

# The Effect of Economic Reform and Industrial Policy in a Panel of Chinese Cities\*

Simon Alder<sup>†</sup>      Lin Shao<sup>‡</sup>      Fabrizio Zilibotti<sup>§</sup>

June 18, 2013

## Abstract

We estimate the development effect of the establishment of Special Economic Zones (SEZ) in China using a panel of 276 Chinese cities from 1988 to 2010. Our difference-in-difference estimator exploits the variation in the establishment of zones across time and space. We propose a strategy to control for potential selection effects. The results from our baseline specification suggest that the establishment of a major zone led to an increase in the level of GDP around 12%, but did not lead to a permanently steeper growth path. This result is confirmed with alternative specifications and in a sub-sample of inland provinces, where the selection of cities in which zones were established was more transparent. Decomposing GDP into physical capital, efficient labor, and TFP shows that the effect of the zones went mainly through the accumulation of physical capital. Using electricity consumption and light intensity as two alternative measures for economic activity confirms positive effects of the zones.

---

\*We are thankful to seminar participants at the University of Zurich, ZEW Mannheim, CUHK Conference on the Chinese Economy, Wustl, University of Bern, DEGIT XVII Milan, RES 2013, and to Janneke Pieters, Ray Riezman, Nico Voigtlaender, and James Kung for valuable comments and data sources. Xiaojun Chen, Florian Hälg, Lingqing Jiang, Liu Liu, Yung-Chieh Huang, and Sebastian Ottinger provided excellent research assistance. Financial support from the European Research Council (ERC Advanced Grant IPCDP-229883) is gratefully acknowledged.

<sup>†</sup>University of Zurich, simon.alder@econ.uzh.ch.

<sup>‡</sup>Washington University in St. Louis, linshao@wustl.edu.

<sup>§</sup>University of Zurich, fabrizio.zilibotti@econ.uzh.ch.

# 1 Introduction

China has been the fastest growing large economy over the last three decades, with an average annual growth rate above 8% between 1978 and 2010. Its economic boom has been associated with a massive process of policy and institutional reforms. While there is little doubt that the reform process altogether was key for economic development, it remains difficult – as of today – to disentangle the contribution of the different elements of the reform. China has been often portrayed as a case in favor and as often as a case against the so-called "Washington consensus" approach. On the one hand, it has progressively liberalized its markets moving from a state-controlled allocation mechanism to a market economy. On the other hand, its economic institutions today continue to be very different from those of Western economies, as witnessed, for instance, by the important persistent role of state-controlled firms and banks, weak contract enforcement, etc.. In addition, over the years the government has pursued proactive industrial policies creating special status privileges for specific cities, industries and regions. While liberalization per se belongs to the realm of orthodox policies, the imbalances and distortions to resource allocation created by the Chinese industrial policy do not.

This paper uses the variation in the time of establishment of different types of Special Economic Zones (SEZ) across cities and years to estimate the development effects of such industrial policy. SEZ are interesting for a variety of reasons. First, they have been a centerpiece of the Chinese development strategy – inspired to gradualism and experimentation. Second, they have exacerbated the uneven development across geographic areas and sectors. Last but not least important, their effects are easier to measure relative to other reforms, as well-defined changes in the legal status were introduced gradually over time and space.<sup>1</sup> SEZ worked initially as experiments with market allocation within geographically limited areas along the coast. The policy changes in the SEZ included liberalization of labor markets, foreign direct investment, ownership, and export. In addition, local political leaders were granted substantial autonomy and could shape important aspects of the industrial policy. After the success of some of the early zones, the SEZ were extended first to other cities along the coast and then starting in the early 1990s also to inland regions. New zones have progressively been established until today.<sup>2</sup>

We use a panel of 276 cities at the prefecture level over the period 1988-2010 to compare the development across treated and non-treated cities. We also compare the effects of different types of zones. Our econometric strategy controls for time-invariant heterogeneity at the city level by using fixed effects. It also controls for province-specific shocks by using province $\times$ time fixed effects. We first perform a difference-in-difference analysis, by regressing (the logarithm of) GDP on a reform indicator that switches on the year after a city has received the SEZ status. We also control for city characteristics such as size and population. In our baseline specification, the introduction of a SEZ is associated with an increase in the city GDP level of 12%. The effect varies across different types of zones comprising different rules and policy packages. The baseline specification controls for population and land area as time-varying city characteristics, but the effect is robust to also controlling for local government spending. Since it is plausible that there are lags in the effects of reforms, we also consider a more

---

<sup>1</sup>Others reforms are more difficult to identify. For instance, the introduction of the "Household Responsibility System" in agriculture in the late 1970's was very important, but came contextually with other reforms. Moreover, its implementation took (at least initially) the form of a gradually increasing tolerance of the government towards pre-existing informal arrangements.

<sup>2</sup>Some of these later zones are called High-Tech Industrial Development Zones (HIDZ) or Economic and Technological Development Zones (ETDZ) and they had policies similar to the first Special Economic Zones established in 1980. We will use the term Special Economic Zones (SEZ) in a broader sense to refer to all types of zones and label the first set of SEZ as comprehensive SEZ.

flexible specification where the effect of the reform is allowed to vary as a function of the time elapsed since the start of the treatment. With this flexible specification, we find that the effect of SEZ flattens out after about ten years; the long-term effect of a SEZ is an increase of about 20% in the GDP level.

Our analysis is subject to two important *caveats*. First, the assignment of cities to treatment and control groups may not be random. The Chinese government may have selected cities based on some prior knowledge that the conditions for industrial development might be especially favorable ("picking winners"), or to the opposite in order to curb regional inequality. Picking winners appears to have been especially important in the first phase of the reforms, when all SEZ were chosen on the coast and close to potential trading partners and investors such as Hong Kong and Taiwan. Ideally, one would like to find valid instruments to isolate sources of exogenous variation. However, in practice, this is very difficult. We partially address the selection concern by two complementary strategies. First, we restrict the sample to cities located in inland provinces where selection of the zones was largely based on a rigid administrative criterion. Second, we add to our flexible specification an indicator of the pre-reform years. The results are in both cases reassuring: the effect of SEZ is robust in the restricted sample, and the pseudo-effects before the actual establishment of the zone are insignificant. There is a clear structural break in the year of the reform when the effects start increasing and the effects become significant.

The second *caveat* is about data quality. First, one might worry that local statistics may have been manipulated strategically in order to create the impression that the SEZ was successful so as to attract further government support. Second, while city-level nominal GDP data are available, city-level price deflators are more problematic (and also less generally available). Our strategy is designed to make use only of nominal variables. First, the inclusion of city fixed effects removes possible bias from time-unvarying price-level differences. Yet, since differences in inflation would remain an issue, we include the interaction between time and province fixed effects to remove the effect of cross-province differences in inflation. This leaves open the issue of within-province differences on inflation. The problem would be especially severe if reformed cities had higher inflation rates than non-reformed cities. To address this concern, we first document, with the limited data available, that reformers do not appear to have higher inflation rates than non-reformers. Next, we complement our analysis with alternative proxies of GDP that do not depend on prices: electricity consumption and light intensity measured by satellites. The results confirm the existence of robust significant effects of SEZ.

We also studied the channels through which GDP increased. We find that the main effect of SEZ appears to be to have attracted investments in physical capital and population. In contrast, there are no significant effects of human capital or TFP, although this may be due to measurement error and to the low quality of local data on education. One might suspect that the SEZ simply led to a concentration of resources into the treated areas, drawing on resources of adjacent areas. We find that this is not the case. When we run the same regressions at the prefectural level (a larger administrative unit) *excluding* the city center (where, typically the SEZ treatment was concentrated) we find large positive effect. This rules out the possibility of negative local spillovers within prefectures, although of course our analysis does not rule out negative externality across prefectures.

## 1.1 Related Literature

This paper is related to a large literature seeking to understand the effects of institutional change on economic development. The main challenge common to such studies is to find sources of variation in institutions in order to estimate causal effects on development. Our contribution is to exploit variation in

economic institutions across Chinese cities and time which allows us to control for potential confounding factors when estimating the effects on GDP.

To the best of our knowledge, Wei (1993) was the first to use Chinese city-level data to assess the effects of different types of SEZ. He finds that a set of coastal cities where special policies were introduced in 1984 had a significantly higher average growth rate during the early reform period, while other types of preferential policies did not.<sup>3</sup> His analysis relies on a comparison (over a small number of years) between cities that were reformed in early years – a very small and selected group – and cities that had not been reformed. Later studies include Démurger *et al.* (2002) and Jones *et al.* (2003) who also study differences in growth rates during a certain period relying only on cross-sectional variation.<sup>4</sup> Different from these studies, our study exploits the variation in the establishment of SEZ across both time and cities. This allows us to perform a difference-in-difference analysis that controls for heterogeneity, by including fixed effects and time-varying province-level shocks.

A number of studies have looked at other outcomes of SEZ. Cheng and Kwan (2000) look at the determinants of foreign direct investment and find that provinces with a zone attract significantly more FDI. This confirms the important role of FDI and agglomeration economies, but does not exploit the within-variation which we are using to identify the effect of economic reform. A recent study by Wang (2013) uses a panel of Chinese cities and finds in a difference-in-difference analysis strong positive effects of SEZ on FDI, exports, and the output of foreign enterprises. These measures are closely linked to the outward oriented nature of SEZ which may explain the high magnitudes of the effects of up to 100% increases in levels. The effects on other outcomes (which do not include GDP) are smaller and not robust. Our findings are complementary to Wang (2013) insofar as we focus on GDP as a comprehensive measure for the development of the local economy, while her study focuses on intermediate targets. Our analysis also differs from hers insofar as we distinguish between state-level and province-level SEZ (which are different policies and turn out to have different effects) and by restricting attention to data featuring a consistent definition of cities over time.<sup>5</sup> Furthermore, we complement our analysis with data on electricity consumption and light intensity which we view as two alternative proxies for GDP, and show that positive reform effects are robust in this dimension.

The effect of SEZ have also been studied at the firm-level. Head and Ries (1996) analyze the location decision of international firms in Chinese cities and find that SEZ had a positive effect and this was amplified by agglomeration economies. Schminke and Van Biesebroeck (2013) estimate the effect of a firm being located inside a SEZ on its productivity and export behavior. Their findings suggest that firms inside of SEZ have higher output per worker and higher capital intensity, but no higher TFP. Furthermore, these firms export more and there seem to be significant differences in the export behavior of firms in high-tech zones compared to zones with a broader industry composition.<sup>6</sup> These findings are broadly consistent with ours.

Zones with special policies are not unique to China, and the effects of such industrial policies have

---

<sup>3</sup>Wei (1993) uses two samples of cities. The first sample has 434 cities for the period 1988-1990 and the second 74 cities for the period 1980-1990.

<sup>4</sup>Both of these papers partially address the heterogeneity problem by including control variables such as distance to the coast. However, they do not control for fixed effects or other time-unvarying shocks.

<sup>5</sup>The distinction between state-level and province-level SEZ is crucial in the sense that when treating them the same, then SEZ do not have any effect on GDP. The definition of cities is relevant because cities were formed out of earlier administrative entities and this formation coincided with other changes that could be confounded with the effects of SEZ. See the discussion in the data section for a more detailed explanation of the definition of the zones and the construction of the sample.

<sup>6</sup>Firms in high-tech zones seem to export goods of higher quality and price, while firms in the broader Economic and Technological Development Zones seem to reach a larger number of export markets.

been studied in other countries.<sup>7</sup> In a recent study on US “Empowerment Zones”, Busso, Gregory, and Kline (2013) compare locations that were selected for special treatment, such as tax-credits and subsidies for disadvantaged neighborhoods, with other similar locations which were rejected or treated in a second round. They have access to detailed micro-data on employment, housing, and commuting which allows them to do an extensive welfare analysis. Their findings suggest that the treatment had significantly positive effects on employment and wages, while the efficiency costs were relatively small. Their study demonstrates that preferential policies can be beneficial in a developed country, while our focus is on China as a fast developing country that has used SEZ to experiment with different possibilities of far-reaching economic reform.

Our study also relates to a large literature on liberalization and industrial policy in general and specifically the Chinese reform experience.<sup>8</sup> Rodrik (2006) attributes a significant role in the success of Chinese reforms to government policies that created distortions in favor of more advanced industries. As we document below, the key characteristic of the Chinese reform process was an experimental approach towards finding a combination of liberalization and a strong role of the state. The SEZ played an important part in this process and our findings are therefore in line with the views of Rodrik (2004) and Dewatripoint and Roland (1995) that the state can generate information about the potential of sectors through experimentation. Liberalization reforms and industrial policies that were staggered across time, space and sectors were also prominent in other countries. For instance, Aghion *et al.* (2008) study the case of India using an empirical strategy similar to that adopted in this paper.

The rest of the paper is structured as follows. Section 2 provides an overview of the institutional background of economic reforms in China and the role of SEZ. Section 3 describes our data sources and sample selection. Section 4 discusses the empirical strategy and the main results and Section 5 establishes the robustness to alternative data. Section 6 concludes.

---

<sup>7</sup>Akinci and Crittle (2008) provide an overview.

<sup>8</sup>See Perkins (1988) for an overview of the early literature on reform effects in China. Xu (2011), Brandt and Rawski (2008), and Naughton (2007) provide a more updated overview.

## 2 China's Economic Reforms and Institutions

During the thirty years after 1949, the year when the People's Republic of China was established, the Chinese Communist Party (CCP) built an economic system based on a rigid socialist planned model. The State Planning Commission, a division of the State Council, controlled the development of the economy. The two decades preceding 1978, the year of Mao's death and Deng Xiaoping's rise, had been characterized by low economic growth, high volatility and an intense social turmoil (most notably, the Great Famine, and the Cultural Revolution). The new reformist political leadership after Mao's death was in desperate need for measures to reconstruct the political institutions and revive the economy. There were, however, no existing blueprints showing how to proceed. As a result, as Deng put it, "one has to grope for stepping-stones as he crossed the river". The experiment-then-verify approach became the guideline for almost every policy innovation during China's past 30 years of reform.

The first policy breakthrough happened in rural areas, where agricultural production had until then been carried out in collective communes. Under a new production system which was later called the Household Responsibility System (HRS), farmers were entitled, after fulfilling their procurement quota, to the rest of their agricultural output. The HRS was first implemented in Anhui and Sichuan provinces and extended to the whole country by the end of 1982. It was a huge success. The national grain harvest increased from 304.8 million tons in 1978 to 407.3 million tons in 1984.

The leadership soon realized that reforms had to be extended to the urban area. Their proposal was to open up China to foreign investment. They believed that foreign investors would not only bring physical capital, but also advanced technology and management skills, enabling China to catch up quickly to the most advanced countries through "learning by doing". Despite Deng's endorsement and local official's desire for urban reform, the reformists' proposal aroused strong resistance from the conservative leaders in CCP's central committee. From the conservative ideologists' point of view, renting China's land to foreign companies and allowing them to exploit China's cheap labor was like selling out China and would expose it to the influence of western ideologies.

### 1980-1984

The establishment of SEZ was the result of a compromise between the reformist and conservative forces. In the year 1980, four cities in the provinces of Fujian and Guangdong, Shenzhen, Zhuhai, Shantou and Xiamen, were granted the status of SEZ. They are geographically limited pieces of land and usually located in the suburban areas of cities. The SEZ were given special economic treatment, including tax deduction and special tariffs for import and export as well as less regulation on foreign exchange and land use. As documented in Wei (1993), foreign firms that resided inside of the SEZ enjoyed first two years of tax holiday, then three years of a low tax rate of 7.5% and after the initial five years, a tax rate of 15% (outside of the zones, the tax rate for foreign firms was 33% and for state-owned firms 55%).

The location of the zones was carefully and strategically chosen. First of all, they were located in cities on the southeastern coast of China, far away from the political center Beijing. Thus, local officials, facing less political resistance from the conservative leaders in the central committee, had more freedom and flexibility to design and implement innovative policies. And due to their distant location and small scale, if the zones were to fail, they would only have a minor influence on the whole country. Second, the zones were geographically close to Hong Kong, Macau and Taiwan. Over the past several hundreds of years, the people in Guangdong and Fujian province had established deep connections with the oversea Chinese through kinship and trade. By locating the zones in these area, Chinese leaders clearly

expected Hong Kong, Macau and Taiwan to be the major sources of capital investment, entrepreneurial dynamism and knowledge about the outside world for the newly established SEZ.

The idea of SEZ was *per se* no Chinese innovation. China's SEZ inherited some essential characteristics of the Export Processing Zones, which had already been established in over 80 countries by 1980 (Naughton (2007) and Vogel (2011)). Like the Export Processing Zones, the SEZ were designed to circumvent the complex rules of import and export. China's SEZ were special in the sense that they also bore the responsibility of policy innovation and experimentation. They were the laboratories for the market economy. According to the official document issued by the party center and State Council, "the four Special Economic Zones would carry on systems and policies that are different from other places. The Special Economic Zones will be regulated primarily by the market." (Vogel (2011: p.399)). The local officials of the zones were implicitly encouraged to be innovative in designing economic policies and institutions. Many of the policy innovations inside of the zone, including the establishment of China's first labor market in Shenzhen, were deemed illegal outside of it. They were, however, later extended to the rest of the country after being proved to be successful.

### **1984-1991**

The success of the SEZ was remarkable. The zones grew at an unprecedented rate, among which Shenzhen experienced an annual growth rate of 54% during the period 1980-1984. In 1984, the four SEZ attracted 26% of China's total FDI. In addition, the zones had developed a set of well-functioning markets for labor, land, capital, transportation and technology (Zeng (2010)). The success of the four early SEZ came in favor of reformists and softened the position of the conservative leaders. In 1984, 14 coastal cities were extended the rights to build Economic and Technological Development Zones (ETDZ). The ETDZ shared essentially the same policies as the initial four SEZ, with the only difference in the income tax rate (Wei (1993)). Interestingly, many of the 14 cities were old treaty ports that were opened up at the end of the Qing Dynasty. Even before receiving the special status, these cities, with an established industrial base and a well educated labor force, were among the most developed areas in China. According to the official statistics, the 14 coastal cities constituted 21.8% of the national total industrial output in 1985.

### **1991-2003**

During January and February of 1992, Deng made his famous tour to southern China, including the SEZ of Shenzhen and Zhuhai, to restate his commitment to the reform process and announce new reformist measures. Shortly afterwards, a new SEZ called Pudong New Area, was established in Shanghai. In May, the CCP's party center issued document NO.4, announcing the opening up of the five inland cities along the Yangtze River, nine border cities and all thirty of the provincial capital cities and granting them the same privileges as the SEZ (Fewsmith (2001)). Following the instruction, 18 state-level ETDZ were approved during 1992-1993 and 17 more during 2000-2002, all located in inland provinces. Another type of zones, the High-tech Industry Development Zone (HIDZ), was also established during the same period. The establishment of the HIDZ was an essential part of the "Torch Program", a program carried out by the Ministry of Science and Technology to guide and facilitate the development of China's high-tech industries. ETDZ and HIDZ were granted the same preferential policies and administration status. However, they emphasized different goals of the development strategy. The main goal of HIDZ was to help transform domestic research outcomes into profitable high-tech companies. The HIDZ were located

in cities with many universities and research institutions. In several cases, the HIDZ and ETDZ were located in the same city, with HIDZ established several years ahead of ETDZ.

### 2003-present

During the past ten years, the reform of SEZ spread quickly across China. By the year 2005, the system of state-level development zones consisted of 54 ETDZ, 53 HIDZ, 15 Bonded Zones (BZ)<sup>9</sup> and 60 Export Processing Zones (EPZ)<sup>10</sup>. In the year 2005, the 54 ETDZ contributed 4.49% of the national GDP and 14.93% of national export (Ministry of Commerce (2006)). Establishing a development zone became the prevalent form for the local government to attract FDI and foster local economic growth. Through shuffling of the local officials across different regions, the governments drew on experiences from the early zones to help develop the later ones (Xu (2011)).

Besides the state-level zones, a large number of lower-level zones were also established during the same period. The preferential policies, given by the central government to the state-level zones, did not apply to these lower-level zones. On the other hand, the lower-level zones were not under close monitoring and regulation of the central government. In December 2003, the State Development and Reform Commission, the Ministry of Land and Resources, the Ministry of Construction, and the Ministry of Commerce together issued Document NO. 2343 to request a thorough investigation on development zones at all levels regarding the violation of the land-use plan. Before the investigation, there were a total number of 6866 development zones of all levels (WEFore (2010)). By the end of 2006 when the investigation was finished, only 1568 zones survived and gained official approval from the state (see State Development and Reform Commission (2006) for the list of zones). A large number of the development zones failed the investigation and were abolished, which included all zones that were lower than province-level and several unqualified province level zones. After 2006, there existed only two levels of development zones – state-level and province-level. Starting from 2010, a number of province-level zones were promoted to state-level conditional on passing certain standards, including performance in economic growth, production safety and environmental protection. By the end of 2010, the number of state-level ETDZ had increased from 54 to 88.

## 2.1 Experimentation and Convergence in the Policies of the Zones

During the early stage of the development of SEZ, the policies were intended not only to attract FDI but also to foster institutional innovation. Therefore, except for tax deduction, protection of private property and land-use policies, local governments were given more freedom to design new institutions. Successful innovations were kept and extended to later waves of development zones (Table 1 of Yeung *et al.* (2009) lists the institutional innovations made by the first five SEZ.). Gradually, the institutional structure of the zones became stable. Policy treatment became uniform across all state-level ETDZs and HIDZs. At the present, the preferential policy treatment for the state-level ETDZ and HIDZ includes: 1) tax and customs duty deduction, 2) discounted land-use price, 3) no regulation on labor contracts and 4) special treatment on bank loans.

---

<sup>9</sup>Bonded Zones were typical free trade zones.

<sup>10</sup>Most of the exports processing zones were established within existing development zones. They were regulated by local customs to assist firms' import and export.

## 2.2 Different Types of the State-level Development Zones

There are five types of state-level development zones: comprehensive Special Economic Zone (comprehensive SEZ), Economic and Technological Development Zone (ETDZ), High-tech and Industrial Development Zone (HIDZ), Bonded Zones (BZ), Export Processing Zone (EPZ) and Border Economic Cooperation Zones (BECZ). The five types of zones share the same preferential treatment in terms of tax deduction, custom duty deduction, reduced land-use price, flexibility in signing labor contract and financing. But the types of zones differ from each other in several dimensions. First, they are under the administration of different government branches. Among them, the comprehensive SEZ, ETDZ and HIDZ are directed by State Council (HIDZ is co-directed by the Ministry of Science and Technology). BZ and EPZ are directed by customs. BECZ were directed by the State Council before 2008. They are now directed by the Ministry of Commerce.

Second, the zones reflect different aspects of the development plan. The comprehensive SEZ, which are located in Shenzhen, Zhuhai, Xiamen, Shantou, Hainan, Shanghai and Tianjin, are the largest in scale and enjoy the most autonomy among all types of the zones. They are expected to play an active role in defining the frontier of economic and social development. The ETDZ share similar policies and development goal with the comprehensive SEZ, such as attracting FDI and boosting export, only at smaller scales. Although the institutional innovation were more active and frequent at the early stage of the development zones, the comprehensive SEZ and ETDZ nowadays are still encouraged to design and experiment with new institutions and policies. The HIDZ, which are co-directed by the Ministry of Science and Technology, focus on fostering the domestic high tech industries. The BZ are typical free trade zones, which are small and closed areas where import and export can be proceeded at a faster speed. They are all located in coastal port cities or border cities, which also help to develop the logistics industry. The function of EPZ is "export processing", which means to import raw materials from abroad, process them and export the final goods without entering the real territory of China. Many of the EPZ are established inside of the ETDZ and HIDZ. The BECZ intend to take advantage of the location of the border cities to foster trade with other countries.

## 2.3 The State-level and Province-Level Zones

State-level and province-level development zones co-existed during the 30 years' history of the economic reform. In some cities, province-level development zones were established before the state-level zones. In fact, a number of the first wave of ETDZ were promoted from the original operating province-level development zones. Despite the equally long history of state-level and province-level zones, they must be distinguished.

One reason to differentiate the two levels of zones is that they have received different policy treatments. Preferential economic policies that the province-level zones have enjoyed have been given by the provincial and lower-level government. In other words, these preferential policies have been constrained by the administrative and legislative power of the provincial government. The state council explicitly requested that "the policies given to the province-level development zones should not be comparable to those given to the state-level ones", in order to prevent excessive competitions between the zones and the waste of land resources (see State Administration of Taxation (2004) for details). In reality, the policy package received by the state-level zones often included both the policies from the central government and the ones from the provincial government.

Another reason is that the policies of many province-level zones targeted specifically the industries

which the local economy had comparative advantages in. In contrast, the fundamental policies of the state-level zones, such as tax and custom duty deduction, were generally industry-blind. However, in recent years some of the state-level zones have leaned towards high-tech industries by providing extra economic incentives for the high tech firms.

In Table 1, we listed the number of state-level and province-level development zones and their average share of industrial output in 3 coastal provinces, where a large fraction of the development zones reside. The data is taken from WEFore (2010) and is for the year 2009. All of the provinces had a larger number of province-level zones than state-level ones. But the state-level zones on average contributed a larger share to industrial output.

## 3 Data

Our data sources can be categorized into two broad categories. The first is official statistics from the National Statistics Bureau of China (NSB). These include GDP, electricity consumption, population, education, government spending, and land area. The second is the light intensity data from weather satellites, which serves as a proxy for GDP. More detailed information about the data sources and the sample selection criteria can be found in the appendix.

### 3.1 Variables and Data Sources

In the following paragraphs, we list the main variables that are used in the empirical part of the paper and their sources. As for the notations, we denote  $i$  as the prefecture city,  $p$  as the province and  $t$  as year.

#### 3.1.1 Dependent Variables

- $\log GDP_{ipt}$  is log of nominal GDP of the prefecture-level cities for the period 1988-2010. It is taken from the *China City Statistical Yearbooks*. We use nominal GDP but control for price changes at the provincial level with a set of province-time fixed effects.
- $\log Electricity_{ipt}$  is the electricity consumption of the prefecture-level cities for the period 1988-2010, which is available in the *China City Statistical Yearbooks* for the same set of cities as GDP. It measures the use of electricity for households' consumption and industrial production and is an approximation for local economic activity.<sup>11</sup>
- $\log Light_{ipt}$ , the average light intensity of the prefecture-level cities is another approximation for local economic activity. In the data provided by the National Geographical Data Center, light intensity is measured on each square km (pixel) on a discrete scale from 0-63. We use digital maps of Chinese cities to aggregate the light intensity of the pixels to administrative units.
- $\log PC_{ipt}$ , the log physical capital stock, is constructed with the perpetual inventory approach. To construct the physical capital stock, we take the data of new investment for the period 1988-2010 from the *China City Statistical Yearbooks*. For some cities, we collect the new investment data from *New China in 60 Year Provincial Statistical Collection* for the earlier period 1978-1987. The province-specific series of investment deflator is obtained from *New China in 60 Years Statistical Collection*.
- $\log EL_{ipt}$ , the log of efficient labor unit, is constructed using population and educational attainment. We take the population data from *China Population Census* for the years 1990, 2000, and 2010 and interpolate the years in between using population growth rate implied by the *China City Statistical Yearbooks*. The educational attainment data also comes from *China Population Census*.
- $\log TFP_{ipt}$ , the log of TFP, is the residual term in log real GDP that can not be accounted for by the physical capital and efficient labor.

---

<sup>11</sup>The electricity consumption of the prefecture area is not available for all years.

### 3.1.2 Explanatory Variables

Our main explanatory variables of interest are indicators for the establishment of SEZ. We thus construct for different types of SEZ an indicator  $I\_Reform_{it}$  which takes value one in the year *after* the establishment of a zone and in all following years. Formally, we define the reform indicator based on the establishment of a zone as

$$I\_Reform_{it} = \begin{cases} 1 & \text{if } ReformYear_i < t \\ 0 & \text{otherwise.} \end{cases},$$

where  $ReformYear_i$  is the year in which a zone was established in city  $i$  and  $t$  is the current year. Note that the second case includes cities which never have a zone. We will also use several variations of this simple indicator to allow a more flexible reform effect and these are discussed in the corresponding empirical sections.

### 3.1.3 Primary Control Variables

The *China City Statistical Yearbooks* contain a large number of time-varying city characteristics. We use two variables which we consider important determinants of economic performance and should not be confounded with the variation in policy. The first control variable is the geographic size of the city, which we refer to as land area and is measured in square kilometers. This variable is available for the city core and the whole corresponding prefecture and the time-variation reflects changes in the legal boundaries of the cities during the reform period. The second control variable is population, which again we observe for the city core and the prefecture.<sup>12</sup>

### 3.1.4 Further control variables

We include in all regressions two sets of fixed effects in order to control for unobserved heterogeneity. Time-invariant city characteristics such as geographical location is absorbed by city fixed effects. Time-varying heterogeneity at the province-level is absorbed by province-time fixed effects. In some regressions we also include city-specific linear time trends. In a robustness analysis, we add expenditure of the local government, obtain from *China City Statistical Yearbooks*, as a further control variable to take into account the impact of public sector spending on aggregate output.

## 3.2 Price Data

The *China City Statistical Yearbooks* published by the National Bureau of Statistics report nominal GDP for the period 1988-2010. We rely on this source because it is the most likely to provide a consistent measurement of GDP across cities and years. Since price data in China is often called into question (see for example Young, 2003), we use nominal figures and address inflation in several ways. First, all our regressions control for city fixed effects and an interaction of province and time dummies. The city fixed effects absorb differences in price levels between cities, while the province-time fixed

---

<sup>12</sup>Although this gives us important information about the changes in the population over time, it has some drawbacks. One problem is that it measures only officially registered residents and may thus exclude immigrant labor force. In one part of our analysis, we address this problem by combining the population from the *China City Statistical Yearbooks* with the more precise data from the census (which is only available every ten years). See the detailed discussion for the level decomposition in the appendix.

effects absorb differential inflation paths across provinces. Second, we check whether reformer cities have different inflation rates than non-reformer in those years for which real GDP is available from the NBS. To do so, we compute an implicit city level deflator from nominal GDP and real GDP. We then compare the average yearly increase in this deflator of cities with a SEZ to cities without a SEZ. Cities with a SEZ do not have higher inflation rates, providing evidence against the concern that inflation could increase with the establishment of a SEZ.<sup>13</sup> Third, we use electricity consumption (in GWh) and light intensity as two alternative measures for economic activity that should not be affected by inflation. As we will show below, positive effects of the SEZ can also be found in these other data sources, which suggests that our results are not driven by price differentials.

### 3.3 Sample Selection

The sample period is 1988-2010 for the yearbook data and 1992-2010 for the light data. For this period, we have a consistent definition of city borders<sup>14</sup> and information on GDP (and the proxies), the date of introduction of SEZ, and the main control variables, land area and population. We focus on 276 prefecture level cities, which are the main urban centers of the corresponding prefectures. Counties are one level below the prefectures and under the jurisdiction of the prefecture level city. If a county's population exceeds a certain threshold, then it is labeled county level city. Because of their lower administrative rank and smaller size, county level cities are difficult to compare to prefecture level cities and we therefore focus on the later.<sup>15</sup> We exclude from our analysis the four cities in which comprehensive SEZ were introduced before 1988, as well as Hainan, where the entire province received the status of SEZ in 1988. Furthermore, we exclude Tibet, where we have data on only one prefecture level city, and the province level municipalities because our set of province-time fixed effect would absorb all variation in GDP.

---

<sup>13</sup>A real GDP index of prefecture level city centers is available from the NBS for the period 2002-2010. For this period, cities with a SEZ had an average yearly inflation rate (as implied by the implicit deflator constructed from nominal and real GDP series) of 3.3%. Cities without a SEZ had an average yearly inflation rate of 4.0%. This suggests that reformer cities, if anything, have a lower inflation rate, but the difference is not statistically significant. The data on real GDP of the prefecture area (instead of the city center) dates back until 1996. When using this longer time series, then we find that cities with a SEZ had an average yearly inflation rate of 1.8%, while cities without a SEZ had an average of 2.3%. The difference is again not statistically significant.

<sup>14</sup>During the period of economic reforms, there was also a reform of the administrative levels and borders of cities. Our data is from the National Bureau of Statistics and provides a consistent definition across cities and time.

<sup>15</sup>For example, the distinction between urban core and periphery which we exploit to analyze spillover effects, could not be made for county level cities.

## 4 Empirical Strategy and Results

In this section, we estimate the effects of SEZ on cities' GDP. Our empirical strategy is a difference-in-difference estimator exploiting the variation in economic policy across cities and years following the establishment of SEZ. The main outcome (dependent) variable is GDP at the city level, which we measure in three alternative ways: first, from official statistics (this section), then using electricity consumption and light intensity as proxies of the level of economic activity (section 5).

Table 2 shows the summary statistics of our dependent variables and of the main control variables. We have over 5100 observations for GDP from an unbalanced panel of 276 cities from 1988 to 2010. Figure 1 shows the trend of GDP and the two most important control variables. All three variables show a clear upward trend, but real GDP grew much more than population and urban land area.<sup>16</sup> Our policy variable, the establishments of SEZ, is illustrated in Figure 2. This figure shows how the shares of cities with the different types of zones evolved over time, restricting attention to the balanced sample. The two most important types of zones are HIDZ and ETDZ with shares reaching 31% and 24% in 2010, respectively. Two types of zones existed already before the start of our sample: the first comprehensive SEZ were established in 1980 (not in our sample) and the first ETDZ were established in 1984 (and continue to be established until today). ETDZ and HIDZ are not only the most frequent zone types over all, but also among those with the most new establishments during the sample period 1988-2010. We will also consider Export Processing Zones (EPZ) and other less frequent types of zones (e.g., BZ and BECZ). Most of these other zones were introduced in cities that already hosted either ETDZ or HIDZ. We control for province-level zones, but we do not combine them with our state-level SEZ because they are less far-reaching in scope and more heterogeneous.

### 4.1 Baseline Specification

Our baseline specification is a city-level panel regression whose dependent variable is the log of GDP in current prices. The main explanatory variables are reform indicators switching on (i.e., taking and then retaining the unit value) in the year after part of a city's territory was granted the status of SEZ, and zero otherwise.<sup>17</sup> Note that cities may have multiple zones of different types. Since our goal is to assess the effect of different types of zones, in some specifications we shall allow each city to be subject to multiple treatments. All regressions control for city fixed effects and province-time interactions, which amounts to a difference-in-difference analysis. Standard errors are clustered at the city-level. More formally, we run regressions of the form

$$y_{ipt} = \phi_i + \gamma_{pt} + \alpha I\_Reform_{it} + X_{it}\beta + \varepsilon_{it}, \quad (1)$$

where  $y_{ipt}$  is log nominal GDP,  $\phi_i$  is a city fixed effect,  $\gamma_{pt}$  is a province-time fixed effect and  $I\_Reform_{it}$  is an indicator switching on, for each city, in the year after a SEZ is established.  $X_{it}$  is a vector of time-varying controls including log land area and log population.  $\varepsilon_{it}$  is a normal error term. City fixed effects absorb time-invariant heterogeneity in city characteristics like initial development or geographical location. Thus, the identification of the effects of reform is across city-time within each province. Province-time fixed effects control for time varying province-specific shocks that can play a confounding role. In particular, they absorb cross-province inflation differentials.

---

<sup>16</sup>The land area increases because the official urban centers of prefectures (i.e. the prefecture level cities) expand over time into surrounding counties.

<sup>17</sup>Including the year of the reform in the dummy does not alter the baseline results significantly.

The econometric specification in (1) restricts the treatment effect to a shift in the after-reform GDP level path, namely, reformed cities can undergo a GDP *level* change after the reform indicator switches on. This specification is clearly restrictive. One might expect reforms to have cumulative effects on developments, such as temporary or permanent changes in growth rates. For this reason, we will later explore more flexible econometric specifications allowing for trend breaks and distributed lags.

We start our analysis by aggregating all state-level reforms into a single indicator switching on after a city is granted for the first time the status of any state-level SEZ. We construct a similar single dummy for province-level reforms. The estimated coefficients are shown in Table 3. In column (1), we include no additional control variable except for the fixed effects. The coefficient of the "post-reform indicator for any state-level reform" is positive and highly significant. Becoming the host of a SEZ increases the average GDP level of the treated city by 19% in post-reform years. In contrast, the effect of province-level reforms is small and insignificant. In column (2) we control for the log of the city center area. This variable controls for changes in city borders, which are relatively frequent in China and change mechanically GDP, possibly in conjunction with the introduction of a SEZ. The size of a city's land area has, as expected, a positive effect on GDP and part of the treatment effect in column (1) appears to absorb the effect of changes in city borders. Yet, the treatment effect remains large (14.7%) and highly significant after this effect is filtered out. In column (3) we add the log population of the city center as a further control. Population has a strong positive correlation with GDP, while land area loses explanatory power. The treatment effect drops further but remains large (12.6%) and highly significant.<sup>18</sup> Finally, in column (4) we construct a measure of GDP per capita and use it as our outcome variable. The reform effect is in this case slightly smaller (11.7%), but remains highly significant. In columns (5)–(8) we repeat the analysis for the sub-sample of inland provinces. In this sub-sample the status of SEZ was granted mostly on the basis of administrative criteria, such as being a provincial capital.<sup>19</sup> This is an interesting sub-sample since it involves less severe a selection. To mitigate concerns with selection even further, we exclude from the inland sub-sample cities that were granted the status of SEZ without being provincial capitals. Thus, the restricted inland sample only contains provincial capitals (treatment group) and cities that were never granted the status of SEZ (control group). Columns (5)–(8) in Table 3 shows that the results are largely robust to this sample restriction.<sup>20</sup>

## 4.2 Pre-reform Trends

A concern with the results of Table 3 is that cities hosting SEZ might have already been on a high-growth trajectory – or were even selected precisely because of their promise of high success. The focus on inland capitals alleviates partially such concerns. However, the year in which capitals were assigned

---

<sup>18</sup>In addition, state-level SEZ may be associated with an increase in government transfers that may again mechanically increase GDP. Unfortunately, we have no information on transfers from the central government. In a robustness analysis, we control for the expenditure of the local government.

<sup>19</sup>In the sub-sample of inland cities, 44 cities were granted the status of SEZ. Of these, 18 were provincial capitals, which is the number of provinces we classified as inland.

<sup>20</sup>Although in this restricted sample the treated cities (capitals) were not selected individually, the group of inland capitals together are a special selection. But since the selection was based on the administrative criterion of being a capital city (and not on unknown and heterogeneous reasons), the way in which these cities differ from other cities is better observable. In particular, we can control for many of the features of capital cities such as infrastructure and education and allow the treatment effect to depend on these. In a separate regression (not reported) we include these interaction terms and find that the main effect remains large and significant. The only significant interaction term is with the number of universities, which shows a positive effect.

to the treatment group may not be random.

We address this concern by two strategies. First, we investigate whether the performance of treated cities was different from that of other cities in the same province in the years shortly pre-dating the reform. Table 4 is the analogue of Table 3, reporting the results of regressions where we add four pre-reform indicators taking on the unit value, respectively, in the year of reform and one, two and three years before the reform.<sup>21</sup> If cities were granted the status of SEZ due to their promising pre-reform trends, these coefficients ought to be positive and significant. In contrast, we find the estimated coefficient of the pre-reform dummies to be insignificant, and often negative. The treatment effect, instead continues to be positive and significant, except in columns (7) and (8), where it turns marginally insignificant. It is useful to note that the point estimates in the restricted sample (including those that are not statistically significant) are similar to those in the full sample, although estimated less precisely. In summary, the results of Table 4 are reassuring, and suggest that there were no important differences in pre-reform economic performance between treated cities and control groups.<sup>22</sup>

Second, we consider a more flexible specification allowing treated cities to have different time trends from the non-reformers. This addresses the potential concerns that, by not allowing any differential time trends, our baseline specification in equation 1 may attribute incorrectly pre-existing trend differences to the reform. The new specification allows reformed cities to have a linear time trend that makes them diverge from the control group already before the reform. In some specification, we allow this trend to be subject to a structural break at the time when a city is granted the status of SEZ. More formally, we consider the following specification:

$$y_{ipt} = \phi_i + \gamma_{tp} + \alpha_1 I\_Reform_{it} + \alpha_2 [(t - 1987) \times I\_Reformer_i] + \alpha_3 [\max\{0, (t - ReformYear_i) \times I\_Reform_{it}\}] + X_{it}\beta + \varepsilon_{it}, \quad (2)$$

where, as above,  $I\_Reform_{it}$  is an indicator switching on in the first year after the reform. Moreover,

- $I\_Reformer_i$  is a dummy identifying cities that were reformed at any time.  $t \geq 1988$  denotes the year of the observation. Therefore,  $\alpha_2$  captures the steepness of a linear trend specific to reformers, i.e., it says how many percentage points the growth rate differs between reformers and non-reformers.
- $ReformYear_i$  is the year in which the first SEZ was introduced in city  $i$  (if a city never became a SEZ, then we let  $ReformYear_i = 0$ ). The interaction  $[(t - ReformYear_i) \times I\_Reform_{it}]$  allows a differential trend (i.e., a trend break) starting as of the introduction of the first SEZ.<sup>23</sup> The coefficient  $\alpha_3$  measures the steepness of such a trend break.
- $\alpha_1$  captures a level shift as in the baseline specification of equation 1

<sup>21</sup>We also explored longer lags. There is evidence of some marginally significant effects at the five-year lag. However, lags longer than three are identified out of a significantly smaller set of reforming city (since many cities were granted the SEZ status in the early 1990's, and our sample only starts in 1988). For instance, in the full sample the first three lags are identified out of 73-to-75 cities, while the fifth lag would only be identified out of 27 cities. In the restricted sample, the first three lags are identified out of 18 cities, whereas the fifth lag would only be identified out of 3 cities.

<sup>22</sup>Note also that the earliest zones, which were also the most selected ones, are excluded from our sample (comprehensive SEZ established in 1980 and 1988) or do not have time-variation in the policy indicators during our sample period (ETDZ established in 1984).

<sup>23</sup>Note that interaction  $[(t - Reformyear_i) \times I\_Reform_{it}]$  equals zero in all periods for never-reforming cities.

The results for the full and restricted (inland) samples are shown in Table 5, columns (1)-(4) and (5)-(8), respectively. We build here on the specification of columns (3) and (7) in Table 3, including all control variables (whose estimated coefficients are not reported, for simplicity). The results are robust to the other specifications presented in Table 3. Columns (1) and (5) of Table 5 simply reproduce for convenience columns (3) and (7) in Table 3, respectively. In the regressions of columns (2) and (5) we add a linear trend specific to reformers. The estimated coefficient  $\hat{\alpha}_2$  ("time trend of reformers (state-level)") is statistically significant in both the full and the restricted sample. Interestingly, the coefficient  $\hat{\alpha}_1$  continues to be highly significant in the full sample, although much of the effect is now absorbed by the trend. However, it becomes marginally insignificant in the restricted sample. The trend in columns (2) and (5) does not distinguish pre- and post-reform periods. Thus, in columns (3) and (6) we allow a structural break in the trend of reformed cities, by including  $\max\{0, (t - ReformYear_i) \times I\_Reform_{it}\}$  in the regression. Interestingly, the estimated coefficient  $\hat{\alpha}_1$  remains almost unchanged in the full sample and increases slightly in the restricted sample. Moreover, the estimated coefficients of the pre- and post-reform trends,  $\hat{\alpha}_2$  and  $\hat{\alpha}_3$  are insignificant in both samples. Altogether, the statistical specification studied so far suggests that the baseline model with a GDP level shift performs better than one allowing for a trend break implying a permanent GDP divergence between treatment and control group.

The specification of columns (2)-(3) and (6)-(7) – allowing for permanent divergence – may be too extreme. We consider, then, an alternative specification allowing SEZ to have a non-linear (cumulative) effect of the SEZ, i.e., to trigger a concave depart from the pre-reform trend. To avoid an over parametrization, we omit the level shift, and we estimate the following alternative econometric specification:<sup>24</sup>

$$\begin{aligned}
 y_{ipt} &= \phi_i + \gamma_{tp} + \alpha_2 [(t - 1987) \times I\_Reformer_i] & (3) \\
 &+ \alpha_3 [\max\{0, (t - ReformYear_i) \times I\_Reform_{it}\}] \\
 &+ \alpha_4 [\max\{0, (t - ReformYear_i) \times I\_Reform_{it}\}]^2 + X_{it}\beta + \varepsilon_{it}.
 \end{aligned}$$

Clearly, if  $\alpha_3 > 0$  and  $\alpha_4 < 0$ , then, SEZ would cause a fast acceleration of growth in the years immediately after the reform, but the acceleration would die off in subsequent years. The regression results from this specification are provided in columns (4) and (8). In both cases, the coefficients  $\hat{\alpha}_3$  and  $\hat{\alpha}_4$  have the expected signs (positive and negative, respectively), and are (individually and jointly) statistically significant. Interestingly, in the full sample there continues to be some evidence of a positive pre-reform differential trend for reformers. In the restricted sample of inner cities, in contrast, we find no such evidence (the estimated coefficients  $\hat{\alpha}_2$  turns negative and totally insignificant). This suggests that the government might have picked winners, to some extent, in the full sample, but not in the restricted inland sample where the status of SEZ was granted according to a strict administrative criterion. In both the full and restricted cases there is evidence of a significant effect of SEZ taking the form of a gradual increase in the GDP level (i.e., a *concave* deviation of the treated cities over time after reforms), rather than in a permanent increase in growth (i.e., a *linear* trend break of the treated cities after reforms).

---

<sup>24</sup>It would be possible to include also the term  $\alpha_1 I\_Reform_{it}$  to this specification. However, not surprisingly, it becomes very difficult to identify separately all the effects in such a highly parametrized model. Therefore, we omit this term, and regard the current specification as a non-nested alternative to equation 2.

### 4.3 Reform Effects Over Time

The analysis of the previous section suggests that post-reform dynamics are non-linear. In this section, we perform a non-parametric analysis of the effects of the reform. To this aim, we estimate a model placing no functional form restrictions on post- (and pre-) reform effects, each captured by a separate lag-specific dummy. More formally, we run the following regression:

$$y_{ipt} = \phi_i + \gamma_{t,p} + \sum_{n=-J_B}^{J_F} \alpha_n I_{it}^n \{ (t - Reformyear_i) = n \} + X_{it}\beta + \varepsilon_{it},$$

where positive values of  $t - Reformyear_i$  measure how long ago (i.e. how many years before the current year  $t$ ) the reform took place in city  $i$ . Negative values measure in how many years city  $i$  will be reformed when the current year is  $t$ . The maximum number of post-reform leads,  $J_F$ , is 26.<sup>25</sup> We construct these indicators also for the year of reform and the three years prior to the reform (i.e.  $J_B = 3$ ), which allows us to test whether reforming cities already had a significantly different performance prior to the establishment of the first zone.<sup>26</sup> The omitted categories (for which all indicators are zero) are never-reforming cities and cities more than three years before the reform. The controls include land area, population, and the usual set of fixed effects.

The results are displayed in Figure 3, showing the effect of the treatment  $n$  years from the reform (for instance,  $n=10$  measures the effect of being ten years past the introduction of a SEZ). This non-parametric specification confirms the results of the previous section. In particular, it shows a break in the GDP path a year after the reform, followed by a temporarily higher growth rate that levels off after ca. ten years.<sup>27</sup> The size of the effects are comparable to those in the previous section.<sup>28</sup> There is only some marginal, statistically insignificant evidence of a higher GDP growth in the three years before the reform, indicating some minor positive selection. Note that the standard errors increase after nineteen years after the establishment of the zone. This is due to a significant drop in the number of observations, since many cities were reformed in 1991 and 1992.<sup>29</sup>

We estimate the same regression for the restricted sample of inland provinces (excluding cities which had a reform but are not provincial capitals), see Figure 4. The qualitative pattern and the point estimates are similar, although the estimation is less precise, and only the effects for the lags 9-12 are statistically different from zero.<sup>30</sup>

### 4.4 Different Types of SEZ

In this section, we attempt to disentangle the effect of different types of SEZ which had distinct policies. To this aim, we create separate post-reform indicators for each of the three most important (and most

<sup>25</sup>This is a single city which is observed in 2010 and had its first reform in 1984.

<sup>26</sup>For the same reasons described in the discussion of Table 4, we do not include more pre-reform indicators. When we include also indicators for four and five years prior to the reform, then these indicators are marginally significant, but identified by only 27 observations.

<sup>27</sup>Note that our variation is within province-years. Therefore, a 20% higher level of GDP than before the reform should be interpreted relative to provincial averages over the years, which are already increasing.

<sup>28</sup>The average over the yearly estimates (weighted with the number of observations identifying each estimate) of all post-reform indicators is 17.1%, which is even somewhat higher than the result in the simple regression in Table 4.

<sup>29</sup>When the cities reformed in 1991 and 1992 reach the year 2010, the subsequent number of cities that identify the individual coefficients drops from 54 to 9. The vertical dashed line in the figure marks this drop.

<sup>30</sup>The reforms in the inland provinces started almost a decade later than in the coastal provinces. The post-reform effects are therefore estimated for a shorter period and based on fewer observations.

common) SEZ: ETDZ, HIDZ and EPZ. In addition, we create a single dummy for other types of state-level SEZ. Table 6 has the same structure as Table 3 but replaces the indicator for "any" state-level zone with the four separate indicators for each type of state-level SEZ. ETDZ and HIDZ appear to have a large effect. In the full sample, the effects of these two types of zones are quantitatively similar to the one of the first zone in Table 3. In the inland sample, there are two deviations. First, the point estimate of ETDZ remains positive but becomes insignificant when the dependent variable is GDP per capita. Second, the OtherTypes have in two cases a higher estimate than ETDZ and HIDZ, although these results are driven by very few observations.<sup>31</sup> EPZ are insignificant throughout, although the coefficient is positive in seven out of eight cases. Overall, the disaggregation highlights the relative importance of the ETDZ and HIDZ, which are the two largest and most comprehensive types of zones in our sample, as well as those emphasizing most explicitly technology development aspects.

The regressions with simple post-reform indicators for the different types of zones is restrictive in the sense that it assumes a jump in the GDP level after the reform. Since we have seen that the effects of "any" zone built up gradually during about ten years and then leveled off, we investigated whether the same pattern holds true for the individual types of zones. Since the pre- and post-reform effects of different types of zones often overlap (treated cities often had multiple zones of different kinds), the non-parametric approach of section 4.3 is quite demanding. Nevertheless, the resulting picture is reasonably clear. Figure 5 shows that the pattern for ETDZ looks remarkably similar to that of Figure 3 (first zone reformed).<sup>32</sup> Figure 6 shows that HIDZ also display a concave pattern, although the effect appears to decline after lag 10.<sup>33</sup> EPZ and OtherTypes show a more mixed picture (see Figures 7 - 8).<sup>34</sup> The standard errors are very large and the effects are typically imprecisely estimated and statistically insignificant. In summary, most development effects appear to stem from ETDZ and HIDZ.

## 4.5 Decomposition of the Effects of SEZ

In the previous sections, we have documented that the establishment of SEZs has had a positive effect on GDP at the city level. In this section, we investigate the channel through which the industrial policy promotes growth. To this aim, we decompose GDP into the four inputs of an aggregate production function: physical capital, size of the labor force, average labor efficiency and TFP. We then estimate how the establishment of a SEZ affects each of the three components.

More formally, we perform a level-decomposition of GDP using a neoclassical production function:

$$\begin{aligned} \log rGDP_{ipt} &= \log TFP_{ipt} + \alpha_k \log PC_{ipt} \\ &\quad + (1 - \alpha_k) \log EL_{ipt} \\ &= \log TFP_{ipt} + \alpha_k \log PC_{ipt} \\ &\quad + (1 - \alpha_k)(\log Pop_{ipt} + \log LabQua_{ipt}), \end{aligned}$$

where TFP denotes total factor productivity, PC the physical capital stock, and EL the number of

---

<sup>31</sup>14 cities have a zone type other than ETDZ, HIDZ, or EPZ, but in 11 of these the zone this is in conjunction with an ETDZ or HIDZ.

<sup>32</sup>This result is not completely surprising, since ETDZ were typically established before other types of zones. Yet, it is interesting that the results are robust to controlling for separate effects of other types of zones.

<sup>33</sup>There is a sharp (statistically insignificant) drop in the last lag (19). This may be due to the changing sample size, as the number of cities identifying this last coefficient drops discontinuously by more than half in this period.

<sup>34</sup>The stark drop in OtherTypes is identified by only one observation. EPZ were established after 2000 and often inside an existing zone. Furthermore, the EPZ may have gained importance after the WTO accession in 2001, which could explain their upward trend (though insignificant).

effective units of labor. In turn, EL can be decomposed into the number of workers – proxied by the local population size – and labor quality, proxied by the average educational attainment of the population following the methodology proposed in Hall and Jones (1999). The details can be found in the Appendix.

In Table 7, we display the results of baseline difference-in-difference regressions analogous to those performed in section 4.1, where each of the components of the production is used sequentially as the dependent variable. Column (1) shows that the establishment of a SEZ has a significantly positive effect on the physical capital stock. Becoming the host of a state-level SEZ is associated with a 13.5% increase in the city’s physical capital relative to non-reformed cities in the same province. Column (2) shows that the effect is even more pronounced in the inland sample – where the average treatment effect on physical capital is 22.7%. The estimated effect on population is 4.2%, significant at the 10% in the whole sample (column 3). However, there is no significant effect in the inland sample (column 4). The effect of SEZ on average labor quality is generally insignificant – see columns (5) and (6). There is some positive effect on total factor productivity in the whole sample – see column (7) – showing that TFP increases by 4% after a city receives the SEZ status. This effect is only significant at the 10% level. Moreover, column (8) shows that in the inland sample the policy has hardly any effect on TFP. Taking these results at face value suggests that the establishment of SEZs has a major positive effect on investments, and some effects on internal migration and total factor productivity. In the inland sample, the only significant effect is that on investments. There is no selective migration effect, i.e., the SEZs do not seem to attract better educated workers.<sup>35 36</sup>

An important *caveat* is that the quality of the data on human capital data is very low. The only data available at the city-level is the *population census*, which are ten years apart from each other. During our sample period, three *population census* took place, in the years 1990, 2000 and 2010. Between the census years, we must resort to an interpolation. This limits the accuracy of our measures of human capital and TFP.

The results are partially different if one evaluates the effects of SEZs on human capital by using only the three years for which direct observation of the educational attainment from the census data are available. Rather than aggregating the existing information to obtain an estimate of the average years of school, we study the effect of SEZ on the share of each educational attainment level for which data are available. This yields a better sense of the impact of the policy on the distribution of human capital in the population. The results are show in Table 8. Columns (1) and (2) suggest that after changing status, the average years of schooling in the city increases by 0.17 years in the full sample and 0.23 years in the restricted (inland) sample.<sup>37</sup> Columns (3) and (4) show that establishing a SEZ has no impact on the the share of population over the age of 6 with a low educational attainment (elementary degree or less). Second, SEZs appear to decrease significantly the share of population with junior and senior high school degrees (columns 3-4). Finally, the share of college graduate in the whole population increases significantly by 2.4% and 3.2% respectively in the full and inland sample (columns 9 and 10). In summary, the main finding is that the establishment of SEZs is associated with an increase in

---

<sup>35</sup>Notice here that we trimmed the sample by top and bottom 1% in terms of annual growth rate. The result is very similar to the full sample. The only difference is for column (7), where in the untrimmed sample, the estimated effect on TFP is 4.2% and marginally significant on 10% level.

<sup>36</sup>Schminke and Van Biesebroeck (2013) also find that after controlling for selection bias, the firms that are located in the zones do not achieve significantly higher TFP than firms outside of the zones.

<sup>37</sup>Ideally, we would want to compute the educational attainment of the working population (age 25-64). Unfortunately, we are unable to do so because the population census only reports educational attainment for the population over the age of 6.

the share of college graduates, at the expense of the middle education.<sup>38</sup> This may be due either to selective immigration (i.e., cities with a SEZ attracting more highly educated immigrants) or to stronger incentives for locals to take higher education.

Figures 9, 10, and 11 display the reform effect on physical capital stock, population and TFP over time. The specification employed here is the analogue to that of Figure 3. The pattern for physical capital is similar to that of GDP in Figure 3. There is a structural break in the path of physical capital at the time of the reform. The effects build up over time, and becomes statistically significant 5 years after the reform. After 19 years, as usual, the effects are estimated very imprecisely. Population shows a similarly concave path after reform, although the reform effects become significant only 12 years after the reform. TFP in Figure 11 is always insignificant.

---

<sup>38</sup>In an alternative specification, we bundle senior high school together with college graduates while leaving junior high school graduates as the middle level category and find a similar result: the share of junior high school decreases significantly and the share of senior high school and college graduate increases significantly.

## 5 Robustness

In the previous section we have focused on the effects of SEZ on GDP at the city level. In this section we first test whether the establishment of a SEZ also affected other neighboring locations (negative or positive spillover effects). We then discuss the robustness of our results to the use of alternative proxies for GDP (consumption of electricity and light measured by satellites) and to the inclusion of additional control variables and years. Finally, we compare our results to a placebo study with randomly assigned pseudo-reforms.

### 5.1 Local Spillovers

We have focused so far on the main urban center of each prefecture, because this is where all state level SEZ in our sample were established. In this section, we investigate the existence of local spillovers by studying whether the policy has had any effect on the area surrounding the main urban center. To this aim, we re-run our baseline regressions of section 4.1 using as the dependent variables, first, the GDP of the entire prefecture, a superset of the city center (Panel A of Table 9); and then the GDP of the prefecture’s periphery, i.e., the whole prefecture excluding the city center (Panel B of Table 9).<sup>39</sup> Panel A shows that the effects at the prefecture level are of comparable magnitude as those obtained focusing only on the city center. Panel B shows that the results hold up when we consider only the periphery.<sup>40</sup>

The results of this section suggest that the positive effects of the SEZ were not at the expenses of surrounding areas. To the positive, it looks as if they exercised positive spillovers on the periphery of the prefecture of which a city is part.

### 5.2 Alternative Measures for GDP

Chinese price-level data are generally regarded as unreliable, especially at the local level. Our empirical methodology has the advantage that it does not rely on any price deflator. Differences in price levels are filtered out by city fixed effects, whereas province-time fixed effects filter out cross-province inflation differentials. Yet, a remaining concern is about within-province inflation differentials. In particular, our treatment effects would be subject to an upward bias if the establishment of a SEZ systematically brought about higher inflation in the treated cities. The fact that the existing price data do not suggest that the establishment of a SEZ is associated with higher inflation is reassuring, although only partially so, precisely because the local deflators are not reliable. Another concern is that the local authorities could over-report the nominal GDP of treated cities, in order to meet the expectation of the central government regarding the performance of SEZ (see Young, 2003, for more discussion of such possible biases in Chinese data).

To address these concerns, in this section we use electricity consumption and light intensity measured by weather satellites as two alternative proxies for GDP. These measures are neither likely to be subject to manipulation nor affected by prices.

---

<sup>39</sup>The main urban center of each prefecture (or prefecture-level city) is the urban center of the prefecture. See the discussion on sample selection for further details about the administrative hierarchy. On average, the GDP of the whole prefecture area (including the center) is about twice as large as the one of the city center only. The size of the population of the whole prefecture is about four times as large as the one of the city center and the size of the land area is about eight times as large.

<sup>40</sup>Land area and population are adjusted accordingly. There is a small drop in the number of observations, since in some cities the city coincides with the whole prefecture, and thus there is no periphery.

### 5.2.1 Electricity Consumption

Electricity consumption is broadly regarded as a good approximation for GDP. Indeed, studies questioning the validity of China’s official GDP refer to the a large discrepancy between the official GDP growth rate and electricity consumption in 1998, when the Asian currency crisis hit the world economy (see, e.g., Rawski, 2001).<sup>41</sup> Electricity consumption by households and firms is reported in the same statistical yearbooks as GDP, and is available at the city level. In column (1) of Table 10 we re-run our baseline regression using the logarithm of electricity consumption as the dependent variable. The result shows that the establishment of a SEZ leads to an 11% increase in electricity consumption, a similar magnitude as we observed for GDP. However, somewhat surprisingly, the effect vanishes in the inland sample. This finding is difficult to interpret. It could be due to differences in industrial structure. There is some uncertainty regarding how this effect should be interpreted in terms of GDP. The raw elasticity of GDP with respect to electricity consumption in our sample is 0.91, such that the estimated effect would translate into a 10% increase in the level of GDP.<sup>42</sup>

### 5.2.2 Light Intensity

A number of recent papers have argued that light intensity at night measured by weather satellites can be used as a proxy for GDP.<sup>43</sup> Most economic activities such as production, transport, and consumption produce light as a byproduct. Therefore, light intensity is positively correlated with the intensity of local economic activities.<sup>44</sup> We calculate the average light intensity within the geographical boundaries of prefecture-level cities and use this as a proxy for economic activity. In column (3) of Table 10 we re-run our baseline regression with the logarithm of the average light intensity as the dependent variable.<sup>45</sup> Unfortunately, light intensity is only available since 1992 and only one third of the first zone establishments took place after that year. Since our empirical strategy relies on within-city variation in SEZ establishments, the post-1992 sample has significantly less variation in the policy measures. The estimated coefficient implies a 5.2% higher level of light intensity after the establishment of a SEZ.<sup>46</sup> The point estimate for the inland sample is similar but statistically insignificant.

---

<sup>41</sup>Our calculations confirm that the growth rate of electricity consumption dipped around 1998 for both reformer and non-reformer cities.

<sup>42</sup>Previous studies with different data and estimation techniques have found other estimates. For example, Henderson *et al.* (2012) analyze a panel of 128 countries and find, controlling for time and country fixed effects, an elasticity of GDP with respect to electricity of 0.28. However, it is unclear if this within-country estimate would be appropriate to predict GDP for our sample.

<sup>43</sup>Elvidge *et al.* (1997) were among the first to discuss the relationship between light and economic activity. See also Henderson *et al.* (2012) and Chen and Nordhaus (2011) and the literature cited there on the use of light to measure economic activity. Ma *et al.* (2012) discusses the use of light data in Chinese prefecture-level cities.

<sup>44</sup>Since most of man-made light is produced with electricity, the two proxies, electricity consumption and light intensity, are closely related. Compared with electricity consumption, the advantages of light data are that it comes from a different source (satellites of the US Air Force) and is available at a higher resolution (1 km<sup>2</sup>). The disadvantages are that it is available only since 1992 after which we have considerably less new SEZ establishments and the measure is censored at the top of the scale. The appendix provides further details about the light data.

<sup>45</sup>We do not control for the size of the land area in the regressions in columns (3) and (4) because light is measured within the city boundaries of 2010. Therefore, unlike for the official GDP data, the area on which we measure economic activity is held constant over the years.

<sup>46</sup>When we run the baseline regression with GDP as the dependent variable for the post-1992 period, then the point estimate is 0.042 but insignificant.

### 5.3 Controlling for Government Spending

The establishment of a SEZ is likely to have been associated with a number of policy changes from the central and local governments. Most notably, the central or the provincial governments may have increased the transfers to cities when these were granted the SEZ status. Unfortunately, we have no direct information on such transfers. SEZs may have also triggered government investments in infrastructure. The effects identified in the previous sections are gross of such investments. On the one hand, the infrastructure investments are part of the government’s strategy to facilitate economic development and are therefore part of the treatment. On the other hand, one may be interested in estimating the net effects after controlling for changes in public investments.

While we have no information on public investments at the city level, we do observe the overall expenditures of the local government for a subset of the years in our sample. This measure can be used as a proxy of the contribution of public investments to GDP. The disadvantage of including the local government expenditure is twofold. First, we lose some observations. Second, causation could run in the opposite direction: government expenditure might have increased because the GDP expansion caused by the SEZ increased the tax revenue accruing to the local authorities.

Table 11 shows that the reform effects are robust to the inclusion of government expenditure among the control variables. The effect of the reform remains positive and highly significant in both samples, being in fact larger than the point estimates in Table 3.

### 5.4 Earlier GDP Data

We have focused our analysis on the period 1988-2010 for which the NBS provides a consistent measurement across cities and years. Furthermore, we can control for potential confounding factors such as changes in the boundaries of the cities and changes in their administrative status. This conservative approach makes us confident that our results are not driven by other factors besides SEZ, but it comes at the cost of losing variation in reforms because some SEZ were established before 1988. In this section, we estimate our baseline specification for a subset of cities for which we observe GDP also for earlier years.<sup>47</sup> In this case, we cannot control for changes in land area, government spending and population as this data is missing for the earlier years. The resulting reform effect for this subsample is a 12.3% increase in the level of GDP, and the estimated coefficient is highly significant.

### 5.5 Placebo Analysis

Our estimation exploits the time and spatial variation in the establishment of SEZs. Since the establishment of the SEZ is staggered, but clustered in few years, one might be concerned with the extent to which the exact timing of the reform matters for the identification of the reform effect. Furthermore, we would like to rule out that our reform indicators pick up shocks unrelated to SEZ that could also be present in other cities. In order to deal with these concerns, we run three placebo exercises based on the specification in column 3 of Table 3 but assign reform years randomly.

In a first exercise, we assign the actual number of new zone establishments in each year to a random selection of cities. The resulting placebo distribution is the same as the true distribution over time, but SEZs are artificially assigned to random cities. We repeat this exercise 1000 times. We find that in no

---

<sup>47</sup>Namely the cities in the following provinces: Fujian, Guizhou, Hebei, Heilongjiang, Henan, Inner Mongolia, Jiangsu, Shaanxi, Shandong and Shanxi.

case are the absolute t values and the R-squared of the placebo regressions larger than those of the true reform.<sup>48</sup> This suggests that the spatial distribution of SEZs indeed drive our result.

In a second more demanding placebo test, we assign the random reforms only to reformers, again holding the distribution of reforms across years constant. However, the timing of the treatment is scrambled across cities. This allows us to assess to what extent the exact timing of the reform matters, because we are only randomizing the year of the reform but not the treated city. We find that the absolute t values of the placebo regressions are higher than the one of the true reform in only 1.8% of the cases.<sup>49</sup> This indicates that the actual year in which the SEZ were implemented is crucial for our results, and supports our identification strategy based on within-city variation.

Finally, we use the random assignment of reforms from above and include the true reform year and the placebo reform year in the same regression.<sup>50</sup> While the estimate for the true reform is always significant at 5%, the placebo reforms are significant in only 24% of the cases.<sup>51</sup>

Overall, these placebo exercises make us confident that our empirical strategy identifies the reform effect. Both the spatial and time variation of the SEZs drive our result.

---

<sup>48</sup>The mean estimate of the placebo reform is -0.0004 and it is never significant and higher than the one of the true reform.

<sup>49</sup>The mean estimate of the placebo reform is 0.088 and in 5% of the draws it is significant and higher than the one of the true reform.

<sup>50</sup>The assignment of random reform years among reformers implies that a placebo reform year is likely to coincide with the true reform year. This is the case in 36% of the 1000 draws.

<sup>51</sup>The mean estimate of the true reform is 0.11 and the mean estimate of the placebo reform is 0.046.

## 6 Conclusion

China experienced an astonishing growth performance during the past 30 years. The Special Economic Zones are a building block of the development strategy pursued by its government. According to Naughton (2007): "Bold, fragmented, open to outside investment, but with a strong role for government: Special Economic Zones typify much of the Chinese transition process" (p. 410). This paper provides estimates for the impact of Special Economic Zones on local economic performance using alternative measures of economic activity. We considered a number of specifications that control for unobserved heterogeneity at the city level and at the province-time level. The evidence using the GDP from official statistics suggests that the establishment of SEZs has yielded large positive effects for the cities in which these were located. Although our estimates are smaller than those found by the earlier literature based on cross-sectional growth regressions (typically on a smaller set of cities and years), the effects are large and robust. We also find that the effect of the SEZs on output worked mainly through the acceleration of physical capital investment. Electricity consumption and light intensity measured by satellites also show positive effects of the SEZ establishments.

What can we learn from the Chinese experience about the role of economic reform and industrial policy during the process of development? Existing theoretical and empirical work suggests that the types of policies that backward countries should follow differ from those of advanced countries. Acemoglu, Aghion, and Zilibotti (2006) show that in economies at early stages of development, there is scope for the government to support investment-oriented activities. The types of policies that the Chinese government tested inside the SEZ, and partly implemented later in the rest of the economy, did envisage an active role for the government. Rodrik (2006) argues that this active role of the government was crucial for the Chinese development because it supported a fast move towards more modern and productive sectors which have positive externalities on the whole economy. We contribute to this literature by providing evidence that the active policies indeed had strong positive effects on GDP and therefore can be beneficial for a country like China at an early stage of development. At the same time, our results also point to the limits of differential policy treatments as there is no evidence that GDP growth or TFP in a preferentially treated city is permanently higher.

## 7 References

- D. Acemoglu, P. Aghion, and F. Zilibotti. Distance to frontier, selection, and economic growth. *Journal of the European Economic Association* 4.1: 37-74, 2006.
- P. Aghion, R. Burgess, S. Redding, and F. Zilibotti. The unequal effects of liberalization: Evidence from dismantling the License Raj in India. *American Economic Review* 98.4: 1397-1412, 2008.
- G. Akinci and J. Crittle. Special economic zones: Performance, lessons learned, and implications for zone development. The World Bank, Washington DC, 2008.
- L. Brandt and T. G. Rawski. *China's Great Economic Transformation*. Cambridge University Press, 2008.
- M. Busso, J. Gregory, and P. Kline. Assessing the incidence and efficiency of a prominent place based policy. *American Economic Review*, 103(2): 897-947, 2013
- F. Caselli. Accounting for cross-country income differences. *Handbook of economic growth*, Jan 2005.
- X. Chen and W. D. Nordhaus. Using luminosity data as a proxy for economic statistics. *Proceedings of the National Academy of Sciences*, 108(21):8589-8594, 2011.
- L. K. Cheng and Y. K. Kwan. What are the determinants of the location of foreign direct investment? The Chinese experience. *Journal of International Economics*, 51: 379-400, 2000.
- S. Démurger, J. Sachs, W. Woo, S. Bao, and G. Chang. The relative contributions of location and preferential policies in china's regional development: Being in the right place and having the right incentives. *China Economic Review*, 13(4): 444-465, 2002.
- M. Dewatripont and G. Roland. The Design of Reform Packages under Uncertainty. *American Economic Review*, 85(5): 1207-23, 1995.
- X.-Y. Dong and L. Putterman. Soft budget constraints, social burdens, and labor redundancy in china's state industry. *Journal of Comparative Economics*, March 2003.
- C. D. Elvidge, K. E. Baugh, E. A. Kihn, H. W. Kroehl, E. R. Davis, and C. W. Davis. Relation between satellite observed visible-near infrared emissions, population, economic activity and electric power consumption. *International Journal of Remote Sensing*, 18:6, 1373-1379, 1997.
- C. D. Elvidge, D. Ziskin, K. E. Baugh, B. T. Tuttle, T. Ghosh, D. W. Pack, E. H. Erwin, and M. Zhizhin. A fifteen year record of global natural gas flaring derived from satellite data. *Energies*, 2(3):595-622, 2009.
- J. Fewsmith. *China since Tiananmen: The Politics of Transition*. Cambridge University Press, 2001.
- K. Head and J. Ries. Inter-city competition for foreign investment: static and dynamic effects of China's incentive areas. *Journal of Urban Economics* 40.1: 38-60, 1996.
- J. V. Henderson, A. Storeygard, and D. N. Weil. Measuring economic growth from outer space. *American Economic Review*, 102(2):994-1028, 2012.
- D. Jones, C. Li, and A. Owen. Growth and regional inequality in china during the reform era. *China Economic Review*, 14(2):186-200, 2003.

- K. Li, L. Yun, and G. Lui. Economic performance of human capital in post-reform china. *Chinese Economy*, January 2009.
- T. Ma, C. Zhou, T. Pei, S. Haynie, and J. Fan. Quantitative estimation of urbanization dynamics using time series of dmsp/ols nighttime light data: A comparative case study from china's cities. *Remote Sensing of Environment*, 124(0):99-107, 2012.
- Ministry of Commerce. Report on the development of state-level economic and technological development zones (in chinese). March 2006.
- B. Naughton. *The Chinese Economy: Transitions and Growth*. The MIT Press, 2007.
- T. Orlik. Understanding china's economic indicators: Translating the data into investment opportunities. FT Press, July 2011.
- D. Perkins. Reforming china's economic system. *Journal of Economic Literature*, 26(2):601-645, 1988.
- T. G. Rawski. What is happening to china's gdp statistics? *China Economic Review*, 2001.
- D. Rodrik. Industrial policy for the twenty-first century. mimeo, 2004.
- D. Rodrik. What's so special about China's exports?. *China & World Economy* 14(5): 1-19, 2006.
- A. Schminke and J. Van Biesebroeck. Using export market performance to evaluate regional preferential policies in China. *Review of World Economics*, 1-25, 2011.
- State Administration of Taxation. Notice of investigation of preferential tax policies in development zones. 2004.
- State Development and Reform Commission. The directory of development zones (in Chinese). Government Documents, 2006.
- E. F. Vogel. *Deng Xiaoping and the Transformation of China*. Belknap Press of Harvard University Press, 2011.
- J. Wang. The economic impact of special economic zones: Evidence from chinese municipalities. *Journal of Development Economics*, 101: 133-147, 2013.
- WEFore. Report: analysis of the risks of development zones (in chinese). Company Report, 2010.
- S.-J. Wei. The open door policy and china's rapid growth: Evidence from city-level data. NBER Working Paper, 1993.
- H. X. Wu. Accounting for china's growth in 1952-2008: China's growth performance debate revisited with a newly constructed dataset. RIETI Discussion Paper Series, 2011.
- C. Xu. The fundamental institutions of china's reforms and development. *Journal of Economic Literature*, 49(4):1076-1151, 2011.
- Y. Yeung, J. Lee, and G. Kee. China's special economic zones at 30. *Eurasian Geography and Economics*, 2009.
- A. Young. Gold into base metals: Productivity growth in the people's republic of china during the reform period. *The Journal of Political Economy*, 111(6): 1220-1261, 2003.

D. Zeng. How do special economic zones and industrial clusters drive china's rapid development. In D. Zeng, editor, Building Engines for Growth and Competitiveness in China: Experiences with Special Economic Zones and Industrial Clusters. The World Bank, 2010.

## 8 Appendix

### 8.1 Data Sources

**Official Statistics: City-Level** The main source for the official city statistics are the *China City Statistical Yearbooks*, which cover all prefecture- and county-level cities from 1988 to 2010. Most of the city-level statistics, including nominal GDP, electricity consumption, population, government spending and land area, are taken from this data set. As complementary sources to this data set, we include three other city-level statistical collections. First, we take the GDP data for the years 1992 and 1993 from *New China City in 50 Years Statistical Collection*, since these years are missing in *China City Statistical Yearbooks*. Second, for a subset of cities, we collect GDP and investment data for the period of 1978-1988 from *New China in 60 Year Provincial Statistical Collection*. Third, we obtain additional population and educational attainment data from the *China Population Census* (for the years 1990, 2000, and 2010).

**Official Statistics: Province-Level** The main source for province-level statistics is the *New China in 60 Years Statistical Collection*. We obtain the province-level price indexes, including the GDP and investment deflator, from this data set.

**Light and Digital Maps** Light intensity at night, an alternative measure for local economic activities, is provided by the National Geographical Data Center for the period of 1992-2010. Using the digital maps of China, we aggregate the light intensity at the level of cities.<sup>52</sup>

**Establishment of SEZ** The information on the establishment of the various zones is taken from three sources. The major source is the website of the Ministry of Commerce.<sup>53</sup> We also use the Information Site of China's Development Zones<sup>54</sup> and the Report of the Ministry of Commerce (2006). From these sources, we can derive the year in which the zone was established, its type, and its location.

### 8.2 Sample Selection

In our main estimations, we focus on a sample of prefecture-level cities for the years 1988-2010. The sample is unbalanced because of the creation of new cities: in the year 1988 the sample has 170 prefecture-level cities and this number increases to 276 in the year 2010.<sup>55</sup> Our sample covers all provinces in China except for Tibet, Hainan and the province-level cities Beijing, Shanghai, Tianjin, and Chongqing. We also exclude the cities of the first wave of comprehensive SEZ.

We discuss below in detail our sample selection criteria. Specifically, we provide reasons for three key choices, 1) time period, 2) prefecture-level cities, and 3) city core and city area.

**Sample Period** The GDP data in *China City Statistical Yearbooks* only go back to the year 1988.<sup>56</sup> Although pre-1988 GDP data for a subset of cities are available from other data sources, we do not

---

<sup>52</sup>The digital maps for several levels of administrative units of the People's Republic of China from 1992 to 2000 were obtained from the Asian Spatial Information and Analysis Network (ACASIAN), where they were produced by Dr. L. W. Crissman.

<sup>53</sup><http://english.mofcom.gov.cn>.

<sup>54</sup><http://www.cdz.cn/www/index.asp>.

<sup>55</sup>See Table 1 in Chung and Lam (2004) for a more detailed assessment of the increase in the number of cities in China.

<sup>56</sup>The earliest city statistical yearbook goes back to 1984. However, the yearbook only starts to report city-level GDP after 1988.

combine them with the *China City Statistical Yearbooks* data in the main empirical studies, due to their inconsistent definition of cities. The inconsistency is a result of the transformation of the administrative structure of local governments, especially at the city-level, during the past 30 years. Before 1983, the administrative structure consisted of four layers. From the top to the bottom, these are province, municipality, county and village.<sup>57</sup> Starting from 1983, the municipalities were gradually transformed into prefecture-level cities. Broadly speaking, the prefecture-level cities replaced municipalities as the third layer in the administrative structure. However, the transformation often coincided with various other changes, which we lack the data to control for. For example, a county which was part of the previous municipality may not be part of the prefecture-level city that is succeeding it. At the same time, new counties which were previously under the jurisdiction of a different municipality may become part of the prefecture-level city. Therefore, the composition of newly formed cities may differ substantially from that of the preceding municipalities.

Most city-level statistical collections fail to distinguish between municipalities and prefecture-level cities. It is therefore impossible to identify the break-point when the transformation was made using just the time-series of a prefecture-level city. The *China City Statistical Yearbooks* are an exception. A prefecture-level city only starts to appear in the *China City Statistical Yearbooks* after it finishes the transformation. The *China City Statistical Yearbooks* therefore give us a consistent sample of prefecture-level cities over the period 1988-2010.

**Prefecture-Level and County-Level Cities** A prefecture-level city is in the new administrative system a level between provinces and counties. A prefecture-level city (or just "prefecture") consists of the urban core (the actual city) and potentially several surrounding counties. As part of the transformation of administrative structures, some counties were promoted into county-level cities after the population exceed a certain threshold. After promotion, they remain at the same administration level as counties, which is one layer below prefecture-level cities. In fact, they were still under the administration of the original prefecture-level government. At the same time, a number of county-level cities were promoted to new prefecture-level cities and thereby cut out of their previous prefecture.

The *China City Statistical Yearbooks* contain statistics for both prefecture-level and county-level cities. To have a consistent definition of cities, we drop the cities that were county-level throughout the sample period. For those cities which were promoted to prefecture-level, we keep only the years after the promotion to prefecture-level city.

**Prefecture City Core and Area** *China City Statistical Yearbooks* report statistics of both city core and city area of prefecture-level cities. City core corresponds to the traditional definition of the urban center, which often consists of several urban districts. The prefecture area covers the whole geographic area of the prefecture, which includes the core of the prefecture-level city and the surrounding counties and county-level cities.

The distinction between the two statistical areas and its implication for the estimation result deserves discussions here. First, city cores are usually more industrialized than the whole area. Second, most of the SEZ are located in the suburban areas of the city core (Zeng, 2011).<sup>58</sup> Therefore, by focusing on the "city core", we get a more direct estimation of the effects of SEZ, but ignore possible interactions

---

<sup>57</sup>As specified in the 1982 constitution, the structure should only consist of three layers: province, county and village. In reality, however, due to practical reasons, another administrative layer called municipality served as the connection between counties and provinces.

<sup>58</sup>In our sample of prefecture level cities, all of the state level zones were located in the city center.

with the surrounding area.

Furthermore, the legal boundaries of the city cores (and thus the official statistics associated with this city core) change more frequently than the legal boundaries of the whole area. The two definitions of a city each have their advantages and drawbacks. We focus on the city core because of the closer link to economic policy but also report results for the city area and the periphery.

**Inland Sample** When we restrict the sample to cities from inland provinces, we define the following provinces as inland: Anhui, Gansu, Guangxi, Guizhou, Heilongjiang, Henan, Hubei, Hunan, Jiangxi, Jilin, Ningxia, Qinghai, Shaanxi, Shanxi, Sichuan, Xinjiang, Yunnan, and Inner Mongolia. This classification was not purely based on access to the sea, but also considers whether the provinces were part of the reform wave targeted towards inland regions.

### 8.3 Level Decomposition

The following paragraphs provide information on the decomposition of real GDP into physical capital stock, employment, labor efficiency and TFP. The decomposition is carried out in the prefecture area, instead of the urban center. This is due to the lack of educational attainment data in the urban center.

**Real GDP** We use the provincial GDP deflators to obtain the real GDP in prefecture cities. They are calculated using provincial constant and current price GDP series for the period 1988-2008.

**Physical Capital Stock** We apply the perpetual inventory approach to construct the physical capital stock in each city. The physical capital ( $PC_{ipt}$ ) is the sum of physical capital stock after depreciation and new investment, such that

$$PC_{ipt} = (1 - \delta_k)PC_{ipt-1} + Inv_{ipt}/deflator_{pt}^{Inv}.$$

The deflator for new investment,  $deflator_{pt}^{Inv}$ , is province-specific. We set  $\delta_k$ , the annual depreciation rate for physical capital, to be 0.08.<sup>59</sup>

In order to carry out the perpetual inventory approach, we need a reasonable estimate for the physical capital stock of the initial year, which is the year of 1988 given our sample period.

For a subset of cities whose investment data go back to 1978, we derive the capital stock for those cities in the year 1978 as follows

$$PC_{ip1978} = \frac{Inv_{ip1978}}{g_{1978} + \delta_k},$$

where  $Inv_{ip1978}$  is the new investment in year 1978 and  $g_{1978}$  is the average growth rate of real physical capital stock before 1978.<sup>60</sup> This is the steady state formula for physical capital stock of a Solow-type growth model (Caselli, 2005). By doing this, we assume that the economy was in steady state in 1978, which is quite plausible.<sup>61</sup>

---

<sup>59</sup>Given the large amount of creative destruction that took place in China, we pick the number to be higher than other cross-country growth accounting exercises, for example Caselli (2005).

<sup>60</sup>The growth rate of real physical capital stock,  $g_{1978}$ , is calculated using the national physical capital stock. See the personal website of Kuai Wai Li and Li *et al.* (2009) for the detailed construction of the data.

<sup>61</sup>Notice that our sample starts from 1988, the error of the estimate for initial physical capital stock (1978) would have only marginal impacts on the estimate of the physical capital stock ten years later.

For those cities whose investment data begins in 1988, we approximate the initial physical capital stock in 1988 using the same formula

$$PC_{ipt1988} = \frac{Inv_{ip1988}}{g_{1988} + \delta_k},$$

where  $g_{1988}$  is the average growth rate of physical capital stock before 1988.

**Efficient Labor** Efficient labor  $EL_{ipt}$  is the total number of employed persons (quantity) adjusted by the human capital (quality), such that

$$EL_{ipt} = HC_{ipt} \times Emp_{ipt}.$$

We use population as an approximation for employment in each city. The reason is that the number of employed persons reported in the *China City Statistical Yearbooks* has some drawbacks: 1) The number of employed persons is defined as registered workers in each city, which might exclude the large number of immigrant workers. 2) There is a huge drop in the number of employed persons in the year 1998, the reason of which is unclear to us.<sup>62</sup>

To construct city-level population data, first we take the population data for the year 1990, 2000 and 2010 from the *China Population Census*. Then we interpolate the population of the years in between using the year-by-year population growth rate, which is calculated using the city-level population from the *China City Statistical Yearbooks*.<sup>63</sup>

Following Hall and Jones (1999), we use the average educational attainment (years of schooling) as an approximation for the level human capital of the cities, such that

$$HC_{ipt} = e^{\phi_t(s_{ipt})},$$

where  $s_{ipt}$  is the average years of schooling and  $\phi_t(\cdot)$  is a piece-wise linear function whose slopes represent the return to schooling. To construct  $\phi_t(\cdot)$ , we take the estimation for the return to schooling in China over the period 1988-2009 from Li *et al.* (2009).<sup>64</sup>

The only data source that reports city-level education attainment is the *China Population Census*. Therefore, the data is only available for the years 1990, 2000 and 2010. We do a simple linear interpolation (extrapolation if needed) to obtain the approximation of human capital for the other years in our sample period.

**TFP** At last, we obtain the log TFP using the following formula,

$$\begin{aligned} \log TFP_{ipt} &= \log rGDP_{ipt} - \alpha_k \log PC_{ipt} \\ &\quad - (1 - \alpha_k) \log EL_{ipt}, \end{aligned}$$

where  $\alpha_k$ , the share of capital in the output function, is set to be 0.4.

<sup>62</sup>Two reasons could potentially contribute to this huge drop. The first reason is that the reform of state-owned enterprises laid off a large number of redundant workers around 1998. According to Dong and Putterman (2003), the labor redundancy rate of SOEs is 30% in 1992. The second reason is that the definition of employed persons changes on 1998. Before 1998, the employed persons include people who are registered as workers. After 1998, the number only includes people who are registered and are currently working in that city. Wu (2011) provides detailed discussion about the issues with the employment data.

<sup>63</sup>The *China Population Census* have more accurate population data than *China City Statistical Yearbooks*. The population reported in the census is the resident population (registered population and unregistered immigrant workers) while the city statistical yearbooks cover registered population only.

<sup>64</sup>We assume that the return of schooling did not change between 2009 and 2010, i.e.  $\phi_{2010}(\cdot) = \phi_{2009}(\cdot)$ .

## 8.4 Light Data

The light data are obtained from the National Geographical Data Center.<sup>65</sup> The data is available in cleaned form (taking into account clouds, forest fires, gas flaring, etc.) and on a yearly basis from 1992 to 2010. Light is measured on each pixel of approximately one square kilometer on an integer scale from 0 (no light) to 63 (maximum light). In order to map the light intensity of pixels to the administrative entities of cities, we use digital maps of Chinese cities from 2010.

Light is measured by different satellites over time and they show different light intensities because of differences in their calibration. These differences do not matter for our empirical analysis as they are absorbed by the year fixed effects, but for the descriptive data we calibrate the values ex-post following Elvidge *et al.* (2009).

---

<sup>65</sup><http://www.ngdc.noaa.gov/dmsp/downloadV4composites.html>

## 9 Tables and Figures

Table 1: State and Province Level Zones in 3 Provinces

Province	#S	#P	Avg indus-output share of S	Avg indus-output share of P
Jiangsu	12	113	3.13%	0.55%
Guangdong	14	56	4.89%	0.56%
Zhejiang	8	57	4.09%	1.18%

Source: WEFore (2010). The table displays the number of state level development zones (#S) and province level development zones (#P) in three provinces: Jiangsu, Guangdong and Zhejiang. In the last two columns, it also displays the average share of the state level and province level zones in the industrial output of each province. The data is for the year 2009.

Table 2: Descriptive statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Real GDP (mil)	10388.9	21776.23	116.62	414700.53	5147
Growth of real GDP (%)	13.07	18.13	-52.19	594.78	4738
Land area (sq km)	1728.36	2028.58	25	20169	5159
Growth of land area (%)	8.44	170.69	-93.23	9852	4750
Population (mil)	1.01	0.87	0.1	8.01	5275
Growth of population (%)	2.71	17.97	-77.18	586.19	4876
Electricity consumption (GWh)	3.08	4.71	0.01	56.3	5085
Growth of electricity consumption (%)	17.41	202.25	-98.97	13486.34	4674
Mean light intensity (calibrated)	13.32	11.27.4	0.12	64.38	4435
Growth of light intensity (calibrated) (%)	5.22	13.98	-38.93	124.57	4178

The table shows the descriptive statistics of our main variables in our sample of 276 cities in 25 provinces. Real GDP is derived from city-level nominal GDP and provincial deflators. Land area is the official size of the prefecture level cities. Population includes registered residents only. Electricity consumption is by households and firms. Mean light intensity is the average brightness of pixels in the city.

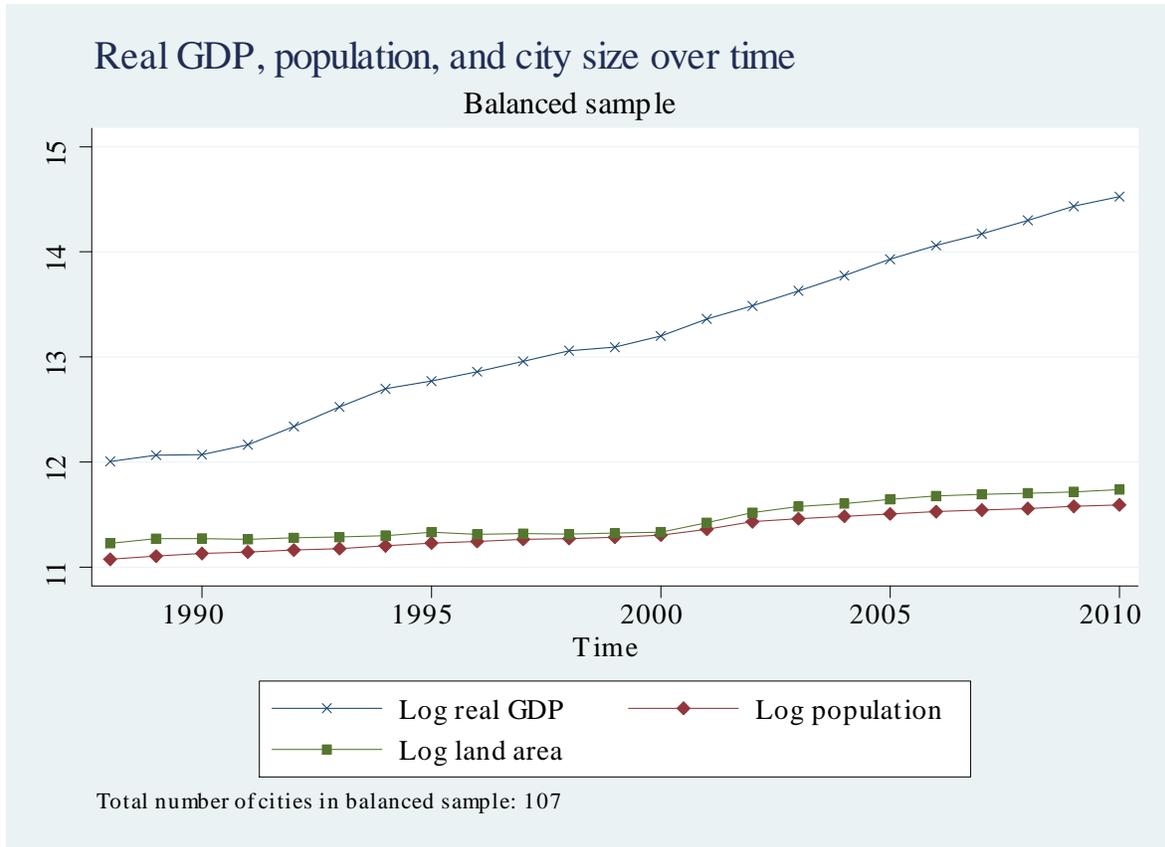


Figure 1: Main variables over time: The figure shows the time path of the logarithm of real GDP (using provincial deflators), the logarithm of the land area, and the logarithm of population of the city center. All variables are for the prefecture level city centers. The sample is restricted to 107 cities that are observed in all years between 1988 and 2010.

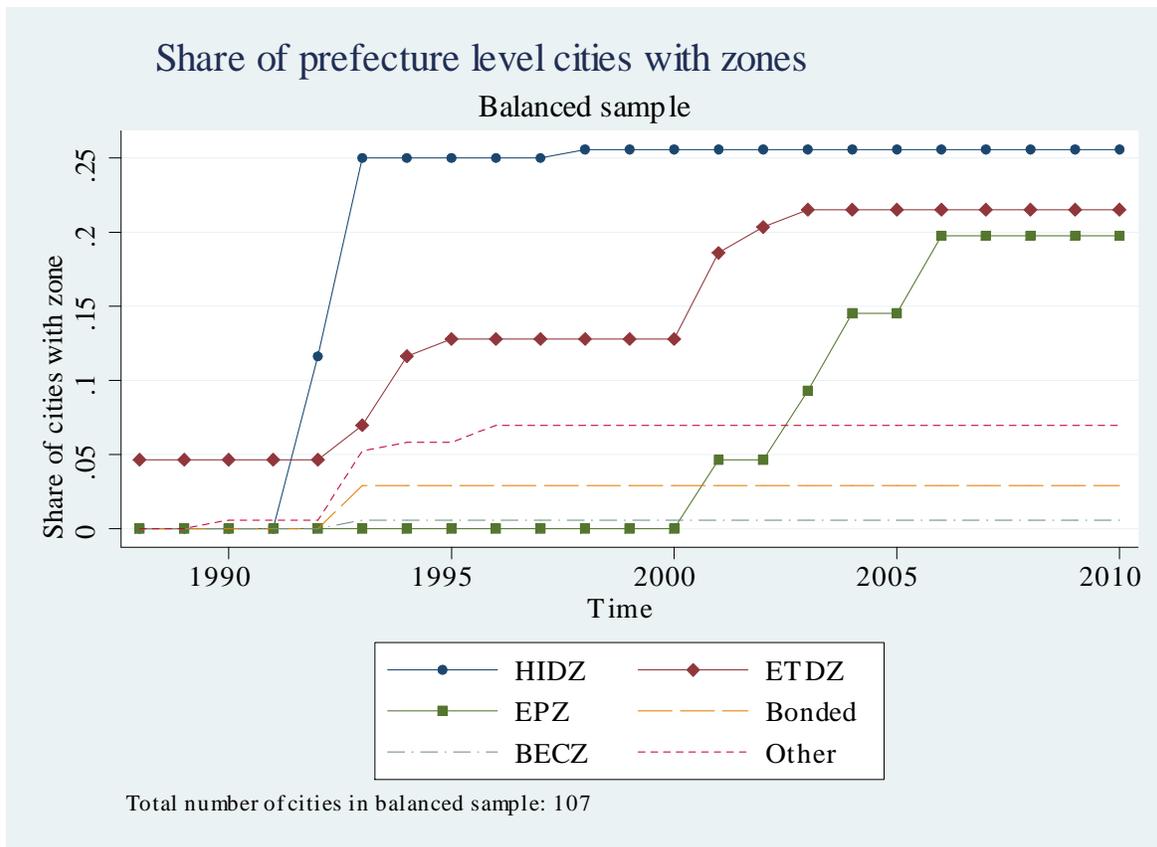


Figure 2: Share of prefecture level cities with different types of zones: The figure shows the share of cities which have different types of SEZ: Hightech Industrial Development Zones, Economic and Technolocial Development Zones, Export Processing Zones, Bonded Zones, Border Economic Cooperation Zones, and other types. The sample is restricted to 107 cities that are observed in all years between 1988 and 2010.

Table 3: Baseline specification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform indicator for any state-level zone	0.190*** (4.55)	0.147*** (4.29)	0.126*** (4.43)	0.117*** (4.15)	0.268*** (4.87)	0.212*** (4.11)	0.181*** (3.50)	0.166*** (3.09)
Post-reform indicator for province-level zone	-0.00188 (-0.07)	-0.00890 (-0.39)	-0.00136 (-0.07)	0.00226 (0.11)	-0.0192 (-0.67)	-0.0288 (-1.05)	-0.0118 (-0.43)	-0.000854 (-0.03)
Log landarea		0.240*** (7.73)	-0.0325 (-1.17)	-0.154*** (-7.66)		0.211*** (5.85)	-0.0527 (-1.21)	-0.175*** (-5.49)
Log population			0.692*** (12.22)				0.672*** (7.37)	
Sample	Full	Full	Full	Full	Inland	Inland	Inland	Inland
N	5160	5143	5141	5141	2692	2554	2686	2686
AR2	0.960	0.969	0.975	0.964	0.961	0.965	0.971	0.961

The dependent variable is the logarithm of annual GDP at the city level in columns (1)–(3) and in columns (5)–(7); it is annual GDP per capita at the city level in columns (4) and (8). GDP is measured in current prices. All specifications include city fixed effects and the interaction of province-time dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the city level. The sample includes 276 cities from 25 provinces over the sample period 1988-2010 (unbalanced panel). The regressions in columns (5)–(8) are restricted to cities in 18 inland provinces (as defined in the appendix).

Table 4: Pre- and post-reform indicators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Indicator for 3 years before any state-level zone	-0.00775 (-0.23)	0.0246 (0.71)	0.0228 (0.82)	0.0220 (0.81)	-0.00671 (-0.08)	0.00418 (0.05)	-0.0186 (-0.22)	-0.0298 (-0.32)
Indicator for 2 years before any state-level zone	-0.0148 (-0.42)	0.0254 (0.70)	0.0237 (0.84)	0.0229 (0.83)	-0.0440 (-0.51)	-0.0233 (-0.26)	-0.0412 (-0.50)	-0.0500 (-0.55)
Indicator for 1 year before any state-level zone	-0.0253 (-0.71)	0.0164 (0.45)	0.0160 (0.57)	0.0157 (0.58)	-0.0583 (-0.65)	-0.0380 (-0.41)	-0.0549 (-0.65)	-0.0631 (-0.68)
Indicator for year of any state-level zone	-0.00300 (-0.08)	0.0212 (0.54)	0.0231 (0.74)	0.0239 (0.79)	-0.0828 (-0.91)	-0.0613 (-0.65)	-0.0755 (-0.87)	-0.0825 (-0.85)
Post-reform indicator for any state-level zone	0.180*** (3.32)	0.164*** (3.36)	0.143*** (3.82)	0.134*** (3.68)	0.229** (2.50)	0.187* (1.90)	0.142 (1.55)	0.120 (1.18)
Post-reform indicator for province-level zone	-0.00219 (-0.09)	-0.00831 (-0.36)	-0.000783 (-0.04)	0.00283 (0.14)	-0.0200 (-0.69)	-0.0348 (-1.25)	-0.0127 (-0.46)	-0.00193 (-0.07)
Log landarea		0.241*** (7.75)	-0.0320 (-1.16)	-0.154*** (-7.63)		0.197*** (5.26)	-0.0530 (-1.22)	-0.175*** (-5.50)
Log population			0.692*** (12.25)				0.672*** (7.38)	
Sample	Full	Full	Full	Full	Inland	Inland	Inland	Inland
N	5160	5143	5141	5141	2692	2686	2686	2686
AR2	0.960	0.969	0.975	0.964	0.961	0.966	0.970	0.961

The dependent variable is the logarithm of annual GDP at the city level in columns (1)–(3) and in columns (5)–(7); it is annual GDP per capita at the city level in columns (4) and (8). GDP is measured in current prices. All specifications include city fixed effects and the interaction of province-time dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the city level. The sample includes 276 cities from 25 provinces over the sample period 1988–2010 (unbalanced panel). The regressions in columns (5)–(8) are restricted to cities in 18 inland provinces (as defined in the appendix).

Table 5: Trend break

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform indicator for any state-level zone	0.126*** (4.43)	0.0823*** (2.83)	0.0835*** (2.86)		0.181*** (3.50)	0.0860 (1.51)	0.0979* (1.89)	
Post-reform indicator for province-level zone	-0.00136 (-0.07)	-0.00117 (-0.06)	-0.00105 (-0.05)	0.000299 (0.01)	-0.0118 (-0.43)	-0.0108 (-0.39)	-0.0118 (-0.43)	-0.0140 (-0.51)
Time trend of reformers (state-level)		0.00548** (2.15)	0.00595* (1.68)	0.00655* (1.88)		0.00854* (1.68)	0.00239 (0.24)	-0.00114 (-0.11)
Post-reform trend (state-level)			-0.000739 (-0.19)	0.0156** (2.25)			0.00647 (0.55)	0.0453** (2.59)
Sq. post-reform trend (state-level)					-0.000739*** (-2.60)			-0.00182*** (-2.79)
Sample	Full	Full	Full	Full	Inland	Inland	Inland	Inland
N	5141	5141	5141	5141	2686	2686	2686	2686
AR2	0.975	0.975	0.975	0.975	0.971	0.971	0.971	0.971

The dependent variable is the logarithm of annual GDP at the city level in all columns. GDP is measured in current prices. All specifications also control for the logarithm of population and land area and they include city fixed effects and the interaction of province-time dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the city level. The sample includes 276 cities from 25 provinces over the sample period 1988-2010 (unbalanced panel). The regressions in columns (5)–(8) are restricted to cities in 18 inland provinces (as defined in the appendix).

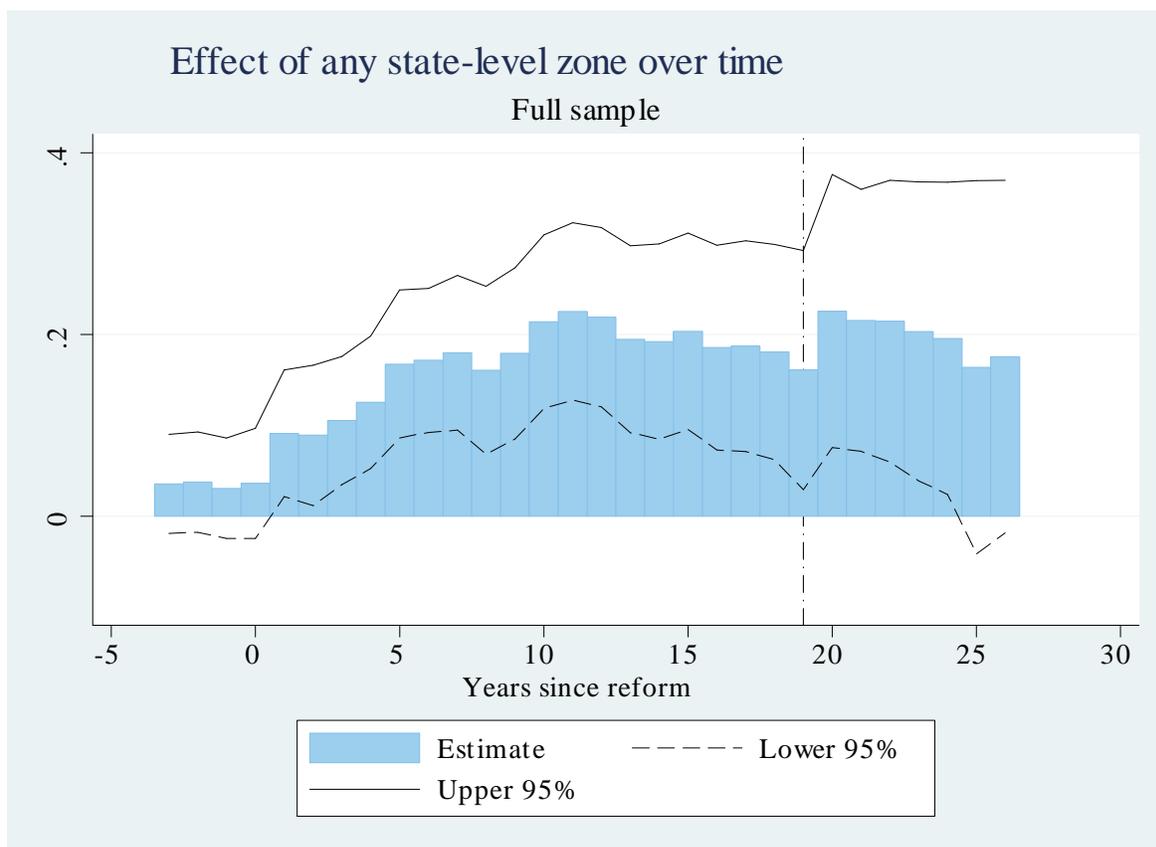


Figure 3: Reform effects over time: The bars show the coefficients of a regression of the logarithm of nominal GDP on indicators for years before and after the first zone. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 2010 and subsequently the number of observations to identify post-reform indicators drops to 9. The regression also controls for an indicator for province-level zones, land area, population, city fixed effects, and province-time fixed effects. Standard errors are clustered by city. The sample includes 276 cities from 25 provinces for the period 1988-2010.

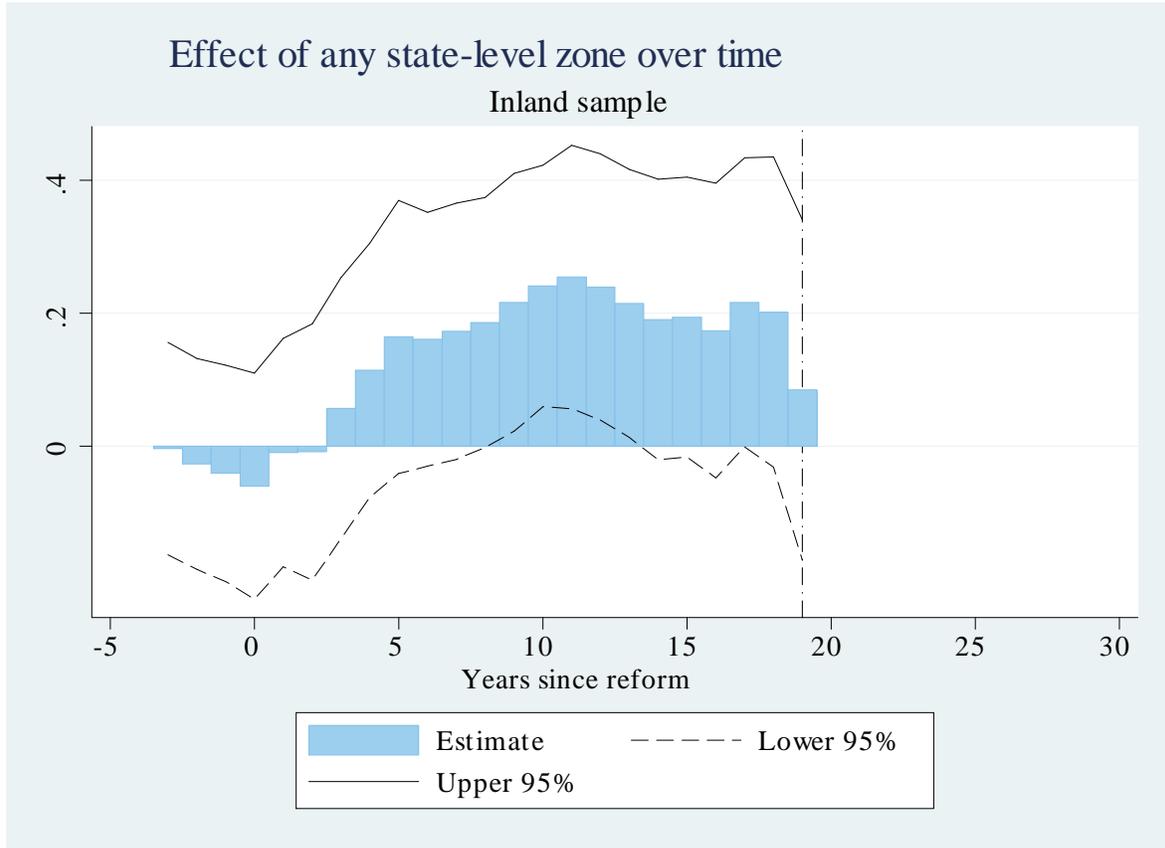


Figure 4: Reform effects over time: The bars show the coefficients of a regression of the logarithm of nominal GDP on indicators for years before and after the first zone. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 2010 and subsequently the number of observations to identify post-reform indicators drops to 9. The regression also controls for an indicator for province-level zones, land area, population, city fixed effects, and province-time fixed effects. Standard errors are clustered by city. The sample includes 158 cities from 18 inland provinces (as defined in the appendix) for the period 1988-2010.

Table 6: Effects of different types of zones

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform indicator for ETDZ	0.220*** (4.61)	0.156*** (3.66)	0.120*** (3.17)	0.104*** (2.71)	0.239*** (3.96)	0.171*** (2.98)	0.0968* (1.66)	0.0578 (0.98)
Post-reform indicator for HIDZ	0.117*** (2.63)	0.0792** (2.19)	0.0753** (2.45)	0.0735** (2.40)	0.122** (2.57)	0.0928* (1.80)	0.106** (2.18)	0.113** (2.25)
Post-reform indicator for EPZ	0.0413 (0.92)	0.0362 (0.94)	0.0206 (0.61)	0.0132 (0.38)	-0.0122 (-0.15)	0.00210 (0.02)	0.0491 (0.65)	0.0740 (1.04)
Post-reform indicator for OtherTypes	0.0709 (0.99)	0.0581 (0.93)	0.0896* (1.81)	0.104** (2.21)	0.148 (1.41)	0.118* (1.69)	0.211** (2.17)	0.128* (1.84)
Post-reform indicator for province-level zone	0.00145 (0.06)	-0.00716 (-0.32)	0.000742 (0.04)	0.00463 (0.23)	-0.0193 (-0.68)	-0.0337 (-1.23)	-0.0129 (-0.47)	-0.00183 (-0.07)
Log landarea		0.233*** (7.20)	-0.0361 (-1.30)	-0.159*** (-7.66)		0.187*** (4.83)	-0.0530 (-1.19)	-0.180*** (-5.55)
Log population			0.686*** (11.84)				0.654*** (6.75)	
Sample	Full	Full	Full	Full	Inland	Inland	Inland	Inland
N	5160	5143	5141	5141	2692	2686	2686	2686
AR2	0.961	0.969	0.975	0.964	0.962	0.967	0.971	0.961

The dependent variable is the logarithm of annual GDP at the city level in columns (1)–(3) and in columns (5)–(7); it is annual GDP per capita at the city level in columns (4) and (8). GDP is measured in current prices. *OtherTypes* include BECZ, Bonded Zones, and zones of unknown type. All specifications include city fixed effects and the interaction of province-time dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the city level. The sample includes 276 cities from 25 provinces over the sample period 1988-2010 (unbalanced panel). The regressions in columns (5)–(8) are restricted to cities in 18 inland provinces (as defined in the appendix).

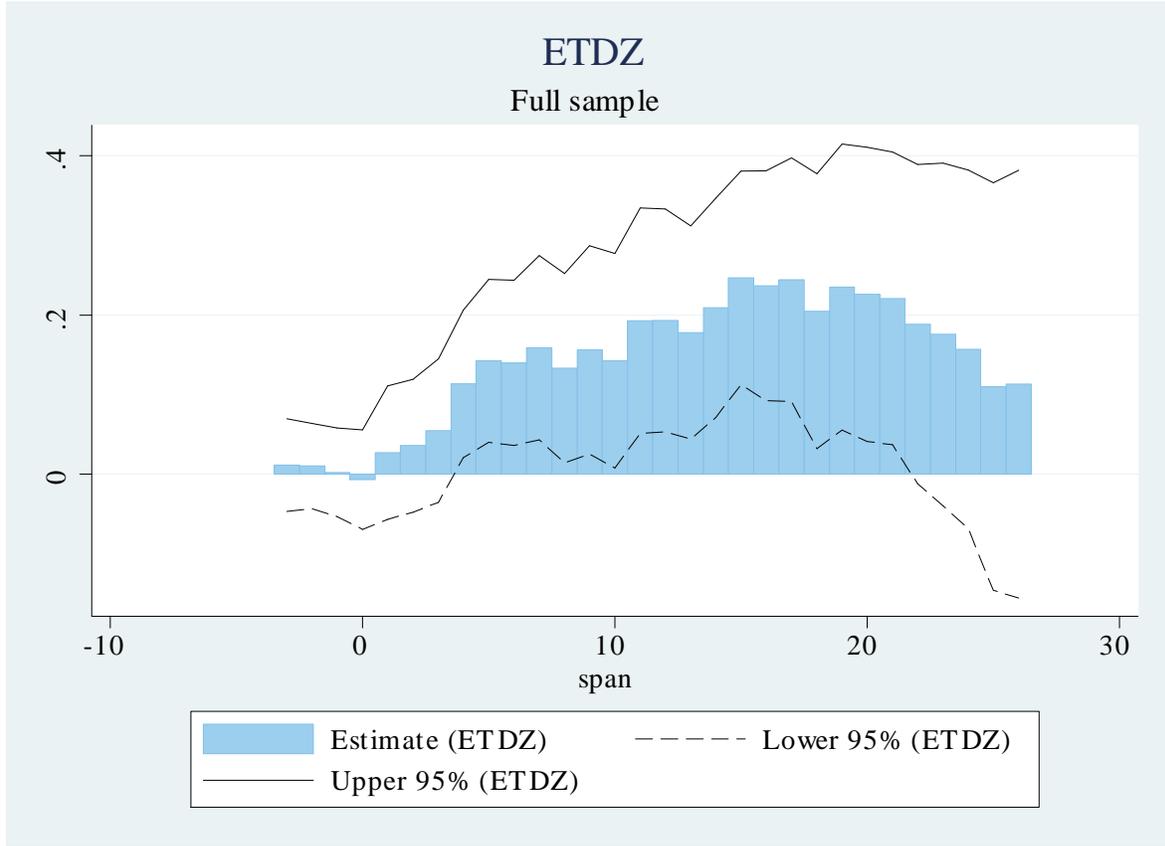


Figure 5: ETDZ effects over time: The bars show the coefficients of a regression of the logarithm of nominal GDP on indicators for years before and after a ETDZ was established. The same regression also controls for yearly indicators of the other state level zones (those coefficients are shown in separate graphs). The solid and dashed lines show the confidence interval. The regression also controls for an indicator for province-level zones, land area, population, city fixed effects, and province-time fixed effects. Standard errors are clustered by city. The sample includes 276 cities from 25 provinces for the period 1988-2010.

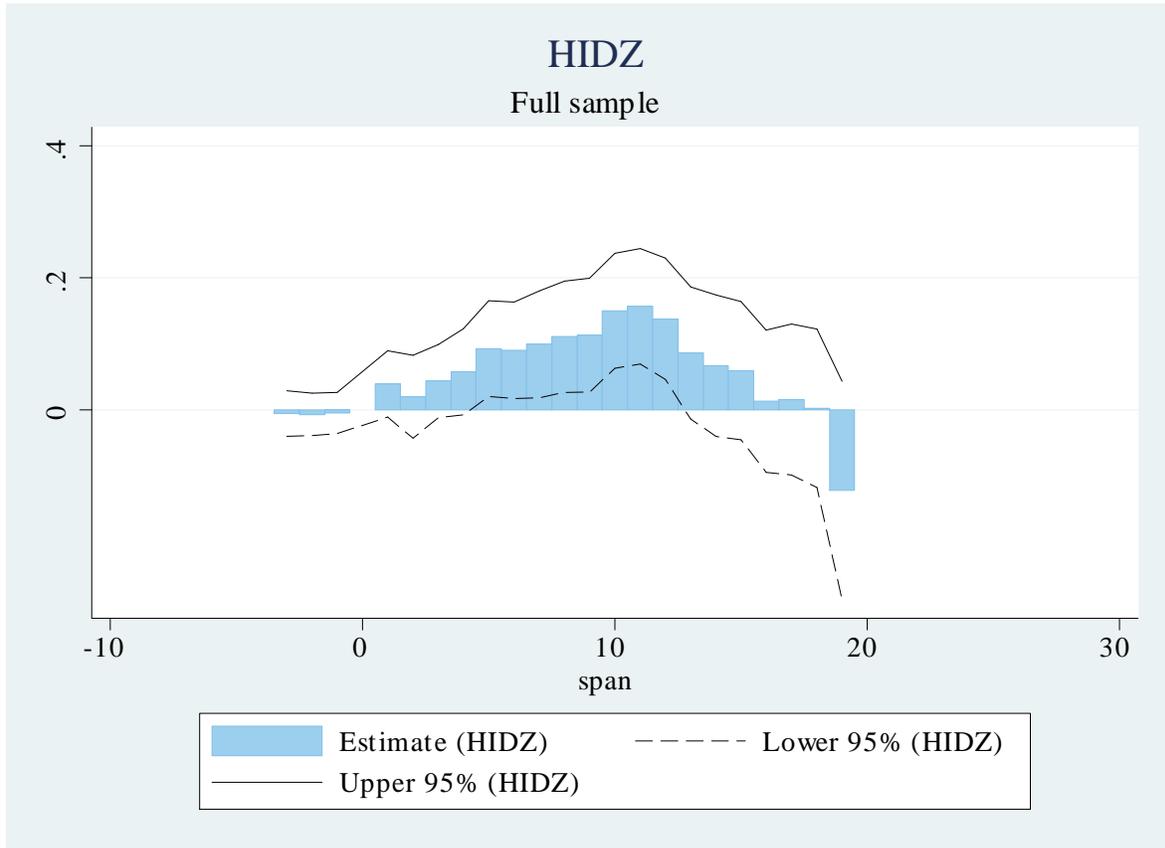


Figure 6: HIDZ effects over time: The bars show the coefficients of a regression of the logarithm of nominal GDP on indicators for years before and after a HIDZ was established. The same regression also controls for yearly indicators of the other state level zones (those coefficients are shown in separate graphs). The solid and dashed lines show the confidence interval. The regression also controls for an indicator for province-level zones, land area, population, city fixed effects, and province-time fixed effects. Standard errors are clustered by city. The sample includes 276 cities from 25 provinces for the period 1988-2010.

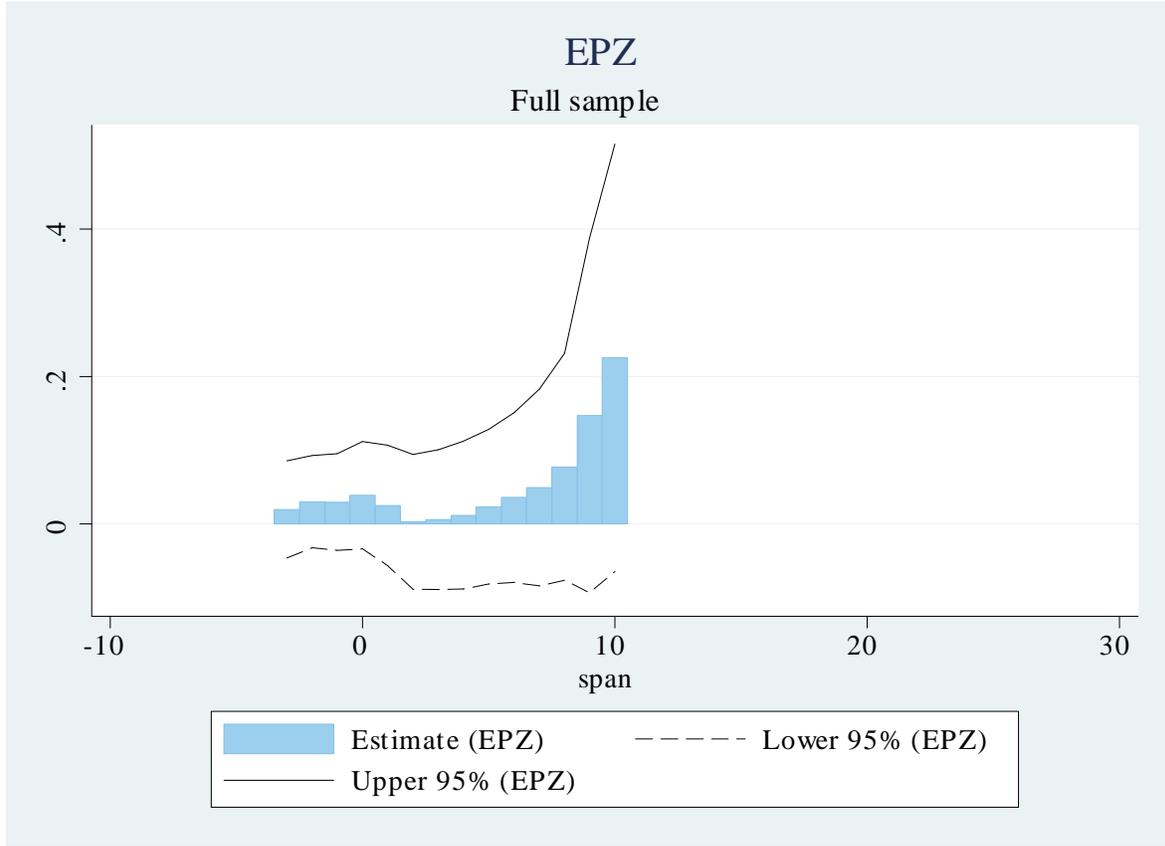


Figure 7: EPZ effects over time: The bars show the coefficients of a regression of the logarithm of nominal GDP on indicators for years before and after an EPZ was established. The same regression also controls for yearly indicators of the other state level zones (those coefficients are shown in separate graphs). The solid and dashed lines show the confidence interval. The regression also controls for an indicator for province-level zones, land area, population, city fixed effects, and province-time fixed effects. Standard errors are clustered by city. The sample includes 276 cities from 25 provinces for the period 1988-2010.

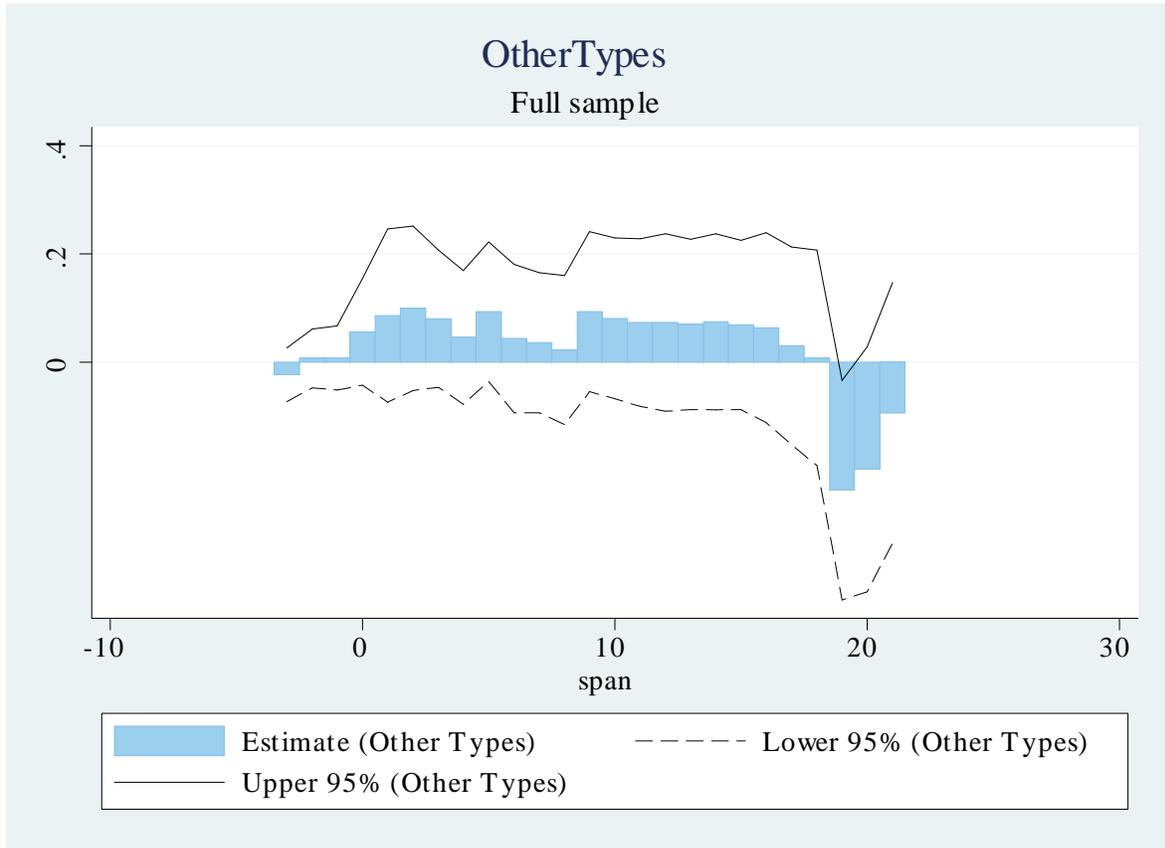


Figure 8: Effects of other zone types over time: The bars show the coefficients of a regression of the logarithm of nominal GDP on indicators for years before and after a zone of another type was established. The same regression also controls for yearly indicators for ETDZ, HIDZ, and EPZ (those coefficients are shown in separate graphs). The solid and dashed lines show the confidence interval. The regression also controls for an indicator for province-level zones, land area, population, city fixed effects, and province-time fixed effects. Standard errors are clustered by city. The sample includes 276 cities from 25 provinces for the period 1988-2010.

Table 7: Decomposition of the reform effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform indicator for first state-level zone	0.137*** (3.43)	0.227*** (4.25)	0.0416* (1.91)	0.0164 (0.60)	0.00319 (0.82)	0.00363 (0.64)	0.0397 (1.64)	0.0133 (0.38)
Post-reform indicator for first province-level zone	-0.000501 (-0.02)	-0.0122 (-0.28)	0.0149 (1.18)	0.00272 (0.22)	0.00234 (1.40)	0.00433* (1.93)	-0.0121 (-0.61)	-0.00255 (-0.09)
Log landarea	0.0476* (1.89)	0.0526* (1.96)	0.624*** (18.10)	0.603*** (15.80)	-0.0157*** (-4.58)	-0.0156*** (-3.97)	0.0281 (1.00)	0.0191 (0.57)
Dependent variable	log(phy. cap.)	log(phy. cap.)	log(pop)	log(pop)	log(hum. cap.)	log(hum. cap.)	log(TFP)	log(TFP)
Sample	Full	Inland	Full	Inland	Full	Inland	Full	Inland
N	4529	2741	5114	3199	4405	2634	3942	2294
AR2	0.971	0.969	0.855	0.873	0.957	0.950	0.943	0.937

The dependent variables are the four decomposed component of logarithm of GDP: logarithm of physical capital stock (column (1)-(2)), logarithms of population (column (3)-(4)), logarithms of average human capital (column (5)-(6)) and logarithm of TFP (column (7)-(8)). All specifications use data from prefecture area and include land area, city fixed effects and the interaction of province-time dummies as control variables. T-statistics are reported in parenthesis. Standard errors are clustered at the city level. The sample includes 276 cities from 25 provinces over the sample period 1988-2010 (unbalanced panel). We trimmed the top 1% and bottom 1% of the annual growth rate of physical capital stock, population, labor efficiency and TFP, respectively. The regression is carried out for the full sample (column (1), (3), (5), (7)) and restricted inland sample ((2), (4), (6), (8)).

Table 8: Effect on Human Capital

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform indicator for first state-level zone	0.173*** (3.46)	0.229*** (3.36)	-0.00985 (-1.58)	-0.00544 (-0.70)	-0.0208*** (-2.93)	-0.0321*** (-3.43)	0.0307*** (5.97)	0.0376*** (5.48)
Post-reform indicator for first province-level zone	0.0993*** (2.84)	0.133*** (2.87)	-0.0116* (-1.88)	-0.0190*** (-2.92)	0.0103 (1.62)	0.0138** (1.99)	0.00129 (0.35)	0.00523 (1.01)
Log landarea	-0.269** (-2.56)	-0.194* (-1.76)	0.0379*** (2.66)	0.0221 (1.53)	-0.0148 (-1.59)	-0.00191 (-0.22)	-0.0230** (-2.49)	-0.0201* (-1.97)
Log population	-0.139 (-0.83)	-0.267 (-1.26)	0.0180 (0.78)	0.0484* (1.66)	-0.00665 (-0.44)	-0.0332** (-2.09)	-0.0114 (-0.72)	-0.0154 (-0.76)
Dependent variable	ave. sch.	ave. sch.	share low	share low	share mid.	share mid.	share high	share high
Sample	Full	Inland	Full	Inland	Full	Inland	Full	Inland
N	577	360	577	360	577	360	577	360
AR2	0.976	0.976	0.968	0.971	0.884	0.900	0.929	0.934

The dependent variables are average years of schooling (column (1)-(2)), share of population over 6 with low level education (primary school or lower)(column (3)-(4)), share of population over 6 with an intermediate level education (junior and senior high school) (column (5)-(6)) and share of population over 6 with high level education (college or above) (column (7)-(8)). All specifications use data from prefecture area and include land area of the prefecture, city fixed effects and the interaction of province-time dummies as extra control variables. T-statistics are reported in parenthesis. Standard errors are clustered at the city level. The sample includes 276 cities from 25 provinces in 1990, 2000 and 2010 (unbalanced panel). The regression is carried out for the full sample (column (1), (3), (5), (7)) and restricted inland sample ((2), (4), (6), (8)).

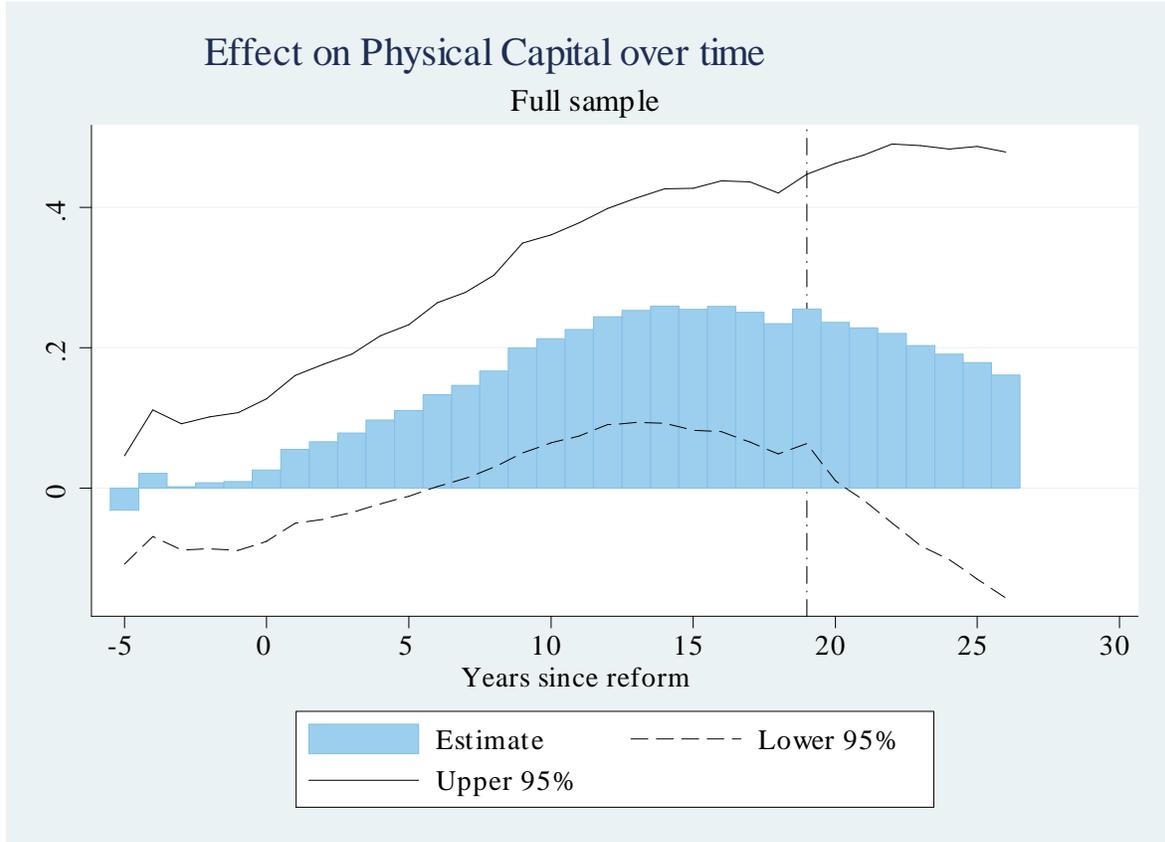


Figure 9: Effects on Physical Capital over time (prefecture area): The bars show the coefficients of a regression of the logarithm of physical capital on indicators for years before and after the first SEZ was established. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 2010 and subsequently the number of observations to identify post-reform indicators drops to 9. The regression uses data from the prefecture area and also controls for an indicator for province-level zones, land area of the prefecture, city fixed effects, and province-time fixed effects. Standard errors are clustered by city. The sample includes 276 cities from 25 provinces for the period 1988-2010.

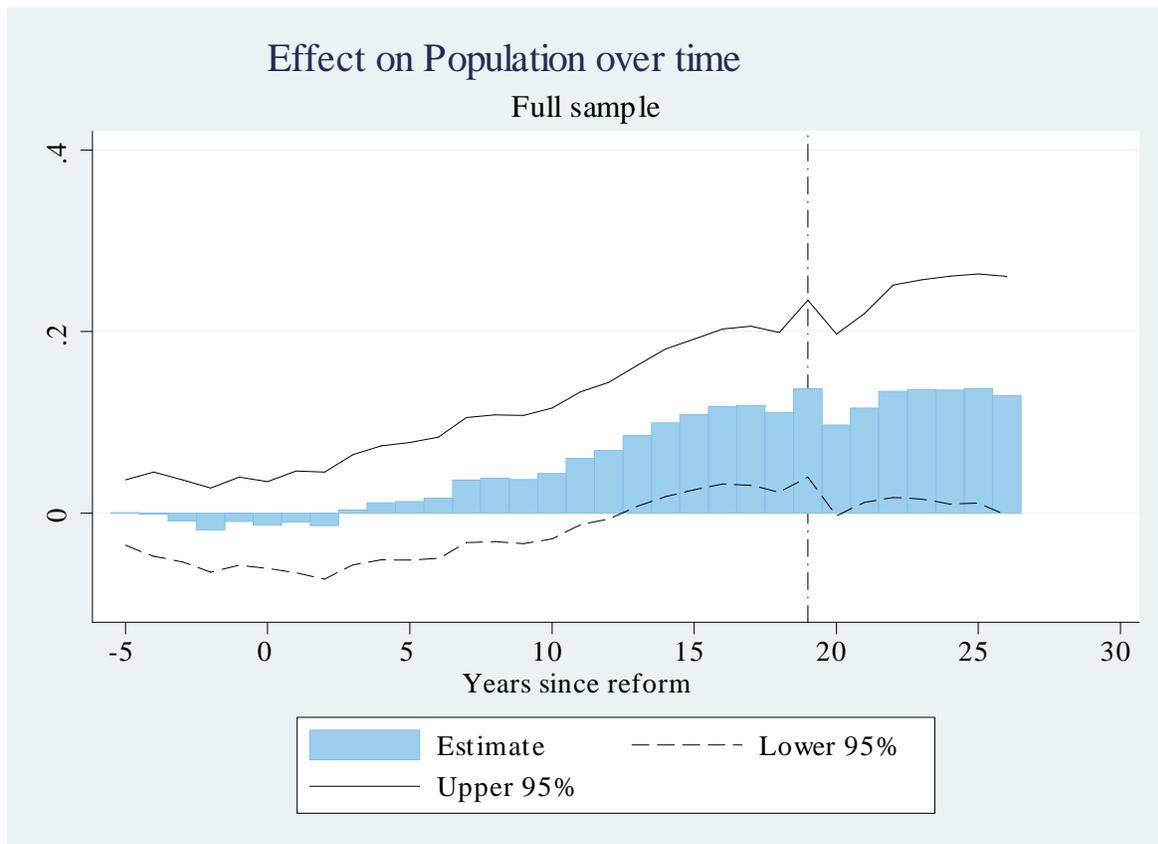


Figure 10: Effects on population over time (prefecture area): The bars show the coefficients of a regression of the logarithm of population on indicators for years before and after the first SEZ was established. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 2010 and subsequently the number of observations to identify post-reform indicators drops to 9. The regression uses data from the prefecture area and also controls for an indicator for province-level zones, land area of the prefecture, city fixed effects, and province-time fixed effects. Standard errors are clustered by city. The sample includes 276 cities from 25 provinces for the period 1988-2010.

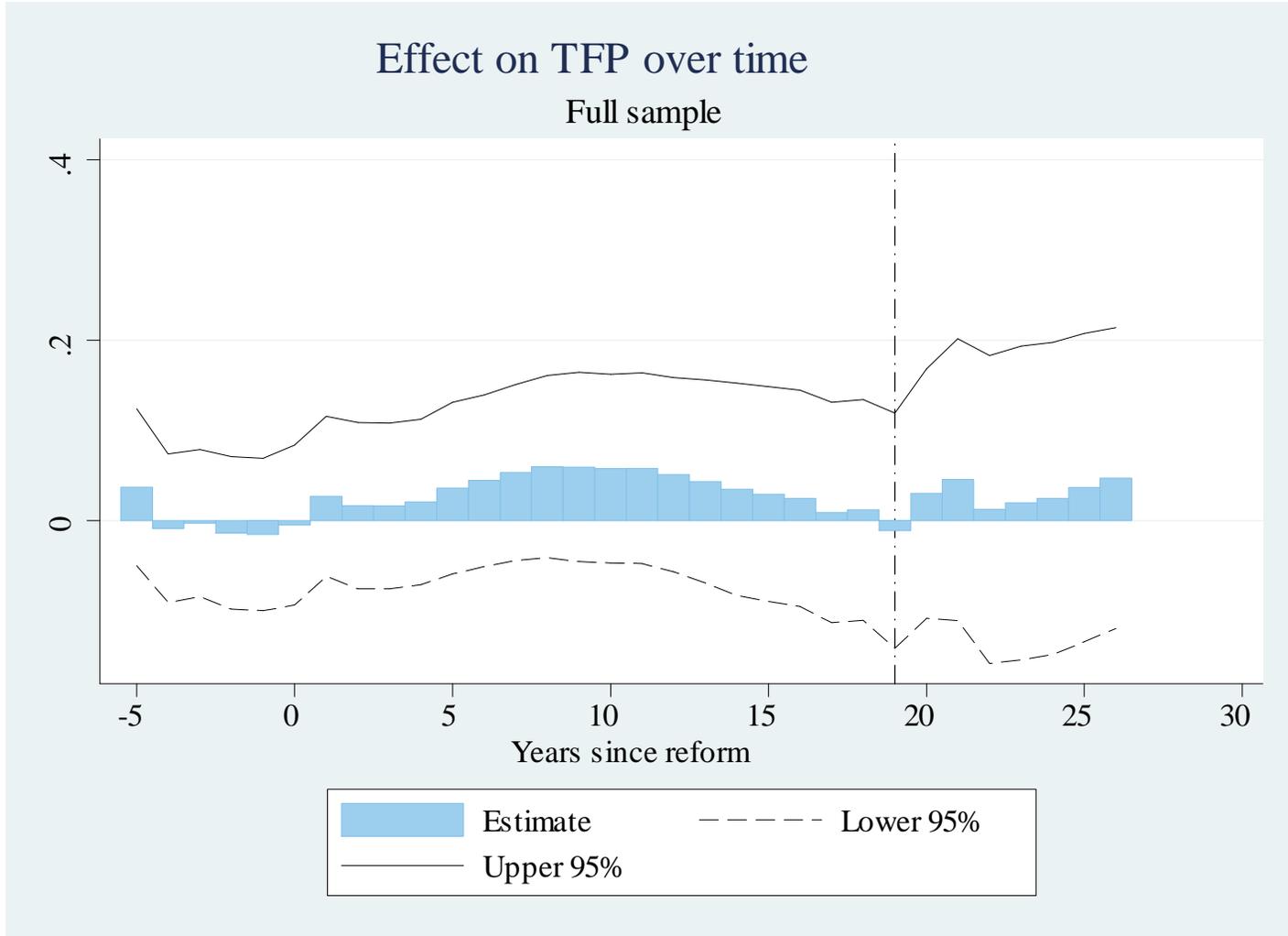


Figure 11: Effects on TFP over time (prefecture area): The bars show the coefficients of a regression of the logarithm of TFP on indicators for years before and after the first SEZ was established. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 2010 and subsequently the number of observations to identify post-reform indicators drops to 9. The regression uses data from the prefecture area and also controls for an indicator for province-level zones, land area of the prefecture, city fixed effects, and province-time fixed effects. Standard errors are clustered by city. The sample includes 276 cities from 25 provinces for the period 1988-2010.

Table 9: Reform effects on entire prefecture and on periphery only

Panel A: Prefecture Area								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform indicator for any state-level zone	0.156*** (4.72)	0.118*** (4.05)	0.132*** (4.80)	0.136*** (4.85)	0.213*** (3.07)	0.186*** (3.39)	0.190*** (3.65)	0.197*** (3.63)
Post-reform indicator for province-level zone	0.0236 (1.01)	-0.00340 (-0.19)	-0.00858 (-0.50)	-0.0102 (-0.60)	0.0272 (0.81)	-0.00933 (-0.37)	-0.0144 (-0.61)	-0.0143 (-0.60)
Log landarea		0.410*** (12.89)	-0.0566 (-0.79)	-0.218*** (-6.35)		0.345*** (5.76)	-0.0178 (-0.22)	-0.210*** (-4.58)
Log population			0.743*** (7.67)				0.686*** (6.04)	
Panel B: Periphery Only								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform indicator for any state-level zone	0.219** (2.23)	0.107*** (3.25)	0.143*** (5.02)	0.149*** (5.12)	0.310 (1.26)	0.123* (1.87)	0.184*** (3.86)	0.194*** (4.01)
Post-reform indicator for province-level zone	0.0789 (1.57)	-0.00561 (-0.25)	-0.0105 (-0.52)	-0.0113 (-0.55)	0.0910 (1.37)	-0.000238 (-0.01)	0.00138 (0.05)	0.000197 (0.01)
Log landarea		0.878*** (18.54)	0.147* (1.75)	0.0287 (0.82)		0.880*** (11.66)	0.216** (2.09)	0.0705* (1.92)
Log population			0.860*** (8.85)				0.816*** (6.10)	
Sample	Full	Full	Full	Full	Inland	Inland	Inland	Inland
N	4944	4913	4912	4912	2561	2425	2546	2546
AR2	0.865	0.966	0.973	0.967	0.865	0.962	0.969	0.962

The dependent variable is the logarithm of annual GDP in columns (1)–(3) and in columns (5)–(7); it is annual GDP per capita in columns (4) and (8). GDP is measured in current prices. Panel A reports the results for the whole prefecture and Panel B reports the results for the periphery only (prefecture less the prefecture level city center). All specifications include city fixed effects and the interaction of province-time dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the city level. The sample includes 276 cities in Panel A and 260 cities in Panel B (in 2010) from 25 provinces over the sample period 1988-2010 (unbalanced sample). The regressions in columns (5)–(8) are restricted to cities in 18 inland provinces (as defined in the appendix).

Table 10: Electricity consumption and light intensity

	(1)	(2)	(3)	(4)
Post-reform indicator for any state-level zone	0.110** (2.26)	0.00181 (0.03)	0.0521** (2.12)	0.0533 (1.07)
Post-reform indicator for province-level zone	0.0305 (0.89)	0.0295 (0.69)	-0.0145 (-0.80)	-0.0447 (-1.65)
Log landarea	-0.0765 (-1.57)	0.0165 (0.28)		
Log population	0.535*** (4.90)	0.211* (1.69)	0.0107 (0.50)	0.00180 (0.05)
Dependent variable	log(Electricity)	log(Electricity)	log(Light)	log(Light)
Sample	Full	Inland	Full	Inland
N	5200	2715	4708	2554
AR2	0.804	0.758	0.836	0.818

The dependent variable is the logarithm of electricity consumption in columns (1)-(2); it is the logarithm of light intensity in columns (3)-(4); always at the city level. All specifications include city fixed effects and the interaction of province-time dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the city level. The sample includes 276 cities from 25 provinces over the sample period 1988-2010 for electricity consumption and 1992-2010 for light intensity. The panel is unbalanced. The regressions in columns (2) and (4) are restricted to cities in 18 inland provinces (as defined in the appendix).

Table 11: Controlling for expenditures of the local government

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform indicator for any state-level zone	0.145*** (4.23)	0.120*** (4.09)	0.107*** (4.30)	0.0983*** (3.89)	0.225*** (5.20)	0.177*** (4.13)	0.152*** (3.61)	0.134*** (2.87)
Post-reform indicator for province-level zone	-0.000446 (-0.02)	-0.00361 (-0.18)	0.00166 (0.09)	0.00534 (0.28)	-0.0125 (-0.46)	-0.0245 (-0.92)	-0.00772 (-0.29)	0.00449 (0.16)
Log gov. spending	0.355*** (7.26)	0.267*** (7.48)	0.206*** (6.65)	0.167*** (5.60)	0.271*** (6.69)	0.234*** (6.03)	0.194*** (5.74)	0.164*** (5.17)
Log landarea		0.214*** (7.99)	-0.0173 (-0.67)	-0.172*** (-9.14)		0.193*** (5.69)	-0.0161 (-0.42)	-0.168*** (-5.49)
Log population			0.599*** (10.81)				0.579*** (6.42)	
Sample	Full 4887	Full 4875	Full 4874	Full 4874	Inland 2558	Inland 2554	Inland 2554	Inland 2554
AR2	0.965	0.972	0.976	0.964	0.963	0.969	0.972	0.962

The dependent variable is the logarithm of annual GDP at the city level in columns (1)–(3) and in columns (5)–(7); it is annual GDP per capita at the city level in columns (4) and (8). GDP is measured in current prices. All specifications include city fixed effects and the interaction of province-time dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the city level. The sample includes 276 cities from 25 provinces over the sample period 1988-2010 (unbalanced panel). The regressions in columns (5)–(8) are restricted to cities in 18 inland provinces (as defined in the appendix).