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

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Abstract

We use the establishment of Special Economic Zones in China to estimate the effect of economic reforms on GDP. A panel of 270 Chinese cities from 1988 to 2010 allows us to exploit the variation in the establishment of zones across time and space. The results from our baseline fixed effects specification suggest that the establishment of a major zone led to an increase in the level of GDP around 12%, but did not lead to a permanently steeper growth path. This result is confirmed with alternative specifications and in a sub-sample of inland provinces, where the selection of cities in which zones were established was more transparent. Decomposing GDP into physical capital, efficient labor, and TFP shows that the effect of the zones went mainly through the accumulation of physical capital. Using electricity consumption and light intensity as two alternative measures for economic activity confirms the positive effects but suggest lower magnitudes.

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1 Introduction

China has been the fastest growing economy over the last three decades, with an average annual growth rate above 8% between 1978 and 2010. Its economic boom has been associated with a massive process of policy and institutional reforms. While there is little doubt that the reform process altogether was key for economic development, it remains difficult – as of today – to disentangle the extent to which different policies have mattered. In particular, there is a debate about the extent to which China should be viewed as a case or a counterexample of success of the so-called "Washington consensus" approach. On the one hand, China has progressively liberalized its economy and moved from a state-controlled allocation mechanism to a market economy. On the other hand, the current organization of the economy is still far from a Western market economy, due to the heavy presence of state-controlled firms and banks, weak contract enforcement, etc.. In addition, the Chinese government has over the years engaged in proactive industrial policies creating special status privileges for specific cities, industries and regions. While the liberalizing elements implied by these policies were consistent with the principles of economic orthodoxy, such industrial policy also entails important unorthodox elements, insofar as it creates imbalances and distortions to resource allocation.

This paper uses the variation in the establishment of different types of Special Economic Zones (SEZ) across cities and years to estimate the development effects of such reforms. SEZ are interesting and worth studying for a variety of reasons. First, they have been a centerpiece of the Chinese development strategy – inspired to gradualism and experimentation. Second, they may have exacerbated the highly uneven development across geographic areas and sectors. Third, relative to other reforms, they are easier to measure, as they involved clear changes in the legal status that were introduced gradually over time and space.[1] SEZ were initially meant as experiments with market mechanisms within geographically limited areas along the coast. The policy changes in the SEZ included for example liberalization of labor markets, foreign direct investment, ownership, and export. But the local leadership of the zones also had substantial autonomy in designing their policy packages and often engaged in active industrial policy. After the success of some of these early zones, the SEZ were extended first to other cities along the coast and then starting in the early 1990s also to inland regions. New zones have progressively been established until today.[2]

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

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

We use panel data of 270 cities at the prefecture level over 23 years that allow us to compare the development across cities and years and distinguish the effects of different types of zones. We control for time-invariant heterogeneity by using city fixed effects. The dynamics at the national and provincial level are controlled for by province-time fixed effects, which not only absorb reform effects in different years in the whole province or country, but also changes in the provincial price levels. As a first-step approach, we perform a difference-in-difference analysis: we regress log GDP on an indicator that takes value one in all years after a SEZ has been established in a city, controlling for the above mentioned fixed effects. We also control for the size of the city (because the establishment of SEZ often coincided with changes of city borders) and for other time-varying city characteristics such as population. The results from our baseline specification is that the introduction of a major zone in a city led to an increase in the GDP level of 12% and the effect is depending on the type of zone. The effect remains significant after controlling for population size and government spending. Since it is plausible that there are distributed lags to the effects of the reform, we then consider a more flexible specification where the effect of the reform is allowed to vary as a function of the time elapsed since the start of the treatment. Using this "flexible specification", we find that the effect of SEZ flattens out after about ten years; the long-term effect of a SEZ is an increase of about 20% in the GDP level.

Our analysis is subject to two important *caveats.* First, the assignment to treatment and control groups is unlikely to be random. The Chinese government may have selected cities based on some prior knowledge that the conditions for industrial development might be especially favorable ("picking winners"), or to the opposite in order to curb regional inequality. Picking winners appears to have been especially important in the first phase of the reforms, when all SEZ were chosen on the coast and close to potential trading partners and investors like Hong Kong and Taiwan. Ideally, one would like to find valid instruments to isolate sources of exogenous variation. However, in practice, this is very difficult. We partially address the concern with selection by two complementary strategies. First, we restrict the sample to cities located in inland provinces where selection of the zones was largely based on a rigid administrative criterion. Second, we add to our "flexible specification" indicators for the pre-reform years. The results are in both cases reassuring: the effect of SEZ is robust in the restricted sample, and the pseudo-effects before the actual establishment of the zone are insignificant. There is a clear break in the year of the reform when the effects start increasing and the effects become significant.

The second caveat is about data quality. First, one might question the credibility of city-level data. There are concerns, in particular, that local statistics may have been manipulated strategically in order to create the impression that the SEZ was successful and to attract further government support. Second, while city-level data for GDP are available, credible and complete price deflators are only available at the provincial level. While city fixed effects remove

comprehensive SEZ.

possible bias from price level differences, differences in inflation over time remain an issue. To address these concerns we use electricity consumption and light intensity measured by satellites as alternative measures for economic activity. The results from these two proxies partly confirm the existence of significant effects of SEZ, although the quantitative interpretation of the result is complicated by some uncertainty about the actual relationship between light (electricity) and GDP.

In summary, our results indicate that SEZ had a significant positive effect on the GDP levels of the treated cities. We also studied the channel of this increase. It appears as if the main effect of SEZ was to attract investments in physical capital. In contrast, there is no evidence of permanent effects of human capital or TFP, although this may partly be due to the low quality of local data on education.

1.1 Related Literature

This paper is related to a large literature that seeks to understand the effects of institutional change on economic development. The main challenge common to such studies is to find sources of variation in institutions in order to estimate causal effects on development. Our contribution is to exploit variation in economic institutions across Chinese cities and time which allows us to control for potential confounding factors when estimating the effects on GDP.

To our knowledge, Wei (1993) was the first to use Chinese city-level data to assess the effects of different types of SEZ. He finds that a set of coastal cities where special policies were introduced in 1984 had a significantly higher average growth rate during the early reform period, while other types of preferential policies did not.[3] But it is unclear to what extend these effects can be interpreted as causal, because the estimation relies only on the variation between cities and the early treated cities were a highly selected group. There are a number of other studies which have followed Wei (1993) in estimating the effect of SEZ on output (see in particular Demurger et al., 2002 and Jones et al., 2003). However, these studies also look at average growth rates over a certain period. There are many potential confounding factors when making comparisons across locations.[4] In our study, we can exploit the variation in the establishment of SEZ across time and cities. This allows us to perform a difference-in-difference comparison which mitigates many of the concerns above.

A number of studies have looked at other outcomes of SEZ. Head and Ries (1996) analyze the location decision of international firms in Chinese cities and find that SEZ had a positive effect and this was amplified by agglomeration

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

[4]Both of these later papers try to solve the problem of confounding the effect of policy with geographic location by including control variables such as distance to the coast. However, there still remains unobserved heterogeneity between cities which may affect the estimation of the treatment effect and our fixed effects specification is better able to deal with these concerns.

effects. Cheng and Kwan (2000) also look at the determinants of foreign direct investment and find that provinces with a zone attract significantly more FDI. These papers confirm the important role of FDI and agglomeration economies, but they cannot exploit the within-city variation which we are using to identify the effect of economic reform. A recent study by Wang (2013) uses a panel of Chinese cities to estimate the effect of SEZ on FDI, exports, output of foreign owned enterprises, domestic investment, TFP growth and prices. Her results from fixed effects panel regressions suggest strong positive effects of SEZ on FDI, exports, and output of foreign enterprises. These measures are closely linked to the outward oriented nature of SEZ which may explain the high magnitudes of the effects of up to 100% increases in levels. The effects on the other outcomes are smaller and not robust. Our findings are complementary to Wang (2013) in the sense that we focus on GDP as a comprehensive measure for the development of the local economy, while she focuses on intermediate targets. Besides the focus on a broader outcome measure, our analysis also differs by making the crucial distinction between state-level and province-level SEZ and by constructing a sample with a consistent definition of cities over time.[5] Furthermore, we compare our results to a sub-sample of cities for which we know the assignment mechanism of SEZ, while Wang (2013) uses matching to compare cities with earlier SEZ to those with later SEZ, which assumes that the assignment mechanism is known and observable in all treated cities.

SEZ have also been studied at the firm level. Schminke and Van Biesebroeck (2013) estimate the effect of a firm being located inside a SEZ on its productivity and export behavior. Their findings suggest that firms inside of SEZ have higher output per worker and higher capital intensity, but not higher TFP. Furthermore, these firms also export more and there seem to be significant differences in the export behavior across types of SEZ. While these findings are in line with ours, they are based on variation across firms and thus rely on the assumption that their set of controls ensures the effect is not confounded with other city characteristics.[6]

Zones with special policies are not unique to China and their general characteristics and their effects have been studied in other countries.[7] In a recent study on US "Empowerment Zones", Busso, Gregory, and Kline (2013) compare locations that were selected for special treatment with similar locations which were rejected or treated in a second round. They have detailed micro-data on

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

[6]Several earlier studies have used more aggregated data at the level of provinces or state to analyze the effect of preferential policies on the inequality and the allocation of capital. Cheng and Kwan (2000) find evidence the more FDI was allocated in provinces with SEZ and Song, Chu, and Cao (2000) find that intra-regional disparities increased due to preferential government policies.

[7]Akinci and Crittle (2008) provide an overview.

employment, housing, and commuting which allows them to do an extensive welfare analysis. Their evidence suggests that the treatment, consisting of tax-credits and subsidies for disadvantaged neighborhoods, had significant positive effects on employment and wages while the costs were relatively small. Their study demonstrates that preferential policies can also be beneficial in developed countries, which makes SEZ an even more attractive tool for governments to initiate and sustain such policies at any stage of development. Our analysis of the Chinese SEZ contributes to this not only by identifying the effects of preferential policies at earlier stages of development, but also by highlighting the key components of the Chinese strategy and its extraordinary success.

Our study relates to a large literature on liberalization and industrial policy in general and specifically the Chinese reform experience.[8] Rodrik (2006) attributes a significant role in the success of reforms to government policies that created distortions in favor of more advanced industries. As we will document below, the key characteristic of the Chinese reform process was an experimental approach towards finding a combination of liberalization and a strong role of the state. The SEZ played an important part in this process and our findings are therefore in line with the view of Rodrik (2004) that the state can generate information about the potential of sectors through experimentation. The argument for a strong role of the state in relatively backward economies is also developed in Acemoglu, Aghion, and Zilibotti (2006), who show that it can be optimal for governments to foster investment at early stages of development.

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

2 China's Economic Reforms and Institutions

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

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

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

1980-1984

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

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

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

1984-1991

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

According to the official statistics, the 14 coastal cities constituted 21.8% of the national total industrial output in 1985.

1991-2003

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

2003-present

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

Besides the state-level zones, a large number of lower-level zones were also established during the same period. The preferential policies, given by the central government to the state-level zones, did not apply to these lower-level zones. On the other hand, the lower-level zones were not under close monitoring and regulation of the central government. In December 2003, the State Development and

[9] Bonded Zones were typical free trade zones.

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

Reform Commission, the Ministry of Land and Resources, the Ministry of Construction, and the Ministry of Commerce together issued Document NO. 2343 to request a thorough investigation on development zones at all levels regarding the violation of the land-use plan. Before the investigation, there were a total number of 6866 development zones of all levels (WEFore (2010)). By the end of 2006 when the investigation was finished, only 1568 zones survived and gained official approval from the state (see State Development and Reform Commission (2006) for the list of zones). A large number of the development zones failed the investigation and were abolished, which included all zones that were lower than province-level and several unqualified province level zones. After 2006, there existed only two levels of development zones – state-level and province-level. Starting from 2010, a number of province-level zones were promoted to state-level conditional on passing certain standards, including performance in economic growth, production safety and environmental protection. By the end of 2010, the number of state-level ETDZ had increased from 54 to 88.

2.1 Experimentation and Convergence in the Policies of the Zones

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

2.2 Different Types of the State-level Development Zones

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

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

2.3 The State-level and Province-Level Zones

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

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

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

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

3 Data

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

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

Dependent Variables

- $logGDP_{ipt}$ is log of nominal GDP of the prefecture-level cities for the period 1988-2010. They are taken from the *China City Statistical Yearbooks*. We use nominal GDP but control for price changes at the provincial level with a set of province-time fixed effects.

- $logElectricity_{ipt}$ is the electricity consumption of the prefecture-level cities for the period 1988-2010, which is available in the *China City Statistical Yearbooks* for the same set of cities as GDP. It measures the use of electricity for households' consumption and industrial production and is an approximation for local economic activity. [11]

- $logLight_{ipt}$, the average light intensity of the prefecture-level cities is another approximation for local economic activity. In the data provided by the National Geographical Data Center, light intensity is measured on each square km (pixel) on a discrete scale from 0-63. We use digital maps of Chinese cities to aggregate the light intensity of the pixels to administrative units.

- $logPC_{ipt}$, the log physical capital stock, is constructed with the perpetual inventory approach. To construct the physical capital stock, we take the data of new investment for the period 1988-2010 from the *China City Statistical Yearbooks.* For some cities, we collect the new investment data from *New China in 60 Year Provincial Statistical Collection* for the earlier period 1978-1987. The province-specific series of investment deflator is obtained from *New China in 60 Years Statistical Collection.*

- $logEL_{ipt}$, the log of efficient labor unit, is constructed using population and educational attainment. We take the population data from *China Population Census* for the year 1990, 2000, and 2010 and interpolate the years in between using population growth rate implied by the *China City Statistical Yearbooks.* The educational attainment data also comes from *China Population Census.*

[11] The electricity consumption of the prefecture area is not available for all years.

- $logTFP_{ipt}$, the log of TFP, is the residual term in log real GDP that can not be accounted for by the physical capital and efficient labor.

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

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

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

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

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

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

4 Empirical Strategy and Results

In this section, we estimate the effects of SEZ on cities' GDP. Our empirical strategy is a difference-in-difference analysis exploiting the variation in economic policy across cities and years following the establishment of SEZ.

Our main outcome variable is GDP at the city level, which we measure in three alternative ways: first, using official statistics (this section), then using electricity consumption and light intensity, two proxies of the level of economic activity (section [Robustness]). The sample period is 1988-2010 for the yearbook data and 1992-2010 for the light data. For these periods, we have a consistent definition of city borders and information on GDP (and the proxies), the date of introduction of SEZ, and the main control variables, land area and population. We exclude from our analysis the four cities with comprehensive SEZ introduced before 1988, and Hainan, where the entire province received the status of SEZ in 1988. Furthermore, we exclude Tibet because of data constraints and all province-level cities because of their different status. For the remaining cities and SEZ, a standard difference-in-difference estimation is possible.

Table [Descriptive statistics] 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 270 cities and 23 years. Figure [Main variables over time] shows the trend of GDP and the two most important control variables. All three variables show a clear upward trend, but real GDP grew much more than population and land size. Our policy variable, the establishments of SEZ, is illustrated in Figure [Share of zone types]. This figure shows how the shares of cities with the different types of zones evolved over time, restricting attention to the balanced sample. The two most important types of zones are HIDZ and ETDZ with shares reaching 31% and 24% in 2010, respectively. Two types of zones existed already before the start of our sample: the first comprehensive SEZ were established in 1980[13] and the first ETDZ were established in 1984 (and continue to be established until today). ETDZ and HIDZ are not only the most frequent zone types over all, but also among those with the most new establishments during the sample period 1988-2010. We will also consider Export Processing Zones (EPZ) and other less frequent types of zones (e.g., BZ and BECZ). Most of these other zones were introduced in cities that already hosted either ETDZ or HIDZ. We control for province-level zones, but we do not combine them with our state-level SEZ because they are less far-reaching in scope and more heterogeneous.

4.1 Baseline Specification

Our baseline specification is a city-level panel regression whose dependent variable is the log of GDP (in current prices). The main explanatory variables are reform indicators switching on (i.e., taking and thereafter retaining the unit

[13]There is no time variation in this zone type in our sample because the only comprehensive SEZ established after 1988 was in a province-level city, which is not part of our prefecture-level city sample. This zone type is therefore excluded from our list of zones.

value) in the year after part of their territory was granted the status of SEZ, and zero otherwise.[14] Note that cities may have multiple zones of different types. Since our goal is to learn about the effect of different types of zones, we will allow in some specifications each city to be subject to multiple treatments. All regressions control for city fixed effects and province-time interactions, which amounts to a difference-in-difference analysis. Standard errors are clustered at the city-level. More formally, we run regressions of the form

$$y_{ipt} = \phi_i + \gamma_{pt} + \alpha I\_Reform_{it} + X_{it}\beta + \varepsilon_{it}, \tag{2}$$

where y_{it} is log nominal GDP, ϕ_i is a city fixed effect, γ_{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. ε_{it} is a normal error term. 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. This is important, since province-level inflation data exist but are not very reliable. City fixed effects absorb time-invariant heterogeneity in city-characteristics like initial development or geographical location. Thus, the identification of the effects of reform is across city-time within each province.

This econometric specification in (2) restricts the treatment effect to a shift in the after-reform GDP level path. Namely, reformed cities can have a higher (or lower) GDP *level* after the reform indicator switches on. This specification is likely to be overly restrictive. One might expect reforms to have cumulative effects on developments, taking the form of temporary or permanent changes in growth rates. For this reason, we will later explore more flexible econometric specifications allowing for trend breaks and distributed lags.

We start our analysis by aggregating all state-level reforms into a single indicator switching on after a city is granted for the first time the status of any state-level SEZ. We construct a similar single dummy for province-level reforms. The estimated coefficients are shown in Table [Baseline specification]. In column (1), we include no control variables except for the fixed effects. The coefficient for "any state-level reform" is positive and highly significant. Switching to being the host of a SEZ increases the average GDP of the treated city by 19% in every post-reform year. 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 can mechanically affect GDP, whether or not in conjunction with the introduction of a SEZ. The size of a city's land area has, as expected, a positive effect on GDP of this city and part of the treatment effect in column (1) seems to be due to changes in city borders. But the treatment effect remains large (14.7%) and highly significant. In column (3) we add log population of the city center as a further control. Population has a strong positive correlation with GDP, while land area loses explanatory power. The treatment effect again drops

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

somewhat but remains large (12.6%) and highly significant.[15] Finally, in column (4) we construct a measure of GDP per capita and use it as our outcome variable. The reform effect is slightly smaller (11.7%), but remains highly significant. In columns (5)–(8) we repeat the analysis for the sub-sample of inland provinces. As discussed above, in this sub-sample the status of SEZ was granted mostly on the basis of administrative criteria, such as being a provincial capital.[16] This is an interesting sub-sample since it involves less severe a selection. To reduce the issue even further, we excludes from the inland city sub-sample cities that were granted the status of SEZ without being provincial capitals. Thus, the restricted inland sample only contains provincial capitals (treatment group) and cities that were never granted the status of SEZ (control group). Columns (5)–(8) in Table [Baseline specification] shows that the results are largely robust to this sample restriction.[17]

4.2 Pre-reform Trends

A concern with our results above is that cities hosting SEZ might have already been on a high-growth trajectory – and possibly were even selected because of their promise of high success. The focus on inland capitals only partially resolves these problems, since there is time variation in the year in which capitals were assigned to the treatment group, and the timing of the reforms may be non-random.

We address this concern by two strategies. First, we investigate whether the performance of treated cities was different from that of other cities in the same province in the years shortly pre-dating the reform. Table [Pre- and post-reform indicators] is the analogue of Table [Baseline specification], 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.[18] If cities were granted the status of SEZ due to their promising pre-

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

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

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

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

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 insignificant. It is useful to note that the point estimates in the restricted sample are similar to those in the full sample, although they are estimated less precisely. In summary, the results of Table [Pre- and post-reform indicators] are reassuring, and suggest that there were no important differences in pre-reform economic performance between treated cities and control groups.[19]

Second, we consider a more flexible specification allowing treated cities to have different time trends from the non-reformers. This addresses the potential concerns that, by not allowing any differential time trends, our baseline specification in equation 2 may attribute incorrectly pre-existing trend differences to the reform. The new specification allows reformed cities to have a linear time trend that makes them diverge from the control group. This trend can be subject to a structural break when a city is granted the status of SEZ. More formally, we consider the following specification:

$$\begin{aligned} y_{ipt} &= \phi_i + \gamma_{tp} + \alpha_1 I\_Reform_{it} + \alpha_2 \left[(t-1987) \times I\_Reformer_i\right] \\ &+ \alpha_3 \left[\max\left\{0, (t - ReformYear_i) \times I\_Reform_{it}\right\}\right] + X_{it}\beta + \varepsilon_{it}, \end{aligned} \quad (3)$$

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, α_2 captures the steepness of a linear trend specific to reformers, i.e., it says how many percentage points the growth rate differs between reformers and non-reformers.

- $ReformYear_i$ is the year in which the first SEZ was introduced in city i (if a city never became a SEZ, then we let $ReformYear_i = 0$). The interaction $[(t - ReformYear_i) \times I\_Reform_{it}]$ allows a differential trend (i.e., a trend break) starting as of the introduction of the first SEZ.[20] The coefficient α_3 measures the steepness of such a trend break.

- α_1 captures a level shift as in the baseline specification of equation 2

The results for the full and restricted (inland) samples are shown in Table [Trend break], columns (1)-(4) and (5)-(8), respectively. We build here on the

the restricted sample, the first three lags are identified out of 18 cities, whereas the fifth lag would only be identified out of 3 cities.

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

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

specification of columns (3) and (7) in Table [Baseline specification], including all control variables (whose estimated coefficients are not reported, for simplicity). The results are robust to the other specifications presented in Table [Baseline specification]. Columns (1) and (5) of Table [Trend break] simply reproduce for convenience columns (3) and (7) in Table [Baseline specification], respectively. In the regressions of columns (2) and (5) we add a linear trend specific to reformers. The estimated coefficient $\hat{\alpha}_2$ is statistically significant in both the full and restricted sample. Yet, such a trend does not distinguish pre- and post-reform periods. 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. Next, in column (3) we allow a structural break in the trend of reformed cities, by including $\max\{0, (t - ReformYear_i) \times I\_Reform_{it}\}$ in the regression. Interestingly, the estimated coefficient $\hat{\alpha}_1$ remains almost unchanged in the full sample and increases slightly in the restricted sample. Moreover, the estimated coefficients of the pre- and post-reform trends, $\hat{\alpha}_2$ and $\hat{\alpha}_3$ are insignificant in both samples. The point estimates suggest the possibility of a structural break (higher growth) at the time of the reform, but the break appears to be statistically insignificant. 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. There is some evidence of differential trends between reformers and non-reformers, but it is non-robust.

The specification above with a trend break allows for a permanently higher growth rate in treated cities. However, a permanent divergence may be too extreme. We consider, then, an alternative specification allowing SEZ to have a non-linear (cumulative) effect of the SEZ, i.e., to trigger a concave depart from the pre-reform trend. To capture this effect we estimate the following alternative econometric specification:[21]

$$\begin{aligned} y_{ipt} &= \phi_i + \gamma_{tp} + \alpha_2 \left[(t-1987) \times I\_Reformer_i\right] \\ &+ \alpha_3 \left[\max\{0, (t - ReformYear_i) \times I\_Reform_{it}\}\right] \\ &\quad +\alpha_4 \left[\max\{0, (t - ReformYear_i) \times I\_Reform_{it}\}\right]^2 + X_{it}\beta + \varepsilon_{it}. \end{aligned} \tag{4}$$

Clearly, if $\alpha_3 > 0$ and $\alpha_4 < 0$, then, SEZ would cause a fast acceleration of growth in the years immediately after the reform, but the acceleration would die off in subsequent years. The regression results from this specification are provided in columns (4) and (8). In both cases, the coefficients $\hat{\alpha}_3$ and $\hat{\alpha}_4$ have the expected signs (positive and negative, respectively), and are (individually and jointly) statistically significant. Interestingly, in the full sample there continues to be some evidence of a positive pre-reform differential trend for reformers. In the restricted sample of inner cities, in contrast, we find no such evidence (the estimated coefficients $\hat{\alpha}_2$ turns negative and totally insignificant). This suggests

[21]It would be possible to include also the term $\alpha_1 I\_Reform_{it}$ to this specification. However, not surprisingly, it becomes very difficult to identify separately all the effects in such a highly parameterized model. Therefore, we omit this term, and regard the current specification as a non-nested alternative to equation 3.

that the government may have picked winners, to some extent, in the full sample, but this is not the case in the restricted (inland) sample where the status of SEZ was granted according to a strict administrative criterion. In both the full and restricted cases there is evidence of a significant effect of SEZ that takes the form of a gradual increase in the GDP level (i.e., a *concave* deviation of the treated cities over time after reforms), rather than in a permanent increase in growth (i.e., a *linear* trend break of the treated cities after reforms).

4.3 Reform Effects Over Time

The analysis of the previous section suggests that post-reform dynamics are non-linear. In this section, we estimate an even more flexible econometric model placing no restrictions on the functional form of post- (and pre-) reform effects. We allow the effect of the reform to vary over the lags (years since the reform) by including indicators for each lag. More formally, we use the following specification:

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

where positive values of $t - Reformyear_i$ measure how long ago (i.e. how many years before the current year t) the reform took place in city i. Negative values measure in how many years city i will be reformed when the current year is t. The maximum number of years since reform, J_F, is 26.[22] We construct these indicators also for the year of reform and the three years prior to the reform (i.e. $J_B = 3$), which will allow us to test whether reforming cities already had a significantly different performance prior to the establishment of the first zone.[23] The omitted categories (for which all indicators are zero) are non-reforming cities and cities more than three years before the reform. The controls include land area, population, and the fixed effects.

We illustrate the results in Figure [Reform effects over time, full sample]. The figure shows that this more flexible specification confirms the results of the previous section. In particular, there is a break in the GDP path at the time of the reform with a temporarily higher growth rate and then a stabilization at a higher level in the long run.[24] Moreover, the magnitudes of the estimated reform effects are comparable to the previous results.[25] There is not much evidence of

[22] This is a city which is observed in 2010 and had its first reform in 1984.

[23] For the same reasons described in the discussion of Table [Pre- and post-reform indicators], we do not include more pre-reform indicators. When we include also indicators for four and five years prior to the reform, then these indicators are marginally significant, but identified by only 27 observations.

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

[25] 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 [Pre- and post-reform indicators]

a higher GDP path in the three years before the reform and the point estimates after ten years are clearly above the confidence interval of the pre-reform years.

We estimate the same regression for the restricted sample of inland provinces (excluding cities which had a reform but are not provincial capitals). The results in Figure [Reform effects over time, inland sample] show a qualitatively similar pattern, but the estimates are less precise and only the effects for the lags 9-12 are statistically different from zero.[26]

4.4 Different Types of SEZ

In this section, we attempt to disentangle the effect of different types of SEZ which had distinct policies. To this aim, we create separate post-reform indicators for each of the three most important (and most common) SEZ: ETDZ, HIDZ and EPZ. In addition, we create a single dummy for other types of state-level SEZ. Table [Effects of different types of zones] has the same structure as Table [Baseline specification] but replaces the indicator for "any" state-level zone with the four separate indicators for each type of state-level SEZ. ETDZ and HIDZ appear to have a large effect. In the full sample, the effects of these two types of zones are quantitatively similar to the one of the first zone in Table [Baseline specification]. In the inland sample, there are two deviations. First, the point estimate of ETDZ becomes insignificant when the dependent variable is GDP per capita (but remains positive). Second, the OtherTypes have in two cases a higher estimate than ETDZ and HIDZ, but they concern only relatively infrequent cases (5% of the SEZ in the sample in 2010). EPZ are insignificant throughout, but 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.

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 effect of "any" zone was building up during about ten years and then stabilized, we now investigate whether this holds also true for the individual types of zones. Since the pre- and post-reform effects of different types of zones can be overlapping (treated cities often had multiple zones of different kinds), this is a demanding specification. Despite this caveat, the result shown in Table [Effects of different types of zones over time] for ETDZ looks remarkably similar to the one for the first zone.[27] HIDZ also show a concave pattern, but a stark drop in the last period. However, this last coefficient is only identified by observations from one year. EPZ and OtherTypes show a more mixed picture and they are largely insignificant.[28]

[26] Note that 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.

[27] It is useful to note here that this is not because ETDZ were established before the other zone types and that they thus would be the first of "any" type. In fact, as Figure [Share of zone types] shows, the HIDZ tended to be established earlier.

[28] 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

4.5 Local Spillover Effects

We have focused so far on the urban center of prefecture-level cities because this is where the zones were established. In order to consider effects which the zones may have on the area surrounding the cities (positive or negative spillover effects), we rerun our baseline regressions with the GDP of the entire prefecture as the dependent variable.[29] The results, shown in Table [Effects on prefecture], indicate that the effects are of comparable magnitudes to the previous case where we focused on the city center and the estimates continue to be significant. Interestingly, this result still holds true when we consider the effects of the zones on the periphery only. To do so, we compute as dependent variable log GDP of the periphery (the difference in GDP of the prefecture and the city center) and the analogue for the control variables land area and population.[30] The results in Table [Effects on periphery] are similar to the ones for the city center. This suggests that the positive effects of the SEZ (which are usually located in the suburban area of the cities) spread to the surrounding areas.

4.6 Level Decomposition

We have shown that the establishment of special economic zones in a city has a positively significant and long-lasting effect on the GDP of that city. The result per se does not provide insights on how the establishment of zones promotes growth. To answer this question, we first decompose GDP into physical capital, efficient labor and TFP. We then estimate the effect of SEZ on the three decomposed components of GDP to identify the channel(s) through which SEZ have an impact on GDP.

We perform a level-decomposition exercise using the log-form of a neoclassical production function.

$$\begin{aligned} \log rGDP_{ipt} &= \log TFP_{ipt} + \alpha_k \log PC_{ipt} \\ &+ (1-\alpha_k)\log EL_{ipt}, \end{aligned} \tag{5}$$

which says that the log real GDP of city can be decomposed into the log TFP, the log physical capital stock and the log of efficient labor. A detailed discussion on the decomposition can be found in the Appendix.

In Table [Decomposition of the Effect], we display the result from baseline difference-in-difference regression with the decomposed components as dependent variables. Column (1) shows that the establishment of a SEZ has significantly positive effect on the physical capital stock. After switching to host a

importance after the WTO accession in 2001, which could explain their upward trend (though insignificant).

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

[30] The small drop in the number of observations is due to some cities for which the center fills the whole prefecture, such that there is no periphery.

state-level SEZ, the city's physical capital stock increases by 14.3% on average. However, the effect pf SEZ is much less prominent in terms of improvement of economic efficiency and productivity. As shown in column (2) and (3), the point estimate of SEZ on human capital and TFP are both positive, but insignificant and almost an order of magnitude lower than the estimate on physical capital. A similar pattern of the regression result is found using only the inland sample, as shown in column (4)-(6). Compared to the full sample, the effect on physical capital is even larger in the inland sample (36%) while the effect on human capital and TFP remains insignificant. Column (7)-(9) display the regression result when we distinguish the zones by their types. The specification is an analog to that of section 1.4. Among all types of zones, the ETDZ seems to have the largest and significant impact on both the physical capital stock (25.5%) and human capital (5.2%). This result confirms the important role of the ETDZ, a discovery we made in section 1.4 with GDP data. As shown in column (9), the effect of SEZ on TFP remains insignificant after we break the policy indicators into different zones types. The above results allow us to conclude that the main effect of the SEZ is to increase the speed of physical capital accumulation of a treated cities. However, we find little evidences suggesting that the SEZ treatment permanently increases human capital and TFP.

5 Robustness

In the previous section we have used a number of different specifications in order to estimate the effects of SEZ on GDP. In this section we discuss the robustness of our results to the use of alternative measures for GDP and to the inclusion of additional control variables. Furthermore, we compare our results to a placebo study with randomly assigned pseudo-reforms.

5.1 Alternative Measures for GDP

Doubts are often raised regarding the reliability of the GDP and price data in China. We are in particular concerned with the following two aspects regarding the reliability of the official statistics: 1) the possibility of a systematic over-reporting of GDP of the reformer cities after the establishment of SEZ,[31] 2) the reliability of city-level price data, because we would like to rule out the possibility that the effect of the SEZ on nominal GDP is simply driven by higher price after the establishment of a SEZ.[32]

To address these concerns, in this section we use electricity consumption and light intensity measured by weather satellites as two alternative proxies for GDP. These measures are unlikely to have a systematic over-reporting related to SEZ and are not affected by prices.

5.1.1 Electricity Consumption

Electricity consumption is widely regarded as a good approximation for GDP. People who have doubt about the validity of China's official GDP data often cite the fact that there existed a large discrepancy between growth rate of official GDP and electricity consumption in 1998, when the Asian currency crisis hit the world economy (for example Rawski (2001)).[33]

Regarding the relationship between electricity consumption and GDP, previous studies report that the elasticity of real GDP with respect to electricity consumption to be around 0.28 at the country level (Henderson et al. (2012)). However, the elasticity at the city-level might be different. Therefore, we first study the relationship between electricity and real GDP on the city-level. The result is presented in Table [Electricity consumption]. Column (1) shows that, in our sample of 275 Chinese prefecture-level cities over the period 1988-2010,

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[31] That is, the local officials have a tendency to over-report the GDP growth in order to meet the expectation from the central government regarding the performance of SEZ. As stated in Young (2003: p. 1224): "Since officials are rewarded for superior performance and punished for failing to meet targets, it is not surprising that they have a tendency to modify their statistical reports in accordance with central policy objectives."

[32] In our main empirical specifications, we always include the province-time fixed effect to control for any changes in province-level prices over time. However, this specification can not rule out the possibility that prices increase in a city after reform relative to non-reformer cities within the same province.

[33] Our calculations also show that the growth rate of electricity consumption dipped around 1998 for both reformer and non-reformer cities.

the elasticity of real GDP with respect to electricity consumption is 0.26. Notice that in column (1), we use nominal GDP as the dependent variable while controlling for the province-time fixed effect to absorb the province level price differences.[34] In column (2), the dependent variable is real GDP and the sample is restricted to the post-1996 period. The real GDP is obtained using city-level GDP deflators, which unfortunately are only available for the post-1996 period. The estimated elasticity drops to 0.18 but remains highly significant. We also use another specification to control for the city-level price differences, namely, we include a city-specific time trend to capture the potentially different path of price change across cities. The estimated elasticity, as shown in column (3), drops further to 0.12, but remains highly significant.[35] We conclude from these results that electricity consumption is, as suggested by previous studies, a valid proxy for economic activity. The elasticity found in previous studies is at the upper bound of our findings.

5.1.2 Light Intensity

Several recent papers have argued that light intensity at night, measured by weather satellites, can be used as a proxy for GDP.[36] Many economic activities such as production, transport, and consumption produce light as a byproduct. Therefore, light intensity is positively correlated with the intensity of local economic activities. [37]

Table [Light intensity in Shenyang and Fushun] shows an example for how this light data can be used. The picture on the left shows the two cities Shenyang and Fushun in 1992, which is the first year for which we have light data available. The picture on the right shows the same area in the year 2009. Higher light intensities are shown as brighter pixels in the pictures. We can see clearly that the illuminated area has expanded and the intensity of light has increased. We calculate the average light intensity within the geographical boundaries (also shown in the pictures) of prefecture-level cities and use this as a proxy for economic activity. Whether light increased more in cities with SEZ like Shenyang

[34]This specification therefore is equivalent to one where we deflate the nominal GDP with the province-specific GDP deflators and the use the price-adjusted real GDP as dependent variable.

[35]It is useful to note here that we are using a demanding specification which also controls for city fixed effects and province-time fixed effects. We believe this is important in order to control for unobserved factors such as variation in technology between cities and over time (see Orlik (2011)).

[36]See Henderson et al. (2012) and Chen (2011) and the literature cited there on the use of light to measure economic activity. Ma et al. (2012) discusses the use of light data in Chinese prefecture-level cities.

[37]Since most of man-made light is produced with electricity, the two proxies, electricity consumption and light intensity, are closely related. Compared with electricity consumption, the advantages of light data are that it comes from a different source (satellites of the US Air Force) and is available at a higher resolution (1 km2). The disadvantages are that it is available only since 1992 after which we have considerably less new SEZ establishments and the measure is censored at the top of the scale. The appendix provides further details about the light data.

than in non-reformer cities like Fushun will determine whether our estimated reform effect is positive.

To capture the relationship between light intensity and real GDP, we run a set of regressions similar to what we do with electricity consumption. The result is presented in column (1)-(3) of Table [Light intensity]. There are two noteworthy differences in terms of control variables. First, the intensity of light is censored at 63 and we therefore include the number of top coded pixels to control for this. Second, light intensity is measured within administrative boundaries of 2010, while the area on which GDP is measured in the yearbooks can vary over time. For this reason, we include the land area as a control variable.

The estimated elasticity between real GDP and light is again similar to the results in Henderson et al. (2012). They find an elasticity of real GDP with respect to light intensity of 0.277 in a panel of 188 countries, while we find an elasticity of 0.294 for our sample of cities (column (1) of Table [Light intensity]). Deflating with city-specific prices (and thereby restricting the sample to years after 1996) reduces the estimated elasticity to 0.13 (column (2) of Table [Light intensity]). When we include city-specific time trends to control for differences in price, the estimate is around 0.19 (column (3) of Table [Light intensity]). Except for the specification with city-specific time trends, the number of top-coded pixels is also positively associated with GDP. These results suggest that there is a strong relationship between GDP and light, but the magnitude depends on how we control for price differences.

5.1.3 The Reform Effect with Electricity Consumption and Light Intensity

We then proceed by estimating our baseline specification with log electricity consumption and log light intensity as the dependent variables.

Column (5) in Table [Electricity consumption] shows the result from regression with electricity consumption. For the purpose of comparison, in column 4 of the same table, we repeat the result from a regression with GDP as the dependent variable, which suggests that on average GDP increases by 12.6% after a city switches to having a SEZ.[38] The estimated coefficient on the reform indicator in column (5) suggests an 11% higher level of electricity consumption in the years after a SEZ was established. The reform effect is significant at the 5% level.[39] The significant effect provides evidence that the SEZ had a positive effect on GDP and this effect is not purely driven by biases in the GDP data or increasing prices in reforming cities. The magnitude of the implied reform effect on GDP, which is the ultimate goal of our study, depends on the elasticity between electricity consumption and GDP. Let us take the elasticity to be 0.259,

[38]Here we use real GDP based on provincial deflators. The results are indistinguishable from the ones with nominal GDP as the dependent variable because the province-time fixed effects absorb this variation.

[39]The coefficient increases to 12.3% and the t-value to 2.45 if top and bottom 1% of the electricity consumption data is trimmed.

which is closest to the one in Henderson et al. (2012) among our estimations. An 11% reform effect on electricity consumption implies the reform effect on GDP to be 2.8%. While this estimate is considerably smaller than the effect on official GDP, one has to take into account that the effect on electricity is measured less precisely and there is a larger error when using electricity as a proxy for economic activity.

Column (4) of Table [Light intensity] estimates the baseline specification with GDP for the sample beginning in 1992. The point estimate is smaller than in the full sample and insignificant. This is most likely because we can exploit less time variation in this sample period: out of the 85 cities in our full sample which had a state-level SEZ, only 31 had their first zone after 1992. The point estimate for the effect on light intensity is somewhat larger and statistically significant at 5% (column (5) of Table [Light intensity]). The estimated effect of SEZ on light of 0.059 translates into 1.73% higher GDP after the introduction of a SEZ.

One caveat of using light intensity as a dependent variable is that top coding is not considered (i.e. light intensity is underestimated). We therefore predict real GDP with light intensity and the number of top coded based on the coefficients in column (1) of Table [Light intensity]. The resulting prediction is used as a dependent variable in our baseline specification and the results are shown in column (6). Once top coding is taken into account, the estimated effect in terms of GDP increases to about 1.9%.

5.2 Controlling for Government Spending

The establishment of a SEZ often triggered government investment in infrastructure and our quantification of the reform effect may depend on how this is taken into account. This is a particularly important concern when one considers transfers from the local government to treated cities. On the one hand, the infrastructure investments are part of the government's strategy to facilitate economic development and are therefore part of the treatment. On the other hand, these investments enter the GDP statistics and our results may overstate the income effects of citizens.

Unfortunately, we do not observe the contribution of such investments to GDP or transfers in detail. However, we do observe the overall expenditures of the local government in most years and we use this measure as a proxy for the contribution of public investments to GDP. The disadvantages of the inclusion of government expenditure are that we lose some observations because we do not observe it for the first year of our sample period and that there could be the reverse effect of GDP on government expenditure via tax revenues. As Table [Government spending] shows, government spending is strongly associated with GDP. The point estimates on the reforms drop somewhat, but they remain positive and highly significant in both samples.

5.3 Earlier GDP Data

We have focused our analysis on the period 1988-2010 for which we have a consistent definition of cities and for which we can control for potential confounding factors such as changes in the boundaries of the cities. This conservative approach makes us more confident that our results are not driven by other factors besides SEZ, but it comes at the cost of losing some variation in reforms because some SEZ were established before 1988. In this section, we estimate our baseline specification for a subset of cities for which we observe GDP also for earlier years.[40] Unfortunately we cannot control for changes in land area, government spending and population as this data is missing for the earlier years. The resulting reform effect is a 12.3% increase in the level of GDP and this estimate is again highly significant. This suggests that our choice of the sample is not driving our results.

5.4 Prices at the City Level

Due to the problematic quality and availability of price data, our analysis of the GDP data controls for price differences by including province-time fixed effects. A more satisfactory approach would be based on real GDP data deflated at the city level. This could be important if differences in prices are affected by the establishment of SEZ. While our city fixed effects absorbs constant price differences between reformers and non-reformers, it remains a concern that prices could increase differentially after the establishment of a SEZ. Although we have shown that alternative measures which are not affected by prices also show positive reform effects, they are of lower magnitude than for GDP.

Official GDP deflators for individual cities are available only after 1996. After this time, only seven cities opened a new SEZ and six of them were EPZ, which is a different type that cannot be compared with the more comprehensive ETDZ and HIDZ. We therefore cannot estimate reform effects when deflating GDP at the city level, but we can compare inflation rates between reformers and non-reformers. Such a comparison shows that reformer cities have a 0.76 percentage points higher inflation rate than non-reformers, after controlling for province-time fixed effects. The average inflation rates over all years for reformers and non-reformers are 12.87% and 12.02%, respectively. If these differences between reformers and non-reformers were persistent, then they would be absorbed by the linear time trend specific to reformers as shown in Table [Trend break]. The remaining uncertainty concerns potential price changes at the time of the reform, which we can only address by using electricity and light.

5.5 Placebo Analysis

In order to check if the true effects of the reform or something unobserved is driving our result, we run placebo regression of the specification in column 3 of

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

Table [Baseline specification]. We first assign the actual number of new zone establishments in each year to a random selection of cities. The distribution of the new zones over time is thus the same as in reality, but they are now artificially implemented in random cities. We repeat this exercise 1000 times. When looking at the placebo coefficient of the first state-level zone, then we observe that it is significant in about 11.7% of the cases, but never has a higher and significant coefficient than the true reform (the average estimate is 0.001 with the placebo regressions, compared to 0.126 with true reforms).

6 Conclusion

China's growth experience during the past 30 years was astonishing. The Special Economic Zones are crucial in understanding this development. As argued by 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." We believe that the Chinese Special Economic Zones are an interesting object to study the effect of economic reform and industrial policy. This paper provides estimates for the impact of Special Economic Zones on local economic performance measured by GDP and two alternative measures of economic activity. We use a number of demanding specifications that control for unobserved heterogeneity at the city level and at the province-time level. The evidence using official GDP suggests that there were indeed positive effects of Special Economic Zones on the cities in which they were located. Our estimates are somewhat smaller than most of the literature has reported so far but still economically meaningful. We also find evidence that the effect of the Special Economic Zones on output went mainly through physical capital. Electricity consumption and light intensity measured by satellites also show some effects but the magnitudes depend on the assumption of the correct relationship between true GDP and electricity or light.

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

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8 Appendix

8.1 Data Sources

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

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

Light and Digital Maps Light intensity at night, an alternative measure for local economic activities, is provided by the National Geographical Data Center for the period of 1992-2010. Using the digital maps of China, we aggregate the light intensity at the level of cities.[41]

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.[42] We also use the Information Site of China's Development Zones[43] and the Report of the Ministry of Commerce (2006). From these sources, we can derive the year in which the zone was established, its type, and its location.

8.2 Sample Selection

In our main estimations, we focus on a sample of prefecture-level cities for the years 1988-2010. The sample is unbalanced because of the creation of new cities: in the year 1988 the sample has 170 prefecture-level cities and this number increases to around 270 in the year 2010.[44] Our sample covers all provinces in

[41] 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.

[42] http://english.mofcom.gov.cn.

[43] http://www.cdz.cn/www/index.asp.

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

China except for Tibet, Hainan and the province-level cities Beijing, Shanghai, Tianjin, and Chongqing. We also exclude the cities of the first wave of comprehensive SEZ.

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

Sample Period The GDP data in *China City Statistical Yearbooks* only go back to the year 1988.[45] 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.[46] Starting from 1983, the municipalities were gradually transformed into prefecture-level cities. Broadly speaking, the prefecture-level cities replaced municipalities as the third layer in the administrative structure. However, the transformation often coincided with various other changes, which we lack the data to control for. For example, a county which was part of the previous municipality may not be part of the prefecture-level city that is succeeding it. At the same time, new counties which were previously under the jurisdiction of a different municipality may become part of the prefecture-level city. Therefore, the composition of newly formed cities may differ substantially from that of the preceding municipalities.

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

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

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

[46]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.

original prefecture-level government. At the same time, a number of county-level cities were promoted to new prefecture-level cities and thereby cut out of their previous prefecture.

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

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

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

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

8.3 Level Decomposition

The following paragraphs provide information on the decomposition of real GDP into physical capital stock, efficient labor and TFP.

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

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

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

The deflator for new investment, $deflator_{pt}^{Inv}$, is province-specific. We set δ_k, the annual depreciation rate for physical capital, to be 0.08.[47]

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

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

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

where Inv_{ip1978} is the new investment in year 1978 and g_{1978} is the average growth rate of real physical capital stock before 1978.[48] 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.[49]

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

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

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

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

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

We use population as an approximation for employment in each city. The reason is that the number of employed persons reported in the *China City Statistical Yearbooks* has some drawbacks: 1) The number of employed persons is defined as registered workers in each city, which might exclude the large number of immigrant workers. 2) There is a huge drop in the number of employed persons in the year 1998, the reason of which is unclear to us.[50]

[47]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).

[48]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.

[49]Notice that our sample starts from 1988, the error of the estimate for initial physical capital stock (1978) would have only marginal impacts on the estimate of the physical capital stock ten years later.

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

To construct city-level population data, first we take the population data for the year 1990, 2000 and 2010 from the *China Population Census*. Then we interpolate the population of the years in between using the year-by-year population growth rate, which is calculated using the city-level population from the *China City Statistical Yearbooks*.[51]

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

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

where s_{ipt} is the average years of schooling and $\phi_t(.)$ 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).[52]

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

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

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

where α_k, the share of capital in the output function, is set to be 0.4.

8.4 Light Data

The light data was downloaded from the National Geographical Data Center.[53] 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 kilometre 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

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

[52] We assume that the return of schooling did not change between 2009 and 2010, i.e. $\phi_{2010}(.) = \phi_{2009}(.)$.

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

Elvidge et al. (2009). Figure [Light from different satellites] shows that light intensity (the average light intensity per pixel, ranging from 0 to 63) increased over time also within the same satellite.

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%

S: State; P: Province

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

Figure 1: Share of zone types

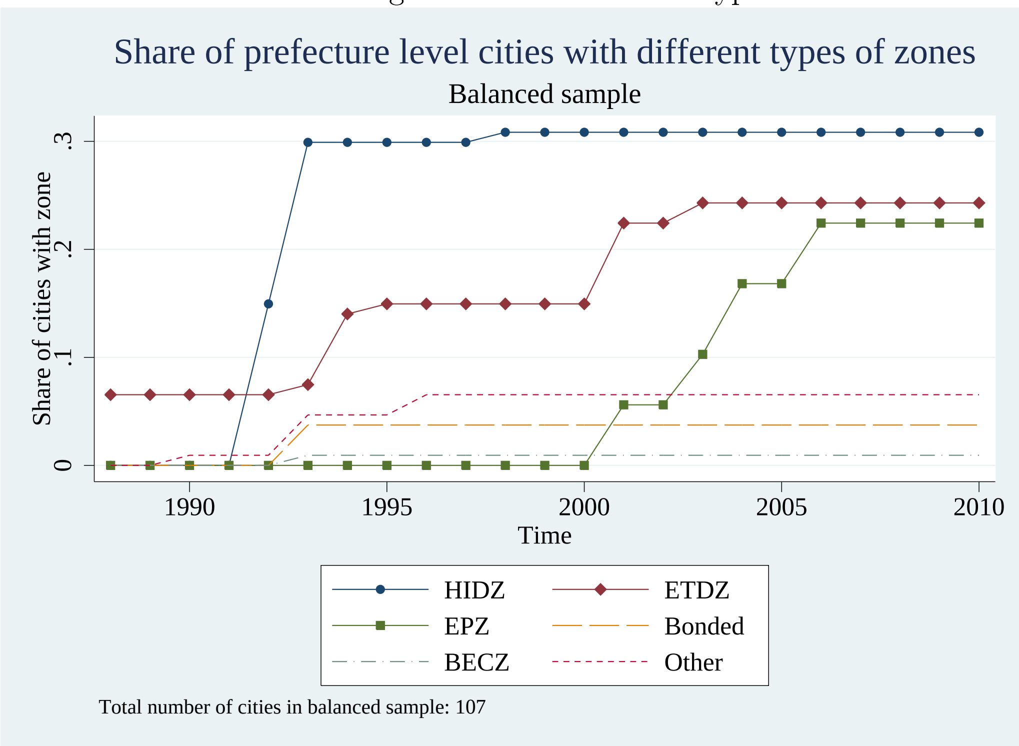


Figure 2: Main variables over time

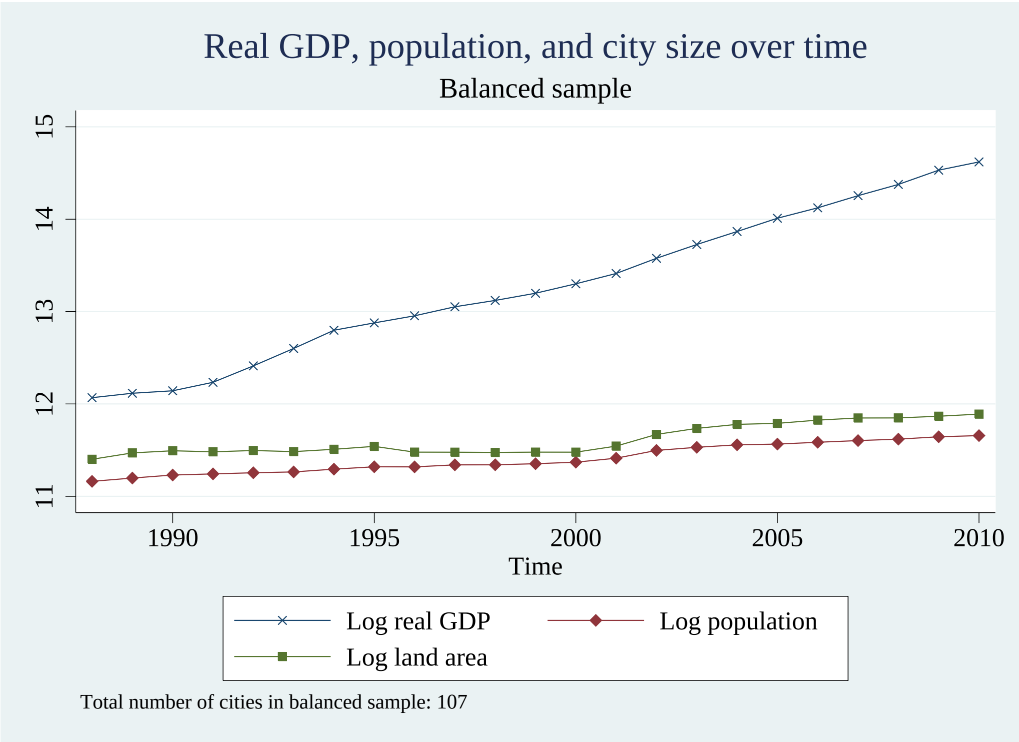


Table 3: Baseline specification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform indicator for any state-level zone	0.190***	0.147***	0.126***	0.117***	0.268***	0.212***	0.181***	0.166***
	(4.55)	(4.29)	(4.43)	(4.15)	(4.87)	(4.11)	(3.50)	(3.09)
Post-reform indicator for province-level zone	-0.00188	-0.00890	-0.00136	0.00226	-0.0192	-0.0288	-0.0118	-0.000854
	(-0.07)	(-0.39)	(-0.07)	(0.11)	(-0.67)	(-1.05)	(-0.43)	(-0.03)
Log landarea		0.240***	-0.0325	-0.154***		0.211***	-0.0527	-0.175***
		(7.73)	(-1.17)	(-7.66)		(5.85)	(-1.21)	(-5.49)
Log population			0.692***				0.672***	
			(12.22)				(7.37)	
Dependent variable	log(GDP)	log(GDP)	log(GDP)	log(GDP p.c.)	log(GDP)	log(GDP)	log(GDP)	log(GDP p.c.)
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

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

City-FE and province-time-FE omitted from table.

Standard errors are clustered by city.

Table 4: Pre- and post-reform indicators

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

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
City-FE and province-time-FE omitted from table.
Standard errors are clustered by city.

Table 5: Trend break

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

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Controlling for land area, population, city-FE and province-time-FE.

Standard errors are clustered by city.

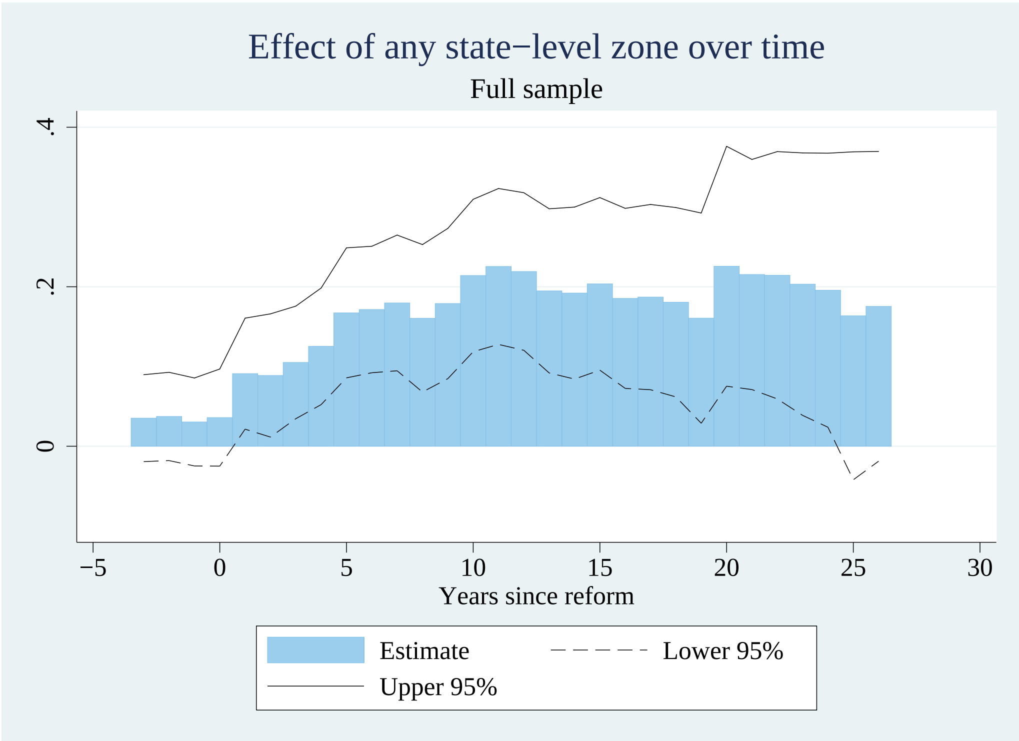


Figure 3: Reform effects over time, full sample. The bars show the coefficients of the indicators for individual years since the establishment of the first state-level zone of any type. The regressions also control for an indicator for province-level zones, land area, population, city fixed effects, and province-time fixed effects. Standard errors are clustered by city.

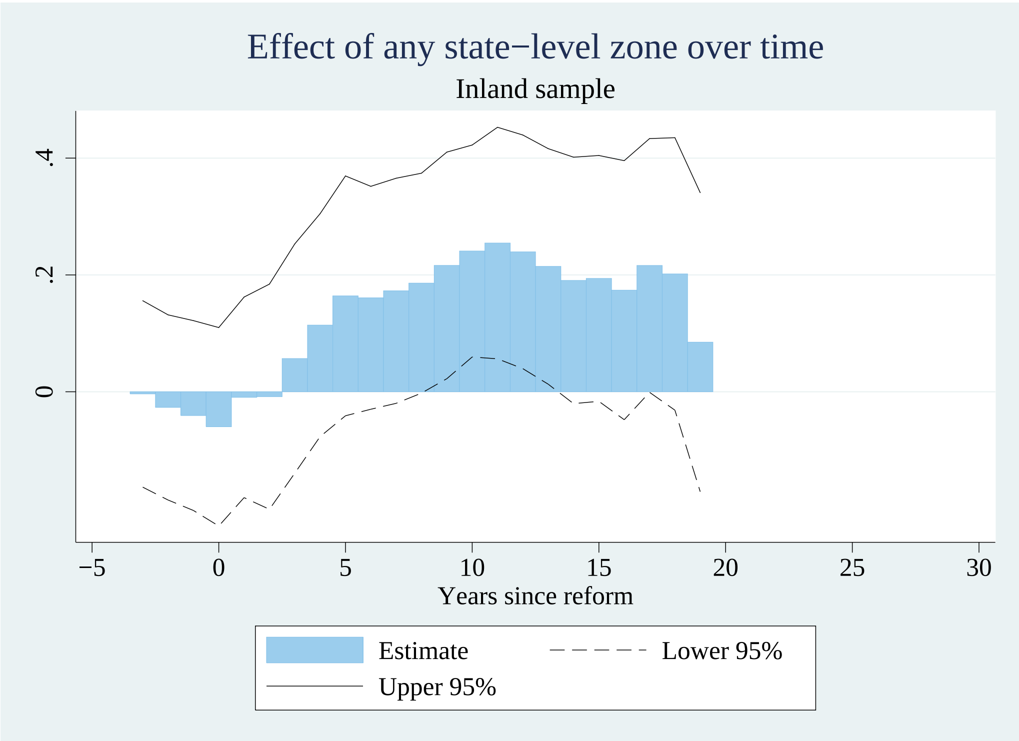


Figure 4: Reform effects over time, inland sample. The bars show the coefficients of the indicators for individual years since the establishment of the first state-level zone of any type. The regressions also control for an indicator for province-level zones, land area, population, city fixed effects, and province-time fixed effects. Standard errors are clustered by city.

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***	0.0968*	0.0578
	(4.61)	(3.66)	(3.17)	(2.71)	(3.96)	(2.98)	(1.66)	(0.98)
Post-reform indicator for HIDZ	0.117***	0.0792**	0.0753**	0.0735**	0.122**	0.0928*	0.106**	0.113**
	(2.63)	(2.19)	(2.45)	(2.40)	(2.57)	(1.80)	(2.18)	(2.25)
Post-reform indicator for EPZ	0.0413	0.0362	0.0206	0.0132	-0.0122	0.00210	0.0491	0.0740
	(0.92)	(0.94)	(0.61)	(0.38)	(-0.15)	(0.02)	(0.65)	(1.04)
Post-reform indicator for OtherTypes	0.0709	0.0581	0.0896*	0.104**	0.148	0.118*	0.211**	0.128*
	(0.99)	(0.93)	(1.81)	(2.21)	(1.41)	(1.69)	(2.17)	(1.84)
Post-reform indicator for province-level zone	0.00145	-0.00716	0.000742	0.00463	-0.0193	-0.0337	-0.0129	-0.00183
	(0.06)	(-0.32)	(0.04)	(0.23)	(-0.68)	(-1.23)	(-0.47)	(-0.07)
Log landarea		0.233***	-0.0361	-0.159***		0.187***	-0.0530	-0.180***
		(7.20)	(-1.30)	(-7.66)		(4.83)	(-1.19)	(-5.55)
Log population			0.686***				0.654***	
			(11.84)				(6.75)	
Dependent variable	log(GDP)	log(GDP)	log(GDP)	log(GDP p.c.)	log(GDP)	log(GDP)	log(GDP)	log(GDP p.c.)
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

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Other Types include BECZ, Bonded Zones, and unkown types.

Standard errors are clustered by city.

Table 7: Effects on prefecture

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform indicator for any state-level zone	0.155***	0.117***	0.131***	0.136***	0.205***	0.178***	0.183***	0.189***
	(4.69)	(4.02)	(4.78)	(4.84)	(2.99)	(3.28)	(3.55)	(3.53)
Post-reform indicator for province-level zone	0.0209	-0.00563	-0.0106	-0.0122	0.0211	-0.0146	-0.0185	-0.0181
	(0.90)	(-0.31)	(-0.63)	(-0.72)	(0.64)	(-0.59)	(-0.80)	(-0.77)
Log landarea		0.409***	-0.0578	-0.218***		0.344***	-0.0181	-0.210***
		(12.88)	(-0.82)	(-6.41)		(5.75)	(-0.23)	(-4.61)
Log population			0.744***				0.686***	
			(7.76)				(6.08)	
Dependent variable	log(GDP)	log(GDP)	log(GDP)	log(GDP p.c.)	log(GDP)	log(GDP)	log(GDP)	log(GDP p.c.)
Administrative level	Prefecture	Prefecture	Prefecture	Prefecture	Prefecture	Prefecture	Prefecture	Prefecture
Sample	Full	Full	Full	Full	Inland	Inland	Inland	Inland
N	5272	5198	5196	5196	2797	2564	2729	2729
AR2	0.957	0.975	0.979	0.973	0.945	0.971	0.977	0.968

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

City-FE and province-time-FE omitted from table.

Standard errors are clustered by city.

Table 8: Effects on periphery

	(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)	
Dependent variable	log(GDP)	log(GDP)	log(GDP)	log(GDP p.c.)	log(GDP)	log(GDP)	log(GDP)	log(GDP p.c.)
Administrative level	Periphery	Periphery	Periphery	Periphery	Periphery	Periphery	Periphery	Periphery
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

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

City-FE and province-time-FE omitted from table.

Standard errors are clustered by city.

Table 9: Decomposition of the Effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post-reform indicator for first state-level zone	0.143***	0.0218	0.0348	0.360***	0.0361	0.00569			
	(3.55)	(0.93)	(1.05)	(6.01)	(0.84)	(0.10)			
Post-reform indicator for ETDZ							0.255***	0.0522*	0.0278
							(4.53)	(1.81)	(0.61)
Post-reform indicator for HIDZ							0.0692	0.0154	0.0130
							(1.56)	(0.50)	(0.29)
Post-reform indicator for EPZ							0.0763	0.0439**	-0.0127
							(1.63)	(2.43)	(-0.39)
Post-reform indicator for OtherTypes							0.0322	0.0860**	0.0651
							(0.40)	(2.51)	(1.52)
Post-reform indicator for first province-level zone	-0.00285	0.0211	0.000508	-0.0416	-0.000426	0.0212	0.000716	0.0235	0.00146
	(-0.09)	(1.36)	(0.02)	(-0.79)	(-0.03)	(0.68)	(0.02)	(1.51)	(0.07)
Log landarea	0.0535**	0.608***	0.0337	0.0674**	0.593***	0.0438	0.0587**	0.609***	0.0344
	(2.20)	(17.96)	(0.70)	(2.05)	(13.85)	(0.70)	(2.45)	(17.94)	(0.71)
Dependent variable	log(phy.cap.)	log(eff.lab.)	log(TFP)	log(phy.cap.)	log(eff.lab.)	log(TFP)	log(phy.cap.)	log(eff.lab.)	log(TFP)
Sample	Full	Full	Full	Inland	Inland	Inland	Full	Full	Full
N	4613	4942	4244	2321	2502	2058	4613	4942	4244
AR2	0.951	0.759	0.741	0.931	0.864	0.775	0.953	0.762	0.741

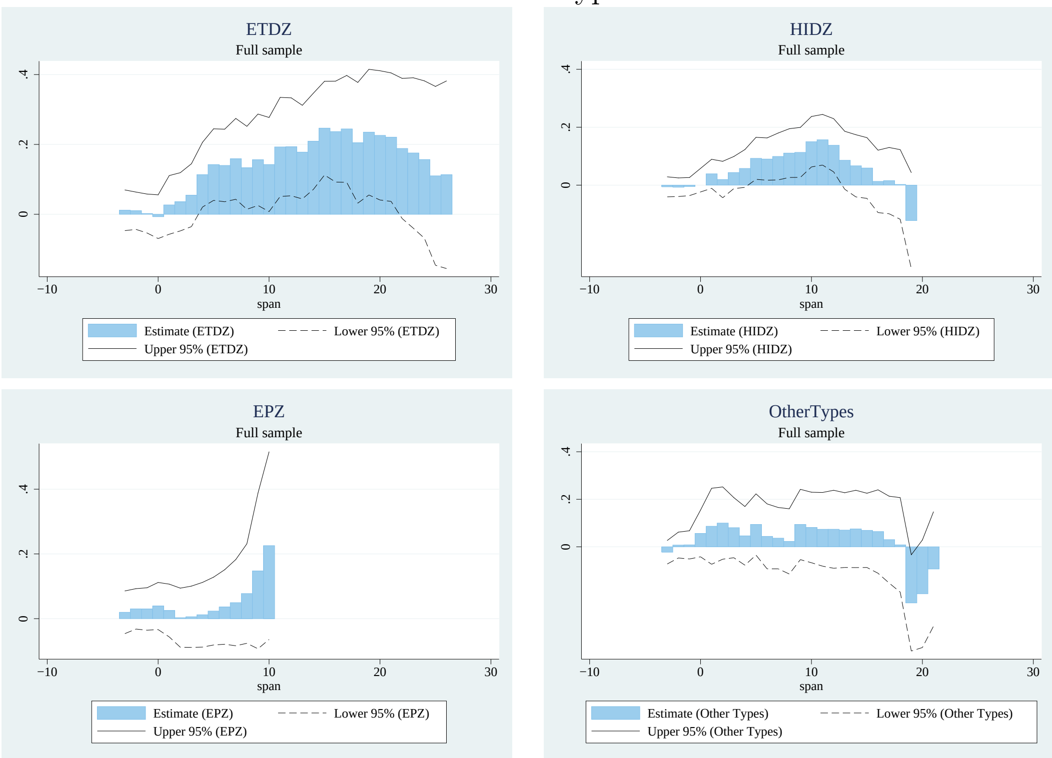
t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

City-FE and province-time-FE omitted from table.

Standard errors are clustered by city.

Table 10: Effects of different types of zones over time



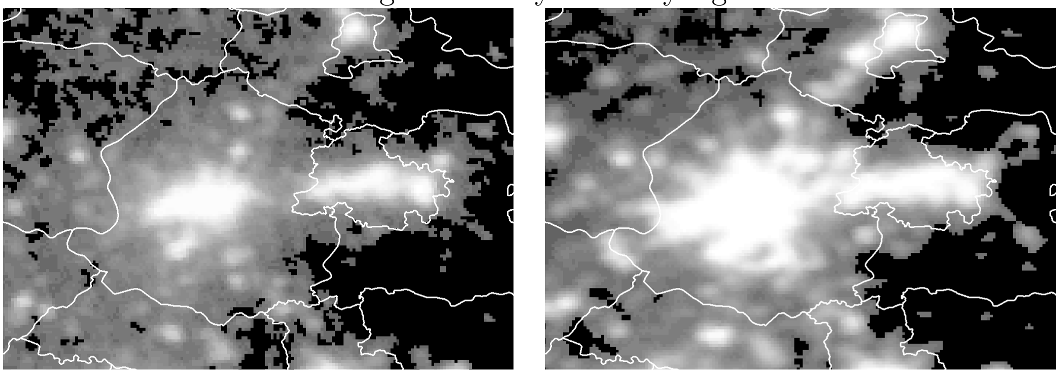
The bars show the coefficients of the indicators for individual years since the establishment of an ETDZ, HIDZ, EPZ, or other types. The regressions also control for an indicator for province-level zones, land area, population, city fixed effects, and province-time fixed effects. Standard errors are clustered by city.

Table 11: Electricity consumption

	(1)	(2)	(3)	(4)	(5)
Log electricity consumption	0.259***	0.208***	0.121***		
	(7.32)	(3.53)	(5.14)		
Post-reform indicator for any state-level zone				0.126***	0.110**
				(4.43)	(2.26)
Post-reform indicator for province-level zone				-0.00136	0.0305
				(-0.07)	(0.89)
Dependent variable	log(Nominal GDP)	log(Real GDP)	log(Nominal GDP)	log(Nominal GDP)	log(Electricity)
Level of GDP deflator	-	City	-	-	-
City-specific linear trend	No	No	Yes	No	No
N	5071	2378	5071	5141	5200
AR2	0.966	0.513	0.982	0.975	0.804

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
City-FE and province-time-FE omitted from table.
Log population and log land area omitted from table.
Standard errors are clustered by city.

Table 12: Light intensity in Shenyang and Fushun



1992 2009

The two satellite pictures show the light intensity on each square kilometer. The brightest light is shown in white. The city on the left hand side is Shenyang, which had an HIDZ in 1991, an ETDZ in 1993, an EPZ in 2004, and another type of zone in 1995. The right hand side city is Fushun, which is a center of the coal industry but did not have a state-level SEZ established during this time. The borders show the boundaries of prefecture-level cities and counties in 2010.

Table 13: Light intensity

	(1)	(2)	(3)	(4)	(5)	(6)
Log light per area	0.294***	0.128*	0.189***			
	(5.82)	(1.87)	(4.49)			
Log number of top coded	0.0215***	0.0159***	0.00343			
	(3.56)	(3.24)	(1.21)			
Post-reform indicator for any state-level zone				0.0416	0.0589**	0.0192**
				(1.27)	(2.34)	(2.30)
Post-reform indicator for province-level zone				-0.000143	-0.0147	-0.00686
				(-0.01)	(-0.84)	(-1.26)
Dependent variable	log(Nominal GDP)	log(Real GDP)	log(Nominal GDP)	log(Nominal GDP)	log(Light)	log(Predicted GDP)
Level of GDP deflator	-	City	-	-	-	-
Controlling for population	No	No	No	Yes	Yes	Yes
City-specific linear trend	No	No	Yes	No	No	No
N	4451	2400	4451	4449	4589	4589
AR2	0.960	0.617	0.980	0.964	0.840	0.997

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
City-FE and province-time-FE omitted from table.
The sample period starts in 1992 for all regressions.
All regressions control for land area.
Standard errors are clustered by city.

Figure 5: Light from different satellites

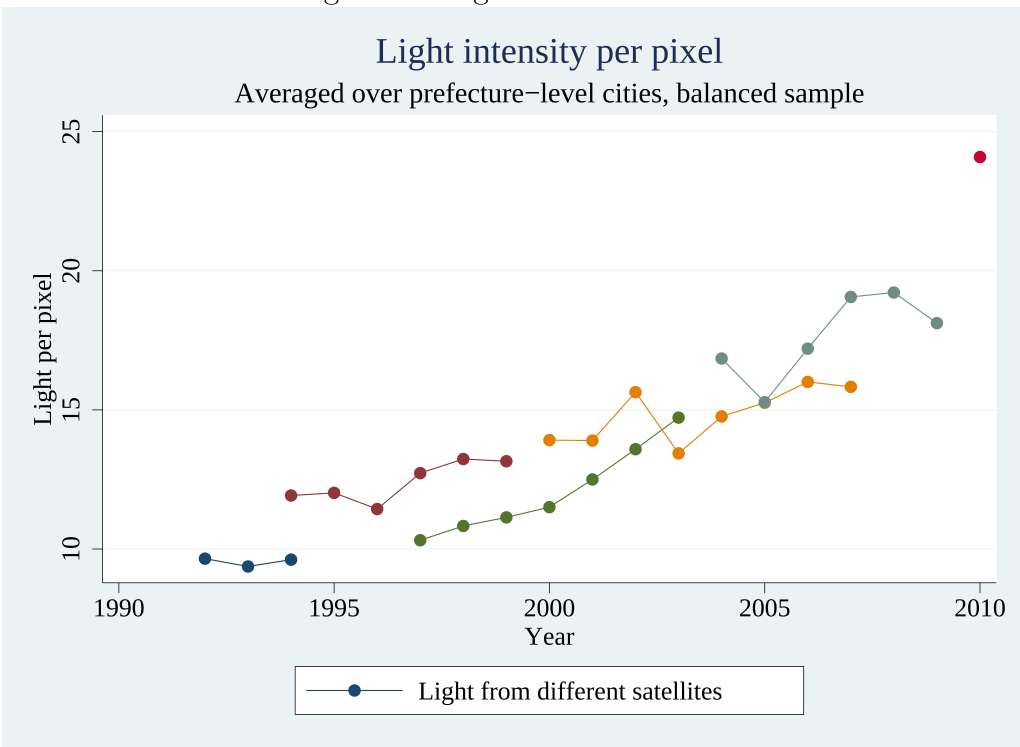


Table 14: Government spending

	(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)	
Dependent variable	log(GDP)	log(GDP)	log(GDP)	log(GDP p.c.)	log(GDP)	log(GDP)	log(GDP)	log(GDP p.c.)
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

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
City-FE and province-time-FE omitted from table.
Standard errors are clustered by city.