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## ECONOMIC REFORMS AND INDUSTRIAL POLICY IN A PANEL OF CHINESE CITIES

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# ABSTRACT

## Economic Reforms and Industrial Policy in a Panel of Chinese Cities*

We estimate the effect on economic development of China's industrial policy, in particular, the establishment of Special Economic Zones (SEZ). We use data from a panel of 276 Chinese cities and prefectures from 1988 to 2010. Our difference-in-difference estimator exploits the variation in the establishment of SEZ across time and space. We find that the establishment of a state-level SEZ is associated with an increase in the level of GDP of about 20%, but not with a permanently steeper growth path. This finding is confirmed with alternative specifications and in a sub-sample of inland provinces, where the selection of cities to host the zones was based on administrative criteria. Decomposing the effect of SEZ on GDP into different channels shows that this worked mainly through the accumulation of physical capital, although there is some evidence of increasing productivity and human capital investments. Using light intensity as an alternative measure for economic activity confirms the positive effects of SEZ.

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Keywords: China, difference-in-difference, economic growth, economic reforms, industrial policy, investments, satellite light, special economic zones and total factor productivity

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# 1 Introduction

The process of economic reforms launched in 1978, and gradually extended until current days, has catapulted China into a stellar growth trajectory that has proven highly resilient. These reforms affect a variety of policies and institutions. Even today, it is difficult to pinpoint which of its components were crucial. Yet, disentangling the sources of China's development boom is key to an accurate appraisal of its experience. China's transition has been fostered by economic institutions that differ significantly from those adopted by Western economies. For instance, market liberalization has gone hand-in-hand with a set of proactive industrial policies granting special status and privileges to specific cities, industries and regions. State intervention continues to play a major role in the economic activity today. While liberalization is a centerpiece of the orthodox doctrine, the imbalances and distortions to resource allocation imposed by China's industrial policy run against the tenets of the Washington consensus.

This paper aims at contributing to a better understanding of the policy roots of China's success by focusing on industrial policy. We exploit the variation across cities and years in the establishment of different types of Special Economic Zones (SEZ) to estimate the effects of SEZ on economic development. SEZ are a salient component of the reform process for a variety of reasons. First, they have been a centerpiece of the gradualist development strategy based on the learning-through-experimentation principle. Second, they have fostered an uneven development across geographic areas and sectors – possibly exacerbating inequality. Last but not least important, their effects are easier to measure than those of other reforms, as they took the form of well-defined changes in the legal status staggered across different Chinese regions and cities. The first SEZ were introduced as experiments in market allocation in geographically restricted areas along the coast. SEZ enjoyed special rules applying to labor markets, foreign direct investments, firms' ownership, and export controls. Another important difference from the rest of the country is that local political leaders were granted substantial autonomy and could shape key aspects of the industrial policy. After the success of the early experiments, SEZ were extended first to other cities along the coast and then, starting in the early 1990s, to inland regions. The establishment of new zones has continued until today. For instance, on September 29, 2013, the government of Li Keqiang has launched the Shanghai Pilot Free Trade Zone in the Pudong area, that will enjoy both full liberalization of foreign trade and capital market liberalization – the innovative aspect of this new zone. Although too recent to assess, the new experiment shows that the guiding principle of the SEZ continues to inform China's economic reform strategy.

We use a panel of 276 cities over the period 1988-2010 to compare development across treated and non-treated cities. Our econometric strategy is a difference-in-difference estimator controlling for time-invariant heterogeneity at the city level. We also control for province-specific shocks by using province×time fixed effects. We first regress (the logarithm of) GDP on a reform indicator that switches on (i.e., takes the unit value) in the year after a city has received SEZ status, controlling for city characteristics such as size and population. In our baseline specification, the introduction of a SEZ is associated with a permanent increase in the city's GDP level of about 12%. The result is robust to controlling for local government spending. To account for gradual effects of the reform, we also consider more flexible specifications where the effect of the reform is allowed to vary, both parametrically and non-parametrically, as a function of the time elapsed since the start of the treatment. We find an increasing cumulative effect of the policy treatment that flattens out after about ten years; the long-term effect of a SEZ is estimated to be a differential 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 might 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. One might suspect the picking winners strategy to have been especially important in the first stage of the reforms, when all SEZ were chosen along the coast and close to potential trading partners and investors such as Hong Kong and Taiwan. Ideally, one would like to have valid instruments to isolate exogenous sources of variation in the reform treatment, but finding instruments is difficult in practice. We mitigate the concern with endogeneity through two complementary strategies. First, we restrict the sample to cities located in inland provinces where the selection of the zones was largely based on a rigid administrative criterion, i.e., being a provincial capital. Second, we augment the regressions with indicators for the immediate pre-reform years to capture differential trends. The results are reassuring in both cases: the effect of SEZ is robust in the restricted sample, and the pseudo-effects before the actual establishment of the zone are insignificant. Therefore, there appears to be a clear structural break around the reform year.

The second *caveat* concerns data quality. One might worry that local statistics be manipulated strategically by local officers in order to create the impression that an SEZ was successful so as to attract government support. In addition, while city-level nominal GDP data are available, city-level price deflators are more problematic (and only available for fewer cities/years). In our main specification, we use only nominal variables. The inclusion of city fixed effects removes any bias arising from time-invarying price level differences. Inflation differences across provinces are absorbed by the interaction between time and province fixed effects. Yet, this leaves open the possibility that different cities within the same province may experience different inflation rates. This would be a problem for our strategy if the SEZ status triggers systematically higher inflation rates, as in this case part of our estimated effect would be due to inflation. To address this concern, we first document that, in the more restricted sample for which we have data on prices at the city level, treated cities do not appear to have experienced higher inflation than did cities without SEZ. Next, we complement our analysis with alternative proxies of GDP that do not depend on prices: light intensity measured by satellites and electricity consumption. The results confirm the existence of robust significant effects of SEZ.

We also study the channels through which GDP increased as cities were granted SEZ status. First, we show that the treatment effect is positive and highly significant when one considers GDP per capita, instead of GDP levels, as the dependent variable. The estimated coefficient is almost identical between coastal and inland cities. Second, the treatment effect is strong and significant for capital labor ratios, showing that SEZ attract investments and trigger capital deepening. The effect on total factor productivity (TFP) and human capital also are positive, although with some *caveats*. For TFP the effect is positive and significant in the total sample, but insignificant (albeit positive) in the inland sample. The results for human capital are subject to data constraints, as census data are only available for three years. When we restrict the sample to census years, we find that the introduction of SEZ has a large and significant effect on the share of college graduates in the population.

Finally, one might suspect that the SEZ led to a concentration of resources in the SEZ, drawing resources away from adjacent areas. We find no evidence of such beggar-thy-neighbor effects. When we run our regressions at the prefectural level, a larger administrative unit that includes both the city core (our main unit of analysis) and a large surrounding periphery, and *exclude* the city core (where SEZ are hosted) we continue to find large positive effects. This suggests that there were no negative spillovers within prefectures, where if anything, crowding-in effects appear to prevail. However, our analysis cannot rule out negative externality across prefectures.

## 1.1 Related Literature

Our paper is related to a large literature on the effects of policy changes on economic development. We contribute to this literature by showing the effects of an important component of industrial policy in China. We are not first to study the effects of China's SEZ, although earlier studies, arguably due to data constraints, rely on comparisons of the cross-sectional variation in economic performance rather than on a difference-in-difference methodology. Wei (1993) uses city-level data for a sample of coastal cities where special policies were introduced in 1984, and documents that cities hosting SEZ have a significantly higher average growth rate during the early reform period, while other types of preferential policies do not produce the same effects.[1] Since his sample ends in 1990, when only a small subset of the cities had been granted the status of SEZ, his identification relies on the cross-sectional comparison between early reformers – a small and arguably selected group – and cities that were never granted the SEZ status at the time of his study. Wei's pioneer study is extended by Démurger *et al.* (2002) and Jones *et al.* (2003), who also document differences in growth rates between treated and non-treated cities. Different from these articles, our study exploits the staggered establishment of SEZ across cities. This allows us to estimate the treatment effect controlling for time-invarying heterogeneity (city fixed effects) and time-varying province-level shocks.

Other studies focus on different economic outcomes. Cheng and Kwan (2000) show that provinces hosting SEZ attract significantly more foreign direct investment (FDI) than do other provinces. A recent study by Wang (2013) uses a panel of Chinese cities and finds, using a difference-in-difference approach similar to ours, a strong positive effects of SEZ on FDI, exports, and the output of foreign enterprises. The effects on other outcome variables (which do not comprise GDP) are smaller and less robust. Our findings are complementary to Wang (2013) insofar as we focus on GDP, a comprehensive measure for the development of the local economy, while her study focuses on direct intermediate targets of the policy. An important difference for our analysis is that we distinguish between state-level and province-level SEZ (see below for a detailed motivation for this choice). Without drawing such a distinction, the introduction of SEZ yields no statistically significant effect in our sample. Some studies document the effect of China's SEZ at the firm level. Head and Ries (1996) analyze the location decision of international firms in Chinese cities and find that SEZ have a positive effect that this is amplified by agglomeration economies. Schminke and Van Biesebroeck (2013) estimate the effect of being located inside SEZ on firm's productivity and export behavior. They find that firms in SEZ export more, have higher output per worker and higher capital intensity, but no higher TFP once selection is controlled for.

Zones with special policies are not unique to China (see Akinci and Crittle 2008). In a recent study on US "Empowerment Zones", Busso *et al.* (2013) compare locations selected for special treatment, such as tax-credits and subsidies for disadvantaged neighborhoods, with similar locations that were rejected or treated in a second round. They conclude that the policy had significantly positive effects on employment and wages, while the efficiency costs were relatively small.

Our study also relates more generally to a large literature on liberalization and industrial policy, including specific applications to the Chinese reform process.[2] Rodrik (2006) argues that government policies creating distortions in favor of more advanced industries had an important role in the success of Chinese reforms. Dewatripoint and Roland (1995) and Rodrik (2004) argue that, through experi-

[1] Wei (1993) uses two samples: the first has 434 cities but only a limited time variation from 1988-1990. The second sample includes fewer cities (74) and covers the period 1980-1990.

[2] See Perkins (1988), Naughton (2007), Brandt and Rawski (2008), and Xu (2011).

mentation, the state can generate information about the potential of different sectors. Our findings are broadly consistent with these theses. Finally, our study has some similarity in both the methodology and motivation with Aghion *et al.* (2008) studying the effect of industrial policy (the demise of the License Raj) in India. Similar to our study, they exploit the fact that the reforms were staggered across time and sectors. However, different from our study, they emphasize the interaction between the reform and state-level characteristics of the labor market. Moreover, they study an episode of pure liberalization (delicensing) while China's industrial policy also entails proactive policy elements (tax credits, subsidies, etc.).

The rest of the paper is structured as follows. Section 2 provides an overview of the institutional background of economic reforms in China with special focus on SEZ. Section 3 describes the data sources and the sample. Section 4 discusses the empirical strategy and the main results. Section 5 performs a variety of robustness checks. Section 6 concludes.

# 2 China's Economic Reforms and Institutions

Since its establishment in 1949, the People's Republic of China relied on rigid economic planning. The State Planning Commission, a division of the State Council, was in charge of economic development. The two decades preceding Mao's death in 1976 were characterized by low and volatile economic growth, and by an intense social turmoil. The reformist political leadership that won the battle for Mao's succession in 1978, led by Deng Xiaoping, faced the desperate need for measures to reconstruct social cohesion and revitalize the economy. There were, however, no existing blueprints showing how to proceed. Learning-through-experiment became then the guiding principle of economic reforms. As Deng put it: "one has to grope for stepping-stones as he crossed the river".

The first policy breakthrough happened in rural areas, where agricultural production until then had been carried out in collective communes. Under a new production system which was later called the Household Responsibility System, farmers were entitled, after fulfilling their procurement quota, to the rest of their agricultural output. The new system was first implemented in Anhui and Sichuan provinces and extended to the whole country by the end of 1982. It was a major 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, and that industrialization necessitated opening up China to foreign investments. However, the reformists' plans to open the economy met the strong resistance of the conservative faction of the Communist Party (CCP) central committee. To the conservative ideologists, renting China's land to foreign companies and allowing them to exploit China's cheap labor would mean selling out China and exposing it to the influence of western ideologies.

**1980-1984**

The establishment of SEZ resulted from the 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. These 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. 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%) (see Wei 1993).

The location of the zones was carefully chosen. First, all were located in cities on the southeastern coast of China, far away from the political center Beijing. There, 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. 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 overseas Chinese through kinship and trade.

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 a 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 proving successful.

**1984-1991**

The success of the SEZ was remarkable: between 1980 and 1984 Shenzhen grew at an annual rate of 54%, and in 1984, the four SEZ alone 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 SEZ strengthened the reformist faction in the CCP and softened the position of the conservative leaders. In 1984, 14 coastal cities were granted the right to build Economic and Technological Development Zones (ETDZ). The ETDZ shared most of the policies and privileges granted earlier to the initial four SEZ. 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 celebrated tour to southern China, including stops at the SEZ of Shenzhen and Zhuhai, to mark the end of a period of political instability and to restate the commitment of the CCP to the reform process. 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 plan to grant the five inland cities along the Yangtze River, nine border cities and all thirty of the provincial capital 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 zone, 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 comprised 54 ETDZ, 53 HIDZ, 15 Bonded Zones (BZ) and 60 Export Processing Zones (EPZ).[3] 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 a common strategy for the local government to attract FDI and foster local economic growth. Through shuffling local officials across different regions, the governments diffused the knowledge and experiences accumulated in the early zones to help develop new SEZ (Xu 2011).

Besides the state-level zones, a large number of lower-level zones also were established during the same period. On the one hand, 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 into development zones 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 came to an end, 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 were abolished following the investigation, including all zones at a lower than province-level and several 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 significant freedom to design new institutions. Successful innovations were retained and extended to later waves of development zones (see Yeung *et al.* (2009)). Gradually, the institutional structure of the SEZ became stable. Policy treatment became uniform across all state-level ETDZs and HIDZs. At 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.

[3] BZ were mainly free trade zones. Most of the EPZ were established within existing SEZ. 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 SEZ (a label we use to distinguish the early created special zones from the broad notion of 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). They all 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 along several dimensions. First, they are administered by different authorities. 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 until 2007, and are now under the control of the Ministry of Commerce.

Second, the zones reflect different aspects of the development plan. The comprehensive SEZ, located in Shenzhen, Zhuhai, Xiamen, Shantou, Hainan, Shanghai and Tianjin, are the largest in scale and enjoy the most autonomy among 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 goals with the comprehensive SEZ, such as attracting FDI and boosting export, only on a smaller scale. Although the institutional innovation was more active and frequent at the early stage of the reform process, even today the comprehensive SEZ and ETDZ are 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: small and closed areas where import and export can take place at a faster speed. They are all located in coastal port cities or border cities, which also helps 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-year history of the economic reform. In some cities, province-level development zones were established before the state-level zones. Despite some commonalities, there are fundamental differences between state-level and province-level SEZ suggesting that their effects might be highly diverse. First, province-level zones include areas where the special status meant *de facto* almost nothing. This causes a rampant measurement error problem that is much less severe for state-level zones.

Second, the preferential economic policies granted to the province-level zones have been heavily 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 competition between the zones and the waste of land resources (State Administration of Taxation 2004). In reality, the policy treatment of the state-level zones normally included both the policies usually accorded to provincial zones and farther-reaching measures granted under the direct control of the central government.

Another difference is that in many cases province-level zones targeted specific industries (often catering to local interest groups). In contrast, the most important policies of the state-level zones, such as tax and custom duty deduction, were generally industry-blind. If there are exceptions to this principle, they reflect a general strategy of the government (such as the promotion of the development of particular industries nationwide) rather than the influence of local interests: for instance, in recent years state-level zones have favored high-tech industries.

In Table 1, we list the number of state-level and province-level development zones and their average share of industrial output in three coastal provinces hosting a large share of SEZ. The data are from WEFore (2010) for the year 2009. All three provinces have a larger number of province-level than of state-level zones. However, the state-level zones account for a far larger share to industrial output. We will show that, empirically, state- and province-level zones have very different effects on economic development at the city level.

# 3 Data

In this section, we describe the variables we use in the empirical analysis. Our data come from two main sources. First, the official statistics from the National Statistics Bureau of China (NSB) including GDP, electricity consumption, population, education, government spending, and land area. We also use the light intensity data from weather satellites as a proxy for GDP. More detailed information about our data sources is provided in the appendix.

The main unit of analysis is the core urban area of a *prefecture-level city,* an administrative division ranking below a province and above a county in China's administrative structure. A prefecture-level city comprises a core urban area (corresponding to the standard notion of city area) and a large surrounding area that may include rural areas, other smaller cities, towns and villages. Since SEZ are located in core urban areas, and the NSB reports separate statistics for the core and periphery of each prefecture-level city, we focus on the urban core of prefecture-level cities, unless we state otherwise. A more detailed discussion and motivation of this choice of the unit of analysis is deferred to the appendix. For simplicity, henceforth, we refer to the the core urban area of a prefecture-level city as a *city* and to the whole prefecture-level city area as a *prefecture*.[4]

## 3.1 Main Variables

We denote by $i$ the city, by $p$ the province, and by $t$ the year.

### 3.1.1 Dependent Variables

- $\log GDP_{ipt}$ is the logarithm of nominal GDP at the city level from the *China City Statistical Yearbooks.*
- $\log \frac{GDP_{ipt}}{L_{ipt}}$ is the logarithm of nominal GDP per capita at the city level. The population $L_{ipt}$ is taken from the *China City Statistical Yearbooks.*
- $\log Electricity_{ipt}$ is the electricity consumption at the city level from the *China City Statistical Yearbooks* (available for the same set of cities as GDP). It measures the use of electricity for household consumption and industrial production and is a proxy for the level of economic activity.
- $\log Light_{ipt}$ is the average light intensity at the city level, another proxy for economic activity. In the data provided by the National Geographical Data Center, light intensity is measured on each

[4] In the *China City Statistical Yearbooks*, the *prefecture* is called "di ji shi" and the *city* is called "shi qu".

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.

- $\log \frac{K_{ipt}}{L_{ipt}}$ is the logarithm of physical capital per capita. The physical capital stock $K_{ipt}$ is constructed with the perpetual inventory method. To construct the physical capital stock, we take the data on new investment for the period 1988-2010 from the *China City Statistical Yearbooks,* and assume an annual depreciation rate of 8%. 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*. The population $L_{ipt}$ is taken from the *China City Statistical Yearbooks.*

- $\log h_{ipt}$ is the logarithm of average human capital, constructed using average educational attainment of population over the age of 6. The educational attainment data comes from *China Population Census*.

- $\log A_{ipt}$ is the logarithm of TFP, constructed as a standard Solow residual.

#### 3.1.2 Explanatory Variables

Our main explanatory variables are indicators for the presence of a SEZ. More precisely, for each of the different types of SEZ we construct a dummy, $I\_Reform_{it}$, which switches on (i.e., takes the unit value) in the year *after* the establishment of a zone and retains the unit value 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 for cities that never host a zone $I\_Reform_{it} = 0$ for all $t$. We also construct separate dummies for each lag from the reform year, as discussed in more detail in the empirical sections.

#### 3.1.3 Primary Control Variables

We use two main control variables from the *China City Statistical Yearbooks*. First, the geographic size of the city, to which we refer as land area measured in square kilometers. This variable is available at both the city and prefecture level, and varies over time reflecting changes in the legal city boundaries during the reform period. The second control variable is population, which again we observe for each city and prefecture. Although population is partly endogenous to the establishment of a SEZ, we find it useful to filter out the part of the SEZ's effect on GDP that originates from mere population changes. In Section 5 we add to the list of control variables the expenditure of the local government, also from the *China City Statistical Yearbooks*.

#### 3.1.4 Fixed Effects

We include in all regressions two sets of dummies in order to control for unobserved heterogeneity. Time-invariant city characteristics are 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.

### 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 provides a consistent measurement of GDP across cities and years. Since Chinese price data are regarded as somewhat unreliable (see, for example, Young 2003), we opt to use nominal data. Time invariant differences in price level across cities and time varying inflation differences across provinces are absorbed, respectively, by city and province×time fixed effects. This approach would be problematic if inflation rates differed significantly across cities within each province. The main concern is that the SEZ treatment might increase systematically local inflation. We check if there are differences in inflation rates between treated and non-treated cities in those years for which real GDP data are available from the NBS. More precisely, we compute an implicit city-level deflator using the data on nominal and real GDP, and compare it between cities with and without a SEZ. We find that, within each province, cities with a SEZ did not have higher inflation.[5] As an alternative strategy that avoids relying on prices altogether, we use electricity consumption (in GWh) and light intensity as proxies for the level of economic activity.

### 3.3 Sample

The sample period is 1988-2010 for the *China City Statistical Yearbooks* and 1992-2010 for the light data. For this period, we have a consistent definition of city borders, and information on the main variables of interest.[6] We focus on 276 cities, excluding 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 for only one city, and the province-level municipalities, including Beijing, Chongqing, Shanghai and Tianjin, because our set of province-time fixed effect would absorb all variation in GDP.

## 4 Empirical Strategy and Results

In this section, we discuss the econometric strategy and the main results. We use a difference-in-difference estimator exploiting the variation in economic policy across cities and years following the establishment of SEZ. The main dependent variable is GDP at the city level, which we measure in three alternative ways: first, from official statistics (this section), then using light intensity and electricity consumption 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. Our policy variable, the establishment of SEZ, is illustrated in Figure 1. This figure shows the time evolution of the shares of cities hosting the different types of zones in the balanced sample. The two most important types of zones are HIDZ and ETDZ with shares reaching 31% and 24% in 2010, respectively.

[5] 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, although the difference is not statistically significant. The data on real GDP of the prefecture (instead of the city) dates back until 1996. When using this longer time series, we find that prefectures with a SEZ had an average yearly inflation rate of 1.8%, while prefectures without a SEZ had an average of 2.3%. The difference is again not statistically significant.

[6] During the period of economic reforms, there was also a reform of the administrative levels and borders of cities. Our data from the National Bureau of Statistics provide a consistent definition across cities and time.

Two types of zones existed before the start of our sample: the comprehensive SEZ, established in 1980, and a few early ETDZ, established in 1984. ETDZ and HIDZ are altogether the most frequent zone types. We also consider Export Processing Zones (EPZ) and other less frequent types of zones (e.g., BZ and BECZ), introduced in cities that already hosted either ETDZ or HIDZ. We control for province-level zones, but we do not combine these with our state-level SEZ because these are more limited in scope and highly heterogeneous, as discussed above.

## 4.1 Baseline Specification

Our baseline specification is a city-level panel regression whose dependent variable is the log of GDP. The main explanatory variables are reform indicators switching on in the year after part of a city's territory is granted the status of SEZ.[7] 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 allow each city to be subject to multiple treatments. All regressions control for city fixed effects and province-time interaction dummies. Standard errors are clustered at the city-level. More formally, we run regressions of the form

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

where $y_{ipt}$ is log nominal GDP, $\phi_i$ is a city fixed effect, $\gamma_{tp}$ 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 effects of reforms are identified 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.

The econometric specification in (1) restricts the treatment effect to a shift in the after-reform GDP level path, namely, in reformed cities the GDP *level* (or trend) is allowed to shift whenever the reform indicator switches on. This specification is clearly restrictive. One might expect reforms to have cumulative effects on development, such as temporary or even permanent changes in growth rates. For this reason, we explore below 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 would change GDP mechanically, possibly at the time of the introduction of a SEZ. The size of a city's land area has, as expected, a positive effect on GDP. Its inclusion reduces the treatment effect, but this remains large (14.7%) and highly significant. In column (3) we add the log population of the city center as a further control. Population has, as expected, a positive coefficient, and its inclusion causes land area to lose explanatory power. The treatment effect falls slightly but remains large (12.7%)

[7] Including the year of the reform in the dummy does not alter the baseline results significantly.

and highly significant. Finally, in column (4) we use GDP per capita as our outcome variable. The estimated reform effect is now 11.7%, highly significant. In columns (5)–(8) we repeat the analysis for the sub-sample of inland provinces. In this sub-sample, cities were granted the status of SEZ on the basis of administrative criteria, such as being a provincial capital.[8] This is an interesting sub-sample since it involves less selection, as discussed above. To mitigate selection concerns even further, we exclude from the inland sub-sample cities that were granted the status of SEZ in spite of not being provincial capitals. Thus, the restricted inland sample only contains provincial capitals (treatment group) and cities that were never granted the SEZ status (control group). Columns (5)–(8) in Table 3 show that the results are largely robust to this sample restriction.[9] The coefficient of interest is positive and significant, and even larger than in the total sample.

## 4.2 Pre-reform Trends

A concern with the results of Table 3 is that cities hosting SEZ might already have been on a higher-growth trajectory – or might even have been selected precisely because of their promise of success. The focus on inland capitals alleviates such concerns. However, the year in which capitals were assigned to the treatment group may not be random. Moreover, provincial capitals may be a special group *per se*.

We address this point through 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.[10] 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 the control group.[11]

Second, we consider a more flexible specification allowing treated cities to have different time trends from the non-reformers. This addresses the potential worry that in our baseline specification the positive effect of SEZ might arise spuriously due to the omission of pre-existing trends. The new specification allows the GDP of cities that host, at some point, a SEZ to have a linear time trend that differs from the

[8] In the sub-sample of inland cities, 44 cities were granted SEZ status. Of these, 18 were provincial capitals.

[9] Arguably, inland capitals is *per se* a special group. Since the selection of treated cities was based on an administrative criterion (rather than on unknown, possible heterogenous criteria), we can control for features making capital cities different from the control group, such as infrastructure and education, and allow the treatment effect to depend interactively on such features. In a regression not reported, available upon request, we find that including these interactions does not alter significantly the main treatment effect which remains in all cases highly significant. The only significant interaction effect is that with the number of universities, whose sign is positive.

[10] We also explored longer lags. There is evidence of some marginally significant effects at the five-year lag. However, lags longer than three years are identified out of a significantly smaller set of reforming cities (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 is only identified out of 3 cities.

[11] Note also that the earliest zones ("comprehensive SEZ") introduced before 1989, likely the most selected group, are either excluded or exhibit no time-variation in the policy indicators in our sample period. Thus, they play no role in the identification of the treatment effect.

control group's already before the reform. In some specifications, we even allow this trend to undergo a structural break at the time when the reform indicator switches on. More formally, we consider the following specification:

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

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., 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. 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).

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 (6) 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 (6) does not distinguish pre- and post-reform periods. Thus, in columns (3) and (7) 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 coefficient of the pre-reform trend, $\hat{\alpha}_2$, is small and only marginally significant in the full sample, while it is insignificant in the inland sample. The post-reform trend, $\hat{\alpha}_3$, is 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 the treatment and control groups.

The specification of columns (2)–(3) and (6)–(7) – allowing for permanently diverging paths – may be too extreme. We consider, then, an alternative specification allowing SEZ to have a non-linear effect of the SEZ relative to the pre-reform trend. To avoid an overparameterization, we omit the level shift, and we estimate the following alternative econometric specification:[12]

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

[12] It would be possible to include also the term $\alpha_1 I\_Reform_{it}$ to this specification. However, it is 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).

The regression results from this specification are provided in columns (4) and (8). In both cases, we find that $\hat{\alpha}_3 > 0$ and $\hat{\alpha}_4 < 0$, implying that the SEZ are associated with an acceleration of growth in the immediate post-reform years, but that the acceleration dies off in subsequent years. The coefficients are both 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 contrast, in the restricted sample of inner cities 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 in the full sample, but not in the restricted inland sample. In summary, this specification suggests that the effect of SEZ is a significant gradual increase in the GDP level, rather than a permanent increase in growth (i.e., a *linear* trend break of the treated cities after reforms).[13]

### 4.3 Lagged Effects of SEZ

Motivated by the findings in the previous section, we now perform a non-parametric analysis of the effects of the reform with the aid of a model that imposes no functional form restrictions on post- (and pre-) reform effects. All effects are captured by separate lag- or lead-specific dummies. 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 many years before year $t$ city $i$ became the host of a SEZ. Negative values measure how many years ahead of $t$ city $i$ will be reformed. Note that this specification allows us to identify some of the lagged effects out of reforms that took place before 1988. For instance a city that hosted its first SEZ in 1986 will have variation for all leads ranging from 2 to 24 years. In our baseline specification, instead, such a city would display no within variation, and the reform indicator would be collinear with the city fixed effect. In our sample, the maximum number of post-reform leads, $J_F$, is 26, corresponding to a single city which hosted its first SEZ in 1984. We construct these indicators also for the year of reform and the three years prior to the reform (i.e. $J_B = 3$), so we can test whether reforming cities already had a significantly different performance prior to the establishment of the first zone.[14] 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 lead and lagged effects of the treatment n years past the reform (for instance, n=10 measures the effect ten years past the introduction of a SEZ). This specification confirms the results of the previous section. In particular, there is a break in the GDP path a year after the reform, followed by a temporarily higher growth rate that levels off after about ten years. The size of the effects are comparable to those in the previous section.[15] There is only some marginal, statistically insignificant evidence of a higher GDP growth in the three years before the

[13] Clearly, the quadratic model is not a correct specification itself, since it would imply a negative long-run effect of SEZ. Given the short sample, the data only capture the increasing part of the quadratic relation. See the next section for a more general specification.

[14] 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, these indicators are marginally significant, but identified by only 27 observations.

[15] 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.

reform, indicating some minor positive selection. Note that the standard errors increase after nineteen years after the establishment of the zone (corresponding to the vertical line added to each figure). This is due to a significant drop in the number of observations, since many cities were reformed in 1991 and 1992.[16]

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 9-12 years after the reform are statistically different from zero.[17] In section 5.7, we show that if residuals are clustered at the province×years of reform (instead of city) level, the effects 7-18 years are statistically significant in the inland sample.

## 4.4 Different Types of SEZ

In this section, we attempt to disentangle the effects of the 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 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 those 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.[18] 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 build up gradually during about ten years and then level off, we investigate 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 approach in section 4.3 is quite demanding. Nevertheless, the resulting picture is reasonably clear. Figure 8, which can be found in the Appendix, plots the coefficients of the different types of zones (estimated in the same regression) over the years since reform. The first panel shows that the pattern for ETDZ looks remarkably similar to that of Figure 3 (first zone reformed). The second panel shows that HIDZ also display a concave pattern, although the effect appears to decline after lag 10.[19] EPZ and OtherTypes show a more mixed picture (the two lower panels in Figure 8).[20] The standard errors are very large

[16]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.

[17]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.

[18]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.

[19]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.

[20]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

and the effects typically are imprecisely estimated and statistically insignificant. In summary, most development effects appear to stem from ETDZ and HIDZ.

## 4.5 Decomposing the Effects of the SEZ

In the previous sections, we document that the establishment of SEZ has a positive effect on GDP at the city level. In this section, we investigate the channel through which the zones promote growth. To this aim, we decompose GDP per capita into the following input factors: physical capital per capita, human capital and TFP. We then estimate how the establishment of a SEZ affects each of the three components.

Following Hall and Jones (1999) and Caselli (2005), we perform a level-decomposition exercise based on the standard Cobb-Douglas production function $Y = AK^{\alpha}(hL)^{1-\alpha}$. More formally,

$$\log \frac{Y_{ipt}}{L_{ipt}} = \log A_{ipt} + \alpha \log \frac{K_{ipt}}{L_{ipt}} + (1-\alpha)\log h_{ipt} \tag{4}$$

where $A$ denotes total factor productivity, $\frac{K}{L}$ the physical capital per capita, and $h$ the human capital. We use the local population size to proxy the size of labor force and use the average educational attainment of the population to proxy the human capital. 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 decomposition equation 4 is used sequentially as the dependent variable. Column (1) shows that the establishment of a SEZ has a significantly positive effect on GDP per capita. Becoming the host of a state-level SEZ is associated with a 13.8% increase in the city's GDP per capita relative to non-reformed cities in the same province. When restricting the sample to inland cities, we find the effect on per capita GDP to be 13.1% and also highly significant, as shown in column (5). The estimated effect on the capital-labor ratio is displayed in column (2) and column (6). Column (2) suggests that becoming a host of a state-level SEZ increases the physical capital per capita of that city in the full sample by 14.8%. The effect on the capital labor ratio is more prominent in the inland sub-sample, as shown in column (6) – the point estimate is 24.4% and highly significant. The estimated effect on TFP in the full sample is 8.3% and highly significant. On the contrary, the SEZ seems to have a smaller (4.4%) and insignificant effect on TFP in the inland sample (column (8)). The SEZ does not have a significant effect on human capital in either of the two samples (column (3) and (7)).

Taking these results at face value suggests that the establishment of SEZ has a major positive effect on investments and leads to capital deepening. The capital deepening effect is more prominent in the inland sample while the full sample shows a more balanced picture of development, where both the capital-labor ratio and TFP experience significant increases after the establishment of a state-level zone.[21] There is no selective migration effect, i.e., the SEZ do not seem to attract better educated workers.[22]

explain their upward trend (though insignificant).

[21] However, as argued in Klenow and Rodrigez-Clare (1997) and others, the increased capital-labor ratio should be attributed to increased TFP.

[22] Note that our specification seeks to identify differential improvements in TFP in cities with SEZ compared to cities without SEZ. Other studies that investigate the differential effect of SEZ include Schminke and Van Biesebroeck (2013) and Wang (2013). Schminke and Van Biesebroeck (2013) 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. Wang (2013) finds an effect

One important *caveat* here is that the quality of the data on human capital is low. The only data available at the city-level for education attainment are from 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 reduces the accuracy of our measures of human capital and TFP.

The results on human capital are somewhat different if one evaluates the effects of SEZ by using only the three years for which direct observations of the education attainment are available from the census data. Rather than aggregating the existing information to obtain an estimate of the average years of schooling, 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 shown in Table 8. Columns (1) and (2) suggest that after changing to SEZ 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.[23] Columns (3) and (4) show that establishing a SEZ has no impact on the share of the population with a low educational attainment (elementary degree or less). Second, SEZ appear to decrease significantly the share of the population with junior and senior high school degrees (columns 5-6). Finally, the share of college graduates in the whole population increases significantly by 3.1% and 3.8%, respectively, in the full and inland sample (columns 7 and 8). In summary, the main finding is that the establishment of SEZ is associated with an increase in the share of college graduates, at the expense of the share with intermediate education.[24] 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 obtain higher education.

Figures 5, 6, and 7 display the reform effect on GDP per capita, physical capital per capita and TFP over time. The specification employed here is the analogue to that of Figure 3. The pattern for GDP per capita in Figure 5 is very similar to that of GDP in Figure 3. The path of GDP per capita shows a structural break one year after the reform. GDP per capita grows at a temporarily higher rate for about 10 years and remains at a permanently higher level afterwards. We see a similar concave post-reform path for physical capital per capita in Figure 6. The path of physical capital per capita also breaks one year after the reform and only becomes statistically significant 5 years after the reform, presumably because it takes time to build up the physical capital. After 19 years, as observed before, the effects are estimated very imprecisely. The path for TFP also features a break one year after the reform takes place and the break becomes significant 6 years after reform. The effect on TFP becomes insignificant 16 years after the reform (see Figure 7).

# 5 Robustness

In this section we perform robustness analysis. First, we test whether there is evidence of negative or positive spillovers to neighboring locations. Second, we repeat the baseline regressions using alternative

of SEZ on TFP growth after a lag of six years. It should be noted that TFP on average may still be increasing even if there is no differential effect for SEZ. For example, Brandt and Zhu (2010) found that increases in TFP in non-state non-agricultural firms contributed significantly to growth.

[23] 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.

[24] 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.

proxies for GDP. Third, we test the robustness of the results to the inclusion of additional control variables and years. Fourth, we perform a variety of placebo exercises. Finally, we consider alternative clustering strategies.

## 5.1 Local Spillovers

We have focused so far on the main urban center (labelled as the "city") of each prefecture which is the area where *all* state-level SEZ in our sample were established. Recall that the urban center is a subset of the prefecture, that also include a large peripheral area comprising smaller cities, towns, villages and rural areas. In this section, we investigate the existence of local spillovers by studying whether the policy has 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 (Panel A of Table 9); and then the GDP of the prefecture's periphery, i.e., the whole prefecture excluding the city (Panel B of Table 9).[25] Panel A shows that the effects at the prefecture level are of comparable magnitude to those obtained for the city only. Panel B shows that the results hold up when we consider only the periphery of the prefecture.[26]

The results of this section suggest that the positive effects of the SEZ did not come at the expenses of surrounding areas. Rather, it looks as if the SEZ brought positive spillovers to the periphery of the prefecture of which the host city is part. However, our analysis cannot rule out negative spillovers across prefectures.

## 5.2 Satellite light as an Alternative Measure of GDP

Chinese price-level data are generally regarded as problematic, especially at the local level. Our empirical methodology has the advantage of not relying 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, one might worry that cities within each province might have experienced different inflation rates. In particular, our estimated treatment effect would be biased upwards if the establishment of a SEZ systematically brought about higher inflation. As discussed above, the fact that the existing price data do not suggest that the establishment of a SEZ is associated with higher inflation is reassuring in this respect. However, one might also worry 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.

To address these issues, in this section we use light intensity measured by weather satellites as an alternative proxy for GDP. A number of recent papers have argued that light intensity at night measured by weather satellites can be used as a proxy for GDP.[27] Most economic activities such as production, transport, and consumption produce light as a by-product. Therefore, light intensity is positively correlated with the intensity of local economic activities. We calculate the average light intensity within the geographical boundaries of cities and use this as a proxy for economic activity.

[25] On average, the GDP of the prefecture (including the city) is about twice as large as that of the city only. The size of the population of the prefecture is about four times as large as that of the city, and the size of the land area is about eight times as large.

[26] Land area and population are adjusted accordingly. There is a small drop in the number of observations, since in some cases the city coincides with the prefecture, and thus there is no periphery.

[27] Elvidge *et al.* (1997) are 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) and Hälg (2012) discuss the use of light data in Chinese prefecture-level cities.

In column (1) of Table 10 we re-run our baseline regression with the logarithm of the average light intensity as the dependent variable.[28] Unfortunately, light intensity is only available since 1992, and only one-third of the (first) SEZ were established after that year. Moreover, even for later reformers we lose annual observations that would be useful for a precise estimation of the within-city effect of SEZ establishment. The loss of precision is confirmed by the observation that if we run the baseline regression of section 4 with GDP as the dependent variable for the post-1992 period we obtain a point estimate of 0.042, statistically insignificant. Yet, when GDP is proxied by satellite light, we find that the establishment of a SEZ triggers a 5.2% increase in light intensity. The point estimate for the inland sample is similar in magnitude, albeit statistically insignificant.

We also check the robustness of our results by using electricity consumption as a proxy of economic activity (see, e.g., Rawski 2001). Data on electricity consumption by households and firms are reported in the same statistical yearbooks as GDP, and is available at the city level. In column (3) 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 is associated with an 11% increase in electricity consumption. 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 GDP level.[29]

## 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 government may increase the transfers to cities when these are granted SEZ status. Unfortunately, we have no direct information on such transfers. SEZ 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 therefore are 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, and is in fact larger than the point estimates in Table 3.

[28] 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.

[29] However, we find no significant effect in the inland sample. We suspect that this is due to the poor quality of electricity data in this subsample, for which we have no explanation. We calculated the correlation between GDP (data) and electricity separately in four sub-samples: inland reformers, inland non-reformers, coastal reformers and coastal non-reformers. The correlation is high and significant in all subsamples except for that of inland reformers where the elasticity of GDP with respect to electricity is very low (0.02) and statistically insignificant. Interestingly, the correlation between GDP (data) and satellite light intensity is instead consistent and significant across the four sub-samples, suggesting that the source of problems is not the GDP statistics, but rather the electricity data.

## 5.4 Earlier GDP Data

Our main analysis focuses on the period 1988-2010 for which the NBS provides a consistent measurement across cities and years. This conservative approach entails the cost of losing variation in the reform variable, since some SEZ were established before 1988. We re-estimate our baseline specification for a subset of cities for which GDP is also available for earlier years.[30] 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 reform effect estimated with this subsample is a 12.3% increase in the level of GDP, and the estimated coefficient is highly significant.

## 5.5 Population Data

In our analysis so far, we have used the population data from the City Statistical Yearbooks. To the best of our knowledge, these data cover only the registered population in the city, that is, people with "*hukou*". The existence of a large number of non-resident immigrant workers in the cities could potentially bias our estimation. To address this issue, we check first the CSY statistics against the population census that in principle should record the entire resident population at the city level. However, as noted above, census data are only available for three years (1990, 2000 and 2010) in our sample. We find that there is a gap between the two data sources. In particular, if the census is right, the population growth rate is overestimated by an annual 0.24% in non-reforming cities, and underestimated by 0.35% in reforming cities in the city statistics. The observation that the population is underestimated in the treatment group and overestimated in the control group is not surprising, as the treatment cities are likely to have attracted many *non-hukou* workers from the control group.

To test the robustness of our baseline results, we repeat the baseline regressions of Table 3 restricting our sample to the three census years and using population census data instead of population CSY data. Columns (1)-(2) and (5)-(6) of Table 12 simply replicate the results in Table 3 in the restricted sample. In column (3) and (7) of Table 12, where population is used as a control variable, the estimated effect of SEZ is found to be highly significant and of similar magnitude to that of the baseline specification in Table 3 (although the estimated coefficient drops from 0.18 to 0.13 in the inland sample). In the specification using output per capita as the dependent variable (column (4) and (8)), the effects of SEZ on per capita GDP are somewhat smaller than in the corresponding columns of Table 3 (the effect being 8% and the full sample and 10% in the inland sample). The estimates continue to be statistically significant, albeit only at the 95% confidence level in the full sample and at the 90% confidence level in the inland sample. It is important to note that by restricting the sample to only three years, we lose precision in the time variation of the treatment effect.[31] This could explain part of the reduction in the estimated treatment effect. In addition, the estimates including *non-hukou* workers should be regarded as a lower bound of the effect, since these workers on average carry a lower human capital than officially resident workers. All in all, our baseline results appear to be robust to using the resident population data from the census.

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[30] Namely the cities in the following provinces: Fujian, Guizhou, Hebei, Heilongjiang, Henan, Inner Mongolia, Jiangsu, Shaanxi, Shandong and Shanxi.

[31] Consider, for instance, columns (5)-(6), which involve no population data. The estimated coefficients in Table 3 are 0.27 and 0.21, respectively, whereas the corresponding coefficients in Table 12 are 0.22 and 0.15, repectively.

## 5.6 Placebo Analysis

Our estimation exploits the time and spatial variation in the establishment of SEZ. Since the establishment of the SEZ is staggered, but clustered in few years, there could be concern about 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 be present also 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 SEZ are assigned artificially to random cities. We repeat this exercise 1000 times. We find that in no case are the absolute t values and the R-squared of the placebo regressions larger than those of the true reform.[32] This suggests that the spatial distribution of SEZ indeed drives 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 the extent to which the time dimension 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 are higher when using the year of the true reform than in the placebo regressions in all but 1.8% of the cases.[33] This indicates that the actual year in which the SEZ were implemented is critical 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.[34] While the estimate for the true reform is always significant at 5%, the placebo reforms are significant in only 24% of the cases.[35] Overall, these placebo exercises strengthen our confidence in the empirical strategy used. Both the spatial and the time variation of the SEZ appear to be important for the results.

## 5.7 Alternative Clustering Strategies

In our main analysis we cluster standard error at the city level, to allow for observations within a given city to be correlated as well as for heteroskedasticity. Our results are robust to alternative clustering strategies. First, we cluster the standard errors by province and year of reform (i.e., the first year in which a city hosts a SEZ). This strategy takes account of the fact that the introduction of SEZ is highly clustered in time. Many HIDZ were introduced in 1991–92, and many ETDZ were introduced in 2001–03, implying that different cities in these years cannot be treated as independent observations. The results are essentially unchanged. Appendix Tables 13-14-15-16-17 and Appendix Figures 9-10 yield the analogues of Tables 3-4-5-6-7 and of Figures 3-4 under the alternative clustering strategy. The results are robust: the statistical significance of the coefficients of interests is even strengthened, and in a few cases coefficients that were marginally insignificant when clustering at the city level turn significant here. Most notably, this is the case of Appendix Figure 9, the analogue of Figure 3, capturing the lagged effect of reforms in the inland sample.

[32] The mean estimate of the placebo reform is -0.0004 and it is never significant and higher than the one of the true reform.

[33] The mean estimate of the placebo reform is 0.088. The placebo specification yields significant coefficients in only 5% of the draws. It yields higher point estimates than the specification with the true reform year in 5% of the draws.

[34] 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.

[35] The mean estimate of the true reform is 0.11 and the mean estimate of the placebo reform is 0.046.

We also run the regressions clustering standard errors at the province level (instead of province×year of first reform). This strategy is even more demanding, and runs into potential problems since we have only 28 provinces in the full sample and 18 provinces in the inland sample, and so the number of clusters is small. The results are robust to even this demanding approach. The coefficients of interests in Appendix Tables 18-19-20-21-22 and the lagged reform effects in Appendix Figure 11-12 remain significant with 2 exceptions, both of which occur in specifications using the inland sample. [36]

# 6 Conclusion

China has experienced an astonishing economic development over the past 30 years. The SEZ 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 estimates the effect of SEZ on local economic performance. We considered a number of specifications that control for unobserved heterogeneity at the city level and at the province-time level. The results suggest that the establishment of SEZ 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 sizeable and robust. We also find that the effect of the SEZ on output worked mainly through the acceleration of physical capital investment – although there is some indication of positive effects on TFP and human capital accumulation.

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 policies and institutions should be "appropriate" to the stage of development, and particularly to the stage of the process of technological convergence (Acemoglu *et al.* 2006). The Chinese reform process was characterized by a mixture of elements of market liberalization and an active role of government in promoting investment and technology adoption. Rodrik (2006) argues that the active role of the government was crucial for China's development because it supported a fast move towards more modern and productive sectors which have positive externalities on the whole economy. The results of our empirical analysis suggest that the industrial policy may have indeed been a catalyst of the development process. At the same time, the estimated effects are not quantitatively very large relative to the high growth rates experienced by China in this period. Therefore, it would be hazardous to conclude that SEZ were *the* most important component of the reform package.

Two limitations to recall are that (i) we cannot quantify the costs of the policy, and thus the judgment about the welfare effects of SEZ must remain suspended; (ii) by design, our methodology can only uncover differential effects. If the establishment of SEZ had positive spillovers outside of the areas where they were introduced, our estimates represent a lower bound of the actual effects. In spite of these limitations, we believe that our results provide a useful starting point for a realistic understanding of the effects of industrial policy in China.

[36] The exceptions are: policy indicators of state-leve zone in column (7) of Table 20 and column (7) of Table 21.

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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 offical 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.

Table 3: Baseline specification

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Post-reform indicator for any state-level zone | 0.190*** | 0.147*** | 0.127*** | 0.117*** | 0.268*** | 0.212*** | 0.181*** | 0.166*** |
| | (4.56) | (4.29) | (4.43) | (4.15) | (4.87) | (4.12) | (3.51) | (3.09) |
| Post-reform indicator for province-level zone | -0.000486 | -0.00706 | 0.000497 | 0.00412 | -0.0157 | -0.0244 | -0.00775 | 0.00319 |
| | (-0.02) | (-0.31) | (0.02) | (0.20) | (-0.55) | (-0.89) | (-0.28) | (0.11) |
| Log landarea | | 0.240*** | -0.0325 | -0.154*** | | 0.211*** | -0.0533 | -0.175*** |
| | | (7.73) | (-1.18) | (-7.66) | | (5.84) | (-1.22) | (-5.49) |
| Log population | | | 0.692*** | | | | 0.673*** | |
| | | | (12.22) | | | | (7.38) | |
| 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-year 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 | -0.00764 | 0.0247 | 0.0229 | 0.0221 | -0.00543 | 0.00564 | -0.0172 | -0.0283 |
| | (-0.22) | (0.71) | (0.82) | (0.81) | (-0.06) | (0.06) | (-0.21) | (-0.30) |
| Indicator for 2 years before any state-level zone | -0.0147 | 0.0255 | 0.0238 | 0.0230 | -0.0429 | -0.0221 | -0.0400 | -0.0487 |
| | (-0.41) | (0.71) | (0.84) | (0.84) | (-0.50) | (-0.25) | (-0.49) | (-0.54) |
| Indicator for 1 year before any state-level zone | -0.0252 | 0.0165 | 0.0161 | 0.0159 | -0.0572 | -0.0367 | -0.0536 | -0.0618 |
| | (-0.71) | (0.46) | (0.57) | (0.58) | (-0.64) | (-0.39) | (-0.64) | (-0.67) |
| Indicator for year of any state-level zone | -0.00290 | 0.0213 | 0.0232 | 0.0241 | -0.0816 | -0.0599 | -0.0742 | -0.0811 |
| | (-0.08) | (0.55) | (0.74) | (0.79) | (-0.90) | (-0.64) | (-0.85) | (-0.84) |
| Post-reform indicator for any state-level zone | 0.180*** | 0.165*** | 0.144*** | 0.134*** | 0.229** | 0.188* | 0.143 | 0.121 |
| | (3.32) | (3.37) | (3.83) | (3.69) | (2.52) | (1.91) | (1.57) | (1.20) |
| Post-reform indicator for province-level zone | -0.000807 | -0.00645 | 0.00110 | 0.00471 | -0.0166 | -0.0307 | -0.00860 | 0.00215 |
| | (-0.03) | (-0.28) | (0.05) | (0.23) | (-0.57) | (-1.10) | (-0.31) | (0.08) |
| Log landarea | | 0.241*** | -0.0321 | -0.154*** | | 0.197*** | -0.0536 | -0.175*** |
| | | (7.75) | (-1.16) | (-7.63) | | (5.25) | (-1.23) | (-5.51) |
| Log population | | | 0.692*** | | | | 0.673*** | |
| | | | (12.26) | | | | (7.39) | |
| 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-year 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 | 0.127*** | 0.0825*** | 0.0837*** | | 0.181*** | 0.0858 | 0.0972* | |
| | (4.43) | (2.84) | (2.87) | | (3.51) | (1.52) | (1.89) | |
| Post-reform indicator for province-level zone | 0.000497 | 0.000738 | 0.000861 | 0.00219 | -0.00775 | -0.00668 | -0.00768 | -0.00996 |
| | (0.02) | (0.04) | (0.04) | (0.11) | (-0.28) | (-0.24) | (-0.28) | (-0.36) |
| Time trend of reformers (state-level) | | 0.00548** | 0.00595* | 0.00656* | | 0.00855* | 0.00262 | -0.000951 |
| | | (2.15) | (1.68) | (1.88) | | (1.69) | (0.26) | (-0.09) |
| Post-reform trend (state-level) | | | -0.000747 | 0.0156** | | | 0.00624 | 0.0450** |
| | | | (-0.19) | (2.25) | | | (0.53) | (2.58) |
| Sq. post-reform trend (state-level) | | | | -0.000740*** | | | | -0.00182*** |
| | | | | (-2.60) | | | | (-2.80) |
| 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-year 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 6: Effects of different types of zones

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Post-reform indicator for ETDZ | 0.220*** | 0.156*** | 0.120*** | 0.104*** | 0.239*** | 0.171*** | 0.0969* | 0.0581 |
| | (4.62) | (3.66) | (3.17) | (2.72) | (3.97) | (2.99) | (1.67) | (0.99) |
| Post-reform indicator for HIDZ | 0.117*** | 0.0794** | 0.0755** | 0.0736** | 0.122** | 0.0925* | 0.106** | 0.113** |
| | (2.64) | (2.19) | (2.45) | (2.40) | (2.57) | (1.80) | (2.18) | (2.26) |
| Post-reform indicator for EPZ | 0.0412 | 0.0361 | 0.0205 | 0.0131 | -0.0123 | 0.00190 | 0.0490 | 0.0738 |
| | (0.92) | (0.94) | (0.60) | (0.38) | (-0.15) | (0.02) | (0.64) | (1.03) |
| Post-reform indicator for OtherTypes | 0.0710 | 0.0583 | 0.0898* | 0.104** | 0.0827 | 0.174* | 0.213** | 0.233** |
| | (0.99) | (0.93) | (1.81) | (2.21) | (1.13) | (1.67) | (2.19) | (2.45) |
| Post-reform indicator for province-level zone | 0.00286 | -0.00532 | 0.00260 | 0.00649 | -0.0158 | -0.0297 | -0.00878 | 0.00223 |
| | (0.11) | (-0.24) | (0.13) | (0.32) | (-0.56) | (-1.08) | (-0.32) | (0.08) |
| Log landarea | | 0.233*** | -0.0361 | -0.159*** | | 0.187*** | -0.0535 | -0.180*** |
| | | (7.20) | (-1.30) | (-7.66) | | (4.83) | (-1.20) | (-5.55) |
| Log population | | | 0.686*** | | | | 0.655*** | |
| | | | (11.85) | | | | (6.76) | |
| 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-year 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 7: Decomposition of the effect

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Post-reform indicator for first state-level zone | 0.138*** | 0.148*** | 0.00343 | 0.0825*** | 0.131*** | 0.244*** | 0.00409 | 0.0442 |
| | (4.81) | (3.26) | (0.86) | (3.04) | (3.00) | (3.75) | (0.70) | (1.23) |
| Post-reform indicator for first province-level zone | -0.0127 | -0.0159 | 0.00242 | -0.0125 | -0.0145 | -0.0201 | 0.00448** | -0.00705 |
| | (-0.74) | (-0.53) | (1.44) | (-0.64) | (-0.67) | (-0.47) | (1.98) | (-0.26) |
| Log landarea | -0.218*** | -0.532*** | -0.0157*** | 0.0120 | -0.212*** | -0.490*** | -0.0157*** | -0.000134 |
| | (-6.41) | (-10.94) | (-4.58) | (0.36) | (-5.35) | (-8.97) | (-3.98) | (-0.00) |
| Dependent Variable | log(Y/L) | log(K/L) | log(h) | log(TFP) | log(Y/L) | log(K/L) | log(h) | log(TFP) |
| Sample | Full | Full | Full | Full | Inland | Inland | Inland | Inland |
| N | 5171 | 4521 | 4381 | 3970 | 3242 | 2744 | 2610 | 2320 |
| AR2 | 0.948 | 0.959 | 0.957 | 0.802 | 0.938 | 0.954 | 0.950 | 0.731 |

The dependent variables are the logarithms of real GDP per capita column (1) and (5), and the three decomposed components: logarithm of physical capital stock (column (2) and (6)), logarithms of average human capital (column (3)-(7)) and logarithm of TFP (column (4)-(8)), of the prefecture area. All specifications include land area, city fixed effect and the interaction of province-year 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). The regression is carried out for the full sample (column (1)-(4)) and restricted inland sample ((5)-(8)).

Table 8: Effect on human capital (census years only)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Post-reform indicator for first state-level zone | 0.173*** | 0.229*** | -0.00985 | -0.00544 | -0.0208*** | -0.0321*** | 0.0307*** | 0.0376*** |
| | (3.46) | (3.36) | (-1.58) | (-0.70) | (-2.93) | (-3.43) | (5.97) | (5.48) |
| Post-reform indicator for first province-level zone | 0.0993*** | 0.133*** | -0.0116* | -0.0190*** | 0.0103 | 0.0138** | 0.00129 | 0.00523 |
| | (2.84) | (2.87) | (-1.88) | (-2.92) | (1.62) | (1.99) | (0.35) | (1.01) |
| Log landarea | -0.269** | -0.194* | 0.0379*** | 0.0221 | -0.0148 | -0.00191 | -0.0230** | -0.0201* |
| | (-2.56) | (-1.76) | (2.66) | (1.53) | (-1.59) | (-0.22) | (-2.49) | (-1.97) |
| Log population | -0.139 | -0.267 | 0.0180 | 0.0484* | -0.00665 | -0.0332** | -0.0114 | -0.0154 |
| | (-0.83) | (-1.26) | (0.78) | (1.66) | (-0.44) | (-2.09) | (-0.72) | (-0.76) |
| Dependent variable | avg. sch. | avg. 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)), of the prefecture area. All specifications include land area of the prefecture, pefecture fixed effects and the interaction of province-year dummies as extra control variables. T-statistics are reported in parenthesis. Standard errors are clustered at the prefecture level. The sample includes 276 cities from 25 provinces in 1990, 2000 and 2010, when the population census were conducted (unbalanced panel). The regression is carried out for the full sample (column (1), (3), (5), (7)) and restricted inland sample ((2), (4), (6), (8)).

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*** | 0.118*** | 0.132*** | 0.136*** | 0.213*** | 0.186*** | 0.190*** | 0.197*** |
| | (4.72) | (4.05) | (4.80) | (4.85) | (3.07) | (3.39) | (3.65) | (3.63) |
| Post-reform indicator for province-level zone | 0.0236 | -0.00340 | -0.00858 | -0.0102 | 0.0272 | -0.00933 | -0.0144 | -0.0143 |
| | (1.01) | (-0.19) | (-0.50) | (-0.60) | (0.81) | (-0.37) | (-0.61) | (-0.60) |
| Log landarea | | 0.410*** | -0.0566 | -0.218*** | | 0.345*** | -0.0178 | -0.210*** |
| | | (12.89) | (-0.79) | (-6.35) | | (5.76) | (-0.22) | (-4.58) |
| Log population | | | 0.743*** | | | | 0.686*** | |
| | | | (7.67) | | | | (6.04) | |
| Sample | Full | Full | Full | Full | Inland | Inland | Inland | Inland |
| N | 5403 | 5329 | 5327 | 5327 | 2871 | 2637 | 2803 | 2803 |
| AR2 | 0.957 | 0.975 | 0.979 | 0.973 | 0.945 | 0.971 | 0.976 | 0.967 |

Panel B: Periphery Only

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Post-reform indicator for any state-level zone | 0.219** | 0.107*** | 0.143*** | 0.149*** | 0.310 | 0.123* | 0.184*** | 0.194*** |
| | (2.23) | (3.25) | (5.02) | (5.12) | (1.26) | (1.87) | (3.86) | (4.01) |
| Post-reform indicator for province-level zone | 0.0789 | -0.00561 | -0.0105 | -0.0113 | 0.0910 | -0.000238 | 0.00138 | 0.000197 |
| | (1.57) | (-0.25) | (-0.52) | (-0.55) | (1.37) | (-0.01) | (0.05) | (0.01) |
| Log landarea | | 0.878*** | 0.147* | 0.0287 | | 0.880*** | 0.216** | 0.0705* |
| | | (18.54) | (1.75) | (0.82) | | (11.66) | (2.09) | (1.92) |
| Log population | | | 0.860*** | | | | 0.816*** | |
| | | | (8.85) | | | | (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 (the prefecture less the city). All specifications include city fixed effects and the interaction of province-year 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: Light intensity and electricity consumption

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Post-reform indicator for any state-level zone | 0.0528** | 0.0544 | 0.111** | 0.00166 |
| | (2.15) | (1.11) | (2.27) | (0.02) |
| Post-reform indicator for province-level zone | -0.00854 | -0.0338 | 0.0331 | 0.0347 |
| | (-0.47) | (-1.22) | (0.97) | (0.81) |
| Log population | 0.0104 | 0.00175 | 0.535*** | 0.212* |
| | (0.49) | (0.04) | (4.90) | (1.70) |
| Log landarea | | | -0.0765 | 0.0158 |
| | | | (-1.57) | (0.27) |
| Dependent variable | log(Light) | log(Light) | log(Electricity) | log(Electricity) |
| Sample | Full | Inland | Full | Inland |
| N | 4708 | 2554 | 5200 | 2715 |
| AR2 | 0.836 | 0.818 | 0.804 | 0.758 |

The dependent variable is the logarithm of light intensity in columns (1)-(2); it is the logarithm of electricity consumption in columns (3)-(4); always at the city level. All specifications include city fixed effects and the interaction of province-year 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*** | 0.120*** | 0.107*** | 0.0983*** | 0.225*** | 0.177*** | 0.152*** | 0.134*** |
| | (4.23) | (4.09) | (4.30) | (3.89) | (5.20) | (4.13) | (3.61) | (2.87) |
| Post-reform indicator for province-level zone | -0.000446 | -0.00361 | 0.00166 | 0.00534 | -0.0125 | -0.0245 | -0.00772 | 0.00449 |
| | (-0.02) | (-0.18) | (0.09) | (0.28) | (-0.46) | (-0.92) | (-0.29) | (0.16) |
| Log gov. spending | 0.355*** | 0.267*** | 0.206*** | 0.167*** | 0.271*** | 0.234*** | 0.194*** | 0.164*** |
| | (7.26) | (7.48) | (6.65) | (5.60) | (6.69) | (6.03) | (5.74) | (5.17) |
| Log landarea | | 0.214*** | -0.0173 | -0.172*** | | 0.193*** | -0.0161 | -0.168*** |
| | | (7.99) | (-0.67) | (-9.14) | | (5.69) | (-0.42) | (-5.49) |
| Log population | | | 0.599*** | | | | 0.579*** | |
| | | | (10.81) | | | | (6.42) | |
| Sample | Full | Full | Full | Full | Inland | Inland | Inland | Inland |
| N | 4887 | 4875 | 4874 | 4874 | 2558 | 2554 | 2554 | 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-year 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 12: Baseline specification (census years and population)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Post-reform indicator for first state-level zone | 0.207*** | 0.162*** | 0.121*** | 0.0782** | 0.215*** | 0.153*** | 0.129** | 0.104* |
| | (4.60) | (4.21) | (3.61) | (2.44) | (3.54) | (2.85) | (2.57) | (1.96) |
| Post-reform indicator for first province-level zone | 0.0473 | -0.00655 | -0.0210 | -0.0361 | 0.0481 | -0.0186 | -0.0152 | -0.0114 |
| | (1.09) | (-0.20) | (-0.69) | (-1.08) | (0.87) | (-0.45) | (-0.41) | (-0.30) |
| Log landarea | | 0.365*** | 0.0999 | -0.178*** | | 0.349*** | 0.104 | -0.163*** |
| | | (9.88) | (1.07) | (-3.45) | | (8.71) | (1.26) | (-2.74) |
| Log population | | | 0.488*** | | | | 0.480*** | |
| | | | (3.24) | | | | (3.76) | |
| Census Years | Y | Y | Y | Y | Y | Y | Y | Y |
| Sample | Full | Full | Full | Full | Inland | Inland | Inland | Inland |
| N | 708 | 693 | 693 | 693 | 453 | 438 | 438 | 438 |
| AR2 | 0.979 | 0.986 | 0.987 | 0.982 | 0.977 | 0.986 | 0.988 | 0.983 |

The dependent variable is the logarithm of annual GDP at the prefecture level in columns (1)–(3) and in columns (5)–(7); it is annual GDP per capita at the prefecture level in columns (4) and (8). GDP is measured in current prices. All specifications include prefecture fixed effects and the interaction of province-year dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the prefecture level. The sample includes 276 cities from 25 provinces in 1990, 2000 and 2010, when the population census were conducted (unbalanced panel). The regressions in columns (5)–(8) are restricted to cities in 18 inland provinces (as defined in the appendix).The column (3) and (7) include logarithms of population from the census as control variable. The dependent variable in column (4) and (8), logarithms of GDP per capita, is computed with the census population data.

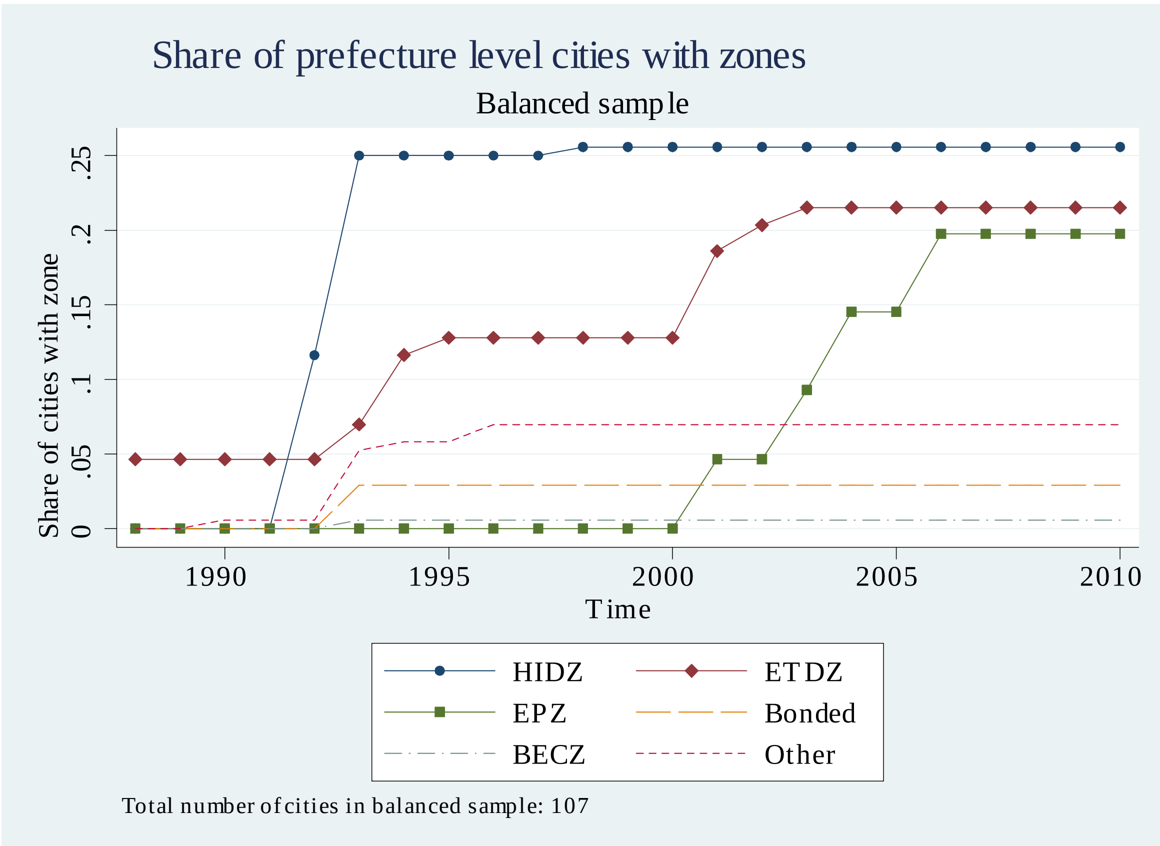


Figure 1: Share of prefecture level cities with different types of zones: The figure shows the share of cities which have different types of SEZ: Hightech Industrial Development Zones, Economic and Technolocial Development Zones, Export Processing Zones, Bonded Zones, Border Economic Cooperation Zones, and other types. The sample is restricted to 107 cities that are observed in all years between 1988 and 2010.

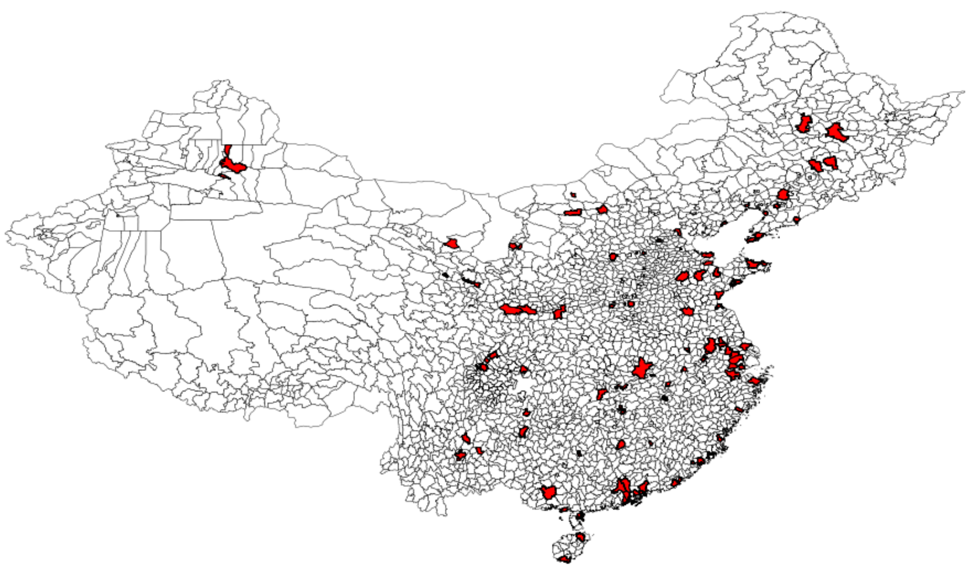

Figure 2: Location of treated cities in 2010: The map shows the boundaries of Chinese prefecture level cities and counties in 2010. The cities in our sample with at least one state-level SEZ in 2010 are marked in red (a city may have more than one zone).

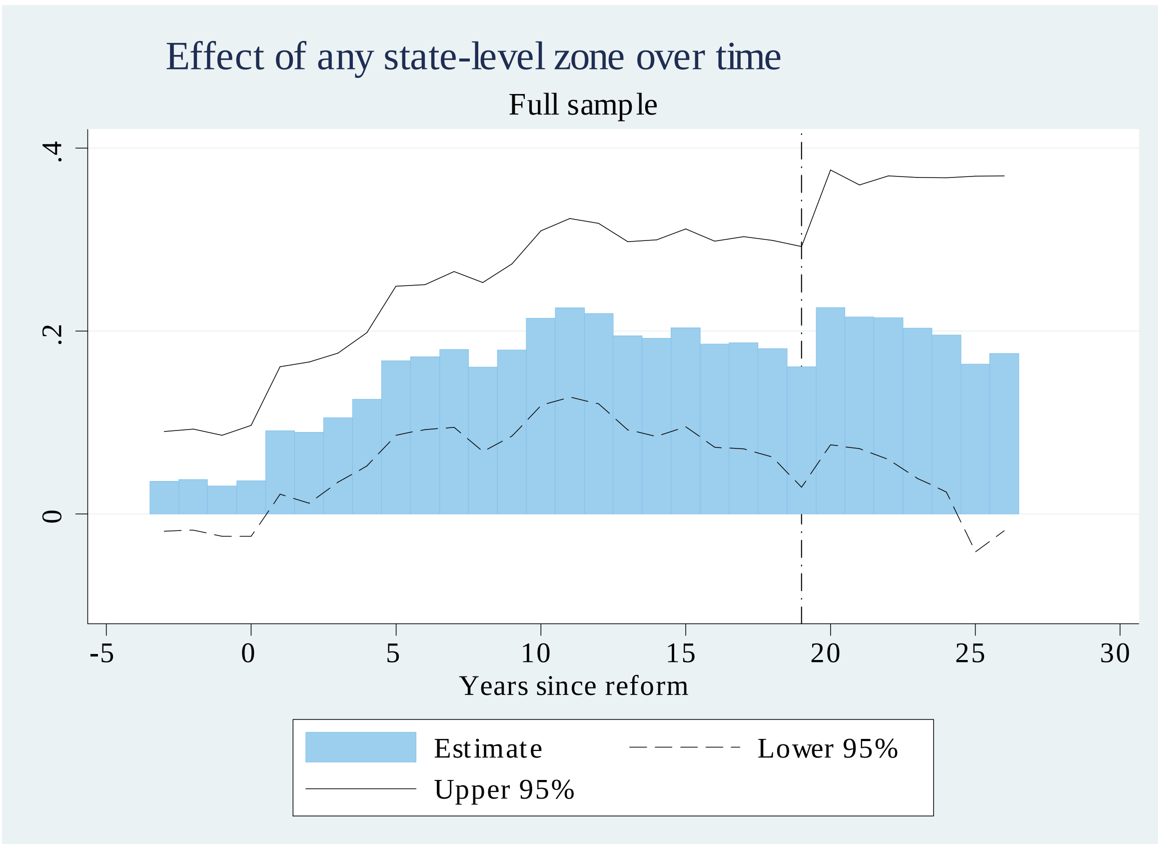


Figure 3: Reform effects over time: The bars show the coefficients of a regression of the logarithm of nominal GDP on indicators for years before and after the first zone. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 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.

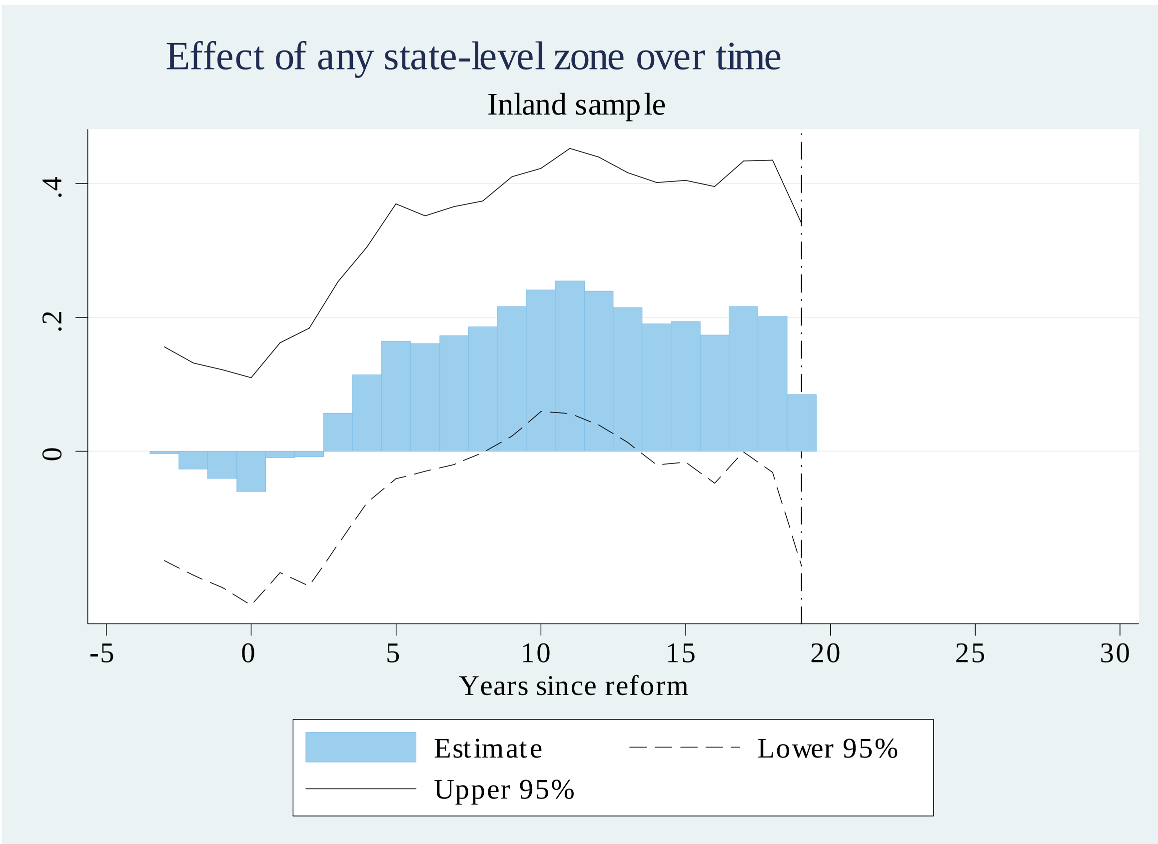


Figure 4: Reform effects over time: The bars show the coefficients of a regression of the logarithm of nominal GDP on indicators for years before and after the first zone. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 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.

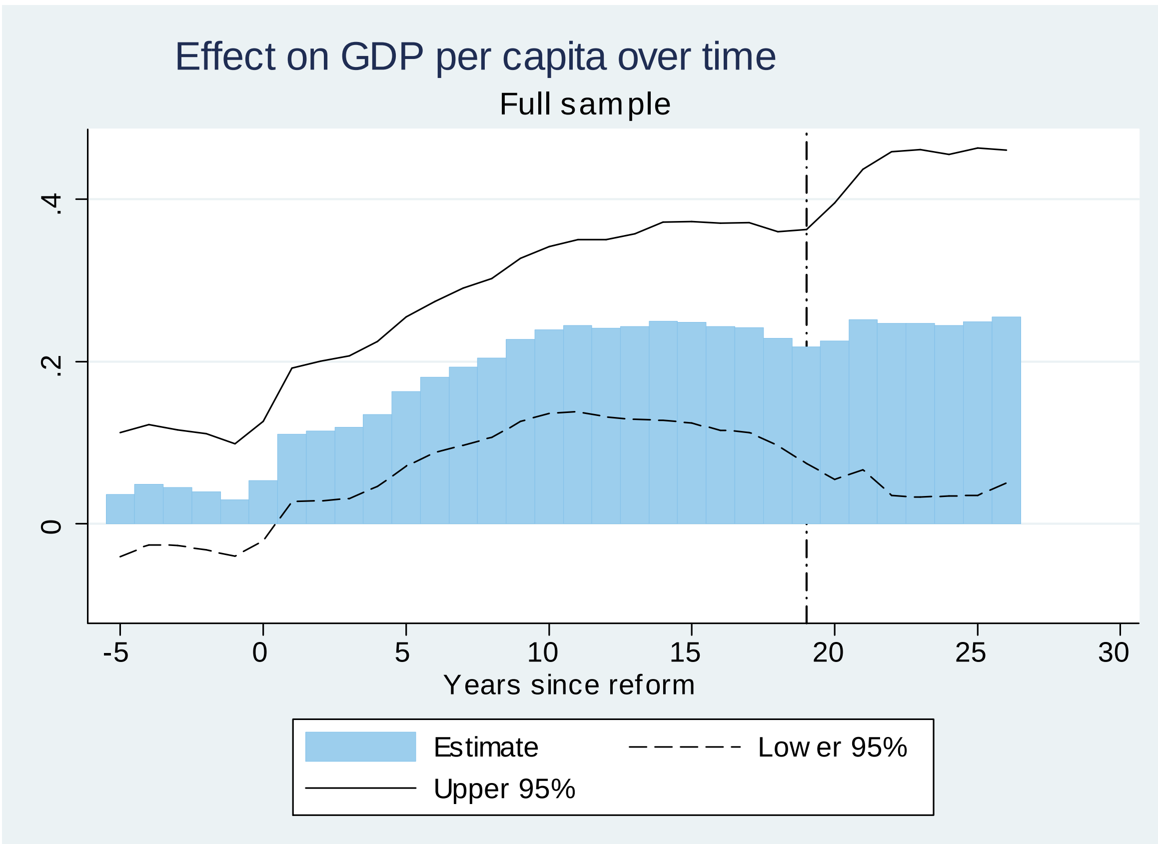


Figure 5: Effects on GDP per capita over time: The bars show the coefficients of a regression of the logarithm of GDP per capita on indicators for years before and after the first SEZ was established. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 2010 and subsequently the number of observations to identify post-reform indicators drops to 9. The regression uses data at the prefecture level and also controls for an indicator for province-level zones, land area of the prefecture, prefecture fixed effects, and province-time fixed effects. Standard errors are clustered by prefecture. The sample includes 276 cities from 25 provinces for the period 1988-2010.

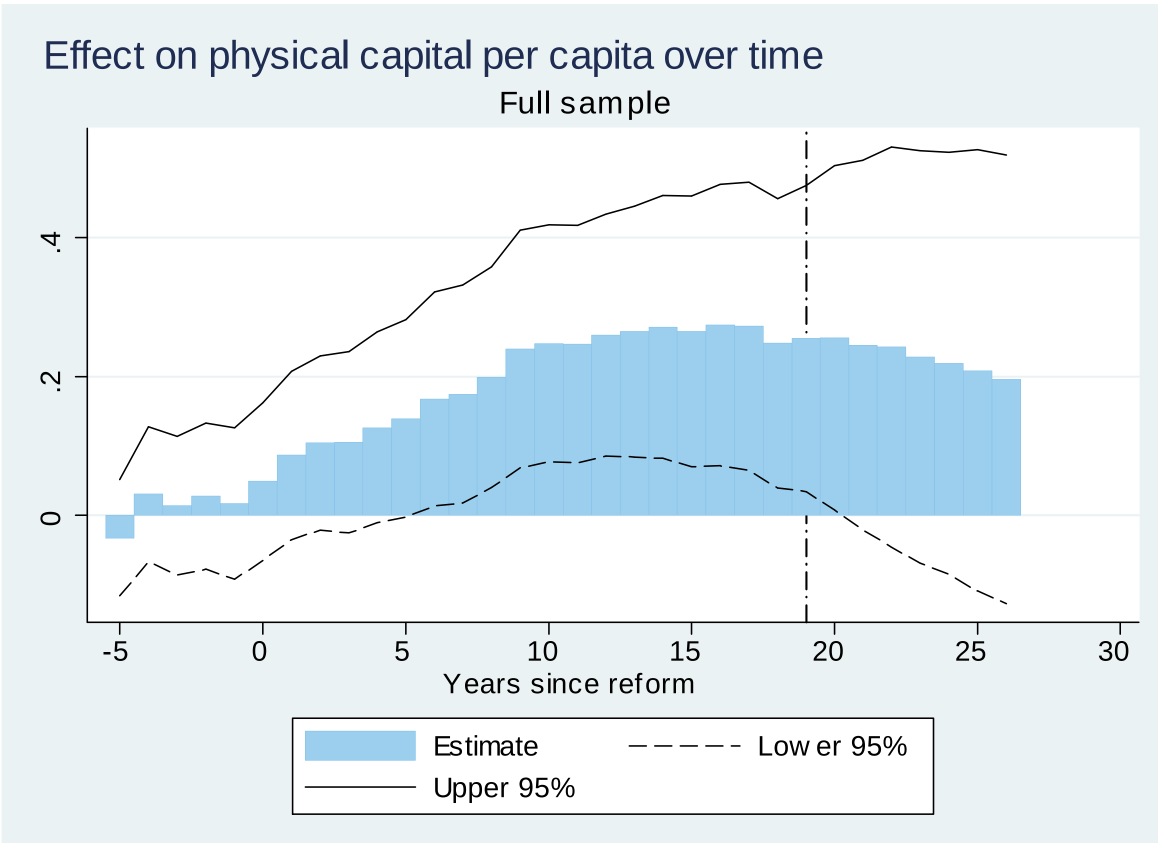


Figure 6: Effects on physical capital per capita over time: The bars show the coefficients of a regression of the logarithm of physical capital per capita on indicators for years before and after the first SEZ was established. The solid and dashed lines show the confidence interval. The vertical dashed 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 at the prefecture level and also controls for an indicator for province-level zones, land area of the prefecture, prefecture fixed effects, and province-time fixed effects. Standard errors are clustered by prefecture. The sample includes 276 cities from 25 provinces for the period 1988-2010.

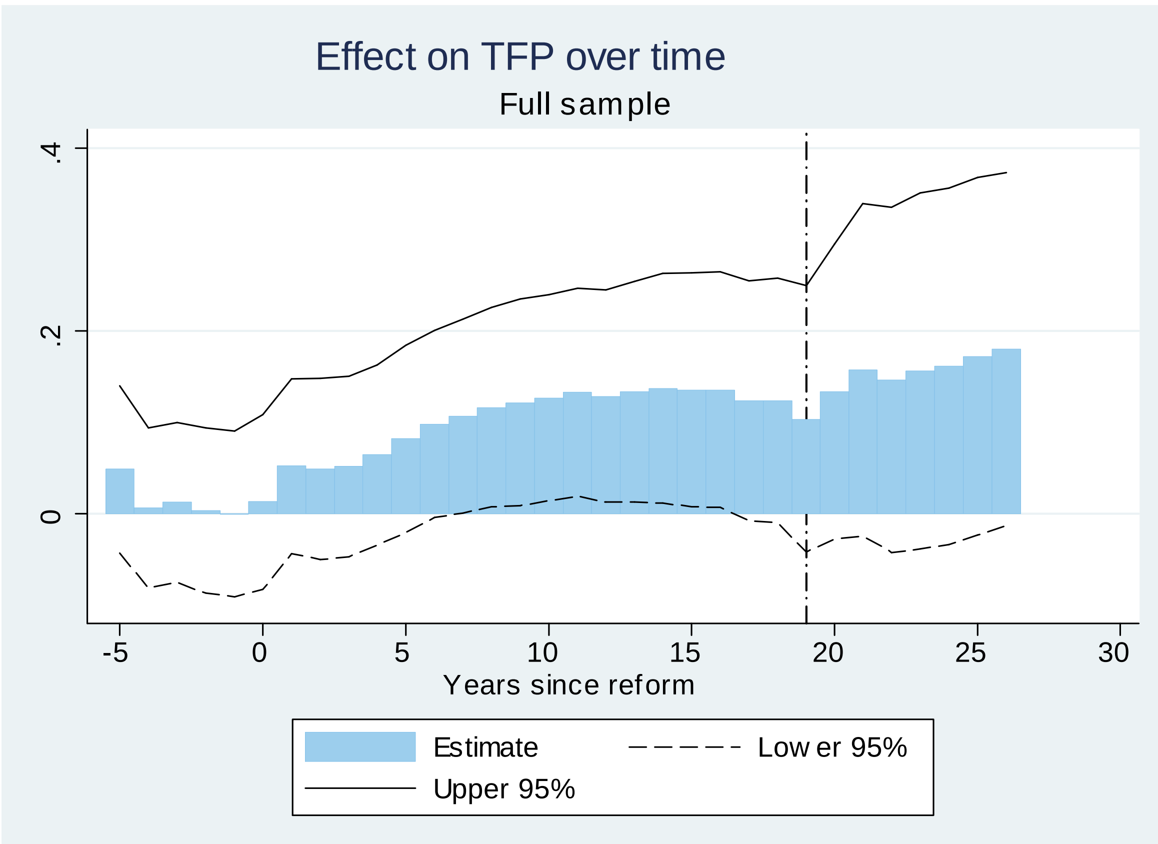


Figure 7: Effects on TFP over time: The bars show the coefficients of a regression of the logarithm of TFP on indicators for years before and after the first SEZ was established. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 2010 and subsequently the number of observations to identify post-reform indicators drops to 9. The regression uses data at the prefecture level and also controls for an indicator for province-level zones, land area of the prefecture, prefecture fixed effects, and province-time fixed effects. Standard errors are clustered by prefecture. The sample includes 276 cities from 25 provinces for the period 1988-2010.

# 8 Webpage 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-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.[37]

The light data are obtained from the National Geographical Data Center.[38] 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).

**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.[39] We also use the Information Site of China's Development Zones[40] 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 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 cities (and prefectures) and

[37] 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.

[38] See http://www.ngdc.noaa.gov/dmsp/downloadV4composites.html

[39] See http://english.mofcom.gov.cn.

[40] See http://www.cdz.cn/www/index.asp.

this number increases to 276 in the year 2010.[41] 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: Shenzhen, Zhuhai, Shantou and Xiamen.

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) urban core. Notice that in the main body of the paper, for simplicity, we call prefecture-level cities "prefecture" and the urban core "city".

**Sample Period** The GDP data in *China City Statistical Yearbooks* only go back to the year 1988.[42] Although pre-1988 GDP data for a subset of cities are available from other data sources, we do not 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.[43] 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 statistic sources 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* as soon a municipalities has been granted the official status of prefecture-level city. Therefore, the *China City Statistical Yearbooks* yields a consistent sample of prefecture-level cities for the period 1988-2010.

**Prefecture- and County-Level Cities** A prefecture-level city (*di ji shi* in Chinese) is in the new administrative system a level between provinces and counties. A prefecture-level city consists of the urban core ("city") and potentially several surrounding counties and rural areas. As part of the transformation of administrative structures, some counties were promoted into county-level cities (*xian ji shi* in Chinese) 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

[41] See Table 1 in Chung and Lam (2004) for a more detailed assessment of the increase in the number of cities in China.

[42] The earliest city statistical yearbook goes back to 1984. However, the yearbook only starts to report city-level GDP after 1988.

[43] 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.

sample period. For those cities which were promoted to prefecture-level, we keep only the years after the promotion to prefecture-level city.

**Prefecture Area and Urban Core** *China City Statistical Yearbooks* report statistics at the level of both the whole prefecture area and the urban core (*shi xia qu* in Chinese). Urban 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 urban core and the surrounding counties, county-level cities and rural areas.

The distinction between the two statistical areas and its implication for the estimation result deserves discussion. First, urban cores are usually more industrialized than the whole prefecture area. Second, most of the SEZ are located in the suburbans of the urban core (Zeng, 2011).[44] Therefore, by focusing on the urban core (or, more simply, *cities* as we call them in the text), we get a more direct estimation of the effects of SEZ. In some parts of the paper we also report results for the entire prefecture.

**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 per capita into physical capital per capita, human capital (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 ($K_{ipt}$) is the sum of physical capital stock after depreciation and new investment ($I_{ipt}$), such that

$$K_{ipt} = (1 - \delta_k)K_{ipt-1} + I_{ipt}/deflator^{Inv}_{pt}.$$

The deflator for new investment, $deflator^{Inv}_{pt}$, is province-specific. We set $\delta_k$, the annual depreciation rate for physical capital, to be 0.08.[45]

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

$$K_{ip1978} = \frac{I_{ip1978}}{g_{1978} + \delta_k},$$

[44] In our sample, all of the state level zones were located in the city and not on the surrounding periphery.

[45] 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).

where $I_{ip1978}$ is the new investment in year 1978 and $g_{1978}$ is the average growth rate of real physical capital stock before 1978.[46] 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.[47]

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

$$K_{ip1988} = \frac{I_{ip1988}}{g_{1988} + \delta_k},$$

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

**Size of Labor Force** We use population as an approximation for employment in each city because the number of employed persons reported in the *China City Statistical Yearbooks* has some drawbacks. The most imporant drawback is that there is a huge drop in the number of employed persons in the year 1998, the reason of which is unclear to us. Two reasons could potentially contribute to this huge drop. The first potential reason is that the reform of state-owned enterprises laid off a large number of redundant workers around 1998.[48] The second reason is that perhaps the definition of employed persons changes on 1998. Specifically, 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. [49]

**Human Capital** 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

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

where $s_{ipt}$ is the average years of schooling and $\phi_t(.)$ is a piece-wise linear function whose slopes represent the return to schooling. To construct $\phi_t(.)$, we take the estimation for the return to schooling in China over the period 1988-2009 from Li *et al.* (2009).[50]

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,

$$\log A_{ipt} = \log \frac{Y_{ipt}}{L_{ipt}} - \alpha \log \frac{K_{ipt}}{L_{ipt}} - (1-\alpha) \log h_{ipt}.$$

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

### 8.4 Additional Tables and Figures

[46] 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.

[47] Notice that our sample starts in 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.

[48] According to Dong and Putterman (2003), the labor redundancy rate of SOEs is 30% in 1992.

[49] Wu (2011) provides a detailed discussion of the issues with the employment data.

[50] The estimation is not available for the year 2010. We simply assume that the return to schooling did not change between 2009 and 2010, i.e. $\phi_{2010}(.) = \phi_{2009}(.)$.

Table 13: Baseline specification clustering at province-reform year level

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Post-reform indicator for any state-level zone | 0.190*** | 0.147*** | 0.127*** | 0.117*** | 0.268*** | 0.212*** | 0.181*** | 0.166*** |
| | (4.15) | (4.37) | (4.57) | (4.26) | (5.43) | (4.57) | (3.71) | (3.24) |
| Post-reform indicator for province-level zone | -0.000486 | -0.00706 | 0.000497 | 0.00412 | -0.0157 | -0.0244 | -0.00775 | 0.00319 |
| | (-0.02) | (-0.34) | (0.02) | (0.19) | (-0.59) | (-0.95) | (-0.29) | (0.12) |
| Log landarea | | 0.240*** | -0.0325 | -0.154*** | | 0.211*** | -0.0533 | -0.175*** |
| | | (6.94) | (-1.42) | (-8.11) | | (4.72) | (-1.08) | (-5.19) |
| Log population | | | 0.692*** | | | | 0.673*** | |
| | | | (14.10) | | | | (6.95) | |
| 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-year dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the province and reform year 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 14: Pre-and post indicators clustering at province-reform year level

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Indicator for 3 years before any state-level zone | -0.00764 | 0.0247 | 0.0229 | 0.0221 | -0.00543 | 0.00564 | -0.0172 | -0.0283 |
| | (-0.27) | (0.83) | (0.94) | (0.91) | (-0.10) | (0.12) | (-0.24) | (-0.31) |
| Indicator for 2 years before any state-level zone | -0.0147 | 0.0255 | 0.0238 | 0.0230 | -0.0429 | -0.0221 | -0.0400 | -0.0487 |
| | (-0.48) | (0.78) | (0.91) | (0.90) | (-0.86) | (-0.50) | (-0.57) | (-0.54) |
| Indicator for 1 year before any state-level zone | -0.0252 | 0.0165 | 0.0161 | 0.0159 | -0.0572 | -0.0367 | -0.0536 | -0.0618 |
| | (-0.79) | (0.48) | (0.60) | (0.61) | (-1.18) | (-0.85) | (-0.77) | (-0.69) |
| Indicator for year of any state-level zone | -0.00290 | 0.0213 | 0.0232 | 0.0241 | -0.0816 | -0.0599 | -0.0742 | -0.0811 |
| | (-0.09) | (0.57) | (0.78) | (0.84) | (-1.55) | (-1.36) | (-1.02) | (-0.87) |
| Post-reform indicator for any state-level zone | 0.180*** | 0.165*** | 0.144*** | 0.134*** | 0.229*** | 0.188*** | 0.143* | 0.121 |
| | (3.29) | (3.47) | (4.02) | (3.88) | (4.31) | (3.53) | (1.83) | (1.23) |
| Post-reform indicator for province-level zone | -0.000807 | -0.00645 | 0.00110 | 0.00471 | -0.0166 | -0.0307 | -0.00860 | 0.00215 |
| | (-0.04) | (-0.31) | (0.05) | (0.21) | (-0.61) | (-1.13) | (-0.31) | (0.08) |
| Log landarea | | 0.241*** | -0.0321 | -0.154*** | | 0.197*** | -0.0536 | -0.175*** |
| | | (6.95) | (-1.41) | (-8.07) | | (4.44) | (-1.08) | (-5.20) |
| Log population | | | 0.692*** | | | | 0.673*** | |
| | | | (14.13) | | | | (6.97) | |
| 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-year dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the province and reform year 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 15: Trend break clustering at province-reform year level

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Post-reform indicator for any state-level zone | 0.127*** | 0.0825*** | 0.0837*** | | 0.181*** | 0.0858 | 0.0972** | |
| | (4.57) | (3.02) | (3.06) | | (3.71) | (1.67) | (2.04) | |
| Post-reform indicator for province-level zone | 0.000497 | 0.000738 | 0.000861 | 0.00219 | -0.00775 | -0.00668 | -0.00768 | -0.00996 |
| | (0.02) | (0.04) | (0.04) | (0.10) | (-0.29) | (-0.25) | (-0.28) | (-0.37) |
| Time trend of reformers (state-level) | | 0.00548** | 0.00595* | 0.00656** | | 0.00855* | 0.00262 | -0.000951 |
| | | (2.39) | (1.82) | (2.04) | | (1.85) | (0.32) | (-0.10) |
| Post-reform trend (state-level) | | | -0.000747 | 0.0156** | | | 0.00624 | 0.0450*** |
| | | | (-0.18) | (2.15) | | | (0.62) | (2.78) |
| Sq. post-reform trend (state-level) | | | | -0.000740** | | | | -0.00182*** |
| | | | | (-2.52) | | | | (-2.96) |
| Sample | Full | Full | Full | Full | Inland | Inland | Inland | Inland |
| Control Variables | Y | Y | Y | Y | Y | Y | Y | Y |
| 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 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-year dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the province and reform year 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 16: Effects of different types of Zones clustering at province-reform year level

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Post-reform indicator for ETDZ | 0.220*** | 0.156*** | 0.120*** | 0.104*** | 0.239*** | 0.171*** | 0.0969* | 0.0581 |
| | (4.86) | (3.85) | (3.29) | (2.78) | (4.28) | (3.14) | (1.78) | (1.03) |
| Post-reform indicator for HIDZ | 0.117** | 0.0794** | 0.0755** | 0.0736** | 0.122*** | 0.0925* | 0.106** | 0.113** |
| | (2.31) | (2.14) | (2.50) | (2.48) | (2.77) | (1.87) | (2.27) | (2.35) |
| Post-reform indicator for EPZ | 0.0412 | 0.0361 | 0.0205 | 0.0131 | -0.0123 | 0.00190 | 0.0490 | 0.0738 |
| | (0.99) | (0.96) | (0.60) | (0.36) | (-0.18) | (0.02) | (0.69) | (1.09) |
| Post-reform indicator for OtherTypes | 0.0710 | 0.0583 | 0.0898* | 0.104** | 0.0827 | 0.174*** | 0.213*** | 0.233*** |
| | (0.97) | (0.94) | (1.79) | (2.16) | (1.40) | (3.83) | (4.23) | (4.42) |
| Post-reform indicator for province-level zone | 0.00286 | -0.00532 | 0.00260 | 0.00649 | -0.0158 | -0.0297 | -0.00878 | 0.00223 |
| | (0.13) | (-0.26) | (0.12) | (0.29) | (-0.60) | (-1.11) | (-0.32) | (0.08) |
| Log landarea | | 0.233*** | -0.0361 | -0.159*** | | 0.187*** | -0.0535 | -0.180*** |
| | | (6.61) | (-1.57) | (-8.22) | | (3.97) | (-1.07) | (-5.22) |
| Log population | | | 0.686*** | | | | 0.655*** | |
| | | | (13.70) | | | | (6.63) | |
| 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. All specifications include city fixed effects and the interaction of province-year dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the province and reform year 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 17: Decomposition of the effect clustering at province-reform year level

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Post-reform indicator for first state-level zone | 0.138*** | 0.148*** | 0.00343 | 0.0825*** | 0.131*** | 0.244*** | 0.00409 | 0.0442 |
| | (4.84) | (3.36) | (0.94) | (3.08) | (3.00) | (4.02) | (0.75) | (1.33) |
| Post-reform indicator for first province-level zone | -0.0127 | -0.0159 | 0.00242 | -0.0125 | -0.0145 | -0.0201 | 0.00448** | -0.00705 |
| | (-0.77) | (-0.48) | (1.46) | (-0.75) | (-0.68) | (-0.39) | (2.19) | (-0.27) |
| Log landarea | -0.218*** | -0.532*** | -0.0157*** | 0.0120 | -0.212*** | -0.490*** | -0.0157*** | -0.000134 |
| | (-5.95) | (-9.75) | (-4.19) | (0.36) | (-4.96) | (-7.62) | (-3.60) | (-0.00) |
| Dependent Variable | log(Y/L) | log(K/L) | log(h) | log(TFP) | log(Y/L) | log(K/L) | log(h) | log(TFP) |
| Sample | Full | Full | Full | Full | Inland | Inland | Inland | Inland |
| N | 5171 | 4521 | 4381 | 3970 | 3242 | 2744 | 2610 | 2320 |
| AR2 | 0.948 | 0.959 | 0.957 | 0.802 | 0.938 | 0.954 | 0.950 | 0.731 |

The dependent variables are the logarithms of real GDP per capita column (1) and (5), and the three decomposed components: logarithm of physical capital stock (column (2) and (6)), logarithms of average human capital (column (3)-(7)) and logarithm of TFP (column (4)-(8)), of the prefecture area. All speci.cations include land area, prefecture fixed effect and the interaction of province-year dummies as control variables. T-statistics are reported in parenthesis. Standard errors are clustered at the province and reform year level. The sample includes 276 cities from 25 provinces over the sample period 1988-2010 (unbalanced panel). The regression is carried out for the full sample (column (1)-(4)) and restricted inland sample ((5)-(8)).

Table 18: Baseline specification clustering at province level

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Post-reform indicator for any state-level zone | 0.190*** | 0.147*** | 0.127*** | 0.117*** | 0.268*** | 0.212*** | 0.181*** | 0.166** |
| | (3.70) | (3.66) | (3.79) | (3.46) | (4.16) | (3.53) | (2.91) | (2.52) |
| Post-reform indicator for province-level zone | -0.000486 | -0.00706 | 0.000497 | 0.00412 | -0.0157 | -0.0244 | -0.00775 | 0.00319 |
| | (-0.02) | (-0.28) | (0.02) | (0.16) | (-0.57) | (-0.96) | (-0.28) | (0.12) |
| Log landarea | | 0.240*** | -0.0325 | -0.154*** | | 0.211*** | -0.0533 | -0.175*** |
| | | (8.36) | (-1.55) | (-8.34) | | (4.66) | (-1.10) | (-4.92) |
| Log population | | | 0.692*** | | | | 0.673*** | |
| | | | (16.53) | | | | (6.66) | |
| 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-year dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the province 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 19: Pre-and post indicators clustering at province level

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Indicator for 3 years before any state-level zone | -0.00764 | 0.0247 | 0.0229 | 0.0221 | -0.00543 | 0.00564 | -0.0172 | -0.0283 |
| | (-0.21) | (0.65) | (0.81) | (0.84) | (-0.08) | (0.10) | (-0.18) | (-0.22) |
| Indicator for 2 years before any state-level zone | -0.0147 | 0.0255 | 0.0238 | 0.0230 | -0.0429 | -0.0221 | -0.0400 | -0.0487 |
| | (-0.39) | (0.67) | (0.86) | (0.88) | (-0.65) | (-0.38) | (-0.42) | (-0.39) |
| Indicator for 1 year before any state-level zone | -0.0252 | 0.0165 | 0.0161 | 0.0159 | -0.0572 | -0.0367 | -0.0536 | -0.0618 |
| | (-0.60) | (0.40) | (0.54) | (0.57) | (-0.88) | (-0.65) | (-0.56) | (-0.50) |
| Indicator for year of any state-level zone | -0.00290 | 0.0213 | 0.0232 | 0.0241 | -0.0816 | -0.0599 | -0.0742 | -0.0811 |
| | (-0.08) | (0.48) | (0.69) | (0.76) | (-1.18) | (-1.03) | (-0.75) | (-0.63) |
| Post-reform indicator for any state-level zone | 0.180*** | 0.165*** | 0.144*** | 0.134*** | 0.229*** | 0.188** | 0.143 | 0.121 |
| | (3.05) | (2.93) | (3.64) | (3.51) | (3.13) | (2.61) | (1.35) | (0.89) |
| Post-reform indicator for province-level zone | -0.000807 | -0.00645 | 0.00110 | 0.00471 | -0.0166 | -0.0307 | -0.00860 | 0.00215 |
| | (-0.03) | (-0.25) | (0.04) | (0.19) | (-0.59) | (-1.11) | (-0.31) | (0.08) |
| Log landarea | | 0.241*** | -0.0321 | -0.154*** | | 0.197*** | -0.0536 | -0.175*** |
| | | (8.52) | (-1.55) | (-8.26) | | (4.54) | (-1.12) | (-4.94) |
| Log population | | | 0.692*** | | | | 0.673*** | |
| | | | (16.58) | | | | (6.73) | |
| 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-year dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the province 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 20: Trend break clustering at province level

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Post-reform indicator for any state-level zone | 0.127*** | 0.0825*** | 0.0837*** | | 0.181*** | 0.0858 | 0.0972 | |
| | (3.79) | (2.85) | (2.93) | | (2.91) | (1.40) | (1.69) | |
| Post-reform indicator for province-level zone | 0.000497 | 0.000738 | 0.000861 | 0.00219 | -0.00775 | -0.00668 | -0.00768 | -0.00996 |
| | (0.02) | (0.03) | (0.04) | (0.09) | (-0.28) | (-0.25) | (-0.28) | (-0.35) |
| Time trend of reformers (state-level) | | 0.00548** | 0.00595 | 0.00656* | | 0.00855 | 0.00262 | -0.000951 |
| | | (2.13) | (1.70) | (1.85) | | (1.65) | (0.23) | (-0.08) |
| Post-reform trend (state-level) | | | -0.000747 | 0.0156* | | | 0.00624 | 0.0450** |
| | | | (-0.15) | (1.81) | | | (0.47) | (2.16) |
| Sq. post-reform trend (state-level) | | | | -0.000740** | | | | -0.00182** |
| | | | | (-2.39) | | | | (-2.46) |
| Sample | Full | Full | Full | Full | Inland | Inland | Inland | Inland |
| Control Variables | Y | Y | Y | Y | Y | Y | Y | Y |
| 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 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-year dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the province 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 21: Effects of different types of Zones clustering at province level

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Post-reform indicator for ETDZ | 0.220*** | 0.156*** | 0.120*** | 0.104** | 0.239*** | 0.171** | 0.0969 | 0.0581 |
| | (4.63) | (3.36) | (2.89) | (2.47) | (3.39) | (2.45) | (1.45) | (0.83) |
| Post-reform indicator for HIDZ | 0.117* | 0.0794* | 0.0755* | 0.0736* | 0.122** | 0.0925 | 0.106* | 0.113* |
| | (1.80) | (1.72) | (2.00) | (2.03) | (2.16) | (1.46) | (1.80) | (1.88) |
| Post-reform indicator for EPZ | 0.0412 | 0.0361 | 0.0205 | 0.0131 | -0.0123 | 0.00190 | 0.0490 | 0.0738 |
| | (1.12) | (0.96) | (0.56) | (0.33) | (-0.14) | (0.02) | (0.56) | (0.89) |
| Post-reform indicator for OtherTypes | 0.0710 | 0.0583 | 0.0898*** | 0.104*** | 0.0827* | 0.174*** | 0.213*** | 0.233*** |
| | (1.14) | (1.20) | (3.14) | (4.27) | (1.76) | (3.18) | (3.96) | (4.42) |
| Post-reform indicator for province-level zone | 0.00286 | -0.00532 | 0.00260 | 0.00649 | -0.0158 | -0.0297 | -0.00878 | 0.00223 |
| | (0.11) | (-0.21) | (0.11) | (0.26) | (-0.63) | (-1.12) | (-0.32) | (0.08) |
| Log landarea | | 0.233*** | -0.0361 | -0.159*** | | 0.187*** | -0.0535 | -0.180*** |
| | | (7.70) | (-1.68) | (-8.25) | | (4.00) | (-1.10) | (-4.94) |
| Log population | | | 0.686*** | | | | 0.655*** | |
| | | | (15.20) | | | | (6.37) | |
| 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. All specifications include city fixed effects and the interaction of province-year dummies. T-statistics are reported in parenthesis. Standard errors are clustered at the province 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 22: Decomposition of the effect clustering at province level

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Post-reform indicator for first state-level zone | 0.138*** | 0.148*** | 0.00343 | 0.0825** | 0.131** | 0.244*** | 0.00409 | 0.0442 |
| | (4.25) | (3.09) | (0.66) | (2.51) | (2.61) | (3.74) | (0.52) | (1.05) |
| Post-reform indicator for first province-level zone | -0.0127 | -0.0159 | 0.00242 | -0.0125 | -0.0145 | -0.0201 | 0.00448* | -0.00705 |
| | (-0.63) | (-0.58) | (1.53) | (-0.69) | (-0.61) | (-0.46) | (2.05) | (-0.27) |
| Log landarea | -0.218*** | -0.532*** | -0.0157*** | 0.0120 | -0.212*** | -0.490*** | -0.0157*** | -0.000134 |
| | (-5.28) | (-8.48) | (-3.77) | (0.31) | (-4.44) | (-7.10) | (-3.14) | (-0.00) |
| Dependent Variable | log(Y/L) | log(K/L) | log(h) | log(TFP) | log(Y/L) | log(K/L) | log(h) | log(TFP) |
| Sample | Full | Full | Full | Full | Inland | Inland | Inland | Inland |
| N | 5171 | 4521 | 4381 | 3970 | 3242 | 2744 | 2610 | 2320 |
| AR2 | 0.948 | 0.959 | 0.957 | 0.802 | 0.938 | 0.954 | 0.950 | 0.731 |

The dependent variables are the logarithms of real GDP per capita column (1) and (5), and the three decomposed components: logarithm of physical capital stock (column (2) and (6)), logarithms of average human capital (column (3)-(7)) and logarithm of TFP (column (4)-(8)), of the prefecture area. All speci.cations include land area, prefecture fixed effect and the interaction of province-year dummies as control variables. T-statistics are reported in parenthesis. Standard errors are clustered at the province level. The sample includes 276 cities from 25 provinces over the sample period 1988-2010 (unbalanced panel). The regression is carried out for the full sample (column (1)-(4)) and restricted inland sample ((5)-(8)).

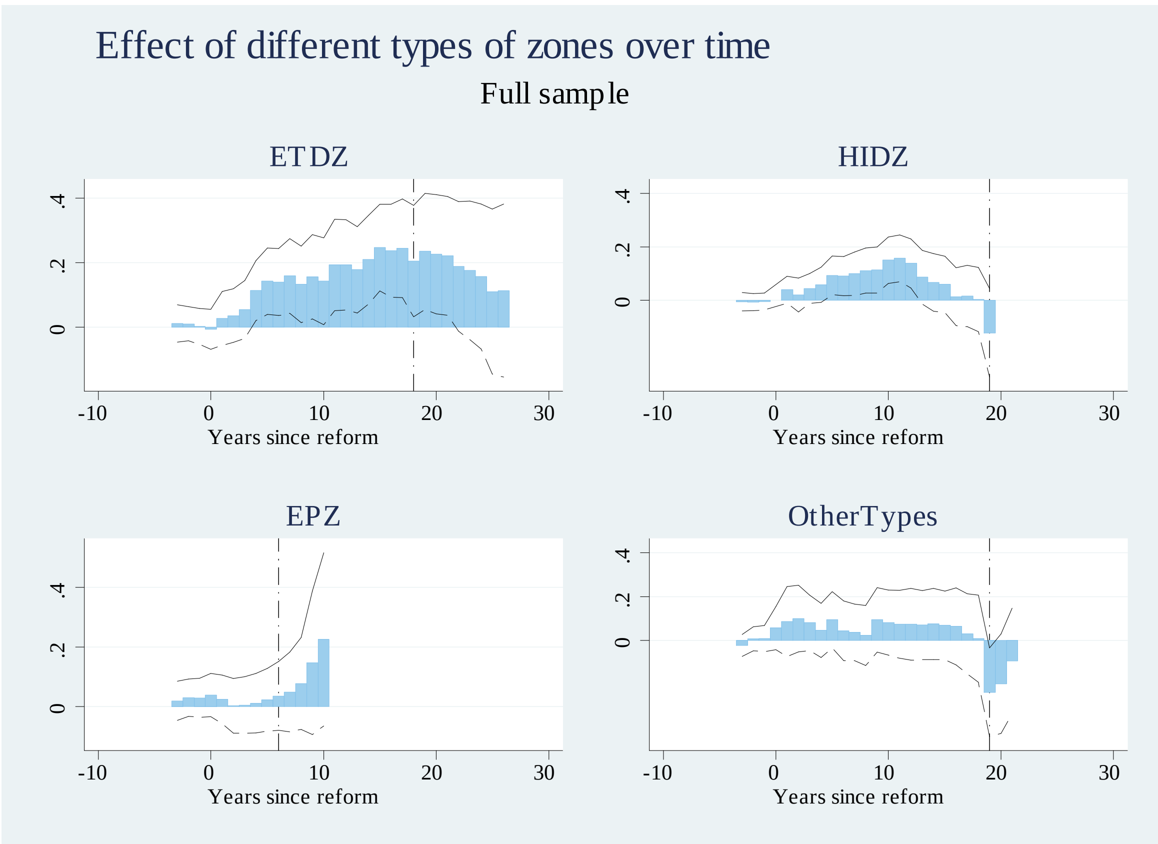


Figure 8: Effects of different types of zones over time: The four panels show the coefficients of different policy variables estimated in the same regression. The bars show the coefficients of a regression of the logarithm of nominal GDP on indicators for years before and after a type of zone was established. The solid and dashed lines show the confidence intervals. The vertical dashed line shows the lag at which the number of observations drops due to the first zones reaching the end of the sample period. The regression also controls for an indicator for province-level zones, land area, population, city fixed effects, and province-year fixed effects. Standard errors are clustered by city. The sample includes 276 cities from 25 provinces for the period 1988-2010.

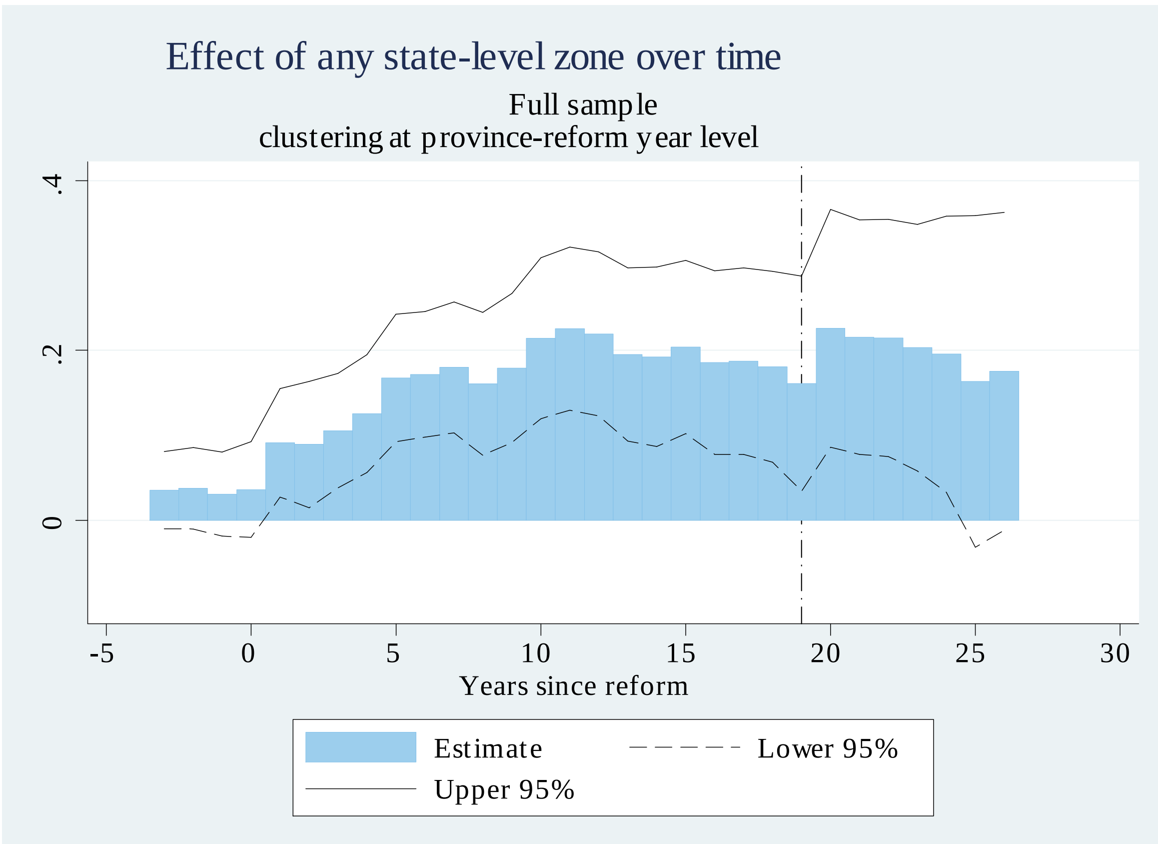


Figure 9: Reform effects over time: The bars show the coefficients of a regression of the logarithm of nominal GDP on indicators for years before and after the first zone. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 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-year fixed effects. Standard errors are clustered by province and reform year. The sample includes 276 cities from 25 provinces for the period 1988-2010.

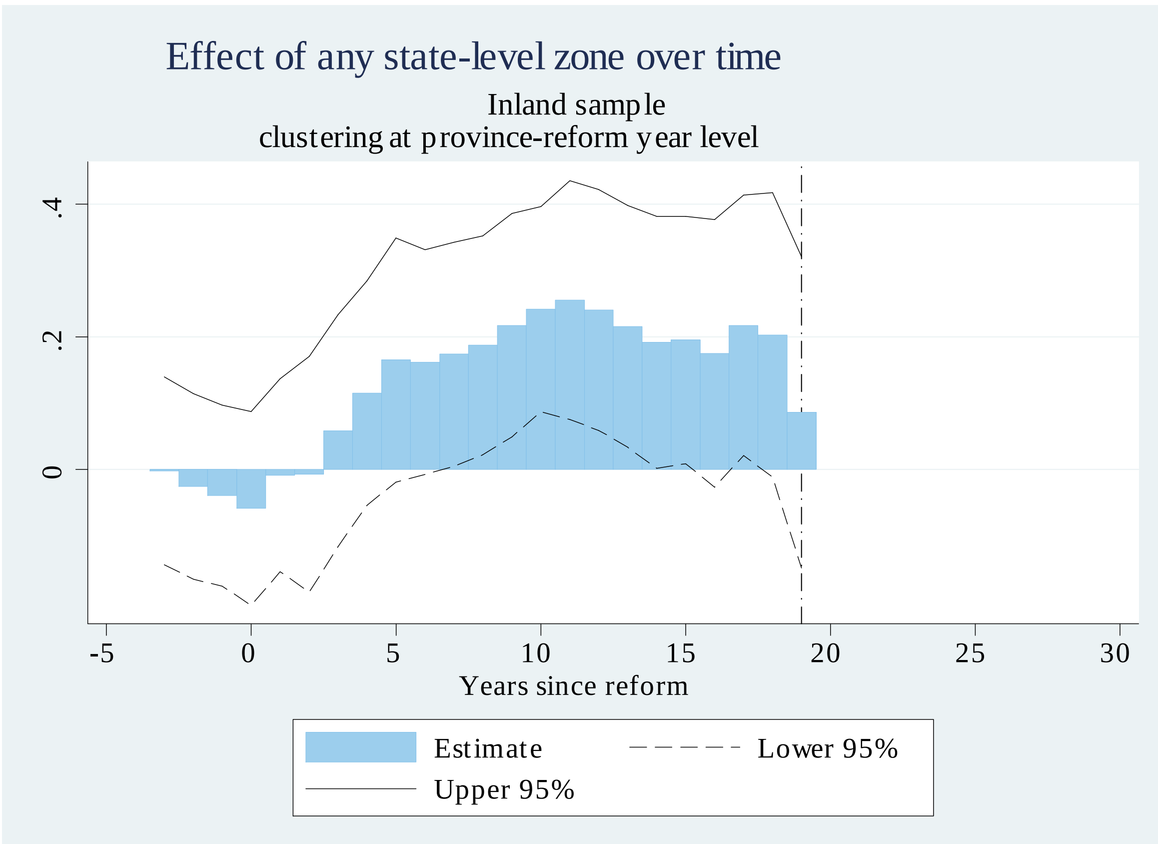


Figure 10: Reform effects over time: The bars show the coefficients of a regression of the logarithm of nominal GDP on indicators for years before and after the first zone. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 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-year fixed effects. Standard errors are clustered by province and reform year. The sample includes 158 cities from 18 inland provinces (as defined in the appendix) for the period 1988-2010.

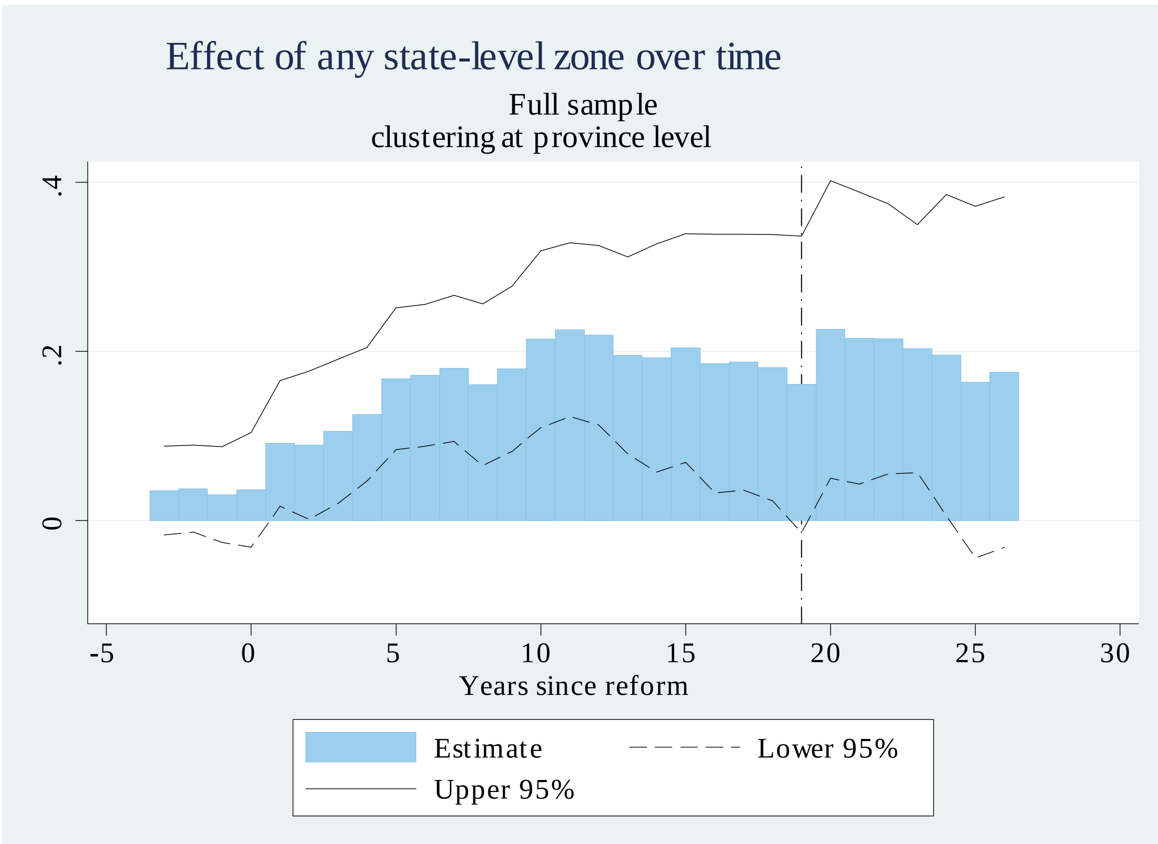


Figure 11: Reform effects over time: The bars show the coefficients of a regression of the logarithm of nominal GDP on indicators for years before and after the first zone. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 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-year fixed effects. Standard errors are clustered by province. The sample includes 276 cities from 25 provinces for the period 1988-2010.

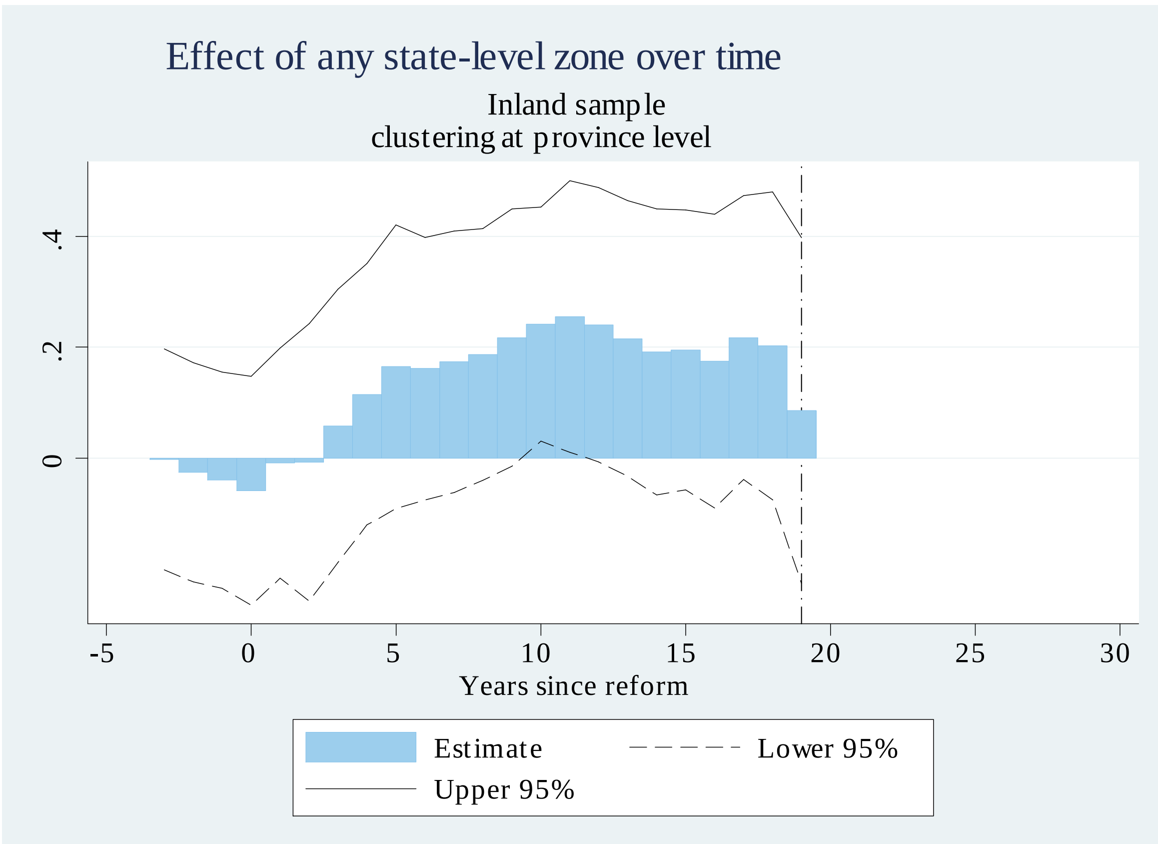


Figure 12: Reform effects over time: The bars show the coefficients of a regression of the logarithm of nominal GDP on indicators for years before and after the first zone. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 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-year fixed effects. Standard errors are clustered by province. The sample includes 158 cities from 18 inland provinces (as defined in the appendix) for the period 1988-2010.