shuffle the political leaders at the province and city levels, which resembles the country's historical subnational leadership rotations that date back to the Qin Dynasty around 221 BC (Zhao, 2015). As shown in Figure 1, there were 47 cross-province exchanges among the number-one and number-two leadership positions of the 31 provinces in mainland China between 2005 and 2019, with an average of 3 times per year, and almost all provinces, except Hainan, had at least sent a number-one or number-two provincial leader to or received such a leader from another province during this 15-year period. In any case, shuffling provincial leaders has been a very common practice over the past 20 years.

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In this article, I endeavor to examine the impacts of this common practice of rotating leaders on the policy choices across different Chinese provinces. Chien (2008), among others, has already taken a case study approach to show that shuffling county-level leaders could result in local policy isomorphism. Many news articles have also provided some convincing evidence. For instance, it is reported that Su Rong, who was later convicted of corruption, focused overwhelmingly on afforestation when he was the number-one leader in Gansu and Jiangxi (Xu, 2018). The cities that

have been governed by Wang Menghui are also reported to be all good at applying a "unified approach" to urban planning (Zheng, 2019), and Li Qiang is famous for diffusing his expertise of constructing characteristic towns from Zhejiang to Jiangsu (Qian, 2016).[3] Among all these examples, how Wang Jun carried on his policies for coal mine safety from Shanxi, where he was the number-two leader between 2009 and 2012, to Inner Mongolia, where he was the number-one leader between 2013 and 2016, is most closely aligned with this article's research design. I discuss this example in more detail in Section 3.

In spite of the case studies in the preceding paragraph, a systematic examination of the relationship between provincial leadership rotations and the convergence of subnational economic policies is still absent. This article, therefore, offers an innovative approach to facilitate our understanding of this relationship. As most of the existing economic indicators could only quantify the policy outcomes instead of reflecting the policies themselves, I try to overcome this challenge by taking advantage of the annual government work reports from all 31 provinces in mainland China between 2005 and 2019. I measure the economic policy distance between each pair of provinces based on the textual difference between their government work reports. Then, with different fixed effects, I regress this textual distance on a specially designed dummy variable representing provincial leadership rotations.

The regression results suggest that subnational leadership rotations have promoted the convergence of provincial economic policies announced in the government work reports, and this convergence has translated into similarities in several policy outcomes, namely (i) the provincial leaders' activities and (ii) the composition of provincial government spending. Numerically, if two government work reports correspond to the same provincial leader, then (i) the difference in each phrase's frequency between these two textual documents would decrease by around 22.5% of the stan-

dard deviation, (ii) the difference in the news headlines reporting provincial leaders' activities would decrease by around 31.2% of the standard deviation, and (iii) the difference in the composition of local government spending would decrease by around 40.6% of the standard deviation.

In view of the takeaways from these baseline estimates, I perform two more exercises. First, I make use of a Latent Dirichlet Allocation (LDA) model to show that the local economic policy isomorphism resulting from shuffling provincial leaders has been driven mainly by convergence in which topics these reports cover, rather than by convergence in how different provincial leaders talk about a given policy issue. Second, I evaluate the dynamic effects of the leadership rotations: according to my event study estimates, the economic policy similarity driven by leadership rotations might disappear soon after the shuffled leader leaves office, thus implying that the convergence effect of provincial leadership rotations might not be permanent.

While acknowledging that one rationale for the shuffled leaders to continue their policies is that their expertise tends to remain unchanged in the short term, I propose an alternative explanation: a leadership exchange indicates that a provincial leader is selected for further promotion in the future because the central government favors the economic policies that person implemented in the origin province, so that shuffled leader would tend to replicate some of these policies in the destination province to maximize the chance of even higher promotions. I present some suggestive evidence in favor of this explanation towards the end of this article.

This article joins the literature on China's subnational leadership rotations. Several recent studies have shown that shuffling local leaders across the country could serve various economic and political purposes. According to Bai et al. (2008) and Hou and Li (2021), frequent leadership rotations across provinces could help mitigate collusion by breaking local government ties with business and also reduce local

protectionism, which has become an obstacle to China's socioeconomic development ever since its economic decentralization.[4] Muratova et al. (2018), Nian and Wang (2019), Jiang and Mei (2020), Shi et al. (2021) and Li et al. (2022) found that, when a municipal/provincial leader is appointed as the leader of a new city/province, the exports and investment flows from the origin city/province to the destination city/province and the land transactions between them would significantly increase, thus implying that leadership rotations could reduce the barriers to cross-regional economic exchanges. Moreover, Yao and Zhang (2015) showed that shuffling city-level leaders could help the provincial and central governments more fairly evaluate these leaders' abilities to manage economic development, which is a key component to be considered in the promotion process.

The studies discussed in the preceding paragraph have documented how the spread of leaders across different subnational regions could affect different economic outcomes. Nevertheless, few of them have paid attention to the more fundamental question of how these shuffled leaders make policy decisions at different locations. The empirical analysis in this article complements this literature by showing that subnational leaders in China have tended to choose similar economic policies wherever under their rule, which has in turn promoted the country's economic and political integration. In the meantime, it is the consistency of a leader's policies across locations and time that makes it possible for the central government to fairly evaluate the abilities of these shuffled leaders.

The findings in this article also speak to some broader literatures. For instance, Jones and Olken (2005), Besley et al., (2011), Hodler and Raschky (2014), Yao and Zhang (2015) and Li et al. (2020) have examined the extent to which political leaders matter for economic growth conditional on the structural factors. Bertrand and Schoar (2003), Watson and Hassett (2004), Lazear et al. (2015), Hassan (2017),

Xu (2018), Khan et al. (2019), Bennedsen et al. (2020), Boudreau et al. (2021), Hoffman and Tadelis (2021) and Fenizia (2022), among others, have also evaluated the role of managers in private-sector firms and officials in public-sector bureaucracies by, for example, taking advantage of their rotations within and across organizations. Consistent with these seminal papers, this article finds that provincial leaders in China have been very influential to the economic activities within their jurisdictions, and the central government could potentially make use of its centralized allocation of these subnational leaders to coordinate local economic policies. Nonetheless, to the best of my knowledge, few existing studies have paid attention to what happens after a leader leaves office. Hence, another contribution of this article is that, based on the event study estimates, I show that it has been difficult for a provincial leader in China to leave a legacy of the economic policies she chose to the successor of her position, thus implying that political leaders might matter for economic development, but such influence seems to be transitory in nature.

In addition, this article also relates to the literature on China's regional disparities (not only in income but also in public service, fiscal regime and economic policies) and the interactions between local governments. As discussed in, for example, West and Wong (1995), Kanbur and Zhang (1999, 2005), Démurger et al. (2002), Zhang and Kanbur (2005), Zhang (2006), Qiao et al. (2008), Lessmann (2012) and Liu et al. (2017), partly due to economic decentralization, China has been experiencing substantial regional inequalities of income, education and healthcare over the past four decades. According to the classical model developed by Tiebout (1956), this large socioeconomic variation across different provinces would potentially motivate people with heterogeneous tastes to move to the communities where their preference patterns would be best satisfied. Although these movements have been deterred in China as the seminal synthesis by Xu (2011) pointed out that China's inter-provincial

factor mobility has been limited, few have denied that the incentives to migrate still exist,[5] and this helps explain how China's regional inequalities might be detrimental to the country's economic development in the long run: local governments have to use extra resources to settle down people's dissatisfaction resulting from being willing but unable to relocate. In this article, I find that leadership rotations could to some extent homogenize the economic policies implemented in different provinces, and this could also translate into similarities in several policy outcomes. That being said, this simple institutional arrangement of shuffling subnational leaders have reduced the large socioeconomic variation across different jurisdictions, which could potentially decrease the number of people who are still willing to relocate given that all provinces are becoming increasingly more similar to one another.

Finally, there is a growing literature on the use of textual data in political science, as summarized by Grimmer and Stewart (2013), and in economics, as summarized in Gentzkow et al. (2019a). The methodology used in this article is most closely related to Gentzkow et al. (2019b) and Jiang and Zhang (2020). While the former measured trends in the partisanship of congressional speech between 1873 and 2016 in the United States by comparing between the phrases used by Republicans and those by Democrats, the latter applied an unsupervised machine learning algorithm to classify the phrases used in China's city-level government work reports in order to show that the municipal leaders connected to provincial leaders have been more likely to express support for provincial government's policy priorities. The analytical methods used in this article are largely inspired by Gentzkow et al. (2019b), but considerable modifications and simplifications are made based on my empirical context. I also take advantage of the topic modeling method used in Jiang and Zhang (2020) to quantify the local economic policy isomorphism between and within policy issues. As a potential contribution, I show that, in addition to each phrase's number of mentions

considered in Gentzkow et al. (2019b) or each topic's number of mentions considered in Jiang and Zhang (2020), the ordering of the phrases/topics in a textual document might also matter. Future studies should verify if such differences in writing structure are also important in other empirical contexts.

The structure of this article is as follows: Section 2 provides institutional background on provincial government work reports and leadership rotations and describes the relevant data sources, Section 3 details the empirical estimation strategies, Section 4 presents and interprets the estimation results, and Section 5 draws the conclusions.

2. Institutional Background and Data

I describe the institutional background as well as the data in this section, with subsection 2.1 explaining how I process the government work reports and transform these textual documents into matrices and subsection 2.2 introducing the data set for the provincial leadership rotations between 2005 and 2019.

2.1. Government Work Reports

Similar to the national government work report annually delivered by the premier, i.e. the head of the central government, to the delegates of the National People's Congress every March, which usually captures enormous media attention across China, each provincial government work report is delivered by the governor, i.e. the head of the provincial government, to the attendees of that province's People's Congress in January or February. Drafted by the research office of the provincial government and revised by the governor, a provincial government work report systematically reviews the socioeconomic development of the province in the previous year and provides guidelines for the economic policies for the current year. One key component of

the report is to announce the province's various economic targets for GDP growth, unemployment rate, consumer price index, retail sales growth, *etc.* (Jiang and Zhang, 2020).[6] Once delivered, these targets would guide state-owned and private enterprises in the province to make their own operational decisions in accordance. In addition, as provincial People's Congresses are held prior to the national People's Congress in March, these provincial reports also serve as important references when drafting the national government work report.

Note that, as shown by the topic modeling results in the appendix of Jiang and Zhang (2020) and also in Appendix E of this article, these reports focus on economic policies far more than politics because they are delivered by the heads of the provincial governments who are responsible for local economic affairs rather than the heads of the provincial committees of the Chinese Communist Party (CCP) who take more responsibility for the personnel and other political duties, as articulated in subsection 2.2 below.[7]

One more piece of evidence in support of representing provincial economic policies by these annual government work reports is the almost perfect match between the GDP growth targets announced in these reports and the actual growth rates as shown in Figure 2. The ratio of the actual growth rate to the target is, on average, 1.058 between 2005 and 2019,[8] with the standard error clustered at the province level being 0.018, and the correlation coefficient is 0.983.[9] This high correlation implies that government work reports analyzed in this article are indicative of provincial economic policies. See more about this comparison between growth targets and actual growth rates and the discussion about some other economic targets announced in the reports in Appendix A.

≪COMP: Place Fig. 2 about here≫

Throughout this article, for each pair of provinces, I use the difference between their government work reports to represent their difference in policy choices. The primary reason for analyzing these reports rather than the more conventional data sets is that most of the existing economic indicators could only quantify the consequences of economic policies instead of reflecting the policies themselves, even though I still conduct some analysis on several policy outcomes by the end of this article. To begin with, I obtain all the government work reports of the 31 provinces in mainland China between 2005 and 2019 from the provincial governments' official websites. More precisely, I collect the reports corresponding to the 31 administrative regions at the province level, with 22 provinces, 4 direct-controlled municipalities (DCMs) and 5 autonomous regions (ARs). Note that the four DCMs, i.e. Beijing, Tianjin, Shanghai and Chongqing, are excluded unless otherwise noted because (i) these four regions are smaller than a regular province but with superior administrative status (Yao and Zhang, 2015), and (ii) the top politicians of these four regions are usually also members of the Politburo with policy objectives different from other provincial leaders (Fisman et al., 2020; Jiang and Mei 2020), such as caring more about regional specialization and practicing less local protectionism (Bai et al., 2008). However, the data preprocessing still considers these four regions, and the estimation results are not sensitive to including or excluding them.

After acquiring all the reports, I delete all punctuation and employ the "jieba" segmentation module in Python developed by Baidu, one of China's tech giants, to parse the raw Chinese texts of these reports (with the number of Chinese characters ranging from 7476 to 24151) into lists of phrases in Chinese.[10] Then, I create a dictionary of phrases appearing in these reports. To reduce the noise, I drop a list of extremely common phrases in Chinese.[11] The phrases that appear no more than 15 times across all reports are also deleted from the dictionary.[12] Next, for each of

the 9984 remaining phrases in the dictionary, I count its number of mentions in each of the 465 (i.e. $31 \times 15 = 465$) provincial government work reports[13] and record it in a 9984×465 matrix, with each row corresponding to a phrase and each column corresponding to a province-year combination. With this matrix as the input, I perform the modeling strategies explained in Section 3 below and compute all pairwise economic policy distances between provinces.

2.2. Provincial Leadership Rotations

Based on the China Political Elite Database used in, for example, Jiang (2018) and Jiang and Mei (2020) and other publicly available websites, I collect the names of provincial leaders. In this article, I restrict attention to the top two provincial leadership positions, i.e. the party secretary and the governor. The governor is, by law, the executive head of the provincial government, but, in the meantime, given the CCP's supremacy, the law explicitly regulates that the governor is always under the supervision of the provincial CCP committee in which the party secretary is the head. In terms of the division of labor, whereas the party secretary is responsible for all kinds of political-economic duties, the governor is responsible for local economic affairs in specific, including delivering provincial government work reports (Yao and Zhang, 2015). As a rule of thumb, the party secretary is the number-one leader and the governor is the number-two leader, but, generally speaking, both of them are of great importance for a province's economic development.

Throughout this article, a provincial leadership rotation/exchange refers to a person's movement from a province's governor or party secretary position to another province's governor or party secretary position. According to the official documents from the CCP Central Committee (2002), cadre exchanges are intended (i) to fulfill

new political and economic tasks in the destination province, especially when these tasks are not in favor of the incumbent leaders, (ii) to provide sufficient opportunities for the selected cadres to cultivate their leadership skills, and (iii) to prevent cadres from holding the same leadership position for too long. As mentioned in the introduction and visualized in Figure 1, over the 2005-2019 period, among the 62 top leadership positions across the 31 provinces in mainland China (i.e. the top two leadership positions of each province) with 189 unique leaders in total, there were 47 cross-province exchanges, with an average of 3 times per year. The average term of these shuffled leaders is 3.5 years (slightly smaller than 3.9 years, the average term of the non-shuffled leaders).[14] All provinces, except Hainan, had at least sent a governor or party secretary to or received a governor or party secretary from another province.

Among all these leadership rotations, most are movements from the governor position in a province to the governor or party secretary position in another province or from the party secretary position in a province to the party secretary position in another province. Movement from the party secretary position in a province to the governor position in another province only happened once between 2005 and 2019, and it was Guo Jinlong who moved from the party secretary of Anhui to the mayor of Beijing in 2007.[15]

Given China's centralized allocation of subnational leaders, one potential threat to the research design of this article is that provincial leadership rotations might be nonrandom in some cases. Fortunately, as pointed out by Jiang and Mei (2020) and Shi et al. (2021) and shown in the former paper's appendix, although whether a provincial leader would be promoted or rotated might be endogenous,[16] the destination of each leadership exchange, which usually depends on the availability of appropriate vacancies, seems to be quite exogenous. As discussed in Jiang and Mei (2020), one of the best-known examples in support of this exogeneity is Xi Jinping's

transfer from Fujian's governor position to Zhejiang's equivalent position in 2002: he admitted in an interview that his initial expectation was to be placed in a province in western China, where he had more connections.

In this article, I take two approaches to account for this potential endogeneity bias. First, I include a variety of fixed effects in my regression model to control for as many confounding factors as possible. Second, I conduct a robustness check in Section 4 by repeating my preferred specification but excluding the leadership rotations that are potentially less exogenous, where the extent to which a rotation is exogenous is determined by what happened to the shuffled leader's predecessor in the destination province.

At this stage, one might also be concerned that the difference between provincial government work reports could only reflect different governors' writing styles given that they are the *de jure* authors of these reports. However, governors seldom write these reports themselves. Rather, each of these reports is drafted by the research office of the provincial government under the guidance of the party secretary and the governor, and, based on publicly available information, the appointments of the officials affiliated with the research offices seem to be independent of provincial leadership rotations: according to the China Political Elite Database, which has information on 138 politicians who had worked for province-level research offices, only two of them had co-moved with their corresponding provincial leaders across different provinces,[17] and, as shown in Appendix D, this article's main estimation results would remain unchanged if these two special cases are excluded. Hence, a comparison between two provincial government work reports should still be perceived as a comparison between two provinces' economic policies instead of a comparison between two politicians' writing styles.

3. Estimation Strategies

In this section, I introduce the estimation strategies used for examining whether provincial leadership rotations could affect the convergence of subnational economic policies. I model, in subsection 3.1, the numbers of mentions of different phrases in government work reports and define the pairwise economic policy distance between provinces based on these reports. Then, in subsection 3.2, I validate this distance measure with external validity checks and several examples of the reports and the economic policies. Finally, I present, in subsection 3.3, the fixed effects model for identifying the causal relationship between leadership rotations and the convergence of provincial economic policies.

3.1. Economic Policy Distance

After transforming government work reports into matrices as explained in Section 2, what can be observed is a K-vector $\mathbf{c}_p$ of phrase counts for each report p (where $K = 9984$). Each element of $\mathbf{c}_p$, denoted by $c_{p,k}$, is the number of mentions of phrase k in report p. I denote the total number of phrases in this report by $m_p = \sum_{k=1}^{K} c_{p,k}$. As in Gentzkow et al. (2019b), I assume that $\mathbf{c}_p$ follows a multinomial distribution: $\mathbf{c}_p \sim MN(m_p, \boldsymbol{\theta}_p)$, where $\boldsymbol{\theta}_p$ denotes the vector of choice probabilities.[18]

As the goal of this article is to investigate the relationship between leadership rotations and the convergence of subnational economic policies, I define the textual distance between government work reports p and q as the square of the weighted Euclidean distance between the two vectors of choice probabilities: $\boldsymbol{\theta}_p$ and $\boldsymbol{\theta}_q$. Formally, this pairwise economic policy distance, denoted by $D_{p,q}$, could be expressed as

follows:

$$D_{p,q} = \|\mathbf{w} \cdot (\boldsymbol{\theta}_p - \boldsymbol{\theta}_q)\|_2^2 = \sum_{k=1}^{K} w_k \left(\theta_{p,k} - \theta_{q,k}\right)^2 \tag{1}$$

where $||\cdot||_2$ is the Euclidean norm on $\mathbb{R}^K$, and each element of $\mathbf{w}$, denoted by w_k, is the weight of phrase k that measures how much information this phrase could provide. For the baseline measure, $w_k = 1/K$ for each phrase k, i.e. all phrases are equally weighted. After presenting my baseline estimation results in Section 4, I also adjust these weights based on the inverse document frequency (IDF) and other weighting schemes as the robustness checks.

For each report p, I estimate $\boldsymbol{\theta}_p$ by using $\mathbf{c}_p$ and m_p, where $\widehat{\theta}_{p,k} = c_{p,k}/m_p$ is the k-th element of the estimator $\widehat{\boldsymbol{\theta}}_p$. Then, the estimator of $D_{p,q}$ could be expressed as follows:

$$\widehat{D}_{p,q} = \left\|\mathbf{w} \cdot (\widehat{\boldsymbol{\theta}}_p - \widehat{\boldsymbol{\theta}}_q)\right\|_2^2 = \sum_{k=1}^{K} w_k \left(\widehat{\theta}_{p,k} - \widehat{\theta}_{q,k}\right)^2 = \sum_{k=1}^{K} w_k \left(\frac{c_{p,k}}{m_p} - \frac{c_{q,k}}{m_q}\right)^2 \tag{2}$$

Intuitively, $\widehat{D}_{p,q} \in [0, 1]$ measures the extent to which the relative frequencies of the phrases used in two government work reports p and q are dissimilar. In terms of the mathematical notations, it quantifies the difference between $\{c_{p,k}/m_p\}_{k=1}^K$ and $\{c_{q,k}/m_q\}_{k=1}^K$. This distance measure would be at its minimum, i.e. $\widehat{D}_{p,q} = 0$, if (i) the set of the phrases used in one report is exactly the same as the set of the phrases used in the other report, and (ii) the relative frequency of each phrase in each report is also identical, i.e. $c_{p,k}/m_p = c_{q,k}/m_q$ for each phrase k. In contrast, this distance measure would be at its maximum, i.e. $\widehat{D}_{p,q} = 1$, if the two reports are extremely polarized, which means that (i) each report only contains one unique phrase, and (ii) this unique phrase for each report is different so that these two reports contain no

common phrase, i.e. $c_{p,k_1}/m_p = 1$ (report p only contains phrase k_1) and $c_{q,k_2}/m_q = 1$ (report q only contains phrase k_2) for some phrases k_1, k_2 with $k_1 \neq k_2$. These are the most extreme cases. In practice, if the weight of each policy issue is similar between two reports, then the distance measure $\widehat{D}_{p,q}$ would be small. In contrast, if two reports focus on some completely different policy issues or hold different opinions on a given subject, then the distance measure $\widehat{D}_{p,q}$ would be large.

3.2. Validating the Distance Measure

As in Kelly et al. (2021), which validated its measure of patent similarities by citations, I also compare my distance measure $\widehat{D}_{p,q}$ with some external measurement of economic dissimilarities between provinces. More specifically, I consider between-province differences in industrial structure and fiscal regime. Intuitively, provinces that are similar in the composition of output or government spending are more likely to implement similar policies, so the economic policy distance between them measured by their government work reports should be small. In line with this intuition, subfigure (a) of Figure C1 of Appendix C suggests that there indeed exists a positive correlation between the difference in industrial structure and my distance measure $\widehat{D}_{p,q}$, while subfigure (b) also exhibits a positive correlation between the difference in fiscal regime and $\widehat{D}_{p,q}$. As the definition for $\widehat{D}_{p,q}$ formulated by equation (2) does not rely on any information related to the composition of provincial output or government spending, the positive correlations displayed in Figure C1 of Appendix C should serve as reliable external validity checks.

In addition, to showcase how this distance measure could differentiate textual documents in practice, in Table C1 of Appendix C, I also provide an example with three paragraphs extracted from three different provincial government work reports:

Henan's report in 2010, and Hebei's reports in 2008 and 2006, respectively. All the three paragraphs are selected to be related to employment so as to make them comparable to each other.[19] It is obvious that the second paragraph (from Hebei's report in 2008), in comparison to the third paragraph (from Hebei's report in 2006), is much more similar to the first paragraph (from Henan's report in 2010) because the first two paragraphs specify some similar policies in detail even with some statistics, while the third paragraph is more abstract. Consistent with this observation, as shown in Appendix C, the distance between the first two paragraphs in terms of equation (2) is 1.215×10^{-4}, while that between the first and third paragraphs is 2.244×10^{-4}, and this distance measure differentiates textual documents in a manner that is similar to the human thought process. The primary reason for the relatively smaller distance between the first two paragraphs is that they both mention the phrases related to some specific employment-related policies, such as "career guidance", "college graduate", "vocational training" and "zero-employment household", and these terms are also mentioned with similar relative frequencies in the first two paragraphs. In the meantime, the similarity in the use of statistical evidence in these two paragraphs is captured by the relative frequencies of the phrases related to units of measurement, such as "thousand people" and "million people". In contrast, almost all of these phrases do not even appear in the third paragraph, so the textual distance between the first and third paragraphs is greater. Hence, although this distance measure defined by equation (2) is far from perfect, it could still reflect many important aspects of the difference between textual documents by comparing the relative frequencies of the phrases used in those documents. One should be confident of interpreting this measure $\widehat{D}_{p,q}$ as an appropriate estimator for the difference in the economic policies announced in government work reports p and q.

The discussion so far has shown that the Euclidean distance approach for-

mulated by equations (1) and (2) is intuitive and has a clear interpretation. This approach is also computationally efficient, which is of great importance given the extremely large number of between-province comparisons I need to make. However, one concern identified in Gentzkow et al. (2019b) is that, even if $E[\widehat{\boldsymbol{\theta}}_p] = \boldsymbol{\theta}_p$ holds true, $E[\widehat{D}_{p,q}] \geq D_{p,q}$ due to Jensen's inequality.[20] As suggested by Gentzkow et al. (2019b), the intuition behind this bias is that, when the number of phrases is large, many random phrases might be mentioned mainly by report p or mainly by report q by chance even if the true choice probabilities do not differ between them. Fortunately, as formally demonstrated in Appendix B, for each phrase k, as long as $\text{Cov}(\widehat{\theta}_{p,k} - \theta_{p,k}, \widehat{\theta}_{q,k} - \theta_{q,k}) = 0$, i.e. the measurement errors for different province-year combinations are not correlated, the positive bias $E[\widehat{D}_{p,q}] - D_{p,q}$ would be removed by the fixed effect for report p and that for report q as specified in equation (3) below, and the coefficient estimates based on equation (3) are still unbiased. To ensure that my estimation results are robust to different measures of economic policy distance, in Section 4, I also present findings based on some other measures of textual similarity.

Finally, I rely on a case study to show more about the association between the economic policy distance $\widehat{D}_{p,q}$ and the difference between actual policies, and this case study should also rationalize the fixed effects model in the next subsection. Soon after the Summer Olympics in Beijing, the collapse of an unlicensed mine landfill in Shanxi took the lives of more than 250 villagers in September 2008. In response, the central government demoted the governor and deputy governor of Shanxi and appointed Wang Jun, the then director of the State Administration for Work Safety, to take over as the province's new acting governor (Bradsher, 2008). Wang took a series of measures to improve safety performance in coal mining during his term in Shanxi. Then, in December 2012, he was promoted and also rotated to be the party secretary of Inner Mongolia, where he continued his efforts for reducing coal mine

deaths until his retirement in 2016.

How Wang's transfer from Shanxi to Inner Mongolia was associated with the diffusion of the policies for coal mine safety could be seen from the descriptive statistics presented in Panel A of Table C2 of Appendix C. First, Shanxi's laws and regulations for coal mine safety almost doubled from 2005-2008 to 2009-2012 while the increase in the rest of the country was only around 17%. This coincided with Wang Jun's transfer to Shanxi in late 2008. Second, Inner Mongolia's number of laws and regulations for coal mine safety had been relatively small before 2013. Its substantial increase from 20 to 81 occurred in 2013-2016, which again concurred with Wang's movement from Shanxi to Inner Mongolia.

This policy diffusion aligns closely with the changes in the economic policy distance $\widehat{D}_{p,q}$ defined in the preceding subsection. Let $f(p)$ be report p's province, $g(p)$ be report p's year, $f(q)$ be report q's province and $g(q)$ be report q's year. Suppose that the destination province $f(p)$ is Inner Mongolia, and report q (from a province other than Inner Mongolia) is restricted to be delivered four years before report p, i.e. $g(q) = g(p) - 4$. Then, according to Panel B of Table C2 of Appendix C, the average of $\widehat{D}_{p,q}$ for the origin province $f(q)$ being Shanxi decreased by 12% on average from $g(p) = 2012$ (before Wang's transfer) to $g(p) \in \{2013, 2014, 2015, 2016\}$ (after Wang's transfer); on the contrary, the average of $\widehat{D}_{p,q}$ for the origin province $f(q)$ being any other province declined only by less than 1%. Hence, this case study for Wang Jun and his efforts for improving coal mine safety indicates that the economic policy distance measured by $\widehat{D}_{p,q}$ could reflect the difference between actual policies.

3.3. Fixed Effects Model

The case study of Wang Jun above suggests that my econometric model should at least take two differences into account: (i) the before-after difference in treated province pairs with leadership rotations and (ii) the difference between the treated province pairs with leadership rotations and the non-treated province pairs without leadership rotations. That being said, I conduct reduced-form regressions based on the econometric model below to evaluate the extent to which leadership rotations could contribute to the convergence of subnational economic policies in China:

$$\widehat{D}_{p,q} = \beta L_{p,q} + \pi_p + \pi_q + \pi_{f(p),f(q)} + \pi_{g(p),g(q)} + \pi_{f(p),g(q)} + \pi_{g(p),f(q)} + \varepsilon_{p,q} \qquad (3)$$

where $f(p)$ refers to government work report p's province, $f(q)$ refers to government work report q's province, $g(p)$ refers to government work report p's year, $g(q)$ refers to government work report q's year, $\widehat{D}_{p,q}$ is the economic policy distance between government work reports p and q defined by equation (2), $L_{p,q}$ is the common leader dummy indicating whether at least one of province $f(p)$'s top two leaders in year $g(p)$ and one of province $f(q)$'s top two leaders in year $g(q)$ is the same person, π_p and π_q are the fixed effects for reports p and q,[21] $\pi_{f(p),f(q)}$ is the fixed effect for report p's province times report q's province, $\pi_{g(p),g(q)}$ is the fixed effect for report p's year times report q's year, $\pi_{f(p),g(q)}$ is the fixed effect for report p's province times report q's year, $\pi_{g(p),f(q)}$ is the fixed effect for report p's year times report q's province, and $\varepsilon_{p,q}$ is the idiosyncratic error term.

To avoid the comparison between a province and itself, I restrict $f(p) \neq f(q)$, and, to avoid duplicated observations when conducting regressions, I restrict $g(p) > g(q)$, where $g(p) = g(q)$ is also prohibited because $L_{p,q} = 0$ for sure if $g(p) = g(q)$,

i.e. there is no variation in the common leader dummy $L_{p,q}$ conditional on $g(p) = g(q)$. Intuitively, by conducting regression analysis based on equation (3) with the restrictions $g(p) > g(q)$ and $f(p) \neq f(q)$, I compare each provincial government work report during the 2005-2019 period with all the previous reports from the other provinces in the sample.[22] The parameter of interest is β. My hypothesis is that $\beta < 0$, which implies that provincial leadership rotations would promote the convergence of subnational economic policies in China.

A central assumption for the identification is that the movements of provincial leaders are exogenous. I discuss in Section 2 that most leadership rotations' destination provinces are random even though whether a provincial leader is selected to be rotated or not might be endogenous in some cases. I also show in the next section that the pooled OLS estimate of β without any fixed effect is already negative and statistically significant. However, there might still be some endogeneity concerns. To address this issue, I include the six fixed effects specified in equation (3) above, each of which absorbs some confounding factors.

I begin with including the fixed effects π_p and π_q for the two government work reports p and q. As pointed out in the preceding subsection and also Appendix B, these two fixed effects could remove the positive bias of $E[\widehat{D}_{p,q}] - D_{p,q}$ arising from the random phrases in each report. In the meantime, the fixed effect π_q for the earlier report q could account for everything taking place in the origin province $f(q)$ before a leadership rotation, while the fixed effect π_p for the later report p fully captures the characteristics of the destination province $f(p)$ after such a rotation.[23] In particular, π_q could address the potential confounding problem that report q might be a determinant of whether this report's corresponding provincial leaders would be rotated or not, and π_p ensures that my estimation results are specific to the similarity between reports p and q, conditional on how report p is similar to all other existing

reports in general.

Next, I choose to also use the fixed effects for province pairs (i.e. report p's province $f(p)$ times report q's province $f(q)$) and those for year pairs (i.e. report p's year $g(p)$ times report q's year $g(q)$), which are denoted by $\pi_{f(p),f(q)}$ and $\pi_{g(p),g(q)}$, respectively, in equation (3) for the following reasons: including the fixed effects for province pairs makes sure that my regression results are not confounded by the speculation that leadership rotations have tended to take place mostly in the province pairs sharing certain characteristics, and including the fixed effects for year pairs takes into account the possibility that there were more leadership rotations in certain years, and the policies announced in those years' reports happened to be much more similar to one another.

Finally, I include the fixed effect for report p's province times report q's year (denoted by $\pi_{f(p),g(q)}$) and that for report p's year times report q's province (denoted by $\pi_{g(p),f(q)}$). Specific to the destination province's report before the shuffled leader arrives and the origin province's report after the shuffled leader departs, these two fixed effects address the following concerns: the former controls for the policy choices of the shuffled leaders' predecessors in the destination provinces, while the latter controls for the policies made by the shuffled leaders' successors in the origin provinces.[24]

These six fixed effects should remove most of the potential endogeneity bias. Nevertheless, it could still be possible that some particular political-economic events related to both the destination and origin provinces have predated or coincided with leadership rotations.[25] Hence, in Section 4, after presenting my baseline estimation results with these fixed effects, I carry out a battery of robustness checks, such as trying with some first-differencing specifications and also an event study approach, to rule out this possibility.

4. Empirical Estimation Results

In this section, I report and interpret the estimation results. Subsection 4.1 presents the findings from the baseline model, the first-differencing specifications and some alternative methods. Subsection 4.2 takes advantage of an event study design to alleviate the endogeneity concern and evaluate the dynamic effects of leadership rotations. Subsection 4.3 conducts a series of robustness checks. Subsection 4.4 re-estimates the preferred specification with some alternative distance measures, including the use of the inverse document frequency (IDF) for adjusting the weights in vector **w** in equations (1) and (2). Subsection 4.5 discusses whether the convergence effect of leadership rotations has been mainly driven by between-topic or within-topic difference. Subsection 4.6 examines if the convergence of the policies announced in the government work reports has translated into similarities in some policy outcomes. Finally, subsection 4.7 explains why provincial leaders tend to implement similar economic policies in different provinces and provides some suggestive but convincing evidence in support of my explanation.

4.1. Baseline Estimation Results

The baseline estimation results for equation (3) are displayed in Table 1. I incrementally add the fixed effects shown in equation (3): column (1) is a pooled OLS model, column (2) includes π_p and π_q (i.e. fixed effects for reports p and q), and column (3) adds $\pi_{f(p),f(q)}$ (i.e. province pair), $\pi_{g(p),g(q)}$ (i.e. year pair), $\pi_{f(p),g(q)}$ (i.e. report p's province times report q's year) and $\pi_{g(p),f(q)}$ (i.e. report p's year times report q's province). To facilitate the interpretation, each coefficient estimate in the table reports how many standard deviations the dependent variable would change as the

corresponding independent variable increases from 0 to 1. Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the level of report p and that of report q.

≪COMP: Place Table 1 about here≫

As shown in these columns of Table 1, the coefficients estimates of the common leader dummy $L_{p,q}$ are all negative and statistically significant, regardless of the inclusion or exclusion of the fixed effects. Nevertheless, I still choose to take column (3) as the preferred specification, given that it controls for all the fixed effects specified in equation (3). Based on this preferred specification, if two provinces shared the same leader (either party secretary or governor), then the economic policy distance between these two provinces reflected by their government work reports would decrease by 22.5% of the standard deviation. This implies that provincial leadership rotations have indeed promoted the convergence of subnational economic policies in China.

To make sure that the findings in the first three columns are free from endogeneity due to the unobserved factors that are specific to each province pair and also evolve smoothly over time, following Baier et al. (2014), Jiang and Mei (2020) and other recent studies using the gravity model, I consider first-differencing specifications. That is, defining the operator Δ_n as $\Delta_n(X_{p,q}) = X_{p,q} - X_{p-n,q-n}$ for any variable $X_{p,q}$, where $p-n$ refers to province $f(p)$'s report delivered in year $g(p)-n$, i.e. n years before report p, I replace the dependent variable in equation (3), i.e. $\widehat{D}_{p,q}$, by its first-differencing over n years, i.e. $\Delta_n \widehat{D}_{p,q}$. Similarly, I replace the independent variable in equation (3), i.e. $L_{p,q}$, also by its first-differencing over n years, i.e. $\Delta_n L_{p,q}$. The fixed effects remain the same as those in equation (3). In these first-differencing specifications, as suggested above, $\pi_{f(p),f(q)}$ accounts for the unobserved factors that

are specific to the province pair and evolve smoothly over time.

I report the estimation results based on the first-differencing specifications in columns (4) through (6) of Table 1, with $n = 1$ in column (4), $n = 2$ in column (5) and $n = 3$ in column (6). As shown at the bottom of the table, the number of observations decreases very fast with n, so I only consider the cases where $n \leq 3$. According to the results displayed in the table, I find that the coefficient of the common leader dummy $L_{p,q}$ after first-differencing over n years is still estimated to be negative and statistically significant, and the magnitude is also similar to that in the preferred specification, i.e. column (3), thus enhancing the credibility of my finding that provincial leadership rotations have advocated the convergence of subnational economic policies in China.

At this stage, I realize that the regression model formulated by equation (3) is complicated even though it could effectively address the endogeneity concerns. Hence, I also try with two simplified methods to quantify the effects exerted by provincial leadership rotations. My first approach is to take advantage of Abowd et al. (1999)'s well-known framework for matched employer-employee data as follows:[26]

$$PC_p = \pi_{n(p)} + \pi_{f(p)} + \pi_{g(p)} + \varepsilon_p \tag{4}$$

where the dependent variable PC_p is the first principal component of government work report p's different phrases' numbers of mentions.[27] Intuitively, this variable could be understood as where the policies announced in report p fall on a given one-dimensional political-economic spectrum, which in turn implies that the similarity between reports p and q could be reflected by the small absolute difference $|PC_p - PC_q|$. For the right-hand side of the equation, I include the idiosyncratic error term ε_p and three fixed effects for (i) report p's corresponding provincial leader $n(p)$, (ii) report p's province

$f(p)$ and (iii) report p's year $g(p)$, respectively. The objective of this exercise is to evaluate the extent to which the fixed effects for provincial leaders $\pi_{n(p)}$ explain the dependent variable's variation.

As presented in Figure D1 of Appendix D which plots the kernel density of the estimated provincial leader effects denoted by $\pi_{n(p)}$, the dependent variable PC_p varies quite substantially from one leader to another. The significance of these leader-specific effects could also be seen by (i) the F-statistic for $\pi_{n(p)}$ being $F(173, 597) = 2.357$ with the resulting p-value being smaller than 0.001 and (ii) $\pi_{n(p)}$ explaining 40.6% of the variation in the residual after partialling out the fixed effects for provinces and years. These findings suggest that the reports corresponding to the same leader would tend to share certain characteristics (measured by PC_p) conditional on their provinces and years, which aligns with the baseline estimation results presented above.

Next, for the second approach, I still use the regression model expressed by equation (3), but I restrict that $g(q) = g(p) - \tau$ for each $\tau \in \{2, 4, 6\}$, i.e. report q has to be delivered exactly τ years before report p. In doing so, I could omit the last three fixed effects in equation (3), and the new equation could be written as below:

$$\widehat{D}_{p,q} = \beta L_{p,q} + \pi_p + \pi_q + \pi_{f(p),f(q)} + \varepsilon_{p,q} \tag{5}$$

The estimation results based on equation (5) are presented in Table D1 of Appendix D. The coefficients are shown to be negative and statistically significant for all three values of $\tau \in \{2, 4, 6\}$, which is consistent with my main findings based on the full model.

4.2. Event Study Estimation

Following Jiang and Mei (2020) and Jiang and Zhang (2020), to verify if leadership rotations are orthogonal to *ex ante* economic policy distance and other unobserved variables related to both the origin and destination provinces, which is a key assumption for the validity of the findings in Table 1, I take advantage of an event study design to examine how the economic policy distance defined in equation (2) would change a few years before and after each leadership rotation. In so doing, I also identify the dynamic effects of such rotations.

4.2.1. Identification

Recall that $f(p)$ is report p's province and $g(p)$ is report p's year. I focus on two types of events regarding leadership exchanges. One type of event is two provinces $f(p)$ and $f(q)$ getting "linked" when a new provincial leader (either party secretary or governor) with past work experience in province $f(q)$ is appointed in province $f(p)$, and this is hereafter referred to as event A. The other type of event is provinces $f(p)$ and $f(q)$ getting "delinked" when a provincial leader (either party secretary or governor) in province $f(p)$ with past work experience in province $f(q)$ is replaced by someone else without such experience, and this is hereafter referred to as event B.

I define $A_{p,q}$ as the dummy variable indicating whether year $g(p)$ is the first year when the party secretary or the governor of province $f(p)$ was the same as that of province $f(q)$ in year $g(q)$ (corresponding to event A) and $B_{p,q}$ as the dummy variable indicating whether year $g(p) - 1$ is the last year when the party secretary or the governor of province $f(p)$ was the same as that of province $f(q)$ in year $g(q)$ (corresponding to event B). Formally, using the leads and lags of event A and the leads of event B as the independent variables,[28] the event study regression model can

be expressed as follows:

$$\widehat{D}_{p,q} = \sum_{\tau=-3}^{2} \delta_{\tau}^{A} A_{p-\tau,q-\tau} + \sum_{\tau=0}^{2} \delta_{\tau}^{B} B_{p-\tau,q-\tau} + \pi_p + \pi_q + \pi_{f(p),f(q)} + \pi_{g(p),g(q)} + \pi_{f(p),g(q)} + \pi_{g(p),f(q)} + \varepsilon_{p,q} \quad (6)$$

where $p-\tau$, again, refers to province $f(p)$'s report delivered in year $g(p)-\tau$, i.e. τ years before report p, and the fixed effects and the error terms are the same as those in equation (3).[29]

δ_{τ}^{A} for any $\tau \in \{-3,-2,-1,0,1,2\}$ and δ_{τ}^{B} for any $\tau \in \{0,1,2\}$ are the coefficients to be estimated in this equation. If provincial leadership rotations are indeed orthogonal to *ex ante* economic policy distance and other unobserved variables related to both provinces $f(p)$ and $f(q)$, then $\delta_{-3}^{A} = \delta_{-2}^{A} = \delta_{-1}^{A} = 0$. In contrast, δ_{0}^{A}, δ_{1}^{A} and δ_{2}^{A} should be estimated to be negative if the baseline results in Table 1 are robust. For δ_{0}^{B}, δ_{1}^{B} and δ_{2}^{B}, if the convergence of provincial economic policies could continue after the shuffled leader leaves office, then these three coefficients should be estimated to be negative. They should be equal to zero if otherwise.

4.2.2. Estimation Results

Table 2 and Figure 3 present the event study estimates. Consistent with the expectation, δ_{-3}^{A}, δ_{-2}^{A} and δ_{-1}^{A} are all shown to be statistically insignificant. The p-value of the joint F-test for the null hypothesis $H_0 : \delta_{-3}^{A} = \delta_{-2}^{A} = \delta_{-1}^{A} = 0$ is far greater than 10%. These results confirm that the outcome variable $\widehat{D}_{p,q}$ has been unaffected before event A, thus showing that there is no pre-treatment trend and validating the use of equation (3) for causal inference. In particular, this should also alleviate the concern arising from the possibility that provincial leaders have been mostly shuffled

among provinces with similar *ex ante* economic policies.

≪COMP: Place Table 2 about here≫

≪COMP: Place Fig. 3 about here≫

In contrast, δ_0^A, δ_1^A and δ_2^A are all estimated to be negative and statistically significant, and the p-value of the joint F-test for the null hypothesis $H_0 : \delta_0^A = \delta_1^A = \delta_2^A = 0$ is even smaller than 1%, which demonstrates that there indeed exists a significant effect of provincial leadership rotations on the convergence of subnational economic policies. The magnitudes of these coefficient estimates, in line with those in Table 1, are between 25.5% and 35.7% of the dependent variable's standard deviation.

Finally, the coefficients corresponding to the periods after event B, i.e. δ_0^B, δ_1^B and δ_2^B, are all shown to be statistically insignificant, with the p-value of the joint F-test for the null hypothesis $H_0 : \delta_0^B = \delta_1^B = \delta_2^B = 0$ being far greater than 10%. As briefly explained above, these results indicate that provincial economic policies might cease to converge soon after the shuffled leader leaves office, hence implying that the effects exerted by leadership rotations might not be permanent. Indeed, many local leaders in China have tended to overturn their predecessors' policies. For instance, Geng Yanbo — the mayor of Datong from 2008 to 2013, the mayor of Taiyuan from 2013 to 2019, and the main character of the award-winning documentary *the Chinese Mayor* — earned his name from rebuilding the cities under his rule, but his successors have shown little interest in continuing his construction plans, and many infrastructure projects in these cities have remained unfinished (Su, 2020).

To verify this finding on how the successors of the shuffled leaders make policy

decisions, I also take an alternative event study approach as follows:

$$\widehat{D}_{j,t} = \sum_{\tau=-2}^{2} \psi_\tau LC_{j,t-\tau} + \pi_j + \pi_t + \varepsilon_{j,t} \tag{7}$$

where the dependent variable $\widehat{D}_{j,t}$ measures how province j's report in year t is different from the same province's report one year ago, $LC_{j,t-\tau}$ is a dummy variable indicating if there is any change in province j's leader (either party secretary or governor) in year $t-\tau$, π_j and π_t are the fixed effects for province j and year t, and $\varepsilon_{j,t}$ is the idiosyncratic error term. As presented in Table D3 and Figure D2 of Appendix D, conditional on the absence of any pre-treatment trend, a province would deviate from the policies announced in its government work reports in the past by 44.8% of the standard deviation if its party secretary changes or by 37.1% if its governor changes. These estimates confirm that a new provincial leader tends to choose some policies significantly different from those implemented by their predecessors, and this is not just specific to the successors of the shuffled leaders.

There are at least two possible reasons behind this difficulty of leaving behind any policy isomorphism. First, many successors of the shuffled leaders might be incapable of continuing the existing policies. For example, as the vice governor of Guizhou and a professional on monetary policy with work experience in the central government, Guo Shuqing initiated a successful dismantling of the in-kind housing distribution system in Guizhou's capital city Guiyang in 1998 (Zhu, 2013).[30] However, soon after Guo left Guizhou in 2001, the reform ended in a stalemate, partly because few of his successors had the financial expertise and the leadership ability to resolve the problems arising from this housing monetarization. Second, even if the predecessors' successful economic policies are replicable, according to Zhu (2014)'s case study on the policy diffusion among the districts in Tianjin, most successors

might still prefer to discover new policy instruments. By doing so, they could avoid being regarded as "followers" and attract more positive attention from the superior governments.[31] In summary, for these two reasons, the central government should not assume that a shuffled leader could leave a legacy of provincial policy isomorphism to the successor of her position.

4.2.3. Staggered Treatment Correction

Notice that recent econometric literature (e.g. De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Athey and Imbens, 2022) has pointed out that, if there exists heterogeneity in treatment effects, then the event study approach used in this subsection would yield a weighted average of all possible permutations of pairwise difference-in-differences estimators, where a pair is either the never-treated control group paired with a cohort of observations treated in year τ, or a cohort of observations treated in year τ paired with another cohort of observations treated in an earlier year $\tau' < \tau$. The concern is that these weights could be negative, which would contaminate the leads and lags in the event study design and potentially invalidate my findings above.

To address this bias, in the spirit of Callaway and Sant'Anna (2021), I define the treated cohorts for event A (i.e. the event that links two provinces) based on the year pairs $g(p)$-by-$g(q)$ in equation (6), estimate a separate event study regression (with three lags and three leads) for each treated cohort but only with the observations from this cohort and those from the never-treated control group, and take weighted averages of the point estimates across all treated cohorts with weights equal to each treated cohort's share among all the treated observations in the sample. I choose to focus on event A in this analysis because the size of each treated cohort would be very small if both events A and B are considered at the same time. As presented in the

first two columns of Table D4 of Appendix D, the estimation results based on these averages weighted by cohort size are similar to those displayed in Table 2 and Figure 3, which validates the event study estimates discussed in the preceding paragraph.

4.3. Robustness Checks

I conduct a series of robustness checks for my main findings in Table 3. First, I repeat the preferred specification, column (3) of Table 1, but (i) with alternative standard errors two-way clustered at both the province pair $f(p)$-by-$f(q)$ level and the year pair $g(p)$-by-$g(q)$ level in column (1) and (ii) with the first lead and the first lag of the common leader dummy as additional controls in column (2). As shown in the first two columns of Table 1, the new coefficient estimates of the common leader dummy are still negative and statistically significant while those of the first lag and the first lead are statistically insignificant. Hence, my findings are robust to different clustering approaches for standard errors, and there seems no anticipation or delay effect of the provincial leadership rotation.[32]

≪COMP: Place Table 3 about here≫

Second, governors might involve more in the drafting of the provincial government work reports than party secretaries because (i) they eventually deliver these reports in the provincial People's Congresses, and (ii) they are usually more responsible for local economic development. To further alleviate the concern that the similarity between the reports could only reflect the administrative path dependence in terms of word choices, I replace the common leader dummy $L_{p,q}$ by the common party secretary dummy $L_{p,q}^{(S)}$ that only indicates whether report p's province $f(p)$'s party secretary in report p's year $g(p)$ and report q's province $f(q)$'s party secretary in report q's year $g(q)$ were the same person. As presented in column (3) of Table 3, the coefficient of

this common party secretary dummy is still estimated to be negative and statistically significant, thus ruling out the possibility that the baseline results are only driven by those governors who could substantially participate in the writing of the reports.

Third, in addition to the leaders of the four direct-controlled municipalities (DCMs), the party secretaries of Guangdong and Xinjiang have also been members of the Politburo in some years. As Bai et al. (2008) suggested that these Politburo members might behave differently in the sense that they are usually more aligned with the central government than other provincial leaders, I exclude these two prestigious provinces in column (4) of Table 3. The new coefficient estimate is still negative and statistically significant, which alleviates this concern.

Fourth, some recent studies have pointed out that policies could still be diffused among neighboring provinces without shuffling the leaders (e.g. Shipan and Volden, 2008; Zhu and Zhang, 2016). To ensure that my main findings are not driven by this kind of horizontal diffusion, I exclude the observations with the distance between the origin and destination provinces smaller than 1500 kilometers. As reported in column (5) of Table 3, the coefficient after imposing this restriction is still estimated to be negative and statistically significant, and the magnitude is even larger. Thus, my main findings are unlikely to be confounded by the geographical proximity or the horizontal policy learning.

Fifth, even though the fixed effects specified in equation (3) could already address much of the endogeneity of leadership rotations, I still perform a robustness check here to show that my main findings are not sensitive to the exclusion of certain leadership rotations that were potentially less exogenous. To this end, I define a leadership rotation to be relatively more exogenous if the destination province's previous leader is promoted to a higher position or hits the age limit of 65. The vacancies as a result of these two reasons are the by-products of the central government's more

important tasks of selecting national leaders and enforcing the rules on retirement, so they are usually less endogenous in comparison to the rotations that aim to make changes to the destination provinces.[33] Given that $L_{p,q} = 1$ means that there exists a provincial leader that corresponds to both government work reports p and q, I denote the dummy variable indicating whether reports p and q are both associated with a shuffled leader that is rotated to fill a more exogenous vacancy defined above by $L_{p,q}^{(X)}$. In the meantime, I denote the dummy variable corresponding to other reasons for leadership rotations by $L_{p,q}^{(D)} = L_{p,q} - L_{p,q}^{(X)}$. Then, I regress the economic policy distance $\widehat{D}_{p,q}$ on these two new dummy variables $L_{p,q}^{(D)}$ and $L_{p,q}^{(X)}$ in column (6) of Table 3. As presented in the table, both coefficient estimates are negative and statistically significant, and their magnitudes are very close to that of the preferred specification, column (3) of Table 1. Hence, excluding the leadership rotations that are potentially less exogenous would not nullify my main findings. Nevertheless, I still choose to include all the rotations for all other regressions throughout this article to keep as much variation as possible.

Finally, following Yao and Zhang (2015) and Shi et al. (2021), I permute leaders' tenures across years within their corresponding provinces and re-estimate the coefficient of the common leader dummy $L_{p,q}$ after each permutation. Given that the number of possible permutations is extremely large, a full permutation is not computationally efficient, so I only conduct 1000 random permutations. As shown by the histogram and the kernel density curve in Figure 4, these 1000 placebo coefficient estimates are centered around zero and range from -0.224 to 0.253.[34] That being said, all of them are larger than -0.225 from column (3) of Table 1, which, therefore, confirms that the baseline estimates do not just result from heteroskedastic shocks.

≪COMP: Place Fig. 4 about here≫

4.4. Alternative Distance Measures

As the estimation results above rely heavily on the unweighted squared Euclidean norm defined by equations (1) and (2) in Section 3, in this subsection I employ some alternative methods for quantifying pairwise economic policy differences between provinces to show that the choice of the distance measure would not invalidate my main findings.

First, as in Kelly et al. (2021), I adjust the vector of weights $\mathbf{w}$ in equations (1) and (2) based on the inverse document frequency (IDF) of each phrase. Formally, each element of $\mathbf{w}$, denoted by w_k for phrase k, is re-defined by $w_k = \frac{\log(N/N_k)}{\sum_{k'=1}^{K}\log(N/N_{k'})}$, where $N = 465$ is the number of government work reports in total, and N_k is the number of reports in which phrase k appears. In doing so, very common phrases that appear in most or all documents would be re-assigned with relatively lower weights so that the reports could be differentiated from one another much more easily. Denoting the pairwise economic policy distance based on this new weighting scheme by $\widehat{D}_{p,q}^{(idf)}$,[35] I regress this new dependent variable on the common leader dummy $L_{p,q}$. As shown in column (1) of Table 4, the coefficient estimate is still negative and statistically significant, thus enhancing the credibility of my main findings.

≪COMP: Place Table 4 about here≫

Second, I change the squared Euclidean norm into the cosine similarity score used in Hoberg and Phillips (2016) and Kelly et al. (2021), among others, when measuring the economic policy distance. That is, I replace $\widehat{D}_{p,q} = ||\mathbf{w}\cdot(\widehat{\boldsymbol{\theta}}_p - \widehat{\boldsymbol{\theta}}_q)||_2^2$ defined in equation (2) by $\widehat{D}_{p,q}^{(cos)} = (||\widehat{\boldsymbol{\theta}}_p||_2||\widehat{\boldsymbol{\theta}}_q||_2)^{-1}(\widehat{\boldsymbol{\theta}}_p\cdot\widehat{\boldsymbol{\theta}}_q)$ (which is positively associated with economic policy isomorphism).[36] I regress this cosine similarity score on the common leader dummy $L_{p,q}$. As reported in column (2) of Table 4, the coefficient is

still shown to be positive and statistically significant as expected, which is consistent with the baseline results and implies that my findings do not seem sensitive to the choice of the distance measure.

Finally, I regress the Levenshtein distance on the common leader dummy $L_{p,q}$. As another popular measure of linguistic difference, the Levenshtein (1966) distance between two textual documents refers to the minimal number of transformations (i.e. deletions, insertions and reversals) needed for one document in order to make it the same as the other (Isphording and Otten 2013, 2014; Adsera and Pytlikova, 2015; Ginsburgh and Weber, 2020; Molina et al., 2021). For each pair of provincial government work reports, I compute its corresponding Levenshtein distance after removing all punctuation and denote it by $\widehat{V}_{p,q}$. As reported in column (3) of Table 4, I find that, after replacing the measure based on the squared Euclidean norm by this Levenshtein distance as the dependent variable, the coefficient of the common leader dummy $L_{p,q}$ is still estimated to be negative and statistically significant, which once again suggests that the use of an alternative distance measure would not nullify my findings.

Although both the squared Euclidean norm and the Levenshtein distance could be used to compare between two textual documents, precisely what they measure is different. As the squared Euclidean norm only considers the probability distribution of the numbers of mentions of different phrases, neither where these phrases are located in the textual documents nor the length of these documents is taken into account. In contrast, the Levenshtein distance considers these two differences in addition to the numbers of mentions of different phrases. Given that the ordering of different policy issues in the official documents could also be very informative,[37] I conduct relevant regressions to examine whether the common leader dummy $L_{p,q}$ is negatively associated with the similarity of where different phrases are placed in

a pair of government work reports, conditional on other types of differences between these two documents.

To this end, I regress the Levenshtein distance (which contains the information with respect to both the distribution of different phrases' frequencies and the locations of these phrases in the reports) on the common leader dummy $L_{p,q}$, conditional on the economic policy distance $\widehat{D}_{p,q}$ measured in terms of the squared Euclidean norm (which only contains the information with respect to the numbers of mentions of different phrases) and the document length difference $\widehat{Q}_{p,q}$, which is defined as the absolute value of the difference in terms of character counts between two reports. The results are reported in column (4) of Table 4. The magnitude of the coefficient of the common leader dummy $L_{p,q}$, around 3.5%, is estimated to be smaller than that in column (3), but it is still negative and statistically significant, which implies that, although the effect of provincial leadership rotations has been exerted predominately on the number of mentions of each phrase used in the government work reports, the locations of different phrases in these reports still matters to some extent.

4.5. Policy Isomorphism Between and Within Topics

My baseline measure of pairwise economic policy distance captures changes both in the topics provincial government work reports choose to cover and in the phrases they use to discuss each topic. To separate between-topic and within-topic variations as in Gentzkow et al. (2019b), I apply the topic modeling method used in Jiang and Zhang (2020) to provincial government work reports. Specifically, I treat each paragraph of each report as a separate document. Given a pre-specified number of topics hereafter denoted as $R \in \{30, 35, 40\}$, I take advantage of the Latent Dirichlet Allocation (LDA) model developed by Blei et al. (2003)[38] to automatically generate R

topics from the entire corpus. Then, for each paragraph, I compute the percentage of the text associated with each topic, and, for each government work report, I calculate the averages of these topic proportions weighted by the length of each paragraph in the report. After these procedures, I obtain each topic's proportion in each report as well as each phrase's relative importance for each of the R topics.

4.5.1. Between-Topic Analysis

To measure the between-topic pairwise economic policy distance, for each pair of reports p and q, I compute the distance between report p's $R \times 1$ vector of topic proportions and report q's $R \times 1$ vector of topic proportions in terms of the squared Euclidean norm defined by equation (2). I denote this new distance measure by $\widehat{LDA}_{p,q}$. Similar to the explanations for $\widehat{D}_{p,q}$ in Section 3, $\widehat{LDA}_{p,q}$ would be small if the relative frequency of each of the R automatically generated topics is similar between two reports, or it would be large if two reports focus on some completely different topics. For each $R \in \{30, 35, 40\}$, I regress $\widehat{LDA}_{p,q}$ on the common leader dummy $L_{p,q}$. As presented in Table 5, the coefficients of the common leader dummy are estimated to be negative and statistically significant across all three specifications, which suggests that leadership rotations have exerted a significant between-topic convergence effect, and this finding is not sensitive to the pre-specified number of topics labelled by R. See Appendix E for further explanations about this topic modeling approach. I also report the top phrases for each of the automatically generated topics in Table E1 of Appendix E.

≪COMP: Place Table 5 about here≫

4.5.2. Within-Topic Analysis

Next, I turn to evaluating the within-topic convergence. As mentioned above, the LDA model returns the relative importance of each phrase for each of the topics it automatically generates from the government work reports. For each topic, I use this measure of relative importance as the weight for each phrase, i.e. w_k in equation (2), and re-compute the pairwise economic policy distance specific to that topic. Then, I regress this topic-specific measure on the common leader dummy $L_{p,q}$ as above. See Appendix E for more discussion of this within-topic analysis based on the provincial government work reports.

The estimation results for all topics are presented in Table 6. Due to the limited space, I only discuss the results for the pre-specified number of topics $R = 30$ in this subsection. See Tables E2 and E3 of Appendix E for the results corresponding to $R \in \{35, 40\}$. As displayed in the table, although 27 out of 30 coefficient estimates are negative, only seven of them are statistically significant. That being said, the convergence in the phrase choice of a given topic has been much weaker in comparison to the between-topic convergence. This result is very different from Gentzkow et al. (2019b), which found that within-topic partisanship (i.e. choosing different sides of the same coin) has been stronger than between-topic partisanship (i.e. choosing different coins) in the United States. However, it is actually consistent with China's political centralization in the sense that, although different provincial leaders could choose to prioritize different policy issues, what they are permitted to do for a given policy issue is limited regardless of leadership rotations, and this is because within-topic partisanship, in comparison to between-topic partisanship, is more likely to provoke debate and undermine the country's solidarity meticulously maintained by the central government.[39]

≪COMP: Place Table 6 about here≫

Nevertheless, there still exists some variation in these within-topic estimates that deserves more exploration. Given China's unique central-regional relationship in which some bureaucracies are administered centrally in the unitary form (i.e. U-form) while others are administered locally in the multi-divisional form (i.e. M-form),[40] one potential determinant might be the provincial governments' discretion over a given policy issue. More specifically, the hypothesis is that the U-form policy issues are less susceptible to the rotations of provincial leaders than the M-form ones, so the within-topic estimates corresponding to the former are less likely to be negative in comparison to the latter.

I evaluate this hypothesis by taking advantage of the reforms in 2016 that switched the Ministry of Environmental Protection from M-form to U-form (Kostka and Zhang, 2018; Jia and Chen, 2019; Wang and Yang, 2021). To this end, I repeat the within-topic estimation for "Topic 17: Pollution Reduction" and "Topic 19: Ecology" in Table 6, one at a time, but with one simple modification: the coefficient of the common leader dummy $L_{p,q}$ is now allowed to vary across report p's year $g(p)$. The new estimation results corresponding to the years between 2015 and 2019 are presented in Table E4 and Figure E1 of Appendix E.[41] For both topics, the coefficients are shown to be negative and statistically significant for the years before 2017 but insignificant afterwards. This implies that the effects of leadership rotations on the convergence of the environmental policies at the province level have declined soon after this vertical management reform in 2016, which is consistent with the hypothesis and should serve as suggestive but convincing evidence on the role of organization structure in how provincial leaders discuss a given policy issue. See Appendix E for more details on this exercise.

4.6. Effects on Policy Outcomes

The discussion thus far has primarily focused on the textual difference between government work reports. However, it has to be acknowledged that the policies eventually implemented by provincial leaders might sometimes deviate from those announced in the reports delivered at the beginning of each year. To address this potential concern, I analyze two alternative dependent variables in this subsection to further understand how leadership rotations at the province level have affected actual policies and their outcomes.

I begin with collecting all the 111,479 headlines of the news articles reporting provincial leaders' activities between 2007 and 2019 from the website of China's official newspaper *People's Daily*.[42] I treat each headline as a separate textual document. After some data processing with the LDA-based topic modeling method discussed in subsection 4.5, I obtain topic proportions for all headlines, where the number of the automatically generated topics is set to be 30. Recall that each p or q corresponds to a unique province-year combination, with $f(p)$ and $f(q)$ are p's and q's provinces and $g(p)$ and $g(q)$ are p's and q's years. Then, my next step is to compute the average of each topic's proportion for each province-year combination p or q, and the distance between province $f(p)$'s vector of topic proportions in year $g(p)$ and province $f(q)$'s vector of topic proportions in year $g(q)$ is still measured based on the squared Euclidean norm as in equation (2). I denote this between-topic distance measure between p and q in terms of the news headlines by $\widehat{LDAH}_{p,q}$. I regress this new distance measure on the common leader dummy $L_{p,q}$, with the fixed effects identical to those in equation (3). See Appendix E for further explanations about how I apply this LDA-based topic modeling method to the data set of the news headlines.

In addition to these news headlines, I also take advantage of the data set on

the composition of provincial government spending, which is used in Section 3 for the external validity checks. Specifically, I collect the data for the share of each category's expenditure in each province's total government spending from the National Bureau of Statistics of China. Then, for each pair of province-year combinations p and q, I compute the difference between province $f(p)$'s vector of category-level government spending shares in year $g(p)$ and province $f(q)$'s vector of category-level government spending shares in year $g(q)$ in terms of the squared Euclidean norm in equation (2). Denoting this difference in fiscal regime by $\widehat{EXP}_{p,q}$, I regress this new variable on the common leader dummy $L_{p,q}$ following equation (3). See Appendix C for more details on how this distance measure $\widehat{EXP}_{p,q}$ is created.[43]

Table 7 presents the estimation result for the difference in the news headlines $\widehat{LDAH}_{p,q}$ in column (1) and that for the difference in the composition of provincial government spending $\widehat{EXP}_{p,q}$ in column (2). Both coefficients are estimated to be negative and statistically significant, which confirms that the convergence of the economic policies in the government work reports resulting from leadership rotations has translated into similarities in (i) the daily activities in which these shuffled leaders participate and (ii) how government spending at the province level is allocated among different expenditure categories. These estimation results should at least partially alleviate the potential concern that the findings based on the government work reports might be meaningless if some policies announced in these reports have never been implemented.

≪COMP: Place Table 7 about here≫

Nevertheless, it deserves mention that the standard errors in this table are larger than that of the baseline estimate in column (3) of Table 1 and that of the between-topic estimate in column (1) of Table 5. One obvious explanation is that the

number of observations for the government work reports is larger, but there might be another possible reason: as many provincial leaders' activities are not policy-oriented and those leaders' control over certain categories of government spending might be limited, the identifying information from the news headlines and the composition of local government spending is probably much noisier than that from government work reports. This also justifies the use of these government work reports for this article's main specifications.

Lastly, to understand the dynamic effects on actual policies and their outcomes, I replicate the event study estimation following equation (6), except that the dependent variable is replaced by either the difference in the news headlines reporting provincial leaders' activities $\widehat{LDAH}_{p,q}$ or the difference in the composition of local government spending $\widehat{EXP}_{p,q}$. Recall that an event A is two provinces $f(p)$ and $f(q)$ getting "linked" when a new provincial leader with past work experience in province $f(q)$ is appointed in province $f(p)$, whereas an event B is provinces $f(p)$ and $f(q)$ getting "delinked" when a provincial leader in province $f(p)$ with past work experience in province $f(q)$ is replaced by someone else without such experience.

The event study estimates are presented in Table 8 and Figure 5. As in subsection 4.2, I also re-conduct the estimation to accommodate the staggered treatment correction in columns (3) through (6) of Table D4 of Appendix D, and the new estimates do not qualitatively change the results. The major takeaways from the event study estimation are threefold. First, all coefficients are shown to be statistically insignificant before any event A that "links" between provinces $f(p)$ and $f(q)$ by a leadership rotation, with the p-values of the joint F-tests being far greater than 10%. This confirms that there seems no pre-treatment trend, thus validating the findings in Table 7.

≪COMP: Place Table 8 about here≫

≪COMP: Place Fig. 5 about here≫

In contrast, the estimation results for the coefficients between events A and B are mixed: although all of them are estimated to be negative and the p-values of the joint F-tests are also smaller than 10%, only half of them are individually statistically significant at the 10% level. This suggests that leadership rotations have, to some extent at least, increased similarities in both the news headlines reporting provincial leaders' activities and the composition of local government spending, but such effects are shown to be much noisier than the effect on the government work reports. This is in line with my argument for the relatively large standard errors in Table 7. Furthermore, the magnitudes of the coefficients for the difference in fiscal regime are also estimated to increase over time after an event A, which implies that, in contrast to the immediate effects on the government work reports, it takes more time for the shuffled leaders to have impacts on the policy outcomes.

Finally, it is interesting to see that, after an event B that "delinks" between provinces $f(p)$ and $f(q)$ by the departure of the shuffled leader, while the coefficient estimates for the difference in the news headlines are statistically insignificant, those for the difference in fiscal regime are still mostly negative and also jointly significant at the 10% level. Different from the statistically insignificant event study estimates for the government work reports in the years after an event B presented in Table 2 and Figure 3, these new estimation results indicate that, although many of the policies chosen by the shuffled leaders would no longer appear in the government work reports delivered after their departures, their successors might still have to continue some of these policies, especially fiscal policies, for a short period of time.[44] Nevertheless, the coefficients of $B_{p-2,q-2}$ in both columns are still estimated to be statistically

insignificant in the end, thus suggesting that the shuffled leaders' impacts on policy outcomes would eventually disappear as well, though the process might be more gradual than what happened to the government work reports.

4.7. Mechanisms

Why have Chinese politicians chosen similar economic policies across different provinces under their rule? An obvious explanation is that each provincial leader's expertise tends to remain unchanged in the short term. For example, Zhang Chunxian is known for promoting highway permeability when he was the party secretary of Hunan and Xinjiang, and many believe that these achievements in highway construction are largely related to his experience as China's minister of transport from 2002 to 2005.

While acknowledging this rationale, in this subsection I show that there exists an alternative explanation that might be equally important: I propose that a leadership exchange could be viewed as an indication that a provincial leader has been selected to be further promoted in the future because the central government favors the economic policies she implemented in the origin province. Because of this indication, a shuffled provincial leader would tend to improve her portfolio of achievements by choosing similar economic policies in the destination province to further maximize the chance of being promoted to an even higher position.

I validate this explanation in two steps. First, I examine whether a provincial leadership rotation is an indication of further promotions in the future. Second, I test if the shuffled cadres moving to the provincial leadership positions associated with higher promotion probabilities have been more likely to continue their previous policies.

4.7.1. Provincial Leadership Rotations and Politburo Promotion

According to Yao and Zhang (2015), one objective of China's leadership rotations is to allow the central government to fairly evaluate local leaders across locations and time. However, subnational leadership exchanges are costly because they create uncertainty about local economic policies as pointed out by Zhu and Zhang (2017),[45] which suggests that the central government could not shuffle all local leaders. As a result, it would prioritize shuffling the leaders who have the potential to be further promoted, and that potential should be largely related to the economic performance of the regions under these leaders' rule.[46] Indeed, previous studies, such as Li and Zhou (2005), have already found that, for example, the local leaders who have been in the same location for more than five years are less likely to be promoted.

To verify this pattern, I employ the turnover data of provincial leaders between 1992 and 2019. More specifically, for each of these provincial party secretaries and governors, excluding those in the four direct-controlled municipalities, I collect information on (i) whether that person experienced any leadership exchange at the province level and (ii) whether that person later became a member of the Politburo or the Politburo Standing Committee (PSC). The relevant descriptive statistics are presented in Table 9 for both shuffled and unshuffled provincial leaders. The major takeaways are twofold. First, as reported in the first row, only around 27% of provincial leaders could be rotated, which is in line with the hypothesis that the central government is unable to shuffle all subnational leaders because doing so would be very costly. Second, while 37.3% of the shuffled provincial leaders entered the Politburo, only 3.4% of those with no experience of provincial leadership rotation could achieve this promotion. In terms of winning a seat in the more prestigious PSC, it is 18.7% versus 0.5%. Hence, a provincial leadership rotation seems to be a strong

signal for further promotions in the future for many provincial leaders.

≪COMP: Place Table 9 about here≫

4.7.2. Moving to "Better" Provincial Leadership Positions

Thus far, I have shown that provincial leadership rotation is positively associated with the probability of ascending into the Politburo. In other words, these rotations are signals of the central government's endorsements of the corresponding shuffled leaders and their achievements in the origin provinces. I now turn to confirming that one key possibility for such a shuffled cadre to continue her policies in the destination province is related to these endorsements. To this end, I test if the tendency of implementing similar economic policies has been stronger among the shuffled cadres moving to the provincial leadership positions associated with higher promotion probabilities, given that this kind of rotation should be viewed as a stronger signal of endorsement.

Given China's substantial inland-coastal inequality, those who have governed a coastal province, such as Shandong and Fujian, tend to have more potential to be promoted to the Politburo. That is to say, leadership positions in coastal provinces appear to be superior to those in inland provinces on average.[47] Hence, I begin with examining the heterogeneity with respect to whether the destination province of a leadership exchange is inland or coastal. More specifically, following equation (3), I separately estimate the coefficients of the common leader dummy $L_{p,q}$ for two groups of the leaders: leaders in the first group moved between inland provinces, while those in the second group moved from an inland province to a coastal one. As presented in column (1) of Table 10, the magnitude of the coefficient estimate corresponding to the inland-to-coast movements is significantly larger than that corresponding to the exchanges between inland provinces. Hence, it is possible that the tendency of

emulating the policies in the origin province is positively associated with whether the shuffled cadre moves to a better provincial leadership position.

≪COMP: Place Table 10 about here≫

However, not all inland provinces are inferior, and not all coastal provinces are superior. For instance, most party secretaries in Xinjiang, an inland region, were also members of the Politburo, whereas many believes that leaders in Guangxi, a coastal region, are unlikely to be further promoted. To address this concern, I rely on the "office-by-province" fixed effects controlled in Jia et al. (2015). In more detail, Jia et al. (2015) conducted the following regression:

$$\begin{aligned} PROB_{nojt} = {} & \alpha_c CONNECTED_{nt} + \alpha_g GROWTH_{nojt} \\ & + \alpha_{cg} CONNECTED_{nt} \times GROWTH_{nojt} + \mathbf{x}'_{nojt}\boldsymbol{\gamma}_x \\ & + GROWTH_{nojt}\mathbf{x}'_{nojt}\boldsymbol{\gamma}_{xg} + \sum_{\tau=2}^{12} \lambda_\tau TIME^{(\tau)}_{nojt} + \mu_{oj} + \mu_{ot} + \epsilon_{nojt} \end{aligned} \quad (8)$$

where $PROB_{nojt}$ indicates whether person n in provincial leadership position o (either party secretary or governor) in province j is promoted in year t, $CONNECTED_{nt}$ indicates whether person n is connected to any Politburo Standing Committee (PSC) member in office, $GROWTH_{nojt}$ is the demeaned average annual growth rate of province p since leader n assumed office o until year t, $\mathbf{x}_{nojt}$ is a set of control variables, $TIME^{(\tau)}_{nojt}$ is a dummy variable that indicates if leader n has been in office o for τ years, μ_{oj} is the "office-by-province" fixed effect (i.e. the province fixed effect that is allowed to differ between party secretaries and governors), μ_{ot} is the "office-by-year" fixed effect (i.e. the year fixed effect that is allowed to differ between party secretaries and governors) and ϵ_{nojt} is the idiosyncratic error term.

While Jia et al. (2015) paid more attention to the estimates of α_c, α_g and α_{cg},

in this subsection I focus on these "office-by-province" fixed effects. Denoted by μ_{oj}, these fixed effects quantify the extent to which holding the leadership position o (either party secretary or governor) of province j is associated with the chance of further promotion in the future,[48] conditional on all other independent variables remaining unchanged. This quantification could be supported by a variety of examples: μ_{oj}'s value for Shandong's party secretary is 0.273, ranking second behind Shanghai among China's 31 province-level regions, and this was consistent with the pattern that many Politburo members had work experience in Shandong, such as Wu Guanzheng, Zhang Gaoli and Li Jianguo; in contrast, the fact that none of Ningxia's governors have ever achieved a position that is equivalent to or higher than a provincial party secretary is also in line with Ningxia's governor's value of μ_{oj} being -0.136, the lowest among all governor positions. See Appendix D for more details on this quantification.

Based on these "office-by-province" fixed effects μ_{oj}, I classify all provincial leadership rotations into five categories: (i) movements between party secretary positions with the destination's value of μ_{oj} smaller than the origin's value (i.e. the destination being "worse" for party secretaries),[49] (ii) movements between party secretary positions with the destination's value of μ_{oj} larger than the origin's value (i.e. the destination being "better" for party secretaries),[50] (iii) movements between governor positions with the destination's value of μ_{oj} smaller than the origin's value (i.e. the destination being "worse" for governors), (iv) movements between governor positions with the destination's value of μ_{oj} larger than the origin's value (i.e. the destination being "better" for governors), and (v) movements between a party secretary position and a governor position. Given that party secretaries are different from governors in terms of their duties and their rankings in China's bureaucratic hierarchy, I only consider the first four categories.

The four coefficient estimates corresponding to these four categories are dis-

played in column (2) of Table 10. Regardless of the use of the common party secretary dummy $L_{p,q}^{(S)}$ or the common governor dummy $L_{p,q}^{(G)}$,[51] the magnitude of the coefficient corresponding to the movements to better destinations is shown to be significantly larger than that corresponding to the movements to worse destinations. Hence, this article's main finding that a shuffled leader would choose similar policies in different provinces seems related to the extent to which such a leadership rotation represents the central government's endorsement of that leader and her policies implemented in the origin province.

At this point, some might claim that this heterogeneity with respect to the promotion probability associated with the destination provincial leadership position conflicts with subsection 4.2's event study results in the sense that, if a provincial leader is selected to be rotated because her policies are favored by the central government, why have the successor of her position not carried on these policies? This seeming contradiction could be explained from several aspects. According to my interpretation of the event study results in subsection 4.2, even if the policies implemented by the shuffled leaders are strongly favored by the central government, their successors might not have the capabilities needed to continue these policies, and they would also like to avoid being regarded as "followers", which could be detrimental to their political careers. Moreover, even if both the shuffled leaders and their successors decide to continue some of the existing policies, they would be very likely to choose two completely different subsets of these policies to carry on.[52] That being said, the coexistence of my explanation in this section and the event study findings does not seem contradictory. Yet, I admit that the political and economic interactions between the shuffled leaders and their successors still deserve further exploration with different theoretical and empirical efforts in the future.

5. Conclusion

As summarized in the high-level review by Xu (2011), one institutional reason behind China's economic success over the past four decades is its economic decentralization, which has allowed local governments to customize their own policies and encouraged a wide range of local experimental reforms. However, economic decentralization so far has also increased the difficulty for the central government to maintain the country's political centralization.

In this article, I show that provincial leadership rotation has been an effective solution to this challenge. I find that, if two provincial government work reports correspond to the same leader, then the difference between these reports in terms of word choices would decrease by around 22.5% of the standard deviation, which suggests that leadership rotations at the province level have promoted the convergence of subnational economic policies across China. Moreover, I show that (i) this economic policy similarity has been driven mainly by convergence in which topics these reports cover, rather than by convergence in how the provincial leaders talk about a given policy issue, and (ii) such policy isomorphism has translated into similarities in some policy outcomes, namely the provincial leaders' activities and the composition of provincial government spending. Nevertheless, according to my event study estimates, this convergence might disappear soon after the shuffled leader leaves office, thus implying that the effect of provincial leadership rotations might not be permanent.

While acknowledging that one rationale for the shuffled leaders to continue their policies is that their expertise tends to remain unchanged in the short term, I propose an alternative explanation: a leadership exchange could indicate that a given leader is being considered for further promotion in the future because the central government favors the economic policies that person implemented in the origin province. As

a result, that shuffled leader would tend to replicate some of these policies in the destination province to maximize the chance of even higher promotions.

Broadly speaking, this article contributes to at least three strands of literature. First, in terms of the literature on how political leaders could shape economic development conditional on the structural factors, I confirm that provincial leaders in China have been influential to local economic policymaking, but my event study estimates show that their successors usually have no interest in continuing their policies. Second, as a contribution to the literature on China's political economy, I find that leadership rotations have contributed effectively, though not permanently, to the political and economic integration across China, potentially reducing some side-effects of economic decentralization. Third, I show that, in addition to the numbers of mentions of different phrases, writing structures are also of great importance when analyzing textual documents. Future studies should pay more attention to where different phrases are placed in textual documents.[53]

The findings in this article suggest that leadership rotations have been beneficial to many aspects of China's economic development. It seems that Beijing has also been aware of the advantages of this institutional arrangement and has been shuffling local leaders more frequently in recent years, especially after announcing the new regulations on selecting cadres in March 2019. It is interesting to see that some of these new regulations have even encouraged county-level and city-level leaders to exchange across provinces, which had been quite unusual before.

However, I do not wish to imply that leadership rotation is an optimal institutional arrangement. One important concern is that provincial leaders might be "misallocated" by the central government. For instance, Su Rong's interest in afforestation, as discussed in the introduction section, was conducive to Gansu, around 15% of which is desert land, but this enthusiasm for planting trees should not be

continued after his movement to Jiangxi, where deforestation is less severe and land should be prioritized for agricultural activities. This is consistent with Allcott (2015), Al-Ubaydli et al. (2019), Wang and Yang (2021) and DellaVigna and Linos (2022), among others, in the sense that scaling up a successful local policy experimentation might not work out. Generally speaking, many economic policies are not one-size-fit-all so that the central government should be cautious about the spread of these policies as a result of shuffling leaders. Ideally, each provincial leader should be allocated to the province where her preferred policies are the best fit to maximize the welfare gains of leadership rotations, though this is very difficult to maneuver in practice.

In addition, it deserves mention that, as a side effect, leadership rotations have also shortened the average term of each provincial leader. As a result, these leaders might care more about short-term rather than long-term economic development within their jurisdictions (Chien, 2008; Jensen and Malesky, 2018). This tendency could create uncertainty about local economic policies and force private businesses to frequently cultivate new connections with newcomers (Zhu and Zhang, 2017). That being said, future studies might want to provide more holistic evaluations of advantages and disadvantages of China's leadership rotations.

Notes

1. See also Qian and Xu (1993), Qian et al. (1999, 2006), Lin and Liu (2000), Jin et al. (2005), Qiao et al. (2008), Han and Kung (2015) and Gong et al. (2021).

2. See also Jia and Liang (2012).

3. Wang Menghui had been the number-one leader in Yunfu, Xiamen and Shenyang, respectively. He is now the number-one leader of Hubei. Li Qiang had been the number-two leader in Zhejiang and

the number-one leader in Jiangsu. Then, he became the number-one leader of Shanghai in 2017. He is now a member of the Politburo Standing Committee (PSC).

4. See Fan et al. (2009), Birney (2014), Che et al. (2017) and Bai et al. (2020) for how economic decentralization could lead to corruption. See Young (2000), Poncet (2003, 2005) and Bai et al. (2004) for more about China's local protectionism.

5. One evidence for the willingness to relocate is that Kanbur and Zhang (1999) found that, given the difficulty of inter-provincial migration, many people move to the suboptimal communities within their provinces when they are allowed to do so.

6. Although China has been transforming into a market economy since the 1980s, it still has economic guidelines. The long-term guidelines are, for example, the five-year plans, and the short-term ones are the economic targets announced in the government work reports.

7. Chen (2009), Lei and Nugent (2018), Jiang et al. (2019), Li et al. (2019), and Jiang and Zhang (2020), among others, also used government work reports to track policy priorities.

8. One plausible reason for this ratio, on average, to be greater than one is that growth target could be an interval rather than an exact number in some government work reports, and, following Xu and Liang (2014), I take the lower bound of the interval as the growth target in this case.

9. Li et al. (2019) found some similar results for the 1997-2014 period. See Li et al. (2019) for more discussion about the importance of government work reports.

10. Unlike English, there is no space between successive characters and words in Chinese. "jieba" is one of the most popular Python packages for segmenting Chinese sentences into lists of phrases. As mentioned in the appendix of Jiang and Zhang (2020), "jieba" is faster than other packages, and special terms, such as "Three Represents" and "Deng Xiaoping", could be correctly identified as a single phrase.

11. These "stopwords" are from the dictionaries created by Baidu, Harbin Institute of Technology and Sichuan University, respectively.

12. Stemming the text is not needed because there is no inflection in Chinese.

13. Among the 465 provincial government work reports, 60 are from the four DCMs, and they are excluded from all regressions in this article.

14. The official term is 5 years, but it is not compulsory.

15. Beijing is a direct-controlled municipality (DCM) at the province level, so its mayor is at least equivalent to a governor. Given the special status of Beijing as the capital city of China, its mayor is usually superior to any leadership positions in other provinces.

16. The determinants of whether a provincial leader is selected to be promoted or rotated are also discussed towards the end of Section 4.

17. Even so, this type of cross-province movement among the officials affiliated with the research offices has to be approved by the central government. See more about this in Appendix D.

18. It is undeniable that this restrictive model might overlook some important aspects of the government work reports. For instance, this model takes for granted that the propensity to use a given phrase is unrelated to other phrases. Nevertheless, I still choose to use this model because of its tractability and also its usefulness proven by many previous studies (e.g. Groseclose and Milyo, 2005; Gentzkow et al., 2019b).

19. See Appendix C for further explanation about why these three particular reports are chosen as the example and why the paragraphs are restricted to be related to employment.

20. $E[\widehat{D}_{p,q}] = D_{p,q}$ only if $\widehat{\boldsymbol{\theta}}_p = \boldsymbol{\theta}_q$, i.e. $\text{Var}(\widehat{\boldsymbol{\theta}}_p) = 0$. This would be a very strong assumption.

21. By definition, $\pi_p = \pi_{f(p),g(p)}$ and $\pi_q = \pi_{f(q),g(q)}$.

22. In this sentence, "previous" means at least one year before but no earlier than 2005.

23. Xu et al. (2007), Zhang and Gao (2007), Bai et al. (2008) and Persson and Zhuravskaya (2016), among others, showed that a leadership exchange could boost the destination province's economic growth and reduce its local protectionism at the expense of the decline in the province's govern-

ment spending on healthcare and education. Hence, including the fixed effect π_p could prevent my estimation results from being contaminated by these effects as well.

24. For example, the predecessors in the destination provinces and the successors in the origin provinces might impose some policies that are very different from those chosen by other provincial leaders. Then, the negative coefficient estimate of β would be at least partially driven by the uniqueness of these policies if the fixed effects $\pi_{f(p),g(q)}$ and $\pi_{g(p),f(q)}$ are not included. Controlling for these two fixed effects should alleviate this concern.

25. In other words, the movements of provincial leaders still have to be orthogonal to the *ex ante* time-varying socioeconomic interactions between the origin and destination provinces, where the fixed effects in equation (3) could only address (i) the time-varying socioeconomic conditions in each province and (ii) the time-invariant socioeconomic interactions between provinces.

26. This model also resembles Yao and Zhang (2015). I explain this model in more detail in Appendix D.

27. In other words, PC_p is the linear combination of the elements in report p's vector of phrase counts $\mathbf{c}_p$ with the largest variance.

28. Intuitively, the lags of event A and the leads of event B are interchangeable, so I only use the former.

29. The definitions for the two following dummy variables deserves some further explanations: $A_{p-2,q-2}$ indicates whether there exists some $\kappa \geq 2$ such that year $g(p)-\kappa$ is the first year when the incumbent party secretary or governor of province $f(p)$ was the same as that of province $f(q)$ in year $g(q)-\kappa$, and $B_{p-2,q-2}$ indicates whether there exists some $\kappa \geq 2$ such that year $g(p)-1-\kappa$ is the last year when the party secretary or the governor of province $f(p)$ was the same as that of province $f(q)$ in year $g(q)-\kappa$.

30. Guo Shuqing currently serves as the chairman of the China Banking and Insurance Regulatory Commission.

31. Wang and Yang (2021) also showed that Chinese politicians associated with higher promotion probabilities have been more interested in differentiating their policies from others so as to win China's

performance-based promotion tournament.

32. The model formulated by equation (3) could also use three-dimensional fixed effects $\pi_{p,g(q)}$, $\pi_{q,g(p)}$, $\pi_{p,f(q)}$ and $\pi_{q,f(p)}$, where p and q are government work reports that correspond to two-dimensional province-year combinations. However, these more restrictive fixed effects would absorb the dependent variable's variation too much, so I choose to only use the two-dimensional fixed effects specified in equation (3) throughout this article. I discuss this issue in more detail in Appendix D.

33. For example, the rotation of Wang Rulin from Jilin to Shanxi in 2014 was intended to solve the latter province's grim corruption problem (Zhai and Huang, 2014). This type of rotation seems to be less exogenous because the goal is to make changes to the destination provinces.

34. The mean of these placebo coefficient estimates is 0.016.

35. The correlation coefficient between this re-weighted measure and $\widehat{D}_{p,q}$ is 0.668.

36. $\widehat{D}_{p,q}^{(cos)} = 1$ if two reports use the exact same set of phrases in the same proportion. $\widehat{D}_{p,q}^{(cos)} = 0$ if two reports have no overlapping phrases. The correlation coefficient between $\widehat{D}_{p,q}^{(cos)}$ and $\widehat{D}_{p,q}$ is -0.910.

37. For instance, most government work reports have a bullet-pointed section listing the policies planned to be implemented in the coming year, and the first several bullet points are usually the top priorities.

38. See Blei (2012) for a high-level overview. See also Manning and Masella (2021) for an application of the LDA model.

39. In other words, there has been a relatively small within-topic variation among the government work reports, so how a pair of reports talk about a given topic is similar anyway regardless of leadership rotations. In contrast, these provincial leaders have more discretion over between-topic variation, so the between-topic estimates are negative and statistically significant.

40. See Qian and Xu (1993) and Qian et al. (1999, 2006) for how the literature on U-form and M-form organizations relates to China's political economy.

41. 2015 and 2016 are the pre-treatment years in this context because the provincial government work reports in 2016 were delivered before the environmental reforms.

42. Taking all the scripts of these news articles into account would be quite computationally inefficient, so I choose to only focus on the headlines.

43. I choose not to analyze the composition of output because much of it is usually out of these provincial leaders' control.

44. This is reminiscent of the principle of "All Action, No Talk" adopted by many Chinese politicians in history.

45. Zhu and Zhang (2017) and Hou and Li (2021) also showed that too many leadership exchanges might also force private businesses to frequently cultivate new connections with new leaders, which would be costly.

46. See Li and Zhou (2005) and Li et al. (2019), among others.

47. This pattern is also supported by the "office-by-province" fixed effects used below. See Appendix E for more information.

48. According to Jia et al. (2015), promotion for governors means being appointed to be a party secretary of the same or a different province, and that for party secretaries means entering the Politburo or becoming a State Councilor.

49. For example, Lu Xinshe moved from Jiangxi's party secretary, with μ_{oj} being 0.157, to Guangxi's counterpart, with μ_{oj} being -0.006, in 2018, and many observers indeed view this movement as a *de facto* demotion.

50. For example, Li Jianguo moved from Shaanxi's party secretary, with μ_{oj} being -0.011, to Shandong's counterpart, with μ_{oj} being 0.273, in 2007, and he indeed entered the Politburo in 2012.

51. Just to be clear, the common party secretary dummy indicates whether report p's province $f(p)$ in report p's year $g(p)$ and report q's province $f(q)$ in report q's year $g(q)$ shared the same party secretary, and the common governor dummy indicates whether report p's province $f(p)$ in report p's year $g(p)$ and report q's province $f(q)$ in report q's year $g(q)$ shared the same governor.

52. One reason is that the shuffled leaders' destination provinces could be very dissimilar to the origin

provinces, and either the shuffled leaders or their successors are only interested in continuing the subset of the policies that are suitable for their jurisdictions.

53. For example, following Gentzkow and Shapiro (2010), one might want to consider n-gram modeling, which is not used in this article because my sample is already very large and counting n-grams would substantially increase computational complexity.

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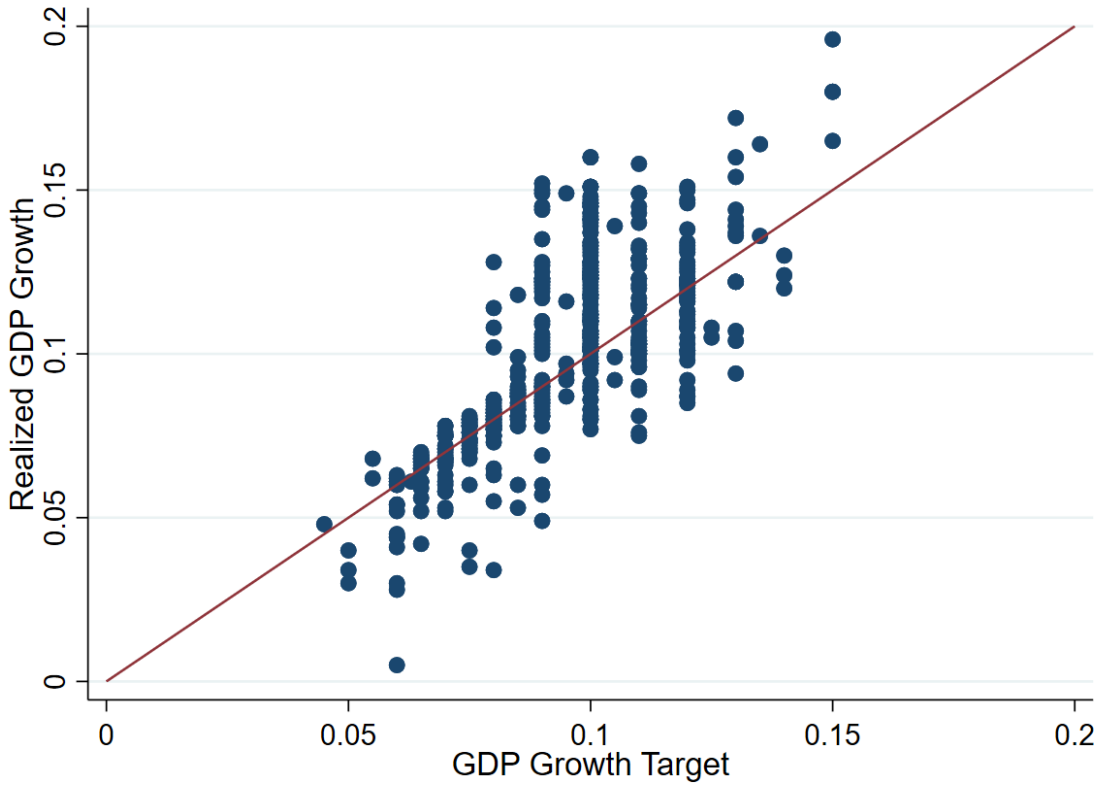
Figure 1: Chord Diagram of Provincial Leadership Rotations (2005-2019)

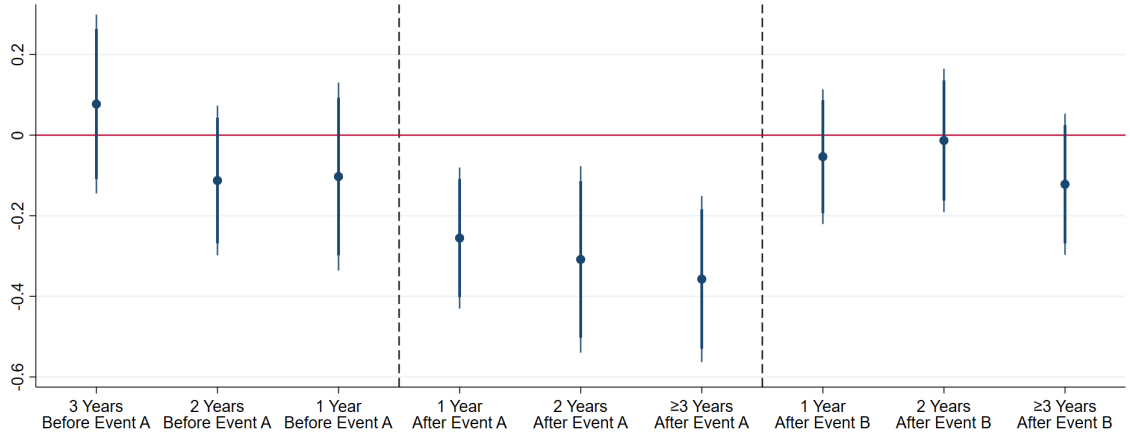
Notes: All four types of provincial leadership rotations (i.e. from governor to governor, from party secretary to party secretary, from governor to party secretary, and from party secretary to governor) are presented above. Each arrow indicates the direction of a movement. Hainan does not appear in the figure because it did not have any leadership exchange with other provinces between 2005 and 2019.

Figure 2: GDP Growth Targets vs. Realized GDP Growth



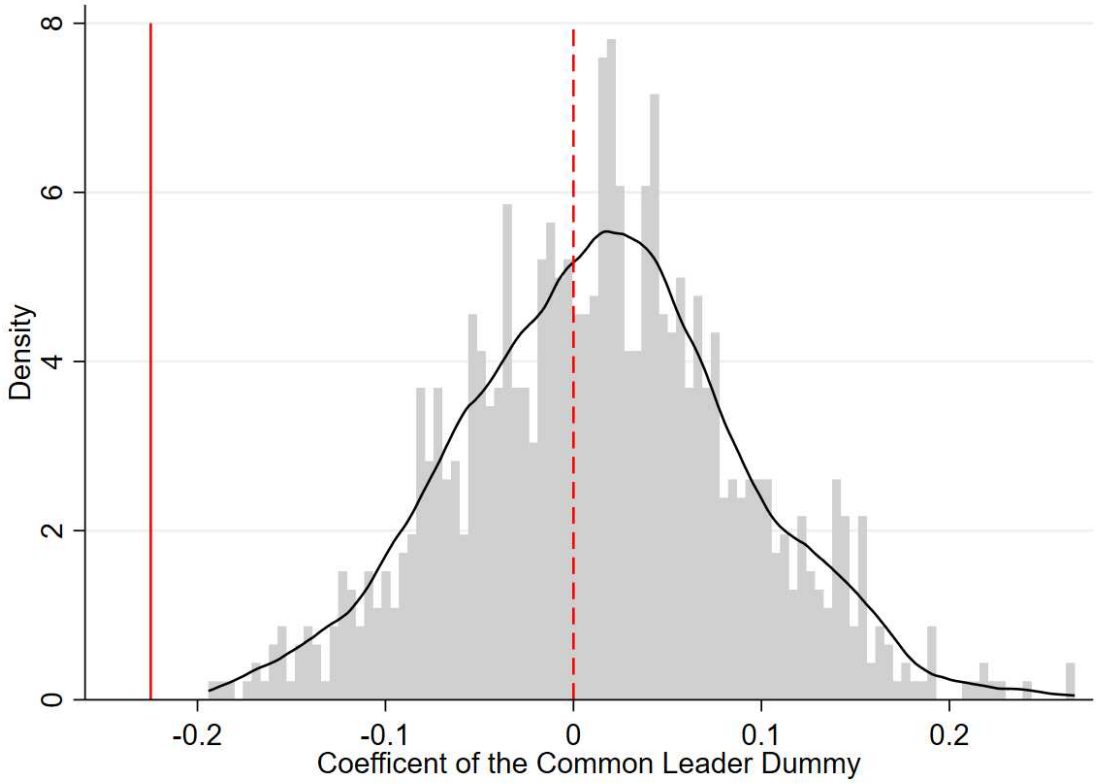
Notes: The horizontal axis is provincial GDP growth target, and the vertical axis is provincial GDP growth rate. The red line is the diagonal line.

Figure 3: Event Study Estimates



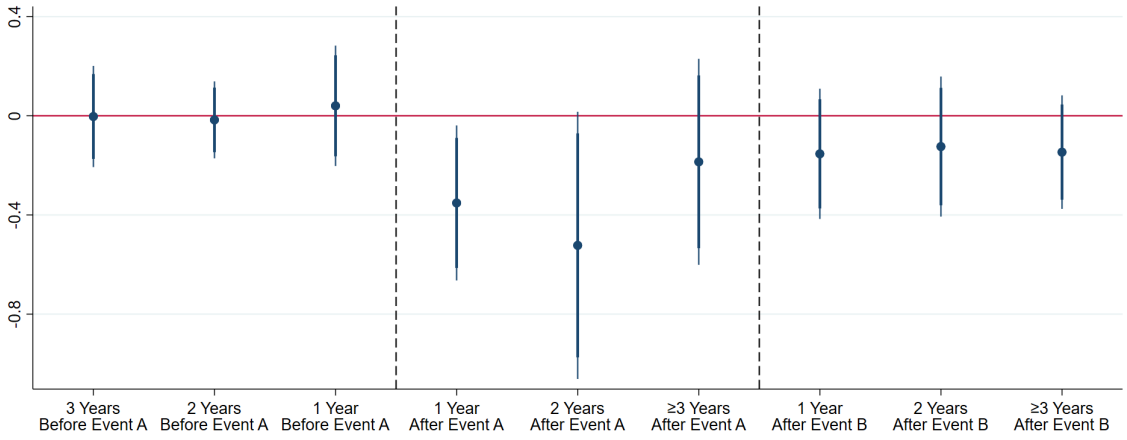
Notes: Event A means two provinces get "linked" (i.e. a new provincial leader with past work experience in report q's province is appointed in report p's province), and event B means two provinces get "delinked" (i.e. a provincial leader in report p's province with past work experience in report q's province is replaced by someone else without such experience). The dependent variable is the economic policy distance $\widehat{D}_{p,q}$ calculated by measuring the textual difference between provincial government work reports. Each dot indicates the coefficient estimate that should be interpreted as how many standard deviations the dependent variable would change as the independent variable increases from 0 to 1. The vertical bars show the 90% and 95% confidence intervals.

Figure 4: Random Permutations of Provincial Leaders' Tenures

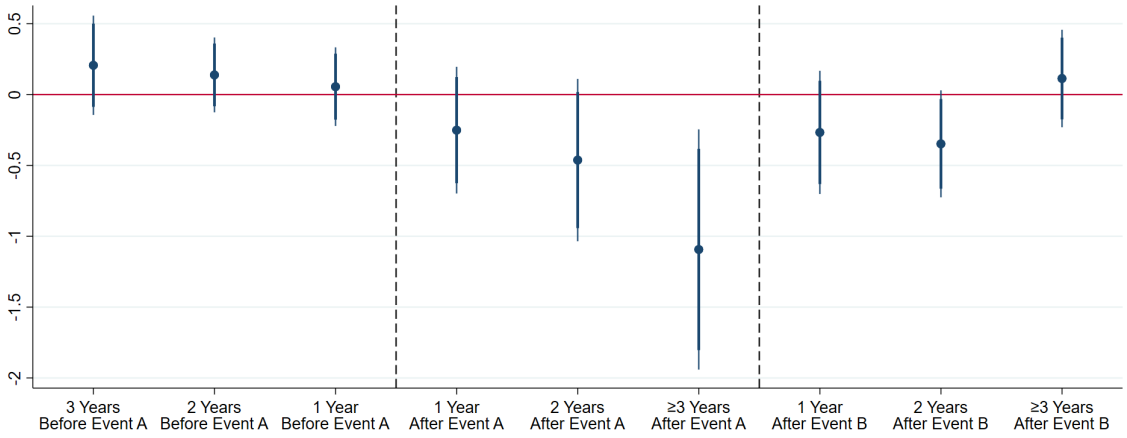


Notes: The grey bars represent the histogram of the coefficient estimates of the common leader dummy when leaders' tenures are randomly permuted across years within their corresponding provinces, and the black curve represents the Epanechnikov kernel density with bandwidth being 0.016. 1000 random permutations are conducted for this figure. The red vertical solid line is the baseline estimator of the coefficient of the common leader dummy presented in column (3) of Table 1. The red vertical dashed line represents zero.

Figure 5: Event Study Estimates for Policy Outcomes



(a) Difference in News Headlines



(b) Difference in Fiscal Regime

Notes: Event A means two provinces get "linked" (i.e. a new provincial leader with past work experience in province $f(q)$ is appointed in province $f(p)$), and event B means two provinces get "delinked" (i.e. a provincial leader in province $f(p)$ with past work experience in province $f(q)$ is replaced by someone else without such experience). The dependent variable in subfigure (a) is the difference between province $f(p)$ in year $g(p)$ and province $f(q)$ in year $g(q)$ in terms of the relative frequencies of different topics in the headlines of the news articles reporting provincial leaders' activities. The dependent variable in subfigure (b) is the difference between province $f(p)$'s composition of government expenditure in year $g(p)$ and province $f(q)$'s composition of government expenditure in year $g(q)$. Each dot indicates the coefficient estimate that should be interpreted as how many standard deviations the dependent variable would change as the independent variable increases from 0 to 1. The vertical bars show the 90% and 95% confidence intervals.

Table 1: Baseline Estimation Results

Dependent Variable	(1) $\widehat{D}_{p,q}$	(2) $\widehat{D}_{p,q}$	(3) $\widehat{D}_{p,q}$
Common Leader Dummy $L_{p,q}$	-0.325***	-0.237**	-0.225***
	(0.096)	(0.104)	(0.069)
New Report p FE		✓	✓
Old Report q FE		✓	✓
New Report p's Province × Old Report q's Province FE			✓
New Report p's Year × Old Report q's Year FE			✓
New Report p's Province × Old Report q's Year FE			✓
New Report p's Year × Old Report q's Province FE			✓
Number of Observations	73710	73710	73710
Dependent Variable	(4) $\Delta_1\widehat{D}_{p,q}$	(5) $\Delta_2\widehat{D}_{p,q}$	(6) $\Delta_3\widehat{D}_{p,q}$
One-Year Difference $\Delta_1 L_{p,q}$	-0.175**		
	(0.076)		
Two-Year Difference $\Delta_2 L_{p,q}$		-0.159***	
		(0.055)	
Three-Year Difference $\Delta_3 L_{p,q}$			-0.162*
			(0.088)
New Report p FE	✓	✓	✓
Old Report q FE	✓	✓	✓
New Report p's Province × Old Report q's Province FE	✓	✓	✓
New Report p's Year × Old Report q's Year FE	✓	✓	✓
New Report p's Province × Old Report q's Year FE	✓	✓	✓
New Report p's Year × Old Report q's Province FE	✓	✓	✓
Number of Observations	63882	54756	19656

Notes: Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the new report level (i.e. p) and the old report level (i.e. q). Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each government work report, p or q, corresponds to a unique province-year combination. The dependent variable in columns (1) through (3) is the economic policy distance $\widehat{D}_{p,q}$ calculated by measuring the textual difference between provincial government work reports p and q. The independent variable in columns (1) through (3) is the common leader dummy $L_{p,q}$. The dependent and independent variables in column (4) are the variables' one-year differences, those in column (5) are their two-year differences, and those in column (6) are their three-year differences. Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1.

Table 2: Event Study Estimates

Dependent Variable	(1) $\widehat{D}_{p,q}$
Before Event A	
Before Linking Report p's and Report q's Provinces	
$A_{p+3,q+3}$ (3 years before an event A)	0.077
	(0.113)
$A_{p+2,q+2}$ (2 years before an event A)	-0.113
	(0.094)
$A_{p+1,q+1}$ (1 year before an event A)	-0.103
	(0.119)
After Event A & Before Event B	
After Linking Report p's and Report q's Provinces	
Before Delinking Report p's and Report q's Provinces	
$A_{p,q}$ (the year when an event A occurs)	-0.255***
	(0.089)
$A_{p-1,q-1}$ (1 year after an event A)	-0.308***
	(0.118)
$A_{p-2,q-2}$ (2+ years after an event A)	-0.357***
	(0.105)
After Event B	
After Delinking Report p's and Report q's Provinces	
$B_{p,q}$ (the year when an event B occurs)	-0.053
	(0.085)
$B_{p-1,q-1}$ (1 year after an event B)	-0.013
	(0.091)
$B_{p-2,q-2}$ (2+ years after an event B)	-0.122
	(0.089)
p-value of the joint F-test for $H_0 : \delta^A_{-3} = \delta^A_{-2} = \delta^A_{-1} = 0$	0.458
p-value of the joint F-test for $H_0 : \delta^A_0 = \delta^A_1 = \delta^A_2 = 0$	0.001
p-value of the joint F-test for $H_0 : \delta^B_0 = \delta^B_1 = \delta^B_2 = 0$	0.568
Number of Observations	73710

Notes: Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the new report level (i.e. p) and the old report level (i.e. q). Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each government work report, p or q, corresponds to a unique province-year combination. The dependent variable is the economic policy distance $\widehat{D}_{p,q}$ calculated by measuring the textual difference between provincial government work reports p and q. $p + \tau$ refers to report p's province's government work report delivered τ years after report p. $A_{p,q}$ indicates if report p is the first report after an event A (i.e. a new provincial leader with past work experience in report q's province is appointed in report p's province). $B_{p,q}$ indicates if report p is the first report after an event B (i.e. a provincial leader in report p's province with past work experience in report q's province is replaced by someone else without such experience). Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1. The six fixed effects used in the preferred specification, column (3) of Table 1, are controlled as specified in equation (3).

Table 3: Robustness Check

	(1)	(2)	(3)
Dependent Variable	$\widehat{D}_{p,q}$	$\widehat{D}_{p,q}$	$\widehat{D}_{p,q}$
Common Leader Dummy $L_{p,q}$	-0.225***	-0.238***	
	(0.069)	(0.092)	
First Lag $L_{p+1,q+1}$		0.007	
		(0.090)	
First Lead $L_{p-1,q-1}$		0.005	
		(0.083)	
Common Party Secretary Dummy $L^{(S)}_{p,q}$			-0.244***
			(0.093)
Alternative Clustered Standard Errors	✓		
Number of Observations	73710	54756	73710

	(4)	(5)	(6)
Dependent Variable	$\widehat{D}_{p,q}$	$\widehat{D}_{p,q}$	$\widehat{D}_{p,q}$
Common Leader Dummy $L_{p,q}$	-0.204***	-0.329***	
	(0.073)	(0.116)	
Common Leader Dummy $L^{(D)}_{p,q}$			-0.219**
(for Less Exogenous Rotations)			(0.088)
Common Leader Dummy $L^{(X)}_{p,q}$			-0.233**
(for More Exogenous Rotations)			(0.110)
No Prestigious Provinces	✓		
Distance Between Provinces $\geq$ 1500 KM		✓	
Number of Observations	63000	29610	73710

Notes: Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the new report level (i.e. p) and the old report level (i.e. q). The exception is column (1), where standard errors are clustered at both the province pair level (i.e. new report p's province times old report q's province) and the year pair level (i.e. new report p's year times old report q's year). Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each government work report, p or q, corresponds to a unique province-year combination. The dependent variable is the economic policy distance $\widehat{D}_{p,q}$ calculated by measuring the textual difference between provincial government work reports p and q. The independent variable in columns (1), (4) and (5) is the common leader dummy $L_{p,q}$. $p+\tau$ in column (2) refers to report p's province's government work report delivered τ years after report p. The common party secretary dummy $L^{(S)}_{p,q}$ in column (3) is the same as the common leader dummy $L_{p,q}$ except that it only takes the party secretaries corresponding to reports p and q into account. The prestigious provinces excluded in column (4) are Guangdong and Xinjiang. In column (6), $L^{(D)}_{p,q}$ is the common leader dummy for the leadership rotations whose destination provinces are less exogenous, $L^{(X)}_{p,q}$ is the common leader dummy for the more exogenous ones, and $L^{(D)}_{p,q} + L^{(X)}_{p,q} = L_{p,q}$. Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1. The six fixed effects used in the preferred specification, column (3) of Table 1, are controlled as specified in equation (3).

Table 4: Alternative Economic Policy Distance Measures

Dependent Variable	(1) $\widehat{D}_{p,q}^{(idf)}$	(2) $\widehat{D}_{p,q}^{(cos)}$	(3) $\widehat{V}_{p,q}$	(4) $\widehat{V}_{p,q}$
Common Leader Dummy $L_{p,q}$	-0.314***	0.264***	-0.241*	-0.035**
	(0.080)	(0.073)	(0.131)	(0.017)
Control for Economic Policy Distance $\widehat{D}_{p,q}$				✓
Control for Document Length Difference $\widehat{Q}_{p,q}$				✓
Number of Observations	73710	73710	73710	73710

Notes: Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the new report level (i.e. p) and the old report level (i.e. q). Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each government work report, p or q, corresponds to a unique province-year combination. In column (1), the dependent variable is $\widehat{D}_{p,q}^{(idf)}$, the economic policy distance re-weighted by the inverse document frequency. In column (2), the dependent variable is $\widehat{D}_{p,q}^{(cos)}$, the cosine similarity score between provincial government work reports p and q. In columns (3) and (4), the dependent variable is the Levenshtein distance $\widehat{V}_{p,q}$ between provincial government work reports p and q. The independent variable is the common leader dummy $L_{p,q}$. Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1. The six fixed effects used in the preferred specification, column (3) of Table 1, are controlled as specified in equation (3).

Table 5: Policy Isomorphism Between Topics

	(1)	(2)	(3)
Dependent Variable	$\widehat{LDA}_{p,q}$	$\widehat{LDA}_{p,q}$	$\widehat{LDA}_{p,q}$
Common Leader Dummy $L_{p,q}$	-0.192**	-0.172**	-0.175**
	(0.087)	(0.078)	(0.086)
Pre-Specified Number of Topics	30	35	40
Number of Observations	73710	73710	73710

Notes: Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the new report level (i.e. p) and the old report level (i.e. q). Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each government work report, p or q, corresponds to a unique province-year combination. The dependent variable is $\widehat{LDA}_{p,q}$, the economic policy distance calculated by measuring the difference between the relative frequencies of different topics in provincial government work reports p and q in terms of the squared Euclidean norm, where all topics are automatically generated by the LDA algorithm given the number of pre-specified topics. The independent variable is the common leader dummy $L_{p,q}$. Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1. The six fixed effects used in the preferred specification, column (3) of Table 1, are controlled as specified in equation (3).

Table 6: Policy Isomorphism Within Topics

Pre-Specified Number of Topics $R = 30$			
Topic ID	Topic Name	Coefficient of $L_{p,q}$	Standard Error
1	Socioeconomic Development	-0.132**	(0.065)
2	Education	-0.094	(0.073)
3	Service Sector	-0.115	(0.073)
4	Labor Economics	-0.005	(0.076)
5	Social Stability	-0.091	(0.077)
6	Rural Development	0.004	(0.071)
7	Industrial Policies	-0.091	(0.068)
8	Gratitude to the Audience	-0.121*	(0.069)
9	Cultural Industries	-0.089	(0.071)
10	Business Risk Management	-0.047	(0.062)
11	Government Effectiveness	-0.113*	(0.068)
12	Economic Challenges	-0.129*	(0.070)
13	Business Environment	-0.126*	(0.071)
14	Agriculture	-0.075	(0.078)
15	Finance	-0.078	(0.059)
16	Infrastructure	-0.017	(0.076)
17	Pollution Reduction	-0.070	(0.073)
18	Economic Plans	-0.100	(0.073)
19	Ecology	0.015	(0.086)
20	Regional Development	-0.088	(0.071)
21	International Cooperation	-0.059	(0.079)
22	Quality of Life	-0.103	(0.070)
23	Higher Level Political Guidance	-0.106	(0.073)
24	Economic Opportunities	-0.101	(0.074)
25	Economic Indicators	-0.169*	(0.088)
26	Reforms	-0.030	(0.073)
27	Innovation	-0.054	(0.071)
28	Government Transparency	-0.091	(0.067)
29	Poverty Alleviation	0.082	(0.064)
30	International Trade and Investment	-0.125*	(0.064)

Notes: The number of observations is 73,710. The number of pre-specified topics is 30. Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the new report level (i.e. p) and the old report level (i.e. q). Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each government work report, p or q, corresponds to a unique province-year combination. The dependent variable is the economic policy distance reweighted by each phrase's relative importance for each of the 30 topics. The independent variable is the common leader dummy $L_{p,q}$. Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1. The six fixed effects used in the preferred specification, column (3) of Table 1, are controlled as specified in equation (3).

Table 7: Effects on Policy Outcomes

Dependent Variable	(1) Difference in News Headlines	(2) Difference in Fiscal Regime
Common Leader Dummy $L_{p,q}$	-0.312***	-0.406**
	(0.120)	(0.187)
FE for Province $f(p)$ times Year $g(p)$	✓	✓
FE for Province $f(q)$ times Year $g(q)$	✓	✓
FE for Province $f(p)$ times Province $f(q)$	✓	✓
FE for Year $g(p)$ times Year $g(q)$	✓	✓
FE for Province $f(p)$ times Year $g(q)$	✓	✓
FE for Year $g(p)$ times Province $f(q)$	✓	✓
Number of Observations	54756	19656

Notes: Each p or q corresponds to a unique province-year combination, where $f(p)$ and $f(q)$ are p's and q's provinces and $g(p)$ and $g(q)$ are p's and q's years. Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the level of p (i.e. $f(p)$-by-$g(p)$ level) and the level of q (i.e. $f(q)$-by-$g(q)$ level). Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable in column (1) is the difference between province $f(p)$ in year $g(p)$ and province $f(q)$ in year $g(q)$ in terms of the relative frequencies of different topics in the headlines of the news articles reporting provincial leaders' activities. The dependent variable in column (2) is the difference between province $f(p)$'s composition of government expenditure in year $g(p)$ and province $f(q)$'s composition of government expenditure in year $g(q)$. The independent variable is the common leader dummy $L_{p,q}$. Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1. The six two-dimensional fixed effects are analogue to those controlled in the preferred specification, column (3) of Table 1.

Table 8: Event Study Estimates for Policy Outcomes

Dependent Variable	(1) Difference in News Headlines	(2) Difference in Fiscal Regime
Before Event A		
Before Linking Provinces $f(p)$ and $f(q)$		
$A_{p+3,q+3}$ (3 years before an event A)	-0.003	0.207
	(0.104)	(0.178)
$A_{p+2,q+2}$ (2 years before an event A)	-0.017	0.139
	(0.079)	(0.134)
$A_{p+1,q+1}$ (1 year before an event A)	0.040	0.056
	(0.123)	(0.141)
After Event A & Before Event B		
After Linking Provinces $f(p)$ and $f(q)$		
Before Delinking Provinces $f(p)$ and $f(q)$		
$A_{p,q}$ (the year when an event A occurs)	-0.352**	-0.251
	(0.159)	(0.227)
$A_{p-1,q-1}$ (1 year after an event A)	-0.523*	-0.462
	(0.274)	(0.290)
$A_{p-2,q-2}$ (2+ years after an event A)	-0.186	-1.093**
	(0.211)	(0.430)
After Event B		
After Delinking Provinces $f(p)$ and $f(q)$		
$B_{p,q}$ (the year when an event B occurs)	-0.154	-0.267
	(0.133)	(0.220)
$B_{p-1,q-1}$ (1 year after an event B)	-0.124	-0.348*
	(0.144)	(0.191)
$B_{p-2,q-2}$ (2+ years after an event B)	-0.147	0.113
	(0.116)	(0.174)
p-value of the joint F-test for $H_0 : \delta^A_{-3} = \delta^A_{-2} = \delta^A_{-1} = 0$	0.975	0.634
p-value of the joint F-test for $H_0 : \delta^A_0 = \delta^A_1 = \delta^A_2 = 0$	0.098	0.043
p-value of the joint F-test for $H_0 : \delta^B_0 = \delta^B_1 = \delta^B_2 = 0$	0.575	0.079
Number of Observations	54756	19656

Notes: Each p or q corresponds to a unique province-year combination, where $f(p)$ and $f(q)$ are p's and q's provinces and $g(p)$ and $g(q)$ are p's and q's years. Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the level of p (i.e. $f(p)$-by-$g(p)$ level) and the level of q (i.e. $f(q)$-by-$g(q)$ level). Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variables are the same as those in Table 7. $p + \tau$ refers to the combination of province $f(p)$ and year $q(p) + \tau$. $A_{p,q}$ indicates if year $g(p)$ is the first year after an event A (i.e. a new provincial leader with past work experience in province $f(q)$ is appointed in province $f(p)$). $B_{p,q}$ indicates if year $g(p)$ is the first year after an event B (i.e. a provincial leader in province $f(p)$ with past work experience in province $f(q)$ is replaced by someone else without such experience). Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1. Fixed effects are controlled as in Table 7.

Table 9: Provincial Leadership Rotation and Further Promotion

	Provincial Leaders	
Year: 1992-2019	Shuffled	Unshuffled
Total	75	207
Those Who Became Politburo Members	28	7
Those Who Became PSC Members	14	1

Notes: Those who had only been leaders of the direct-controlled municipalities are excluded. PSC is the abbreviation for the Politburo Standing Committee.

Table 10: Moving to "Better" Leadership Positions

Dependent Variable	(1) $\widehat{D}_{p,q}$	(2) $\widehat{D}_{p,q}$
Common Leader Dummy $L_{p,q}$ × Inland-to-Inland	-0.201**	
	(0.093)	
Common Leader Dummy $L_{p,q}$ × Inland-to-Coast	-0.371**	
	(0.179)	
Common Party Secretary Dummy $L^{(S)}_{p,q}$ × Worse Destination		-0.011
		(0.243)
Common Party Secretary Dummy $L^{(S)}_{p,q}$ × Better Destination		-0.374***
		(0.102)
Common Governor Dummy $L^{(G)}_{p,q}$ × Worse Destination		-0.447**
		(0.180)
Common Governor Dummy $L^{(G)}_{p,q}$ × Better Destination		-0.967***
		(0.241)
Number of Observations	73710	73710

Notes: Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the new report level (i.e. p) and the old report level (i.e. q). Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each government work report, p or q, corresponds to a unique province-year combination. The dependent variable is the economic policy distance $\widehat{D}_{p,q}$ calculated by measuring the textual difference between provincial government work reports p and q. $L_{p,q}$ is the common leader dummy. The common party secretary dummy $L^{(S)}_{p,q}$ is the same as the common leader dummy $L_{p,q}$ except that it only takes the party secretaries corresponding to reports p and q into account, and the common governor dummy $L^{(G)}_{p,q}$ is the same as the common leader dummy $L_{p,q}$ except that it only takes the governors corresponding to reports p and q into account. "Inland-to-Inland" indicates if a provincial leadership rotation is between inland provinces. "Inland-to-Coast" indicates if a provincial leadership rotation is from an inland province to a coastal one. "Better Destination" and "Worst Destination" are defined based on Jia et al. (2015)'s "office-by-province" fixed effects, which are also reported in Table D5 of Appendix D. Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1. The six fixed effects used in the preferred specification, column (3) of Table 1, are controlled as specified in equation (3).

Online Appendices

Leadership Rotations and the Convergence of Subnational Economic Policies in China: Evidence from Provincial Government Work Reports

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Appendix A

As briefly discussed in Section 2, there exists an almost perfect correlation between the GDP growth targets announced in provincial government work reports and the actual growth rates over the 2005-2019 period. This appendix formalizes this analysis and also extends it to five other economic targets announced in these reports.

Consider the simple regression model as follows:

$$y_{jt} = \xi y_{jt}^{T} + \nu_{jt}$$

where y_{jt} is the realized value of an outcome variable corresponding to province j in year t, y_{jt}^{T} is the target of that outcome variable corresponding to province j in year t announced in the government work reports, and ν_{jt} is the idiosyncratic error term. If these economic targets are indeed influential to China's subnational economic development, then $E[y_{jt}] = y_{jt}^{T}$, which implies that ξ should be estimated to be statistically significant and also close to one.

The estimation results based on the equation above are presented in Table A1. Column (1) reports the results for provincial GDP growth rates over the 2005-2019 period. Then, columns (2) through (6) report the results for government revenue growth rates, investment growth rates, retail sales growth rates, unemployment rates and consumer price indexes, respectively. Since announcing these economic targets are not mandatory, the numbers of observations in Table A1 are all smaller than 465 (i.e. 31 provinces $\times$ 15 years). Some provincial government work reports also announce some other economic targets, but, they are not examined in this appendix because of the large amounts of missing values.

As shown in the table, all the coefficient estimates are statistically significant

and close to one, which confirms that provincial government work reports are indicative of provincial economic policies. In terms of the magnitudes of these coefficients, the first four are shown to be greater than one while the last two are shown to be smaller than one. One plausible reason is that, as mentioned in Section 2, the target corresponding to each of the first four outcome variables could be an interval rather than an exact number, and, following Xu and Liang (2014), I take the lower bound of the interval. Another reason is that economic well-being is positively associated with the first four variables but negatively associated with the last two. Provincial governments might prefer to announce slightly lower targets for the first four variables and slightly higher targets for the last two variables to reduce the possibility that they fail to achieve the targets, which would be politically embarrassing.

Table A1: Targets Announced in Provincial Government Work Reports

	(1)	(2)
Dependent Variable	GDP Growth	Government Revenue Growth
Target of the Dependent Variable	1.058***	1.584***
	(0.016)	(0.029)
Number of Observations	462	433
R^2	0.965	0.805
	(3)	(4)
Dependent Variable	Investment Growth	Retail Sales Growth
Target of the Dependent Variable	1.091***	1.119***
	(0.040)	(0.038)
Number of Observations	290	315
R^2	0.788	0.792
	(5)	(6)
Dependent Variable	Unemployment Rate	Consumer Price Index
Target of the Dependent Variable	0.825***	0.721***
	(0.013)	(0.012)
Number of Observations	399	388
R^2	0.982	0.700

Notes: Standard errors reported in parentheses are block-bootstrapped at the province level. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. For each column, the dependent variable is the realized value of an economic indicator, and the independent variable is the target of that indicator announced in the government work reports.

Appendix B

A key assumption for the validity of interpreting the regression results based on equation (3) as causal evidence is that the positive bias $E[\widehat{D}_{p,q}] - D_{p,q}$ (due to Jensen's inequality) should be removed by the fixed effects π_p and π_q so that the within estimator $\widehat{\beta}$ of the coefficient β in equation (3) would be unbiased. This appendix shows that this assumption is true as long as $\text{Cov}(\widehat{\theta}_{p,k} - \theta_{p,k}, \widehat{\theta}_{q,k} - \theta_{q,k}) = 0$, i.e. the measurement errors for different province-year combinations are not correlated, for any phrase k.

Denote $\widehat{u}_{p,k} = \widehat{\theta}_{p,k} - \theta_{p,k}$. For each $k \in \{1, 2, \cdots, K\}$, $E[\widehat{u}_{p,k}] = 0$ because $E[\widehat{\boldsymbol{\theta}}_p] = \boldsymbol{\theta}_p$. As the measurement errors for different province-year combinations are not correlated for all phrases, $\text{Cov}(\widehat{u}_{p,k}, \widehat{u}_{q,k}) = 0$. Then, $E[\widehat{D}_{p,q}] - D_{p,q}$ could be expressed as follows:

$$
\begin{aligned}
E[\widehat{D}_{p,q}] - D_{p,q} &= E\left[\sum_{k=1}^{K} w_k \left(\widehat{\theta}_{p,k} - \widehat{\theta}_{q,k}\right)^2 - \sum_{k=1}^{K} w_k \left(\theta_{p,k} - \theta_{q,k}\right)^2\right] \\
&= \sum_{k=1}^{K} w_k \left(E\left[\left(\widehat{\theta}_{p,k} - \widehat{\theta}_{q,k}\right)^2\right] - \left(E\left[\widehat{\theta}_{p,k} - \widehat{\theta}_{q,k}\right]\right)^2\right) \\
&= \sum_{k=1}^{K} w_k \text{Var}\left(\widehat{\theta}_{p,k} - \widehat{\theta}_{q,k}\right) \\
&= \sum_{k=1}^{K} w_k \text{Var}\left(\widehat{u}_{p,k} - \widehat{u}_{q,k}\right) \\
&= \sum_{k=1}^{K} w_k \text{Var}\left(\widehat{u}_{p,k}\right) + \sum_{k=1}^{K} w_k \text{Var}\left(\widehat{u}_{q,k}\right)
\end{aligned}
$$

where the first term is only related to report p, and the second term is only related to report q. That being said, the fixed effect π_p perfectly controls for the first term, and the fixed effect π_q perfectly controls for the second term. Hence, the estimator

of β following equation (3) is unbiased even though the positive bias $E[\widehat{D}_{p,q}] - D_{p,q}$ exists due to Jensen's inequality.

Appendix C

External Validity Checks

As in Kelly et al. (2021), which validated its measure of patent similarities by citations, I also compare my distance measure $\widehat{D}_{p,q}$ with some external measurement of the economic similarities between provinces. More specifically, I consider between-province differences in industrial structure and fiscal regime. To this end, I collect data from the National Bureau of Statistics of China for (i) the share of each sector's output in each province's total output[1] and (ii) the share of each category's expenditure in each province's total government spending[2]. Then, for each pair of province-year combinations, I compute (i) the difference between the vectors of sector-level output shares and (ii) the difference between the vectors of category-level government spending shares, in the same spirit of the squared Euclidean norm in equation (2). Let report p be province $f(p)$'s report in year $g(p)$ and report q be province $f(q)$'s report in year $g(q)$. I denote this measure of the distance between province $f(p)$'s industrial structure in year $g(p)$ and province $f(q)$'s industrial structure in year $g(q)$ by $\widehat{IND}_{p,q}$, and the similar measure for fiscal regime is defined by $\widehat{EXP}_{p,q}$.

Formally, let $\widehat{h}_{p,m}^{IND}$ be the share of sector m's output in province $f(p)$'s total output in year $g(p)$ and $\widehat{h}_{p,m}^{EXP}$ be the share of category m's expenditure in province $f(p)$'s total government spending in year $g(p)$. $\widehat{\mathbf{h}}_p^{IND} = [\widehat{h}_{p,1}^{IND}, \widehat{h}_{p,2}^{IND}, \cdots, \widehat{h}_{p,M_1}^{IND}]$ is the vector of sector-level output shares for province $f(p)$ in year $g(p)$, and $\widehat{\mathbf{h}}_p^{EXP} = [\widehat{h}_{p,1}^{EXP}, \widehat{h}_{p,2}^{EXP}, \cdots, \widehat{h}_{p,M_2}^{EXP}]$ is the vector of category-level government spending shares for province $f(p)$ in year $g(p)$. Then, $\widehat{IND}_{p,q} = ||\widehat{\mathbf{h}}_p^{IND} - \widehat{\mathbf{h}}_q^{IND}||_2^2 = M_1^{-1} \sum_{m=1}^{M_1} (\widehat{h}_{p,m}^{IND} -$

[1]The sector-level GDP data are only available from 2003 to 2016.
[2]The government spending data specific to each category are only available between 2012 and 2019.

$\widehat{h}_{q,m}^{IND})^2$ and $\widehat{EXP}_{p,q} = ||\widehat{\mathbf{h}}_p^{EXP} - \widehat{\mathbf{h}}_q^{EXP}||_2^2 = M_2^{-1}\sum_{m=1}^{M_2}(\widehat{h}_{p,m}^{EXP} - \widehat{h}_{q,m}^{EXP})^2$. There are $M_1 = 25$ sectors for output and $M_2 = 19$ categories for government spending in the data set.

Then, I bin all the observations of the industrial structure distance $\widehat{IND}_{p,q}$ and estimate the conditional expected value $E[\widehat{D}_{p,q}|\widehat{IND}_{p,q}]$ by computing the average of the economic policy distance $\widehat{D}_{p,q}$ conditional on the industrial structure distance $\widehat{IND}_{p,q}$. I also repeat this calculation to estimate the conditional expected value $E[\widehat{D}_{p,q}|\widehat{EXP}_{p,q}]$ with the average of the economic policy distance $\widehat{D}_{p,q}$ conditional on the fiscal regime distance $\widehat{EXP}_{p,q}$. Intuitively, provinces with similar industrial structures or with similar fiscal regimes are more likely to choose similar economic policies, so the economic policy distance between them should be small. In line with this intuition, both of the conditional averages of the economic policy distance $\widehat{D}_{p,q}$ indeed increase with the industrial structure distance $\widehat{IND}_{p,q}$ and the fiscal regime distance $\widehat{EXP}_{p,q}$ as shown in Figure C1. As the definition for my pairwise economic policy distance $\widehat{D}_{p,q}$ formulated by equation (2) does not rely on any information related to the composition of output or local government spending, the positive correlations displayed in Figure C1 should serve as powerful external validity checks.

How Does the Economic Policy Distance Differentiates Textual Documents?

To showcase how the economic policy distance $\widehat{D}_{p,q}$ differentiates textual documents, I extracted the paragraph related to employment from each of the three chosen government work reports: Henan's report in 2010, and Hebei's reports in 2008 and 2006, respectively. These three paragraphs are displayed in Table C1. The Chinese version

shall prevail.[3] I apply the distance measure formulated by equation (2) to compute the textual similarity between the first two paragraphs and that between the first and third paragraphs, respectively.

There are several reasons for choosing these three paragraphs in particular. First, Henan's governor in 2010, Guo Gengmao, was Hebei's governor in 2008, but he was not a number-one or number-two leader in Hebei in 2006.[4] Hence, these three paragraphs could also serve as an example suggesting that a shuffled leader has tended to choose similar economic policies in different provinces under her rule if the second paragraph (from Hebei's report in 2008) is more similar to the first paragraph (from Henan's report in 2010) in comparison to the third one (from Hebei's report in 2006). Second, these paragraphs are chosen to be related to employment because Governor Guo has been famous for inviting Taiwan's electronic manufacturer Foxconn to build the world's largest smartphone production base in Henan's capital city Zhengzhou in 2010, and one of its purposes was to create more jobs for local residents. Thus, it has been widely believed that employment promotion was one of Governor Guo's top policy priorities, and it might be one of the best policy issues that could substantially differentiate the government work reports associated with Governor Guo from those reports not associated with him.

As briefly discussed in Section 3, it is obvious that, the second paragraph in Table C1, in comparison to the third paragraph, is much more similar to the first paragraph because the first two paragraphs specify some similar policies in detail, while the third paragraph is more abstract. For instance, the first two paragraphs provide some specific guidelines for the policies related to the people experiencing

[3]Since there is no official English version of any of the three reports, I translate these Chinese texts by myself. To preserve the original meaning of the Chinese version, some sentences in the translated version might sound wired.

[4]Guo Gengmao was the executive vice governor and also a member of the provincial CCP standing committee in Hebei in 2006.

difficulty finding a job, and a few sentences even give the audience some specific numbers, but these do not exist in the third paragraph.

Consistent with this observation, the squared Euclidean distance (in terms of equation (2)) between the first two paragraphs in Table C1 is 1.215×10^{-4}, while that between the first and third paragraphs in the table is 2.244×10^{-4}. This distance measure differentiates textual documents in a manner that is similar to the human thought process. The primary reason for the relatively smaller distance between the first two paragraphs is that they both mention the phrases related to some specific employment-related policies, including but not limited to "career guidance (就业指导)", "college graduate (高校毕业生)", "vocational training (技能培训)" and "zero-employment household (零就业家庭)", and these terms are mentioned with similar relative frequencies in the first two paragraphs. In the meantime, the similarity in terms of the use of statistical evidence in these two paragraphs is captured by the relative frequencies of the phrases related to units of measurement, such as "thousand people" and "million people". In contrast, almost all of these phrases do not even appear in the third paragraph, so the textual distance between the first and third paragraphs is greater.

In summary, although this distance measure defined by equation (2) is far from perfect, it could still reflect many important aspects of the difference between textual documents by comparing the relative frequencies of the phrases used in those documents. One should be confident of interpreting this measure $\widehat{D}_{p,q}$ as an appropriate estimator for the distance between the policies announced in reports p and q.

How Does the Economic Policy Distance Reflect the Differences Between Actual Policies?

I rely on a case study to show the association between the pairwise economic policy distance $\widehat{D}_{p,q}$ defined by equation (2) and the difference between actual policies: soon after the Summer Olympics in Beijing, the collapse of an unlicensed mine landfill in Shanxi took the lives of more than 250 villagers in September 2008. In response, the central government demoted the governor and deputy governor of Shanxi and appointed Wang Jun, the then director of the State Administration for Work Safety, to take over as the province's new acting governor (Bradsher, 2008). Wang took a series of measures to improve safety performance in coal mining during his term in Shanxi. In December 2012, he was promoted and also rotated to be the party secretary of Inner Mongolia, where he continued his efforts for reducing coal mine deaths until his retirement in 2016.

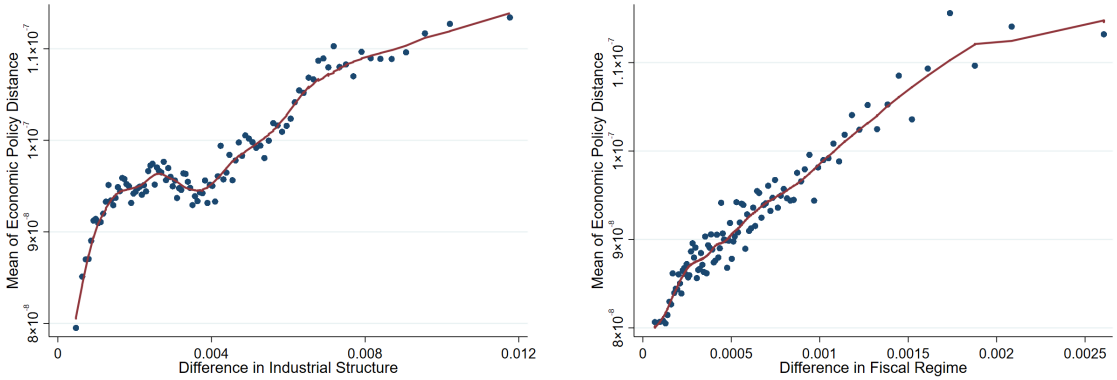
How Wang's transfer from Shanxi to Inner Mongolia was associated with policy diffusion could be seen from Panel A of Table C2, where I tabulate the number of laws and regulations for coal mine safety passed in Shanxi, Inner Mongolia and the rest of the country for each of the four-year period from 2005 to 2020. The takeaways are twofold. First, Shanxi's relevant laws and regulations almost doubled from 2005-2008 to 2009-2012 while the increase in the rest of the country was only around 17%. This coincided with Wang Jun's transfer to Shanxi in late 2008. Second, Inner Mongolia's number of laws and regulations for coal mine safety had been relatively small before 2013. Its substantial increase from 20 to 81 occurred in 2013-2016, which again concurred with Wang's movement from Shanxi to Inner Mongolia.

Based on the observation that Inner Mongolia's policies in 2013-2016 were similar to Shanxi's policies in 2009-2012 at least in terms of workplace safety, I show that

this policy diffusion aligns closely with the changes in the economic policy distance $\widehat{D}_{p,q}$ in Panel B. Let $f(p)$ be report p's province, $g(p)$ be report p's year, $f(q)$ be report q's province and $g(q)$ be report q's year. Suppose that the destination province $f(p)$ is Inner Mongolia, and report q (from a province other than Inner Mongolia) is restricted to be delivered four years before report p, i.e. $g(q) = g(p) - 4$. Then, Panel B suggests that the average of $\widehat{D}_{p,q}$ for the origin province $f(q)$ being Shanxi decreased by 12% on average from $g(p) = 2012$ to $g(p) \in \{2013, 2014, 2015, 2016\}$. In contrast, the average of $\widehat{D}_{p,q}$ for the origin province $f(q)$ being any other province declined only by less than 1%. Hence, the magnitudes of these two changes in the economic policy distance $\widehat{D}_{p,q}$ are very much consistent with the trends of the policies for coal mine safety enacted in Shanxi and Inner Mongolia.

In summary, this case study for Wang Jun and his efforts for improving coal mine safety indicates that the economic policy distance measured by $\widehat{D}_{p,q}$ indeed reflects the difference between actual policies.

Figure C1: External Validity Checks



(a) Industrial Structure **(b) Fiscal Regime**

Notes: For subfigure (a), the horizontal axis is the difference between two province-year combinations' compositions of output, and the vertical axis is the average of the economic policy distance $\widehat{D}_{p,q}$ conditional on the difference in industrial structure. For subfigure (b), the horizontal axis is the difference between two province-year combination's compositions of local government expenditure, and the vertical axis is the average of the economic policy distance $\widehat{D}_{p,q}$ conditional on the difference in fiscal regime. More specifically, all the observations of the differences in industrial structure or fiscal regime are evenly distributed into 100 groups from the smallest to the largest. I compute the mean of the values of $\widehat{D}_{p,q}$ for each group, which is represented by each blue dot. The red curve in each subfigure is the lowess curve based on these 100 blue dots.

Table C1: Examples for How the Economic Policy Distance Differentiates Textual Documents

Province	Year	Selected Paragraph (English)	Selected Paragraph (Chinese)
Henan	2010	Be sure to do a good job in employment and reemployment. Put employment as the top priority for people's livelihood. Adhere to more proactive employment policies. Vigorously implement the "Plan for the Employment Promotion Act". Give full play to the role of government investments and large projects in stimulating employment. Improve the ability of providing employment among small- and medium-sized enterprises, labor-intensive industries, private sector and service industries. Extend the employment support policy of "Five Delays, Four Reductions and Three Subsidies" for another year. Improve the career guidance for college graduates. Hold the career fair named "Wan Qi Qian Chang" for the employment of college students in specific. Do a good job in the employment of veterans. Vigorously develop more public-service jobs. Provide employment-related assistance to members of zero-employment households, women over 40, men over 50 and other people experiencing difficulty finding a job. Improve the employment-related services for migrant workers. Strengthen vocational training. Create jobs for the 1 million labor transferred from rural areas. Carry out activities related to mass entrepreneurship. Strive to create 400 thousand new jobs by mass entrepreneurship.	切实做好就业再就业工作。把就业作为民生之首，坚持更加积极的就业政策，大力实施"就业促进行动计划"。发挥政府投资和重大项目带动就业的作用，提高中小企业、劳动密集型产业、民营经济和服务业吸纳就业的能力。去年到期的"五缓四减三补贴"就业扶持政策延长一年。加强对高校毕业生就业指导，举办"万企千场"大学生就业专场招聘会。做好复转军人就业安置工作。大力开发公益性岗位，做好"零就业家庭"、"4050"人员等就业困难群众的就业援助。改善农民工就业服务，强化技能培训，新增转移就业农村劳动力100万人。深入开展全民创业活动，力争实现以创业带动就业40万人。
Hebei	2008	Proactively create new jobs. Be sure to do a good job in the implementation of the Employment Promotion Law and the Labor Contract Law. Work out local laws and regulations for employment promotion. Improve the policies that support entrepreneurship and self-employment. Coordinate the labor markets in urban and rural areas. Create new jobs by mass entrepreneurship. Improve the employment-related assistance to the people experiencing difficulty finding a job. Focus on the employment and the employment stability of the members of zero-employment households. Improve the services related to employment agency, career guidance and vocational training. Do a good job in the employment of college graduates, veterans and people with disabilities. Ensure that more than 450 thousand jobs are created in urban areas. Help 200 thousand of the laid-off workers to get reemployed. Carefully solve illegal employment, arrears of wage and other problems. Safeguard the legal rights of the workers. Promote harmonious employer-employee relations.	积极扩大就业。突出抓好就业促进法和劳动合同法的贯彻，研究制定促进就业的地方性法规。完善支持自主创业、自谋职业政策，统筹城乡就业，促进以创业带动就业。健全困难群众就业援助制度，重点解决零就业家庭就业及其就业的稳定性。加强职业介绍、就业指导、技能培训等服务，重视做好高校毕业生、退伍转业军人、残疾人就业工作，确保城镇新增就业45万人以上，帮助20万名下岗失业人员实现再就业。认真解决非法用工、工资拖欠等问题，维护职工合法权益，促进劳动关系和谐。
Hebei	2006	Proactively create new jobs. Employment is the foundation of people's livelihood. Adhere to place job creation in a more prominent position. Establish a mechanism for the coordination between economic growth and job creation. Continue implementing proactive employment policies. Effectively create more job opportunities. Improve the service system for employment and reemployment. Improve the employment and entrepreneurial environment. Strengthen labor supervision. Protect the legal rights of the workers in accordance with the law.	积极扩大就业。就业是民生之本。坚持把扩大就业放在更加突出的位置，建立经济增长与扩大就业协调推进机制。继续实施积极的就业政策，有效创造更多的就业机会和就业岗位。强化就业再就业服务体系建设，改善就业和创业环境。加强劳动监察，依法保障劳动者的合法权益。

Table C2: Examples for How the Economic Policy Distance Reflects the Difference Between Actual Policies

Panel A	Policies for Coal Mine Safety Enacted in		
Time Period	Shanxi	Inner Mongolia	Rest of the Country
2005-2008	43	10	553
2009-2012	82	20	648
2013-2016	68	81	899
2017-2020	106	37	872

Panel B	Economic Policy Distance $\widehat{D}_{p,q}$ (Relative to the $g(q) = 2012$ Level)	
Destination Province $f(p)$	Inner Mongolia	
Origin Province $f(q)$	Shanxi	Average for the Rest of the Country
$g(p) = 2009$, $g(q) = 2005$	0.900	1.038
$g(p) = 2010$, $g(q) = 2006$	1.206	1.158
$g(p) = 2011$, $g(q) = 2007$	1.156	1.206
$g(p) = 2012$, $g(q) = 2008$	1.000	1.000
$g(p) = 2013$, $g(q) = 2009$	0.975	1.056
$g(p) = 2014$, $g(q) = 2010$	0.833	0.958
$g(p) = 2015$, $g(q) = 2011$	0.803	0.984
$g(p) = 2016$, $g(q) = 2012$	0.899	0.970
$g(p) = 2017$, $g(q) = 2013$	0.978	1.114
$g(p) = 2018$, $g(q) = 2014$	1.128	1.294
$g(p) = 2019$, $g(q) = 2015$	0.936	1.155

Notes: The data for the policies related to coal mine safety is collected from pkulaw.com. $f(p)$ and $f(q)$ are government work reports p's and q's provinces. $g(p)$ and $g(q)$ are government work reports p's and q's years. I restrict $g(q) = g(p) - 4$. All numbers in Panel B are relative to the level in $g(q) = 2012$.

Appendix D

Simplified Regression Models

The first simplified approach is to use Abowd et al. (1999)'s well-known framework for matched employer-employee data as in equation (4). The dependent variable PC_p is first principal component of government work report p's phrase counts. In other words, it is the linear combination of the elements in report p's vector of phrase counts $\mathbf{c}_p$ with the largest variance. Intuitively, this variable could also be understood as where the policies announced in report p fall on a given one-dimensional political-economic spectrum, which in turn implies that the similarity between reports p and q could be reflected by the small absolute difference $|PC_p - PC_q|$. For the right-hand side of the equation, I include the idiosyncratic error term ε_p and three fixed effects for (i) report p's corresponding province leader $n(p)$, (ii) report p's province $f(p)$ and (iii) report p's year $g(p)$, respectively. The objective of this exercise is to evaluate the extent to which the fixed effects for provincial leaders $\pi_{n(p)}$ explain the dependent variable's variation.

Note that there are two leadership positions in each province: party secretary and governor, but the fixed effect $\pi_{n(p)}$ in equation (4) only has one dimension. I address this identification problem by following Yao and Zhang (2015)'s approach of treating the pair of leaders working in the same province in the same year as if they were working in two separate but identical provinces, and the fixed effects for provinces are the same for these two leaders. In the meantime, I only consider the connected sample created by the leaders who moved between provinces as in Yao and Zhang (2015). Among the 31 province-level regions across China, only Hainan had never sent a number-one or number-two provincial leader to or received such a leader

from another province over the 2005-2019 period. Hence, the connected sample could be constructed by simply excluding Hainan from the data set.

The kernel density of the estimated provincial leader effects denoted by the fixed effect $\pi_{n(p)}$ is presented in Figure D1. The dependent variable PC_p is shown to vary quite substantially from one leader to another. The significance of these leader-specific effects could also be seen by (i) the F-statistic for $\pi_{n(p)}$ being $F(173, 597) = 2.357$ with the resulting p-value being smaller than 0.001 and (ii) $\pi_{n(p)}$ explaining 40.6% of the variation in the residual after partialling out the fixed effects for provinces and years. These estimation results suggest that the reports corresponding to the same leader would tend to share certain characteristics (measured by PC_p) conditional on their provinces and years, which aligns with the baseline findings of this article.

The second approach is much more similar to the preferred specification (i.e. column (3) of Table 1): I still use the regression model expressed by equation (3), but I restrict that $g(q) = g(p) - \tau$ for each $\tau \in \{2, 4, 6\}$, i.e. report q has to be delivered exactly τ years before report p. In doing so, I could omit the last three fixed effects in equation (3). The estimation results based on this new specification formulated by equation (5) are presented in Table D1. The coefficient estimates are negative and statistically significant for all three values of $\tau \in \{2, 4, 6\}$, which is consistent with my main findings based on the full model.

Three-Dimensional Fixed Effects

For the model formulated by equation (3), I also try to use three-dimensional fixed effects $\pi_{p,g(q)}$, $\pi_{q,g(p)}$, $\pi_{p,f(q)}$ and $\pi_{q,f(p)}$, where p and q are government work reports that correspond to two-dimensional province-year combinations, $f(p)$ and $f(q)$ are report p's and report q's provinces, and $g(p)$ and $g(q)$ are report p's and report q's

years. Table D2 reports the estimation results with these more restrictive specifications. Specifically, column (1) replicates column (3) of Table 1, the preferred specification that only includes two-dimensional fixed effects, column (2) controls for the three-dimensional report-by-year fixed effects $\pi_{p,g(q)}$ and $\pi_{q,g(p)}$ and the fixed effects for province pairs $\pi_{f(p),f(q)}$, column (3) controls for the three-dimensional report-by-province fixed effects $\pi_{p,f(q)}$ and $\pi_{q,f(p)}$ and the fixed effects for year pairs $\pi_{g(p),g(q)}$, and column (4), as the most restrictive specification, controls for all the four three-dimensional fixed effects.

As presented in the table, the coefficient estimates are only negative and statistically significant in the first two columns. Nevertheless, this does not necessarily imply that my main findings following equation (3) are not robust because these three-dimensional fixed effects, especially the report-by-province ones in columns (3) and (4), might absorb the dependent variable's variation too much. This could be seen by (i) the degrees of freedom lost due to the fixed effects being around 25%-33% of the number of observations and (ii) the R-squared scores being 0.967 in column (3) and 0.984 in column (4), both of which are very close to 1.[5] As absorbing too much variation might discard a great deal of valid identifying information (e.g. Neumark et al., 2014), I choose to only use the less restrictive fixed effects specified in equation (3) throughout this article, especially given that those two-dimensional fixed effects could already address much of the potential endogeneity bias as thoroughly discussed in Section 3.

[5]Intuitively, the two report-by-province fixed effects in columns (3) and (4) tend to absorb more variation across province pairs than that across year pairs. As the dependent variable has more cross-sectional variation than time-series variation, including these two fixed effects makes the regression model too restrictive.

Officials Affiliated with Province-Level Research Offices

The China Political Elite Database has information on the past experience of almost all senior cadres in China between late 1990s and 2015. I filter out the politicians who had worked for any research office affiliated either to a provincial CCP committee or to a provincial government.[6] 138 out of the 3923 politicians in the data set had such experience. I track turnover among these 138 politicians and find that only two of them had co-moved with their corresponding provincial leaders across different provinces: Tang Guozhong moved with his corresponding provincial leader Li Changchun from Henan to Guangdong, and Liu Kewei moved with his corresponding provincial leader Hu Chunhua from Hebei to Guangdong. Hence, the officials affiliated with province-level research offices have indeed been very unlikely to co-move with their corresponding provincial leaders across different provinces.

To enhance the credibility of the main findings of this article, I repeat the preferred specification (i.e. column (3) of Table 1) following equation (3) but with the observations corresponding to Li Changchun and Hu Chunhua excluded.[7] The new coefficient estimate is -0.205 (in terms of how many standard deviations the dependent variable would change as the common leader dummy increases from 0 to 1), with the two-way clustered standard error being 0.072, which means that this coefficient is still shown to be negative and statistically significant after excluding these two leaders.

[6]Drafting government work reports is more likely to be a task for the research office affiliated with the provincial government, but those who work for the research office affiliated with the provincial CCP committee may also join the writing group for the government work reports.

[7]Li Changchun's movement from Henan to Guangdong was before 2005, and the data used in this article only covers from 2005 to 2019, so there is no observation corresponding to him.

Change in Provincial Leader

To verify that provincial leaders indeed have no interest in continuing their predecessors' economic policies, I take an alternative event study approach as formulated by equation (7), where the dependent variable measures how a province's report deviates from its report one year ago and the independent variables are the leads and lags of the dummy variable indicating whether there is a change in leader in that province in a given year. I also include the fixed effects for provinces and years and the idiosyncratic error term. As presented in Table D3 and Figure D2 of this appendix, while all coefficient estimates before and after the event of changing a provincial leader, i.e. ψ_{-2}, ψ_{-1}, ψ_1 and ψ_2, are statistically insignificant for both party secretaries and governors, the coefficient ψ_0 exactly corresponding to this event is shown to be positive and statistically significant. The magnitude is 44.8% of the standard deviation for a party secretary change or 37.1% for a governor change. These estimation results confirm that a new provincial leader tends to choose some policies significantly different from those implemented by their predecessors. More importantly, this pattern is not just specific to the successors of the shuffled leaders.

Staggered Treatment Correction

As briefly explained in subsection 4.2, a recent literature in econometrics, such as De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Goodman-Bacon (2021), Sun and Abraham (2021) and Athey and Imbens (2022), has shown that the traditional event study design yields a weighted average of all possible permutations of pairwise difference-in-differences estimators, where a pair is either the never-treated control group paired with a cohort of observations treated in year τ, or a cohort of observations treated in year τ paired with another cohort of observations

treated in an earlier year $\tau' < \tau$. The concern is that these weights could be negative, which would contaminate leads and lags in the event study design and potentially invalidate my main findings if there exists heterogeneity in treatment effects.

To address this bias, following Callaway and Sant'Anna (2021), I define the treated cohorts for event A based on the year pairs $g(p)$-by-$g(q)$ in equation (6) and estimate the "cohort-time average treatment effects". As treated cohorts are defined by the year pairs when the province pairs are first treated, considering events A and B simultaneously would make the size of each cohort too small to conduct the event study regressions. Hence, I choose to only verify the event study estimates corresponding to event A in this appendix.

The estimation procedures have two steps. First, for each treated cohort, I estimate a separate event study regression (with three lags and three leads as in equation (6)) only with the observations from this cohort and those from the never-treated control group. Second, I take weighted averages of the point estimates of the lags and leads across all the treated cohorts with weights equal to each treated cohort's share of all the treated observations.

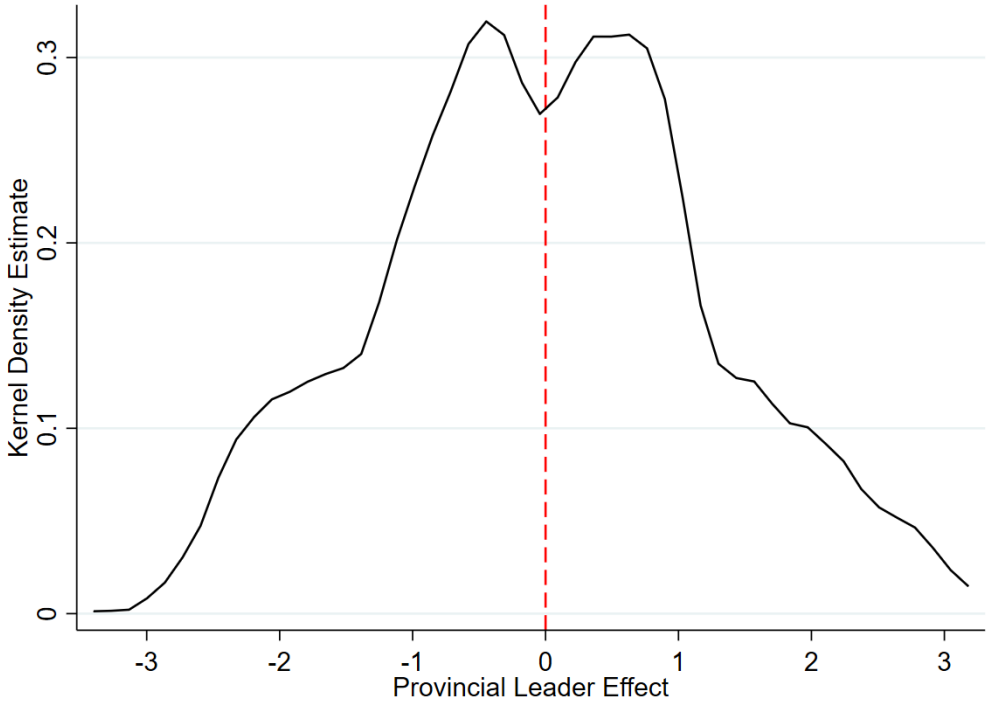
As presented in Table D4 of this appendix, the estimation results weighted by cohort size are shown to be similar to the point estimates displayed in Tables 2 and 8 and visualized in Figures 3 and 5. The differences between the estimates in the odd-numbered and even-numbered columns are all reasonably small, which suggests that the conclusions drawn from event study estimates should be valid.[8]

[8]Slightly different from the standard panel data, the two dimensions in my data set are not location and time. The treatment effects in my context are also not permanent given that the shuffled leaders would eventually leave office. As the statistical inference methods used in most of the existing studies are designed for the standard two-dimensional panel data with permanent treatment effects, I choose not to compute the standard errors here. Note that, even for the standard data sets, the applied literature has not reached a consensus on how to make statistical inference yet.

"Office-by-Province" Fixed Effects in Jia et al. (2015)

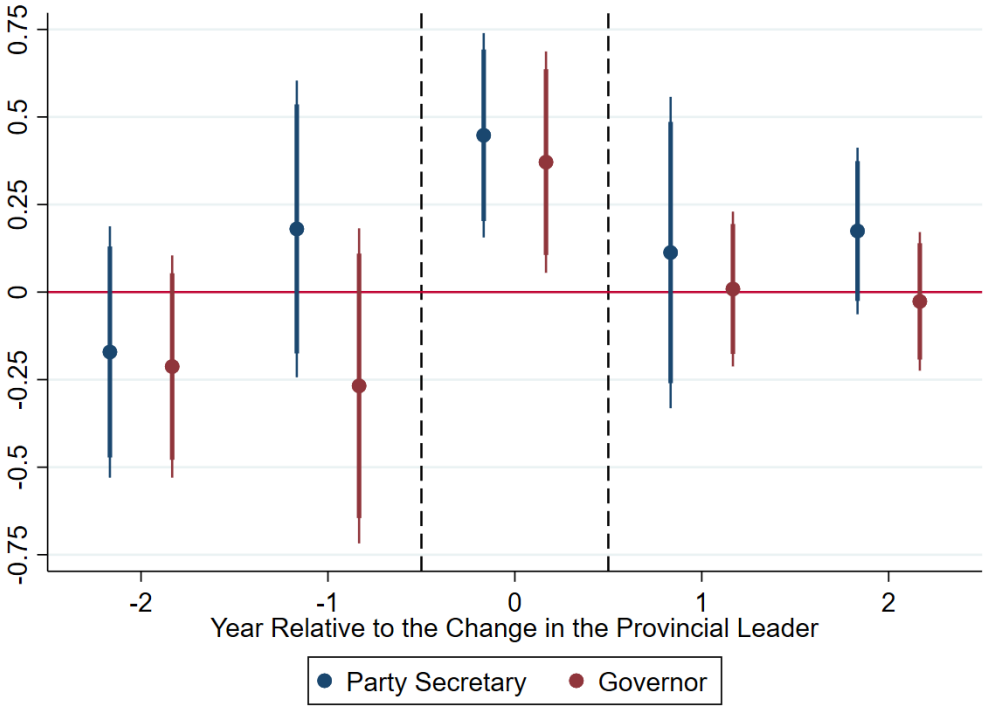
I replicate column (6) of Jia et al. (2015)'s Table A4, which controls for all available independent variables (including $CONNECTED_{nt}$) and their interactions with growth. The estimate of μ_{oj} for each office o (either party secretary or governor) and each province j is presented in Table D5. The estimates for Ningxia's party secretary position and Jiangxi's governor position are missing since they are omitted because of collinearity in Jia et al. (2015)'s regression. According to these estimates, after excluding the four direct-controlled municipalities (DCMs), the top party secretary positions are in Shandong, Guangdong, Jiangxi, Jiangsu and Liaoning, while the top governor positions are in Fujian, Liaoning, Gansu, Jilin and Qinghai. These results are in line with the common understanding of China's provincial politics. In addition, the averages of the estimates of μ_{oj} among coastal provinces are 0.120 for party secretary positions and 0.010 for governor positions, while those averages among inland provinces are 0.052 and -0.009, respectively. This is consistent with the pattern that inland provinces are politically inferior to their coastal counterparts on average, which validates the results in column (1) of Table 10.

Figure D1: Kernel Density Estimate of Provincial Leader Effect



Notes: This figure presents the kernel density of the estimated provincial leader effects (denoted by the fixed effects for these provincial leaders $\pi_{n(p)}$), where each estimate should be interpreted as how many standard deviations the dependent variable would change if a given provincial leader is replaced by an average person. With heteroskedasticity-robust standard errors, the resulting F-statistic for these fixed effects for provincial leaders is $F(173, 597) = 2.357$, and the p-value is less than 0.001. In the meantime, based on the analysis of variance, these provincial leader effects explain 40.6% of the variation in residual after partialling out the fixed effects for provinces and years.

Figure D2: Change in Provincial Leader



Notes: This figure shows how a change in provincial leader affects the dependent variable representing the extent to which a government work report is different from another report from the same province but one year ago, as reported in Table D3. The blue dots and lines correspond to the event of changing a province's party secretary. The red dots and lines correspond to the event of changing a province's governor. Each dot indicates the coefficient estimate that should be interpreted as how many standard deviations the dependent variable would change as the independent variable increases from 0 to 1. The vertical bars show the 90% and 95% confidence intervals based on block-bootstrapping at the province level.

Table D1: More Restrictive Specifications

	(1) $\widehat{D}_{p,q}$	(2) $\widehat{D}_{p,q}$	(3) $\widehat{D}_{p,q}$
Common Leader Dummy $L_{p,q}$	-0.290**	-0.281**	-0.418**
	(0.138)	(0.137)	(0.162)
Year Difference Between Reports p and q	2	4	6
New Report p FE	✓	✓	✓
Old Report q FE	✓	✓	✓
New Report p's Province × Old Report q's Province FE	✓	✓	✓
Number of Observations	9126	7722	6318

Notes: Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the new report level (i.e. p) and the old report level (i.e. q). Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each government work report, p or q, corresponds to a unique province-year combination. The dependent variable is the economic policy distance $\widehat{D}_{p,q}$ calculated by measuring the textual difference between provincial government work reports p and q. The independent variable is the common leader dummy $L_{p,q}$. The three specifications in this table are more restrictive than those in Table 1 in the sense that each government work report is only compared with reports from other provinces two, four or six years ago. This also reduces the number of fixed effects to be controlled. Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1.

Table D2: Three-Dimensional Fixed Effects

	(1) $\widehat{D}_{p,q}$	(2) $\widehat{D}_{p,q}$	(3) $\widehat{D}_{p,q}$	(4) $\widehat{D}_{p,q}$
Common Leader Dummy $L_{p,q}$	-0.225***	-0.234***	-0.073	0.061
	(0.069)	(0.082)	(0.156)	(0.196)
"Three-Dimensional" FEs				
New Report p × Old Report q's Year FE		✓		✓
Old Report q × New Report p's Year FE		✓		✓
New Report p × Old Report q's Province FE			✓	✓
Old Report q × New Report p's Province FE			✓	✓
"Two-Dimensional" FEs				
New Report p FE	✓			
Old Report q FE	✓			
New Report p's Province × Old Report q's Province FE	✓	✓		
New Report p's Year × Old Report q's Year FE	✓		✓	
New Report p's Province × Old Report q's Year FE	✓			
New Report p's Year × Old Report q's Province FE	✓			
Number of Observations	73710	73710	72306	72306
Degrees of Freedom Lost Due to the Fixed Effects	2319	6372	18355	23814
R^2	0.933	0.949	0.967	0.984

Notes: Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the new report level (i.e. p) and the old report level (i.e. q). Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. For columns (3) and (4), 1404 singleton observations are dropped so that the number of observations is 72,306. Each government work report, p or q, corresponds to a unique province-year combination. The dependent variable is the economic policy distance $\widehat{D}_{p,q}$ calculated by measuring the textual difference between provincial government work reports p and q. The independent variable is the common leader dummy $L_{p,q}$. Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1.

Table D3: Change in Provincial Leader

	(1) Party Secretary	(2) Governor
2 years before a change in provincial leader	-0.171	-0.213
	(0.183)	(0.162)
1 year before a change in provincial leader	0.180	-0.268
	(0.216)	(0.230)
year of the change in provincial leader	0.448***	0.371**
	(0.149)	(0.161)
1 year after a change in provincial leader	0.113	0.009
	(0.227)	(0.113)
2 years after a change in provincial leader	0.174	-0.027
	(0.121)	(0.101)
Province FE	✓	✓
Year FE	✓	✓
Number of Observations	378	378

Notes: Standard errors reported in parentheses are block-bootstrapped at the province level. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the textual difference between a government work report and another report in the same province one year ago. Column (1) presents the coefficient estimates before and after the event of changing a province's party secretary. Column (2) presents the coefficient estimates before and after the event of changing a province's governor. Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1.

Table D4: Staggered Treatment Correction

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	$\widehat{D}_{p,q}$ Difference in GWR		Difference in News Headlines		Difference in Fiscal Regime	
	Table 2	Reweighted	Table 8	Reweighted	Table 8	Reweighted
Before Event A						
3 years before	0.077	0.155	-0.003	-0.037	0.207	-0.036
	(0.113)	[0.690]	(0.104)	[-0.327]	(0.178)	[-1.365]
2 years before	-0.113	-0.063	-0.017	-0.015	0.139	0.034
	(0.094)	[0.532]	(0.079)	[0.025]	(0.134)	[-0.784]
1 year before	-0.103	-0.105	0.040	0.024	0.056	0.116
	(0.119)	[-0.017]	(0.123)	[-0.130]	(0.141)	[0.426]
After Event A & Before Event B						
0 year after	-0.255***	-0.246	-0.352**	-0.386	-0.251	-0.023
	(0.089)	[0.101]	(0.159)	[-0.214]	(0.227)	[1.004]
1 year after	-0.308***	-0.322	-0.523*	-0.492	-0.462	-0.294
	(0.118)	[-0.119]	(0.274)	[0.113]	(0.290)	[0.579]
2+ years after	-0.357***	-0.305	-0.186	-0.130	-1.093**	-0.780
	(0.105)	[0.495]	(0.211)	[0.265]	(0.430)	[0.728]

Notes: Odd-numbered columns copy the event study estimates and their corresponding clustered standard errors from Table 2 and Table 8. Even-numbered columns report the estimates computed by conducting the event study regression for each treated cohort and reweighting the cohort-specific point estimates by cohort size. The difference between each pair of estimates per unit of the standard error (corresponding to the estimate without reweighting) are reported in brackets in the even-numbered columns. For example, $0.690 = (0.155 - 0.077)/0.113$. The dependent variables are the economic policy distance $\widehat{D}_{p,q}$ calculated by measuring the textual difference between provincial government work reports in columns (1) and (2), the difference in terms of the relative frequencies of different topics in the headlines of the news articles reporting provincial leaders' activities in columns (3) and (4), and the difference in terms of the composition of provincial government expenditure in columns (5) and (6). Event A is defined as a provincial leader with past work experience in the origin province being appointed in the destination province. Event B is defined as a provincial leader in the destination province with past work experience in the origin province being replaced by someone else without such experience. Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1. The fixed effects are controlled as in Table 2 and Table 8.

Table D5: "Office-by-Province" Fixed Effects in Jia et al. (2015)

	Fixed Effect μ_{oj} for	
Province	Party Secretary Position	Governor Position
Beijing	0.178	0.087
Tianjin	0.088	-0.098
Hebei	0.052	0.005
Shanxi	-0.010	-0.039
Inner Mongolia	0.013	-0.057
Liaoning	0.100	0.129
Jilin	-0.006	0.103
Heilongjiang	0.060	-0.022
Shanghai	0.294	-0.026
Jiangsu	0.118	0.031
Zhejiang	0.073	-0.097
Anhui	-0.010	0.002
Fujian	0.073	0.300
Jiangxi	0.157	
Shandong	0.273	-0.040
Henan	0.086	0.023
Hubei	0.064	0.060
Hunan	-0.007	0.031
Guangdong	0.216	-0.047
Guangxi	-0.006	-0.075
Hainan	0.040	0.034
Chongqing	0.202	-0.102
Sichuan	0.065	-0.081
Guizhou	0.051	0.046
Yunnan	0.009	-0.062
Tibet	0.026	-0.031
Shaanxi	-0.011	-0.068
Gansu	0.043	0.113
Qinghai	0.016	0.077
Ningxia		-0.136
Xinjiang	0.058	-0.107

Notes: The estimates for Ningxia's party secretary position and Jiangxi's governor position are missing since they are omitted because of collinearity in Jia et al. (2015)'s regression.

Appendix E

LDA Model with Government Work Reports

Subsection 4.5 briefly explains the LDA topic modeling method. More specifically, given the pre-specified number of topics R, this method could be broken down into the following five steps:

- Step 1: Obtain all the government work reports. Treat each paragraph of the report as a separate document. In doing so, there are 35,753 documents in the sample. As in Section 2, use the "jieba" package in Python to segment the Chinese text for each document, and remove the stop words and the phrases that appear no more than 15 times across all reports.

- Step 2: Apply the LDA model in the "tomotopy" package in Python to the entire collection of documents. The LDA model would return the relative importance of each phrase for each of the R automatically generated topics. The top phrases for each topic are displayed in Table E1 of this appendix for $R = 30$. The model would also compute the topic proportions (i.e. the percentage of the text associated with each topic) of each document.

- Step 3: In terms of the between-topic analysis, for each government work report p, denote the set of its paragraphs by P_p. For each paragraph $\rho \in P_p$, denote its word count by l_ρ and the $R \times 1$ vector of topic proportions by $\mathbf{T}_\rho$. (The r-th element of $\mathbf{T}_\rho$ represents the proportion of paragraph ρ related to the r-th topic.) Then, the $R \times 1$ vector of topic proportions for the government work report p, denoted by $\mathbf{T}_p$, is defined as $\mathbf{T}_p = \sum_{\rho \in P_p} \frac{l_\rho}{\sum_{\rho' \in P_p} l_{\rho'}} \mathbf{T}_\rho$. As in Section 3 and in Gentzkow et al. (2019b), $\mathbf{T}_p \sim MN(m_p^T, \boldsymbol{\theta}_p^T)$, where m_p^T is the sum of

the elements in $\mathbf{T}_p$, and $\boldsymbol{\theta}_p^T$ is the $R \times 1$ vector of choice probabilities for the R topics.

- Step 4: As in equation (1), define $\left\|\mathbf{w} \cdot (\boldsymbol{\theta}_p^T - \boldsymbol{\theta}_q^T)\right\|_2^2$ as the new infeasible measure of the economic policy distance between reports p and q, where each element of $\mathbf{w}$ is equal to $1/R$, i.e. all topics are equally weighted. Then, use $\left\|\mathbf{w} \cdot (\mathbf{T}_p/m_p^T - \mathbf{T}_q/m_q^T)\right\|_2^2$ to approximate $\left\|\mathbf{w} \cdot (\boldsymbol{\theta}_p^T - \boldsymbol{\theta}_q^T)\right\|_2^2$ as in equation (2), and regress this estimator on the common leader dummy $L_{p,q}$, with fixed effects specified in equation (3).[9] This concludes the between-topic analysis.

- Step 5: In terms of the within-topic analysis, $\left\|\mathbf{w}^{(r)} \cdot (\boldsymbol{\theta}_p - \boldsymbol{\theta}_q)\right\|_2^2$ is the infeasible measure of the economic policy distance specific to topic r, where the k-th element of $\mathbf{w}^{(r)}$ is the relative importance of the k-th phrase for topic r from Step 2. As in equations (1) and (2) in Section 3, use $\left\|\mathbf{w}^{(r)} \cdot (\mathbf{c}_p/m_p - \mathbf{c}_q/m_q)\right\|_2^2$ to approximate $\left\|\mathbf{w}^{(r)} \cdot (\boldsymbol{\theta}_p - \boldsymbol{\theta}_q)\right\|_2^2$. Then, regress this topic-specific estimator on the common leader dummy $L_{p,q}$ for each topic r, with fixed effects specified in equation (3), and the coefficient estimate would imply the extent to which the convergence of subnational economic policies has been driven by the convergence in the phrases used for this specific topic r.

As suggested by step 2, for each of the $R = 30$ topics, I report the top 10 phrases in Table E1. I name these topics based on the top phrases. In terms of the average topic proportions across all the reports, the top 5 topics are as follows: 1. #20 Regional Development, 2. #26 Reform, 3. #7 Industrial Policies, 4. #24 Economic Opportunities, 5. #25 Economic Indicators. The automatically generated topics shown in the table and this ranking suggest that provincial government work reports

[9] As demonstrated in Appendix B, the positive bias of this estimator would be removed when the fixed effects π_p and π_q are controlled.

indeed focus on economic policies more than politics, which validates the research design of this article once more.

In addition, I also conduct the within-topic analysis for the pre-specified number of topics $R \in \{35, 40\}$. More specifically, given $R \in \{35, 40\}$, I regress the within-topic economic policy distance defined in step 5 on the common leader dummy $L_{p,q}$ for each topic. The estimates for $R = 35$ are reported in Table E2 while those for $R = 40$ are reported in Table E3. As presented in the two tables, although most coefficient estimates are negative, few of them are statistically significant. These results once again confirm that China's local economic policy isomorphism has been driven mainly by convergence in which topics these government work reports cover, rather than by convergence in how the provincial leaders talk about a given policy issue.

Environmental Vertical Management Reform

One hypothesis is that, given China's unique central-regional relationship in which some bureaucracies are administered centrally in the unitary form (i.e. U-form) while others are administered locally in the multi-divisional form (i.e. M-form), the U-form policy issues are less susceptible to the rotations of provincial leaders than the M-form ones, so the within-topic estimates corresponding to the former are less likely to be negative in comparison to the latter. I evaluate this hypothesis by taking advantage of the reforms in recent years that switched the Ministry of Environmental Protection from M-form to U-form (Kostka and Zhang, 2018; Jia and Chen, 2019; Wang and Yang, 2021). Specifically, after the Fifth Plenum of the 18th CCP Central Committee, two important reforms were announced in 2016: (i) local environmental policymaking would be centralized at the province level, and (ii) the central government would monitor provincial environmental authorities by deploying its own inspection teams

across the country on a yearly basis. These changes, especially the second one, have substantially reduced provincial leaders' discretion over local environmental issues.

Given these environmental reforms, I repeat the within-topic estimation for "Topic 17: Pollution Reduction" and "Topic 19: Ecology" in Table 6, one at a time, but with one simple modification: the coefficient of the common leader dummy $L_{p,q}$ is now allowed to vary across report p's year $g(p)$. Formally, the new regression model could be formulated as follows:

$$Y_{p,q} = \beta_{g(p)} L_{p,q} + \pi_p + \pi_q + \pi_{f(p),f(q)} + \pi_{g(p),g(q)} + \pi_{f(p),g(q)} + \pi_{g(p),f(q)} + \varepsilon_{p,q}$$

where the dependent variable $Y_{p,q}$ is the within-topic measure of the textual difference between reports p and q on either pollution reduction or ecology, and $\beta_{g(p)}$ is the coefficient varying across report p's year $g(p)$. The fixed effects and the idiosyncratic error term are the same as those in equation (3). Note that I exclude the observations with $g(p) < 2015$ to focus on the time periods around the environmental reforms. In addition, 2016 should be considered as a pre-treatment year because the reforms were carried out after the provincial government work reports were delivered in that year.

The estimation results corresponding to $g(p) \in \{2015, 2016, 2017, 2018, 2019\}$ are presented in Table E4 and Figure E1. For both topics, the coefficients are shown to be negative and statistically significant for the years before 2017 but insignificant afterwards. This implies that the effect of leadership rotations on the convergence of the environmental policies at the province level has dissipated soon after the initialization of the reforms in 2016. This finding is consistent with the hypothesis and should serve as suggestive but convincing evidence on the role of organization structure in how provincial leaders discuss a given policy issue.

LDA Model with News Headlines

data.people.com.cn/pd/sbzyl provides the news headlines reporting provincial leaders' activities. Some examples of these headlines are presented in Table E5.[10] These examples should suggest that many of these headlines are highly related to local economic policies. The procedures of the LDA topic modeling method for the collection of these news headlines are very similar to those explained above with three differences. First, this alternative data set is only used for between-topic analysis. Second, when preprocessing the raw data of these headlines, in addition to removing the "stopwords" defined in Section 2, I also delete the names of provincial leaders so that the estimation results would not be affected by the mentions of the same leader's name.[11] Third, instead of using $l_\rho / \sum_{\rho' \in P_p} l_{\rho'}$ as weights in step 3 in the preceding subsection, because the relative importance of a headline among all headlines associated with a province-year combination is unknown, I choose to simply take the unweighted average. In addition, given that each headline is specific to a date rather than a year, to align with provincial government work reports which are usually delivered in January, I define a year as the twelve-month period beginning from July so that January is in the middle of the redefined year.

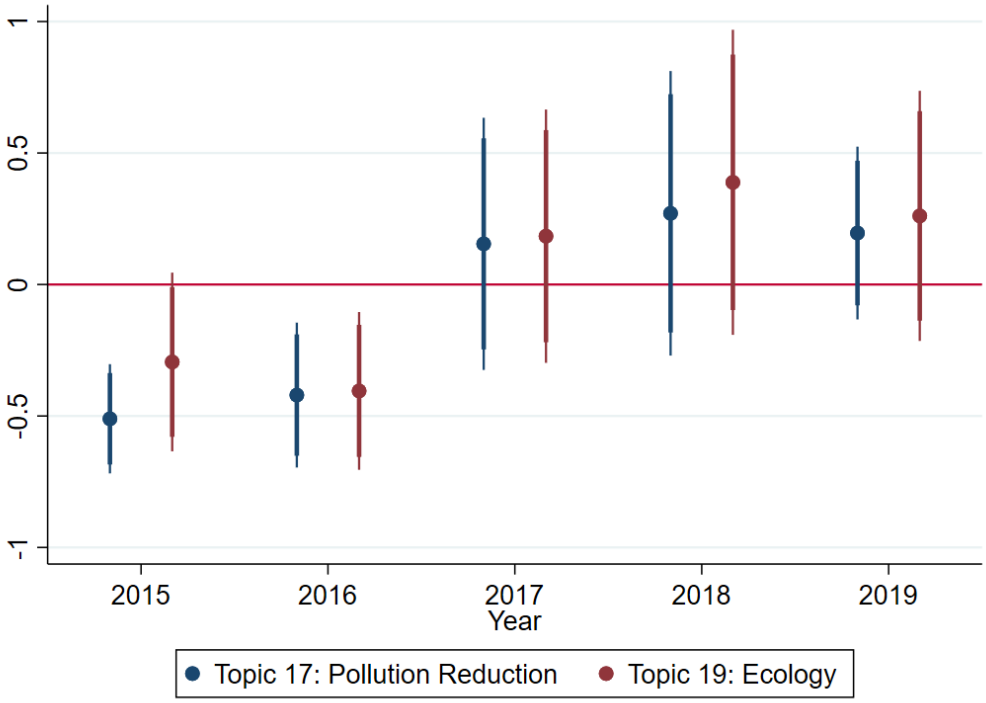
Table E6 displays the top 10 phrases for each topic given the pre-specified number of topics being 30, and I name these topics based on the top phrases. These topics listed in the table suggest that, although there are slightly more topics pertinent to politics and ideology in comparison to provincial government work reports, a majority of the topics are still related to economic affairs. Hence, these news headlines

[10] Since there is no official English version of any of the headlines, I translate these Chinese texts by myself. To preserve the original meaning of the Chinese version, some sentences in the translated version might sound wired.

[11] Leaders' names might appear in these news headlines, but they never appear in any government work report to the best of my knowledge.

are useful for us to understand if the convergence of the policies announced in the government work reports has translated into any similarity in policy outcomes.

Figure E1: Environmental Vertical Management Reform



Notes: The dependent variable is the within-topic measure of the textual difference between reports p and q on "Topic 17: Pollution Reduction" or "Topic 19: Ecology" in Table 6. Each dot indicates the coefficient estimate that should be interpreted as how many standard deviations the dependent variable would change as the independent variable increases from 0 to 1. The vertical bars show the 90% and 95% confidence intervals.

Table E1: Topics in Provincial Government Work Reports

Pre-Specified Number of Topics $R = 30$		
Topic ID	Topic Name	Top Phrases (In Chinese)
1	Socioeconomic Development	发展,经济,增长,社会,结构,民生,快,加快,改善,投资
2	Education	教育,建设,发展,推进,义务教育,提高,农村,实施,加快,服务
3	Service Sector	旅游,发展,消费,服务业,建设,加快,服务,文化,物流,市场
4	Labor Economics	就业,创业,收入,政策,培训,劳动力,农村,毕业生,万人,转移
5	Social Stability	社会,管理,建设,生产,工作,稳定,体系,完善,群众,推进
6	Rural Development	农村,推进,建设,改革,农业,农民,乡村,试点,新,城乡
7	Industrial Policies	产业,发展,加快,工业,企业,建设,项目,推进,基地,大
8	Gratitude to the Audience	代表,全省,支持,领导,发展,人民,关心,感谢,重建,党中央
9	Cultural Industries	文化,建设,发展,工作,推进,事业,民族,文化产业,活动,公共
10	Business Risk Management	企业,市场,风险,价格,产能,落实,化解,债务,降低,供给
11	Government Effectiveness	政府,建设,工作,群众,人民,落实,服务,提高,行政,能力
12	Economic Challenges	发展,不,还,经济,困难,矛盾,仍,面临,中,清醒
13	Business Environment	发展,经济,市场,机制,创新,社会,企业,环境,改革,完善
14	Agriculture	农业,发展,建设,农产品,粮食,生产,农民,加快,推进,万亩
15	Finance	投资,亿元,支持,金融,企业,融资,资金,项目,发展,金融机构
16	Infrastructure	建设,项目,工程,加快,铁路,开工,建成,推进,投资,机场
17	Pollution Reduction	节能,重点,推进,减排,污染,建设,实施,循环,企业,治理
18	Economic Plans	工作,年,规划,发展,目标,做好,重点,五年,二,代表
19	Ecology	生态,保护,建设,治理,推进,实施,环境,文明,工程,万亩
20	Regional Development	发展,建设,城市,加快,推进,区域,产业,规划,经济,中心
21	International Cooperation	合作,建设,国际,开放,国家,推进,中国,经济,新,试验区
22	Quality of Life	提高,住房,标准,元,保障,生活,制度,社会保障,农村,城乡
23	Higher Level Political Guidance	新,发展,工作,建设,精神,党,习近平,党中央,思想,科学
24	Economic Opportunities	发展,新,经济,更,创新,人民,加快,战略,建设,改革
25	Economic Indicators	增长,亿元,生产总值,收入,年,人均,全省,社会,投资,经济
26	Reforms	改革,推进,深化,制度,企业,管理,审批,行政,试点,体制改革
27	Innovation	创新,科技,人才,建设,企业,实施,技术,国家,加快,中心
28	Government Transparency	政府,行政,工作,监督,接受,决策,推进,制度,件,建设
29	Poverty Alleviation	农村,扶贫,脱贫,工程,实施,万人,亿元,万,民生,解决
30	International Trade and Investment	企业,出口,合作,贸易,服务,市场,投资,建设,发展,加快

Table E2: Within-Topic Analysis (Pre-Specified Number of Topics $R = 35$)

Pre-Specified Number of Topics $R = 35$					
Topic ID	Coefficient of $L_{p,q}$	Standard Error	Topic ID	Coefficient of $L_{p,q}$	Standard Error
1	0.002	(0.076)	21	-0.007	(0.082)
2	-0.119*	(0.071)	22	-0.156***	(0.058)
3	-0.003	(0.071)	23	-0.054	(0.071)
4	-0.057	(0.076)	24	-0.089	(0.067)
5	-0.125*	(0.070)	25	-0.110	(0.071)
6	-0.079	(0.072)	26	-0.108*	(0.062)
7	-0.003	(0.061)	27	-0.093	(0.072)
8	-0.089	(0.078)	28	-0.075	(0.074)
9	-0.057	(0.075)	29	-0.118*	(0.071)
10	-0.106	(0.073)	30	-0.005	(0.069)
11	-0.016	(0.076)	31	0.017	(0.072)
12	-0.094	(0.070)	32	-0.112	(0.074)
13	-0.107	(0.068)	33	-0.112	(0.072)
14	-0.119*	(0.070)	34	-0.032	(0.074)
15	-0.030	(0.077)	35	-0.082	(0.067)
16	-0.107	(0.073)			
17	-0.051	(0.074)			
18	-0.167*	(0.088)			
19	-0.105	(0.075)			
20	-0.112	(0.071)			

Notes: The number of observations is 73,710. The number of pre-specified topics is 35. Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the new report level (i.e. p) and the old report level (i.e. q). Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each government work report, p or q, corresponds to a unique province-year combination. The dependent variable is the economic policy distance reweighted by each phrase's relative importance for each of the 35 topics. The independent variable is the common leader dummy $L_{p,q}$. Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1. The six fixed effects used in the preferred specification, column (3) of Table 1, are controlled as specified in equation (3).

Table E3: Within-Topic Analysis (Pre-Specified Number of Topics $R = 40$)

Pre-Specified Number of Topics $R = 40$					
Topic ID	Coefficient of $L_{p,q}$	Standard Error	Topic ID	Coefficient of $L_{p,q}$	Standard Error
1	-0.030	(0.073)	21	-0.141**	(0.070)
2	-0.067	(0.072)	22	-0.149*	(0.083)
3	0.024	(0.085)	23	-0.072	(0.073)
4	-0.120*	(0.072)	24	-0.168**	(0.065)
5	-0.108	(0.066)	25	-0.110*	(0.065)
6	-0.106	(0.068)	26	-0.075	(0.070)
7	-0.101	(0.073)	27	-0.087	(0.070)
8	-0.095	(0.070)	28	-0.195**	(0.084)
9	-0.096	(0.070)	29	-0.082	(0.073)
10	-0.099	(0.073)	30	-0.052	(0.068)
11	-0.075	(0.074)	31	-0.024	(0.070)
12	-0.015	(0.076)	32	-0.090	(0.072)
13	-0.079	(0.078)	33	-0.127*	(0.070)
14	-0.128*	(0.069)	34	-0.099	(0.076)
15	-0.120	(0.076)	35	-0.050	(0.071)
16	-0.077	(0.071)	36	-0.069	(0.081)
17	-0.011	(0.068)	37	-0.111	(0.072)
18	0.089	(0.066)	38	-0.117*	(0.070)
19	-0.109	(0.072)	39	-0.095	(0.078)
20	-0.100	(0.074)	40	-0.001	(0.074)

Notes: The number of observations is 73,710. The number of pre-specified topics is 40. Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the new report level (i.e. p) and the old report level (i.e. q). Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each government work report, p or q, corresponds to a unique province-year combination. The dependent variable is the economic policy distance reweighted by each phrase's relative importance for each of the 40 topics. The independent variable is the common leader dummy $L_{p,q}$. Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1. The six fixed effects used in the preferred specification, column (3) of Table 1, are controlled as specified in equation (3).

Table E4: Environmental Reform

Dependent Variable	(1) Within-Topic Difference in "Topic 17: Pollution Reduction"	(2) Within-Topic Difference in "Topic 19: Ecology"
$L_{p,q} \times 1\{g(p) = 2015\}$	-0.508***	-0.291*
	(0.104)	(0.170)
$L_{p,q} \times 1\{g(p) = 2016\}$	-0.418***	-0.400***
	(0.138)	(0.150)
$L_{p,q} \times 1\{g(p) = 2017\}$	0.154	0.181
	(0.241)	(0.241)
$L_{p,q} \times 1\{g(p) = 2018\}$	0.269	0.384
	(0.272)	(0.290)
$L_{p,q} \times 1\{g(p) = 2019\}$	0.195	0.258
	(0.165)	(0.237)
Number of Observations	42120	42120

Notes: Following Cameron et al. (2011), standard errors reported in parentheses are two-way clustered at both the new report level (i.e. p) and the old report level (i.e. q). Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each government work report, p or q, corresponds to a unique province-year combination. The dependent variable in column (1) is the within-topic measure of the textual difference between reports p and q on "Topic 17: Pollution Reduction" in Table 6. The dependent variable in column (2) is the within-topic measure of the textual difference between reports p and q on "Topic 19: Ecology" in Table 6. $L_{p,q}$ is the common leader dummy. $g(p)$ is report p's year with the underlying assumption that $g(p) > g(q)$. Each coefficient estimate should be interpreted as how many standard deviations the dependent variable would change as the corresponding independent variable increases from 0 to 1. The six fixed effects used in the preferred specification, column (3) of Table 1, are controlled as specified in equation (3).

Table E5: Examples of the News Headlines Reporting Leaders' Activities

Province	Date	News Headline
Henan	2007-08-31	加强基层组织选准发展路子发展旅游产业建设好新农村 Strengthen grassroots organizations; choose the right development path; develop tourism; build a new countryside
Hunan	2008-03-25	确保实现今年价格调控目标和新型农村合作医疗全覆盖 Make sure that this year's inflation target and the full coverage of the new rural cooperative medical scheme will be achieved
Yunnan	2009-04-03	云南环保产业大有可为 Yunnan's environmental protection industry has a bright future
Zhejiang	2010-01-22	在应对国际金融危机中推进科学发展 Push forward scientific development in response to the global financial crisis
Hubei	2011-03-22	积极拓宽民间投资领域，优化民营经济发展环境 Actively expand the private sector's access to different sectors; improve the business environment for private enterprises
Anhui	2012-04-21	千方百计上项目扩投入，坚定不移调结构促转型 Do everything possible to increase investments; unswervingly adjust industrial structure
Guizhou	2013-10-22	努力推动产业结构转型升级 Promote industrial transformation and upgrading
Fujian	2014-03-20	发挥独特优势，密切闽台合作，进一步提高对台工作水平 Increase cooperation with Taiwan based on Fujian's unique advantages; improve our work related to Taiwan Affairs
Sichuan	2015-09-09	全力推动教育改革发展，努力办好人民满意教育 Advocate the education reform; make education satisfied by people
Shaanxi	2016-05-06	着眼打赢精准脱贫攻坚战，进一步加快移民搬迁步伐 Win the battle against poverty; accelerate the poverty alleviation relocation programs
Gansu	2017-07-13	在"一带一路"建设背景下谋划大项目谋求大发展 Make investment plans and economic development plans based on the Belt and Road Initiative
Guangxi	2018-04-27	精准助力打赢脱贫攻坚战 Provide accurate support for the battle against poverty
Hainan	2019-03-14	以更大力度把自由贸易试验区和中国特色自由贸易港建设推向前进 Push forward the development of pilot free trade zones with Chinese characteristics

Table E6: Topics for the News Headlines Reporting Leaders' Activities

Pre-Specified Number of Topics $R = 30$		
Topic ID	Topic Name	Top Phrases (In Chinese)
1	Economic Plans	发展,抓,经济,确保,增长,目标,好,工作,落实,保
2	Socioeconomic Development	发展,新,建设,提供,推动,保障,科学,推进,贡献,坚强
3	People's Congress	代表团,审议,报告,工作,参加,考察,委员,政府,代表,时
4	Poverty Alleviation	脱贫,攻坚战,攻坚,好,打赢,精准,扶贫,确保,推进,目标
5	Forums	干部,中国,论坛,开幕,领导,大会,举办,国际,培训班,专题
6	Political Speeches	工作,会议,全省,讲话,出席,上,座谈会,副省长,山东省,电视电话会议
7	Industrial Policies	发展,经济,推动,大,加快,产业,做,创新,转型,升级
8	Scientific Outlook on Development	发展,加快,建设,推进,科学,推动,统筹,步伐,城乡,机遇
9	Anti-Corruption Inspections	加大,中央,力度,教育,工作,巡视,办好,整改,反馈,情况
10	Reforms	发展,推进,建设,改革,生态,加快,融合,推动,经济,走
11	Economic Prospects	新,局面,开创,工作,慰问,努力,看望,发展,创新,时代
12	Safety	确保,群众,工作,保障,人民,做好,生产,检查,生活,春节
13	Ideology	精神,大,党,十九,学习,贯彻,十八,届,宣传,工作
14	Policymaking	工作,部署,政府,推进,决策,报告,专项,建设,中央,省委
15	Higher Level Political Guidance	精神,贯彻,学习,总书记,重要讲话,工作,传达,会议,中央,贯彻落实
16	Mass Work	上,群众,工作,再,推动,新台阶,人民,重建,水平,发展
17	Two Sessions	会议,省,省政协,十一届,闭幕,人大常委会,十二届,人大,政协,代表大会
18	Inspections	调研,工作,座谈会,领导,专题,省,省委,民主,生活,省政府
19	Innovation	发展,创新,发挥,作用,优势,充分发挥,科技,改革,推动,中
20	Social Stability	稳定,责任,社会,落实,工作,维护,强化,和谐,确保,环境
21	Urbanization, Environment and Culture	建设,打造,城市,推进,加快,生态,旅游,文化,工程,国际
22	Cooperation	合作,发展,战略,交流,深化,协议,签署,推动,国家,座谈会
23	Anniversaries	周年,成立,座谈会,大会,纪念,暨,讲话,庆祝,表彰大会,出席
24	Xi Jinping Thought	新,时代,中国,思想,社会主义,担当,使命,特色,精神,牢记
25	Solidarity	力量,凝聚,发展,走,前列,聚力,智慧,凝心,弘扬,全国
26	Rural Development	调研,时,农民,农村,农业,建设,推进,增收,发展,工作
27	Government Effectiveness	工作,建设,推进,水平,提高,改革,提升,政治,扎实,党建
28	External Meetings	会见,一行,集团,座谈,调研,领导,代表团,考察,主席,副
29	Ideological Education	活动,学习,实践,教育,科学,发展观,省委,中心组,做,扎实
30	Natural Disasters	工作,防汛,做好,生产,抗旱,全省,调研,检查,确保,部署