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

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March 4, 2016

Abstract

We study the effect of place-based industrial policy on economic development, focusing on the establishment of Special Economic Zones (SEZ) in China. We use data from a panel of Chinese (prefecture-level) cities from 1988 to 2010. Our difference-in-difference estimation exploits the variation in the establishment of SEZ across time and space. We find that the establishment of a state-level SEZ is associated with an increase in the level of GDP of about 20%. This finding is confirmed with alternative specifications and in a sub-sample of inland provinces, where the selection of cities to host the zones was based on administrative criteria. The main channel is a positive effect on physical capital accumulation, although SEZ also have a positive effect on total factor productivity and human capital investments. We also investigate whether there are spillover effects of SEZ on neighboring regions or cities further away. We find positive and often significant spillover effects.

JEL Codes. H72, L52, O25, O38, O53, P21, R11.

Keywords: China, Economic Growth, Economic Reforms, Difference-in-difference, Industrial Policy, Investments, Satellite Light, Total Factor Productivity, Special Economic Zones, Spillovers.

*We thank Oded Galor and four referees as well as Simeon Alder, Thomas Chaney, Silvio Contessi, Florian Hälg, James Kung, Minho Kim, Stelios Michalopoulos, Ben Olken, Jody Ono, Janneke Pieters, Raymond Riezman, Dominic Rohner, Zheng Song, Kjetil Storesletten, Nico Voigtlaender, Xiaodong Zhu, Josef Zweimüller, as well as seminar participants at the Chinese University of Hong Kong, DEGIT XVII, Royal Economic Society Meeting, SED Meeting, Tsinghua Macro Workshop, University of Bern, University of Zurich, Washington University in St. Louis, Missouri Economics Conference, ZEW Mannheim, and UNC Charlotte. We also thank Xiaojun Chen, Florian Hälg, Lingqing Jiang, Yung-Chieh Huang, Liu Liu, Sebastian Ottinger, Matthias Schief, and Laura Zwyssig for excellent research assistance. Financial support from the European Research Council (ERC Advanced Grant IPCDP-229883) is gratefully acknowledged. Each of the three authors declares that he or she has no relevant or material financial interests that relate to the research described in this paper.

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1 Introduction

The process of economic reforms launched in 1978, and gradually extended until current days, has catapulted China into a stellar growth trajectory that has proven resilient. Because a variety of new policies and institutions were introduced simultaneously, even today it is difficult to pinpoint which of them were crucial. This paper aims at contributing to a better understanding of the policy roots of China's success by focusing on a major component of its industrial policy. It also provides new evidence in the debate about the effect of place-based policies.

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

We use a panel of 276 cities over the period 1988-2010.[1] Our econometric strategy is a difference-in-difference estimation controlling for time-invariant heterogeneity at the city level. We also control for province-specific shocks by using province$\times$time fixed effects. We first regress (the logarithm of) GDP or GDP per capita on a reform indicator that switches on (i.e., takes the unit value) in the year after a city has received SEZ status, controlling for city characteristics such as land area. In our baseline specification, the introduction of a SEZ is associated with a permanent increase in the city's GDP level of about 12%. The effect on GDP per capita is about 9%. The result is robust to controlling for local government spending. To account for gradual effects of the reform, we also consider more flexible specifications where the effect of the reform is allowed to vary, both parametrically and non-parametrically, as a function of the time elapsed since the start of the treatment. We find an increasing cumulative effect of the policy treatment that flattens out after about ten years; the long-term effect of a SEZ is estimated to be a differential increase of about 20% in the GDP level. We also study the channels through which GDP and GDP per capita increased as cities were granted SEZ status. SEZ attract larger populations, more investments in physical and human capital, and experience stronger increases in total factor productivity (TFP).

A common objection to place-based industrial policy is that it may induce a concentration of economic activity in some areas by drawing resources away from other locations. We find no evidence of such beggar-thy-neighbor effects on GDP. To detect potential cross-city spillovers, we investigate how the performance of cities varies with their distance from SEZ in other cities. The identifying assumption is that the spillover intensity decays with the distance from the SEZ. Distance is measured in

[1] More precisely, we use data on prefecture-level cities, which are administrative units below provinces and above counties. See Section 3 for details.

three alternative ways: geodesic distance, driving time on the current roads network, and the computed shortest path through the physical geography. In all specifications, SEZ appear to generate positive (often highly significant) spillovers on nearby cities. We also compute measures of *exposure* to other zones by creating a sum over GDP in other cities with a SEZ, weighted by the inverse of distance to those cities. We again find some evidence of positive spillovers, especially strong in inland provinces, albeit often imprecisely estimated. We then investigate whether SEZ lead to a reallocation from areas that are further away from the zone to areas in the proximity of the zone. We consider various rings of up to 400 kilometer around the zone. Spillovers typically decline with distance. Interestingly, we find no negative spillovers even at these medium distances. These results are inconsistent with the hypothesis that the effect of SEZ is driven exclusively by direct transfers and political connections of the cities involved. This could not explain why cities *close* to SEZ benefited from the policy.

Our analysis is subject to two *caveats.* First, the assignment of cities to treatment and control groups may not be random. The Chinese government might have selected cities based on some prior knowledge that the conditions for industrial development might be especially favorable (picking winners), or to the opposite, in order to curb regional inequality. The narrative suggests that a picking-the-winner strategy may have been especially important in the first stage of the reforms, when all SEZ were chosen along the coast and close to potential trading partners and investors such as Hong Kong and Taiwan. Ideally, one would like to have instruments to isolate exogenous sources of variation in the reform treatment, but finding valid instruments is difficult in practice. We mitigate the concern with endogeneity through three complementary strategies. First, we restrict the sample to cities located in inland provinces where the selection of the zones was largely based on a rigid administrative criterion, i.e., being a provincial capital. Second, we augment the regressions with indicators for the immediate pre-reform years to capture differential trends. Third, we control for flexible differential trends depending on the initial conditions of the different cities. This is potentially important, since the cities hosting SEZ are on average more densely populated and more developed than those that did not host SEZ. The results are reassuring: the effect of SEZ is robust in the restricted sample, differentials before the actual establishment of the zone are insignificant, and allowing for differential trends based on the initial development or population density does not significantly affect the coefficients of interest.

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

1.1 Related Literature

Our paper contributes to the large international literature studying the effect of place-based policy, comprehensively reviewed by the recent papers of Glaeser and Gottlieb (2008), Kline and Moretti (2014a), and Neumark and Simpson (2015). In developed countries, place-based policies often target the development of lagging regions. China's SEZ incorporate both efficiency and equity motivations together with the additional target of experimenting with market reforms. On the efficiency side, SEZ pursued the reduction of pre-existing distortions and the exploitation of agglomeration effects. On the equity side, the expansion since 1992 toward inland cities promoted the development of poorer Chinese regions.[2]

In line with the results of our paper, the literature finds positive effects of place-based policies in a number of instances. Criscuolo *et al.* (2012) use firm level data to study an investment subsidy program in the U.K. and find positive effects on employment, investment, and net entry. However, contrary to our study, they find no effect on TFP. Busso *et al.* (2013) compare locations selected for special treatment, such as tax-credits and subsidies for disadvantaged neighborhoods, with similar locations that were rejected or treated in a second round. They conclude that the policy had significant positive effects on employment and wages, while the efficiency costs were relatively small. Kline and Moretti (2014b) study the long-run effects of place-based policies by focusing on a subsidy program in the U.S. that supported lagging regions. They find positive direct effects on productivity. Martin *et al.* (2011a) in contrast do not find positive effects from subsidies to Local Productive Systems in France.

Some papers try to assess, as we do, whether place-based policies generate spillovers – either positive or negative – to non-treated areas. The evidence is mixed. Criscuolo *et al.* (2012) aggregate their observations to larger geographical units that incorporate neighboring non-treated areas. They find that the positive treatment effect is not reduced by this aggregation, suggesting that there were no negative spillovers through reallocation from non-treated to treated firms within the same area. This is similar to our finding that SEZ had a positive effect on the prefecture area around the urban core. Furthermore, we also find some evidence of positive cross-city spillovers. Neumark and Kolko (2010) find insignificant employment spillovers of California's enterprise zones and Martin *et al.* (2011a) obtain a similar result for France. One economic rationale for place-based policy is to foster local agglomeration forces. Kline and Moretti (2014b) find no aggregate gains through agglomeration forces, because local gains are offset by losses elsewhere. Greenstone *et al.* (2010) estimate the effect of large plant openings on incumbent firms' TFP. They find that these agglomeration spillovers are positive but vary substantially across different cases. Briant *et al.* (2015) and Devereux *et al.* (2007) also find evidence of heterogeneous effects of place-based policies.

We are not the first to study the effects of China's SEZ. Most of the earlier studies, arguably due to data constraints, rely on comparisons of the cross-sectional variation in economic performance rather than on a difference-in-difference methodology. Wei (1993) uses city-level data for a sample of coastal cities where special policies were introduced in 1984, and documents that cities hosting SEZ have a significantly higher average growth rate during the early reform period, while other types of preferential policies do not produce the same effects.[3] Since his sample ends in 1990, when only a small subset of the cities had been granted the status of SEZ, his identification relies on the cross-sectional comparison between early reformers – a small and arguably selected group – and cities that were never granted the

[2] Akinci and Crittle (2008) provide a cross-country comparison specifically focusing on different types of special economic zones and their role for development.

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

SEZ status at the time of his study. Wei's pioneer study is extended by Démurger *et al.* (2002) and Jones *et al.* (2003), who also document differences in growth rates between treated and non-treated cities. Different from these articles, our study exploits the staggered establishment of SEZ across cities. This allows us to estimate the treatment effect controlling for time-invariant heterogeneity (city fixed effects) and time-varying province-level shocks.

A recent study by Wang (2013) also uses a panel of Chinese cities and finds, using a difference-in-difference approach similar to ours, positive effects of SEZ on foreign direct investments (FDI), exports, and the output of foreign enterprises. The effects on other outcome variables (which do not comprise GDP) are smaller and less robust. Our findings are complementary to Wang (2013) insofar as we focus on GDP and GDP per capita, a comprehensive measure for the development of the local economy, while her study focuses on intermediate targets of the policy. An important difference for our analysis is that we distinguish between state-level and province-level SEZ (see below for a detailed motivation for this choice). Without drawing such a distinction, the introduction of SEZ would yield no statistically significant effect on GDP in our sample. Other studies focus on different economic outcomes. For example, Cheng and Kwan (2000) show that provinces hosting SEZ attract significantly more FDI than do other provinces. Head and Ries (1996) analyze the location decision of international firms in Chinese cities and find that SEZ have a positive effect that is amplified by agglomeration economies.

A number of studies look at firm-level data. Schminke and Van Biesebroeck (2013) estimate the effect of being located inside SEZ on firms' productivity and export behavior. They find that firms in SEZ export more, have higher output per worker and higher capital intensity, but no higher TFP once selection is controlled for. Their control group consists of firms outside of the SEZ in the same industry and in the same broadly defined regions (west, central and coastal). Our finding of positive effects of SEZ on TFP hinges on a comparison of the average performance of firms before and after the onset of a SEZ. Lu *et al.* (2015) compare firms that are located inside of SEZ with firms across the zone boundary and find positive effects. Similarly, Zheng *et al.* (2015) study local spillover effects of SEZ in eight Chinese cities using firm-level data for the period 1998–2007. They find positive spillover effects of SEZ on productivity and consumption in the area surrounding the SEZ. This result is consistent with our finding that there are positive effects of SEZ on the periphery around the urban core. Finally, Brooks *et al.* (2015) study the role of collusion in industrial clusters and find that collusion is particularly strong in SEZ.

Our study also relates more generally to a large literature on liberalization and industrial policy, including specific applications to the Chinese reform process.[4] Rodrik (2006) argues that government policies creating distortions in favor of more advanced industries played an important role in the success of Chinese reforms. Dewatripont and Roland (1995) and Rodrik (2004) argue that, through experimentation, the state can generate information about the potential of different sectors. Brandt and Zhu (2010) find that rising TFP in the private sector was an important driver of China's growth. Our findings are broadly consistent with these views. Finally, our study has some similarity in both the methodology and motivation with Aghion *et al.* (2008) studying the effect of industrial policy (the demise of the License Raj) in India. Similar to our study, they exploit the fact that the reforms were staggered across time and sectors. However, different from our study, they emphasize the interaction between the reform and state-level characteristics of the labor market. Moreover, they study an episode of pure liberalization (delicensing), while China's industrial policy also entails proactive policy elements (tax credits, subsidies, etc.).

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

The rest of the paper is structured as follows. Section 2 provides the historical and institutional background of the Chinese industrial policy. Section 3 describes the data sources and the sample. Section 4 discusses the empirical strategy and the main results. Section 5 decomposes the effects of the SEZ into factor accumulation and total factor productivity. Section 6 discusses the spillover effects of the policy. Section 7 performs a variety of robustness checks. Section 8 concludes. The Online Appendix contains additional tables, figures and details on the data.

2 Institutional Features of SEZ

Since its establishment in 1949, the People's Republic of China relied on rigid economic planning. The two decades preceding Mao's death in 1976 were characterized by a volatile economic performance and by an intense social turmoil.[5] The reformist political leadership that won the battle for Mao's succession in 1978, led by Deng Xiaoping, faced the desperate need for measures to restore social cohesion and revitalize the economy. There were, however, no existing blueprints showing how to proceed. Learning-through-experimentation then became the guiding principle of economic reforms. As Deng put it: "one has to grope for stepping-stones as he crosses the river." The first policy breakthrough happened in rural areas, where the Household Responsibility System entitled farmers, after fulfilling their procurement quota, to the rest of their agricultural output. However, the leadership soon realized that reforms had to be extended to urban China, and that industrialization necessitated opening up China to foreign investments.

The idea of SEZ was *per se* no Chinese innovation. China's SEZ inherited some essential characteristics of the Export Processing Zones (EPZ), which had already been established in over 80 countries by 1980 (Naughton 2007 and Vogel 2011). Like EPZ, 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 (Vogel 2011). The local officials of the zones were implicitly encouraged to be innovative in designing economic policies and institutions. Successful innovations were retained and extended to later waves of development zones (Yeung *et al.* 2009).

2.1 The Timeline of SEZ

In the year 1980, four cities in the provinces of Fujian and Guangdong, Shenzhen, Zhuhai, Shantou, and Xiamen, were granted the SEZ status.[6] The success of the experiment was remarkable: between 1980 and 1984 Shenzhen grew at an annual rate of 54%, and in 1984 the four SEZ alone attracted 26% of China's total FDI. In addition, the zones had developed a set of well-functioning markets for labor, land, capital, transportation, and technology (Zeng 2010).

The establishment of SEZ met the resistance of the conservative fraction of the Communist Party's (CCP) central committee that viewed renting China's land to foreign companies and allowing them to exploit China's cheap labor as unacceptable. However, the success of the experiment strengthened the reformist fraction in the CCP and softened the conservative opposition. In 1984, 14 coastal cities were

[5] For more detailed analyses of the economic growth of China before and after the start of economic reforms, see Cheremukhin *et al.* (2015), Song *et al.* (2011), and Storesletten and Zilibotti (2014).

[6] The SEZ status implied tax deductions, special tariffs for import and export, and exemptions from the regulations on foreign exchange and land use. Foreign firms that resided inside of the SEZ first enjoyed two years of tax holiday, then three years of a low tax rate of 7.5%, and after the initial five years a tax rate of 15%. Outside of the zones, the tax rate for foreign firms was 33% and for state-owned firms 55% (see Wei 1993).

granted the right to build Economic and Technological Development Zones (ETDZ). The ETDZ shared most of the policies and privileges granted earlier to the initial four SEZ.

During January and February of 1992, Deng made a celebrated tour to southern China, including stops at the SEZ of Shenzhen and Zhuhai, to mark the end of a period of political instability and to restate the commitment of the CCP to the reform process. Shortly afterwards, a new SEZ called Pudong New Area was established in Shanghai. In May, the CCP announced the plan to grant the five inland cities along the Yangtze River, nine border cities, and all thirty of the provincial capital the same privileges as the SEZ (Fewsmith 2001). Following the instruction, several ETDZ and High-tech Industry Development Zones (HIDZ) were approved during 1992-1993 and 2000-2002, all located in inland provinces.

In the first decade of the XXIst Century, the introduction of SEZ spread quickly across China. By 2005, the system of state-level development zones comprised 54 ETDZ, 53 HIDZ, 15 Bonded Zones (BZ) and 60 Export Processing Zones (EPZ).[7] In the year 2005, the 54 ETDZ accounted for 4.49% of the national GDP and for 14.93% of national export (Ministry of Commerce 2006). Establishing a development zone became a common strategy for the local government to attract FDI and foster local economic growth. Through shuffling local officials across different regions, the governments diffused the knowledge and experiences accumulated in the early zones to help develop new SEZ (Xu 2011). Figure 1 shows that by 2010 SEZ had been established throughout the country.

2.2 Different Types of the Special Economic Zones

To summarize the discussion above, there exist five types of state-level SEZ: Comprehensive SEZ (CSEZ, a label we coin to distinguish the early zones from the general notion of SEZ), ETDZ, HIDZ, BZ, EPZ, and in addition Border Economic Cooperation Zones (BECZ). They all share preferential treatment in terms of tax deduction, custom duty deduction, reduced land-use price, flexibility in signing labor contract and financing. However, they are administered by different authorities: the CSEZ, ETDZ and HIDZ are directed by the State Council (the HIDZ being co-directed by the Ministry of Science and Technology); BZ and EPZ are directed by customs; BECZ were directed by the State Council until 2007, and are now under the control of the Ministry of Commerce.

In addition, the zones differ in their stated mission. The goal of the CSEZ and of the ETDZ is to attract FDI and to boost export activity. They are also explicitly encouraged to design and experiment with new institutions and policies. The goal of HIDZ is to foster domestic high-tech industries. The BZ are free-trade zones located in coastal port cities or border cities where import and export can be expedited at a higher speed. The function of EPZ is 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 within pre-existing ETDZ and HIDZ. The BECZ intend to take advantage of the location of the border cities to foster trade with other countries.

Aside from *de jure* changes, the central government is likely to have supported SEZ by assigning capable local leaders and providing administrative support. Because of data limitations (in particular, we have no data for transfers from the central government to cities), in the baseline regressions we simply regard any such complementary measure as part of the treatment. However, in the robustness analysis of Section 7.2 we attempt to separate the effects of government spending and road infrastructure, for which we have data.

[7] See section 2.2 for details on the difference between the zones.

Together with state-level SEZ, China saw the proliferation of a variety of development zones under the authority of provinces.[8] There are some important differences between state-level and province-level SEZ. The state council explicitly requests that "*the policies given to the province-level development zones should not be comparable to those given to the state-level ones,*" in order to prevent excessive competition between the zones and the waste of land resources (State Administration of Taxation 2004).[9] The political autonomy of the province-level zones is also much more limited. Finally, many province-level zones target specific industries whose selection depends on the capture of local interests. Overall, province-level SEZ are a patchwork of different policies rather than a coherent policy instrument. This causes a severe measurement error problem. In our analysis below, we find that province-level zones have an insignificant effect on economic development.

3 Data

The main data source is the National Bureau of Statistics of China (NBS), that publishes the *China City Statistical Yearbooks* including GDP, electricity consumption, population, education, investment, foreign direct investment, government spending, government income, and land area. In addition, we use the light intensity data from weather satellites as a proxy for GDP. More detailed information about the data sources is provided in the appendix.

The main unit of analysis is a *prefecture-level city*, an administrative division ranking below a province and above a county in China's administrative structure. A prefecture-level city comprises a core urban area and a surrounding periphery that may include rural areas, other smaller cities, towns and villages. The NBS reports separate statistics for the core and the periphery of each prefecture-level city. In our baseline we use the larger definition of the prefecture-level city that includes the core and the periphery, but we have also done the analysis when restricting to the urban core. One advantage of considering the larger area as opposed to focusing on the urban core is that border changes are less frequent for the former.[10] Henceforth, unless an ambiguity arises, we refer to a prefecture-level city as a *city*.

The sample period is 1988-2010. At instances, city borders were changed by administrative reforms. While this was less frequent for the borders for the broad definition of a city (including the periphery) than for the urban cores, it is important to take the changes of borders into account. This information on changes in the land area is reported in the *China City Statistical Yearbooks*. We focus on 276 cities, excluding from our analysis the four cities in which CSEZ were introduced before 1988, as well as Hainan, where the entire province received the status of SEZ in 1988. We drop two city-year observations where a county-level city was promoted to a prefecture-level city which implied that it incorporated the periphery, but the associated border change occurs with a one-year delay in some variables. Furthermore, we exclude Tibet, where we have data for only one city, and the province-level municipalities, including Beijing, Chongqing, Shanghai, and Tianjin, because our set of province-time fixed effects would absorb all variation in GDP.

[8] Online Appendix Table A1 lists the number of state-level and province-level development zones and their average share of industrial output in three coastal provinces hosting a large share of SEZ. The data are from WEFore (2010) for the year 2009. All three provinces have a larger number of province-level than of state-level zones (a ratio of 7:1). However, the state-level zones account for a far larger share of industrial output.

[9] Such competition is also a concern in other countries. See for example Ossa (2015) for a general equilibrium analysis of subsidy competition in the U.S.

[10] Although we can track border changes (of the core and the periphery) over time by controlling for land area as reported in the statistical yearbooks, they are less of a concern when considering the larger area.

3.1 Main Variables

We start by listing the outcome variables that are used as the dependent variables in the regression analysis. Unless stated otherwise, the variables from the yearbooks are for the city area that combines the urban core and the periphery.

- $\log GDP$ and $\log (GDP/L)$ are, respectively, the (logarithm of) GDP and of GDP per capita at the city level. Population measures are constructed based on the census and the statistical yearbooks.
- $\log Electricity$ is the electricity consumption and is available for the same set of cities as GDP but only for their urban cores. It measures the use of electricity for household consumption and industrial production.
- $\log Light$ is the average light intensity. In the data provided by the National Geographical Data Center, light intensity is measured on approximately each square km (pixel) on a discrete scale from 0-63. We use digital maps from 2010 to aggregate the light intensity of the pixels to administrative units. We use the maps of urban cores, which corresponds to the level at which the electricity data are available.[11] When using $\log Light$, we must restrict the sample to the period 1992-2010 for which the light data are available.
- $\log (K/L)$ is the physical capital per capita. The physical capital stock is constructed by applying the perpetual inventory method to the investment data for the period 1988-2010, assuming an annual depreciation rate of 8%. For some cities, we collect the investment data from the *New China in 60 Years Provincial Statistical Collection* for the earlier period 1978-1987. The province-specific investment deflator is from the *New China in 60 Years Statistical Collection*.
- $\log L$ is the population size (a proxy for the labor force). Population data is available from the census and, annually, from the *China City Statistical Yearbooks*. The census data is more comprehensive (in particular, it includes *non-hukou* population), but it is only available every ten years. Therefore, we construct the observations between two editions of the census based on the growth rate from the *China City Statistical Yearbooks*.[12]
- $\log h$ is the average human capital, constructed using average educational attainment of the population over the age of 6. The educational attainment data comes from the *China Population Census*.
- $log\ TFP$ is total factor productivity, constructed with an estimated production function and physical capital, human capital, and population of each city.

Next, we discuss the construction of the explanatory variables. The main variables of interest are indicators for the presence of SEZ. For each of the different types of SEZ we construct a dummy, I_Reform_{it} (where i denotes the city, and t denotes the year), which switches on (i.e., takes the unit value) in the year *after* the establishment of a zone and retains the unit value in all following years.

[11] Note that, unlike for GDP, we can hold the area of the urban core constant when measuring light intensity based on the 2010 maps. The concerns due to border changes therefore do not apply here.

[12] A detailed description of this process can be found in Online Appendix B.

Formally, we define the reform indicator based on the establishment of a zone as

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

where $ReformYear_i$ is the year in which the zone was established in city i and t is the current year. In our baseline specification we will focus on the *first* state-level zone that was established in city i. Note that for cities that never host a zone $I\_Reform_{it} = 0$ for all t. We also construct separate dummies for each lag from the reform year, as discussed in more detail in the empirical sections.

3.2 Control Variables

We use two main control variables from the *China City Statistical Yearbooks*. First, the geographic size of the city, to which we refer as land area measured in square kilometers. This variable is available annually and varies over time, reflecting changes in the legal city boundaries. Second, in some specifications, we control for population size.

In order to assess spillover effects that may depend on distance or transport costs between cities, we calculate a variety of different measures related to distance or driving time between cities. First, we calculate the geodesic distance in kilometers between all pairs of cities in our sample. Second, we calculate the driving time on the current road infrastructure using Google maps. Third, we use topographical features such as the slope of the terrain and use shortest path algorithms to construct transport cost measures.

3.3 Summary Statistics

Table 1 shows the summary statistics of the dependent variables and of the main control variables.[13] We have over 5000 observations for GDP from an unbalanced panel of 276 cities from 1988 to 2010. Our policy variable, the establishment of SEZ, is illustrated in Figure 2. This figure shows the time evolution of the shares of cities hosting the different types of zones in the balanced sample. The figure also shows the share of cities that have any state-level zone. The two most important types of zones are HIDZ and ETDZ with shares reaching 26% and 22% in 2010, respectively. Two types of zones existed before the start of our sample: the CSEZ, established in 1980, and a few early ETDZ, established in 1984. ETDZ and HIDZ are altogether the most frequent zone types. We also consider Export Processing Zones (EPZ) and other less frequent types of zones (e.g., BZ and BECZ), introduced in cities that already hosted either ETDZ or HIDZ.

We report the mean values of city characteristics separately for reformers and non-reformers in Table 2. We distinguish three broad categories, with breakdown by coastal and inland cities: cities that received the first SEZ before 1988, cities that received the first SEZ in 1988 or later, and cities that never hosted a SEZ in the sample. As the table shows, cities hosting a SEZ were larger in terms of population and richer in terms of GDP per capita. They also tended to have more universities relative to other cities. Government spending over GDP was instead higher in non-reformers. Our empirical specification controls for city fixed effects filtering out the effect of time-invariant heterogeneity. However, one might be concerned about pre-treatment differences having differential effects on growth or on the effectiveness

[13] For the dependent variable we show the statistics for real GDP based on provincial price deflators, but in the empirical analysis we use nominal GDP because the province-year fixed effects absorb price changes at the province level. See also the next section.

of the policy treatment. Our strategies to address these challenges are explained in detail in Sections 4 and 7.

3.4 Price Deflators

The *China City Statistical Yearbooks* report nominal GDP for the period 1988-2010. Since Chinese price data are regarded as somewhat unreliable (see, for example, Young 2003), we opt to use nominal data. Time-invariant differences in price levels across cities and time-varying inflation differences across provinces are absorbed, respectively, by city and province×time fixed effects. This approach would be problematic if inflation rates differed significantly across cities within each province. The main concern is that the SEZ treatment might systematically increase local inflation. We check if there are differences in inflation rates between treated and non-treated cities in those years for which real GDP data are available from the NBS. More precisely, we compute an implicit city-level deflator using the data on nominal and real GDP, and compare it between cities with and without a SEZ. We find that, within each province, cities with a SEZ did not have higher inflation.[14] As an alternative strategy that avoids relying on prices altogether, we use electricity consumption (in GWh) and light intensity as proxies for the level of economic activity.

4 Empirical Strategy and Results

4.1 Motivation

In this section, we discuss the econometric strategy and the main results. We use a difference-in-difference estimator exploiting the variation in economic policy across a panel of cities and over 23 years following the establishment of SEZ.

Although the focus of the paper is empirical, and we do not present a formal model, it is useful to motivate and interpret our analysis in the light of spatial equilibrium models such as Greenstone *et al.* (2010) and Redding (2012). Greenstone *et al.* (2010) construct a model economy comprising many locations where firms produce using labor, capital, and land. Firms are perfectly mobile, and their profits are equalized in equilibrium. Workers are only partially mobile due to idiosyncratic preferences for certain locations, such that utility is equalized across location but wages are not. Local productivity spillovers imply that total factor productivity depends on the pool of labor that works and lives in a given location. Their framework can be applied to our environment by interpreting the onset of a SEZ as a policy shock that reduces firms' costs in the treated locations. This induces firms to relocate or expand their activity within the SEZ. Agglomeration externalities and technology transfer from foreign firms (or from more productive Chinese firms that relocate to the SEZ) may increase total factor productivity. The (possibly gradual) inflow of firms is limited by congestion externalities, as new firms bid up the prices for local factors such as land and labor. The higher costs offset the initial increase in profits, providing an equilibrating mechanism. The dynamic adjustment eventually comes to a halt when firms' profits and workers' utility are equalized across locations. In the new spatial equilibrium, total factor productivity, the stock of capital and labor, and ultimately the GDP are permanently higher in the SEZ.

[14] The real GDP index of cities is available from the NBS for the period 1996-2010. For this period, cities with a SEZ had an average yearly inflation rate of 1.8%, while cities without a SEZ had an average of 2.3%. The difference is not statistically significant. We also run a panel regression of prices on the reform indicator and control for city and province-year fixed effects. The estimate is -0.008 and insignificant.

Guided by this model, we investigate, first, if the onset of a SEZ triggers an increase in GDP and GDP per capita relative to other cities. In a world of perfect capital and labor mobility, we should expect a permanent increase in TFP, factor accumulation, and GDP while labor productivity (GDP per capita) should eventually be equalized across locations. To the opposite, in a world with no labor mobility GDP per capita would also be permanently higher in treated cities. In China, labor is not immobile but migration is subject to frictions such as the *hukou* system. Thus, we test whether the onset of a SEZ affects both GDP (and its components) *and* GDP per capita. We defer the analysis of the effect of the policy on factor accumulation and TFP to Section 5 below.

4.2 Baseline Specification

In this section, we run regressions whose dependent variables are the logarithms of either GDP or GDP per capita. When we run regressions for GDP, we do not control for changes in labor since these are part of the outcome variable. When we run regressions using GDP per capita as the dependent variable, we do control for population to account for decreasing returns to labor.[15]

The main explanatory variables are reform indicators switching on in the year after part of a city's territory is granted the status of a state-level SEZ.[16] All regressions control for city fixed effects and province-time interaction dummies. Standard errors are clustered at the city level. More formally, we run regressions of the form

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

where y_{ipt} is the logarithm of nominal GDP or nominal GDP per capita, 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 state-level SEZ is established. X_{it} is a vector of time-varying control variables and ε_{it} is a normal error term. City fixed effects absorb time-invariant heterogeneity in city characteristics like initial development or geographical location. Thus, the effects of reforms are identified across city-time within each province. Province-time fixed effects control for time-varying province-specific shocks that can play a confounding role. In particular, they absorb cross-province inflation differentials.

The econometric specification in (1) restricts the treatment effect to a shift in the after-reform GDP (GDP per capita) level path; namely, in reformed cities the GDP per capita *level* (or trend) is allowed to shift whenever the reform indicator switches on. Below, we explore more flexible econometric specifications allowing for trend breaks and distributed lags.

The estimated coefficients are shown in Table 3. In column (1), we include no additional control variable except for the city fixed effects and province-time dummies. The coefficient of the state-level SEZ is positive and highly significant. Becoming the host of a SEZ increases the average GDP of the treated city by about 15.6% in post-reform years. In contrast, the effect of province-level reforms is small and insignificant. In column (2) we include the logarithm of the city's land area as a control. This variable controls for changes in city borders, which are relatively frequent in China and would change GDP mechanically.[17] Increases in land area appear to be positively associated with aggregate GDP. The estimated effect of the SEZ decreases to about 11.6% but remains highly significant.

[15] In an earlier version, we also show results for GDP per capita if one does not control for population. The results are qualitatively similar to those shown in Table 3.

[16] We also construct a similar separate dummy variable for province-level reforms. Note that including the year of the reform in the dummy does not alter the baseline results significantly.

[17] In the robustness section 7.5 we discuss the results when instead of controlling for land area we allow for structural breaks in the city fixed effects when there are border changes.

In column (3) we show the results of regressions where GDP per capita is the outcome variable and where we control for the logarithm of population.[18] The estimated effect of the reform is 9.27%.[19] This suggests that part of the increase in GDP is due to labor reallocation (something we document more explicitly in Section 5 below). In columns (4)–(6) we repeat the analysis for the sub-sample of inland provinces.[20] This sub-sample involves a less discretionary selection of individual cities. To mitigate concerns about the selection further, we exclude cities that were granted the status of SEZ in spite of not being provincial capitals. Thus, the restricted inland sample only contains provincial capitals (treatment group) and cities that were never granted the SEZ status (control group). Columns (4)–(6) in Table 3 show that the results are robust to restricting the sample to inland provinces.[21] The coefficient of interest is positive and significant, and even larger than in the full sample.

4.3 Pre-Reform Trends

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

We address this point through a variety of 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 predating the reform. Table 4 is the analogue of Table 3, reporting the results of regressions where we add four pre-reform indicators taking on the unit value, respectively, in the year of reform and one, two and three years before the reform.[22] If cities were granted the status of SEZ due to their promising pre-reform trends, these coefficients ought to be positive and significant. In contrast, we find the estimated coefficient of the pre-reform dummies to be mostly negative and insignificant. In column (5) the indicators for the reform year and for one year before the reform are marginally significant but negative. The treatment effect in the full sample continues to be positive and significant (columns (1)-(3)). In the inland sample the estimate is positive and significant in column (4), and it is positive but insignificant in columns (5)-(6). In summary, the results of Table 4 are reassuring and suggest that treated cities did not show higher economic performance already before the reform.[23]

[18] The coefficient on population size is negative, suggesting that an increase in the population size due, e.g., to immigration, has a negative effect on labor productivity.

[19] This specification in column (3) is equivalent to controlling for the logarithm of population density and land area. In Section 7.5 we investigate the role of density in more detail, and we also discuss the concern that population and population density could be endogenous. The results are robust to using lagged variables and alternative ways of controlling for border changes.

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

[21] Arguably, inland capitals are *per se* a special group. Since the selection of treated cities was based on an administrative criterion (rather than on unknown, possibly heterogeneous criteria), we can better control for features making capital cities different from the control group. In Section 4.4 we allow cities to have year fixed effects that depend on such city characteristics, and we find that the results are similar.

[22] We also explored longer lags. The lags for five years prior to the reform are never significant in the full sample. In the inland sample some of the earlier lags become significantly negative but only in the specification in column (4) that does not control for changes in land area. Note that lags longer than three years are identified out of a significantly smaller set of reforming cities (since many cities were granted the SEZ status in the early 1990's, and our sample starts in 1988). For instance, in the full (inland) sample the first three lags are identified out of 75 (31) cities, while the fifth lag would only be identified out of 31 (18) cities.

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

Second, we consider a more flexible specification allowing treated cities to have different time trends from the non-reformers. This addresses the potential worry that in our baseline specification the positive effect of SEZ might arise spuriously due to the omission of pre-existing trends. The new specification allows the GDP of cities that are hosting a SEZ to have a linear time trend that differs from the control group's trend already before the reform. In some specifications, we even allow this trend to undergo a structural break at the time when the reform indicator switches on. More formally, we consider the following specification:

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

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

- $I\_Reformer_i$ is a dummy identifying cities that were reformed at any time. $t \geq 1988$ denotes the year of the observation. Therefore, α_2 captures the steepness of a linear trend specific to reformers, i.e., how many percentage points the growth rate differs between reformers and non-reformers.
- $ReformYear_i$ is the year in which the first SEZ was introduced in city i (if a city never became a SEZ, then we let $ReformYear_i = 0$). The interaction $[(t - ReformYear_i) \times I\_Reform_{it}]$ allows a differential trend (i.e., a trend break) starting as of the introduction of the first SEZ. The coefficient α_3 measures the steepness of such a trend break.
- α_1 captures a level shift as in the baseline specification of Equation (1).

The results for the full and restricted (inland) samples are shown in Table 5, columns (1)–(4) and (5)–(8), respectively. The results are robust to using GDP per capita as the dependent variable and controlling for population. Columns (1) and (5) of Table 5 reproduce columns (2) and (5) of Table 3 for comparison. In the regressions of columns (2) and (6) we add a linear trend specific to reformers. The estimated coefficient $\hat{\alpha}_2$ (time trend of reformers (state-level)) is statistically significant in both the full and the restricted sample. Interestingly, the coefficient $\hat{\alpha}_1$ continues to be highly significant in the full sample, although much of the effect is now absorbed by the trend. However, it becomes insignificant in the restricted sample. The trend in columns (2) and (6) does not distinguish pre- and post-reform periods. Thus, in columns (3) and (7) we allow a structural break in the trend of reformed cities, by including $\max\{0, (t - ReformYear_i) \times I\_Reform_{it}\}$ in the regression. Interestingly, the estimated coefficient $\hat{\alpha}_1$ remains almost unchanged in the full sample and increases in the restricted sample. Moreover, the estimated coefficient of the pre-reform trend, $\hat{\alpha}_2$, decreases and becomes insignificant in both samples. The post-reform trend, $\hat{\alpha}_3$, is positive but insignificant in the full sample and positive and significant in the inland sample. Altogether, the statistical specification studied so far suggests that the baseline model with a GDP level shift performs better than one allowing for a trend break implying a permanent GDP divergence between the treatment and control groups.

The specification of columns (2)–(3) and (6)–(7) – allowing for permanently diverging paths – may be too extreme. We consider, then, an alternative specification allowing SEZ to have a non-linear effect of the SEZ relative to the pre-reform trend. To avoid an over-parameterization, we omit the level shift, and we estimate the following alternative econometric specification:[24]

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

The regression results from this specification are provided in columns (4) and (8). In both cases, we find that $\hat{\alpha}_3 > 0$ and $\hat{\alpha}_4 < 0$, implying that the SEZ are associated with an acceleration of growth in the immediate post-reform years, but that the acceleration dies off in subsequent years. The coefficients are both individually and jointly statistically significant in the full sample, while the square term is negative but insignificant in the inland sample.[25] In summary, this specification suggests that the effect of SEZ is a significant gradual increase in the GDP level, rather than a permanent increase in growth (i.e., a *linear* trend break of the treated cities after the reforms).[26]

4.4 Heterogeneous City Characteristics

In the previous section, we allow different trends between treated cities and non-reformers. An alternative strategy is to control for differential trends associated with the initial characteristics of cities. This is an important check, since Table 2 shows that cities hosting a SEZ were more populated and had a higher initial development measured by GDP per capita than other cities. One might worry that the heterogeneity in these initial characteristics might be the actual driver of economic performance over time, and that our baseline specification might spuriously attribute those effects to the establishment of SEZ.

To address this concern, we interact each year dummy with the log difference between certain city characteristics and their respective median values in the year they were first measured. We do this for the city characteristics GDP per capita, population, population density, and number of universities and include the interactions together in the regressions.[27] This allows the flexible growth path to depend on cities' initial characteristics and assumes this interaction to be log-linear.[28]

[24] It would be possible to also include the term $\alpha_1 I\_Reform_{it}$ to this specification. However, it is very difficult to identify separately all the effects in such a highly parameterized model. Therefore, we omit this term, and regard the current specification as a non-nested alternative to Equation (2).

[25] $\hat{\alpha}_3$ and $\hat{\alpha}_4$ are jointly significant at 5% in the full sample and at 10% in the inland sample.

[26] Clearly, the quadratic model is not a correct specification itself, since it would imply a negative long-run effect of SEZ. Given the short sample, the data only capture the increasing part of the quadratic relation. See Section 4.5 for a more general specification.

[27] We calculate for each year the median of the variables across all cities. When we restrict the regression sample to inland provinces, then we calculate the difference relative to the median in this restricted sample. Since our sample is an unbalanced panel, the year in which cities appear in our sample can vary. However, the results are robust to restricting the sample to a balanced panel of 172 cities. The sample size is reduced here because of missing data for the number of universities, but the results are also robust to excluding the interactions with the initial number of universities and thus using the larger sample.

[28] Consider for example a city i that enters our sample in 1988, and whose GDP p.c. is reported in the yearbook. The interaction effect between a year dummy (for example 1995) and the log difference between GDP p.c. in 1988 and the median in that year then is

$$D_{1995} \times \left[\log(GDP_{1988,i}) - \log(GDP_{1988,median})\right].$$

The estimate on this interaction would capture how much higher GDP p.c. is in 1995 for city i when the log difference changes by some percentage. Therefore, cities with median initial characteristics have a time path as given by the main year dummies, and the interactions with initial characteristics allow differential relative paths for cities above or below the median.

The results are shown in Panel A of Table 6. The coefficients of interest are similar to Table 3 in the full sample and larger in the inland sample. In both cases, they remain highly significant. In Panel B, we provide the results from an alternative specification where the year dummies are interacted with a set of indicators for whether a city has a GDP per capita, population, population density, number of universities, respectively, that is above the median in the year in which that variable first appears in the yearbooks for that city. The four sets of interaction effects are then included together in the regressions. The results of Panel A and Panel B are relatively similar.[29]

In summary, the effects of SEZ are robust to controlling in a flexible way for differential trends associated with heterogeneous initial conditions.

4.5 Event Study

In this section, we perform a non-parametric analysis of the effects of the reform with the aid of a model that imposes no functional form restrictions on post- (and pre-) reform effects. All effects are captured by separate lag- or lead-specific dummies. More formally, we run the following regression:

$$y_{ipt} = \phi_i + \gamma_{t,p} + \sum_{n=-J_B}^{J_F} \alpha_n I_{it}^n \{(t - Reformyear_i) = n\} + X_{it}\beta + \varepsilon_{it}, \tag{4}$$

where positive values of $t - Reformyear_i$ measure how many years before year t city i became the host of a SEZ. Negative values measure how many years ahead of t city i will be reformed. Note that this specification allows us to identify some of the lagged effects out of reforms that took place before 1988. For instance, a city that hosted its first SEZ in 1984 will have variation for all leads ranging from 4 to 26 years. In our baseline specification, instead, such a city would display no within variation, and the reform indicator would be collinear with the city fixed effect. In our sample, the maximum number of post-reform leads, J_F, is 26, corresponding to cities which hosted their first SEZ in 1984. We also construct these indicators for the year of reform and the three years prior to the reform (i.e. $J_B = 3$), so we can test whether reforming cities already had a significantly different performance prior to the establishment of the first zone.[30] The omitted categories (for which all indicators are zero) are never-reforming cities and reformed cities more than three years before the reform. The controls include the logarithm of land area and the usual set of fixed effects.

The results for GDP are displayed in Figure 3. The results for GDP per capita are shown in Figure 4 and will be discussed in Section 5 where we decompose the effect. The graphs show the lead and lagged effects of the treatment n years after the reform (for instance, n=10 measures the effect ten years after the introduction of a SEZ). The upper graph in Figure 3 shows the effect on GDP in the full sample. This specification confirms the results of the previous section. In particular, there is a break in the GDP path a year after the reform, followed by a temporarily higher growth rate that levels off after about ten years. The size of the effect is comparable to that in the previous section. There is only some marginal, statistically insignificant evidence of a higher GDP growth in the three years before the reform, indicating a possibility for some minor positive selection. Note that the standard errors increase nineteen years after the establishment of the zone (corresponding to the vertical line added to

[29] The difference between Panel A and B in the sample size is due to cities with zero universities in the first year, such that the log difference in Panel A is not defined.

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

each figure). This is due to a significant drop in the number of observations, since many cities were reformed in 1991 and 1992.[31]

We estimate the same regression for the restricted sample of inland provinces (excluding cities which had a reform but are not provincial capitals), see the lower graph in Figure 3. The qualitative pattern and the point estimates are similar, although the estimation is less precise.[32]

4.6 Different Types of SEZ

In this section, we attempt to disentangle the effects of the different types of state-level SEZ. 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. Appendix Table A2 has the same structure as Table 3 but replaces the indicator for *any* state-level zone with the four separate indicators for each type of state-level SEZ. ETDZ and HIDZ individually appear to have a significant effect on the level of GDP. In the full sample, the effects of ETDZ and – to some degree – also HIDZ are relatively similar to those of the first zone in Table 3. The point estimates on ETDZ and HIDZ in the inland sample are relatively similar to the full sample, but less precisely estimated. The effects of ETDZ and HIDZ in the inland sample tend to be lower than for the first zone reported in Table 3. EPZ are insignificant in both samples, while OtherTypes are mostly significant and have particularly large estimates in the inland sample.[33] Overall, the disaggregation highlights the relative importance of the ETDZ and HIDZ, which are the two largest and most comprehensive types of zones in our sample, as well as those most explicitly emphasizing technology.

Since the effects of any zone has been shown to build up gradually during about ten years and then level off, we investigate whether the same pattern holds true for the individual types of zones. Since the pre- and post-reform effects of different types of zones often overlap (treated cities often had multiple zones of different kinds), the approach in Section 4.5 is demanding. Nevertheless, the resulting picture is reasonably clear. Figure A1, which can be found in the appendix, plots the coefficients of the different types of zones (estimated in the same regression) over the years since the reform. The first panel shows that the pattern for ETDZ looks remarkably similar to that of Figure 3 (first zone reformed). The second panel shows that HIDZ also display a concave pattern, although the effect appears to decline after lag 13. EPZ and OtherTypes show a more mixed picture (the two lower panels in Figure A1).[34] The standard errors are large and the effects are estimated imprecisely. In summary, most development effects appear to stem from ETDZ and HIDZ.[35]

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

[32] 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. In separate regressions not shown here, we find that if residuals are clustered at the province×years of reform (instead of city) level, the effects after nine years are mostly statistically significant and positive in the inland sample. Two of the pre-reform indicators are also significant but negative.

[33] It should be noted that the estimates on OtherTypes are based on few observations. 14 cities have a zone type other than ETDZ, HIDZ, or EPZ, but in 11 of these the zone this is in conjunction with an ETDZ or HIDZ.

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

[35] Recall that some zone types like ETDZ and HIDZ may target cities with certain characteristics such as having universities. This could raise concerns about selection and we address this in a similar fashion as in Section 4.4. When we include the interactions of year fixed effects with initial characteristics (GDP p.c., population, density, and number of universities), then the estimates on these zone types are relatively similar. The two exceptions are that in column (5)

5 Decomposing the Effects of SEZ

In this section, we investigate the channels through which the SEZ promote economic development by decomposing the effects on physical capital per capita, average human capital, population, and TFP. To construct TFP, we assume the aggregate technology to be described by a Cobb-Douglas production function in physical capital and efficiency units of labor (raw labor $\times$ average human capital). We use the local population size as a proxy for raw labor and the average years of schooling to measure human capital (see appendix for details). The aggregate production function is estimated using an OLS estimator from the panel of observations of output, capital, population, and average educational attainment of the population, including city fixed effects and province-time fixed effects.[36] We then use the estimated parameters to compute (the logarithm of) TFP as the residual component.[37]

In panel A of Table 7, we display the results of baseline difference-in-difference regressions analogous to those in Table 3, where, respectively, GDP per capita, capital-labor ratio, population, and human capital are used as the dependent variables. In both the full sample (column (1)) and the inland sample (column (5)), becoming the host of a state-level SEZ is associated with a significant and positive increase in the GDP per capita. This result is identical to that of columns (3) and (6) in Table 3. Columns (2) and (6) show that the establishment of SEZ is associated with an increase in the capital-labor ratio by 13.1% and 33.9% for the full and inland sample, respectively, both effects being highly significant. Columns (3)-(4) and (7)-(8) suggest that the SEZ have no significant effect on population and the human capital measure in the *China City Statistical Yearbook* data. However, both effects are positive and significant when one restricts the analysis to more precise population data from the decennial census, as is shown in panel B in columns (2)-(3) and (5)-(6). Population increases by 9.7% and 11.1% after the establishment of a SEZ in the full and the inland sample, respectively,[38] while the average years of schooling of the population above 6 increase by 0.18 years in the full sample and 0.36 years in the inland sample.[39] The increase in human capital can be explained by either selective immigration (i.e., cities with a SEZ attract more educated immigrants) or by stronger incentives for locals to accumulate

ETDZ becomes significant while HIDZ loses significance and that in column (6) ETDZ becomes significant.

[36]The estimation of production functions can suffer from simultaneity bias, because profit-maximizing firms choose inputs after knowing the realization of productivity shocks, and selection bias, related to exit and survival of firms. In the firm-level literature, it is common to use the correction proposed by Olley and Pakes (1996). For example, Brandt *et al.* (2012) find that the TFP growth of Chinese firms is underestimated when the endogeneity bias is uncontrolled for. Martin *et al.* (2011b) estimate a Cobb-Douglas production function using firm level data. They find that after controlling for simultaneity bias, TFP is still very close to the one obtained using a simple OLS estimation. Since we use aggregate data, we follow the traditional approach and use an OLS estimator. This is related to the growth accounting literature including Hall and Jones (1999) and Caselli (2005). See also Hsieh and Moretti (2015) for an application to city-level data.

[37]More formally, we let

$$\log TFP_{it} = logY_{it} - \hat{\alpha} \times logK_{it} - \hat{\beta} \times log\left(h_{it}L_{it}\right) - \hat{\gamma}_{pt} - \hat{\chi}_i, \tag{5}$$

where Y_{it} is GDP, K_{it} is physical capital stock, h_{it} is human capital, and L_{it} is population; $\hat{\alpha}$ and $\hat{\beta}$ are the estimated coefficients of the Cobb-Douglas production function; $\hat{\gamma}_{pt}$ is the estimated province-year dummy, and $\hat{\chi}_i$ is the estimated city fixed effect capturing, respectively, province-level common trends and city-level time-invariant components of productivity. TFP_{it} measures the city$\times$time variation in TFP.

[38]The difference is likely due to non-hukou population which is captured in the census data but not in the yearbook data. Since in panel A we compute population for the years in between the census based on the growth rate in the yearbooks, the annual variation does not fully reflect non-hukou migrants and is subject to measurement error. See also robustness section 7.4, where we discuss the use of census data.

[39]Ideally, we would prefer to use the educational attainment of the working population (age 25-64). However, this is not available in the census. In Appendix Table A4 we break down the result by different educational levels. The most salient effect is the increase in the share of college graduates.

human capital. Despite the higher population, GDP per capita increases after the introduction of a SEZ because GDP increases more than population. This is shown for the census sample in columns (1) and (3) in panel B.

The estimated effect of SEZ on TFP is shown in panel C of Table 7. In the specification of columns (1) and (4), TFP is estimated without imposing any restriction on the parameters of a Cobb-Douglas production function. The unconstrained estimation of the production function yields output elasticities of capital and labor of 0.3 and 0.6, respectively. In columns (2) and (5), we impose constant returns to scale, obtaining elasticities of 0.35 and 0.65. Since there is some evidence that the labor share has been declining in China (see Bai and Qian 2010), in columns (3) and (6) we estimate the production function separately for pre- and post-1995 subperiods.[40] In all specifications of the full sample, the SEZ have a positive and significant effect on TFP (columns (1)-(3)). As shown in columns (4)-(6), the estimated effects on TFP in the inland sample are positive but insignificant, except in column (6), where TFP is estimated separately for pre- and post-1995 sub-samples.

Figure 4 shows the effect of SEZ on GDP per capita, capital-labor ratio, human capital, and TFP, respectively, as an event study. The effects on GDP per capita and on the capital-labor ratio are concave over time. Both paths feature a break one year after the reform. In particular, the effects of SEZ on GDP per capita and on the capital-labor ratio become statistically significant around seven years after the reform. There appears to be some concavity in the effect on TFP as well, although less clearly and not statistically significant in the individual years. Human capital appears to be higher in cities with SEZ (in this case, some effects are already detected prior to the reform).

6 Spillovers

In this section we study whether the effects of SEZ spill over to other locations. SEZ could have negative spillovers on other cities if the policy attracts investments and workers away of other areas (beggar-thy-neighbor effect). Positive spillovers could accrue from the diffusion of knowledge and an increase in market access. Investigating the spillover effects of the SEZ is important to assess the overall effect of SEZ on economic development. The existing literature on spillover effects on non-treated locations is ambiguous (see Neumark and Simpson (2015) for an overview of the evidence).

In order to estimate the spillovers of SEZ on other cities, we make the identifying assumption that spillovers – either positive or negative – are decreasing in the distance from SEZ.[41] This assumption is motivated by the evidence documented in the previous literature that distance plays a crucial role for spillovers. For example, Jaffe *et al.* (1993) and Keller (2002) find that spillover of knowledge significantly decreases with geographic distance.[42] We consider various alternative measures of geographical distance (as described below) in order to test the robustness of our results. To estimate the spillovers based on these distance measures, we first assume that the spillovers decay log linearly in distance from the closest SEZ. We then use a non-parametric approach based on various distance bands and more comprehensive

[40] The result is similar when we split the sample into a pre- and post-2000 period.

[41] This is consistent, among others, with Rosenthal and Strange (2004). Geographic distance (or transportation costs) plays also a central role in the literature on trade and economic geography (Fujita *et al.* 1999). An alternative measure of distance is used by Bloom *et al.* (2013) who argue that cross-firm spillovers depend on the distance in technology and product markets. Neumark and Kolko (2010) also use the identification assumption that the effect of place-based policy on non-targeted areas differs in the distance to the treated areas.

[42] Jaffe *et al.* (1993) find that patent citations are highly spatially clustered, which implies that there is a distance decay in the knowledge diffusion. Keller (2002) finds that the benefit of technology spillover is halved with a distance of 1200 kilometers.

measures of exposure to other cities' SEZ. As provincial borders may act as barriers, we also compare our results when restricting to spillovers within provincial borders. It is important to note that all of these variables are time-varying because they depend on the introduction of SEZ in other cities. Thus, identification hinges on this time variation. Note also that we always include the cities' own policy indicator for SEZ in the regression. This allows us to test whether the own reform effect changes when we allow for spillovers from other cities.

6.1 Measures of Distance and Transportation Costs Between Cities

Our first and simplest measure of distance between cities is the geodesic kilometer distance between all the city centers in our sample. This measure does not take into account geographical barriers between cities or transportation infrastructure. The second measure of distance is the driving time between cities derived from Google Maps.[43] The advantage of this measure is that it captures how well cities are connected through road infrastructure, which is likely to be an important determinant of the interactions between them. The drawback is that it focuses on road transportation and that it is based on the current transportation network, which is potentially endogenous to the zone locations.

The third set of distance measures is based on the topography of the Chinese terrain. This has the important advantage of being entirely based on exogenous factors. We have detailed information on slope and land cover that allows us to construct a local measure of transportation costs on 10×10 kilometer cells throughout China. We then use a shortest-path algorithm in ArcGIS to find the shortest route between cities through this cost surface and we measure the total cost along this route.[44] Since we must make a number of assumptions for how to map slope into transportation costs, we investigate the robustness of the result to alternative ways to compute this measure. The first mapping of terrain slope to driving speeds is based on a scale that relates slope to driving speed in the US and has 10 different levels (AASHTO 2001). The second mapping is based on a similar scale for China and has 7 different levels. In a further variation of this approach, we use a higher resolution for the transport cost cells (3 km instead of 10 km), and we exclude larger water bodies. All measures based on topography are normalized so that they have the same median as the driving times according to the Google maps. This is to facilitate comparisons between the different specifications.[45]

6.2 Results on Spillover Effects Across Cities

We use three complementary empirical strategies.

Distance to closest SEZ Our first approach to estimate SEZ spillovers is based on the distance of each city from the closest city hosting a SEZ (excluding zones in the own city). This variable varies over time; the establishment of a new SEZ that is closer than the previous ones causes a reduction in this measure. Our regression equation is as follows:

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

[43] We use the tool *traveltime3* in Stata that accesses the Google maps. Since only a limited number of queries can be submitted and there are more than 75'000 routes, we measured the distance of each bilateral connection in only one direction and imposed symmetry.

[44] The tool in ArcGIS is *cost distance* and is an implementation of the Dijkstra algorithm. See for example Alder (2015) for a description of this method and the data.

[45] We assume that all distance measures have a linear relationship with effective transport costs. While this is only an approximation, it facilitates the comparison across the various distance measures.

where $DistClosestZone_{it}$ is the distance to the closest city that has a state-level SEZ in year t. The distance to the next SEZ is an *inverse* measure of the spillover intensity. Therefore, if spillovers were negative, we would expect $\lambda > 0$. On the contrary, the results in Panel A of Table 8 suggest that there are positive spillovers, since a longer distance to the next SEZ is associated with a lower GDP, controlling for land area and the usual fixed effects. The spillovers are especially large in the inland sample. Remarkably, the reform effect of the own SEZ, α, remains large and significant.

Indicator for SEZ within radius Our second approach is to include a binary indicator for having a zone within a given radius. We report the results for a 150 kilometer radius (or for the equivalent in driving time).[46] The regression equation is

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

where $I\_ReformRadius_{it}$ is the indicator for having another city with a SEZ within the specific radius. If spillovers were negative, we would expect $\mu < 0$. Instead, we typically find positive estimates of μ. Panel B of Table 8 reports the results for a radius in kilometers and minutes in columns (1) and (2), respectively. The remaining columns use the distance measures based on topography, which are normalized such that their median is equal to the median travel time in minutes. All estimates are positive, although the estimated coefficients are sometimes insignificant.

We also perform a similar analysis where, instead of one indicator for 150 kilometers, we simultaneously include multiple indicators for various rings (excluding the own zone): 0-50, 50-100, 100-200, and 200-400. These indicators take on the value 1 if there is at least one zone within the corresponding ring. The omitted group consists of cities for which the closest SEZ is more than 400 kilometers (or the corresponding alternative distance measures based on driving time or topography) away. The results are shown in Appendix Table A5. Most of the indicators have a positive coefficient, and in some cases they are statistically significant. We only observe negative coefficients for the geodesic distance beyond 50 kilometers, but the estimates are relatively small and insignificant. For all other distance measures, we find positive effects that tend to be larger and more significant for zones that are closer. This analysis suggests that the positive spillover effects of the zones on cities within a radius of up to 100 kilometers is not driven by reallocation from areas between 100 and 400 kilometers.

Exposure measure Our final spillover measure is inspired by Briant *et al.* (2015) and mimics the idea of a market access measure such as[47]

$$MA_{it} = \sum_{j \neq i} \frac{GDP_{j,t}}{dist(i,j)}. \tag{6}$$

We adjust this measure by summing only over cities that have a SEZ in that year.[48] Our measure of exposure to other cities with SEZ is therefore given by

$$B_{it} = \sum_{j \neq i} \frac{GDP_{j,t}}{dist(i,j)} \mathbb{I}\{I\_Reform_{j,t} = 1\},$$

[46] This is approximately the median distance to the next SEZ. The results are similar for a radius of 100. The coefficients vary more when we use a variety of different radii between 20 and 900 km, but we never find significant negative spillover effects. In robustness checks, we also computed the distance to the closest zone in the same province, and the results are qualitatively similar.

[47] Such measures of market access or market potential appear in models of trade and economic geography, see for example Fujita *et al.* (1999).

[48] Briant *et al.* (2015) weigh by population instead of GDP. The results are robust to using population.

where $\mathbb{I}\{I\_Reform_{j,t} = 1\}$ is an indicator function for cities that have a SEZ (are reformed) at time t. This measure allows us to capture the exposure by taking into account both the economic size of other cities with SEZ and the distance from them.

This exposure measure varies over time because of the introduction of SEZ in other cities, but also because of GDP growth in these cities. The latter channel implies the risk that this measure may confound the effect of other zones with growth in market access.[49] In order to control for growth in neighboring cities in general, we therefore also control for the logarithm of market access, which is measured across all cities in our sample as shown in Equation (6). The regression equation then becomes

$$y_{ipt} = \phi_i + \gamma_{pt} + \alpha I\_Reform_{it} + \xi \ln(B_{it}) + \eta \ln(MA_{it}) + X_{it}\beta + \varepsilon_{it}.$$

We would typically expect a positive coefficient on market access and, in the presence of negative spillover effects, a negative coefficient on the exposure to other SEZ, hence $\xi < 0$ and $\eta > 0$. The results are shown in Appendix Table A6. The coefficients on exposure are always positive but not significant. The measure of market access shows a negative estimate in the full sample and a positive estimate in the inland sample. However, it is generally insignificant except for column (1). The result is broadly consistent with the one from the two previous specifications, and it indicates that there is no evidence of negative spillovers. The comparison to the market access measure in fact suggests that proximity to a reformed city is more beneficial than higher market access in general. Interestingly, the effect of the own zone remains large and significant in all specifications.

The analysis of potential spillover effects based on various distance measures and identification strategies suggests the existence of positive spillovers across cities. Although these effects are not always significant, the fact that we never find significant negative effects provides strong evidence against the presence of negative spillovers.

6.3 Spillovers Over Time

In Section 4.5, we observed that the effect of SEZ on the own city tends to flatten out over time (see Figure 3). One possible explanation for this pattern could be that the effect of the SEZ spills over to other cities as time goes by. In this section we investigate how the spillover effects evolve over time. The two upper graphs in Figure 5 show the estimates of a regression where the spans of the own SEZ are included in a regression together with the spans of the first zone that is established within 150 minutes driving time. The two lower graphs show the results from an analogous specification with a 150 kilometer radius. The point estimates on the spans for the neighboring zone generally become significant at the 5% level when a 150 minutes driving time radius is used, but not (or only marginally so) when a 150 kilometer radius is used. In both cases the patterns suggest that the spillover effects become stronger during the first ten years. The diffusion of positive spillovers could reduce the difference between treated and neighboring cities, which can potentially explain why the effect of the own zone flattens out over the years.

[49] For example, if several cities in the close neighborhood experience GDP growth but only one of them has a SEZ, then this measure of exposure may partly capture the general increase in market access. Although we control for province-time interactions in all of our regressions and therefore absorb much of the regional growth trends, this measure gives higher weight to close neighbors and hence may capture spatial trends at the local level.

6.4 Decomposition of Spillover Effects

In this section, we decompose spillovers into investment, TFP, and population spillovers. Negative investment spillovers would indicate that the SEZ attract investments at the expense of neighboring cities. Positive investment spillovers would instead arise if firms choose to locate geographically close to their suppliers and customers. This would lead to higher investments in cities located near to growing SEZ. A similar argument applies to population and TFP. If innovative firms are attracted by the SEZ, this could yield a negative selection in nearby cities and lower TFP. Conversely, technological diffusion could induce positive spillovers. This could in turn trigger more investments in nearby cities.

Appendix Table A7 shows that the spillover effects on investment are insignificant in the full sample (columns (1)–(4)). The point estimates are small and – depending on the distance measure – either positive or negative. In the inland sample (columns (5)–(8)), the spillovers tend to be more significant and are always positive (since the effect of distance is negative). Appendix Table A8 shows that the spillovers in TFP are in all cases positive and mostly significant.[50] The coefficient on the indicator of a city's own SEZ remains stable. Appendix Table A9 shows that the effect of SEZ on population in nearby cities is positive, but usually not significant.[51]

One possible channel for productivity spillovers is foreign direct investment (see Gorodnichenko *et al.* 2014), an explicit target of SEZ. Appendix Table A10 shows that the onset of SEZ increases the FDI flows to the cities hosting SEZ, consistent with the results of Wang (2013). The spillover effect on neighboring cities is positive but insignificant. There is no evidence that SEZ have negative spillover effects on FDI in other cities.[52]

6.5 Spillover Effects in the Periphery of Cities

We have so far investigated cross-city spillover effects. Our data additionally allow us to explore the effect of SEZ on neighboring non-urban areas. Our baseline specification focuses on the entire area of cities, which include an urban core (where all state-level SEZ in our sample were established) and the periphery around the urban core. To investigate whether and how SEZ affect economic activity in the area surrounding the center, we re-run our baseline regressions of Section 4.2, using two distinct geographical definitions of GDP as the dependent variables. First, we use the logarithm of GDP of the urban core only as the dependent variable (see Appendix Table A11, Panel A).[53] Then, we use the logarithm of GDP of the periphery only, i.e. excluding the urban core (Appendix Table A11, Panel B). The effects for the urban core are comparable in magnitude to those obtained above for the combined area. Moreover, the results hold up when we consider only the periphery. In summary, there is no evidence that SEZ impoverished neighboring non-core city areas.[54]

[50] Here TFP is constructed using the full-sample unrestricted production function estimation. The other two measures of TFP give similar results.

[51] When we restrict the sample to the years when we have better population data from the census, then the signs of the coefficients vary and they are never significant.

[52] Different from us, Wang (2013) finds some evidence of negative FDI spillovers in neighboring cities. A potential explanation for the difference in the results is that she does not distinguish between state-level and province-level zones and only considers the spillover effect of FDI on neighboring cities.

[53] The strategy of estimating the effects at different levels of aggregation in order to verify the presence of spillovers from the treated location to neighboring areas is also applied in Criscuolo *et al.* (2012) in their analysis of place-based policies in the UK.

[54] The positive effect may be due to firms active in SEZ setting up facilities in the periphery. To the extent to which firms do not benefit from special exemptions for the activities performed outside of the SEZ, we regard this as as a spillover. However, one might conjecture that firms located inside the SEZ can benefit from special treatment even if they

7 Robustness

In this section we perform a number of robustness exercises.

7.1 Satellite Light as an Alternative Measure of GDP

Chinese price data are generally regarded as problematic, especially at the local level. Our empirical methodology has the advantage of not relying on any price deflator. Differences in price levels are filtered out by city fixed effects, whereas province × year fixed effects filter out cross-province inflation differentials. Yet, one might worry that within each province cities might experience different inflation rates. In particular, our estimated treatment effect would be biased upwards if the establishment of a SEZ causes systematically higher inflation. The existing price data do not suggest any such pattern. However, one might also worry that the local authorities over-report the *nominal* GDP in cities hosting SEZ, in order to meet the expectation of the central government regarding their success.

To address this issue, in this section we use light intensity measured by weather satellites as a proxy for GDP. A number of recent papers have argued that nighttime light intensity measured by weather satellites is a good proxy for GDP.[55] Most economic activities such as production, transport, and consumption produce light as a by-product. Therefore, light intensity is positively correlated with the intensity of local economic activity. We calculate the average light intensity within the geographical boundaries of the urban cores and use this as a proxy for economic activity. The light data has the advantage that it can be measured within the same administrative boundaries over time. We can use digital maps from 2010 to calculate the light statistics for all years. The change in administrative borders – which are relatively frequent for urban cores – are therefore not a concern.[56] A drawback of the light data is that it is only available from 1992 on.

In column (1) of Table 9 we re-run our baseline regression with the logarithm of the average light intensity as the dependent variable. The estimate suggests that SEZ have a positive and significant effect on economic activity as measured by light intensity. However, the point estimate of about 5% is lower than what we observed in the baseline regressions using GDP as the dependent variable. The point estimate for the inland sample is similar in magnitude, albeit statistically insignificant. The lower point estimate could be due to the sample period starting in 1992, because only one-third of the (first) SEZ were established after that year. Moreover, even for later reformers we lose annual observations that would be useful for a precise estimation of the within-city effect of the establishment of a SEZ.[57]

We also check the robustness of our results by using electricity consumption as a proxy of economic activity (see, e.g., Rawski 2001). Data on electricity consumption by households and firms are reported in the same statistical yearbooks as GDP and are available only for the urban core. In column (3)

locate some facilities in neighboring areas. We could not find any precise information in this regard.

[55] Elvidge *et al.* (1997) are among the first to discuss the relationship between light and economic activity. See also Henderson *et al.* (2012) and Chen and Nordhaus (2011) and the literature cited there on the use of light to measure economic activity. Ma *et al.* (2012) and Hälg (2012) discuss the use of light data for Chinese cities. See also the Online Appendix for further details on the data source.

[56] When there are no data constraints due to border changes, then the urban core is a reasonable unit of analysis, since the SEZ in our sample were located in the urban cores. The analysis using light data exploits this advantage, but we have also done the analysis for the larger definition of a city that includes the periphery, which is the unit that the baseline GDP results are based on. The effects of SEZ at that level are smaller and insignificant. We have no explanation for the difference in the result between urban core and the area that includes the periphery. It appears to be specific to the light data, since such large differences were not observed for other data.

[57] This loss of precision is confirmed by the observation that if we run the baseline regression of Section 4.2 with GDP as the dependent variable for the post-1992 period we obtain a positive (0.043) but statistically insignificant point estimate.

of Table 9 we re-run our baseline regression using the logarithm of electricity consumption as the dependent variable. The result shows that the establishment of a SEZ is associated with a 15.7% increase in electricity consumption.[58]

7.2 Controlling for Local Government Spending and Road Infrastructure

One might conjecture that the establishment of a SEZ may be associated with additional transfers from the central or the provincial government. SEZ may also have triggered government investments in infrastructure. Although one might regard both transfers and investments in infrastructure as being part of the place-based policy, one may be interested in estimating the *net* effects after controlling for them.

While we have no information on transfers, we observe the area of finished roads in the urban cores in each year, which is an important component of infrastructure investments. Furthermore, we observe the overall *expenditures* of the local government for a subset of the years in our sample. Finally, we can also control for government *income* and hence the deficit of the local government. These measures can be used as a proxy for the contribution of public investments to GDP. The disadvantage of including these variables is twofold. First, we lose some observations. Second, causation could run in the opposite direction: government expenditure might have increased because the GDP expansion caused by the SEZ increased the tax revenue accruing to the local authorities. We have therefore also used one-year lags of government spending and income. The estimates reported below are for the contemporaneous years, but the results are relatively similar when using lags.

Table 10 shows that the reform effects are robust to the inclusion of controls for local road infrastructure (columns (1) and (4)) and government expenditure (columns (2) and (5)). The results are also robust to controlling for the log difference between government spending and government income – a proxy for the deficits of local governments (columns (3) and (6)). The effect of the reform remains positive and highly significant in both samples.

7.3 Earlier GDP Data

Our main analysis focuses on the period 1988-2010, for which the NBS provides a sample of cities that allows us to also track border changes over the years.[59] This approach entails the cost of losing variation in the reform variable, since some SEZ were established before 1988. We re-estimate our baseline specification for a subset of cities for which GDP is also available for earlier years.[60] In this case, we cannot control for changes in land area, government spending, and population as these data are missing for the earlier years. The reform effect estimated with this subsample is a 16.8% increase in the level of GDP in the full sample, and the estimated coefficient is highly significant. This estimate is

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

[59] It is important to note here that the city size could vary over time, and there were changes in the administrative system. The yearbooks allow us to match the city names over the years and control for these border changes by including land area as an explanatory variable.

[60] Please see the Online Appendix B for more detailed descriptions of the data source.

similar to our baseline results reported in Table 3. In the inland sample, the estimate is 32.7%, which is higher than our baseline result.

7.4 Population from Census

In our analysis so far, we have combined population data from the census and from the City Statistical Yearbooks. Using the yearbook data allowed us to calculate the annual fluctuations for the years between the three censuses (1990, 2000, and 2010). To the best of our knowledge, the yearbook data cover only the registered population in the city, that is, people with "*hukou.*" The existence of a large number of non-resident immigrant workers in the cities could potentially bias our estimation. To address this issue, we first check the City Statistical Yearbook data against the population census that in principle should record the entire resident population at the city level. We find that there is a gap between the two data sources. In particular, if the census is correct, then the population growth rate is overestimated by an annual 24 basis points in non-reforming cities, and underestimated by 35 basis points in reforming cities in the city statistics. The observation that the population is underestimated in the treatment group and overestimated in the control group is not surprising, as the treatment cities are likely to have attracted many *non-hukou* workers from the control group.

By relying on the census data in 1990, 2000, and 2010 and using the yearbook data only to infer the population growth rates for the years in between, we have already attempted to address this concern. To further test the robustness of our results, we repeat the baseline, regressions of Table 3 and restrict our sample to the three census years, using only population census data. Table A3 simply replicates the results in Table 3 for the restricted sample. The estimates are somewhat larger compared to our baseline and they remain significant. This is the case for all specifications and in both samples. It is important to note that by restricting the sample to only three years, we lose some time variation in the treatment effect. However, the baseline results do appear to be robust to using the resident population data from the census.[61]

7.5 Population and Population Density

Our results suggest that SEZ have a positive effect on both GDP and population, but the effect on GDP is larger than the effect on population. This is consistent with the increase in GDP per capita shown in columns (3) and (6) of Tables 3 and A3. These specifications for GDP per capita also control for population in order to account for agglomeration effects, but this raises the concern that population is endogenous. For instance, an increase in productivity and wages can induce immigration. The typical instruments proposed in the literature are time-invariant, and it is difficult to find time-varying instruments that fit in our difference-in-difference framework.[62] To mitigate the concern, we adopt two strategies. First, we show that the results are robust to a specification where we use the lagged

[61] The same holds true for the capital-labor ratio and for TFP (result not shown).

[62] The literature finds a relatively small endogeneity bias in the coefficient for population density. For example, Combes and Gobillon (2015) document that the endogeneity bias on the elasticity of density is between 10% and 20%, sometimes the bias is close to zero and even negative. Combes *et al.* (2010) provide a detailed comparison of different identification strategies. In particular, they note how difficult it is to find valid time-varying instruments (most attempts in the existing literature have resulted in weak instrumentation). An example for time-invariant instruments is given in Ciccone and Hall (1996), who study the effect of density by using historical population as an instrument. Combes *et al.* (2008), Duranton and Puga (2004), and Glaeser and Gottlieb (2009) provide a more general discussion of spatial concentration and productivity. An example of an analysis of agglomeration forces in China is Combes *et al.* (2013), who use Chinese household survey data.

values instead of current values of population (and its density). The results in the Appendix Table A12 show that the reform effects on GDP per capita and TFP are robust when we include population or population density together with land area with one period lag. The results are also robust to using the lagged population (and its density) as an instrument for current population (and its density).[63]

Second, we explore other specifications where we do not control for population (so, the results are gross of agglomeration effects). Column (2) in the baseline regression of Table 3 already shows that the results are robust to a specification that includes changes in city areas but not in population. However, one might worry that a specification where the effect of border changes is log linear in land area is overly restrictive. Changes in land areas reflect changes in borders, and the effects are likely to be heterogeneous across cities. To address this concern, we propose a specification that controls for border changes in a more flexible way by allowing each city's fixed effect to undergo a structural break whenever the land area of a city changes in our data - indicating a change in city borders. In other words, we replace city fixed effects with city-land area fixed effects.[64] For instance, if a border change brings a poorer periphery into the city, this effect is absorbed by the new, and more flexible fixed effects. The results are shown in Appendix Table A13. Columns (1) – (2) for GDP show that the point estimates are similar to Table 3 both in the full sample and the inland sample. Columns (3) – (4) show that the estimates are also positive for TFP. In the inland sample the estimate is also positive but lower than in column (4) of Table 7 (Panel C) and marginally insignificant.

7.6 Heterogeneity in the Treatment Effect

The literature on place-based policies suggests that the effectiveness of such policies may vary with location characteristics such as city size, density, or market access (see for example Briant *et al.* 2015 or Devereux *et al.* 2007). In this section, we include in our baseline specification interaction terms of our reform indicators with indicators for whether initial population, population density, GDP p.c., and market access were above the median value of reformers.[65] Since our sample is unbalanced, we take as the initial year for each city the year in which the corresponding variable is reported the first time in our sample. The results are shown in Appendix Table A14. In the full sample there is evidence for interaction effects with population and population density, but the main effect remains positive and significant. In the inland sample the interaction effects are stronger and the main effect is reduced when including the interaction with population and population density. Interestingly, the interaction effect with initial GDP per capita is negative, suggesting that SEZ in relatively less developed capital cities were particularly effective in inland provinces.

7.7 Placebo Analysis

Our estimation exploits both the time and spatial variation in the establishment of SEZ. Since the establishment of the SEZ is staggered, but clustered in a few years, there could be a concern about the extent to which the exact timing of the reform matters for the identification of the reform effect. Furthermore, we would like to rule out that our reform indicators pick up shocks unrelated to SEZ

[63] See for example Martin *et al.* (2011b) for a panel analysis where lagged variables are used as instruments.

[64] An average city then has roughly three different fixed effects over the years because of changes in the land area variable.

[65] We compare the characteristics to reformer cities because for some variables all reformers are above the median, such that the interaction effect would be collinear with the main effect.

that could be present also in other cities. In order to deal with these concerns, we run three placebo exercises based on the specification in column (3) of Table 3, but assign reform years randomly.

In a first exercise, we assign the actual number of new zone establishments in each year to a random selection of cities. The resulting placebo distribution is the same as the true distribution over time, but SEZ are assigned artificially to random cities. We repeat this exercise 1000 times. We find that in no case are the absolute t values and the R-squared of the placebo regressions larger than those of the true reform.[66] This suggests that the spatial distribution of SEZ indeed drives our result.

In a second more demanding placebo test, we assign the random reforms only to reformers, again holding the distribution of reforms across years constant. However, the timing of the treatment is scrambled across cities. This allows us to assess the extent to which the time dimension of the reform matters, because we are only randomizing the year of the reform but not the treated city. We find that the absolute t-values are higher when using the year of the true reform than in the placebo regressions in all but 5% of the cases.[67] This indicates that the actual year in which the SEZ were implemented is critical for our results, and supports our identification strategy based on within-city variation.

Finally, we use the random assignment of reforms from above and include the true reform year and the placebo reform year in the same regression.[68] While the estimate for the true reform is always significant at 5%, the placebo reforms are significant in only 33% of the cases. Overall, these placebo exercises strengthen our confidence in the empirical strategy used. Both the spatial and the temporal variation of the SEZ appear to be important for the results.

7.8 Effects by Year of Reform

In Section 4.5 we allowed the effect of SEZ to depend on the number of years since the reform. However, the reform effect may also depend on the year in which a city received the SEZ. Late SEZ could for instance imply a less intense treatment, since the Chinese economy was altogether more liberalized than in earlier periods. In this section we investigate whether there are significant differences between early and late SEZ. To this aim, we first construct separate policy indicators for early and late SEZ introductions. We use 1992 as the threshold year after which we label SEZ as "late" reforms. Note that 1992 is the median reform year in our sample. The policy indicators for early and late reforms are then used together in our baseline regression to replace the single indicator for reform. Table A15 columns (1) and (4) show the results with the two indicators. Early and late reformers both show positive point estimates, but the effect of the early reformers is larger and more precisely estimated. This is true for both the full sample and the inland sample, although the difference is smaller in the inland sample. This could suggest that earlier reforms had a larger impact, but it could also be driven by the fact that for earlier reformers the effect had more time to accumulate over the years since the reform. This seems particularly relevant in light of the patterns we observe for the flexible reform effects in Section 4.5, which suggest that the reform effect accumulates over about ten years. We then test for this pattern separately for early and late reformers in order to investigate how reform effects may differ between early and late reformers.

We parameterize the pattern we found in Section 4.5 by allowing a linear increase during ten years

[66] The mean estimate of the placebo reform is 0.0003, and is never significant and higher than the one of the true reform.

[67] The mean estimate of this placebo reform is 0.0911. In only 13% of the draws does the placebo specification yield significant coefficients that are higher than the actual coefficients.

[68] The assignment of random reform years among reformers implies that a placebo reform year is likely to coincide with the true reform year. This is the case in 36% of the draws.

and then a constant effect during all following years.[69] Furthermore, we impose that the linear trend after ten years is equal to the constant effect after ten years. More precisely, we impose the restriction that $\beta_{11-30} = 10 * \beta_{trend}$, where β_{11-30} is the effect after ten years and β_{trend} is the coefficient on the linear time trend. We then run this regression separately for early and late reformers. The results are shown in Table A15 columns (2)–(3) and columns (5)–(6) for the full sample and the inland sample, respectively. We see that the pattern in the full sample is relatively similar for early reformers (column (2)) and late reformers (column (3)), but the coefficients are less precisely estimated for the late reformers. In the inland sample (columns (5)–(6)) we observe that the coefficients on the trend are similar but again less precisely estimated for late reformers. We also note that in the inland sample we cannot identify the effect after ten years for the late reformers because there were no SEZ established in the inland sample within the period 1993–2010. The lower precision for late reformers is not surprising, since there are fewer observations to identify the effect until the end of the sample. However, these specifications suggest that the patterns and broad magnitudes are comparable for early and late reformers.

7.9 Alternative Clustering Strategies

In our main analysis we cluster standard errors at the city level, to allow for observations within a given city to be correlated as well as for heteroskedasticity. Our results are robust to alternative clustering strategies. First, we cluster the standard errors by province and year of reform (i.e., the first year in which a city hosts a SEZ). This strategy takes account of the fact that the introduction of SEZ is highly clustered in time. Many HIDZ were introduced in 1991–92, and many ETDZ were introduced in 2001–03, implying that different cities in these years cannot be treated as independent observations. The baseline results (not shown but available upon request) are essentially unchanged, and in some cases the statistical significance of the coefficients of interest is even strengthened.

We also run the regressions clustering standard errors at the province level (instead of province×year of first reform). This strategy is even more demanding, and runs into potential problems since we have only 28 provinces in the full sample and 18 provinces in the inland sample, and so the number of clusters is small. The results are robust to even this demanding approach. The coefficients of interest remain significant, although in the inland sample for GDP per capita only at 10%.

8 Conclusion

The place-based industrial policy is a building block of the development strategy pursued by the Chinese government. According to Naughton (2007): “Bold, fragmented, open to outside investment, but with a strong role for government: Special Economic Zones typify much of the Chinese transition process” (p. 410). This paper estimates the effect of SEZ on local economic performance. The results suggest that the establishment of SEZ has yielded large positive effects on GDP and GDP per capita for the cities in which these were located. Although our estimates are smaller than those found by the earlier literature based on cross-sectional growth regressions (typically on a smaller set of cities and years), the effects are sizeable and robust. We also find that the SEZ generated positive spillovers to neighboring areas.

What can we learn from the Chinese experience about the role of economic reform and industrial policy during the process of development? Existing theoretical and empirical work suggests that policies

[69] The fully flexible specification with separate indicators for each year is very demanding, and yields imprecise estimates.

and institutions should be appropriate to the stage of development, and particularly to the stage of the process of technological convergence (Acemoglu *et al.* 2006, Lorenz *et al.* 2016). The Chinese reform process was characterized by a mixture of elements of market liberalization and an active role of the government in promoting investment and technology adoption. Rodrik (2006) argues that the active role of the government was crucial for China's development because it supported a fast move towards more modern and productive sectors which have positive externalities on the whole economy. The results of our empirical analysis suggest that the industrial policy may have indeed been a catalyst of the development process. At the same time, the estimated effects are quantitatively not very large relative to the high growth rates experienced by China in this period.

Our analysis is subject to a number of limitations that we leave to future work to address. First, cities assigned to SEZ were not randomly allocated. To alleviate concerns for endogeneity, we focus on a subsample of inland cities where the allocation was driven by rigidly selected criteria, and we compare pre- vs. post-reform trends in treated cities. However, ideally one would like to have valid instruments for the spatial and time distribution of the policy intervention. This is very hard to find in the context of Chinese SEZ.

Second, we did not attempt a proper assessment of the welfare effect of SEZ. On the one hand, this would require a quantification of the budgetary costs of the policy. On the other hand, the local gains from spillovers through the agglomeration of labor may be partially offset by losses in other locations that experience an outflow of firms. As pointed out in Greenstone *et al.* (2010) and Kline and Moretti (2014a), whether the gains offset the losses depends on the shape of the agglomeration force.

Third, it would be interesting to disentangle which of the different components of the policy package had the largest effects. While the reduction in tax wedges must have been important, there are other channels through which SEZ may affect local and regional outcomes. As discussed in Kline and Moretti (2014a), place-based policies may also be used to reduce frictions such as excessive regulation. There may be political constraints that prevent the central government from implementing a reform nationally, such that a reduction within a subset of cities may be the best achievable alternative. In this case, firms would again relocate towards the SEZ and increase total factor productivity further through a larger labor pool. The reduction in frictions may also reduce prices, which could explain why prices are not increasing as much in our data as we may expect based on a simple spatial equilibrium framework. Another friction that can be relaxed by SEZ is the hukou system that restricts labor mobility, as SEZ may make it easier for workers to move there.

Finally, although the establishment of SEZ appears to have generated positive spillovers outside of the areas where they were introduced, we cannot rule out that the industrial policy drew resources away from locations that are remote and far away from the SEZ. In spite of these *caveats*, our results provide the basis for a realistic assessment of the effects of industrial policy in China, and some useful new evidence in the debate on place-based industrial policy in different countries.

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Table 1: Descriptive statistics

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Real GDP (mil)	20938.83	30120.88	210	451172.34	5289
Growth of real GDP (%)	13.02	17.71	-52.19	594.78	4736
Real GDP per capita	5228.68	5066.3	220.26	51513.13	5252
Growth of real GDP per capita (%)	10.8	12.12	-75.77	391.7	4969
Land area (sq km)	14059.24	17185.24	137	253356	5335
Growth of land area (%)	16.58	276.78	-59.63	9423.84	5055
Population (mil)	3.85	2.92	0.1	48.51	5306
Growth of population (%)	2.26	16.28	-77.31	347.56	5030
Electricity consumption (GWh)	3.08	4.72	0.01	56.3	5210
Growth of electricity consumption (%)	17.67	198.6	-98.97	13486.34	4914
Mean light intensity	11.74	10.31	0.1	56.04	4730
Growth of mean light intensity per satellite (%)	4.38	14.48	-45.64	117.23	4274

The table shows the descriptive statistics of our main variables in our sample of 276 cities in 25 provinces from 1988 to 2010. Real GDP is derived from city-level nominal GDP and provincial deflators. Land area is the official size of the cities. Population includes registered residents only. Electricity consumption is by households and firms. Mean light intensity is the average brightness of pixels in the city.

Table 2: Descriptive statistics (at beginning of sample period) by region and reform year

Region:		Coast			Inland		
Reform year:	All	< 1988	1988-2010	Never	< 1988	1988-2010	Never
Population (1,000 people)	3113.0	5282.5	3913.0	2968.8	-	3381.2	2319.7
Land area (km2)	10266.6	9811.4	8059.6	10247.1	-	10414.4	11384.2
Population density (people/km2)	402.3	561.1	553.9	353.9	-	375.3	344.9
Real GDP (millions)	4870.1	11363.7	7936.9	4215.4	-	5290.5	2515.9
Real GDP p.c.	1777.7	2072.9	2290.0	1722.7	-	1867.1	1441.0
Nominal GDP p.c.	1862.0	2180.9	2408.7	1816.4	-	1945.3	1502.9
Government spending/GDP (%)	8.7	7.5	7.0	8.4	-	9.3	9.6
# of universities (per mil. people)	2.0	1.6	1.8	1.4	-	3.3	1.4

The table shows the mean values of selected city characteristics at the beginning of our sample (averaged over 1988 and 1989), separately for reformers before 1988, reformers between 1988 and 2010, and cities that never had a reform. The table also distinguishes inland and coastal cities. Note that no inland city was reformed before 1988. We restrict the sample to a balanced panel.

Table 3: Baseline regression

	(1)	(2)	(3)	(4)	(5)	(6)
State-level SEZ	0.156***	0.116***	0.0927***	0.213***	0.175***	0.130**
	(0.0330)	(0.0292)	(0.0283)	(0.0693)	(0.0560)	(0.0532)
Province-level SEZ	0.0217	-0.00166	-0.0113	0.0209	-0.0106	-0.00580
	(0.0226)	(0.0182)	(0.0165)	(0.0310)	(0.0252)	(0.0232)
Dependent variable (log)	GDP	GDP	GDPpc	GDP	GDP	GDPpc
Controlling for log land area	No	Yes	Yes	No	Yes	Yes
Controlling for log population	No	No	Yes	No	No	Yes
Sample	Full	Full	Full	Inland	Inland	Inland
N	5392	5321	5269	2864	2798	2768
Adj. Rsq.	0.960	0.975	0.974	0.949	0.972	0.971

The dependent variable is the logarithm of annual GDP or GDP per capita. State-level (respectively province-level) SEZ is a dummy switching on in the year after the introduction of any SEZ at that level. All specifications include city fixed effects and the interaction of province-year dummies. Standard errors are clustered at the city level.

Table 4: Pre-reform trends

	(1)	(2)	(3)	(4)	(5)	(6)
Indicator for 3 years before state-level SEZ	-0.00836	-0.00322	-0.0129	0.128	-0.104	-0.0109
	(0.0327)	(0.0284)	(0.0299)	(0.0972)	(0.0932)	(0.0801)
Indicator for 2 years before state-level SEZ	-0.0286	-0.0190	-0.0278	0.0765	-0.135	-0.0479
	(0.0334)	(0.0285)	(0.0315)	(0.0954)	(0.0944)	(0.0787)
Indicator for 1 year before state-level SEZ	-0.0221	-0.0164	-0.0291	0.0508	-0.165*	-0.0836
	(0.0342)	(0.0287)	(0.0322)	(0.0984)	(0.0968)	(0.0821)
Indicator for year of state-level SEZ	-0.00807	-0.00737	-0.0154	0.0689	-0.167*	-0.0787
	(0.0346)	(0.0290)	(0.0342)	(0.0950)	(0.0954)	(0.0830)
Indicator for years after state-level SEZ	0.143***	0.107***	0.0758*	0.280***	0.0551	0.0834
	(0.0445)	(0.0377)	(0.0417)	(0.0985)	(0.0989)	(0.0920)
Indicator for years after province-level SEZ	0.0212	-0.00198	-0.0119	0.0227	-0.0134	-0.00684
	(0.0228)	(0.0183)	(0.0167)	(0.0314)	(0.0256)	(0.0234)
Dependent variable (log)	GDP	GDP	GDPpc	GDP	GDP	GDPpc
Controlling for log land area	No	Yes	Yes	No	Yes	Yes
Controlling for log population	No	No	Yes	No	No	Yes
Sample	Full	Full	Full	Inland	Inland	Inland
N	5392	5321	5269	2864	2798	2768
Adj. Rsq.	0.960	0.975	0.974	0.949	0.972	0.971

The dependent variable is the logarithm of annual GDP or GDP per capita. State-level (respectively province-level) SEZ is a dummy switching on in the year after the introduction of any SEZ at that level. Lags are defined as described in the table. All specifications include controls for land area, city fixed effects, and the interaction of province-year dummies. Standard errors are clustered at the city level.

Table 5: Differential trends for reformers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State-level SEZ	0.116***	0.0588**	0.0511*		0.175***	0.0259	0.0738	
	(0.0292)	(0.0278)	(0.0286)		(0.0560)	(0.0627)	(0.0607)	
Province-level SEZ	-0.00166	-0.00112	-0.00200	-0.000637	-0.0106	-0.00913	-0.0135	-0.0143
	(0.0182)	(0.0182)	(0.0182)	(0.0184)	(0.0252)	(0.0254)	(0.0256)	(0.0257)
Time trend of reformers (state-level)		0.00711***	0.00410	0.00433		0.0130**	-0.0122	-0.0116
		(0.00260)	(0.00330)	(0.00328)		(0.00590)	(0.0131)	(0.0122)
Post-reform trend (state-level)			0.00470	0.0168**			0.0264*	0.0455**
			(0.00403)	(0.00658)			(0.0145)	(0.0202)
Sq. post-reform trend (state-level)				-0.000558*				-0.000986
				(0.000289)				(0.000813)
Sample	Full	Full	Full	Full	Inland	Inland	Inland	Inland
N	5321	5321	5321	5321	2798	2798	2798	2798
Adj. Rsq.	0.975	0.976	0.976	0.976	0.972	0.972	0.972	0.972

The dependent variable is the logarithm of annual GDP. State-level (respectively province-level) SEZ is a dummy switching on in the year after the introduction of any SEZ at that level. The trend variables are described in Equation (2). All specifications include controls for land area, city fixed effects, and the interaction of province-year dummies. Standard errors are clustered at the city level.

Table 6: Controlling for differential trends

Panel A : Year fixed effects interacted with initial characteristics (relative to median)

	(1)	(2)	(3)	(4)	(5)	(6)
State-level SEZ	0.119***	0.111***	0.0936***	0.257***	0.238***	0.257***
	(0.0389)	(0.0370)	(0.0341)	(0.0685)	(0.0908)	(0.0680)
Province-level SEZ	0.0273	0.0116	-0.0154	0.0148	0.00248	-0.0180
	(0.0215)	(0.0181)	(0.0162)	(0.0345)	(0.0281)	(0.0269)
Dependent variable (log)	GDP	GDP	GDPpc	GDP	GDP	GDPpc
Controlling for log land area	No	Yes	Yes	No	Yes	Yes
Controlling for log population	No	No	Yes	No	No	Yes
Sample	Full	Full	Full	Inland	Inland	Inland
N	4727	4663	4618	2375	2315	2292
Adj. Rsq.	0.968	0.977	0.978	0.959	0.975	0.974

Panel B: Year fixed effects interacted with initial characteristics (dummies for above median)

	(1)	(2)	(3)	(4)	(5)	(6)
State-level SEZ	0.140***	0.102***	0.0772**	0.241***	0.182**	0.187***
	(0.0363)	(0.0333)	(0.0314)	(0.0886)	(0.0737)	(0.0667)
Province-level SEZ	0.0246	-0.00204	-0.0148	0.0197	-0.00532	-0.00683
	(0.0217)	(0.0180)	(0.0164)	(0.0321)	(0.0270)	(0.0237)
Dependent variable (log)	GDP	GDP	GDPpc	GDP	GDP	GDPpc
Controlling for log land area	No	Yes	Yes	No	Yes	Yes
Controlling for log population	No	No	Yes	No	No	Yes
Sample	Full	Full	Full	Inland	Inland	Inland
N	5392	5321	5269	2864	2798	2768
Adj. Rsq.	0.963	0.976	0.976	0.953	0.973	0.971

The dependent variable is the logarithm of annual GDP or GDP per capita. State-level (respectively province-level) SEZ is a dummy switching on in the year after the introduction of any SEZ at that level. The specifications in Panel A control for year dummies interacted with the logarithm of population, GDP per capita, population density, and number of universities relative to the median in the year in which a city enters the sample. The specifications in Panel B control for year dummies interacted with indicators for population, GDP per capita, population density, and number of universities being above the median in the year in which a city enters the sample. The specifications also include city fixed effects and the interaction of province-year dummies. Standard errors are clustered at the city level.

Table 7: Decomposition of the reform effect

Panel A : GDP per capita, capital-labor ratio, population, and human capital

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State-level SEZ	0.0927***	0.131***	0.0453	0.00295	0.130**	0.339***	0.0599	0.00278
	(0.0283)	(0.0404)	(0.0274)	(0.00367)	(0.0532)	(0.0614)	(0.0374)	(0.00816)
Province-level SEZ	-0.0113	-0.00402	0.0187	0.00214	-0.00580	-0.0385	-0.00177	0.00390
	(0.0165)	(0.0297)	(0.0130)	(0.00159)	(0.0232)	(0.0525)	(0.0123)	(0.00271)
Dependent Variable (log)	Y/L	K/L	L	h	Y/L	K/L	L	h
Sample	Full	Full	Full	Full	Inland	Inland	Inland	Inland
N	5269	4495	5275	4561	2768	2219	2769	2261
Adj. Rsq.	0.974	0.965	0.822	0.961	0.971	0.960	0.883	0.950

Panel B: GDP per capita, population and human capital (census years only)

	(1)	(2)	(3)	(4)	(5)	(6)
State-level SEZ	0.141***	0.0965***	0.182***	0.175**	0.111***	0.360***
	(0.0374)	(0.0359)	(0.0485)	(0.0720)	(0.0368)	(0.0807)
Province-level SEZ	-0.0259	0.0527**	0.0747**	-0.0238	-0.00423	0.102**
	(0.0308)	(0.0246)	(0.0318)	(0.0408)	(0.0193)	(0.0476)
Dependent Variable	log(Y/L)	log(L)	avg. sch.	log(Y/L)	log(L)	avg. sch.
Sample	Full	Full	Full	Inland	Inland	Inland
N	694	695	582	366	366	303
Adj. Rsq.	0.984	0.784	0.979	0.984	0.892	0.979

Panel C: Total factor productivity

	(1)	(2)	(3)	(4)	(5)	(6)
State-level SEZ	0.0617**	0.0553**	0.137***	0.0631	0.0471	0.189***
	(0.0274)	(0.0274)	(0.0346)	(0.0455)	(0.0446)	(0.0527)
Province-level SEZ	-0.00370	-0.00395	-0.0144	0.0203	0.0210	0.0336
	(0.0185)	(0.0187)	(0.0212)	(0.0259)	(0.0268)	(0.0291)
Prod. func. est.	No restriction	CRS	Pre/Post	No restriction	CRS	Pre/Post
Sample	Full	Full	Full	Inland	Inland	Inland
N	4019	4019	4019	1895	1895	1895
Adj. Rsq.	0.958	0.949	0.992	0.952	0.942	0.992

State-level (respectively province-level) SEZ is a dummy switching on in the year after the introduction of any SEZ at that level. In Panel A, the dependent variable is the logarithm of GDP per capita, capital labor ratio, population, or human capital based on the yearbook data. In Panel B, the dependent variable is the logarithm of GDP per capita, population, and average schooling years based on the census data. In Panel C, the dependent variable is the logarithm of TFP. In columns (1) and (2), TFP is estimated using the whole sample without restrictions on the return to scale. In columns (3) and (4), they are estimated in the whole sample while imposing the constant return to scale restriction. In columns (5) and (6), they are estimated in the pre- and post-1995 sample separately, without imposing the restriction on the return to scale. All specifications include city fixed effects, the interaction of province-year dummies, and controls for land area. All regressions except columns (3) and (7) in Panel A and columns (2) and (5) in Panel B also include controls for population. Standard errors are clustered at the city level.

Table 8: Spillovers across cities

Panel A : Distance to closest SEZ

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State-level SEZ	0.124***	0.123***	0.123***	0.122***	0.213***	0.200***	0.197***	0.193***
	(0.0305)	(0.0299)	(0.0297)	(0.0296)	(0.0493)	(0.0526)	(0.0501)	(0.0507)
Province-level SEZ	-0.00390	-0.00217	-0.00174	-0.00151	-0.0120	-0.0152	-0.0145	-0.0134
	(0.0181)	(0.0182)	(0.0182)	(0.0182)	(0.0251)	(0.0249)	(0.0250)	(0.0250)
Log km distance next zone	-0.0528**				-0.111***			
	(0.0249)				(0.0342)			
Log driving time next zone		-0.0816***				-0.124***		
		(0.0269)				(0.0390)		
Log transport costs next zone (10 cat)			-0.0694***				-0.110***	
			(0.0210)				(0.0307)	
Log transport costs next zone (7 cat)				-0.0655***				-0.105***
				(0.0210)				(0.0309)
Sample	Full	Full	Full	Full	Inland	Inland	Inland	Inland
N	5254	5321	5321	5321	2775	2798	2798	2798
Adj. Rsq.	0.977	0.976	0.976	0.976	0.972	0.972	0.972	0.972

Panel B: Effect of other SEZ within a 150 distance radius

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State-level SEZ	0.121***	0.116***	0.117***	0.117***	0.183***	0.173***	0.175***	0.177***
	(0.0295)	(0.0295)	(0.0297)	(0.0294)	(0.0538)	(0.0527)	(0.0538)	(0.0545)
Province-level SEZ	-0.000753	-0.00228	-0.00261	-0.00139	-0.00957	-0.0138	-0.0152	-0.0107
	(0.0183)	(0.0182)	(0.0181)	(0.0182)	(0.0253)	(0.0252)	(0.0246)	(0.0252)
Indicator for other zone in 150 radius (distance)	0.0514*				0.0688			
	(0.0306)				(0.0452)			
Indicator for other zone in 150 radius (driving time)		0.102***				0.102**		
		(0.0325)				(0.0438)		
Indicator for other zone in 150 radius (transport costs, 10 cat)			0.0685*				0.128***	
			(0.0354)				(0.0475)	
Indicator for other zone in 150 radius (transport costs, 7 cat)				0.0286				0.0560
				(0.0330)				(0.0414)
Sample	Full	Full	Full	Full	Inland	Inland	Inland	Inland
N	5321	5321	5321	5321	2798	2798	2798	2798
Adj. Rsq.	0.975	0.976	0.975	0.975	0.972	0.972	0.972	0.972

The dependent variable is the logarithm of annual GDP. State-level (respectively province-level) SEZ is a dummy switching on in the year after the introduction of any SEZ at that level. In Panel A, the additional independent variable is *distance to the next zone* and is an inverse measure of spillover intensity, and a negative coefficient therefore implies a positive spillover effect. In Panel B, the additional independent variable is *indicator for other zone in radius* and is expected to have a positive coefficient in the case of positive spillovers. All specifications include controls for land area, city fixed effects, and the interaction of province-year dummies. Standard errors are clustered at the city level.

Table 9: GDP proxied by light and electricity

	(1)	(2)	(3)	(4)
State-level SEZ	0.0501**	0.0460	0.157***	0.0389
	(0.0254)	(0.0537)	(0.0555)	(0.0722)
Province-level SEZ	-0.00754	-0.0322	0.0344	0.0339
	(0.0183)	(0.0281)	(0.0358)	(0.0425)
Dependent variable (log)	Light	Light	Electricity	Electricity
Controlling for log land area	No	No	Yes	Yes
Sample	Full	Inland	Full	Inland
N	4730	2570	5207	2718
Adj. Rsq.	0.836	0.817	0.792	0.755

The dependent variable is the logarithm of light intensity at the city level or the logarithm of electricity consumption in the urban core of the city. State-level (respectively province-level) SEZ is a dummy switching on in the year after the introduction of any SEZ at that level. All specifications include city fixed effects and the interaction of province-year dummies. Standard errors are clustered at the city level.

Table 10: Controlling for road infrastructure and government spending

	(1)	(2)	(3)	(4)	(5)	(6)
State-level SEZ	0.146***	0.111***	0.109***	0.203***	0.186***	0.159***
	(0.0299)	(0.0299)	(0.0284)	(0.0542)	(0.0533)	(0.0428)
Province-level SEZ	-0.00786	0.00163	-0.00130	-0.0138	0.00201	-0.0140
	(0.0177)	(0.0183)	(0.0178)	(0.0236)	(0.0261)	(0.0241)
Log road area	0.0761***	0.0433***	0.0329**	0.0628**	0.0270	0.0128
	(0.0215)	(0.0161)	(0.0150)	(0.0302)	(0.0220)	(0.0158)
Log government spending		0.480***	0.505***		0.432***	0.466***
		(0.0573)	(0.0602)		(0.0628)	(0.0483)
Log government spending/income			-0.281***			-0.319***
			(0.0458)			(0.0458)
Sample	Full	Full	Full	Inland	Inland	Inland
N	4423	2633	2632	2336	1427	1427
Adj. Rsq.	0.978	0.988	0.990	0.975	0.986	0.989

The dependent variable is the logarithm of annual GDP. State-level (respectively province-level) SEZ is a dummy switching on in the year after the introduction of any SEZ at that level. All specifications include city fixed effects, the interaction of province-year dummies, and controls for land area. Standard errors are clustered at the city level.

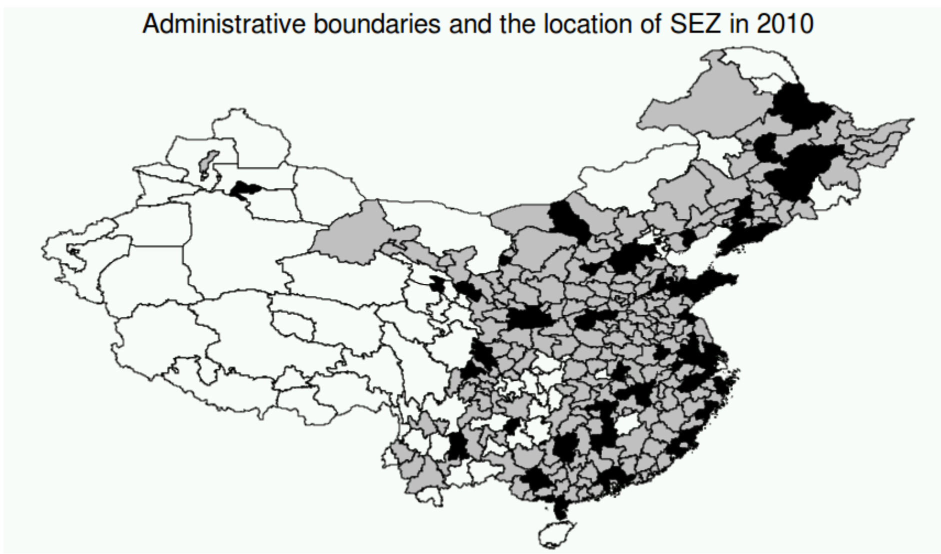

Figure 1: Location of treated cities in 2010: The cities in our sample with at least one state-level SEZ in 2010 are marked in black (a city may have more than one zone). The cities in our sample without a SEZ in 2010 are marked in grey.

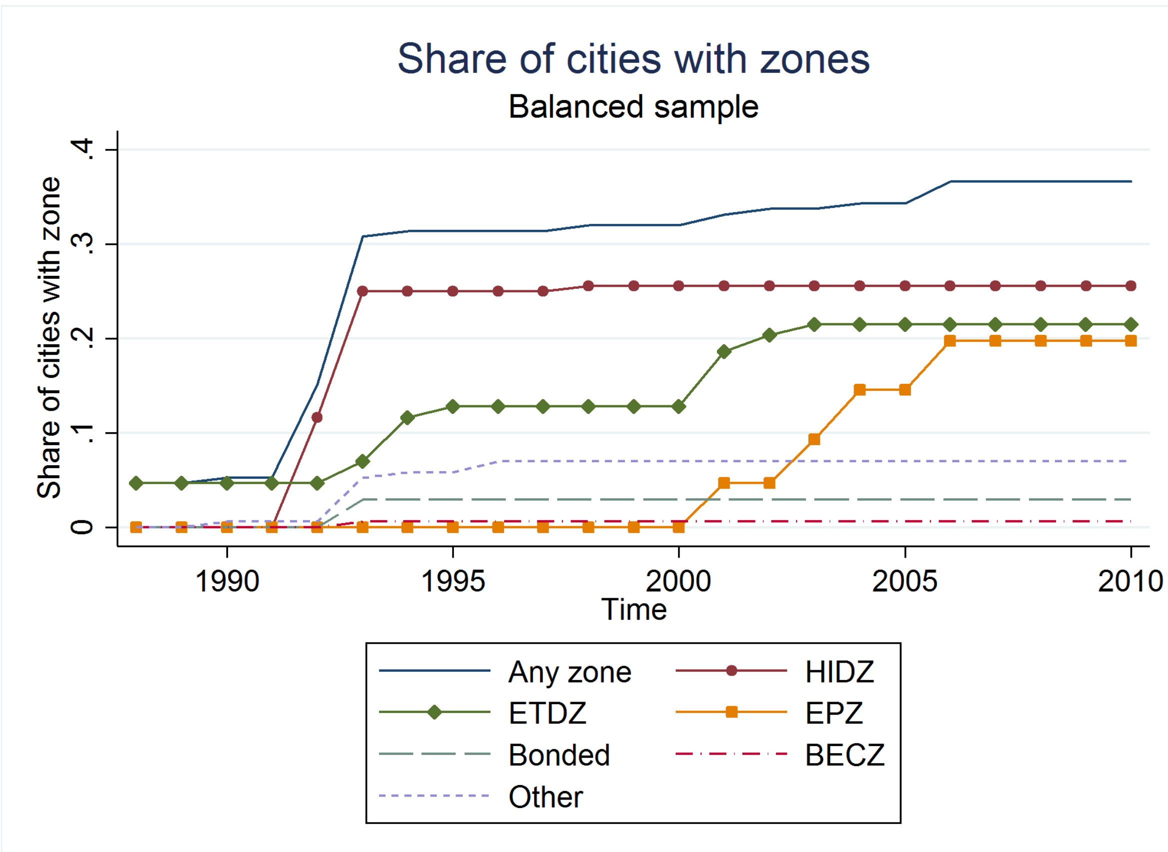

Figure 2: Share of cities with different types of zones: The figure shows the share of cities which have a state-level SEZ. The figure also shows the different types of SEZ: Hightech Industrial Development Zones, Economic and Technological Development Zones, Export Processing Zones, Bonded Zones, Border Economic Cooperation Zones, and other types. The sample is restricted to 172 cities that are observed in all years between 1988 and 2010.

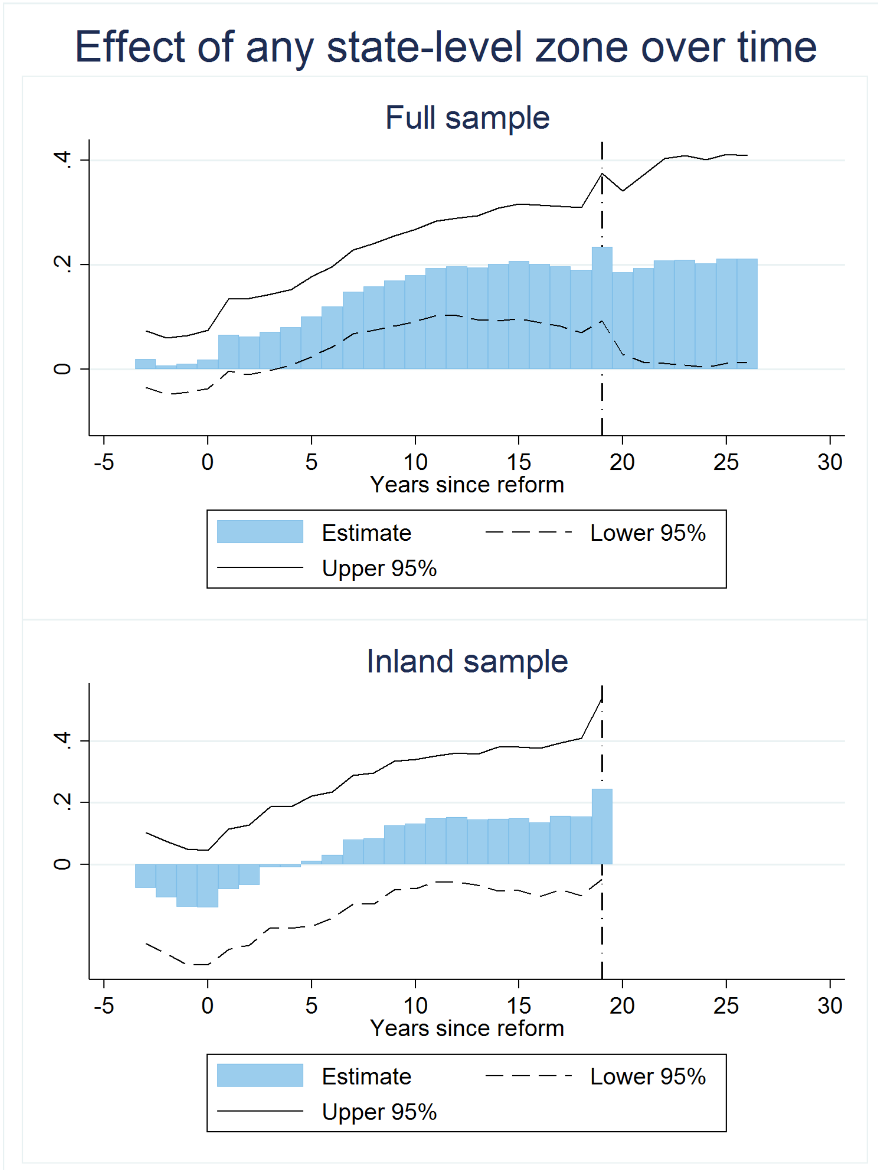

Figure 3: Reform effects over time in the full sample and inland sample: The bars show the coefficients of a regression of the logarithm of nominal GDP on indicators for years before and after the first zone. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows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

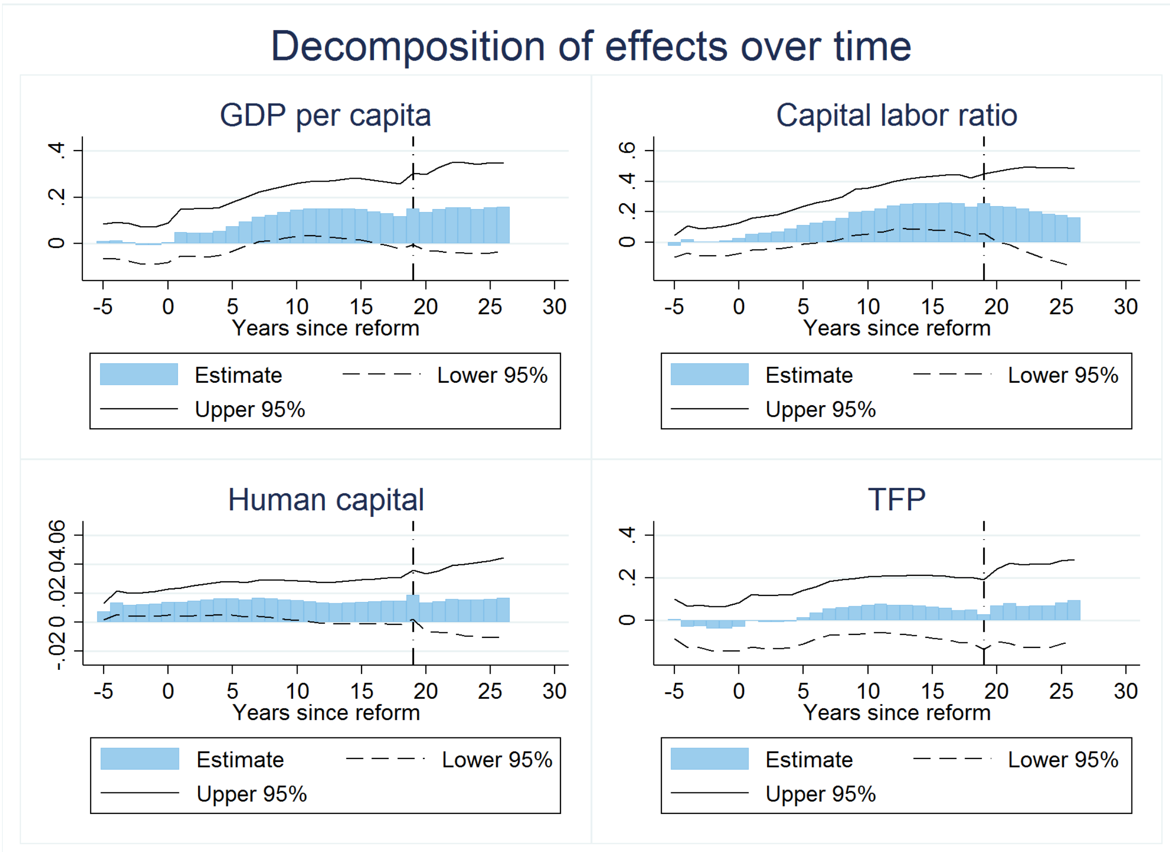

Figure 4: Reform effects on GDP per capita, physical capital per capita, human capital, and TFP over time: The bars show the coefficients of a regression of the four dependent variables on indicators for years before and after the first SEZ was established. TFP is computed using a full-sample unrestricted production function estimation. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 2010 and subsequently the number of observations to identify post-reform indicators drops to 9. The regressions control for an indicator for province-level zones, population, land area, city fixed effects, and province-time fixed effects. Standard errors are clustered by city.

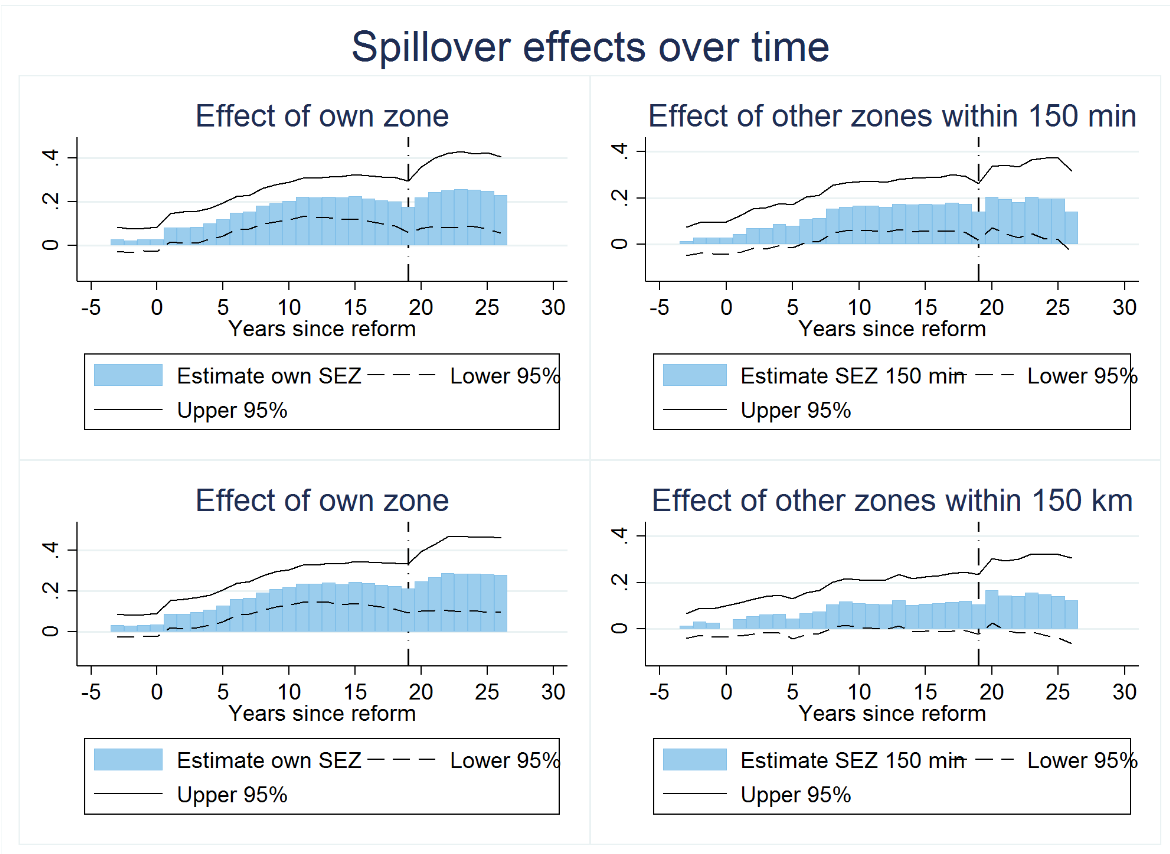

Figure 5: The upper two graphs show the estimates from a regression of the logarithm of nominal GDP on indicators for years before and after the first zone in the own city and zones within 150 minutes driving time. The effect of the time lag since the own reform year (left graph) is included together with the time lags from the first zone within 150 minutes driving time (right graph). The lower two graphs show the same when a radius of 150 kilometers is used. The solid and dashed lines show the confidence interval. The vertical dashed line at 19 shows when the reformers from 1991 reach 2010 and subsequently the number of observations to identify post-reform indicators drops to 9. The regressions also control for an indicator for province-level zones, land area, city fixed effects, and province-time fixed effects. Standard errors are clustered by city.