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by

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Abstract

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This dissertation contains three chapters that examine development and urbanization in China since 1980. The first chapter investigates the impact of Special Economic Zones established since 1992 on local economic outcomes and urbanization rates. The second chapter studies local bureaucrats' incentives while balancing the goals of economic growth and resource or environmental protection. The last chapter examines patterns of urbanization in China as related to the administrative hierarchy between urban and rural regions.

In the first chapter, I examine the local and aggregate productivity impacts of Chinas Special Economic Zones (SEZ) program from the 1990s to 2006. Using an event study design where I exploit variation in the timing of the first zone establishment across counties, I find that an SEZ increased GDP by 1% to 2% per year over 5 years. In order to assess the aggregate impact of the program, I develop a spatial equilibrium model with two sectors – agriculture and manufacturing. In the model, the economy-wide output impact of an SEZ depends on the elasticities of inter-regional and inter-sectoral labor supply, as well as the relative productivity levels of the SEZ-hosting regions and other regions. I do not find any evidence that the SEZ program induced inter-regional labor reallocation. However, the program did accelerate the shift of employment from the agricultural sector.

In the second chapter, I explore whether Chinese bureaucrats' policy decisions depend on connections with their superiors through the appointment of bureaucrats to office. In particular, I consider appointment as a kind act on the part of the promoter, and ask whether the promoted bureaucrat enacts policies that are in the interest of his benefactor. This framework is applied to the policy outcome of rural land conversion and air pollution. I examine whether rural land conversion is more prevalent, and whether air pollution is more abundant, when the local bureaucrat works under the Party Secretary who promoted him. My analysis exploits variation in the length of politicians terms within individual and pairs of politicians. Preliminary findings suggest that promoted land bureaucrats may exhibit reciprocal behavior, while environmental protection bureaucrats may not.

In the last chapter, I study the impact on urbanization of the *Cities Leading Counties* (CLC) policy implemented in China beginning in 1983. This nationwide program granted large metropolitan governments administrative control over counties in the surrounding

hinterland. I use annual county-level nighttime lights data to examine changes in urbanization in the hinterland after integration with a city. I find that while the policy increased urbanization in counties subsumed into cities, the cities themselves did not experience faster urbanization.

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Chapter 1

Place-based policy in a development context – evidence from China

1.1 Introduction

Policy makers in many reaches of the world implement spatially-targeted development policies. In developing countries, these programs are commonly known as special economic zones. They are often set up to attract foreign direct investment (FDI) by way of manufacturing firms, generate employment, and act as experimental laboratories for new policies (Farole (2011)). The typical bundle of zone policy offered include lowered tax rates and subsidized inputs for manufacturing firms, as well as a boost to the infrastructural investment in the region that hosts the zone. While hosting regions stand to see an increase in their level of economic activity, it is unclear how much of this gain comes at the expense of loss in activity elsewhere. In countries where there is a large agricultural sector, there could also be loss in agricultural activity as some of the labor force engaged in agriculture moves into the manufacturing sector. These different sources of displacement all need to be considered when determining the economy-wide impact of special economic zones.

In this paper, I examine the local and aggregate impacts of China's Special Economic Zones (SEZ) program from 1993 to 2006. This program offered foreign-invested firms a significant tax break and access to cheap land in exchange for locating in a designated zone area within a county. The zones established during this time period were almost entirely sponsored by provincial governments, who also pledged to invest in the improvement of infrastructure and public utilities so as to provide a productive business environment for zone firms. The 1993–2006 SEZ program is an expansion of the well-known program that began in 1980 with the opening of a handful of large coastal and provincial capital cities to FDI and trade. This paper focuses on the later period SEZs since they were established in a social context that is much more comparable to that of zones in other developing countries.

The 1993–2006 phase of the SEZ program is worth close examination for two reasons. First, the program coincided with China's period of transition from a primarily

agricultural to a mostly industrial economy. Though mega cities like Shanghai and Shenzhen received many migrant workers, a large part of the labor force transition out of agriculture has also happened at the local level.[1] Formal migration in China is notoriously restricted due to the household registration system, which is essentially an internal passport program that also distinguishes between urban and rural citizens. While informal migration has taken place in large numbers since 1978, it can be very costly for the migrants workers. However, since the 1990s, many local governments have loosened restrictions on rural-to-urban migration *within* a region (Chan and Buckingham (2008), Fan (2008)). Thus, if an SEZ could attract firms to its hosting county, then it could shift the county's labor force composition from agriculture to manufacturing.[2]

Second, China's SEZ program in the 1993–2006 period is, more than its early incarnation, similar to the SEZ programs in other developing countries, such as the Philippines and India. By the 1990s, there was a functioning land leasehold market in China, FDI has penetrated many regional markets, and formal rural-to-urban migration has become less restricted. These conditions largely did not apply in the 1980s. Therefore, the early Chinese SEZs received special treatment not only in the form of tax breaks and investments, but also free market institutions. By studying Chinese SEZs in the more open era of 1990s and 2000s, I develop an analytical framework that can be generalized to assess the aggregate productivity impacts of SEZ programs in other developing countries.

My analysis begins with estimating the local economic impacts of the SEZ program in the 1990s and 2000s, where local refers to the county.[3] I explore the dynamic effects of the program using an event study design. In doing so, I exploit variation in the timing of the *first* SEZ establishment across counties. Most zone creation is initiated by county officials, and must be approved by the provincial government in order to be realized. Given that a county meets the broad guidelines set by the provincial government, the timing of zone establishment is plausibly random.[4] Identification of the causal impact of SEZs in this design requires that treatment cohorts exhibit parallel trends in the outcome variable prior to treatment. In the event study results, I show strong evidence that the data support this condition by rejecting the existence of pre-trends.

Using annual county-level data from *Province Statistical Yearbooks* spanning 1996–2012, I estimate the dynamic impact of SEZs established in 1999–2006 on county-level GDP. I find that the SEZs generated a 6% to 10% gain in GDP of the hosting county 5 years after the creation of the zones. The increase in GDP manifests gradually after the

[1]For instance, intra-county migration accounted for 30 percent of all migrants and 45 percent of the floating population according to the 2000 Population Census. The average change in the county-level agriculture's share of employment dropped from 75 percent in 1990 to 61 percent in 2000, and 51 percent in 2010. Source: 1990, 2000, and 2010 Population Census. Author's own calculations.

[2]It is possible that the 1993–2006 zones were established with the simultaneous change in migration policy in mind.

[3]Previous studies by Wang (2013) and Alder et al. (2013) look at impacts of the entire SEZ program at the prefecture city or prefecture city proper level, respectively. Given the much smaller scale of zones established in the 1990s and 2000s, it is unlikely that these zones' immediate impact would extend beyond the boundaries of the county into the much larger prefecture.

[4]However, the timing of zone establishment can be influenced unobserved factors such as the political capital of the county official.

second year of SEZ establishment. I find very similar results using nighttime lights as a proxy for the level of economic activity, which allows me to estimate more pre-treatment period effects. This finding also alleviates the concern that the official yearbook data are subject to systematic reporting biases.

In addition to the SEZs' impact on local output, I also find that the program increased county government revenue collection to a large degree – 20% to 26% 5 years after the establishment of the zones. This effect is justifiable given that the zones are meant to attract new business establishments, and that business tax income make up a large share of the county government's budgetary revenue. This result is also consistent with the finding in the Chinese political economy literature that revenue collection is an important way in which a local politician can signal competence in order to boost his chance of being promoted (Lü and Landry (2012)).

I also use firm data from the *Annual Survey of Industry*, 1998–2007, aggregated up to the county-level, to assess more directly the impact of zone creation on the manufacturing employment and firm count. I find that SEZs generated a statistically significant 6% higher manufacturing employment in a post-SEZ year relative to a pre-SEZ year. I also find that SEZs led to more manufacturing firms in the county as observed in the survey. However, this second effect is not precisely estimated due to the firm-size cut-off threshold for being included in the survey.

In order to assess the effect of the SEZ program on aggregate productivity, I develop a simple spatial equilibrium model that incorporates two sectors – agriculture and manufacturing. Workers in the model have heterogeneous preferences over localities and sectors. This heterogeneity can alternatively be interpreted as capturing idiosyncratic costs of moving between localities or sectors. As such, this preference/cost parameter reflects China's particular institutional features with respect to labor mobility.

I model the SEZ program as an investment in public infrastructure that boosts the manufacturing productivity in the targeted locality, and increases the manufacturing wage there. As a result, the SEZ program has an unambiguously positive direct impact on the manufacturing productivity in the hosting county. The indirect impact of the SEZ program comes from the reallocation of workers across counties and sectors. If manufacturing is more productive than agriculture, then the indirect impact from inter-sector reallocation is also positive. On the other hand, the indirect impact from inter-county reallocation of labor within manufacturing depends on the manufacturing productivity level of the hosting county relative to that of other counties.[5] Thus, the aggregate productivity impact will depend on the magnitudes of the direct impact and the two indirect impacts.[6]

Taking the insight from the model to data, I find no effect of the SEZ program on population in the hosting counties. This indicates that the program did not induce

[5]This result is similar to the result Kline and Moretti (2014). In their paper, the authors consider only productivity in the manufacturing sector. However, they model local manufacturing productivity to respond to agglomeration economies, whereas I do not consider agglomeration in my model.

[6]It is important to note that my approach to assess the aggregate impact of SEZs focuses on the distribution of the labor force across sectors and localities, and does not address the issue of capital reallocation. This issue is beyond the scope of this paper, and is an interesting area for future work.

displacement of labor from outside the county (nor did it induce potential movers to stay). This lack of inter-county displacement can be rationalized by China's strong inter-regional mobility restrictions, and the fact that the SEZs in my sample were established in small- to medium-sized cities. Given this finding, I interpret the local productivity impact of the SEZ program as the sum of the direct impact on manufacturing productivity and the indirect impact on inter-sector labor reallocation within the county. Indeed, using county-level Population Census data, I find that after controlling for a rich set of county characteristics, an extra year of hosting an SEZ between 1990 and 2000 is associated with a 0.5% to 1% decline in agriculture's share of employment during that decade. Similarly, an extra year of hosting an SEZ between 2000 and 2010 is associated with a 0.7% to 1% decline in agriculture's share of employment during that decade. This result is not due simply to the inflow of non-agricultural workers. I find the same pattern with the percent change in agricultural employment as the outcome variable.

This paper is related to several strands of literature in urban and development economics. First of all, this paper fits in to the literature on evaluating special economic zones in developing countries. While the literature on the local impacts of place-based policies is extensive, it focuses mainly on programs implemented in the US and Europe (Bronzini and de Blasio (2006), Neumark and Kolko (2010), Mayer et al. (2012), Gobillon et al. (2012), Criscuolo et al. (2012), among many others). Three recent papers examine programs implemented in developing countries – Chaurey (2013) on India, and Wang (2013) and Alder et al. (2013) on China's SEZs. My contribution is in developing a spatial equilibrium model that allows me to extrapolate beyond the estimated local impacts in order to assess the aggregate effect of the policy.

The two papers on China's SEZs study the SEZ program from the early 1980s to the 2000s, with either the entire prefecture city (Wang (2013)) or the prefecture city center (Alder et al. (2013)) as the unit of observation. However, the pre-1992 and post-1992 SEZs are very different in three important aspects – later zones are physically smaller, their creation is decentralized, and they were established at a time when China already had many large urban centers where it was possible for firms to lease land and for foreign investment to flow. This paper focuses on the later cohorts of the SEZ program. As such, I am able to examine the program's impact at the scale of the smallest administrative unit containing the zones (i.e. the county level). Also, it is more feasible to consider a meaningful counterfactual for the later zones that were established far from China's planned economy period.

One of the main goals of this paper is to assess the impact of China's SEZ program on aggregate productivity. Therefore this paper is closely related to the literature on evaluating the aggregate impact of place-based or geographically-varying policies. Since this literature is focused on programs implemented in developed economies, it often does not explicitly address the structural transformation effects of the place-based programs (Glaeser and Gottlieb (2008), Albouy (2009), Busso et al. (2013), Gaubert (2014), Kline and Moretti (2014)). For instance, Kline and Moretti (2014) find that the Tennessee Valley Authority in the 1940s to 1960s decreased agriculture's share of employment and increased manufacturing's share in the targeted region, yet their assessment of the program's overall productivity impact is restricted to the manufacturing sector. In my model,

I allow workers to choose between a less productive agricultural sector and a more productive manufacturing sector. Due to differences in the productivity levels across agriculture and manufacturing, the sectoral reallocation effects of the SEZ program has important implications for its impact on overall productivity.[7]

Second, this paper contributes to the literature on evaluating special economic zones in developing countries. While the literature on the local impacts of place-based policies is extensive, it again covers mainly programs implemented in the US and Europe (Bronzini and de Blasio (2006), Neumark and Kolko (2010), Mayer et al. (2012), Gobillon et al. (2012), Criscuolo et al. (2012), among others). Three recent papers have began to examine programs implemented in developing countries – Chaurey (2013) on India, and Wang (2013) and Alder et al. (2013) on China's SEZs. My contribution is in developing a spatial equilibrium model that allows me to extrapolate beyond the estimated local impacts in order to assess the aggregate effect of the policy.

In documenting the productivity effect of reallocation of labor from agriculture to non-agriculture, this paper adds to the set of papers that study the productivity impacts of factor misallocation (see Restuccia and Rogerson (2013) for a review). Brandt et al. (2008) find the shift of labor away from agriculture accounts for a large portion of China's growth phenomenon in the last 30 years, though the reallocation of capital from state-owned to private companies within the non-agricultural sector accounted for the majority of China's growth. Hsieh and Klenow (2009) suggest that manufacturing productivity can yet be dramatically improved through capital reallocation away from state-owned firms. On the side of labor misallocation, Au and Henderson (2006) examine the large productivity costs of China's migration restrictions across localities within the manufacturing sector. The results in this paper suggest that the SEZ program could have been one way in which the central government tried to compensate for the adverse productivity effects of labor mobility restrictions. That is, given the limits on movement across localities, establishing SEZs was a kind of remedial policy.

There is a long tradition of growth accounting with structural transformation. This paper is particularly related to the works that emphasize the role of sectoral productivity as a driver for structural change, as in Gollin et al. (2002), Ngai and Pissarides (2007), Restuccia et al. (2008), Duarte and Restuccia (2010) and Vollrath (2009). The structural transformation literature is centered on cross-country comparisons, and there is a common insight that transition of the labor force out of agriculture accounts for a significant share of the gains in aggregate productivity over time (Kuznets (1966), Echevarria (1997), Duarte and Restuccia (2010), and Herrendorf et al. (2013)). Within this literature, few papers address the implications of different rates of structural transformation across localities within a country, with the notable exceptions of Caselli and Coleman II (2001) and Michaels et al. (2012). My paper is closest in spirit to Caselli and Coleman II (2001), particularly in that I explicitly model agricultural workers' decisions to switch into the manufacturing sector. The main contribution of my paper to this literature is in applying its insights to the analysis of place-based policies in developing countries with

[7]It is important to acknowledge that while this paper focuses on the aggregate *productivity* impacts of the SEZ program, it does not address the *welfare* impacts of the program.

large agricultural sectors and potentially high mobility costs.

Finally, this paper contributes to the literature on Chinese political economy. Competition for promotion among politicians of the same rank has long been observed as an important motivation behind efforts for local development (Li and Zhou (2005), Shih et al. (2012), Jia (2012)) and for revenue extraction (Lü and Landry (2012)). Bai et al. (2014) point out that local officials often provide cronyism-like protection to local businesses in exchange for a share of the profit pie. I interpret the later period of the SEZ program in the context of China's local political economy. In particular, my findings are consistent with the idea that county politicians rationally compete to host SEZs, which attract economic activity to and increase revenue for the hosting county, and may thus boost the career prospects of the politicians in charge.

The rest of this paper proceeds as follows. Section 1.2 provides background information for China's SEZ program and labor mobility restrictions. Section 1.3 provides estimates of the SEZ program's local impact on output, government revenue, manufacturing employment, wage, and firm count. Section 1.4 presents a simple spatial equilibrium model with inter-regional and inter-sectoral labor mobility, and lays out a framework for assessing the aggregate impact of the SEZ program. Section 1.5 presents empirical evidence on the two types of labor reallocation induced by the SEZ policy, and interprets the aggregate output impact of the SEZ program in lights of these results. Section 1.6 discusses several extensions of the framework adopted in this paper, and concludes.

1.2 Institutional Background

In this section, I provide some background context for this paper. First of all, China's governing system is a strict nested hierarchy, beginning with the central government in Beijing at the top, followed by province-, prefecture-, county-, and township-level governments. Cities can exist as a province, prefecture, or a county. A province-level city and a prefecture-level city both contain a large urban area made up of urban districts – *the city proper*, as well as several counties or county-level cities. I define a *county-level unit* as either a county, a county-level city, or the entire city proper of urban districts. Figure 1 shows an example of a prefecture-level city, Anqing, in Anhui province, that contains 8 county-level units in addition to the city proper, with a total of 9 county-level units by my count.

[Figure 1 about here.]

During the 1980s and 1990s, prefecture-level cities have increased in number and as many county-level units were promoted to the prefecture level. In the process, new and existing prefecture-level cities also expanded in size as they subsumed other county-level units (Ma (2005)).

Below I give a brief history of the SEZ program, and the household hold registration system's impact on limiting labor mobility.

China's Special Economic Zones (SEZ) program

I introduce China's SEZ program in two parts – the early phase from 1980 to 1992, and the later phase from 1993 to 2006. The early phase of the SEZ program is by far the more well-known than the later phase for its dramatic interception of China's decades of isolation and the credit given it for having created some of China's mega-cities. In 1980, Deng Xiaoping's central government announced the establishment of 4 SEZs, located in coastal cities in the South – 3 in Guangdong province, and 1 in Fujian province. At the time, these zones were the first and only places in China where foreign capital could be invested, and where firms can lease land, and have private property rights (Pak (1997)). These zones were meant to be the testing ground for capitalism in China. In 1984, 14 additional zones on the coast and along the Yellow River were approved by the central government as a way to deepen economic liberalization and encourage the inflow of foreign capital and technical know-how. By 1992, all 30 provinces, autonomous regions, and provincial cities had opened at least one SEZ.[8]

These SEZs in the 1980–1992 phase merited the "special" part of their title much more so than did the later SEZs. This is because the early zones practiced market policies at a time where few other places could in the country. The most prominent policies implemented in these early zones were a reduction in the business income tax rate from 33 to 15 percent, the ability to lease land at a subsidized price, reduced hiring costs, and in some cases cheaper access to capital (Pak (1997)). Consider that, for the rest of the country, a land leasehold market was only developed after 1988 (Ho and Lin (2003)). These zones were also hand-picked by the central government for their proximity to foreign capital (by way of Hong Kong, Macau, and Taiwan) and often to a body of water for transportation purposes.

In January of 1992, Deng Xiaoping visited the original SEZs in the South in a much publicized trip that was recognized as a publicity tour to laud the open economic policies (Yeung (1992)). Following this trip and the official announcement encouraging the expansion of the SEZ program at the extensive margin , local governments immediately began to establish SEZs of their own (Hsing (2010)). Figure 2 shows the expansion of the SEZ program with 4 snapshots: 1984, 1991, 2003, and 2006. The expansion phase of the SEZ program consisted of the establishment of many more smaller zones in coastal as well as inland provinces.

[Figure 2 about here.]

These zones typically occupy an area of 10 to 50 square kilometers, situated well within the boundaries of a county, and near an existing market center. Figure 3 shows

[8]There are several types of SEZs, most of which are production-oriented. They are (by frequency as of 2006): Economic Development Zones (EDZ, 624), Industrial Development Zones (IDZ, 422), High-Tech Industrial Development Zones (HTIDZ, 99), Open Economic Areas (OEA, 102), and Export Processing Zones (EPZ, 47). All of these zones share the same policy goals. The first three types differ in terms of the industries they target, with EDZs being the most general. OEAs are typically entire city centers. Three other types of SEZs that I do not consider here are: Free Trade Zones, National Tourism and Holiday Resorts, and Border Economic Cooperation Zones, because they are they were established before 1993, or established after 1993 but not as the first SEZ in the county.

the physical extent of one such SEZ in 1998, three years before its establishment. This SEZ was eventually established in 2001, in Tongcheng city, which is one of the county-level units depicted in Figure 1. In Figure 3, the county's borders are outlined in blue, and the placement of the SEZ is outlined in a red box. We can see that the zone is much smaller relative to the county's area, and is situated near the already brightest part of the county.

[Figure 3 about here.]

Instead of being handpicked by the central government, the zones established since 1993 tended to be initiated by lower-level governments, who saw the opportunity to improve their local economy with programs subsidized by the provinces. The proposal for a province-level zone is usually drawn up by the county-level government, and submitted for approval by the provincial government. Provinces have general guidelines for zones establishment, which often stipulate that zones need to be near an existing market center, and in an area that can easily be serviced with public utilities and transportation infrastructure.

Province-level zones offered the same tax breaks to foreign-invested firms, as well as guaranteed investors access to improved infrastructure and public services Panel A of Figure 4 shows the time line of SEZ establishment by the level of government sponsorship. By 2003, province-level SEZs outnumbered national-level SEZs 672 to 306.

[Figure 4 about here.]

While SEZs continue to exist and offer preferential policies to attract new business, they fundamentally changed after 2006. The spike in SEZ establishment in Panel A of Figure 4 corresponds to the anticipation of a tax policy reform that affected equalized the business income tax rate for foreign and domestic firms across the country to 25 percent.[9] Zones established after the tax reform could not offer the same discounted tax rate to foreign investors, but zones established before this tax reform were allowed to honor their promise to businesses. Since 2007, SEZs have become more individualistic in the concessions they offer to new businesses. Moreover, investment in the zones have become largely skewed toward domestic rather than foreign capital.[10]

Labor mobility under the household registration system

The household registration system, commonly known by its Chinese name, *hukou*, has been in place since 1958. Each Chinese person has a registration status made up of two parts (Chan and Zhang (1999), Fan (2008), Chan and Buckingham (2008)). The first part

[9]For instance, the *Special Economic Zones and tax exemption in China* document published by the accounting firm Ernst & Young in 2006 clearly stated: "The tax reform planned for 2007 aims to abolish the above- cited tax concessions granted to foreign-invested companies... Companies wishing to commence operations in China should therefore do so before the end of this year and take into account the planned changes in Chinese tax law when selecting their location and consider the legal structure."

[10]See, for instance, **?**.

determines the person's socio-economic status by designating her as either agricultural or non-agricultural. Traditionally only people born with the non-agricultural designation are entitled to government-provided employment, welfare, health care, education, etc. People born with the agricultural designation are traditionally entitled to farmland, and employment and any social safety nets provided by the rural collective. The second part of a person's registration status is a place of legal residence. For a person with non-agricultural designation, her legal residence determines the place where she receives her entitlements (Chan and Buckingham (2008)). It is commonly acknowledged that the household registration system renders people with agricultural status second-class citizens, and that it is the fundamental underpinning of rural-urban, and inter-regionally inequality in China (Meng and Zhang (2001), Whalley and Zhang (2007), Meng (2014)).

During Mao Zedong's reign, the household registration system kept people strictly in their place as part of central economic planning. Urban areas in counties and cities almost exclusively contained people with non-agricultural status, and people with agricultural status were kept in the rural countrysides. In the planned economy, one's status was also tied to one's employment. The rural labor force was supposed to farm ensure food supply to urban residents, who worked in factories or other government functions and received food rations. In one harrowing example, the requirement that rural areas needed to fulfill quotas of food supply to urban areas, coupled with inflexible central planning, led to dire outcomes during what became known as the Great Famine (Meng et al. (2013)).

Since 1978, a host of reforms have dramatically increased productivity in the agricultural and non-agricultural sectors, and created both an excess supply of agricultural workers as well as increased demand for low-skilled manufacturing labor in the non-agricultural sector (Lin (1992), Chow (2004), Meng (2012)). During this time, the household registration system has remained staunchly in place. Until the 1990s, legal migration – both inter-regional and rural-to-urban – entailed an official change in one's household registration, which had to comply with strict rules and quotas set down by the central government.[11] Illegal migration, which refers to taking up employment and residence outside of one's officially registered sector and locale, became the primary form of migration. However, illegal migration can be extremely costly, as it means that migrants may lose or temporarily forfeit social benefits and farmland to which they are entitled (whether in their home city or village, respectively).

Beginning in the 1990s, there have been some major *hukou* forms. According to Chan and Buckingham (2008), these reforms have happened along two dimensions. The first is the devolvement of *hukou* management from the central government to the local governments, who can now set their own criteria for granting local urban status. However, these criteria tend to be tougher for larger cities, which also have the largest pool of potential migrants. The second dimension is the gradual removal of the non-agricultural/agricultural distinction *within* certain locales, which tend to be small and medium cities. This means that in these cities will grant urban status to people with rural status who are *already* local, mostly people living in the outlying rural areas of the

[11] These rules have traditionally been stricter for larger cities. See Chan and Buckingham (2008).

city center.[12] Chan and Buckingham (2008) emphasize that "the equalization of rural and urban services does not mean anything to the migrant population who do not hold a local *hukou* at the destination."

Therefore, while in many cases the institutional costs of taking up urban status and employment has decreased for rural residents of the same administrative region, the institutional costs of taking up urban status and employment in a location other than that on one's *hukou* registration has remained high.

1.3 Local impacts of SEZs

In this section, I first present my main research design and the data I use. Then I proceed to show the local impacts of the SEZ program on output, and then on local government finance.

Research design

I use an event study design to estimate the local county-level impacts of the SEZ program. The sample includes only those counties that received their *first* SEZ in the 1999 (or 2000) to 2006 period. My main estimation equation is the following:

$$y_{ct} = \mu_c + \lambda_{p(c),t} + \sum_{j=a}^{b} \theta_j D_{ct}^j + \epsilon_{ct} \tag{1.1}$$

$$D_{ct}^j = \begin{cases} \mathbb{1}(t \leq \tau_c + a) & \text{for } j = a, \\ \mathbb{1}(t = \tau_c + j) & \text{for } a < j < b, \\ \mathbb{1}(t \geq \tau_c + b) & \text{for } j = b \end{cases}$$

y_{ct} is an outcome (e.g. $\log(GDP)$) for county c in year t. $\lambda_{r(c),t}$ represent region-by-year effects. In the results presented below, I take $\lambda_{r(c),t}$ to be either constant across counties, so that they are in fact year effects λ_t, or I take them to be region-by-year effects, where region refers to one of six traditionally defined regions of the country, or finally I take them to be province-by-year effects. D_{ct}^j is an indicator for county c having had its first SEZ by year t. In this expression, τ_c is the year in which county c received its first SEZ. Since I bin up the end points of the event window $[a, b]$, I need to make a normalization of the θ_j coefficients. I choose to normalize $\theta_{-1} = 0$, so that each estimated θ_j can be interpreted as the difference in outcome between the year immediately prior to the first SEZ establishment in county c, and year j relative to the SEZ establishment. If $\theta_j = 0$ for all $j < 0$, then I take the estimates of θ_j where $j > 0$ to be the impact of the SEZ program.

[12] A part of these new urban hukou granted was to rural residents whose land and livelihood were appropriated by the government for industrialization (Hsing (2010)).

In all reported specifications, I estimate the event-time dummies from a balanced sample of treated counties. The length of the event window, as measured by a and b, is mainly taken as $[-3, 6]$ to accommodate the limited time frame represented by the county-level data from *Province Statistical Yearbooks*. -3 is chosen so I estimate at least 2 pre-treatment periods, and 6 is chosen so that the last period of the event window is just estimated from a balanced sample of counties. I also present corroborating results using nighttime lights data. Since this data is available from 1992 on, I can test for the existence of a pre-trend in lights over a much longer pre-treatment horizon, where I take the event window to be $[-8, 6]$.

This event study design relies on there being variation in the timing of SEZ adoption across counties. Panel A of Figure 4 shows the variation of SEZ establishment over time. There is a steady annual increase in the number of new province-level SEZs in the country from 1993 to 2003. While no SEZ was established in 2004 and 2005 due to a temporary freeze on rural land conversion, the number of new SEZs established in 2006 peaked quite dramatically. This peak was due to both a delay in the creation of zones that would likely have been established in 2004 or 2005 had it not been for the land conversion freeze, and an anticipation of the tax amendment to come in 2007 that took away the tax advantage of SEZs vis-à-vis foreign-invested firms. Panel B of Figure 4 shows the number of *counties* that received their first SEZ in each year, again by the status of the SEZ.

Given the rush of SEZ creation in 2006, one concern may be that the event-time effects estimated from Equation (1.1) are driven by the effect on one cohort. I address this concern by estimating specifications where the 2006 cohort is not used to estimate the event-time effects. In those regressions, the event-time effects are identified from differential timing of the establishment of the first SEZ across counties. While the 2006 cohort represents 80% of the 2000–2006 treated counties, removing them from the estimation of event-times does not remove all heterogeneity in timing. For instance, 21 provinces have variation in the timing of SEZ establishment during 2000–2006, of which 12 provinces have variation during 2000–2003.

In all main specifications, the sample includes only counties that received an SEZ by 2006. By focusing on just the "treated" counties, I ensure that the θ_j estimates are identified off of the differential timing of SEZ establishment. The main issue with including the no-SEZ counties as a control group (in estimating the $\lambda_{r(c),t}$ only) is that, given the rush to establish SEZs in 2006 as depicted in both panels Figure 4, those counties that were left without an SEZ by 2007 are unlikely to be a good control group for the counties that did have an SEZ by 2007 (see Table 1 below). Thus, the trend in economic activity of the treated counties may not be well proxied by the pool of counties that never received an SEZ. On the other hand, those counties who have *yet* to receive an SEZ may represent the only viable counterfactual. This motivates my sample restriction to include only the ever-treated counties in the analysis.

If the SEZs were successful in attracting foreign-invested businesses into their hosting counties, then the expected impact on the hosting county's GDP would be positive. In addition, there would be a positive impact on county government revenue. This is because while new firms in the SEZs pay a reduced tax rate, they nevertheless contribute to the county government's revenue. In some cases, new firms in SEZs may receive a further

reduction in the income tax rate in the first 2–3 years of establishment. Thus, the impact of SEZs on county government revenue may increase over time.

[Table 1 about here.]

In Table 1, I present some summary statistics by each treatment cohort from 1999 to 2006, as well as the group of counties that did not have an SEZ by 2007. These are unweighted means and standard deviations of population, and farm share of employment from the Census years 1982, 1990, 2000, and 2010, *share of the county lit* according to the nighttime lights data in 1992, 2000, and 2010, as well as the time-invariant characteristics of maximum elevation, distance to the nearest port, and area. For all Census years, farm share tends to be larger for the later treated counties, with the exception of the 9 counties treated in 1999. However, taking the difference of the farm share variable between 1990 and 2000, this *change* in mean farm share of employment doesn't vary systematically across cohorts. It is also interesting to note that for the counties represented in the last 3 columns, population actually declined between 2000 and 2010. The bottom 3 rows show that the counties that did not have an SEZ by 2007 had on average much higher elevation than the treated counties. The untreated counties are also likely to be quite a bit larger than the treated counties. This suggests that the untreated counties on average occupied very different landscapes that limited their ability to develop a densely populated urban center. Indeed, as Figure 2 shows, the areas without an SEZ by 2007 are mostly in the western part of the country that tends to be less hospitable to settlement. Thus, the untreated counties may serve as a poor comparison group to the counties in the treatment sample.

Data

I bring together five county-level datasets in order to explore the dynamic effects of Special Economic Zones in the 1990s and 2000s.

The outcome variables in the event study analyses are based on annual county-level data from *Province Statistical Yearbooks* on GDP from 1997 to 2012, and government finance and population from 1996 to 2012.[13] Since these variables are generally not available at the county-level for the early- to mid-1990s, I will focus my event study analysis on the cohorts of SEZs established in 1999–2006. This group makes up about 70% of the total number of SEZs established since 1993. Second, I use data on population, employment count, employment shares in broad sectors, from the 1982, 1990, 2000, and 2010 County-Level Population Census. Information on SEZ establishment year, location, and type are obtained from the National Development and Reform Commission. I combine these administrative datasets with a dataset obtained from the Chinese Academy of Sciences, which includes county-level geographic variables and highway density as of the mid-1990s.[14]

[13] I obtain this data from the *www.chinadataonline.com* website hosted by University of Michigan's China Data Center.

[14] This is the data is used in Deng et al. (2008), and generously shared by Deng Xiangzheng.

To these official local government data, I bring in nighttime lights data for 1992–2012 from the Defense Meteorological Satellite Program's Operational Linescan System (DMSP-OLS). This dataset consists of an annual composite measure of nighttime light intensity on a 0-63 scale for each 30 arc-second grid (about 1 square kilometer at the equator), which has been edited to remove ephemeral lights and exclude pixels with thick cloud cover. I incorporate nighttime lights data for two reasons. The first is that due to the government data's limited coverage of the 1990s, I am restricted to estimating a relatively short pre-treatment event window when I estimate the SEZ effect on the official variables. Since nighttime lights are highly correlated economic activity and development, and they are available for all counties for most of the 1990s, they allow me to examine the existence of a pre-trend in a much longer pre-treatment event window.[15] The second reason is that a measure of economics activity that is independent from the government statistics bureau. Local government data tend to be more reliable than their aggregated central government counterpart, but we might still be concerned about systematic misreporting. Therefore the event study estimation using nighttime lights data provides a robustness check to the estimation using county GDP data. This second advantage of nighttime lights data come with its own caveats, for they are not a direct measure of output, and they tend to respond slowly to development in the short-run.[16]

Combining county-level datasets from multiple sources that span many years is complicated by the widespread administrative restructuring that took place since the 1980s. This led to a number of changes in the governance structure and identification codes of some county-level units. My approach to combining these county-level datasets involves matching counties by their time-varying identification codes and names throughout the sample period. I try to keep the area of the county-level observation constant as much as possible. Wherever county-level units were merged into other counties, I take the sum of these units to be a single county-level unit.[17] After merging together the above-mentioned datasets, my final dataset consists of $2,280$ consistent county observations.[18]

Results on economic activity

Main results on county GDP

I begin with presenting results from the event study analysis on county-level $\log(GDP)$. Baseline results from estimating Equation 1.1 are presented in Figure 5.

[Figure 5 about here.]

[15] See recently Henderson et al. (2011), Pinkovskiy (2013), and Michalopoulos and Papaioannou (2014) for a discussion of the use of nighttime lights in economics.

[16] Henderson et al. (2011) find that lights tend to reflect output produced using durable goods, which take time to accumulate.

[17] Most of these instances involve outlying counties of cities becoming a district in the urban center of the prefecture-level city.

[18] As an example of the consistent county-matching across time, 158 of the $2,280$ observations are aggregated from multiple 1982 county units.

The sample are counties that received their first SEZ between 2000 and 2006, where the first SEZ was at the province level. The 3 specifications represented in Figure 5 vary by the time effects estimated – year effects only, region-by-year effects, or province-by-year effects, in increasing order of saturation. Across all 3 specifications, there is no evidence of an upward sloping pre-trend. If the timing of SEZ establishment is predicated upon the county's growth rate, then we would expect the pre-treatment event time dummies to have negative effects. Instead, the year and region-by-year effect specifications show very flat pre-trends, while the province-by-year specification actually shows a *downward* sloping pre-trend. This suggests that within a province, counties may receive an SEZ in response to sluggish growth in order to reverse the trend. It's useful to recall that province-level SEZ establishment decisions are in fact made by provincial governments. In that case, it seems that the SEZ program succeeded in doing so.

Table 2 presents the estimates from the same three estimations as in Figure 5. Across all columns, there is a common pattern of a gradually increasing effect of the SEZ establishment on GDP, which manifests prominently beginning in the second after the program. While the post-treatment event-time effects are very similar and precisely estimated in Columns (1) and (2), they attenuated in the province-by-year effects specification in Column (3). Given the pre-trend estimates in Column (3) tell a story of counties being adversely selected into treatment within province, it is not surprising that the within-province post-treatment effects are less pronounced. On the other hand, the comparison between columns in Table 2 also suggests that there may exist complementarity between the SEZ treatment and the treated county's pre-treatment growth.

[Table 2 about here.]

Overall, the estimates in Table 2 suggest that the SEZ program led to between 6% and 11% increase in GDP by the 5th year of the program for the counties that received their first province-level zone between 2000 and 2006. From the second year on, there is a 1% to 2% per year increase in GDP relative to the year prior to the SEZ establishment. The effect of the SEZ on GDP is almost linear over time. The estimate on the binned up $t + 6$ event-time dummy suggests that there could be a larger impact of the program on local output after 6 or more years, although this cannot be tested with the balanced sample of 2000–2006 treated counties.

Robustness check 1: Excluding 2006 cohort

Since the 2006 cohort represents about 80% of the counties treated between 2000 and 2006, we may be concerned that the event-time effects in Table 2 are driven by the 2006 cohort only. I address this issue by estimating Equation 1.1 on the same sample of counties treated in 2000–2006, but excluding the 2006 cohort from the estimation of the event-time effects. However, the 2006 treated counties are kept in the estimation of the time or region by time nuisance parameters. Table 3 shows the results from these estimations.

Columns (1) through (3) of Table 3 mirror the specifications in Table 2, where the only difference here is that the event-time effects are identified from timing heterogeneity

between 2000 and 2003. This reduction in the variation of treatment timing increases the standard errors quite a bit in comparison to Table 2. While the estimates in Columns (1) and (2) of Table 3 are attenuated compared to their counterparts in Table Table 2, the estimates in Column (3) remain almost exactly the same. Across the first 3 columns, the results remain qualitatively the same as those with the 2006 cohort. In particular, there is still no evidence of an upward pre-trend, and the impact of the SEZ program is still increasing gradually over time.

[Table 3 about here.]

Since I do not use the 2006 treated counties to estimate event-time effects, I can extend the event window to $[-3, 9]$ using the 2000–2003 cohorts in order to estimate post-treatment effects over a longer horizon. The results from this estimation is reported in Column (4). The coefficients on the additional post-treatment dummies suggest that the SEZ's impact does not grow as strongly after year 5. The effects on $t+6$ and $t+7$ show a reversal of the growth effect, while the effects on $t+8$ and $t+9$ suggest that the SEZ effect returns to be about 1% per year in the longer run. However, it is not possible to draw a strong conclusion regarding the longer term effects of the SEZ program, given the large standard errors.

Overall, the estimates in Table 3 show that the impact of SEZs on county-level GDP does not differ qualitatively, or differ by much quantitatively, whether the 2006 cohort is included in the estimation of the event-time effects.

Robustness check 2: restricted subsamples

In Table 2, I presented results of the SEZ's impact on county-level GDP, estimated on the sample of all counties that received their first SEZ (province-level) in 2000–2006. In this section, I present results estimated on 3 restricted subsamples in order to address potential heterogeneous treatment effects.

I present results in Table 4. Column (1) reports the estimates from Column (3) of Table 2 for comparison purposes. The overall takeaway from Table 4 is that the results using the full sample of treated counties do not differ much quantitatively from the results in each of these three subsamples.

[Table 4 about here.]

In Column (2) of Table 4, the sample includes only counties that are not prefecture city centers, which are aggregated from districts. The post-treatment event dummy estimates here stronger in this subsample. This could be due to there being adverse selection in the treatment timing of prefecture-city centers, given that they were on average more urban and more likely to have had an SEZ earlier. Then those city centers treated in the 2000s may have been likely to respond little to the SEZ treatment. In Column (3), the sample includes counties that only ever have 1 SEZ before 2007. It is possible that multiple SEZs can dramatically amplify the SEZs' impact. However, the results estimated on the one-SEZ counties do not differ from the results using the full sample. Finally, in Column (4), I

exclude counties that are under province-level cities (i.e. Beijing, Tianjin, Shanghai, and Chongqing). One may expect the impact of SEZs in these counties is confounded by their proximity to some of the biggest cities in the country. As in Columns (2) and (3), there is no evidence in Column (4) that the estimates excluding counties under province-level cities are different from those using the full sample of counties treated between 2000 and 2006.

Robustness check 3: nighttime lights results

Using nighttime lights data aggregated to the county level, I examine the dynamic impact of SEZs on economic activity. Given this, my usage of nighttime lights data here is mainly to validate the finding of no pre-trend prior to SEZ establishment. Following Ma et al. (2012), my main measure of nighttime lights is the county's share of pixels lit.[19] I estimate two regressions of the form of Equation 1.1, each with region-by-year effects. The first takes the sample of 2000–2006 treated counties, and extends the event window to $[-8, 6]$, with 5 more pre-treatment event-time dummies. The second estimation keeps the event window at $[-3, 6]$, and instead extends the sample of counties to those treated in 1995–2000, whose first SEZ was sponsored by the province. This second specification allows me to generalize the findings to SEZs counties that were treated between 1995 and 1999.

[Figure 6 about here.]

The top panel of Figure 6 supports the no pre-trend finding in the regressions with county log(GDP) as the outcome variable. The pre-treatment event-time effects hover closely around 0, and alternate between signs. Though the post-treatment effects are not precisely estimated, they do seem to be increasing, especially after the 3rd year. More over the point estimates lie above the pre-treatment event-time estimates starting already in the 1st year after the SEZ.

The bottom panel of Figure 6 shows the results from estimating (1.1) on the sample of counties that received their first SEZ between 1995 and 2006, keeping the event window at $[-3, 6]$. In this specification, some of the post-treatment event-time effects are statistically different from 0. These results indicate that since the first year of SEZ treatment, counties have 1% more area lit due to the SEZ program. This effect may be increasing slightly over time as well.

Overall, Figure 6 shows that the nighttime lights data provide very similar qualitative results as do the regression s with county log(GDP) as the dependent variable. These findings help to alleviate concerns that the GDP data may be subject to systematic misreporting.

Results on county government finance

Now I turn to examine the impact of the SEZ program on the revenue and expenditure of the county government. In particular, revenue collection has been shown to be a boon to

[19] I find very similar results using county's log(light intensity) as the outcome variable, though the estimates are slightly less precise.

officials vying for promotion (Lü and Landry (2012)). If an SEZ increases the revenue of a county, then it would have been rational for the bureaucrat to lobby for the establishment of an SEZ in his jurisdiction in order to increase his chance of promotion.

Table 5 reports the event study estimates with $\log(Revenue)$ as the dependent variable. The samples represented in the columns are the same as those in Table 2, with the exception that I include the 1999 treated cohort in the analysis, since the revenue data begin one year earlier than the GDP data. As in Table 2, there is no evidence of an upward pre-trend in county government revenue leading up to the SEZ establishment. In Columns (1), (4), and (5), there is some evidence that government revenue may have been trending *downward*. The post-treatment event-time effects are much stronger for revenue than for GDP. Starting the first year after the SEZ establishment, government revenue has already increased 4% to 7% above trend. This effect persists in magnitude until about year 4, then it dips slightly in year 5. The results are consistent across all 3 specifications of time effects.

[Table 5 about here.]

This sizable effect on county government revenue is consistent with SEZs attracting new businesses to the targeted county. Since the local government is the sole claimant of business tax, it is not surprising that the SEZ would boost tax revenue.[20] Kung and Chen (2013) estimate that by 2008, business tax income represented 20 percent of county's budgetary revenue. The larger effect estimated for the binned up $t + 6$ event dummy is consistent with an even larger increase in tax revenue after the expiration of additional tax breaks. Moreover, the finding on revenue supports the idea that the county officials rationally lobbied for the establishment of SEZs in their jurisdictions.

I also examine the impact of SEZs on the county government's *expenditure*. Table 6 reports these estimates from the same samples as in Tables 2 and 5. Since SEZs are sponsored by the province, county-level governments do not bear most of the burden of funding SEZs. The findings here indicate that county government expenditure increased about 1% to 2% per year in the 5 years after the establishment of the SEZ, though unlike the previous event study results, the estimates in Table 6 show some evidence of an upward pre-trend.

[Table 6 about here.]

1.4 A model to assess the aggregate output impact of SEZs

So far I have found a positive impact of the SEZ program on local output and manufacturing employment. Part of this local productivity gain is likely due to the increase

[20] It is important to remember that the preferential tax policy granted by the SEZs applied only to foreign-invested firms that did not exist previously in the county. This means that county governments expected to receive more tax income from these new firms, even though the latter were taxed at a lower rate.

in capital in the hosting county. However, even if we assume that the supply of international capital to Chinese counties is infinitely elastic, so that capital reallocation is of no concern to the displacement of economic activity from elsewhere, we still cannot extrapolate from this local impact to the aggregate impact of SEZs. This is because the zones likely increased the marginal productivity of labor in the manufacturing sector of the hosting county, which would have resulted in an increase in manufacturing wage. This initial localized wage increase could then induce labor to reallocate from other localities or from the agricultural sector within and outside the hosting county. Therefore, the SEZ program's impact on economy-wide productivity is the sum of the productivity impact in each sector and locality.

In this section, I present a spatial equilibrium model adapted from the models underlying Moretti (2011), Busso et al. (2013), and Kline and Moretti (2014). This model features two cities, and each city has a labor market with two sectors – agricultural and manufacturing. Workers are perfectly substitutable for each other, but they have heterogeneous preferences over cities and sectors. I model the workers' choice over sectors and locations in a discrete choice framework.

The purpose of developing this simple model is to characterize the impact on aggregate productivity level of an SEZ-type policy. In the model, the equilibrium allocation of labor across cities and sectors depends on the cities' productivity levels, housing supply elasticity, and the distribution of workers' preference over cities and sectors. The equilibrium pinned down by these factors leads to a level of total productivity in the economy.

A two-city, two-sector model

Consider 2 cities, a and b, which I take to be small open economies. Each city supports two sectors, agriculture A and manufacturing M. Assume there is a continuum of agents of measure one, who choose to live and work in one of these two cities. Each worker also inelastically supplies one unit of labor one of the two sectors. If a worker chooses the manufacturing sector, she will consume one unit of housing and pay rent. This assumption reflects that agricultural workers often live on the land which they farm, and therefore do not participate in the city's housing market. Furthermore, one can think of the two sectors as occupying non-overlapping areas in the same city, as is often the case in counties and county-level cities in China.

The indirect utility of worker i in sector s of city c can be written as

$$\begin{aligned} u_{isc} &= \log w_{sc} - \log r_c \mathbb{1}\left[s = M\right] + \varepsilon_{isc} \\ &= v_{sc} + \varepsilon_{isc} \end{aligned}$$

where w_{sc} is the prevailing nominal wage in sector s in city c, r_c is the rental cost of housing in c, which applies only for M sector workers. v_{sc} is the average utility across all workers in the same sector-city cell. Here, ε_{isc} is the individual-specific preference over cities and sectors. This term can also be interpreted to represent heterogeneity in the costs that workers face for being in a sector or a city, including any exogenous restrictions

on moving outside of one's home city or working in the non-agricultural sector. Having heterogeneous agents is important for allowing different elasticities of cross-sector and cross-city labor supply when the equilibrium shifts due to a productivity shock to one sector of one city.

Let D_{isc} be a set of indicators, where $D_{isc} = 1$ if and only if $\max_{s'c'} u_{is'c'} = u_{isc}$, where $s' \in \{A, M\}$, and $c' \in \{a, b\}$. Then the measure of workers in city c and sector s is $N_{sc} = P(D_{isc} = 1|v_{s'c'})$. I assume that ε_{isc} are independently and identically distributed across workers according to a continuous multivariate distribution, and that they are independent of v_{sc}.

Now let's turn to the side of labor demand. The agricultural sector in each city produces output using only labor in the agricultural sector N_{Ac} and technology X_{Ac} according to the constant marginal returns production function

$$Y_{Ac} = X_{Ac} N_{Ac}$$

Output Y_{Ac} is traded globally at price ϕ. Farms are assumed to be competitive, so the inverse agricultural labor demand is equal to the marginal revenue product of labor.

$$\log w_{Ac} = \log \phi + \log X_{Ac} \tag{1.2}$$

The manufacturing sector in each city produces output using labor in the manufacturing sector N_{Mc}, capital K_c, and a fixed factor F_c according to the constant returns to scale Cobb-Douglas production function

$$Y_{Mc} = X_{Mc} K_c^{\alpha} F_c^{\beta} N_{Mc}^{1-\alpha-\beta}$$

where X_{Mc} is the city's exogenous manufacturing productivity level, which includes productivity enhancing infrastructure and geographical features that may be conducive to manufacturing activity. I assume that capital is supplied perfect elastically by the global financial market at a fixed price ρ.[21] Output Y_{Mc} is sold on the international market at a price normalized to one. Assuming firms are price takers, the inverse manufacturing labor demand is

$$\log w_{Mc} = C - \frac{\beta}{1-\alpha} \log N_{Mc} + \frac{\beta}{1-\alpha} \log F_c - \frac{\alpha}{1-\alpha} \ln \rho + \frac{1}{1-\alpha} \log X_{Mc} \tag{1.3}$$

where $C \equiv \log(1-\alpha-\beta) + \frac{\alpha}{1-\alpha} \log \alpha$.

I assume that, the marginal productivity of labor in the manufacturing sector is always greater than the marginal productivity of labor in the agricultural sector, so that for each $c \in \{a, b\}$, $\log w_{Mc} \geq \log w_{Ac}$. This simplifying assumption allows us to focus on the predictions in the more realistic and interesting case where manufacturing is by large the more productive sector. This is not a reasonable assumption given that urban

[21] Perhaps a more compelling assumption in the case of China may be that cities face different costs of capital. Then the SEZ program can be modeled as a subsidized reduction in ρ_c. However, this way of modeling the SEZ would generate a similar wage increase in the manufacturing sector of city a, and have similar implications for the subsequent adjustment of labor across cities and sectors.

household income has persisted at about three times that of rural households throughout the 2000s.[22]

To close the model, I specify conditions in the housing market. Manufacturing workers in city c have demand for housing given by

$$N_{Mc} = P(D_{isc} = 1|v_{s'c'}(r_{c'}))$$

For the supply of housing units to manufacturing workers, I assume that there are absentee land developers who develop and supply urban housing units at a rate that increases with the number of manufacturing workers who reside in each city

$$r_c = zN_{Mc}^{k_c} \tag{1.4}$$

where k_c captures the elasticity of housing supply in city c, which depends on the city's building regulations and geographical features. For instance, if $k_c = 0$, then housing is perfectly elastically supplied at a constant price. In reality, the absentee landlords are in fact the local government. However, I assume here that the government does not invest the revenue from housing development in a way that affects productivity.[23]

In equilibrium, workers maximize utility, and labor and housing markets clear. The share of workers in each sector-city cell is determined by $P(D_{isc} = 1|v_{s'c'})$. Heterogeneity in agents' preference over sector-city cells leads to unequal worker utility across cities and sectors. Suppose that the exogenous productivity level in the manufacturing sector is greater in one city than in the other, then we would expect more workers in the manufacturing sector of the relatively more productive city, so long as the supply of housing in that city as it becomes more populated with M workers is fairly elastic, and that the underlying distribution of workers' taste over cities is not overwhelmingly skewed toward the less productive city. If manufacturing productivity is equal across cities, then the city with the more elastic housing supply is expected to have a larger population of M workers, assuming again that workers do not inherently find the city unattractive.

In this model, the choice between the relatively less productive agricultural sector and the relatively more productive manufacturing sector depends on the housing supply elasticity and the workers' preference one sector over another. It could be that many workers find it inherently costly to take up manufacturing jobs in the urban area, so they would rather choose the agricultural sector despite facing a much lower wage. In the case of China, the rural-urban designation set down by the government is the major determinant of this cost/preference parameter.

For the purpose of illustration, I consider a particular example of ε_{isc} as the joint distribution of two independent uniform distributions, where one is for the preference over sectors, and another is for the preference over cities:

$$(e_{iM} - e_{iA}) \sim \text{Uniform}[\mu_s - \sigma_s, \mu_s + \sigma_s]$$
$$(e_{ia} - e_{ib}) \sim \text{Uniform}[\mu_c - \sigma_c, \mu_c + \sigma_c]$$

[22]Source: *China Statistical Yearbooks*.

[23]Cai et al. (2013) find evidence of corruption in the land leasehold market, and Kung and Chen (2013) find that county government officials do not spend revenue windfalls from land development on projects that could potentially enhance social welfare.

The scale parameters, $\sigma_s \geq 0$ and $\sigma_c \geq 0$ determine how willing workers are to move from one sector or one city to another. For instance, a large σ_c means that workers strongly prefer to live in either city a or city b, so that they will need a large price inducement to move across city borders. $\mu_s \in \mathbb{R}$ and $\mu_c \in \mathbb{R}$ shift the distribution of sector and city preference distributions, respectively, so the latter need not be symmetric over the choices.

A strict inter-regional migration policy can be reflected in a high σ_c, while a high inter-sector switching cost is reflected in a high σ_s. $\mu_s < 0$ indicates that some workers find it inherently costly to work in the manufacturing sector. Thus, there may be many workers in the less productive agricultural sector despite higher wages in the manufacturing sector, as in the case of China.

The equilibrium productivity of this two-city, two-sector, economy is given by the sum of productivity over the four sector-city cells

$$Y = \sum_c \sum_s Y_{sc}$$

It is straightforward to see in this formulation that economy-wide productivity Y is larger when there are more workers in the manufacturing sector, and when the there are more workers in the city that hosts the more productive manufacturing sector.

SEZ's impact on economy-wide output

Let us consider now how an SEZ in city a might affect aggregate output. I model the SEZ as a boost to the productivity level of city a – $X_{Ma'} > X_{Ma}$. This formulation captures the part of the SEZ program that invests a large sum into the building of infrastructure and fixed capital. Given that manufacturing firms make use of factors such as utilities, waste management, and road transportation in their production, this investment serves to boost productivity of all manufacturing firms.

The increase in manufacturing productivity level of city a has a positive direct impact on total productivity in the M sector of city a only. The indirect impact of this policy depends on the shift in the equilibrium allocation of labor across the 4 sector-city cells. This indirect productivity impact can be decomposed into the part that results from the shift of labor across within manufacturing from city b to city a, and the part that results from the shift of labor from agriculture into manufacturing within city a and from city b.

The elasticity of economy-wide productivity with respect to X_{Ma} can be written, in Equation 1.5 as the sum of direct and indirect impacts. Define $\varsigma_{sc} \equiv \frac{\partial \log N_{sc}}{\partial \log X_{Ma}}$. The last line of Equation 1.5 rearranges the expression to consist of the impact on the targeted

city and the displacement impact from elsewhere.

$$\begin{aligned}
\frac{d\log Y}{d\log X_{Ma}} &= \frac{1}{Y}\sum_s\sum_c Y_{sc}\frac{d\log Y_{sc}}{d\log X_{Ma}} \\
&= \frac{1}{Y}\sum_s\sum_c Y_{sc}\Bigg(\underbrace{\frac{\partial\log Y_{sc}}{\partial\log X_{Ma}}}_{\text{direct impact}} + \underbrace{\frac{\partial\log Y_{sc}}{\partial\log N_{sc}}\varsigma_{sc}}_{\text{indirect impact}}\Bigg) \\
&= \frac{1}{Y}\Bigg[\underbrace{Y_{Ma}\left(\frac{\partial\log Y_{sa}}{\partial\log X_{Ma}} + \frac{\partial\log Y_{Ma}}{\partial\log N_{Ma}}\varsigma_{Ma}\right)}_{\text{effect on targeted county-sector}} \\
&\quad + \underbrace{Y_{Aa}\left(\frac{\partial\log Y_{Aa}}{\partial\log N_{Aa}}\varsigma_{Aa}\right) + Y_{Ab}\left(\frac{\partial\log Y_{Ab}}{\partial\log N_{Ab}}\varsigma_{Ab}\right) + Y_{Mb}\left(\frac{\partial\log Y_{Mb}}{\partial\log N_{Mb}}\varsigma_{Mb}\right)}_{\text{dispacement effect}}\Bigg]
\end{aligned} \tag{1.5}$$

The indirect productivity impact associated with reallocation across cities within manufacturing is the gain in manufacturing productivity in city a net of the loss in manufacturing productivity in city b. This means that, if the SEZ program is targeted at the relatively less productive city, then there is a net loss in productivity resulting from workers' reallocation across the cities. The indirect productivity impact associated with reallocation across sectors is the gain in manufacturing productivity in city a net of the loss in agricultural productivity in cities a and b. The sign on this effect is unambiguously positive under the assumption that manufacturing is the more productive sector. Thus, the *total* indirect productivity impact of the SEZ program depends on the relative magnitudes of the two types of labor reallocation, the cross-city difference in manufacturing productivity, and the cross-sectoral differences in productivity.

Referring back to the joint uniform distribution example of ε_{sc}, when σ_c is large relative to σ_s, an increase in the manufacturing wage in city a will lead to more reallocation of labor from the agricultural sector of city a than from the manufacturing or agricultural sector of city b (provided that μ_s is not very small).

Finally, since the number of workers is fixed in this model, it must be that reallocation satisfies:

$$\sum_s\sum_c \Delta N_{sc} = 0 \tag{1.6}$$

That is, changes in sector-city cells' population must sum up to 0.

In the next section, I show that SEZs did not have an impact on *total* population in city a. This implies that the only reallocation that occurred is the inter-sector reallocation from agriculture to manufacturing, since

$$\Delta N_{Ma} + \Delta N_{Aa} = 0 \implies \Delta N_{Mb} + \Delta N_{Ab} = 0 \tag{1.7}$$

1.5 Aggregate impact of SEZs

In this section, I take to the data to estimate the economy-wide impact on productivity of the SEZ program. First, I implement the same event study analysis as in Section 1.3 with $\log(Population)$ as the outcome variable. I then examine changes in the within-county *share* of the labor force employed in the agricultural sector, as well as the change in the *size* of the agricultural labor force in the county. Then, with these findings I calculate the share of the SEZs' overall productivity impact that can be attributed to the reallocation of labor from the agricultural sector.

Inter-regional labor reallocation

As a first step in uncovering the aggregate effect of SEZs, I examine the program's impact on population in the targeted counties. Table 7 reports the event study results with $\log(Population)$ as the dependent variable. As before, the specifications differ by the time and region effects included.

Across all specifications, there is a common result that the SEZ program had zero impact on the population of the targeted counties. That is, while output and government revenue both increased due to the SEZ, population did not. This result may not be surprising in light of the fact that, as discussed in Section 1.2, mobility across counties was more restricted after the 1990s relative to mobility within county from rural areas to urban areas. Therefore the inter-regional mobility costs may be high enough to have prevented much in-migration. Moreover, since the counties that received their first SEZ in the late 1990s and 2000s were not the biggest cities, then it's possible that very few of the workers in the country who find it worthwhile to move would move to *these counties*, rather than to bigger cities that potentially offer still a higher real wage.

[Table 7 about here.]

Table 7 show that SEZs did not induce labor reallocation across counties. In the context of the model presented in Section 3, this means that $\varsigma_{Mb} = 0$ and $\varsigma_{Ab} = 0$, where b corresponds to all counties other than the county hosting the SEZ in the generalized multi-county setting. Given that inter-regional displacement does not enter into the aggregate productivity impact of SEZs, the total local impact estimated in Section 2 can be interpreted as the effect of SEZs on aggregate productivity.

Inter-sector labor reallocation

In this section, I examine within-county labor reallocation from agriculture to manufacturing. Given that in the previous section I find no evidence that SEZs led to inter-regional labor reallocation, we are left to see if within-county labor movement away from agriculture does enter into the SEZs' impact on aggregate productivity. To investigate this latter type of displacement, I use county-level Population Census data from 1982, 1990, 2000, and 2010. In particular, I examine whether a county's decadal change in

agriculture's share of employment or the change in the county's agricultural population is affected by the number of years the county has had its first SEZ during the decade.

I estimate regressions of the following form

$$\Delta_{c,decade} = \alpha + \beta X_c + \lambda_{p(c)} + \gamma L_{c,decade} + \epsilon_c \tag{1.8}$$

where $\Delta_{c,decade}$ is county c's change in agriculture's share of employment (or change in agricultural employment), over a specified decadal interval: 1990–2000, 2000–2010, or 1990–2010. X_c is a vector of control variables that include beginning-of-decade agriculture's share of employment (or log agricultural employment), log population, the previous decade's change in population and agriculture's share of employment (or agricultural employment), as well as the county's elevation and distance to the nearest port. $\lambda_{p(c)}$ are province fixed effects, and $L_{c,decade}$ is the intensity of the SEZ treatment in county c, as measured in years having had the first SEZ, during the specified decade. The results are presented in Tables 8 and 9.

[Table 8 about here.]

[Table 9 about here.]

In Columns (1) – (3) of Tables 8 and 9, the estimation sample is the group of counties that had their first SEZ during the decade (not inclusive of the last year of the decade). In Columns (4) – (6) of Tables 8 and 9, the estimation sample includes those counties that were treated in the following decade (as in Panel A), or the counties that were never treated (as in Panels B and C). These additional counties all have 0 years treated during the specified decade.

Panels A, B, and C of Table 8 show the relationship between counties' SEZ treatment intensity and the change in agriculture's share of employment over the 1990–2000, 2000–2010, and 1990–2010, respectively. In Column (3) of Panel A, where I control for the set of pre-treatment characteristics as well as province effects, I find that an extra year of hosting an SEZ in 1990–2000 is associated with a 1% decrease in agriculture's share of employment over the decade. Moving down to Panel B Column (3), I find that an extra year of hosting an SEZ in 2000–2010 is also associated with a 1% decrease in agriculture's share of employment over that decade. Pooling both decades, in Panel C Column (3), I find that an extra year of hosting an SEZ during 1990–2010 is associated with a 0.23% decrease in agriculture's share of employment over the 20 years. Columns (4) – (6) show similar results qualitatively, though the binning up of the 0 treatment year group attenuates the estimates, for instance Column (6) relative to Column (3). These results suggest that while SEZ establishment did not induce workers to move in from other counties, it did induce agricultural workers in the hosting county to move into the non-agricultural sector.

Table 9 follows the same format as Table 8, where the dependent variable is the difference between the log agricultural employment at the end of the decade and the log agricultural employment at the beginning of the decade. The results here show that not only did agriculture's share of employment decline with SEZ treatment intensity, so did

the raw number of agricultural workers. Moreover, the magnitudes of the effects found in Tables 9 are in line with the findings in Table 9. For instance, treated counties between 2000 and 2006 began the 2000–2010 decade with 70% of employment in agriculture. In that case, a 1.4% per year decline (Panel B, Column (3)) in agricultural employment translates into about 1% change in agriculture's share of employment.

1.6 Discussion

In this paper, I find that the SEZ program in 1993–2006 generated a positive impact on the output in the counties that hosted the zones. I develop a simple spatial equilibrium model to assess the aggregate impact of the SEZ program. The main result of the model is that the economy-wide output impact depends on the elasticities of inter-regional and inter-sector labor supply, as well as the relative productivity levels of the manufacturing sector of different cities. Taking this model to the data, I do not find an effect of SEZs on inter-regional labor reallocation. Thus, the sum of the local impact of SEZs can be interpreted as the impact of the program on aggregate output. Furthermore, this finding is consistent with the nature of labor mobility restrictions in China, and the idea that the SEZ program was instituted partly as a way to ameliorate the adverse productivity effects of the restrictions to inter-regional mobility.

Chapter 2

Bureaucrat incentives and protection of land and environment in China

2.1 Introduction

Bureaucracy plays an operative role in enforcing government policy and the incentives of bureaucrats often have important consequences on social welfare. The bureaucracy of China regulates most economic activities, including land use and environmental quality. As rapid industrialization and urbanization call for increasing development of rural land and generate ever higher levels of pollution, the bureaucracy's actions have mounting economic as well as social significance. This project aims to understand the institutional reasons behind rural land conversion and air pollution in China by examining the incentives of the bureaucrats involved in implementing land and environmental protection policies in local governments.

In China, land is classified as either rural or urban. While urban land belongs to the state and can be leased and used for industrial and other development purposes as sanctioned by the Land Administration Law (first adopted in June 1986 and heavily amended in December 1988), rural land is owned by the village collective and is intended for agricultural uses only. In the last two decades, land lease sales have constituted a substantial portion of local government revenue (Lin and Ho (2005) and Lichtenberg and Ding (2009)).[1] Associated with the development of urban land is the conversion of rural land to urban land status, since rural land is restricted to being used for agriculture. Rural land can be developed only after it has been officially converted to the status of urban land, transferring its ownership from the village collective to the state. This conversion of rural land is initiated by the local government (at the county, prefecture, or provincial level) and is heavily monitored and restricted by the central government, who is concerned about preserving food and feed security, as well as social stability. The central government delegates the supervision of rural land conversions to the provincial Bureau of Land and Resources (BLR). The BLR is the provincial extension of the Ministry of

[1] From here on, I use the term "local government" to refer to government *at or below* the provincial level.

Land and Resources.[2] Local politicians have a strong incentive to convert rural land. Not only does the sale of urban land-use leases generate enormous revenue for the local government, high revenue along with visible urban development are also shown to boost the chances for promotion of local officials (Shih et al. (2012)).

It is the duty of the BLR to carry out the policies set down by the State Council in Beijing. However, the head of the BLR is appointed to his position not by the central government, but by the provincial Party Committee which is almost always headed by the provincial Party Secretary, from here on referred to as the provincial PS.[3] Therefore, the head of the BLR is accountable to two superiors with conflicting objectives - the central government in Beijing that wants to restrict rural land conversion and the provincial leadership that is interested in converting rural land.

Similarly, the provincial Environmental Protection Bureau (EPB) is tasked by the central government to regulate environmental pollution in the provinces. The leaders of the EPBs, like those of the BLRs, are appointed by the provincial Party Committee. Therefore, the head of the EPB is also simultaneously accountable to the central as well as the provincial government. Since pollution is often a byproduct of rapid economic growth, we would expect the head of the EPB to also be conflicted, given the diverging goals of the two superior bodies of government.

In this project, I focus on the personal relations between the provincial PS and the BLR (EPB) head that he appoints. I ask whether the appointment of a bureaucrat to the position of the BLR (EPB) head can be viewed as a kind act on the part of the provincial PS. In this context of reciprocity, the BLR (EPB) head will want to reciprocate the PS's career advancement favor to him by aligning his actions (i.e. the quantity of rural land conversion or air pollution) more with the desires of the PS and away from the goals set down by the central government.

I test whether there is more rural land conversion (or air pollution) in a province when the head of the BLR (EPB) is appointed by the incumbent provincial PS and not by a previous PS. The provincial PS and the head of the BLR (EPB) serve mostly 2-4 year terms that do not necessarily overlap completely, and the provincial PS does not appoint new people to all high-level posts in his jurisdiction at the start of his term. I exploit variation in the overlap of the terms of provincial PS's and of the corresponding BLR (EPB) heads to categorize each province-year as either one where the provincial PS *appointed* the BLR head or one where the provincial PS *inherited* the BLR (EPB) head. I hypothesize that an "appointing" provincial PS converts more rural land than an "inheriting" provincial PS because the former is aided by a BLR head who wants to reciprocate the PS's kind act of appointing him to him post.

[2]The Ministry of Land and Resources is a branch of the State Council, which is the policy-making organization of the central government. The BLR is a branch of the provincial people's government. The State Council and the provincial people's government are overseen by the Party committee at the central and provincial levels, respectively.

[3]The provincial PS serves as the head of the provincial Party Committee unless he is appointed as a member of the Politburo, which consists of around 20 members. Huang (2002) tallies that 14 percent and 17 percent of provincial PS's served on the Politburo in the 13th (1987-1992) and 14th Party Congress (1992-1997), respectively.

I study a particular aspect of the bureaucracy in China and its implications for land use and environmental policy. Although the conflict between the central government and the sub-national level governments has been documented widely, few studies look at how this conflict unfolds in practice and its subsequent effects on policy outcomes. I focus on land and pollution not only because these are areas where central and local government interests clash, but also because the implementation of these policies has strong implications for the livelihood of the rural population, productivity, and political stability in China. Therefore, understanding the incentives of the bureaucrats who determine and implement these policies is essential for understanding the future of resource use, environmental protection, social welfare and the political institutions of China.

2.2 Related literature and contributions

Literature review

This project is embedded in the literature that explores the institutional determinants of land use policy in China. More broadly, this work is related to other papers that try to pin down or test specific hypotheses of bureaucrat incentives in China. Below, I discuss how my project relates to existing work in these areas of research as well as how my project differs from existing work.

This project spotlights the incentives and actions of the BLR (EPB) head, a bureaucrat caught between conflicting goals of the central and the provincial governments. This tension between the center and sub-national regions is a theme that pervades studies of the political organization of China. China's bureaucracy is organized in multi-divisional form (M-form), where provincial governments function as self-containing divisions of the national government. This is in contrast to the unitary form (U-form) organization of the former USSR, where various ministries performed complementary tasks; all ministries were interdependent sections of the national government. Maskin et al. (2000) show that, under certain conditions, the M-form organization generates better incentives for the subdivisions of the government than the U-form organization because the former promotes yardstick competition. Huang (2002) acknowledges personnel management as a major challenge faced by China's central government. Huang examines the industrial organization of the Chinese bureaucracy and points out the explicit and implicit mechanisms used by the central government to monitor provincial and ministerial officials.

Some previous works on the institutional determinants of land conversion have focused specifically on the different objectives of the central and local governments. Lin and Ho (2005) examine how land management is shaped by the state structure vis a vis the conflict between the central and local governments. Using field data and data from the 1996 land survey conducted by the Chinese government, they find evidence of much subversion at the local levels of government in response to the central government's land policies. Chau and Zhang (2011) theoretically evaluate a land development allowance policy that is similar to the 2007 policy that allows 60 percent of newly rehabilitated farmland to be used as compensation for the land conversion quota. They argue that

such a policy can resolve the issue of opposing interests of the different administrative levels.

A number of works have already explored how land use policy is affected by other institutional factors such as the tax policy and the term length of local bureaucrats. Lichtenberg and Ding (2009) highlight the effects of fiscal decentralization on the rate of urban expansion via rural land acquisition. Fiscal reforms in the 1990s generated strong incentives for local officials to leverage their exclusive control over the allocation of land to finance local government projects. Lichtenberg and Ding conclude that these reforms led local officials to behave like land developers by showing that the rent gradient patterns in Shanghai and nearby provinces are similar to those generated by a free land market. Cai (2011) studies the allocation of land conversion quotas across industries and finds that municipal PS's who have longer time horizons are more likely to allocate quotas to industry rather than to commercial development, where industrial development tends to yield longer-term benefits for the locality but commercial development generates a higher upfront payment. This finding is consistent with the local politician maximizing personal gain when his time horizon is short.

This project is rooted in the broad body of work that tries to understand the behavior of Chinese bureaucrats, whose incentives (for personal profit, promotion, etc.) ultimately shape policy outcomes. Rozelle and Boisvert (1994) try to pin down the incentives and motivations of village leaders in China. They find that personal profit and independence from higher-level officials dictate the actions of the village heads. Using the same dynamic control model, Rozelle and Boisvert (1995) find that the ineffectiveness of certain agricultural reforms can be traced to the village heads' manipulating policy instruments, such as shifting resources from agriculture to industry in order to boost personal gain. Although village leaders are technically not members of the bureaucracy, they are nevertheless enforcers of government policy and are liable to structure policy according to personal incentives.

Cai (2011) shows the "end-of-term" effect applies to municipal politicians in China.[4] Moreover, Cai shows that the personal incentives of the PS of the municipality, and not the mayor, that matter for the allocation of land quotas. This finding corroborates my focus on provincial PS instead of the provincial governor. Personal incentives have been shown to influence policy outcomes at the province level. Persson and Zhuravskaya (2011) test the hypothesis that elite capture can serve as an informal substitute for direct accountability for provincial PS. They find that a provincial PS who is informally tied to provincial elites provide more public goods by shifting resources toward health care and education and away from construction. Finally, Jia (2012) shows the complementarity between political connections and growth for the promotion of provincial governors. Jia's paper also exploits the within-term variation of the connection between a policy-enacting bureaucrat – in this case the provincial governor, and his superior – in this case the members of the Standing Committee. Compared to Jia's paper, my paper has additional identifying variation that comes from the ending of the term of the provincial PS.

[4]Besley and Case (1995) document differential behavior among Democrat governors near the end of their terms depending on whether they are able to run for office again.

Contributions

While Lichtenberg and Ding (2009) and Cai (2011) study county and municipal officials, only the *central and provincial* governments have the "right to requisition rural land for infrastructure, public services and other designated uses as well as exercising oversight over the activities of local urban and village officials."[5] My project focuses on provincial administration because it is the provincial government that controls all plans for rural land requisition and conversion originated at the township, municipal and county levels.[6] Even though by focusing on provinces instead of counties or municipalities I will have fewer observations than did most previous studies, I will have the benefit of examining the level of regional government that actually has final authority over most of the county- and municipality-level land requisitions.

Compared to works that highlight the central-local conflict, this project goes beyond identifying potential problems with China's bureaucracy, and attempts to measure the effects of the structure of the bureaucracy on rural land conversion in China. By explicitly delineating the incentives of the central government, the provincial Party bureaucrat and the provincial bureau leader, I generate testable hypotheses regarding the effects of central-local conflict on rural land conversion. Given the relative opaqueness of China's bureaucratic operations, this project contributes to our understanding of such operations by evaluating the importance of personal relations within the bureaucratic structure.

2.3 Institutional background

Central government - China's land policy

Land ownership in China is divided between the state, which owns urban land, and village collectives which own rural land. However, the state retains the right to requisition rural land if it is in the "interest of the public" to do so. In 1988, the State Council established land leasehold markets by separating land use rights from land ownership rights.[7] As a result, state-owned urban land is allowed to be leased to private developers for a fixed number of years. Conveyance fees collected by the local governments from selling land use leases began to make up a dominant portion of the extra-budgetary revenue (Cai (2011)).

Under the dual ownership system, rural land must first be converted to the status of urban land before it can be used for non-agricultural development purposes. Rapid urban development since 1988 also caused rural land to be increasingly converted to

[5]Lichtenberg and Ding (2008)

[6]The Land Administration Law specifies that all land requisition plans originating below the provincial level (i.e. county, municipality and township) must be reviewed and approved by the provincial government, save the plans for municipalities with population exceeding one million, which must be reviewed directly by the State Council - Regulations on the Implementation of the Land Administration Law, 1998, Article 21.

[7]The duration of land use leases varies from 70 years for residential construction, 50 years for industrial and 40 years for commercial uses. See Interim Regulations on Transfers of Urban State-owned Land Use Rights, 1990, Article 12.

urban status in order to be used for residential, industrial or commercial construction. As urbanized area increased by 7.5% annually in China from 1985 to 1994, farmland area decreased (Ding, 2003). The tax reforms implemented in the mid-1990s further boosted local officials' incentive to convert rural land by making local governments more reliant on extra-budgetary revenue to finance local public expenditure.

In 1994, the State Council passed the "Basic Farmland Protection Regulations" that prohibited the conversion of basic farmland.[8] In 1998, the State Council passed the "Regulations on the Implementation of the Land Administration Law" that legally increased the degree of regulation on land conversion and set quotas for rural land conversion and farmland protection.[9] The 1998 regulations did not lead to the leveling off of rural land conversion, as urban area continued to expand and land-related revenue grew from less than 10% of total budgetary revenue in 1999 to 55% of budgetary revenue in 2003-2004 (Deng et al, 2006 and Lichtenberg and Ding 2009). The next major amendment of the Land Administration Law came in 2004. Among the revisions, the State Council instituted a moratorium on *net* conversion of farmland, and stipulated that any conversion of farmland must be accompanied by the rehabilitation of other land for agricultural purposes.[10] Violation of this regulation can lead to planned land development projects being halted by central government regulators.

In sum, after the introduction of land leasehold markets in the late 1980s, the central government has felt the need to regulate rural land conversion, and each subsequent revision of the Land Administration Law has been more restrictive than the previous one.

Bureaucracy - the provincial Party committee and the BLR

China operates under the dual-track political system made up of the CCP and the People's government. China's *nomenklatura* system leaves the power to appoint personnel in the hands of the Party. Moreover, when the preferences of the government and the Party diverge, the Party "should use its authority over Party members in the bureaucracy to impose (the Party's) preferences on the government." (Lieberthal et al. (1992)) In what follows, I use the term "provincial government" to indicate provincial leadership as represented by the provincial Party organization, which holds authority over the provincial People's government.

This project zooms in on the tension between the central government and the provincial government in the realm of land use policy. As described in the previous section, the central government is wary of rapid rural land conversion due to potential threats to food security, feed security and political stability. The provincial government, on the other hand, faces fiscal pressures and incentives to promote local growth that draw heavily on

[8]Basic farmland include: (1) agricultural production bases of crops, cotton, edible oils, and other high quality agricultural products, as approved by the government; (2) high productivity farmland that has a good irrigation system and that has been exploited; (3) vegetable production bases for large and middle cities; and (4) experimental field for agricultural science and educational purposes.

[9]Violation of these land quotas can result in warnings or decrease in future quotas.

[10]In my empirical analysis I will control for the observation being from after 2004.

revenues from land lease sales, including the sales of converted farmland. Maskin et al. (2000) and Li and Zhou (2005) find that provincial growth increases provincial bureaucrat's chance for promotion. Recent work by Shih et al. (2012) demonstrates that local politicians who achieve higher local revenue growth are more likely to be promoted. Cai (2011) conducted numerous interviews with municipal leaders in China and confirms the importance of local revenue for cadre evaluation. Thus the central and the provincial governments have conflicting objectives regarding the conversion of farmland - the center wants to contain it while the provincial government wants to promote it. This tension is manifested in the central government's attempt to limit rural land conversion by issuing restrictive addenda to the LAL beginning in the mid-1990s.

The BLR is at the nexus of the conflict between the center and the province. The BLR is the province-level subsidiary of the Ministry of Land and Resources, which is an organ of the State Council. The responsibilities of the BLR (as published on the websites of the provincial BLRs) include drafting and implementing land use plans for the province and reviewing land use plans drafted at the county-, municipality- and township-levels, even those plans that ultimately have to be approved by the State Council. The BLR also supervises all land development and rehabilitation within the province. The list of duties of the BLR suggests that this organization is guided by the goals of the central government, as the BLR is tasked with the overall preservation of farmland and the strict protection of basic farmland.

Despite that the BLR is professionally accountable to the central government, the leader of the BLR is not appointed by the Ministry of Land and Resources in Beijing, but by the provincial Party committee. The provincial Party committee is the group that heads the provincial Party agency. Manion (1985) states that "Provincial Party committees control (by territorial hierarchy) offices at the prefectural and county levels and (by functional hierarchy) offices at the provincial departmental (*ting*) and divisional levels in organs of the provincial Party committees and provincial people's governments." In particular, the provincial Party committee is in charge of appointing officials to posts at the provincial departments in organs of the people's government, including the provincial BLR. The chair of the provincial Party committee is the provincial PS, unless the PS currently serves on the Politburo, which is fairly uncommon. These features of the provincial Party committee allow me to trace the appointment decision of the BLR head to the provincial PS. Thus, while the BLR is supposed to enforce land policy set down by the central government, its leadership is determined by a provincial politician who is likely to have land use goals that disagree with those of the central government.

The head of the BLR operates as the middleman between the center and the province. Melanie Manion's chapter in Lieberthal and Lampton (1992) presents a study of the link between the top and the bottom of the policy implementation process by focusing on the bureaucrats who act as middlemen. In the case study of the cadre retirement policy, she notes that "in making choices about transforming policies into actions, it made sense for middlemen to consider the relative difficulty of their tasks. Obviously, policies that challenged vested interests would not meet with ready compliance." She concludes that

> "When there is a lack of consensus among policymakers, what middlemen

> do or do not do is a choice that reflects a stand-and potential political risk. Further, middlemen are in positions of responsibility toward policymakers and of authority over ultimate targets of a policy. This means they must justify their actions to both, with reference to policy. Finally, middlemen have a number of tasks in their issue area at any given time. Anything they do consumes limited resources (such as personnel), and when they face contradictory demands they must choose to use resources on one task or another."[11]

The dilemma faced by the middlemen who had to implement the cadre retirement policy can be readily translated to fit the context of the middlemen (the BLR heads) who have to implement restrictive land use policies. The BLR head is accountable to policymakers at the central government, but must also justify his actions to the provincial PS, whose promotional chances (and possibly personal benefits) may suffer as a result of strict control of rural land conversion. See Figure 7 for a summary of the relationship flows between the central government, the provincial PS and the provincial BLR head. This relationship dynamic applies also to the head of the EPB, since the EPB is similarly caught between its stated objective of regulating pollution as set by the central government and the local government pressure to increase industrial development and as a consequence air pollution.

[Figure 7 about here.]

2.4 Research design

Hypothesis

In this paper, I test whether in years when BLR (EPBs) heads work concurrently with the provincial PS who appointed them (heretofore referred to as working *in alliance*), there is more rural land conversion (air pollution) than years when BLR (EPB) heads do not work in alliance with the incumbent provincial PS. When the bureau is headed by someone who is working in alliance with the provincial PS, the bureau is less independent from the provincial government, whose objectives may be more likely to influence the actions of the bureau than when the bureau is headed by someone who is not working in alliance with the provincial PS.

The idea that the independence of a policy-enforcement organization from politicians matters for policy outcomes is explored in the literature on how the independence of the central bank from the government affects monetary policy outcomes such as inflation. Eijffinger and De Haan (1996) review many studies that find a negative relationship between central bank independence and inflation. While most of the studies they discuss use cross-sectional analysis, Cukierman (1992) and Cukierman et al. (1992) use an instrumental variables approach and reach a similar conclusion regarding the effects of central

[11] Lieberthal et al. (1992), p.242.

bank independence. Fratianni et al. (1997) argue that as long as politicians hold power over appointment power over the central bank, they will use their political leverage to influence monetary policy.

In the context of the present study, I conjecture that the provincial PS can exert pressure on the provincial bureau leader in a way analogous to how a government may exert political pressure on the leadership of the central bank. Relative to the executive politician's political hold over the central bank leaders, the provincial PS is likely to be able to exert more personal pressure on the bureau head. This is likely to be the case because the appointment of the bureau head is not subject to the same review process and potential scrutiny from the public. Rather, the appointment decision is made in a highly opaque process by members of provincial Party committee, all of whom are subordinate to the provincial PS.

I study the dynamics between the provincial government and the BLR or EPB in a personalized framework by focusing on the provincial PS and the BLR or EPB head. In particular, I outline the personal relations between these two levels of bureaucrats in a simple framework of fairness and reciprocity.

The appointment of a bureaucrat to the post of bureau head generally constitutes a promotion. Thus, by appointing a bureaucrat to the post of bureau head, the provincial PS is carrying out an act of kindness toward the newly appointed bureau head. In turn, the bureau head may want to use his control over land policy to achieve outcomes more favorable to the PS who promoted him as a way of reciprocating the kindness of advancing the bureau head's career. At the same time, the bureau head may be jeopardizing his future career advancement by aligning his actions closer to the desires of the provincial PS and farther from the wishes of the central government.

On the flip side of positive reciprocity, the new bureau head may feel the need to align his actions with the desires of the provincial PS due to fear of retribution from the PS who appointed him. If the bureau head sticks strictly to central government guidelines while dismissing the wishes of the provincial government, then the *appointing* PS is more likely to be offended by the actions of the bureau head than the *inheriting* PS, because the former feels that his kindness (from having appointed the BLR head) is not reciprocated. As a result, the *appointing* PS is willing to punish the bureau head for being unkind.[12] Knowing this, the BLR head working *in alliance* with the provincial PS will take the PS's wishes more into account when choosing his actions.

Empirical research design

I investigate the effects of reciprocity in bureaucracy on land use policy in China by exploiting variation in the overlap between the terms of the provincial PS and the bureau heads. Since the provincial PS chairs the provincial Party Committee that appoints

[12]Here I assume that punishment is costly. For example, the PS may want to sabotage the future career of the BLR head through favors rendered by other personal connections. Such favors are most certainly costly. Although punishment may be equally costly to the *appointing* PS as to the *inheriting* PS, the *appointing* PS has a higher incentive for punishing an uncooperative BLR head.

provincial bureau-level officials, I categorize a PS as an *appointing* PS in a given province-year if in that province and that particular year the BLR (or EPB) head is someone who was appointed by a Party Committee chaired by the PS under consideration. Otherwise, a PS is categorized as an *inheriting* PS in a province-year if in that province and that particular year the BLR head is someone who was appointed by a Party Committee chaired by a previous PS in that province. I create an indicator variable *alliance* that is equal to 1 for a province-year if the PS is an *appointing* PS, and equal to 0 for a province-year if the PS is an *inheriting* PS. For each distinct pairs of provincial PS and BLR head whose terms overlap, I characterize the pair as *in alliance* if the pair of bureaucrats are working *in alliance* during the overlapping portion of their terms, and equal to 0 otherwise. See Figure 8 below for examples of the *alliance* variable.

[Figure 8 about here.]

The province-year units covered by the red arrows in Figure 8 are those that I classify as *in alliance*. I would like to know whether there is more rural land conversion in province-years covered by red arrows than in province-years not covered by red arrows.

Using the *alliance* variable, I estimate the following regression:

$$\Delta L_{it} = \beta_0 + \beta_1 alliance_{it} + \beta_2 post_{it} + \beta_3 post_{it} \times alliance_{it} + Prov_i + Year_t + X_{it}\Gamma + \epsilon_{it}, \quad (2.1)$$

where ΔL_{it} is the annual change in farmland in province i between year t and year $t-1$. I control for province-level fixed effects, year fixed effects, as well as personal characteristics of the provincial PS and the BLR head (X_{it}). I also control for whether the observation takes place before or after the passage of the 2004 amendment of the LAL that put a moratorium on net land conversion; $post_{it}$ is equal to 0 for years prior to 2004, and equal to 1 for 2004 and after.

The coefficient β_1 is the additional annual change in farmland when a provincial PS works with a BLR head that he appointed relative to when a provincial PS works with a BLR head appointed by a previous PS. My hypothesis predicts $\beta_1 < 0$. This tells us that in a year where there is an alliance between the PS and the BLR (as I have defined), the net gain in farmland from the beginning to the end of the year is smaller (or the net loss in farmland is larger) than in a year where there is no such alliance. The interaction of $post_{it}$ and $alliance_{it}$ is another coefficient of interest. If reciprocity matters more in the era of the moratorium, we expect the coefficient on the interaction (β_3) to be negative.

I estimate a similar regression with the level of air pollution as the dependent variable. The time variable in the air pollution data I use, aerosol optical depth (AOT), is recorded at the monthly level.

I also estimate the same regression for observations at the level of pairs of provincial PS and BLR head:

$$\overline{\Delta L_c} = \gamma_0 + \gamma_1 * alliance_c + \gamma_2 * post_c + \gamma_3 * alliance_c * post_c + X_c\Phi + \epsilon_c, \quad (2.2)$$

where $\overline{\Delta L_c}$ is the *average* of annual changes in farmland in the province over the years served by the pair c of the provincial PS and BLR head. I also control for whether the pair

c worked before or after the passage of the 2004 LAL amendment, which takes on value 1 for pairs that served the majority of their terms after August 2004, and 0 otherwise, and the interaction of their alliance and whether they worked after the 2004 LAL amendment. The coefficient γ_1 is the additional average annual change in farmland when a provincial PS works with a BLR head that he appointed relative to when a provincial PS works with a BLR head appointed by a previous PS. An estimate of $\beta_1 < 0$ tells us that for pairs of bureaucrats where there is an alliance between the PS and the BLR head (as I have defined), the net gain in farmland from the beginning to the end of the year is smaller (or the net loss in farmland is larger) than in a year where there is no such alliance. Finally, a negative γ_3 would imply that alliances between pairs becomes even more important for being able to convert land in the moratorium period.

Addressing potential omitted variable bias

The OLS estimation may not yield an unbiased estimate of β_1 because the *alliance* variable is potentially correlated with other unobserved variables that also affect rural land conversion or air pollution. Variation in *alliance* for a given province-year is driven by all the factors that determine (1) whether a PS gets to stay in office and (2) whether a PS has the chance to appoint a BLR head in a given year. Personnel management of the Chinese bureaucracy is not very well understood because of the secretive nature of operations of the Communist Party.[13] Although the number of years a bureaucrat serves in a particular post has only small variation, the length of the terms of bureaucrats are not strictly institutionalized (i.e. they do not serve terms of a fixed number of years). Below I explore the possible drivers of the variance in *alliance* and pin down potential causes of omitted variable bias.

Whether an incumbent PS gets to stay in office in a given year (as opposed to being retired, promoted, demoted or rotated to a different position in the same rank) might depend on:

1. the age of and the years already served by the PS
2. the factional affiliations of the PS to the central Party committee
3. the economic performance of the province during the years already served by the PS
4. provincial revenue collection during the years already served by the PS

The first and second factors have been shown to shape the career paths of bureaucrats (Huang (2002), Landry (2008) and Shih et al. (2012)). Contrary to Li and Zhou (2005)'s finding that the likelihood for promotion of a provincial bureaucrat increases with the economic performance of the province, the recent study by Shih et al. found that after controlling for bureaucrats' factional ties, the provincial economic performance no longer matters for promotion, while provincial revenue collection increases the likelihood of

[13] Lieberthal et al. (1992), p.34.

promotion for the years 1994-2002. Fortunately, these factors are observable and can be controlled for in the regression estimation.

Whether an incumbent PS has the chance to appoint a new BLR (or EPB) head might depend on:

1. the age of and the years already served by the incumbent BLR head
2. the political influence of the incumbent PS
3. the alignment of the PS and the BLR head's preferences for rural land conversion
4. the political influence of the incumbent and the new BLR head

Very little has been written about the career paths of provincial bureau-level bureaucrats compared to the career paths of provincial PSs, as the latter group is much more visible and politically more powerful. The first factor is observable and can be controlled for explicitly. The other factors listed above are not observable (or would be very difficult to obtain data for, as in the case political influence of provincial bureau-level officials). The political influence of the PS can be proxied to a limited extent with the PS's factional affiliations. However, I would worry that the unobserved degree of political influence is different between the PS's who appoint a BLR head and the PSs who do not, and that this unobserved element is correlated with unobserved characteristics that drive rural land conversion (e.g. the PS's appetite for urban development). Furthermore, if the unobservable traits of the PS's are correlated with land conversion, then the alignment of the PS and the BLR head's preferences for rural land conversion is also potentially worrisome.[14] If the PSs who get to appoint a new BLR head tend to appoint BLR heads with similar preferences as their own, then variation in *alliance* would be correlated with some unobservable characteristic of the appointing PSs and of the BLR heads they appoint that also drive rural land conversion (e.g. appetite for urban development).

To assess the potential omitted variable problem arising from unobservable PS and BLR preferences, I compare the observable traits of the PS's who appoint BLR heads and the traits of those who do not. Systematic differences in observable traits between the PS's who appoint and those who do not would suggest the existence of other unobservable factor traits between these two groups. I will also examine the within-PS variation in land conversion for those PS's who have served in multiple provinces. If these PS's have served with both BLR heads that they inherited and those that they appointed, then we have reason to believe that having the chance to appoint a new BLR head is not correlated with the preferences or political influence of the provincial PS.

Another source of confounding interpretation of the *alliance* variable comes from the fact that the PS and the BLR head tend to be in alliance during the latter portion of the PS's term. This feature of the data follows naturally from the PS's tendency to not appoint a new BLR head as soon as the PS comes into office. The OLS estimate β_1 may

[14]Note that so far I have not explicitly dealt the BLR head's personal preferences for rural land conversion by making the assumption that the BLR head would simply carry out the bidding of the central government in the absence of pressure from the provincial government.

be picking up the difference between rural land conversion near the end of the PS's term relative to near the start of his term, as a result of incentives that change during the course of the PS's term. I address this concern in two ways. Referring back to Figure 1, I test for the PSs in Case 2 who never appoint a BLR head, whether there is higher land conversion near the end of their terms. Also, I will also test whether PS's in Case 3 who appoint multiple BLR heads, whether the amount of land conversion at the end of the PS's term is different from the amount of land conversion at the start of the PS's term.[15] If both of these tests yield no significant result, then I can argue that the OLS estimate is not driven by an end-of-term effect.

2.5 Data

My research project calls for datasets on land use, air pollution, and on the career profiles of provincial PSs and land and environmental bureaucrats. I will also supplement my analysis with other province-level statistics. Below I describe the data that I collect in order to carry out the analysis as laid out in the previous section, as well as the data that I have collected up until now.

Land use

I collect province-level measures of total land area, farmland area, urban land area and newly rehabilitated farmland. I plan to gather both the official published statistics as well as measurements of the same variables with satellite images. Official measures of these land use variables can be found in various official publications, such as the *China Land Yearbooks*, which are published annually by the Ministry of Land and Resources since 1999. These measures are also reported in the annual *Province Yearbooks* submitted by all provinces.

The land use data that I have collected thus far are official government statistics that come from the *China Land Yearbooks*. I have collected variables measuring the quantity of farmland (measured in *mu*) in each province at the beginning and at the end of the year from the *China Land Yearbooks* for the years 1999 and 2002-2008.[16] From these data I can calculate the rate of change in the quantity of farmland in each province in the years for which I have data. This measure of the "change in farmland" is what I use in the descriptive statistics.

I can also collect and code Landsat TM/ETM satellite digital images data, some of which have been compiled by the China Academy of Sciences and used in works such as the Deng et al. series of papers. The existing compiled data are available for 1986, 1995 and 2000, with an upcoming wave of coded images from 2006. I plan to access the Landsat satellite images via the Global Land Cover Facility at the University of Maryland. The

[15] We might worry that PSs who appoint multiple BLR heads are systematically different from PSs who only appoint one BLR head.

[16] Each *mu* is 0.1647 acres.

satellite images are available from the mid-1980s on, and with higher frequency for the years after 2000.

One obvious advantage of using satellite data is that satellite images are much less prone to human manipulation compared to official data. Given diverging land conversion objectives of the central government and the local governments, we might expect the officially reported data on land conversion to be systematically biased downward. On the other hand, satellite data classification of land use could be less precise, and the level of precision depends on the weather conditions, especially cloud coverage, at the time the images were captured. By comparing my results from using the official statistics and from using satellite image data, I will be able to identify any systematic ways in which misreporting might occur in the official data.

Air pollution

I use satellite data on aerosol optical thickness (AOT) to measure the quality (or lack thereof) of air across provinces in China. AOT measures the degree to which aerosols prevent the transmission of light by absorption or scattering of light (Goddard Earth Sciences Data and Information Services Center). This measure is highly correlated with air quality in terms of particulate matter as measured by ground stations. AOT is recorded at monthly frequency from March 2000, for each 0.5 degree by 0.5 grid area of the eath. I use the monthly grid-level average of AOT for each province as the outcome variable. As with satellite land use data, AOT is relatively free from concerns of human manipulation compared to officially reported data.

Bureaucrat tenure

For each province-year, I collect the names of the provincial PS and the BLR head. I also collect information on the month of the year when each PS and BLR head comes into his post. The names of bureaucrats are collected from official government publications via the *Provincial Yearbooks* and the *China Land Yearbooks*. I verify this information and collect additional data on bureaucrats' terms via various sources, such as *www.chinavitae.com*, *The People's Daily*, and official websites of provincial governments and BLR's.

In province-years where a new PS comes into office while there is not a new BLR, the two bureaucrats are not in alliance ($alliance_c = 0$). In province-years where a new BLR head comes into office while there is not a new PS, the pair of bureaucrats are in alliance ($alliance_c = 1$). For province-years where both the provincial PS and the BLR head stay in office for the entire year, I categorize the pair of bureaucrats (PS and BLR head) as in alliance ($alliance_c = 1$) if the PS is the person who chaired the provincial Party Committee in the year that the BLR head was appointed to his post. Conversely, for province-years where no new bureaucrat comes into either post, I categorize the pair of bureaucrats as not in alliance ($alliance_c = 0$) if the BLR head came into office before the PS did. For province-years where there were both a new PS and a new BLR head, I look to the months in which the bureaucrats came into office to determine which bureaucrat came into his post first and whether the pair of new bureaucrats are in alliance.

Other datasets

Aside from the two main sets of data on land use and bureaucrat terms, I am collecting the following datasets:

- Detailed career and biographical data on the provincial PS's as well as the BLR heads. For instance, I want to control for bureaucrats' age in order to take into account actions that arise out of bureaucrats being close to the retirement age.
- Province-level fiscal budget data. Rural land conversion is closely tied to the need to raise extra-budgetary revenue, and the fiscal status of a province in a given time period could be correlated with whether the provincial PS works in alliance with the BLR head.

In total, I will collect data for 31 provincial units for the years 1987-2009, which amount to 713 province-year observations. For the preliminary analysis below, I have 127 province-year observations in the data that I have collected. Within these 127 observations, I identify 57 distinct pairs of provincial PS and BLR head who have worked concurrently. Thus, I expect to find 300 or so pairs of provincial PS and BLR head in the universe of data that will eventually be collected.

2.6 Descriptive statistics and preliminary findings

In this section I present some preliminary findings using the data that I have collected so far. These findings are meant to motivate my hypothesis as well as guide future data collection and analysis.

Descriptive statistics – land conversion

In the official Ministry of Land and Resources data that I have collected on the quantity of farmland by province, there is an average per-province annual loss in farmland of 22,654 hectares for the years 2002-2007 ($n = 186$). Table 10 below presents the breakdown of farmland lost by year and by province.

[Table 10 about here.]

We can see that average loss in farmland has been decreasing every year since 2002. The drop in the annual loss in farmland from 2003 to 2004 may have been a result of local land bureaus reacting to more restrictions on land conversion from the central government even before any official changes in the Land Administration Law. By 2007, the net change in farmland has become less than 1 percent of the net change in farmland in 2002. However, another thing to note is that there was not strict adherence to the moratorium on net rural land conversion, seeing as the change of farmland was still negative for the years after 2004.

The breakdown by province shows that Beijing and Shanghai have experienced the largest loss in farmland over 2002–2007. This is not surprising given the commercial significance of the two cities. All other provincial units had a net loss in farmland over 2002–2007except for Heilongjiang and Xinjiang, which had a net gain of 0.89 percent and 0.21 percent, respectively.

Summary statistics on the provincial PS and BLR officials are presented below in Table 11. Table 11 contains all the data on bureaucrats that I have collected thus far from the *China Land Yearbooks* and the *Province Yearbooks*. I am missing bureaucrat names for some municipalities and autonomous regions for the years 2002-2008.[17]

[Table 11 about here.]

There are 47 distinct PSs and 51 distinct BLR heads in the data that I have collected. 11 of the PSs served as the provincial PS in two provinces in my data, creating 58 distinct PS *terms*. There are 77 distinct pairs of PS and BLR head who have worked concurrently. An alliance exists between the provincial PS and the BLR head in 77 of the 151 province-year observations.

Out of the 151 province-year observations, 25 had an appointment of a new BLR head. Disregarding 2002, for which I have only 8 observations, 2003 had 2 appointments, 2004 had 1 appointment, 2005 had 4 appointments, 2006 had 4 appointments, 2007 had 2 appointments and 2008 had 12 appointments. It seems that the revision of the Land Administration Law in 2004 had increased the likelihood that a PS appoints a new BLR head in any year. I will need to collect more bureaucracy data before 2003 to confirm this trend. However, if this finding proves to be robust, then it would suggest that the PS cares more about having a BLR head who is in alliance with them after 2004. While this finding complicates the interpretation of *alliance*, it is however consistent with my hypothesis that having a subordinate who is in alliance lead the BLR benefits the PS, especially when the central government's goals diverge more from those of the provincial PS.

Of the PSs and BLR heads for whom I observe either the complete term or the complete overlap of their terms, I calculate the length of each bureaucrat's term as well as the number of years of overlap for each pair of PS and BLR head with overlapping terms. I find the average PS tenure to be 2.72 years, while the average BLR head tenure to be 3.22 years.

Of the 58 distinct PS terms, 14 belong to Case 1, 27 belong to Case 2, 0 belongs to Case 3 and 17 belong to Case 4 in Figure 8. That is, in the data that I have collected so far, there is no provincial PS who has appointed more than 1 BLR head during his term.

Table 12 presents summary statistics of the annual change in farmland by whether the PS and the BLR head are characterized as working *in alliance*. There is some evidence in this simple presentation that the distribution of annual change in farmland tends to be more negative (representing more loss) in years where the PS worked with the bureaucrat that he appointed to office.

[17] In my regression analysis I use only the subset of the bureaucrat data from 2002-2007, since I do not yet have land data for 2008.

[Table 12 about here.]

Table 13 breaks down *alliance* and average loss in farmland by the cases of PS term. Overall, Case 4 ("always in alliance") PSs tended to oversee more land conversion than Case 1 ("sometimes in alliance") PSs. Both Case 4 and Case 1 PSs tended to over see more land conversion than Case 2 ("never in alliance") PSs did. This is consistent with the idea that political alliance through appointment can affect land conversion .

[Table 13 about here.]

Descriptive statistics – air pollution

Table 14 presents the average AOT for each province in China over the period March 2000 to November 2013. There is a large range of values across provinces. Not unexpectedly, densely populated cities such as Shanghai and Tianjin have particularly high levels of AOT, while relatively sparse and remote areas such as Qinghai province and Inner Mongolia have significantly lower levels of AOT. Table 15 presents the summary statistics on the tenures of provincial PSs and EPB heads. Note that the data on EPB, corresponding to the data on air pollution, is reported at the monthly level.

[Table 14 about here.]

[Table 15 about here.]

Preliminary regression estimates

Table 16 presents the preliminary regression results with the levels of annual change in farmland as the dependent variable and alliance as the main explanatory variable.

[Table 16 about here.]

The estimates of the coefficient on *alliance*, though not statistically significant in the current preliminary sample with few observations, are consistent with the theory of reciprocity. In the basic OLS specification, Column (1), where *alliance* is the only control variable, the province-years where the provincial PS and the BLR head were in alliance lost 12478 hectares more farmland than the observations where the bureaucrats were not in alliance. In Column (2) I add to the specification the *post* variable for post-2004, as well as the interaction of *post* and *alliance*. The coefficient on *post* indicates that the net annual change in farmland post-2004 is higher than the net annual change in farmland pre-2004, by 33,901 hectares per province per year. This is in line with the moratorium on land conversion having its desired effect; land conversion in the post-2004 era is abated relative to pre-2004 conversion rates as indicated by the constant (-37,287 hectares per province per year).[18] While the coefficient on the interaction term between *post* and

[18]See Table 1 for average conversion rates by year

alliance is negative, it is not precisely estimated, as is the coefficient on *alliance*. The negative estimated coefficient on the interaction term indicates that, in the post-2004 era when land conversion is heavily restricted, alliances between the provincial PS and the BLR head are even more important for converting farmland than pre-2004. In Column (3), I examine the cases of PS tenure. The results show that in province-years with Case 4 PSs who are always in alliance with the BLR head, there are 25,734 more hectares of farmland lost compared to province-years with Case 1 PSs who are only sometimes in alliance with the BLR head.

I run similar regressions with the pair of bureaucrats as the unit of observation. The dependent variable is now the average annual change in farmland over the period of PS and BLR head term overlap. The results are presented in Table 17, and qualitatively mirror the results in Table 16. The additional data that I plan to collect will increase precision of these estimates while also allowing for more controls and tests of heterogeneity by bureaucrat and pair characteristics. Also, the satellite land use data should address concerns of systematic misreporting in the official statistics.

[Table 17 about here.]

In Table 18, I present preliminary results of the impact of bureaucrat alliance on the level of AOT in provinces from 2000 to 2013. The main result here is that the appointment of EPB heads to office by the PS does not affect the level of AOT in the province. This could be due to the fact that the connection between pollution and GDP growth is a somewhat weak, as pollution is only a byproduct of the process of industrialization and not a direct input.

[Table 18 about here.]

2.7 Discussion

More work needs to be done to collect a more complete panel data set to carry out the analysis related to the impact of bureaucratic alliances on rural land conversion and air pollution. Having detailed information on politicians and bureaucrats' careers would help analyze whether the impact of bureaucratic alliance varies by career incentives.

Chapter 3

Cities leading counties and the process of urbanization in China

3.1 Introduction

In this project, I explore the consequences of some major organizational changes in the Chinese political system that gave political preference to urban centers with the goal of facilitating decentralized industrialization. My goal in studying this policy is to better understand the process of urbanization in developing countries, the role of economies of scale in local governance, and the importance of bureaucrat incentives in shaping local economic outcomes.

The Chinese central government first expressed its wish to extend the geographic boundaries of existing urban centers in 1982, in a report given by then Premier Zhao Ziyang on the sixth Five Year Plan (1981-1985). In 1983, China began to enact organizational changes as part of a key new policy called *Cities Leading Counties* (CLC). This policy abolished prefectures, and placed counties that used to be make up these prefectures under the administrative control of nearby urban centers. The larger merged regions then became prefecture-level cities.[1] While prefecture governments are essentially delegates of the provincial government and do not interfere with county-level policies independently of the province, prefecture-level cities have concrete power over counties within their boundaries and many more resources with which to exercise that power.

[Figure 9 about here.]

Since the early 1980s, these organizational changes have affected the vast majority of China's counties. The new administrative system contrasted starkly with the old system, where "cities and counties were separated by various administrative hurdles that prevented them from interacting directly." (Ma (2005)) While CLC was a national policy, the decisions of when and where to implement the policy were made by provincial governments. While *cities* with more densely populated urban centers tended to receive

[1] Prefecture is the administrative level immediately below the province and above the county. See Figure 9 for an illustration of the 3 levels of government from province, down to counties.

precedence in subsuming nearby counties, the characteristics of *counties* were not part of the stated selection criteria. Overall, the policy of "cities leading counties" was likely to have been implemented earlier in regions that had had a more clearly defined urban center. However, there was substantial variation in when the policy was implemented even across regions with similar baseline levels of urbanization.

3.2 Guiding Theories

Cities Leading Counties affected treated counties in a number of ways. I list some potentially relevant theories below and use them to guide my data collection and empirical work.

1. **Cities internalize positive externalities from long-term investments**
   Cities that have gained subordinate territories and population may be more inclined to make infrastructural investments since the larger administrative unit is able to internalize the positive externalities derived from large-scale projects.

2. **Skill/resource complementarity**
   Are county-city mergers more likely to happen when the two exhibit production complementarity? For instance, perhaps the city has knowledge and capital, while the county has land and unskilled labor? If this type of mergers are not more likely, are they more successful? Or, post merger, the industrial composition of the county might become more geared toward industries that complement those in the city.

3. **Lower bureaucratic costs without city border**
   It may be cheaper (in terms of bureaucratic barriers and land prices) to build factories and offices in a subordinate county relative to a non-subordinate that is at the same distance. Therefore we may see more urban development in subsumed counties that are on or close to the prefecture city border.

4. **Political incentives lead to extraction from counties**
   A county-city merger could represent a shift of the county's political center of gravity toward the city, while the county loses bargaining power. If it is the case that selling land is a good way to generate revenue, and that is something that city officials care about, then a city-county merger may lead to more land sales in the subsumed counties. Of course, county officials may benefit from this as well.

5. **Political incentives lead to improvements in counties**
   On the other hand, if city officials care about the outcomes of their new citizens, then we may see the education and health services gap between the county and the city narrow after they merge.

3.3 Empirical strategy

I plan to examine the impact of the CLC policy on urbanization and employment in the affected counties. The roll-out of CLC was staggered over time and space. This set-up lends itself to an event study analysis. To estimate the average treatment effect of a county being subjected to CLC in a particular year, I run the regression in (3.1). This is the basic event study model, à la Jacobson et al. (1993). I include province-year fixed effects here, as in Wang (2013).

$$Y_{cpt} = \alpha_c + \delta_{pt} + \sum_{k=\underline{T},k\neq -1}^{\overline{T}} \beta_k D_{cpt}^k + \epsilon_{cpt} \tag{3.1}$$

Y_{cpt} is any one of the outcome variables (e.g. population density, share of employment in industry) for county c in year t. α_c are county fixed effects. δ_{pt} are province-year fixed effects, which are meant to control for province-level aggregate policies that vary with time. Let e_cpt be the date at which county c in province p was subsumed into a prefecture-level city. Then define $D_{cpt}^k = I[t = e_i t + k]$, so D_{cpt}^k is a dummy variable for county c having been subsumed k periods ago. [2] I define the range of k to be between $\underline{T} < 0$, and $\overline{T} > 0$, where these limits may vary depending on the availability over time of the dataset I use.

For most years, the yearly treatment effects from the baseline estimation are estimated from all the treated counties regardless of the date of treatment. We might think that treatment effects for counties subjected to CLC in the early years may differ from the treatment effects for counties subjected to CLC in the later years. To capture heterogeneous treatment effects over time, I define several intervals of time where CLC took place in China, and estimate separate treatment effects for each of these intervals, as in equation (3.2).

$$Y_{cpt} = \alpha_c + \delta_{pt} + \sum_{g=1}^{\overline{G}} \sum_{k=\underline{T},k\neq -1}^{\overline{T}} \beta_k^g D_{cpt}^k g_c + \epsilon_{cpt} \tag{3.2}$$

I define $\overline{G}$ waves of treatment, and g_c is a dummy variable indicating that county c was subjected to the CLC as part of wave g.

The pre-treatment trend estimates from both (3.1) and (3.2) reveal whether the treatment and control counties were similar prior to treatment.

3.4 Data

Administrative Data

First, I collect data on the exact timing of administrative changes from 1982 to 2005. For 1982 to 1994, these data come from the GuoBiao (GB) Codes for the Administrative

[2] Treatment is an absorbing state.

Divisions of the Peoples Republic of China, hosted at the Socioeconomic Data and Applications Center at Columbia University. For 1995 to 2005, the administrative changes are collected from the official *China Administrative Divisions Handbook*. While I have cleaned up the former dataset covering 1982 to 1994 for this write-up, I am still working on putting together the 1995 to 2005 portion of the dataset.

I obtain county boundary shapefiles and county-level Census data for the years 1982, 1990, and 2000 from the *Historical China County Population Census Data with GIS Maps (1953-2000)*, a database maintained by the China Data Center at the University of Michigan. I match counties in these Census shapefiles to counties in the administrative changes data in order to construct the spatial relationships between counties and cities.

I am also in the process of collecting yearly county- and city-level data on population, education, employment by sector (agriculture and industry), output by sector, government revenue, government expenditure, and public goods provision in terms of road lengths, and the number of schools and hospitals. These variables will be assembled from various sources – *Province Statistical Yearbooks*, *Prefectural and County Financial Statistics*, *City Yearbooks*, and *County General Statistics*.

Nighttime Lights Data

One drawback of administrative data is that they do not report spatial variation within an administrative unit. Thus, they do not allow me to test spatial hypotheses of urban settlement. To remedy this, I incorporate nighttime lights data in my analysis. The United States Air Force Defense Meteorological Satellite Program (DMSP) maintains a digital archive of annual averages of nighttime lights data beginning in 1992. These are processed satellite data that are meant to represent stable man-made lights. Each yearly raster reports light intensity for all the 30 arc second grids within the area between -180 and 180 degrees longitude, and -65 and 75 degrees latitude.

3.5 Results

Summary statistics

Using the cleaned administrative change data from 1982 to 1994, I illustrate the county-level roll-out of "Cities Leading Counties" in Figure 10. In this period, CLC was implemented gradually, but not uniformly across time. Notably, there was a large spike in treatment in 1983, and relatively little treatment between 1988 and 1992. This temporal variation represents both the attitude of the central government, who, for instance wanted to slow down CLC after 1984, and provinces' individual decisions regarding the implementation of CLC. I plan to use the completed (with data from 1995 - 2005) version of this figure to determine the different waves of treatment.

[Figure 10 about here.]

Table 19 shows the number of counties in each province in each year that were subjected to CLC. Provinces did not follow the same timeline of take-up. For example, note that while Anhui province had fairly spaced out take-up with 5 years where such changes took place, Jiangsu province adopted CLC in 1983 only.

[Table 19 about here.]

The maps in Figures 11 and 12 show the geographic extent of CLC in 1982 and 1994. With the exception of the autonomous regions in the North and West, the CLC was widely implemented across all provinces.

[Figure 11 about here.]

[Figure 12 about here.]

Table 20 reports summary statistics of some variables from the 1982 Census for 2 groups of counties – those that had been subsumed by a prefecture-level city by 1994, and those have had not. As expected, these two groups look very different in the baseline already. I also plan to compare 2 waves a time. It is likely that consecutive waves look more similar in the pre-treatment period. This table motivates the decision to include only ever-treated counties in the analysis below, since the never-treated counties are very likely to experience different underlying trends in urbanization.

[Table 20 about here.]

Figure 13 shows the roll-out of CLC in Anhui province. The shaded regions are the prefecture-level cities, and the different colors correspond to year of treatment. The dotted lines indicate connections between counties and the prefecture-level cities that have subsumed them. This map shows that cities in Anhui did not subsume only the adjacent counties. Thus, counties that are very closely located often received treatment at different times. I plan to use the nighttime lights data to study differential development in the intensity of light between nearby treated and control counties in the part of the counties that border the prefecture-level city.

[Figure 13 about here.]

Results

This section presents results from event study analysis using nighttime lights data from 1992 to 2009. The results are shown either for the long window of -4 to 6 years around the year of CLC treatment, or the short window of -3 to 4 years around the year of CLC treatment. The longer window will be estimated on a smaller sample of counties or cities – 1996–2003 treated instead of 1995–2005 treated as for the shorter window.

Figure 14 shows the impact of the CLC treatment on the group of counties treated between 1996 and 2003 ($N = 401$) in a 10-year window, and Figure 15 shows the impact

on the group of counties treated between 1995 and 2005 ($N = 432$) in a 7-year window. The blue line in both figures indicate the coefficient estimates on the event-time variables, and the dashed lines represent the 95% confidence intervals. Both figures show that subsumed counties seemed to have increased urbanization as manifested through higher levels of nighttime light intensity. On average, the impact of CLC treatment was 1% per year, and the statistically different from zero. Moreover, treatment doesn't seem to have been preceeded by statistically significant positive trend, which supports the interpretation of the post-event estimates as the causal effects of the CLC treatment on light intensity.

[Figure 14 about here.]

[Figure 15 about here.]

I investigate whether the impact of CLC varies across counties according to the counties' pre-treatment level of industrialization. One might imagine that counties that were *ex ante* more industrialized would be more likely to be targeted by their leading counties due to (for instance) having better infrastructure. Figure fig:hetbyman shows the estimated event-time coefficients for counties that had below or above 10% of employment in manufacturing by the time they were under CLC treatment. It seems that counties with higher *ex ante* manufacturing employment may have experienced a higher urbanization impact after the CLC treatment.

[Figure 16 about here.]

On the other hand, there is mixed evidence regarding whether cities that subsumed nearby counties saw increased urbanization rates after they were promoted to the prefecture level in the administrative hierarchy. Figures and show that, while cities that subsumed counties did not have above-trend urbanization prior to CLC, they also may not have had higher increased urbanization after CLC. However, there are relatively few cities that were affected by CLC, only 54 in the long window sample and 56 in the short window sample.

[Figure 17 about here.]

[Figure 18 about here.]

3.6 Discussion

So far this paper has found evidence of increased urbanization levels in counties that were subsumed into cities as part of the *Cities Leading Counties* policy. Next, I will further explore the heterogeneous treatment effect across counties in terms of outcome variables other than nighttime light intensity. The light intensity data can also be used in a more disaggregated way to study the location new urban developments, for instance whether the increased urban development in subsumed counties is near the leading cities.

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Figures

Figure 1: Anqing city (prefecture-level) in Anhui province

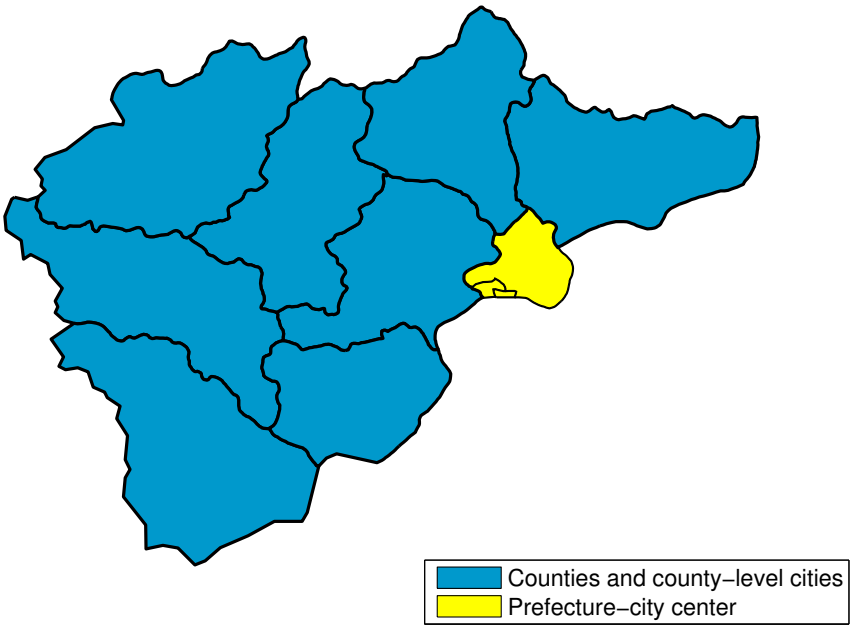
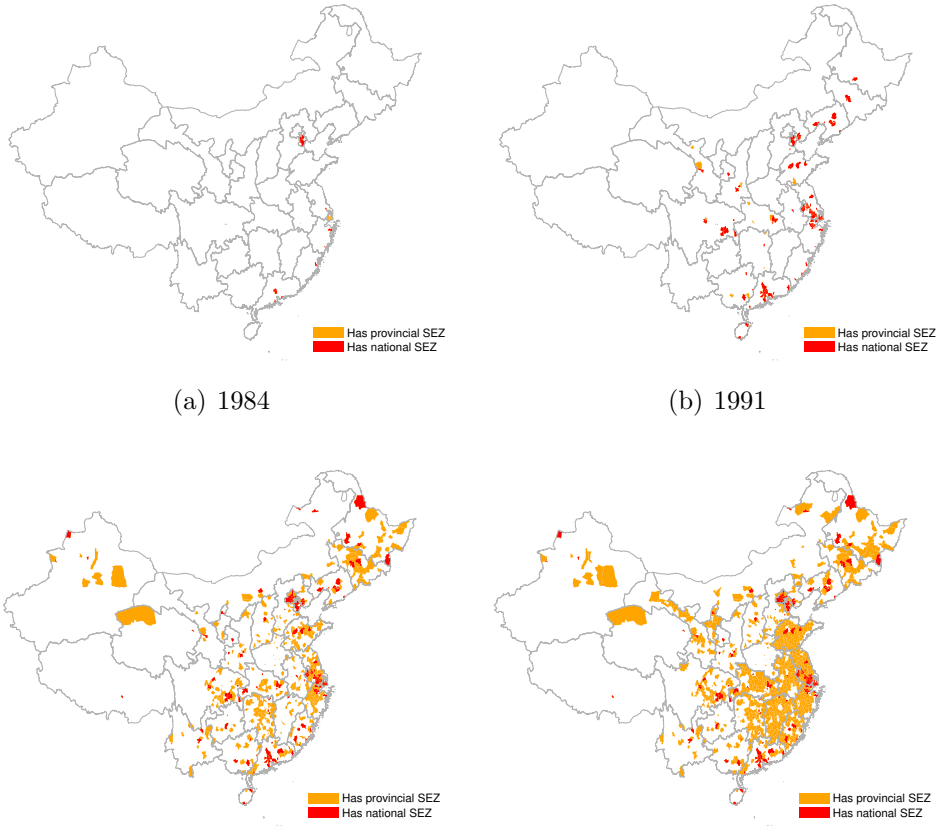


Figure 2: Expansion of the SEZ program at the county level



(a) 1984 (b) 1991

(c) 2003 (d) 2006

Figure 3: Tongcheng city (county-level) in Anqing province, Anhui province

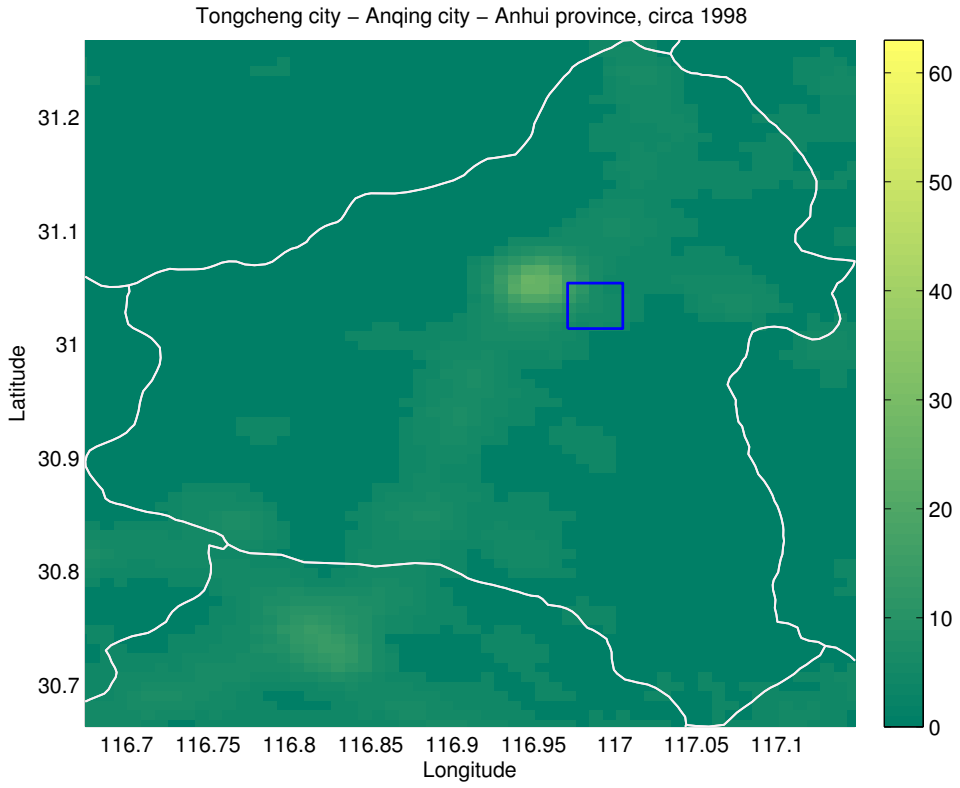
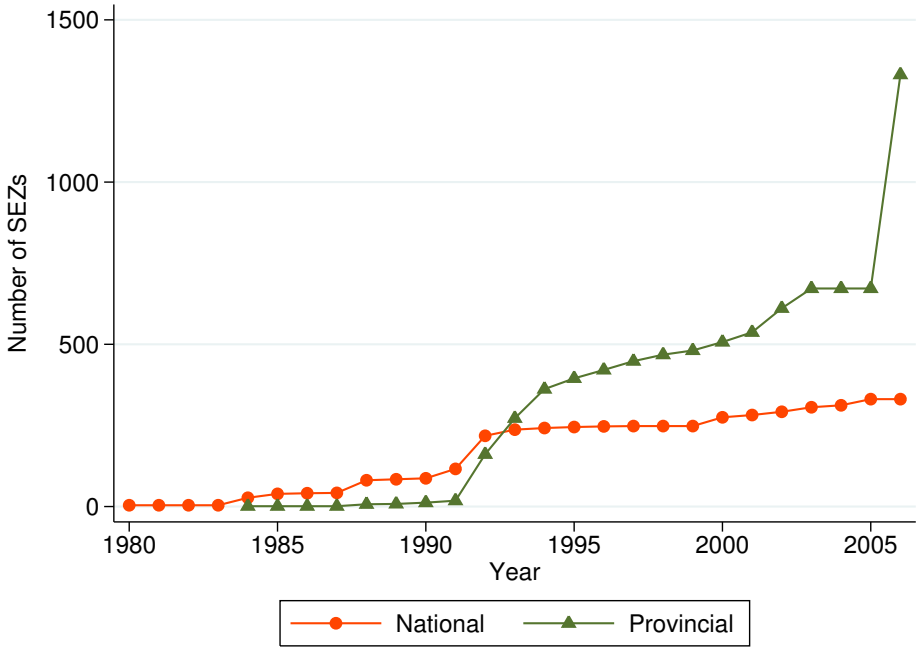
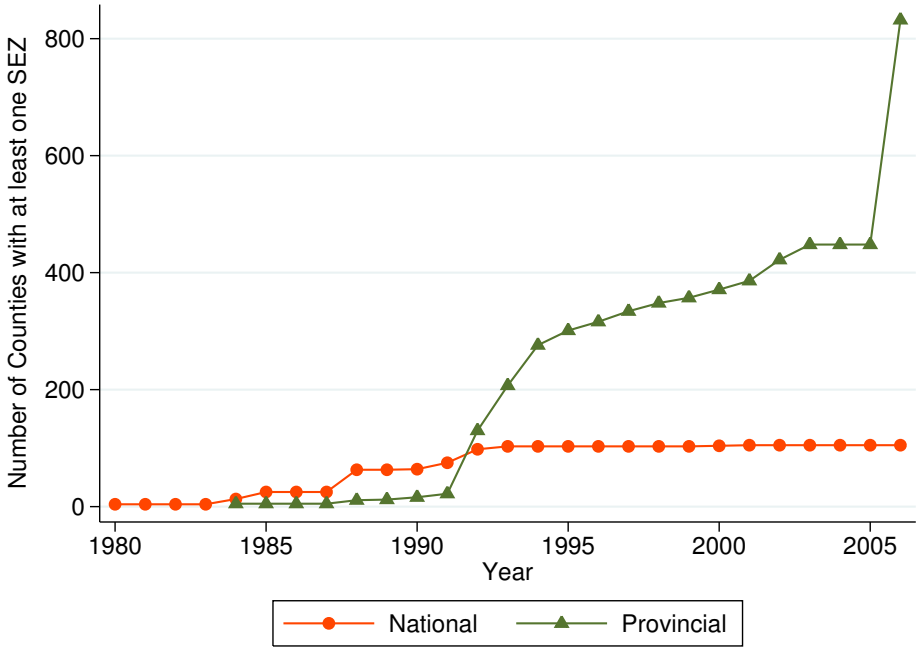


Figure 4: SEZs by national or provincial status



(a) Number of SEZs, by status



(b) Number of counties with at least one SEZ, by status

Figure 5: Impact of SEZ on county-level log(GDP), 2000–2006

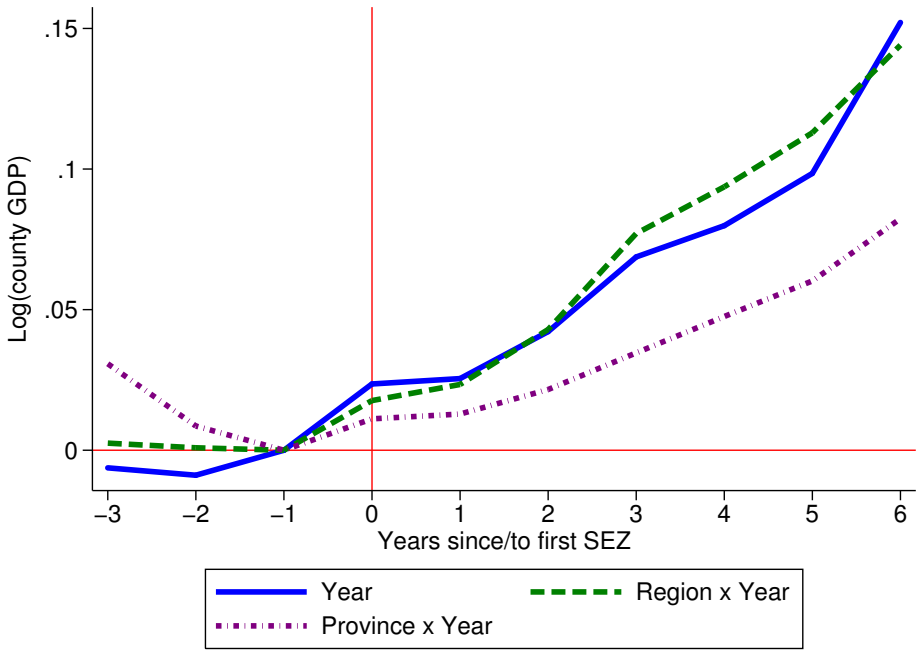
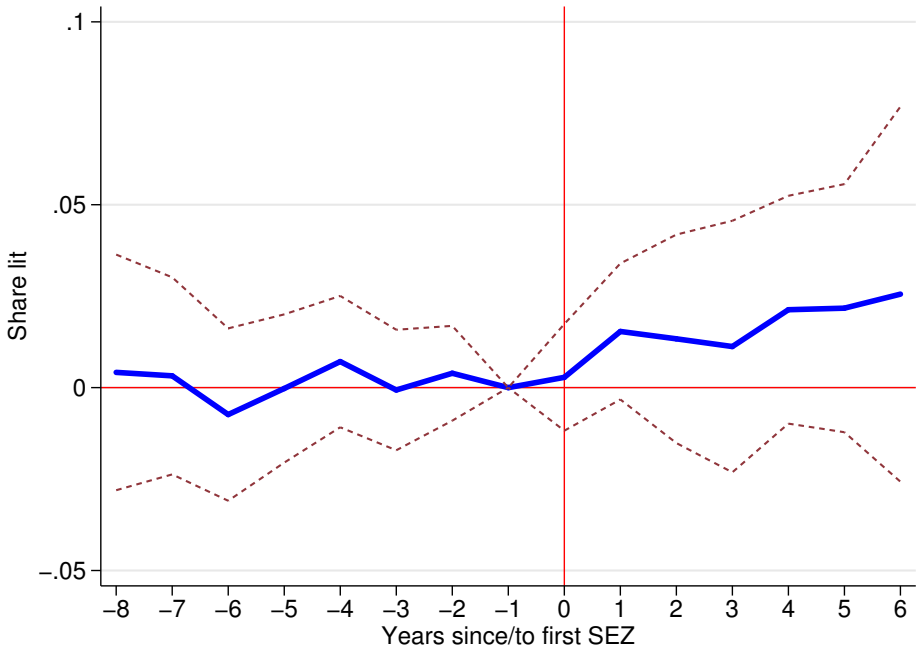
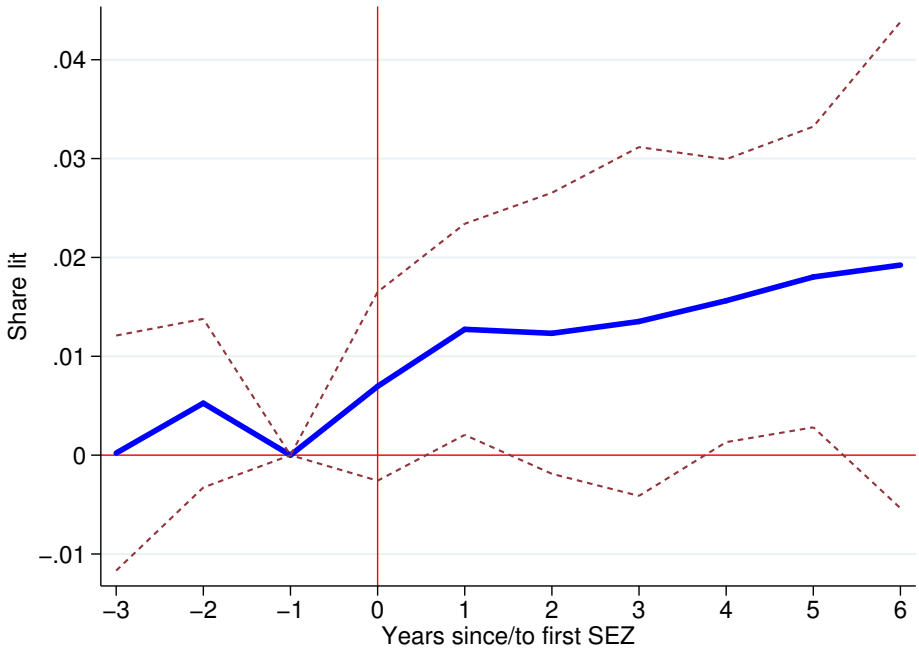


Figure 6: Local impacts of SEZs as measured by nighttime lights



(a) Long window, sample = 2000–2006 treated counties



(b) Short window sample = 1995–2006 treated counties

Figure 7: Bureaucracy diagram

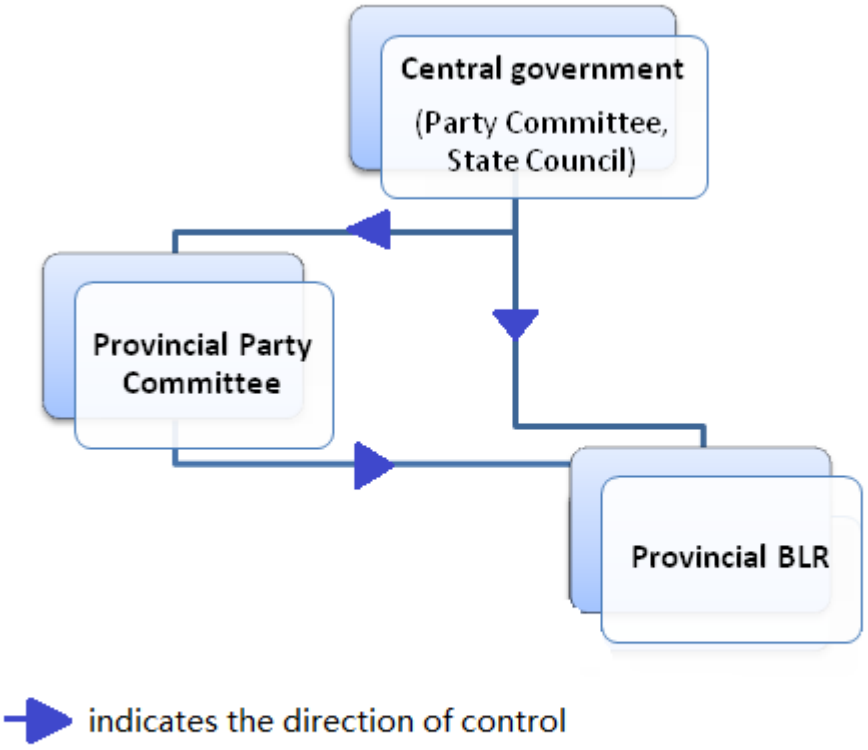


Figure 8: Four cases of overlap in bureaucrats' terms

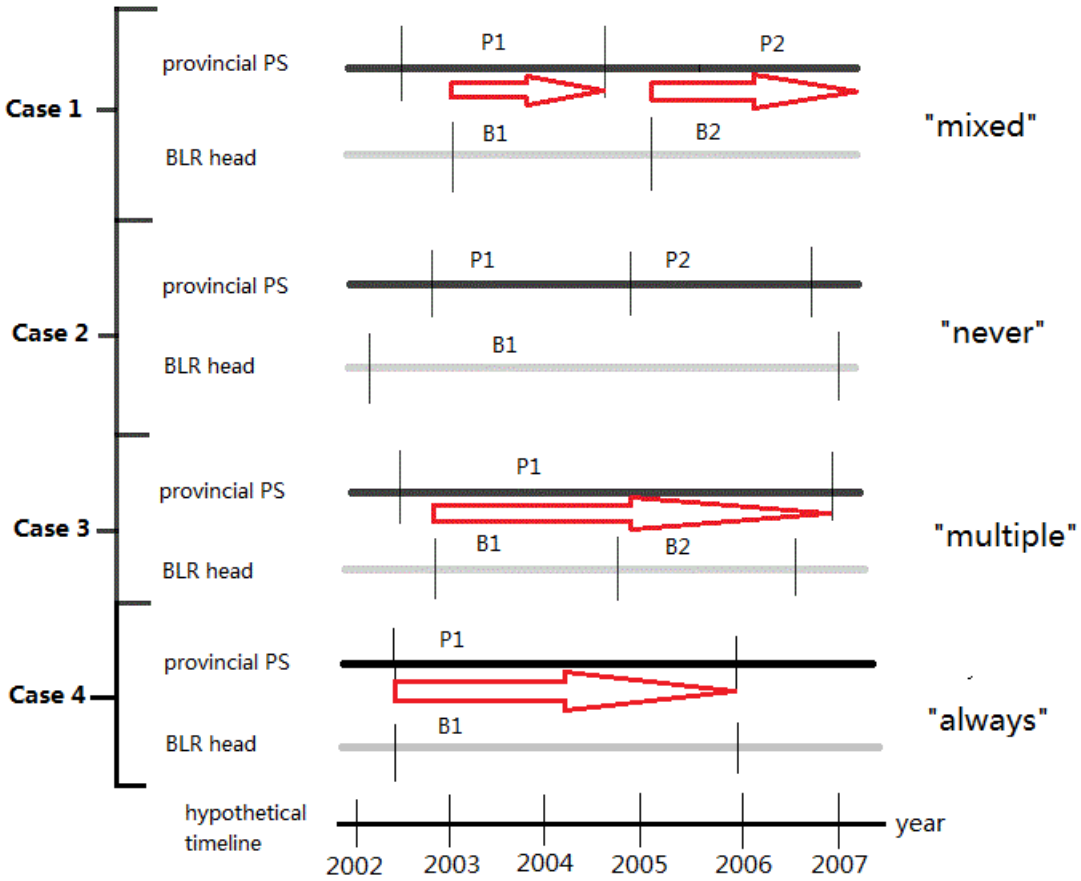


Figure 2 - 4 cases of terms overlap between the provincial PS and the BLR head

indicate the years in which the incumbent PS is the one who appointed the BLR head.

Case 1 - P1 (or P2) has worked with a BLR head that he appointed as well as with a BLR head that he inherited from a previous provincial PS

Case 2 - P1 (or P2) has worked only with a BLR head that he inherited from a previous provincial PS

Case 3 - P1 has worked with 2 BLR heads he appointed and with one he inherited from a previous provincial PS

Case 4 - *Perfect* overlap between the terms of the provincial PS and the BLR head

Figure 9: Three middle levels of government

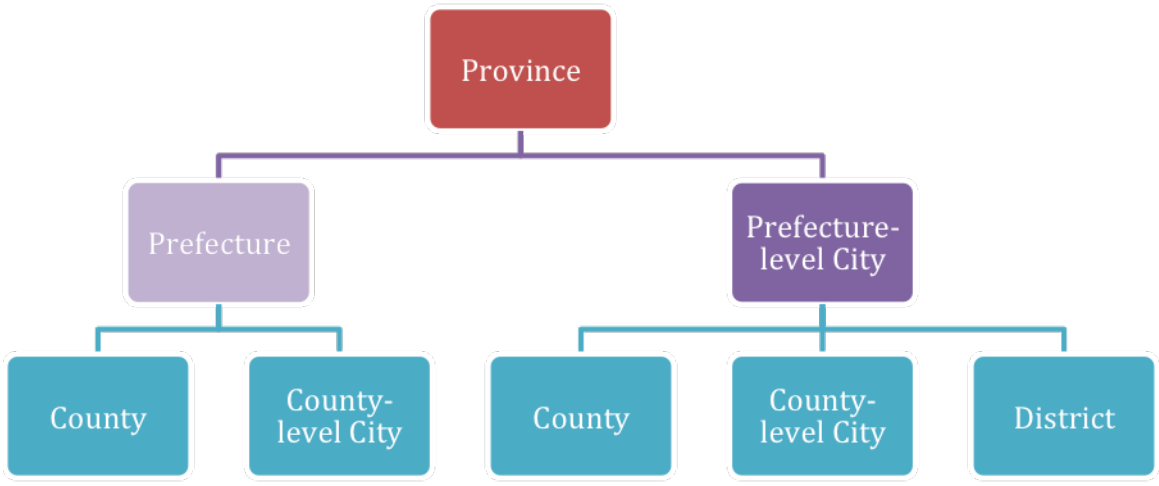


Figure 10: Roll-out of CLC

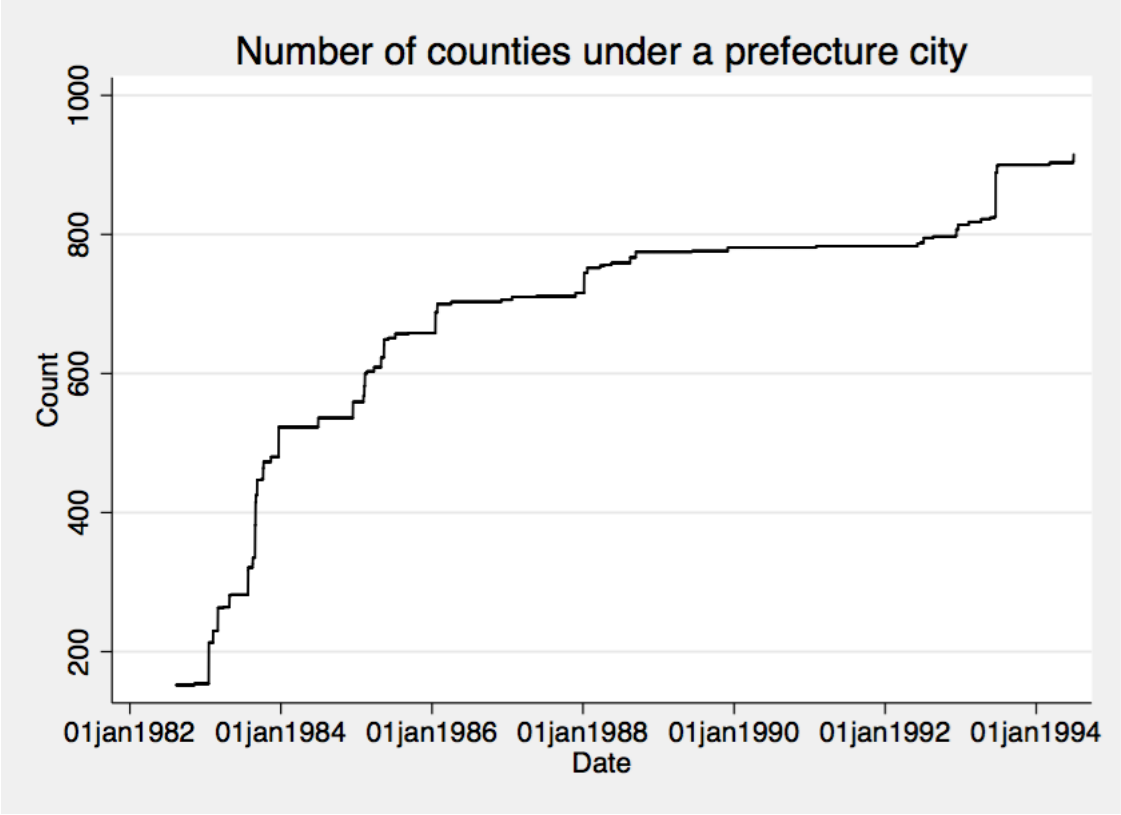


Figure 11: Extent of CLC in 1982

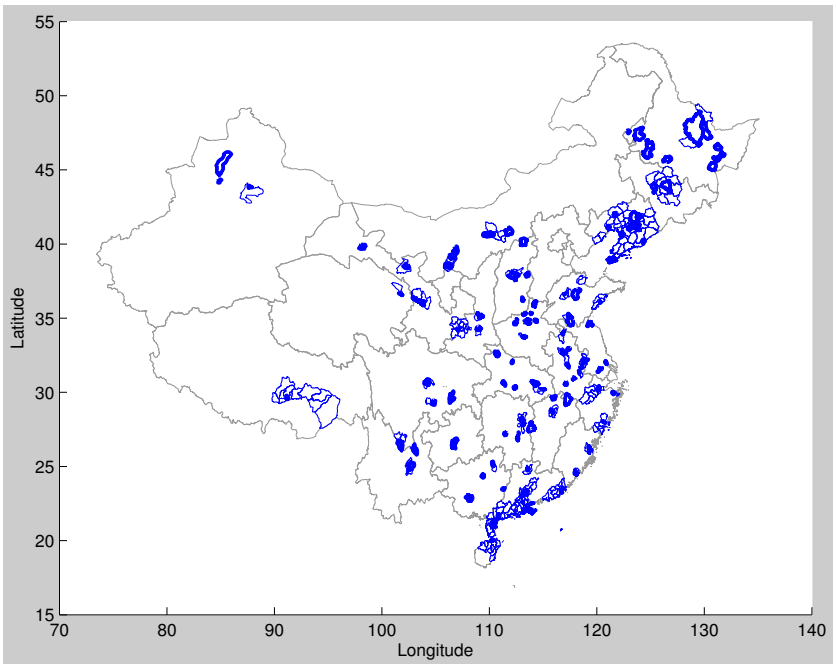


Figure 12: Extent of CLC in 1994

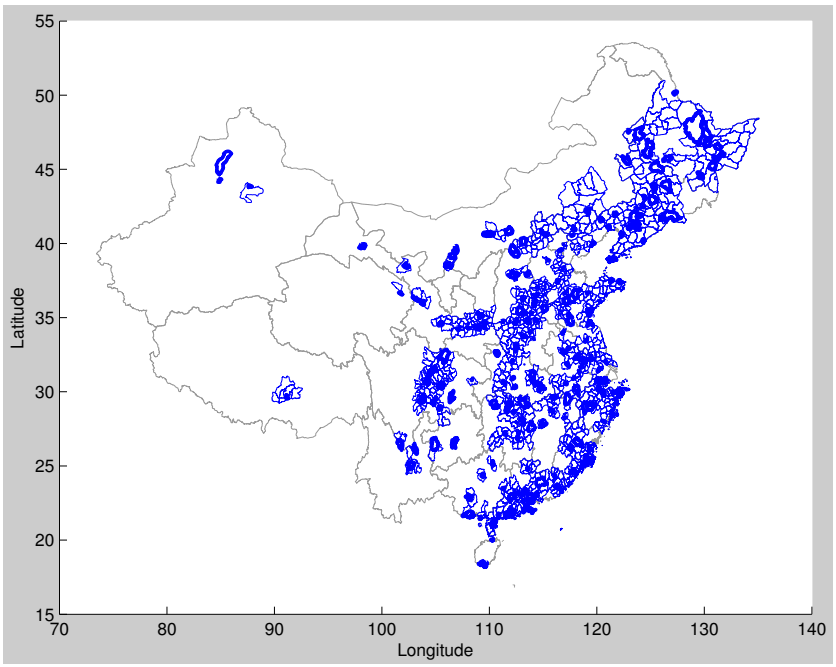


Figure 13: Roll-out of CLC from 1982 to 1994 in Anhui province

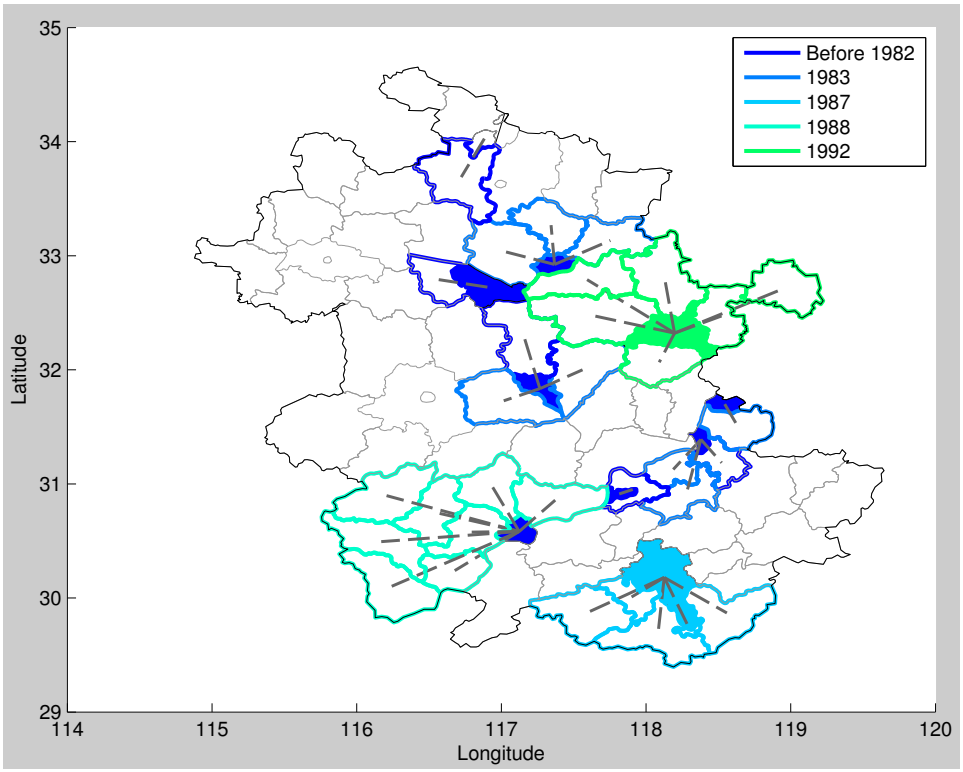


Figure 14: CLC treatment on log(light intensity) of counties, long window

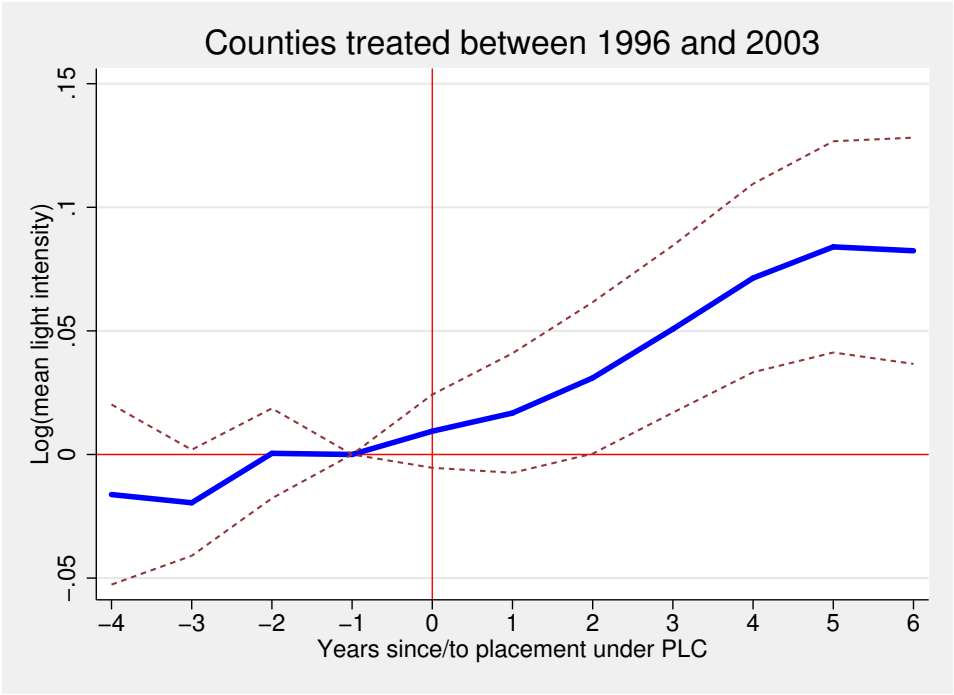


Figure 15: CLC treatment on log(light intensity) of counties, short window

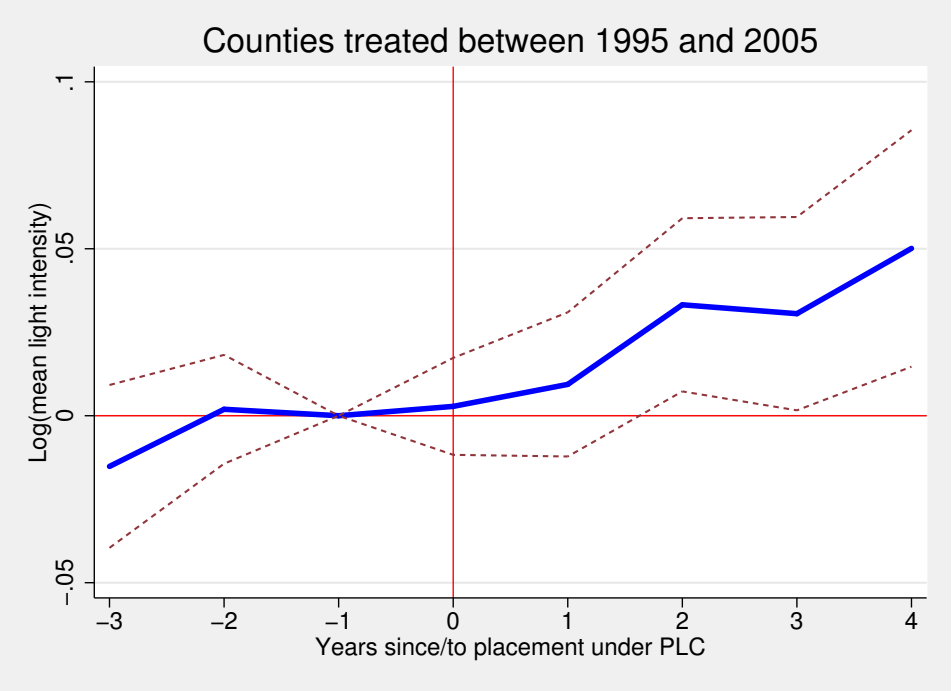


Figure 16: Heterogeneous treatment effects on counties by pre-treatment manufacturing employment

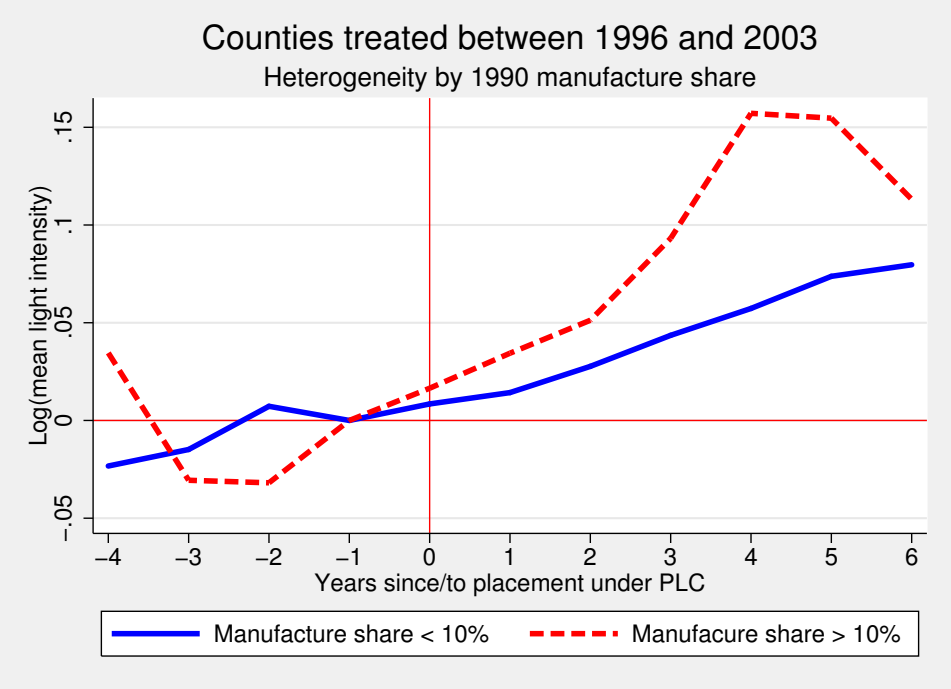


Figure 17: CLC treatment on log(light intensity) of cities, long window

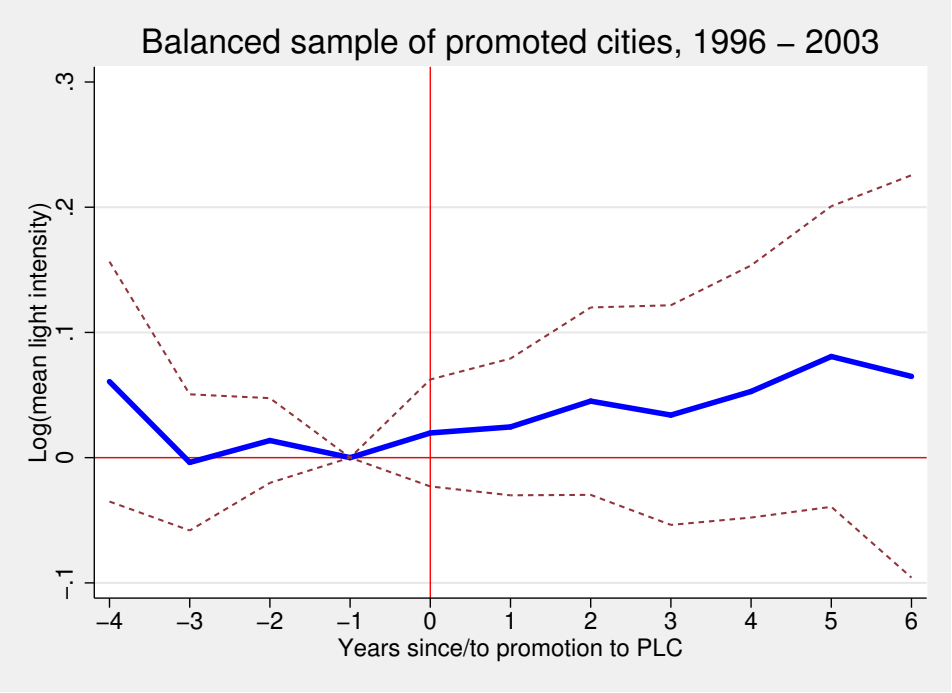
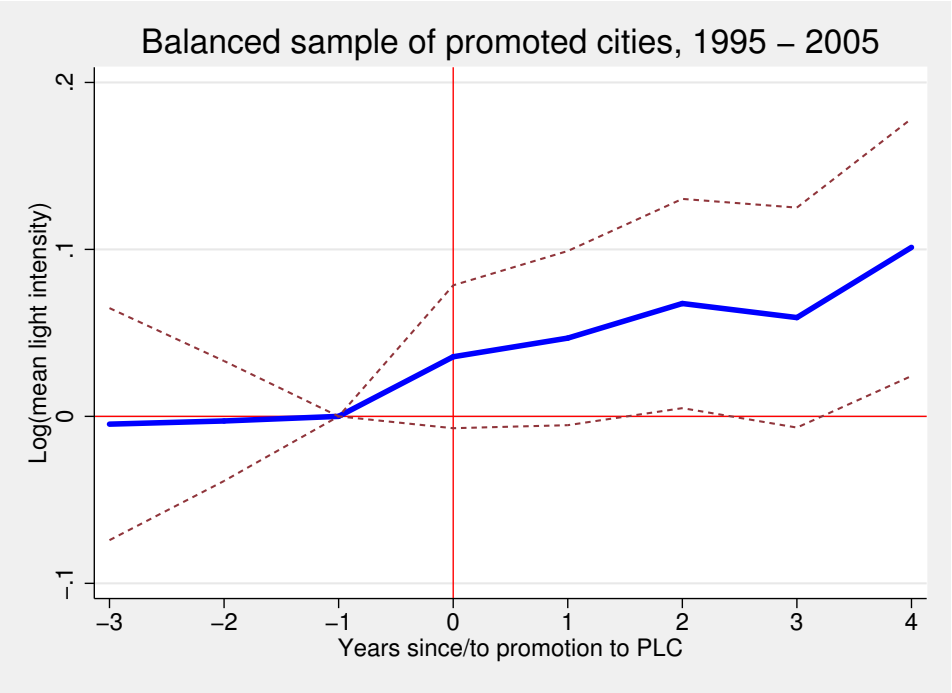


Figure 18: CLC treatment on log(light intensity) of cities, short window



Tables

Table 1: Summary statistics by cohort of treatment

Year	Variable	1999	2000	2001	2002	2003	2006	No SEZ
1982	Population	323,019	415,293	482,228	669,100	595,371	502,567	307,517
		[148,621]	[245,332]	[264,083]	[298,826]	[426,756]	[288,047]	[246,969]
	Ag share of employment	0.70	0.71	0.71	0.75	0.76	0.82	0.83
		[0.20]	[0.17]	[0.20]	[0.14]	[0.23]	[0.12]	[0.13]
1990	Population	374,489	460,590	514,546	721,836	656,416	554,,873	338252
		[186,510]	[270,590]	[306,091]	[317,528]	[437,979]	[327,688]	[269,492]
	Ag share of employment	0.68	0.64	0.67	0.74	0.76	0.81	0.81
		[0.20]	[0.21]	[0.24]	[0.15]	[0.23]	[0.14]	[0.14]
1992	Share lit	0.26	0.29	0.19	0.26	0.33	0.24	0.15
		[0.31]	[0.33]	[0.23]	[0.26]	[0.36]	[0.26]	[0.24]
2000	Population	417,010	509,375	552,339	719,244	702,560	582,081	351,606
		[211,516]	[269,882]	[330,759]	[322,484]	[457,925]	[348,848]	[277,376]
	Ag share of employment	0.67	0.54	0.62	0.69	0.69	0.74	0.78
		[0.12]	[0.24]	[0.21]	[0.16]	[0.23]	[0.16]	[0.15]
	GDP (100 mil 2000 RMB)	24.53	43.94	35.62	51.38	44.33	30.40	15.23
		[19.12]	[30.62]	[22.80]	[35.79]	[36.03]	[28.44]	[17.92]
	Revenue (100 mil 2000 RMB)	1.56	1.92	1.30	1.68	1.44	1.24	0.58
		[1.75]	[1.51]	[0.71]	[0.98]	[1.39]	[1.07]	[0.67]
	Share lit	0.37	0.44	0.37	0.45	0.46	0.42	0.26
		[0.32]	[0.33]	[0.25]	[0.31]	[0.38]	[0.34]	[0.32]
2010	Population	422,074	548,310	569,757	703,016	677,965	577,390	347,502
		[237,607]	[286,911]	[348,056]	[327,214]	[444,445]	[336,214]	[264,703]
	Ag share of employment	0.51	0.42	0.47	0.55	0.57	0.57	0.69
		[0.12]	[0.23]	[0.19]	[0.19]	[0.25]	[0.18]	[0.16]
	GDP (100 mil 2010 RMB)	94.77	203.45	168.88	232.86	203.12	127.12	67.65
		[67.90]	[117.34]	[109.93]	[162.27]	[200.67]	[112.39]	[79.46]
	Revenue (100 mil 2010 RMB)	6.91	12.69	7.80	11.91	13.25	6.52	3.19
		[8,60]	[7.93]	[5.66]	[9,36]	[21.92]	[7.40]	[4.72]
	Share lit	0.47	0.52	0.49	0.54	0.59	0.53	0.33
		[0.26]	[0.33]	[0.26]	[0.29]	[0.34]	[0.32]	[0.32]
–	Land slope (degrees)	3.42	2.85	3.26	1.97	2.16	2.09	3.79
		[1.92]	[2.22]	[2.86]	[1.59]	[2.45]	[2.03]	[3.31]
–	Elevation (m)	776	667	744	308	547	422	1,215
		[589]	[558]	[1,045]	[227]	[546]	[569]	[1,260]
–	Dist to port (km)	621	990	757	529	733	455	844
		[267]	[1,062]	[489]	[329]	[444]	[361]	[679]
–	Area (km2)	1,504	2,832	2,943	3,039	3,628	2,223	5,267
		[947]	[2,414]	[2,462]	[1,974]	[7,616]	[1,876]	[12,051]
	N	9	14	16	35	26	374	1,356

Notes: Standard deviations in brackets. Sources: Population Census, *Province Statistical Yearbooks*, Nighttime lights from NASA DMSP-OLS, and the Chinese Academy of Sciences.

Table 2: Baseline event study estimates, with outcome variable log(GDP)

	(1) Full sample	(2) Full sample	(3) Full sample
$t \leq -3$	-0.0063	0.0025	0.031
	(0.025)	(0.025)	(0.025)
$t = -2$	-0.0089	0.00087	0.0086
	(0.014)	(0.015)	(0.015)
$t = 0$	0.024**	0.018	0.011
	(0.0098)	(0.012)	(0.013)
$t = 1$	0.025	0.023	0.013
	(0.016)	(0.017)	(0.018)
$t = 2$	0.042*	0.043*	0.022
	(0.023)	(0.025)	(0.025)
$t = 3$	0.069**	0.077***	0.035
	(0.029)	(0.029)	(0.029)
$t = 4$	0.080**	0.094***	0.048
	(0.034)	(0.035)	(0.033)
$t = 5$	0.098**	0.11***	0.060
	(0.040)	(0.042)	(0.040)
$t \geq 6$	0.15**	0.14**	0.082
	(0.061)	(0.062)	(0.058)
Year effects	Yes	–	–
Region-by-year effects	–	Yes	–
Province-by-year effects	–	–	Yes
Observations	7213	7213	7213
Adjusted R^2	0.914	0.921	0.942
Counties	462	462	462

Notes: Each column represents event study estimates from one regression. Column (1) controls for year effects, Column (2) controls for region-by-year effects, and Column (3) controls for province-by-year effects. Standard errors in parentheses, and clustered at the county-level throughout. *, **, and *** indicate significance at the 10, 5, and 1 percent level, respectively. The sample is the set of counties that received their first SEZ (at the province-level) in 2000–2006.

Table 3: Event study estimates excluding 2006 cohort, with outcome variable log(GDP)

	(1) Full sample	(2) Full sample	(3) Full sample	(4) Full sample
$t \leq -3$	-0.037	-0.016	0.016	0.016
	(0.030)	(0.032)	(0.030)	(0.030)
$t = -2$	-0.0067	-0.0017	0.012	0.012
	(0.020)	(0.021)	(0.021)	(0.021)
$t = 0$	0.026*	0.020	0.015	0.015
	(0.015)	(0.019)	(0.020)	(0.020)
$t = 1$	0.017	0.012	0.0080	0.0080
	(0.022)	(0.024)	(0.024)	(0.024)
$t = 2$	0.030	0.036	0.026	0.026
	(0.029)	(0.031)	(0.031)	(0.031)
$t = 3$	0.044	0.058	0.038	0.038
	(0.036)	(0.036)	(0.037)	(0.037)
$t = 4$	0.056	0.074*	0.056	0.055
	(0.041)	(0.041)	(0.042)	(0.043)
$t = 5$	0.068	0.081*	0.065	0.063
	(0.045)	(0.046)	(0.047)	(0.047)
$t \geq 6$	0.069	0.057	0.067	0.058
	(0.052)	(0.052)	(0.052)	(0.050)
$t = 7$				0.039
				(0.054)
$t = 8$				0.071
				(0.054)
$t \geq 9$				0.086
				(0.056)
Year effects	Yes	–	–	–
Region-by-year effects	–	Yes	–	–
Province-by-year effects	–	–	Yes	Yes
Observations	7213	7213	7213	7213
Adjusted R^2	0.914	0.921	0.942	0.942
Counties	462	462	462	462

Notes: Each column represents event study estimates from one regression. Column (1) controls for year effects, Column (2) controls for region-by-year effects, and Columns (3) and (4) control for province-by-year effects. Standard errors in parentheses, and clustered at the county-level throughout. *, **, and *** indicate significance at the 10, 5, and 1 percent level, respectively. The sample is the set of counties that received their first SEZ (at the province-level) in 2000–2006, and counties in the 2006 cohort are used only to estimate the time effects.

Table 4: Event study estimates on restricted subsamples, with outcome variable log(GDP)

	(1) Full sample	(2) Counties only	(3) One SEZ only	(4) No prov cities
$t \leq -3$	0.031	0.025	0.038	0.037
	(0.025)	(0.029)	(0.025)	(0.026)
$t = -2$	0.0086	0.0048	0.013	0.011
	(0.015)	(0.016)	(0.016)	(0.016)
$t = 0$	0.011	0.0077	0.013	0.012
	(0.013)	(0.016)	(0.014)	(0.014)
$t = 1$	0.013	0.031	0.014	0.013
	(0.018)	(0.020)	(0.019)	(0.019)
$t = 2$	0.022	0.046	0.023	0.016
	(0.025)	(0.029)	(0.026)	(0.026)
$t = 3$	0.035	0.064*	0.039	0.030
	(0.029)	(0.034)	(0.031)	(0.030)
$t = 4$	0.048	0.076*	0.053	0.045
	(0.033)	(0.039)	(0.035)	(0.035)
$t = 5$	0.060	0.10**	0.070	0.055
	(0.040)	(0.047)	(0.043)	(0.042)
$t \geq 6$	0.082	0.12*	0.095	0.078
	(0.058)	(0.070)	(0.061)	(0.061)
Province-by-year effects	Yes	Yes	Yes	Yes
Observations	7213	6587	7038	6916
Adjusted R^2	0.942	0.943	0.942	0.941
Counties	462	420	451	443

Notes: Each column represents event study estimates from one regression. Column (1) replicates Column (3) of Table 2. Relative to the full sample in Column (1), Column (2) excludes prefecture city centers, Column (3) excludes counties with multiple SEZs, and Column (4) excludes counties under province-level cities. Province-by-year effects included throughout. Standard errors in parentheses, and clustered at the county-level throughout. *, **, and *** indicate significance at the 10, 5, and 1 percent level, respectively. The sample is the set of counties that received their first SEZ (at the province-level) in 2000–2006.

Table 5: Event study estimates, with outcome variable log(revenue)

	(1) Full sample	(2) Full sample	(3) Full sample
$t \leq -3$	0.0092	-0.029	0.026
	(0.031)	(0.030)	(0.028)
$t = -2$	0.019	0.0046	0.038*
	(0.023)	(0.020)	(0.021)
$t = 0$	0.040**	0.036*	0.010
	(0.019)	(0.021)	(0.020)
$t = 1$	0.077***	0.072**	0.044
	(0.027)	(0.029)	(0.027)
$t = 2$	0.13***	0.15***	0.11***
	(0.037)	(0.041)	(0.036)
$t = 3$	0.17***	0.21***	0.14***
	(0.048)	(0.053)	(0.046)
$t = 4$	0.24***	0.27***	0.18***
	(0.057)	(0.062)	(0.055)
$t = 5$	0.27***	0.31***	0.22***
	(0.063)	(0.068)	(0.061)
$t \geq 6$	0.40***	0.42***	0.31***
	(0.091)	(0.100)	(0.085)
Year effects	Yes	–	–
Region-by-year effects	–	Yes	–
Province-by-year effects	–	–	Yes
Observations	7829	7829	7829
Adjusted R^2	0.853	0.861	0.902
Counties	471	471	471

Notes: Each column represents event study estimates from one regression. Column (1) controls for year effects, Column (2) controls for region-by-year effects, and Column (3) controls for province-by-year effects. Standard errors in parentheses, and clustered at the county-level throughout. *, **, and *** indicate significance at the 10, 5, and 1 percent level, respectively. The sample is the set of counties that received their first SEZ (at the province-level) in 1999–2006.

Table 6: Event study estimates, with outcome variable log(expenditure)

	(1) Full sample	(2) Full sample	(3) Full sample
$t \leq -3$	-0.040*	-0.060***	-0.038*
	(0.023)	(0.022)	(0.023)
$t = -2$	-0.015	-0.019	0.013
	(0.016)	(0.016)	(0.019)
$t = 0$	0.0055	-0.0079	-0.021
	(0.015)	(0.018)	(0.019)
$t = 1$	0.041**	0.018	-0.00029
	(0.018)	(0.020)	(0.019)
$t = 2$	0.062***	0.047*	0.024
	(0.023)	(0.026)	(0.024)
$t = 3$	0.083***	0.062*	0.034
	(0.029)	(0.033)	(0.030)
$t = 4$	0.088***	0.070*	0.051
	(0.033)	(0.037)	(0.034)
$t = 5$	0.093**	0.057	0.055
	(0.037)	(0.041)	(0.038)
$t \geq 6$	0.13***	0.066	0.062
	(0.051)	(0.056)	(0.053)
Year effects	Yes	–	–
Region-by-year effects	–	Yes	–
Province-by-year effects	–	–	Yes
Observations	7816	7816	7816
Adjusted R^2	0.964	0.966	0.974
Counties	471	471	471

Notes: Each column represents event study estimates from one regression. Column (1) controls for year effects, Column (2) controls for region-by-year effects, and Column (3) controls for province-by-year effects. Standard errors in parentheses, and clustered at the county-level throughout. *, **, and *** indicate significance at the 10, 5, and 1 percent level, respectively. The sample is the set of counties that received their first SEZ (at the province-level) in 1999–2006.

Table 7: Event study estimates, with outcome variable log(population)

	(1) Full sample	(2) Full sample	(3) Full sample
$t \leq -3$	0.0064**	0.0077**	0.0065*
	(0.0029)	(0.0033)	(0.0034)
$t = -2$	0.0015	0.0017*	0.00073
	(0.0011)	(0.0010)	(0.00095)
$t = 0$	0.00050	0.00023	0.00084
	(0.0011)	(0.00100)	(0.0010)
$t = 1$	-0.00059	-0.000087	0.00065
	(0.0028)	(0.0024)	(0.0020)
$t = 2$	-0.00057	-0.0000076	-0.00013
	(0.0034)	(0.0034)	(0.0030)
$t = 3$	-0.00082	-0.00070	-0.00084
	(0.0039)	(0.0040)	(0.0037)
$t = 4$	-0.0021	-0.0023	-0.0031
	(0.0047)	(0.0048)	(0.0048)
$t = 5$	0.0029	0.0026	-0.0011
	(0.0073)	(0.0074)	(0.0060)
$t \geq 6$	0.0037	0.0063	0.0038
	(0.010)	(0.0098)	(0.0084)
Year effects	Yes	–	–
Region-by-year effects	–	Yes	–
Province-by-year effects	–	–	Yes
Observations	7876	7876	7876
Adjusted R^2	0.286	0.296	0.352
Counties	471	471	471

Notes: Each column represents event study estimates from one regression. Column (1) controls for year effects, Column (2) controls for region-by-year effects, and Column (3) controls for province-by-year effects. Standard errors in parentheses, and clustered at the county-level throughout. *, **, and *** indicate significance at the 10, 5, and 1 percent level, respectively. The sample is the set of counties that received their first SEZ (at the province-level) in 1999–2006.

Table 8: Change in agriculture's share of employment and SEZ treatment intensity

	(1)	(2)	(3)	(4)
Panel A: Δ $Agshare_{1990-2000}$	Sample = 1993-1999 treated		Sample = 1993-2006 treated	
Years with SEZ, 1990 - 2000	-0.013*	-0.010	-0.0066***	-0.0055***
	(0.0073)	(0.0073)	(0.0019)	(0.0019)
Controls	No	Yes	No	Yes
Province FEs	Yes	Yes	Yes	Yes
Observations	229	223	694	674
Adjusted R^2	0.332	0.374	0.293	0.333
Mean of outcome	-0.11	-0.11	-0.085	-0.084
Panel B: Δ $Agshare_{2000-2010}$	Sample = 2000-2006 treated		Sample = 2000 treated –	
Years with SEZ, 2000 - 2010	-0.0068**	-0.0098***	-0.0031***	-0.0035***
	(0.0028)	(0.0032)	(0.0010)	(0.0011)
Controls	No	Yes	No	Yes
Province FEs	Yes	Yes	Yes	Yes
Observations	457	451	1796	1775
Adjusted R^2	0.346	0.398	0.317	0.347
Mean of outcome	-0.16	-0.16	-0.11	-0.11
Panel C: Δ $Agshare_{1990-2010}$	Sample = 1993-2006 treated		Sample = 1993 treated –	
Years with SEZ, 1990 - 2010	-0.000044	-0.0023**	-0.0016**	-0.0028***
	(0.0012)	(0.0011)	(0.00076)	(0.00074)
Controls	No	Yes	No	Yes
Province FEs	Yes	Yes	Yes	Yes
Observations	682	663	2027	1962
Adjusted R^2	0.466	0.530	0.457	0.500
Mean of outcome	-0.24	-0.24	-0.16	-0.16

Notes: Each estimate is from a separate regression. Robust standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1 percent level, respectively. Panel A and Panel C controls include 1990 farm share, log(1990 population), 1982–1990 population growth, 1982–1990 change in agriculture's share of employment, elevation, and distance to the nearest port. Panel B controls include 2000 farm share, log(2000 population), 1990–2000 population growth, 1990–2000 change in agriculture's share of employment, elevation, and distance to the nearest port.

Table 9: Change in agricultural employment and SEZ treatment intensity

	(1)	(2)	(3)	(4)
Panel A: $\%\Delta$ $Agemp_{1990-2000}$	Sample = 1993-1999 treated		Sample = 1993-2006 treated	
Years with SEZ, 1990 - 2000	-0.016	-0.00065	-0.018***	-0.014***
	(0.032)	(0.030)	(0.0056)	(0.0049)
Controls	No	Yes	No	Yes
Province FEs	Yes	Yes	Yes	Yes
Observations	229	223	694	674
Adjusted R^2	0.172	0.186	0.118	0.159
Mean of outcome	-0.18	-0.17	-0.099	-0.098
Panel B: $\%\Delta$ $Agemp_{2000-2010}$	Sample = 2000-2006 treated		Sample = 2000 treated –	
Years with SEZ, 2000 - 2010	-0.012*	-0.014	-0.0046	-0.0031
	(0.0070)	(0.0093)	(0.0037)	(0.0044)
Controls	No	Yes	No	Yes
Province FEs	Yes	Yes	Yes	Yes
Observations	457	451	1797	1777
Adjusted R^2	0.218	0.563	0.144	0.213
Mean of outcome	-0.31	-0.31	-0.23	-0.23
Panel C: $\%\Delta$ $Agemp_{1990-2010}$	Sample = 1993-2006 treated		Sample = 1993 treated –	
Years with SEZ, 1990 - 2010	-0.0096**	-0.0085**	-0.0068**	-0.0043
	(0.0039)	(0.0034)	(0.0030)	(0.0030)
Controls	No	Yes	No	Yes
Province FEs	Yes	Yes	Yes	Yes
Observations	682	663	2029	1963
Adjusted R^2	0.374	0.426	0.261	0.319
Mean of outcome	-0.44	-0.43	-0.22	-0.23

Notes: Each estimate is from a separate regression. Robust standard errors in parentheses. *, **, and *** indicate significance at the 10, 5, and 1 percent level, respectively. Panel A and Panel C controls include 1990 farm share, log(1990 population), 1982–1990 population growth, 1982–1990 change in agricultural population, elevation, and distance to the nearest port. Panel B controls include 2000 farm share, log(2000 population), 1990–2000 population growth, 1990–2000 change in agricultural population, elevation, and distance to the nearest port.

Table 10: Summary statistics of farmland change by year and by region

Panel A. Average net change in farmland by year		
Year	Net change in farmland (hectare)	Net change as % of farmland at year's start
2002	-81808	-2.51%
2003	-30622	-1.41%
2004	-11471	-0.05%
2005	-10090	-0.05%
2006	-1312	-0.01%
2007	-623	-0.02%
All	-22654	-0.08%

Panel B. Average net change in farmland by province (2002-2007)		
Province	Net change in farmland 2002-2007 (mu)	Net change as % of farmland at start of 2002
Anhui	-188280	-3.18%
Beijing	-43022	-15.66%
Chongqing	-229831	-9.32%
Fujian	-41117	-3.00%
Gansu	-255782	-5.20%
Guangdong	-237895	-7.75%
Guangxi	-152602	-3.49%
Guizhou	-214226	-4.56%
Hainan	-34116	-4.48%
Hebei	-354565	-5.31%
Heilongjiang	103914	0.89%
Henan	-85227	-1.06%
Hubei	-156341	-3.24%
Hunan	-101622	-2.61%
Jiangsu	-170343	-3.45%
Jiangxi	-105015	-3.58%
Jilin	-29131	-0.52%
Liaoning	-53944	-1.30%
Neimenggu	-82381	-1.14%
Ningxia	-131260	-10.60%
Qinghai	-61950	-10.25%
Shaanxi	-455603	-10.11%
Shandong	-128967	-1.69%
Shanghai	-44189	-15.34%
Shanxi	-256517	-5.95%
Sichuan	-318008	-5.08%
Tianjin	-37415	-7.82%
Tibet	-4865	-1.33%
Xinjiang	8656	0.21%
Yunnan	-226827	-3.60%
Zhejiang	-125241	-6.12%

Notes: Data from *China Land Yearbooks*, 2003-2008.

Table 11: Summary statistics of provincial Party Secretaries and BLR leaders' tenures

Total number of province-year obs	151
Unique pairs of PS & BLR head	77
Unique PS positions	58
Unique bureaucrats who served as PS	47
Unique BLR head positions	51
# of province-year obs *in alliance*	77
# of province-year obs where a BLR head is appointed	25
# of PS who appointed a BLR head 2002 (n=8)	0
# of PS who appointed a BLR head 2003 (n=22)	2
# of PS who appointed a BLR head 2004 (n=23)	1
# of PS who appointed a BLR head 2005 (n=22)	4
# of PS who appointed a BLR head 2006 (n=25)	4
# of PS who appointed a BLR head 2007 (n=26)	2
# of PS who appointed a BLR head 2008 (n=24)	12
Average PS tenure, years (n=32)	2.72
s.d.	0.99
Average BLR head tenure, years (n=23)	3.22
s.d.	1.17
Average length of PS/BLR head collaboration, years (n=45)	2.18
s.d.	1.21
# of PS terms where the PS has been *in alliance* and *not in alliance* during his term (Case 1)	29
# of PS terms where the PS was never *in alliance* (Case 2)	27
# of PS terms where the PS was *in alliance* with multiple BLR heads (Case 3)	0
# of PS terms where the PS overlapped perfectly with a BLR head that he appointed (Case 4)	21

Notes: Data from China Land Yearbooks & Province Yearbooks 2002-2008.

Table 12: Summary statistics of the loss farmland by *alliance*

	Mean	S.D.	Min	25th pct	Median	75th pct
Alliance	-26603	47166	-264111	-28393	-9671	-630
n=63						
No alliance	-11626	33802	-102382	-18467	-2590	415
n=64						

Notes: Unit of observation is province-year.

Table 13: Summary statistics of the loss of farmland by case of PS term

Case	Pairs in pre-2004	Pairs in post-2004	Mean annual change in farmland pre-2004 (hectare)	Mean annual change in farmland post-2004 (hectare)
1	5	24	-42868	-3859
n=29			n=5	n=17
2	3	24	15717	-4404
n=27			n=3	n=14
4	11	10	-28919	-9618
n=21			n=11	n=7
All	19	58	-25542	-5162
n=77			n=19	n=38

Notes: Observations are pairs of bureaucrats (PS and BLR head) with overlapping terms.

Table 14: Summary statistics of AOT by province, March 2000 – November 2013

Province	Average AOT
Anhui	0.42
Beijing	0.28
Chongqing	0.4
Fujian	0.25
Gansu	0.21
Guangdong	0.36
Guangxi	0.42
Guizhou	0.31
Hainan	0.28
Hebei	0.31
Heilongjiang	0.16
Henan	0.47
Hubei	0.43
Hunan	0.44
Inner Mongolia	0.18
Jiangsu	0.47
Jiangxi	0.36
Jilin	0.19
Liaoning	0.24
Ningxia	0.25
Qinghai	0.18
Shaanxi	0.29
Shandong	0.45
Shanghai	0.48
Shanxi	0.25
Sichuan	0.3
Tianjin	0.42
Tibet	0.14
Xinjiang	0.27
Yunnan	0.2
Zhejiang	0.34

Notes: Data on Aerosol Optical Thickness from NASA.

Table 15: Summary statistics of provincial Party Secretaries and EPB leaders' tenures

Total number of province-month obs	4242
Unique pairs of PS & EPB head	163
Unique PS positions	109
Unique bureaucrats who served as PS	90
Unique EPB head positions	82
# of province-month obs *in alliance*	1960
Annual average # of EPB appointments	5.35
Average PS tenure, months (n=79)	49.19
s.d.	23.71
Average EPB head tenure, months (n=58)	57.69
s.d.	30.12
Average length of PS/EPB head collaboration, months (n=141)	28.84
s.d.	20.01
# of PS terms where the PS has been *in alliance* and *not in alliance* during his term (Case 1)	41
# of PS terms where the PS was never *in alliance* (Case 2)	46
# of PS terms where the PS was *in alliance* with multiple EPB heads (Case 3)	9
# of PS terms where the PS overlapped perfectly with a EPB head that he appointed (Case 4)	0

Notes: Data from *China Statistical Yearbooks* and *Province Yearbooks*, 2000-2013.

Table 16: Province-year level results with annual change in farmland as outcome variable

	(1) OLS	(2) with post-2004	(3) with province FEs	(4) with case dummies
Alliance	-12477.79	-3545.86	-78.28	
	(9326.56)	(12579.03)	(12459.88)	
Post		33900.51**	33796.64**	
		(6418.85)	(9470.29)	
Alliance × Post		-7239.21	-9684.89	
		(11574.37)	(13228.47)	
Case1				-6438.57
				(8277.35)
Case4				-23888.76**
				(11914.10)
N	127	127	127	127
R^2	0.02	0.15	0.39	0.06

Notes: Cluster robust standard errors (at the pair level) in parentheses.
* $p < 0.10$, ** $p < 0.05$

Table 17: Pair-level results with average change in farmland as outcome variable

	(1) OLS	(2) with post-2004	(3) with province FEs	(4) with case dummies
Alliance	-9608.99	9264.36	13588.37	
	(7079.26)	(16125.40)	(18773.39)	
Post		34806.37**	44439.63**	
		(14972.84)	(19524.52)	
Alliance × Post		-15999.28	-24858.57	
		(15079.09)	(21644.08)	
Case1				-8496.94
				(6104.29)
Case4				-21646.00**
				(8044.07)
N	57	57	57	57
R^2	0.03	0.24	0.57	0.11

Notes: Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$

Table 18: Province-year level results with AOT as the outcome variable

	(1)	(2)	(3)	(4)
Alliance	0.012	0.014	0.0081	-0.0021
	(0.021)	(0.020)	(0.021)	(0.0037)
PS birth year		-0.000100	-0.0013	-0.00036
		(0.0019)	(0.0025)	(0.00048)
PS born in province		0.084**	0.084**	0.0030
		(0.038)	(0.037)	(0.0066)
N	4235	4235	4235	4235
R^2	0.001	0.022	0.219	0.627

Notes: Standard errors clustered at the pair level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 19: Roll-out by year and province

Year	Province	Counties newly subjected to CLC
1982	Shandong	2
	Zhejiang	1
1983	Anhui	9
	Fujian	19
	Guangdong	42
	Guangxi	6
	Hebei	20
	Heilongjiang	11
	Henan	33
	Hubei	9
	Hunan	17
	Inner Mongolia	9
	Jiangsu	58
	Jiangxi	17
	Jilin	15
	Shaanxi	16
	Shandong	34
	Shanxi	4
	Sichuan	26
	Yunnan	4
	Zhejiang	18
1984	Heilongjiang	23
	Liaoning	13
1985	Fujian	15
	Gansu	9
	Jilin	9
	Shandong	6
	Shanxi	14
	Sichuan	38
	Zhejiang	8
1986	Hebei	6
	Henan	30
	Hunan	12
1987	Anhui	5
	Guangxi	1
	Zhejiang	4
1988	Anhui	8
	Guangdong	29
	Hainan	1
	Hebei	8
	Hunan	10
	Shanxi	3
1989	Shandong	6
1991	Heilongjiang	2
1992	Anhui	7
	Heilongjiang	2
	Hubei	7
	Jiangxi	1
	Jilin	4
	Shandong	1
	Sichuan	9
1993	Guangxi	2
	Hebei	63
	Heilongjiang	4
	Hubei	4
	Jilin	2
	Shanxi	10
	Sichuan	1
1994	Guangxi	2
	Henan	10
	Hunan	3

Table 20: 1982 county summary statistics by treatment

	Not under PC by 1994		Under PC by 1994	
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Population density (persons/km2)	183.59	225.61	350.77	236.01
Share of population employed	49.92	5.25	51.15	6.09
Share of employment in agriculture	83.32	11.42	81.3	9.82
Share of employment in industry	7.91	8.15	10.51	7.81
Shortest distance to a city (arc length)	1.05	1	0.53	0.32
N	1316		760	