

Fiscal Competition and Coordination: Evidence from China

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Preliminary Version

Abstract: This paper intends to provide causal evidence on fiscal competition and coordination between local governments. We first introduce a unique empirical setting created by the combination of two national programs in China, the “Rural School Consolidation (RSC)” program and the “Special Economic Zones (SEZs)” program, in which spatially adjacent townships have discontinuous incentives to either compete or coordinate with a neighboring SEZ. Exploiting such quasi-experimental variation with a spatial-discontinuity design, we find a series of empirical evidence on the existence of competition and coordination between local governments, which can be formally rationalized by a simple model where asymmetric jurisdictions compete for mobile labor. Our results suggest that the strategic interactions among local governments play important roles in public goods provision and labor migration: during the RSC program (2001-2011), townships that coordinate with their neighboring SEZs closed an extra 15% of local primary schools, and lost an extra 7% of local population, as compared to townships that compete with their neighboring SEZs.

Keywords: Tiebout Sorting; Fiscal Externalities; Fiscal Competition; Fiscal Coordination; Rural School Consolidation; Special Economic Zones

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1. Introduction

In his seminal 1956 paper, Charles Tiebout proposed the idea of “Tiebout Sorting,” under which different communities provide different bundles of public goods, and mobile households sort to the communities whose bundles best satisfy their needs. Under the Tiebout framework, inter-jurisdictional fiscal competition leads to an efficient provision of public goods and an optimal allocation of households across different communities. The idea of Tiebout sorting is not unique to labor, and has been generalized to the competition for other production factors, such as mobile capital or the locational choices of firms (White, 1976; Wellisch, 1996). Not surprisingly, the Tiebout results have traditionally been used to support fiscal federalism.

In stark contrast, a large literature on fiscal competition argues the exact opposite: competition among local jurisdictions distort the provision of public goods, or the allocation of production factors, or both. The key insight behind this argument is the existence of fiscal externalities: a local jurisdiction regards the outflow of its production factors as a pure cost, but fails to internalize the potential benefits to other jurisdictions receiving extra production factors due to such relocations. This literature started with tax competition, where competing jurisdictions fail to internalize the positive externalities of their potential capital outflow, therefore tending to set an inefficiently low tax rate (Oates, 1970), and has been generalized to various contexts (Wildasin, 1991; Wilson and Gordon, 2003; Saavedra, 2000). Typically, this literature tends to focus on the inefficiencies associated with inter-jurisdictional competition, and thus advocate for a centralized coordinating policy to internalize the externalities created by resource flows across local jurisdictions.

How empirically relevant is fiscal competition? Would fiscal competition lead to a higher or lower level of public good provision, as compared to fiscal coordination? How different is the allocation of production factors under these two regimes? Rigorous answers to these questions are crucial for the evaluation of different theoretical frameworks, and even more importantly, central to the long-standing debates on fiscal federalism.

Identifying causal evidence on the existence and implications of fiscal competition and coordination is difficult, mainly due to the lack of a good counterfactual: jurisdictions that compete and those that coordinate are usually also different in many other unobservable ways. Therefore, despite rich suggestive evidence on the existence and implications of these two opposite regimes, a credible causal interpretation

is still largely missing in the literature.

In this paper, we intend to contribute to the empirical studies on fiscal competition, by providing causal evidence on the existence and implications of fiscal competition and coordination, using a unique empirical setting created by two novel national programs in China.

The first national program is the massive construction of “Special Economic Zones (SEZs),” under which certain rural counties are selected by prefecture city governments, and given policy priorities to quickly become urbanized and industrialized. China built more than 1300 SEZs since the 1980s, and within each prefecture city (henceforth, city),¹ there exists at least one SEZ. Past studies have established that the SEZs have significantly higher labor productivity than their neighboring agricultural counties (Wang, 2013; Cheng 2015), and the successful construction of a SEZ is highly rewarding for the prefecture city it belongs to, both politically and economically.

The second national program is the “Rural School Consolidation (RSC)” movement in China, under which the prefecture city governments were encouraged to rearrange the distribution of primary schools within their constituency, by closing small primary schools and consolidating them into larger schools, to realize economy of scale in education. Within one decade, under the RSC program, China closed more than 50% of its rural primary schools, which led to severe social problems and heated debates. Generally believed to have been over-implemented, the RSC program was terminated by the central government in 2012.

The combination of these two national programs creates an ideal empirical setting to identify the causal effects of fiscal competition and coordination, as illustrated in figure 1. In this setting, the SEZ lies on the border of prefecture cities A and B, and is more productive than its neighboring counties. The SEZ belongs to city A, and in its neighborhood, some counties (treated) belong to the same prefecture city, A; while other counties (control) belong to a different prefecture city, B.

Before the RSC program started, the provision of public goods was highly decentralized at the county level, so the treated and control counties each made their own decisions on local public goods, and neither of them would internalize the positive externalities of their labor flowing to the (more productive) SEZ. Therefore, the treated counties, the control counties, and the SEZ engaged in fiscal competition over local public good provision, inducing the reallocation of mobile labor. Because the treated and control counties

¹ In the Chinese system, prefecture city is the level of government between province (comparable to state in the U.S. system) and county (comparable to county in the U.S. system).

are spatially adjacent, and they make the same competitive decision (provide high levels of public good to prevent labor from leaving for the SEZ), we expect that before the RSC started, the treated and control counties were well balanced in every measure, including the levels of public good provision and population outflow.

After the RSC program started, the provision of primary schools became centralized at the prefecture city level. Since prefecture city A owns the SEZ, it internalizes the positive externalities of migration flows from the treated counties to the SEZ. Therefore, the leader of prefecture city A now makes a joint decision on primary schools for both the treated counties and the SEZ, and he has the incentives to coordinate them: by providing fewer primary schools in the treated counties, more labor would concentrate to the SEZ, increasing aggregate production for prefecture city A. In contrast, since prefecture city B does not own the SEZ, it does not internalize the positive externalities of migration flows from the control counties to the SEZ. Therefore, the leader of prefecture city B makes a decision on primary schools only for the control counties, and he still has incentive to compete with the SEZ: by providing more primary schools in the control counties, fewer workers would leave city B for the SEZ, increasing production for prefecture city B. Due to the opposite incentives of prefecture cities A and B, after the RSC program started, there should emerge a discontinuous gap between the spatially adjacent treated and control counties: the treated counties should close more schools and lose more population, as compared to the control counties.

Since the RSC program only applies to primary schools but not to other types of public good, even after the RSC started, the provision of other types of local public good remains decentralized at the county level. Since both the treated and control counties still do not internalize the benefits of the SEZ, they keep on making competitive decisions for those other types of public good. While the differential amounts of primary schools imposed by the prefecture city governments might have indirect impacts on the decision for other types of public good, since the county governments have control over many other different types of public good, such indirect impacts would be split and attenuated between all those other types of public good. Therefore, for each specific type of local public good (other than primary schools), the impact of the RSC program is expected to be minimal, which means that other types of public good would likely remain balanced between the treated and control counties.

In addition, given that economic growth is a key factor in the Chinese meritocracy system, politicians with stronger promotion incentives will likely care more about local GDP. Therefore, during the post-RSC period, if the leader of prefecture city A has strong promotion incentives, we would expect to see a higher

level of coordination between the treated counties and the SEZ, which means a higher level of primary school closure in the treated counties; if the leader of prefecture city B has strong promotion incentives, we would expect to see a higher level of competition between the control counties and the SEZ, which means a lower level of primary school closure in the control counties.

To formalize the aforementioned intuitions, we present a simple model of asymmetric fiscal competition, which mimics our empirical setting, and transforms the intuitions into four key testable hypotheses: (1) before the RSC started, the treated and control counties should be well balanced in every measure; (2) after the RSC started, the treated counties closed more primary schools and had more population outflow than the control counties; (3) after the RSC started, except for primary schools, other types of local public good remain balanced between the treated and control counties; (4) after the RSC started, if the leader of prefecture city A has strong promotion incentives, the treated counties would close even more primary schools; if the leader of prefecture city B has strong promotion incentives, the control counties would close even fewer primary schools.

Using a unique panel dataset of Chinese townships,² we apply a spatial discontinuity design as illustrated in figure 1, and find strong empirical evidence consistently confirming all four testable hypotheses. In addition, we also adopt various alternative specifications and placebo tests to check for robustness, explore for heterogeneity, and rule out competing explanations.

This paper relates to several strands of literature.

First and foremost, it contributes to the large literature on fiscal competition and strategic interactions among local governments, which falls more generally into the literature on decentralization and fiscal federalism. Tiebout (1956) first argues that mobile households can lead to regional competition that improves welfare, and this argument was later extended to mobile capital and firms (White, 1976; Wellisch, 1996). Using a case of tax competition, Oates (1972) shows that inter-governmental competition can lead to distortions in the public sector (tax rate in his case) and misallocation of the production factor (capital in his case). This idea was later formalized (Zodrow and Mieszkowski, 1986), and extended to many other forms of competition, including income redistribution (Wildasin, 1991), government expenditure (Wilson and Gordon, 2003), environmental policies (Fredriksson and Millimet, 2002), and welfare transfers (Saavedra, 2000). It has also been argued that regional competition may increase the provision of public

² The county dataset does not contain information on primary schools, so our unit of analysis is at the township level, which in the Chinese system is a smaller unit below county.

good by “taming Leviathan governments” (Brennan and Buchanan, 1980), which is consistent with various stylized facts pointed out in the literature (Oates, 1985; Oates, 1989). While there exists a large body of theoretical literature with different forms and implications, solid causal evidence directly testing these models are surprisingly inadequate. As pointed out by Brueckner (2003), existing empirical literature typically justifies the existence of regional competition by testing some indirect implications of the theoretical models, and usually suffers from serious endogeneity problems. Our paper intends to estimate the causal effects of fiscal competition and coordination on public good provision and labor allocation. We address the identification threats mentioned in Bruckner (2003) using a spatial discontinuity design, and find that when governments are Leviathan, competition would increase public good supply and decrease the level of migration, which are consistent with the propositions of Brennan and Buchanan (1980).

Second, this paper adds to the empirical tests of the Tiebout Sorting hypothesis. Since Tiebout (1956), there has been continuous efforts trying to justify Tiebout Sorting empirically. Earlier works generally focus on testing some indirect implications of the model, including capitalization and hedonics (Oates, 1969; Rosen 1974), efficient provision of public good (Brueckner, 1982), stratification of demand for public goods and the link between diversity in income and provision of public good (Epple and Sieg, 1999; Rhode and Strumpf, 2003). More recently, the empirical literature tends to directly test the Tiebout mechanism using quasi-experimental changes in public goods, for example, among many others, Urquiola (2005) shows that inter-district school choice leads to sorting, Banzhaf and Walsh (2008) finds that people sort toward communities with increased air quality. In the context of China, a paper closely related to ours is Xing (2014), which uses individual-level census data to show that in villages where the RSC program is implemented (local primary school is closed), parents with school-aged children become much more likely to migrate out and seek for external schooling opportunities for their children. Although not its main focus, our paper also shows that when RSC program is implemented, students, teachers, and parents would sort after schools, which confirms the Tiebout hypothesis in the context of China.

Third, this paper also speaks to the literature documenting China’s state-led style of urbanization. The central government of China always had the temptation to boost urbanization in the country (Fan et al., 2012), and it has been estimated that the revenues from converting agricultural land into industrial use could account for 60-80% of the total government revenues in many parts of China (Zhu, 2005). As a result, promoting urbanization becomes highly profitable for the local governments, both politically and

economically, and is highly prioritized by the local officials (Han and Kung, 2015). Among many attempts to promote urbanization, constructing SEZs has been proved highly successful in many regions (Wang, 2013), but there are also cases of failure where the SEZs could not concentrate enough population and ended up being “ghost townships” (Chen and Li, 2015). This paper documents that the relocation of public good is an effective method through which many local governments boost urbanization for their SEZs, which is new to this literature, and could help improve our understanding of the patterns and welfare implications of the state-led urbanization in China.

The remaining parts of this paper are organized as follows. Section 2 discusses the empirical context, provides qualitative evidence, and presents the identification strategy. Section 3 presents a simple model of asymmetric fiscal competition, which mimics the empirical setting, and formalizes the intuitions into four main testable hypotheses. Section 4 introduces the three datasets used in the paper. Section 5 formally examines the four testable hypotheses, and presents the main findings of the paper. Section 6 shows additional analysis to check for robustness, explore heterogeneity, and rule out alternative explanations. Section 7 concludes.

2. Context

The empirical context of this paper is created by the combination of two national programs in China, the massive construction of the “Special Economic Zones (SEZs),” and the “Rural School Consolidation (RSC)” movement, which we introduce in sections 2.1 and 2.2, respectively. In section 2.3, we summarize anecdotal evidence on the interactions between these two national programs, which provides qualitative support for the existence of fiscal competition and coordination in our context. In section 2.4, we present the spatial-discontinuity design, which is the main identification strategy of this paper.

2.1. Special Economic Zones

As a place-based industrial policy tool, various kinds of Special Economic Zones (SEZs) have been constructed in many different countries, and overall, they show a mixed record of success (Farole & Akinci, 2011).

In the case of China, SEZs are usually county-level units based on land retired from agricultural production, which are given tax cuts and other preferential policies by the corresponding prefecture city governments to attract FDI and domestic investments.

The Chinese experience with SEZs has been highly successful: Wang (2013) finds that SEZs bring higher levels of FDI to the corresponding prefecture cities in general, without crowding out domestic investment; Cheng (2014) finds that transforming an agricultural county into a SEZ increases local GDP level by about 6 percent after 5 years; Alder et al. (2015) find that the establishment of a SEZ is associated with an increase in the level of per capita GDP by about 18% for the host prefecture city.

Since the promotion of local officials largely depends on their ability to boost economic growth (Li and Zhou, 2005), there also exists high political returns to the successful construction of a SEZ. Driven by the high economic and political benefits, China constructed more SEZs than any other country in the world (Boyenge, 2007). From 1980 to 2006, more than 1300 SEZs were built, with every prefecture city constructing at least one SEZ.

As shown in the existing literature, due to reasons such as higher physical capital, agglomeration effects, tax cuts, and better technology, the marginal product of labor is significantly higher in the SEZs than in their neighboring counties, for both high-skilled and low-skilled workers (Wang, 2013; Alder et al., 2015).

2.2. Rural School Consolidation

The “One-Child Policy” in China led to steady decrease of school-aged children in rural areas since the 1980s, and the massive rural-urban migration further exacerbated this problem. Yet, more rural primary schools were constructed during the 1980s and 1990s under the “Compulsory Education” policy. As a joint result of these three forces, by 2000, there was an obvious surplus of rural primary schools, and some schools could hardly enroll enough students to be sustainable.

To make efficient use of educational resources and realize economy of scale in education, in 2001, the central government issued the *Decisions on the Reform and Development of Basic Education*, which required the prefecture city governments to close small rural primary schools and consolidate them into larger schools. While starting from good intentions, this “Rural School Consolidation (RSC)” program was over-implemented by the local governments: in only one decade, China closed more than 50% of its rural primary schools,³ which means closing more than 4 schools every hour, and has continued over ten years. Not surprisingly, such ultra-large number of school closure far exceeded the magnitude of the shortage

³ From 553622 in 2000 to 257410 in 2010.

of rural students, and caused a series of negative aftermath for rural families: increased distance from home to schools, more frequent school bus accidents in rural areas, the number of boarding schools insufficient to meet increased boarding demands, higher schooling costs for rural families, and higher dropout rates, etc (Yang, 2012). These problems have been pointed out since 2005, and the central government warned the local governments twice on not over-implementing the RSC program, first in 2006 and again in 2010, which, however, failed to slow down the striking trend of school closure in the rural areas. As a result, the central government chose to terminate the RSC program in 2012, and specifically emphasized that rural schools should be protected from being closed by local governments.⁴

It is critical to note that before the RSC program started, the provision of rural education, like other types of local public good, was highly decentralized: decisions were made almost exclusively at the county and lower levels, and the prefecture city government had very limited ability to interfere;⁵ however, after the introduction of the RSC program, the prefecture city government suddenly obtained the authority to manage the distribution of primary schools across different counties. Therefore, the RSC program could be regarded as introducing a planner to make a joint decision on primary schools for all the counties in the same prefecture city. Meanwhile, since the RSC program only applies to primary schools, other types of public good are not directly influenced by this program.

2.3. Qualitative Evidence on the Interactions between SEZ and RSC

There exists rich anecdotal evidence from various sources that documents the interactions between the SEZ program and the RSC program: after the RSC program started, the prefecture city governments obtained the power to rearrange the distribution of primary schools across different counties within their constituency. To induce agricultural labor to migrate to their own SEZs, which have higher marginal products of labor, the prefecture city governments intentionally closed schools in the neighborhoods of their SEZs, and consolidated their resources into the schools in the neighboring SEZs.

In their guidelines on the implementation of the RSC program, nearly all the provinces emphasize the intention of using the RSC program to promote urbanization (for examples, see documents of Hebei Province, Fujian Province, and Henan Province).⁶ A number of media reports also confirm that

⁴ For more details, see the State Council (2006, 2010, 2012).

⁵ For more discussion on the decentralized provision of public goods in rural China, see Wang (2008), Sato (2008), and Martinez-Vazquez et al. (2008).

⁶ Hebei: <http://www.fnjy.net/Item/5427.aspx>, 04-29-2006

prefecture city governments utilize rural school consolidation to concentrate students and their families into the SEZs to promote urban and industrial development (see examples in Chengdu, Shuyang, and Lishui).⁷ In recent evaluations of the RSC program that were published on the state-owned media, researchers argue that many local governments have used the RSC program as an important tool to induce rural-urban migration.⁸

Specifically, in a media interview with the leader of the education department of Shou County in Anhui Province, the local official clearly indicated that he was instructed by the upper-level officials to “close schools in the old regions and move them to the new SEZ.” The county government thus closed the best schools in the old region, although the RSC program was intended to close only those small and dilapidated schools.⁹ There are plenty of similar records in other regions of China.

In a government report on the RSC program of Yulin City in Shaanxi Province, local officials found that among all the migrant workers, more than 30% chose to migrate because their children’s needs for schooling could no longer be satisfied in their hometown, which suggests that access to education is an important factor that affects people’s migration decisions in the context of rural China. This observation is also supported quantitatively by Xing (2014), who uses nationally representative datasets and finds that in villages where primary schools are closed, parents with school-aged children are much more likely to become migrant workers.

2.4. Identification Strategy

The identification strategy is based on a spatial-discontinuity design, as illustrated in figure 1. Prefecture cities A and B are next to each other, there exists a SEZ on the border of the two prefecture cities, and it belongs to prefecture city A. Among the neighboring counties of this SEZ, some belong to the same prefecture city, A; others belong to the other prefecture city, B. We define those counties that are “in the neighborhood of the SEZ and belong to the same prefecture city” as “treated counties,” and those that

Fujian: http://www.fjshjy.net/zcfg/zcwj/201012/t20101209_74634.htm, 11-22-2010

Henan: <http://xsaq.haedu.cn/2010/10/12/1286866728093.html>, 12-17-2010

⁷ Chengdu: http://www.jyb.cn/basc/ts/201005/t20100513_359799.html, 05-13-2010

Shuyang: http://paper.people.com.cn/jnsb/html/2007-09/13/content_19716392.htm, 09-13-2007

Lishui: <http://www.jslszx.gov.cn/ReadNews.asp?NewsID=1712>, 07-23-2010

⁸ See examples in “Pay attention to the negative impacts of rural school consolidation,” Guangming Daily, http://epaper.gmw.cn/gmrb/html/2015-05/24/nw.D110000gmrb_20150524_3-08.htm, 05-25-2015; “Misconceptions in the rural school consolidation policy,” China Education Daily, http://www.jyb.cn/basc/xw/201109/t20110929_455976.html, 09-29-2011

⁹ http://old.shouxian.gov.cn/contents/topic_view.php?id=169

are “in the neighborhood of the SEZ but belong to a different prefecture city” as “control counties.”

The treated and control counties are spatially adjacent to each other, so if the regional characteristics change smoothly over space, the treated and control counties should be balanced in these measures. Since the treated and control counties are all in the neighborhood of the same SEZ, they face the same spill-over effects from the development of this SEZ. Given that every prefecture city has a SEZ, it is also reasonable to assume that prefecture city A and prefecture city B are not systematically different.¹⁰ Before the RSC program started, the provision of local public goods was determined at the county level, and neither the treated nor the control counties would internalize the positive externalities of their labor flowing to the SEZ, so the treated and the control counties would make the same competitive decisions: they would all provide high levels of public goods to prevent their labor from moving to the SEZ. All these reasons combined together, we expect that the treated and control counties should be well-balanced in every measure before the RSC program started.

After the RSC program started, the leader of prefecture city A now makes a joint decision on primary schools for both the treated counties and the SEZ, so he internalizes the externalities associated with labor moving from the treated counties to the SEZ. In contrast, the leader of prefecture city B makes a decision on primary schools only for the control counties, so he does not internalize the externalities associated with labor moving from the control counties to the SEZ. That is to say, in terms of the provision of primary schools, there exists “coordination” between the treated counties and the SEZ, but “competition” between the control counties and the SEZ. Therefore, after the RSC program started, there should emerge a discontinuous gap between the treated counties and the SEZ, and the magnitude of this gap measures the causal effect of changing from a “competitive regime” to a “coordinative regime.”

Since the RSC program only applies to primary schools but not to any other types of public good, as a placebo, we should not observe the same magnitudes of impacts on other types of public good. Also, since politicians with different promotion incentives might have different objective functions, we expect to observe heterogeneity in competition and coordination among politicians with stronger promotion incentives.

3. Model

¹⁰ Nonetheless, we do provide direct tests in section 6.2 showing that even if there exists differences between cities A and B, such differences could hardly be driving the main results of the paper.

In this section, we formalize the aforementioned intuitions using a simple model of asymmetric fiscal competition, where labor is the mobile production factor, and governments are Leviathan (rent-maximizing). By comparing jurisdictions that make competitive decisions to those that make coordinated decisions, we derive our four main testable hypotheses.

3.1. Setup

This model modifies the classic models of income transfer competition (Wildasin, 1991) and asymmetric tax competition (Bucovetsky, 1991) to mimic our empirical setting illustrated in figure 1. In our setup, as shown in figure 2, we assume that there are three jurisdictions, indexed by 1, 2, and 3, corresponding to the “treated county,” the “SEZ,” and the “control county,” respectively.

The production function of jurisdiction i takes a quadratic form: $f_i(l_i) = \alpha_i \cdot l_i - \beta \cdot l_i^2$, where l_i stands for the population in jurisdiction i . By assumption, jurisdictions 1 and 3 are identical, while jurisdiction 2 is more productive: $\alpha_2 > \alpha_1 = \alpha_3$.¹¹

Workers are freely mobile across jurisdictions. A worker in jurisdiction i earns a wage equal to the local marginal product of labor: $w_i = f'_i(l_i) = \alpha_i - 2 \cdot \beta \cdot l_i$. An individual in jurisdiction i consumes a numeraire good (equals to income w_i) and the local level of public good g_i , the utility function is linear and separable: $u_i = w_i + g_i$.

In equilibrium, every worker must be indifferent between the three jurisdictions:

$$f'_1(l_1) + g_1 = f'_2(l_2) + g_2 = f'_3(l_3) + g_3 \quad (1)$$

and labor in all jurisdictions must add up to total population:

$$l_1 + l_2 + l_3 = L \quad (2)$$

Equations 1 and 2 are similar to the conditions derived in the traditional tax competition models, and could fully characterize the allocation of labor among the three jurisdictions.

Combining equations 1 and 2, we derive the population of every jurisdiction as a function of both its own public good provision, and the levels of public good in the other two jurisdictions:

$$l_1 = \frac{L}{3} + \frac{(2 \cdot g_1 - g_2 - g_3) + (2 \cdot \alpha_1 - \alpha_2 - \alpha_3)}{6 \cdot \beta} \quad (3)$$

$$l_2 = \frac{L}{3} + \frac{(2 \cdot g_2 - g_1 - g_3) + (2 \cdot \alpha_2 - \alpha_1 - \alpha_3)}{6 \cdot \beta} \quad (4)$$

¹¹ This assumption means that if each jurisdiction has the same amount of labor, jurisdiction 2 would then have a higher marginal product of labor.

$$l_3 = \frac{L}{3} + \frac{(2 \cdot g_3 - g_1 - g_2) + (2 \cdot \alpha_3 - \alpha_1 - \alpha_2)}{6 \cdot \beta} \quad (5)$$

The cost of providing public good g_i in jurisdiction i is $c(g_i) = c \cdot g_i^2$, and jurisdictional governments are Leviathan: they maximize their own rent, which equals to total production minus total wage and total cost of public good provision: $R_i = f_i(l_i) - f_i'(l_i) \cdot l_i - c(g_i)$. This rent could be understood as a general form of “government revenue.”

3.2. Case 1: Each Jurisdiction Making its Own Decision (Pre-RSC)

Case 1 characterizes the situation before the RSC program starts: each jurisdiction chooses its own level of public good g_i to maximize its own rent, without internalizing any other jurisdiction’s benefits. The three jurisdictions engage in fiscal competition, and each jurisdiction i solves:

$$\max_{g_i} R_i = f_i(l_i) - f_i'(l_i) \cdot l_i - c(g_i)$$

First order conditions give $l_i = 3 \cdot c \cdot g_i$ for each jurisdiction. Plugging equations (3), (4), and (5) into the three first order conditions, it is straightforward to show that:

$$g_2 > g_1 = g_3 \quad \text{and} \quad l_2 > l_1 = l_3.$$

3.3. Case 2: Jurisdictions 1 and 2 Making a Joint Decision (Post-RSC)

Case 2 characterizes the situation after the RSC program starts: a planner is introduced to make a joint decision for jurisdictions 1 and 2 (corresponding to the leader of prefecture city A in the empirical setting), so he internalizes all the externalities caused by labor relocating between these two jurisdictions. Meanwhile, jurisdiction 3 still makes its own decision (corresponding to the leader of prefecture city B in the empirical setting), and still does not internalize the externalities of the outflow of its labor.

The planner of jurisdictions 1 and 2 chooses the levels of public good in these two constituencies to maximize the joint rent:

$$\max_{g_1, g_2} R_1 + R_2 = f_1(l_1) + f_2(l_2) - f_1'(l_1) \cdot l_1 - f_2'(l_2) \cdot l_2 - c(g_1) - c(g_2)$$

leading to the first order conditions:

$$2 \cdot l_1 - l_2 = 6 \cdot c \cdot g_1, \quad \text{and} \quad 2 \cdot l_2 - l_1 = 6 \cdot c \cdot g_2$$

Jurisdiction 3 chooses the local level of public good to maximize its own rent:

$$\max_{g_3} R_3 = f_3(l_3) - f_3'(l_3) \cdot l_3 - c(g_3)$$

leading to the first order condition:

$$l_3 = 3 \cdot c \cdot g_3$$

Plugging equations (3), (4), and (5) into the three first order conditions, we obtain the reaction function (in terms of public good provision) of each jurisdiction. Combining the three reaction functions, we can show that:

$$g_2 > g_3 > g_1, \text{ and } l_2 > l_3 > l_1.$$

3.4. Extension: Other Public Goods

The RSC program only applies to primary schools, but not any other types of public good. To mimic this empirical feature accurately, in this section, we extend the baseline model to incorporate other types of public good, and explore how they would be affected by the RSC program.

In addition to g_i (primary schools), assume there are n other types of public good, and for computational tractability, assume that all the other public goods are identical, therefore the level of other public goods in jurisdiction i could be written as $n \cdot k_i$. Assume for simplicity that other types of public good are perfect substitutes of primary schools, the individual utility function then becomes $u_i = w_i + g_i + n \cdot k_i$, we could modify equations (1) and (2) to solve for the population of each jurisdiction at given levels of public goods.¹²

Given the extended setup, we could repeat the analysis of the cases in sections 3.2 and 3.3, to produce testable hypotheses for other public goods during the pre-RSC period and the post-RSC period, respectively.

Prior to the RSC program, each jurisdiction makes its own decision on g_i and k_i . Solving the maximization problem, given the symmetry assumptions for jurisdictions 1 and 3, $g_1 = k_1 = g_3 = k_3$ is trivially satisfied.

After the RSC program is implemented, a planner makes a joint decision on g_i for both jurisdictions 1 and 2, another planner makes a decision on g_3 only for jurisdiction 3; and each jurisdiction then separately makes its own decision on k_i . Solving the problem for the two planners and three jurisdictions altogether, we derive:

¹² Note that the “identical” and “perfect substitution” assumptions are stronger than what we need, they are made to simplify computations. To derive the same qualitative implications, we only need to assume that “no other type of public good has specific substitutability or complementarity with primary schools.”

$$k_3 = \frac{l_3}{3 \cdot n \cdot c} > \frac{l_1}{3 \cdot n \cdot c} = k_1,$$

which leads to:

$$\frac{k_3 - k_1}{g_3 - g_1} = \frac{1}{(c-1) \cdot n}.$$

Same as in the baseline model, $g_3 - g_1 > 0$ still holds; but the sign of $k_3 - k_1$ is ambiguous: it is positive if $c > 1$, and negative if $c < 1$. However, so long as $c \neq 1$, when n becomes large enough, $\frac{k_3 - k_1}{g_3 - g_1}$ converges to zero.

Since in reality there are many different kinds of public good provided at the county level, it is likely that n is a large number, which means that for each specific type of public good other than primary schools, the impact of the RSC program is indirect and largely attenuated. Therefore, the difference in other public goods between jurisdiction 1 (treated counties) and jurisdiction 3 (control counties) would likely remain minimal, even after the RSC program is implemented.

3.5. Extension: Political Incentives

In the baseline model, we assumed that all governments are rent-maximizing, which means there is no heterogeneity in political incentives. However, it is possible that when the leader of prefecture city A (B) has strong incentives for promotion, he will exert more efforts in coordination (competition), which means closing (opening) more primary schools in the treated (control) counties. To allow for such heterogeneity, in this section, we incorporate political incentives into our baseline model.

Specifically, we modify the objective function of a politician as a weighted average of rent and GDP:

$$\pi_i = (1 - \theta) \cdot R_i + \theta \cdot Y_i = f_i(l_i) - (1 - \theta) \cdot f'_i(l_i) \cdot l_i - (1 - \theta) \cdot C(g_i)$$

where $\theta \in (0,1)$ is the measure of political incentives. When θ is larger, the politician cares more about promotion, so he puts a higher weight on GDP and a lower weight on rent, when making his decisions.

With this modified objective function, we repeat the optimizations of case 2 (section 3.3), under certain regularity conditions, we derive the comparative statics for public good provision:¹³

$$\frac{dg_1}{d\theta} < 0, \quad \frac{dg_3}{d\theta} > 0.$$

Intuitively, these results indicate that, if the planner for jurisdiction 1 and jurisdiction 2 has strong

¹³ The cost of public good provision (c) must be larger than a lower bound. Due to length limits, details of this proof are not included in this draft, but are available upon request.

promotion incentives, he would provide a lower level of public good in jurisdiction 1; if the planner for jurisdiction 3 has strong promotion incentives, he would provide a higher level of public good in jurisdiction 3.

3.6. Summary of Testable Hypotheses

In the empirical setting illustrated in figure 1, the theoretical results could be summarized as four main testable hypotheses:

- (1). Before the RSC program started, the treated counties and the control counties should be well balanced in every measure.
- (2). After the RSC program started, the treated counties close more primary schools and lose more population than the control counties.
- (3). After the RSC program started, except for primary schools, any other types of public good should remain balanced between the treated counties and the control counties.
- (4). If the leader of prefecture city A has strong promotion incentives, there will be a higher level of school closure in the treated counties; if the leader of prefecture city B has strong promotion incentives, there will be a lower level of school closure in the control counties.

These four testable hypotheses are used to guide the empirical investigations.

4. Data

We combine datasets from three different sources.

First, we have a novel dataset based on the “Township Conditions Survey (TCS)” conducted by the National Bureau of Statistics.¹⁴ The TCS is a longitudinal survey conducted yearly since 2001, which covers all the 40906 townships in China. The TCS data includes a rich set of variables that are useful for our study, such as the number of schools, number of students, number of teachers, education expenditure, population, GDP, government expenditure, number of government officials, agricultural production, industrial output, etc. The TCS also provides detailed information on many different types of local public good, including hospitals, kindergartens, electricity, roads, water and sanitation, libraries, etc. The TCS is unique because it is the only dataset that has a complete coverage nationally at such a low administrative

¹⁴ The county level administrative dataset does not include information on primary schools, so our unit of analysis is at the township level, which is a smaller administrative unit than the county.

level, and to our best knowledge, this dataset has never been accessed for research before. By agreement, we have obtained access to the TCS data for all the townships in 20 major provinces from 2001 to 2011,¹⁵ which perfectly overlaps with the timing of the Rural School Consolidation program. One potential caveat of the TCS data is that in some years it surveyed only some of the townships, while in other years it surveyed all of them, so selective attrition might be an issue. To address this concern, in addition to using the whole sample for analysis, in every table showing regression results, we present one column using only a balanced panel, and as we will see, the key results are essentially the same.

Second, we make use of rich information from the GIS maps of China. We first collect from administrative files a complete list of all the 1380 Special Economic Zones (SEZs) in China, as well as the type, area, level, and year of establishment for each of them. Then using GIS tools, we are able to pin down the geographic location of every SEZ, and the location of every county in China, and thus calculate the distance between every county-SEZ pair. Based on the GIS information, we could identify all the counties in the neighborhood of every SEZ, and categorize them into two types: those in the same city as the SEZ, and those in a different city as the SEZ. Matching this geographical information with the aforementioned TCS dataset constitutes the main dataset for this paper. The distribution of treated and control counties are shown in figure 3, which are all the cases that follow the spatial discontinuity pattern illustrated in figure 1.¹⁶

Third, to examine the fourth testable hypothesis of the model, we also make use of a prefecture city-level panel dataset on the Prefectural Party Secretaries (PPSs) of China from 2000 to 2010.¹⁷ This dataset covers a total of 989 PPSs from 333 prefecture cities in 27 provinces, documenting detailed personal information including name, age, gender, nationality, education, experience, etc. Matching this dataset with the main dataset described above, we obtain an unbalanced panel covering 2737 townships from 2001 to 2010.

5. Empirics: Testing the Model

In this section, we formally examine the four testable hypotheses of our model: (1) before the RSC started,

¹⁵ There are 31 provinces, 2856 counties, 40906 townships in China.

¹⁶ Some counties are in the neighborhood of a SEZ, but excluded from the sample, because they are urban districts, and are not affected by the RSC program.

¹⁷ PPS is the highest level of government official at the city-level. For more details on the construction of the PPS dataset, see Chen (2015).

the treated and control townships should be balanced in every measure; (2) after the RSC started, the treated townships close more primary schools and lose more population than the control townships; (3) after the RSC started, except for primary schools, other types of local public good remain well-balanced between the treated and control townships; (4) when the leader of prefecture city A has strong promotion incentives, the treated townships close more primary schools, when the leader of prefecture city B has strong promotion incentives, the control townships retain more primary schools.

5.1. First Hypothesis: Balance Prior to RSC

Table 1 presents pre-treatment (before the RSC started) differences in means among key variables between the treated townships and the control townships. Following Imbens (2015), we use the “normalized differences” instead of the t-statistic as the measure of overlap,¹⁸ which is independent of sample size.

The normalized difference is below 0.18 for all variables, below 0.12 for most of the variables, and even lower for the key outcome variables of interest. According to Imbens (2015), a normalized difference below 0.25 could be considered as “well balanced” between two groups. Hence, there is no statistically significant difference in any measure between the treated and control townships before the RSC started, and the key outcome variables (schools, students, teachers, population, employment) are especially well-balanced, supporting the first testable hypothesis of our model.

5.2. Second Hypothesis: Coordination Leads to More School Closure and Population Outflow than Competition

The second hypothesis indicates that after the RSC started, the treated townships should close more primary schools and lose more population. Specifically, since we expect that the people sorting after primary schools are mainly primary students, their parents, and primary school teachers, our empirical investigations focus on identifying the post-RSC differential trends between the treated and control townships in 5 key outcome variables: primary schools, primary students, primary school teachers, population, and employed population.

5.2.1. Econometric Model

¹⁸ Defined as difference in mean values over standard deviation: $(\mu_{xt} - \mu_{xc}) / \sqrt{(s_{xc}^2 + s_{xt}^2) / 2}$, where μ_{xt} and μ_{xc} are the sample averages of x in the treated and control groups, respectively; s_{xt}^2 and s_{xc}^2 are the sample variances of x in the treated and control groups, respectively.

The sample for analysis includes only townships in the neighboring counties of the borderline SEZs. Define the key variable $treatment_i$ as equal to 1 if township i lies in a treated county (in the same prefecture city as the neighboring SEZ), and 0 if township i lies in a control county (in a different prefecture city as the neighboring SEZ). The estimation equation is:

$$y_{ist} = \alpha \cdot treatment_i \cdot t + X'_{it} \cdot \beta + \lambda_{st} + \mu_i + \varepsilon_{ist} \quad (6)$$

where y_{ist} is the outcome of interest in township i , in the neighborhood of SEZ s , in year t . X'_{it} is a vector of control variables measuring local characteristics, λ_{st} is SEZ-Year fixed effect, which restricts the variation being used for estimation to be within each SEZ neighborhood to increase the precision. μ_i is township fixed effect, ε_{ist} is the error term.

Since the township data ranges from 2001 to 2011, which perfectly overlaps with the implementation of the RSC program, the coefficient α of the interaction term $treatment_i \cdot t$ would then capture the break in trends between the treated and control townships during the post-RSC period. According to our second testable hypothesis, α should be negative and significant, for all five key outcome variables (primary schools, primary students, primary school teachers, population, and employment).

To address concerns about spatial correlation among adjacent townships, we present standard errors clustered at the SEZ level; moreover, to address the concerns that non-adjacent townships within the same city could also be spatially correlated, we also present alternative standard errors that are two-way clustered at the City-SEZ levels (Cameron and Miller, 2015).¹⁹

5.2.2. Results

Table 2 presents the main results for primary schools. It shows that α is always negative and significant, which is consistent with our hypothesis: after the RSC started, because the treated townships coordinate with the SEZ, while the control townships compete with the SEZ, there emerges a negative gap in primary schools between the treated and control townships.

Column 1 controls for township fixed effect and year fixed effect, and indicates that on average a treated township closes 0.225 more primary schools every year than a control township. This could explain the closure of more than 13% of all primary schools during the 11 years that the RSC program was implemented.

Column 2 is the preferred specification, it further controls for SEZ-by-Year fixed effect. By doing

¹⁹ Each SEZ cluster includes all the townships in the neighboring county of this SEZ, each City cluster includes all the townships in that prefecture city.

so, we restrict the variation used for estimation to be within each SEZ cluster, thus could avoid making comparisons between treated and control observations that are actually far away from each other, which is a typical problem due to dimension reduction in many traditional Spatial-RD designs (Magruder, 2012). The magnitude of the estimated effect is even slightly larger than that of column 1: an average treated township closes an extra 2.9 primary schools in 11 years, which is more than 15% of the total amount of primary schools in an average township.

In column 3, we further include Province-by-Year fixed effect, and the point estimate is qualitatively the same, but smaller in magnitude. A potential explanation for the drop in magnitude is that there exists some level of cooperation between prefecture cities in the same province, and relatively less cooperation (or more fierce competition) between prefecture cities from different provinces, so the negative gap in public good provision should be even larger when prefecture city borders coincide with province borders.

Since our sample is an unbalanced panel, we are concerned that potential non-random attrition of townships in certain years might drive our results. In column 4, we keep only a balanced panel and run the same regression: the estimates are, if anything, even larger than that of the unbalanced panel. Therefore, the panel being unbalanced did not drive our main findings.

In column 5, we keep only those national level SEZs (selected by the central government, much larger than other SEZs), since those SEZs need more migrant workers and are also more politically rewarding for the city leaders, we expect the city governments to devote more efforts into these national SEZs, which would translate into a larger α . As we can see, the magnitude of estimated coefficient α doubles in column 5, which is consistent with our expectations.

In addition to differential trends in the number of primary schools, our model also predicts differential trends in local population. More specifically, in this empirical context, we expect the differential trends in population to be driven by differential trends in primary students, primary school teachers, and parents of school-aged children. And since the SEZs want to attract relatively skilled labor,²⁰ it is reasonable to assume that the migrating parents were originally employed back home.

We estimate the model (equation 6) with the same set of specifications for four additional outcome variables: primary students, primary school teachers, population, and employed population. In every case, we expect α to be negative and significant.

²⁰ They usually impose strict restrictions that do not allow the children of unemployed migrants to enroll in local schools.

Table 3 presents the estimated coefficient α for these four outcome variables (results for primary schools are also kept for comparison), where each row uses a different outcome variable, each column uses a different specification, so every cell is an estimated coefficient from a separate regression. The overall patterns are highly consistent: for each of the outcome variables, and across different specifications, the estimated coefficient α tends to be negative and significant.

According to our preferred specification which controls for town fixed effect and SEZ-by-Year fixed effect (column 2), on average, a treated township loses 0.264 extra primary schools every year, which leads to an additional reduction of 3.76 teachers and 78 students. On average, a treated township has 173 additional out-migrants every year, among which 88 are employed. Notably, these estimates indicate a large treatment effect: between 2001 and 2011, the differential migration between the treated and control townships is as large as 7% of the total population of an average township in the sample.

In other specifications presented in table 3, with township fixed effect always included, we explore models controlling for Year fixed effect (column 1), controlling for both SEZ-by-Year fixed effect and Province-by-Year fixed effect (column 3), using a balanced panel (column 4), the main results are highly robust. In column 5, when we keep only national SEZs, similar to the results for primary schools, the estimated effects on the other four outcome variables also become much larger.

5.2.3. Alternative Explanation

One potential concern about our results is that even if the treated and control townships are spatially adjacent, since they lie in different prefecture cities, it is still possible that there might be some other factors that make it relatively easier for people from the treated townships to migrate to the SEZ, resulting in higher outflows of population in the treated townships, including higher outflows of students. And as a result of more students leaving, schools in the treated townships become unnecessary and are thus closed.

Basically, this is a reverse causality concern: instead of the treated townships closing more schools to induce more out-migration, differential school closure might actually be the result of differential migration. One quick response to this argument would be: as presented in table 1, the treated and control townships were very well balanced in every measure before the RSC started, including in students, population, and employment. This indicates that the presumption of “migration from the treated townships being easier” is unlikely to hold in the first place.

Nonetheless, we attempt to quantitatively rule out this reverse causality concern. To do so, we point out two critical features of the results in table 3, which are consistent with our theoretical model, but

inconsistent with the reverse causality explanation.

The first observation is that if we divide $\alpha_{students}$ by $\alpha_{schools}$, we could conduct a rough back of the envelope calculation for the average size of the additional schools being closed in the treated townships. Using our preferred specification (column 2), for example, an additional school being closed in the treated townships has on average 295 students, significantly larger than the average school size in our sample (253 students). This pattern of closing large schools is inconsistent with the intentions of the RSC program (which requires the local governments to close the smallest schools); and inconsistent with the reverse causality explanation (which indicates that schools are closed because they do not have enough students); but only consistent with the incentive to induce migration proposed by our model.

The second observation is that, if there exists some other factors that cause differential migration between the treated and control townships, which in turn cause the observed difference in primary students, it is then highly likely that in addition to primary students, such factors should also have differential impacts on other similar population groups between the treated and control townships, i.e., we should expect differential migration of kindergarten kids, middle school students, high school students, college students, retired population, disabled population, etc. If any of these variables is also affected by these omitted factors, we would then expect that: $\alpha_{students} + \alpha_{employment} < \alpha_{population}$. However, according to our preferred specification, $\alpha_{students} + \alpha_{employment}$ (-166) is very close to $\alpha_{population}$ (-174), and the two numbers are statistically indistinguishable. Therefore, the reverse causality explanation is highly unlikely to be substantial.

5.3. Third Hypothesis: Other Public Goods are Unaffected

In this section, we formally examine the third testable hypothesis using the same econometric model (equation 6), but with other types of public good as outcome variables. Our model indicates that other types of public good would likely remain balanced between the treated and control townships even after the RSC started.

This hypothesis could also be perceived as a highly demanding placebo test, because it requires that if our main results are driven by any omitted variable, then whatever it is, this confounding factor must correlate and only correlate with primary schools, but not with any other types of local public good, not even middle schools or kindergartens.

Table 4 presents the results of this test. Among the 12 other types of public good measured in the

data,²¹ while all the coefficients are precisely estimated (with small standard errors), only one variable (number of villages that installed telephones) exhibits a differential trend between the treated and control townships, which is barely statistically significant at the 10% level. Moreover, the sign for this variable is positive, which is inconsistent with findings in the main results. All the 11 other types of public good remain very well balanced between the treated and control townships after the RSC started.

Therefore, the results of this test are consistent with our expectation, and strongly support the third testable hypothesis of our model.

5.4. Fourth Hypothesis: Promotion Incentives of Prefecture City Leaders Intensify Competition and Coordination

In this section, we examine the fourth testable hypothesis derived from the extension our model: when the leader of prefecture city A has strong promotion incentives, the treated townships close even more primary schools, when the leader of prefecture city B has strong promotion incentives, the control townships close even fewer primary schools.

As well documented in the literature on Chinese meritocracy, other things being equal, the chance of promotion for a local official decreases discontinuously at a certain age threshold, and as a result, local officials generally devote much more efforts into boosting economic growth when they are below that critical age threshold (Li and Zhou, 2005). For politicians at the prefecture city level, the critical age threshold documented in the existing empirical literature generally falls in the [55, 57] interval (Yao and Zhang, 2011; Xi et al., 2015).

Therefore, we combine our main dataset with a panel dataset on the chief leaders of prefecture cities,²² and follow the literature by defining a prefecture city leader as “incentivized” if his age is below 56.²³ The main estimation equation is:

$$y_{isct} = \alpha \cdot treatment_i \cdot t + \beta \cdot control_i \cdot incentive_{ct} + \gamma \cdot treatment_i \cdot incentive_{ct} + \lambda_{st} + \mu_i + \varepsilon_{isct}$$

where $incentive_{it}$ is a dummy variable that equals one if the leader of prefecture city i in year t is younger than 56, and zero otherwise. $control_i$ is a dummy variable that equals to 1 if township i lies in a control county,

²¹ Middle schools, kindergartens, number of villages with electricity, number of villages with phone signals, number of villages connected with paved roads, number of villages with TV signals, number of villages with tap water, total length of paved road, hospitals, libraries, cinemas, and sports stadiums.

²² Prefecture Party Secretaries. We drop the “outliers” that are younger than 40, which is smaller than 1% of the sample.

²³ Our results are qualitatively robust to slightly different choices of this threshold.

and zero otherwise; $treatment_i$ is a dummy variable that equals to 1 if township i lies in a treated county, and zero otherwise. y_{isct} , as before, represents the outcome of interest.

Intuitively, we could interpret β as the “effect of prefecture city B having an incentivized leader,” and γ as the “effect of prefecture city A having an incentivized leader.” Since incentivized leaders would have stronger incentives to promote economic growth, they are more likely to fight for labor, which intensifies fiscal competition and coordination. Therefore, we expect to see β being positive and significant (promotion incentives intensify competition), γ being negative and significant (promotion incentives intensify coordination).

Since in this section we are no longer focused on estimating and interpreting the average differential trends between the treated and control townships (coefficient α), we could now control more flexibly (less parametrically) for the differential dynamics between the two groups, which leads to the following specification:

$$y_{isct} = \beta \cdot control_i \cdot incentive_{ct} + \gamma \cdot treatment_i \cdot incentive_{ct} + \omega_{Tt} + \lambda_{st} + \mu_i + \varepsilon_{isct}$$

where ω_{Tt} is Treatment-by-Year FE, it fully absorbs the differential dynamics between the treated and control groups, and would therefore improve the precision of estimation for political incentives (coefficients β and γ).

As shown in table 5, the results are exactly as we expected, and they have huge magnitudes: according to the preferred flexible specification (column 4), having an incentivized leader in prefecture city A means a treated township closes an extra 0.66 primary schools every year, while having an incentivized leader in prefecture city B means a control township retains an extra 0.97 primary schools every year. The results are qualitatively robust to using less flexible specifications (columns 1 and 2), using only a balanced panel (column 3), and controlling for personal characteristics of prefecture city leaders (column 5).

Therefore, more incentivized politicians exert more efforts in fiscal competition and coordination, confirming the fourth prediction of our model. In addition, the opposite signs and comparable magnitudes of β and γ also suggest that both “coordination” and “competition” are important in producing the discontinuous gaps in the key outcome variables between the treated and control townships.

6. Additional Results

In this section, we present three additional results: section 6.1 investigates the impacts of fiscal competition and coordination on the quality of education; section 6.2 introduces an alternative empirical strategy to confirm that our results are not driven by underlying differences between adjacent prefecture cities; section

6.3 presents robustness checks using only SEZs that were established before the RSC program started.

6.1. Quality of Education

The discussions of this paper have mainly focused on the quantity of education (number of primary schools). However, for prefecture city A (B) to induce high-skilled labor to migrate (prevent high-skilled labor from migrating), the quality of education could also be important. As discussed in section 2.3, qualitative evidence confirms that some prefecture cities did close their best schools and relocate them to their SEZs, suggesting that the quality of education also plays an important role in local competition and coordination.

We proxy for the quality of education with “education expenditure per student,” which is widely used in the economics of education literature (Jackson et al., 2015). However, unlike the key outcome variables used before (schools, students, teachers, population, employment), this variable is only available after 2006, because of a national reform on educational finance happened at that time, which required the local governments to keep detailed electronic records of educational expenditures.²⁴

Adopting our main specification (equation 6) with “education expenditure per student” as the outcome variable, we could test whether the quality of education has differential trends between the treated and control townships. If it does, we would expect α to be negative and significant.

The results are presented in table 6. As expected, from 2007 to 2011, the gap in average education expenditure between the treated and control townships increases at a rate of 156 Yuan per year, more than 8% of the education expenditure per student in the sample. However, unlike previous findings on other outcome variables, this result does not hold for national SEZs, which might be due to the fact that data for this variable is only available after 2006, by which time the quality adjustment in the national SEZs might have already been achieved.

Worth noting is that other than our proposed mechanism, where prefecture city A coordinates between the treated townships and the SEZ to induce migration (and prefecture city B competes against the SEZ to prevent labor from leaving the control townships), this finding on the quality of education could hardly be reconciled with other confounding explanations. For instance, if the aforementioned reverse causality hypothesis holds (migration happened before school closure), then there is a reason to

²⁴ The “New Mechanisms Reform,” see Wang (2008) for more details.

close schools after people leave, but there is no reason to also lower the quality of education in those local schools that are not being closed. Also, were it not for the coordinative incentive, one might even expect that high fiscal income generated by the SEZ could create positive spill-over effects for the treated townships, and thus lead to an improvement of education quality in the treated townships, rather than the other way round.

6.2. Prefecture City Heterogeneity

As discussed in section 2.1, by 2006, every prefecture city in China has at least one SEZ. So in our empirical setting, although the variation for identification relies on the fact that prefecture city A has a SEZ lying on its border, prefecture city B also has its own SEZ, albeit not lying on the border shared with prefecture city A. Therefore, there is no reason to be concerned about the possibility that if prefecture city A has a SEZ but prefecture city B does not have one, they might be systematically different in many unobservable ways, which drive the results we found.

However, one might still be concerned about the fact that prefecture city A's SEZ lies on its border shared with prefecture city B, while prefecture city B's SEZ does not lie on the same border. If such difference in locational choices of SEZs could reflect some underlying differences between the two prefecture city governments, for instance, different philosophies in decision-making or different abilities in lobbying, it is then possible that such differences might be omitted from the main specification, and could potentially drive the results of differential school closure between the treated and control townships.

To address this concern, we adopt an alternative empirical strategy that avoids the potential selection issues at the prefecture city level. The setting for this strategy is illustrated in figure 4. As shown, we identify all the cases where prefecture city C has a SEZ on its border that is not shared with prefecture city D, and prefecture city D has a SEZ on its border that is shared with prefecture city C. We define the counties that are in prefecture city C and in the neighborhood of prefecture city C's SEZ as "treated," and those counties that are in prefecture city C but in the neighborhood of prefecture city D's SEZ as "control." Since the treated and control groups are all in the neighborhoods of SEZs, they are expected to have comparable "natural" trends for urbanization. More importantly for this exercise, since both the treated and control townships are in prefecture city C, the decision of school closure for both groups are all decided by the same agent: the leader of prefecture city C. Therefore, if the results in section 5.2 are mainly driven by selection at the prefecture city level, or any other omitted confounding differences between the two

adjacent prefecture cities, we would expect that the treated and control townships in this alternative setting no longer have any difference in school closure. However, if our proposed mechanism is correct, prefecture city C would want to close more schools in the treated townships to induce migration to its own SEZ, but retain more schools in the control townships to keep its labor. In that case, we would expect to find the same pattern as before: the number of schools decreases faster in the treated townships than in the control townships.

For estimation, we simply adopt the specification of equation 6, but replace the SEZ-by-Year fixed effect with City-by-Year fixed effect. Then, if selection at the prefecture city level is really an important concern, the coefficient α should be statistically indistinguishable from zero; if our proposed mechanism is correct, α should be negative and significant.

Table 7 presents the results. In column 1, we control for township fixed effect and year fixed effect, and the estimated coefficient is -0.198, similar to what we obtained using the main identification strategy in the whole sample (-0.225). Column 2 shows the preferred specification where we further control for City-by-Year fixed effect. The coefficient is negative and significant, and almost doubled in magnitude. This strongly rejects the “city heterogeneity” hypothesis, but could be explained by competition and coordination between local governments. Column 3 uses only a balanced panel, and the results go through, with even larger magnitudes.

Compared to our main identification strategy, a potential caveat for this alternative strategy is that we no longer have the unique spatial-discontinuity feature: the treated and control townships are now in the neighborhoods of different SEZs. Therefore, one might worry about the size of SEZs being a confounding factor: if for some reason the SEZ in prefecture city C is larger than the SEZ in prefecture city D, then even absent of differential incentives (to coordinate or to compete), prefecture city C would still naturally close more primary schools in the treated townships than the control townships, which leads to an over-estimation of the coefficient α . In column 4, we attempt to address this concern by controlling for the interaction term of the “area of a SEZ” and a continuous time variable, which absorbs the differential trends caused by SEZ size. As we can see, the interaction term has a negative coefficient, meaning that more schools are closed in the neighborhood of a larger SEZ. After we control for such impacts of SEZ size, the estimated gap between the treated and control townships shrinks to -0.219, which is statistically significant at the 5% level, and is extremely close to our baseline result using the main identification strategy (-0.225).

As suggested by this alternative empirical strategy, it is unlikely that our results are driven by underlying differences between adjacent prefecture cities, instead, it is highly likely that the proposed mechanism (coordination against competition) is dominant in explaining our main findings.

6.3. SEZs Established after the RSC Started

Another potential concern comes from the fact that some SEZs were established after the start of the RSC program. Specifically, the RSC program provided a prefecture city government with the ability to coordinate between its own counties and its own SEZ, and we might expect that the prefecture city government would take this factor into consideration when they make locational choices for new SEZs after the RSC started. While this possibility does not necessarily contradict the hypotheses of competition and coordination, it might be a concern if our previous regression results were partly capturing the strategic locational choices of SEZs that were established after 2002. To address this issue, in this section, we replicate our main results (table 3) using only the subsample of SEZs that were established before 2002.²⁵

In table 8, we present the estimated coefficients for the five key outcome variables: primary schools, primary students, primary school teachers, population, and employed population. The results are overall very similar to that presented in table 3, and if anything, slightly larger in magnitude. The estimated standard errors are also slightly larger, potentially due to the reduction in sample size by excluding those SEZs established after 2002. This suggests that our main results are not driven by the strategic locational choices of SEZs after the RSC started.

7. Conclusion

In this paper, we identify causal evidence on fiscal competition and coordination among local governments.

We first introduce a unique empirical context, which is created by the combination of two national programs in China: the “Special Economic Zones (SEZ)” program and the “Rural School Consolidation (RSC)” program. In a spatial discontinuity setting, we identify all the SEZs on the prefecture city borders, and for their neighboring counties, we define those that lie in the same prefecture city as “treated,” and those that lie in the different prefecture city as “control.”

We present a simple model of asymmetric fiscal competition which mimics the empirical setting, and

²⁵ Among the 58 SEZs satisfying our empirical setting of figure 1, 11 were established after 2002, and those are excluded from the subsample used in this section.

formally derive four testable hypotheses: (1) before the RSC program started, the treated counties and the control counties should be well balanced in every measure; (2) after the RSC program started, the treated counties close more primary schools and lose more population, as compared to the control counties; (3) after the RSC program started, except for primary schools, any other types of public good would likely remain balanced between the treated counties and the control counties; (4) if the leader of prefecture city A has strong promotion incentives, the treated counties would close even more primary schools, if the leader of prefecture city B has strong promotion incentives, the control counties would close even fewer primary schools.

Applying the spatial discontinuity design to a unique panel dataset of Chinese townships, we find empirical evidence strongly supporting all four testable hypotheses. Further analysis provides three additional results: (1) the quality of education also follows the same pattern of competition and coordination; (2) the main results are not driven by underlying differences between adjacent prefecture cities; (3) the main results are not driven by strategic establishment of SEZs after the RSC program started. Overall, the empirical findings provide causal evidence on fiscal competition and coordination among local governments in China, which could also add to the more general literature on fiscal federalism and decentralization.

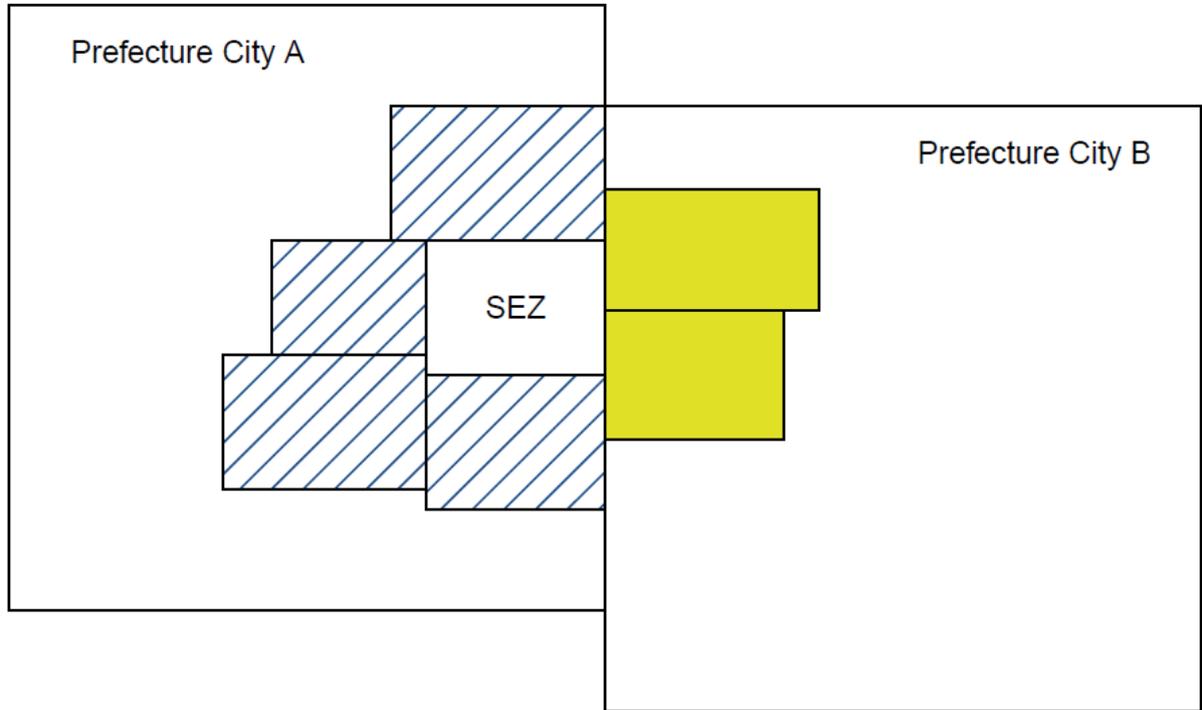
Finally, our results also have important policy implications: when designing centralized programs, to avoid unexpected consequences, the strategic interactions among local governments should be systematically taken into consideration.

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Figure 1. Empirical Setting



▨ Treatment counties ■ Control counties

Figure 2. Theoretical Setting

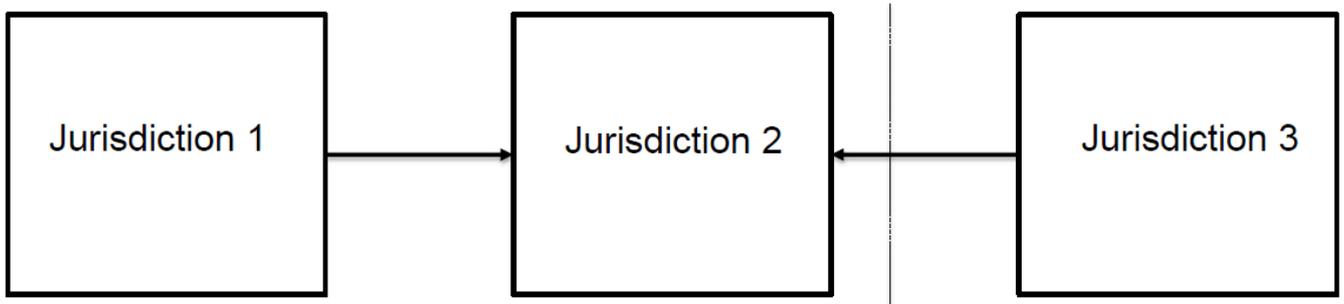


Figure 3. Sample Distribution

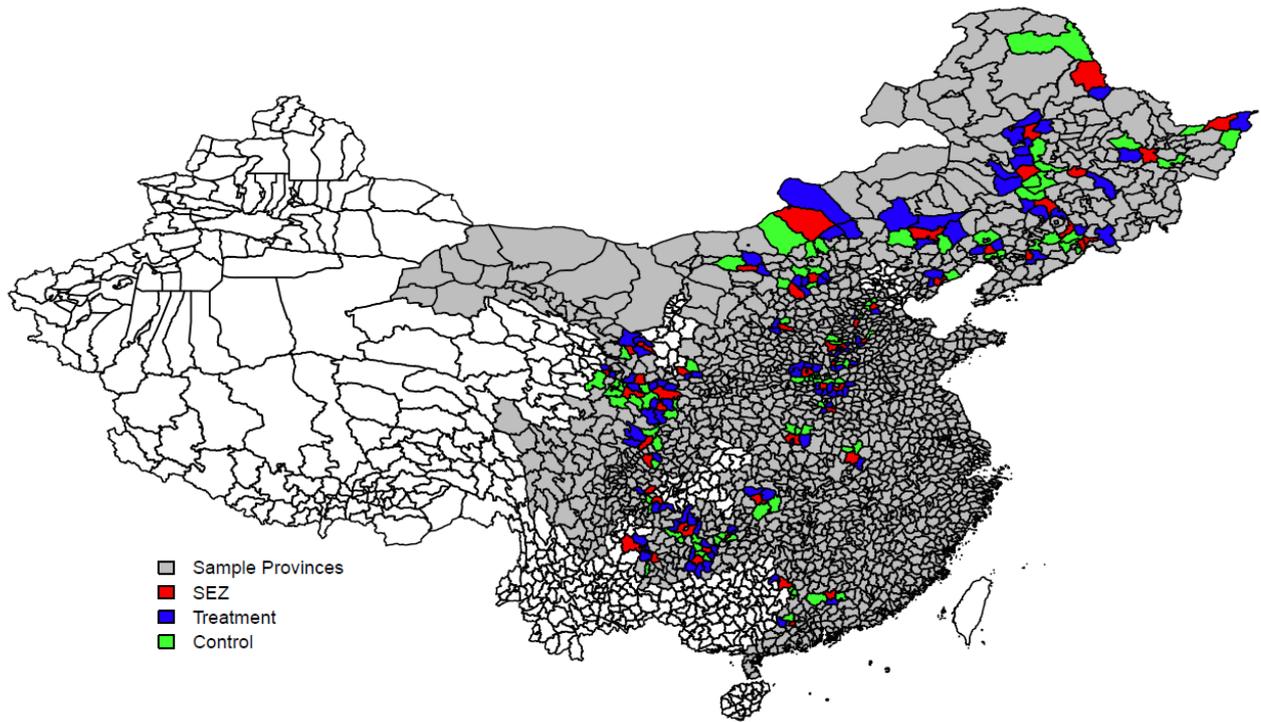
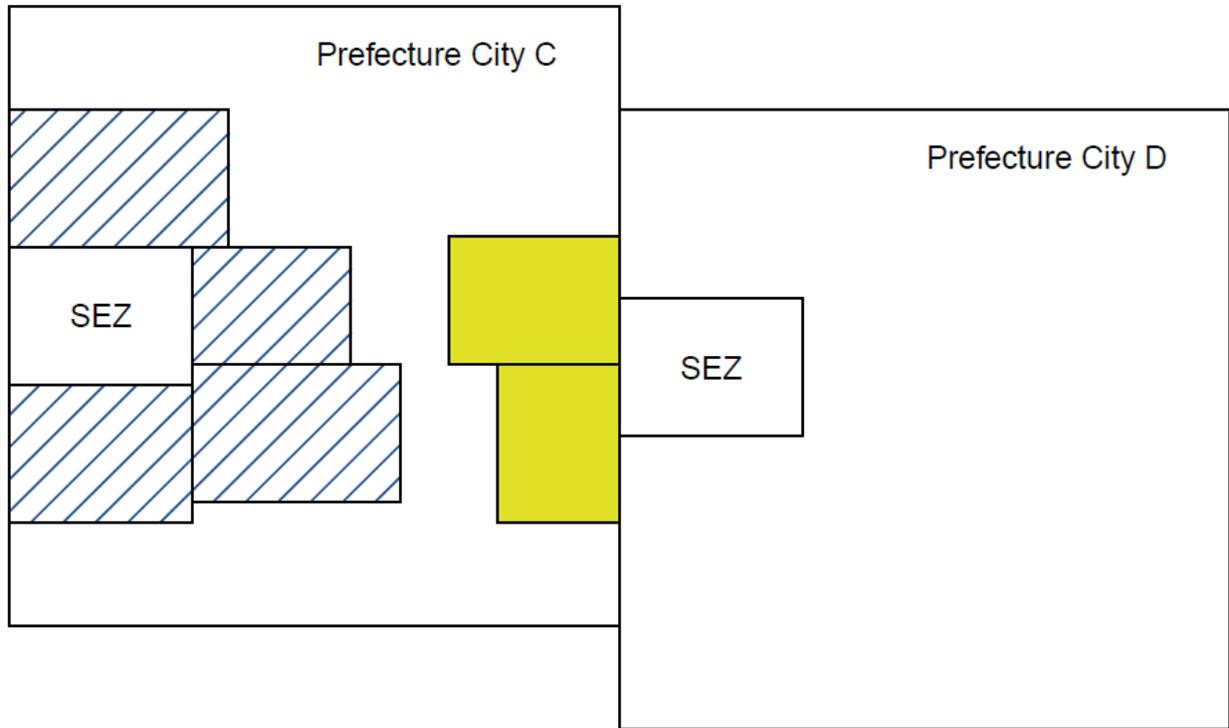


Figure 4. Alternative Empirical Setting



▨ Treatment counties ■ Control counties

Table 1. Pre-RSC Balance Test

VARIABLES	Control		Treated		Normalized Difference
	Mean	S.D.	Mean	S.D.	
Primary Schools	18.85	8.84	19.35	11.87	0.05
Primary Students	4619.27	3467.60	4904.75	3891.52	0.08
Primary Teachers	272.04	210.39	297.39	240.69	0.11
Population	28346.54	18125.95	30350.22	19263.48	0.11
Employed Population	14146.94	10305.99	15169.82	11000.17	0.10
Income	2041.27	772.75	1997.45	849.69	0.05
Area	16263.93	32480.79	17307.90	28386.50	0.03
Number of Communities	1.95	5.23	2.21	7.23	0.04
Number of Villages	16.93	9.79	18.45	10.13	0.15
Electrified Villages	16.81	10.18	18.51	11.40	0.16
Villages w\ Telephones	15.17	10.18	15.92	11.59	0.07
Villages w\ Roads	15.52	9.88	17.42	11.30	0.18
Villages w\ TV	7.65	8.79	8.26	9.78	0.07
Villages w\ Tap Water	8.22	9.89	8.90	9.71	0.07
Villages w\ Incinerators	0.26	0.72	0.30	1.78	0.03
Number of Hospitals	5.38	10.46	5.49	12.35	0.01
Number of Kindergartens	6.22	20.14	6.87	9.93	0.04
Number of Libraries	1.31	2.06	1.85	5.59	0.13
Number of Stadiums	0.37	1.94	0.28	1.11	0.06
Observations	662		729		

Notes: This table reports the summary statistics by treatment and control townships in 2001 (pre-RSC). Following Imbens (2015), we use Normalized Difference to measure the overlap between the two groups, and all the variables are found to be well balanced. Two measures of public good (Number of Middle Schools, Number of Cinemas) are not included in this table because there are not available in the 2001 TCS dataset.

Table 2. Main Strategy: Primary Schools

Dependent Variable:	Number of Primary Schools				
	(1)	(2)	(3)	(4)	(5)
Treatment*Year	-0.225*** (0.0429) [0.142]	-0.264*** (0.0414) [0.0918]	-0.155*** (0.0416) [0.0620]	-0.223*** (0.0692) [0.0589]	-0.470*** (0.108) [0.0986]
Township FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No	No
SEZ-Year FE	No	Yes	Yes	Yes	Yes
Province-Year FE	No	No	Yes	Yes	Yes
R-squared	0.343	0.477	0.513	0.536	0.457
Observations	20,774	20,774	20,774	8,206	2,390

Notes: This table reports the post-RSC differential trends in primary schools between the treated and control townships. Column 1 controls for township FE and year FE, column 2 (preferred specification) controls for township FE and SEZ-by-Year FE, column 3 further controls for Province-by-Year FE, column 4 uses only a balanced panel, column 5 uses only the townships that are adjacent to national-level SEZs. Standard errors are clustered at the SEZ level; ***, **, and * indicate significance at 1%, 5% and 10% levels. Alternative standard errors (two-way clustered at the city and SEZ levels) are also presented.

Table 3. Main Strategy: Key Outcomes

	(1)	(2)	(3)	(4)	(5)
$\alpha_{schools}$	-0.225*** (0.0429) [0.142]	-0.264*** (0.0414) [0.0918]	-0.155*** (0.0416) [0.0620]	-0.223*** (0.0692) [0.0589]	-0.470*** (0.108) [0.0986]
$\alpha_{students}$	-45.85** (19.53) [40.97]	-77.76*** (23.49) [27.85]	-67.42*** (25.33) [31.53]	-56.96 (48.90) [44.39]	-250.4*** (77.06) [96.35]
$\alpha_{teachers}$	-1.779 (1.182) [2.214]	-3.756** (1.578) [1.631]	-3.291** (1.530) [1.763]	-3.381 (2.891) [2.281]	-7.728*** (2.964) [2.843]
$\alpha_{population}$	-115.0* (63.27) [105.5]	-173.5** (76.69) [103.5]	-210.0*** (80.40) [75.19]	-241.5* (145.5) [137.0]	-909.4*** (286.8) [233.1]
$\alpha_{employment}$	11.10 (41.78) [62.96]	-87.81* (48.40) [68.70]	-124.1** (52.68) [58.57]	-156.4* (93.36) [91.11]	-566.2*** (167.9) [42.27]

Notes: This table reports the post-RSC differential trends in all the five key outcome variables between the treated and control townships. Each cell presents an estimated coefficient obtained from a separate regression, with each row using a different outcome variable, and each column using a different specification: column 1 controls for township FE and year FE, column 2 (preferred specification) controls for township FE and SEZ-by-Year FE, column 3 further controls for Province-by-Year FE, column 4 uses only a balanced panel, column 5 uses only the townships that are adjacent to national-level SEZs. Standard errors are clustered at the SEZ level; ***, **, and * indicate significance at 1%, 5% and 10% levels. Alternative standard errors (two-way clustered at the city and SEZ levels) are also presented

Table 4. Other Public Goods

Dependent Variables:	Middle Schools	Kindergarten	Electric Villages	Phone Villages	Road Villages	TV Villages	Water Villages	Road Length	Hospitals	Libraries	Cinema	Stadiums
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment*Year	0.00285 (0.00898) [0.0105]	-0.0510 (0.0518) [0.0788]	0.0372 (0.0327) [0.0668]	0.113* (0.0677) [0.0823]	-0.00751 (0.0392) [0.0762]	0.0318 (0.0557) [0.102]	0.0124 (0.0398) [0.0474]	0.451 (0.655) [1.101]	0.00008 (0.0004) [0.0004]	-0.0175 (0.0233) [0.0236]	0.00125 (0.00260) [0.00199]	-0.0041 (0.0179) [0.0105]
Township FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SEZ-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.421	0.198	0.762	0.743	0.726	0.602	0.594	0.330	0.070	0.209	0.078	0.142
Observations	16,656	20,770	20,774	20,774	20,774	20,773	20,773	20,774	20,774	20,765	16,570	20,707

Notes: This table reports the parallel post-RSC trends in 12 different measures of local public good using the preferred specification. Township FE and SEZ-by-Year FE are included in every specification. Standard errors are clustered at the SEZ level; ***, **, and * indicate significance at 1%, 5% and 10% levels. Alternative standard errors (two-way clustered at the city and SEZ levels) are also presented.

Table 5. Political Incentives

Dependent Variable:	Number of Primary Schools				
	(1)	(2)	(3)	(4)	(5)
Incentive*Control	0.423 (0.367) [0.418]	1.012** (0.405) [0.450]	1.403** (0.573) [0.503]	0.974** (0.410) [0.461]	1.780*** (0.529) [0.444]
Incentive*Treatment	-0.349 (0.293) [0.335]	-0.580* (0.304) [0.361]	-0.478 (0.432) [0.390]	-0.663** (0.307) [0.360]	-0.794** (0.331) [0.374]
Treatment*Year	-0.360*** (0.0671) [0.175]	-0.0677 (0.0594) [0.0843]	-0.178** (0.0869) [0.0738]		
Treatment-by-Year FE	No	No	No	Yes	Yes
PPS Characteristics	No	No	No	No	Yes
Township FE	Yes	Yes	Yes	Yes	Yes
SEZ-by-Year FE	Yes	Yes	Yes	Yes	Yes
Province-by-Year FE	No	Yes	Yes	Yes	Yes
R-squared	0.439	0.486	0.487	0.486	0.498
Observations	13,074	13,074	5,981	13,074	11,338

Notes: This table reports the effects of the political incentives of prefecture city leaders on the post-RSC differential trends in primary schools between the treated and control townships. Column 1 controls for Township FE and SEZ-by-Year FE, column 2 further controls for Province-by-Year FE, column 3 uses only a balanced panel, column 4 replaces the interactions of treatment and year with Treatment-by-Year FE which is more flexible, column 5 further controls for personal characteristics of the Prefectural Party Secretary (gender, nationality, years of working experience, political faction, previous position). Standard errors are clustered at the SEZ level; ***, **, and * indicate significance at 1%, 5% and 10% levels. Alternative standard errors (two-way clustered at the city and SEZ levels) are also presented.

Table 6. Quality of Education

Dependent Variable:	Education Expenditure per Student				
	(1)	(2)	(3)	(4)	(5)
Treatment*Year	0.00147 (0.00624) [0.0132]	-0.0156*** (0.00415) [0.00733]	-0.0149*** (0.00447) [0.00644]	-0.0186*** (0.00640) [0.00915]	0.0146 (0.0135) [0.0170]
Township FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No	No
SEZ-Year FE	No	Yes	Yes	Yes	Yes
Province-Year FE	No	No	Yes	Yes	Yes
R-squared	0.054	0.378	0.391	0.197	0.232
Observations	4,973	4,973	4,973	2,266	489

Notes: This table reports the post-2006 differential trends in education expenditure per student. Column 1 controls for township FE and year FE, column 2 (preferred specification) controls for township FE and SEZ-by-Year FE, column 3 further controls for Province-by-Year FE, column 4 uses only a balanced panel, column 5 uses only the townships that are adjacent to national-level SEZs. Standard errors are clustered at the SEZ level; ***, **, and * indicate significance at 1%, 5% and 10% levels. Alternative standard errors (two-way clustered at the city and SEZ levels) are also presented.

Table 7. Alternative Strategy

Dependent Variable:	Number of Primary Schools			
	(1)	(2)	(3)	(4)
Treatment*Year	-0.198*	-0.386***	-0.665***	-0.219**
	(0.116)	(0.104)	(0.234)	(0.0902)
	[0.350]	[0.207]	[0.295]	[0.134]
SEZ Area*Year				-0.0011***
				(0.00021)
				[0.00036]
Township FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	Yes
City-by-Year FE	No	Yes	Yes	No
R-squared	0.280	0.440	0.505	0.454
Observations	3,674	3,674	847	3,674

Notes: This table reports the post-RSC differential trends in primary schools using the alternative specification. Columns 1 controls for township FE and Year FE, columns 2, 3, and 4 control for township FE and City-by-Year FE. Column 3 uses only a balanced panel. Column 4 controls for the linear trend caused by the size (area measured in square kilometers) of the neighboring SEZ. Standard errors are clustered at the SEZ level; ***, **, and * indicate significance at 1%, 5% and 10% levels. Alternative standard errors (two-way clustered at the city and SEZ levels) are also presented.

Table 8. Robustness Check

	(1)	(2)	(3)	(4)	(5)
$\alpha_{schools}$	-0.271*** (0.0463) [0.155]	-0.292*** (0.0445) [0.0980]	-0.175*** (0.0447) [0.0632]	-0.249*** (0.0736) [0.0620]	-0.470*** (0.108) [0.0986]
$\alpha_{students}$	-52.74** (22.50) [42.83]	-102.4*** (26.40) [28.45]	-96.39*** (28.79) [32.74]	-95.08* (55.40) [48.65]	-250.4*** (77.06) [96.35]
$\alpha_{teachers}$	-1.898 (1.391) [2.236]	-4.687*** (1.806) [1.793]	-4.347** (1.768) [1.949]	-4.718 (3.340) [2.490]	-7.728*** (2.964) [2.843]
$\alpha_{population}$	-166.2** (71.74) [113.1]	-178.2** (85.29) [116.4]	-222.8** (90.71) [85.71]	-225.7 (163.4) [156.6]	-909.4*** (286.8) [233.1]
$\alpha_{employment}$	-22.08 (47.50) [69.70]	-103.6* (53.98) [76.81]	-143.9** (59.75) [66.39]	-167.5 (105.6) [103.4]	-566.2*** (167.9) [42.27]

Notes: This table replicates table 3 using only the subsample of SEZs that were established before 2002. Each cell presents an estimated coefficient obtained from a separate regression, with each row using a different outcome variable, and each column using a different specification: column 1 controls for township FE and year FE, column 2 (preferred specification) controls for township FE and SEZ-by-Year FE, column 3 further controls for Province-by-Year FE, column 4 uses only a balanced panel, column 5 uses only the townships that are adjacent to national-level SEZs. Standard errors are clustered at the SEZ level; ***, **, and * indicate significance at 1%, 5% and 10% levels. Alternative standard errors (two-way clustered at the city and SEZ levels) are also presented.