Geography versus Policy: Exploring How Location Matters in Placed-Based Policies Using a Natural Experiment in China

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Using the mass closure of development zones in 2004 as a natural experiment, we examine the causal effect of development zone closure on firm-level TFP in China, and its locational heterogeneity. The difference-in-difference estimator shows that on average, loss of protection by zones results in 6.5% loss of firms' TFP, while the effect varies with locations. Within 500 kilometers of the three major seaports in China, closure of zones reduced firm-level TFP by 9.62%, whereas closure of zone farther away did not show significant effects. We also found that market potential plays a role in explaining the locational heterogeneous effect.

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I. Introduction

Balancing regional development while simultaneously taking advantage of a market base is a problem faced by all countries with massive populations and vast

territories. However, a basic characteristic of the global economy is that economic activities always agglomerate in a few areas, no matter the distances between countries or within a specific country (World Bank, 2009). Faced with the disparities of regional development, many countries turn to place-based policies to promote the development of less developed regions. Place-based policies differ in many aspects, as they are generally launched in countries with differing institutional backgrounds. In China, the government launched similar place-based policies in different regions, which allows our study to focus on interregional differences that determine the effectiveness of place-based policies while excluding institutional differences in policies.

Place-based policies used in China include special economic zones (SEZs) and numerous kinds of industrial parks and high-tech parks designated as "development zones" (*kaifaqu*). As carriers of China's open-door experiment, SEZs were first implemented in a few coastal cities in early 1980s. Development zones are similar to SEZs in terms of preferential policies, but are much smaller in area size and account for only a small part of a city's jurisdiction. Thus, the effects of development zone policies can be separated from city-fixed effects in China. From the 1980s onward, more and more development zones with similar preferential policies were implemented all over China, and their role changed from a development and reform experiment to promoting regional economic growth (which is the typical objective of place-based polices). As Wang (2013) demonstrated, development zones exert positive effects on the development of their host cities, but these positive effects decline with the passage of time. In the future, targeted places of place-based policy may become substitutions of former targets (Busso et al., 2013), and this may help to explain Wang's (2013) findings on the fading effects of development zone policies.

In this study, we focus on the role of geography in making place-based policies effective. As argued by Glaeser and Gottleib (2008), the most effective method of

implementing placed-based policies is to encourage the flow of resources into places with high productivity and high elasticity of productivity with respect to agglomeration. However, in China, the location of development zones is highly influenced by the central government, which gives high priority to regional balance. Since 1990, more and more newly designated development zones have been located in inland China. The reason why the Chinese government was willing to build development zones in economically lagging central and western areas is straightforward: they wanted to promote the economic development of inland areas by duplicating the policies used in coastal areas, and thus create national balance in regional development. However, there are major concerns on whether governments are able to pick the best areas for place-based policies (Glaeser and Gottleib, 2008). Competition between local governments can improve the efficiency of place-based policies at the national level (Moretti, 2011), but such competition-based mechanisms may take a long time to produce positive results, especially in a country like China where serious market distortions exist.

In this study, we use the massive administrative closure of development zones between2004 and 2006 as a natural experiment to identify the causal effects of development zone policies on manufacturing firms' total factor productivities (TFP). Our empirical data shows that the average effect of development zone closures on treated firms' TFP is negative. Moreover, we found that geographic heterogeneity does exist: the magnitude of zone closures effects is smaller if the city is located farther away from three major seaports: Shanghai, Hong Kong, and Tianjin. Significantly, when the distance from these cities is large enough, the negative effects of development zone closures disappears. When analyzed further, these findings show that development zones can improve the TFP of firms inside these zones, but the positive effects fade as the distance from these major seaports increases. Our analysis shows that cities closer to the sea enjoy greater market

potential, which helps firms exposed to preferential policies improve TFP through a scale economy.

Our contributions in this study are two-fold. First, we use a natural experiment based on development zone closures in China to obtain a difference-in-difference (DD) estimation of the effects of development zones on firm-level TFP. There have been disputes about what possible benefits and distortions place-based policies may cause and our study attempts to resolve such disputes with empirical data. Furthermore, most existing empirical studies on place-based policies have not explored why different placed-based policies may have positive effects (Busso et al., 2013; Bernini and Pellegrini, 2011; Ham et al., 2011; Criscuolo et al., 2012; Freedman, 2013; Reynolds and Rohlin, 2014; Givord et al., 2013; Mayer et al., 2015) or insignificant effects (Crozet et al., 2004; Bronzini and de Balsio, 2006; Elvery, 2009; Neumark and Kolko, 2010; Hanson, 2009; Hanson and Rohlin, 2013). There are several exceptions. Besides Wang (2013) who found the sequential heterogeneities of zones' effect in China, Kolko and Neumark (2010) found that enterprise zones with different policies may exhibit different effects. However, as argued by Neumark and Simpson (2015), one of the most important questions is where place-based policies may work; however, existing literature lacks evidence on the specific conditions of successful place-based policies. Although Briant et al. (2015) found zones that are more isolated show less positive effects, they only focused on the role of the relative position of a specific zone in an urban area. In order to bridge this gap, our study explores how the efficiency-improving effects of development zones depend on their economic geography as in a whole country.

Concerning development zones in China, Wang (2013) found that the establishment of development zones had positive effects on city-level FDI and exports. Alder et al. (2013) found that the establishment of major development zones led to an increase in the GDP (around 12%), mainly as a result of physical

capital accumulation; however, they also found that the establishment of major development zones did not lead to a permanent increase in growth. The only firm-level study is conducted by Schminke and Van Biesebroeck (2013), who studied the effects of development zones on firms' exports; however, they only paid attention to two kinds of development zones, economic and technological development zones and science and technology industrial parks, both of which are national-level zones that offer the most preferential policies for firms. All of the above mentioned studies failed to examine the efficiency of development zone policies using firm-level data, and also failed to explore how the policies could be successful from a regional perspective. Moreover, the possible endogeneity of development zone policies is a significant challenge that previous studies ignored. The location of development zones and whether a firm is in a zone may be correlated to either regional or firm-level characteristics. In order to make a contribution to the literature on place-based policies, we provide a DD estimation of the effects of development zones and regional differences found between development zones.

Second, we add a regional perspective to the literature on the misallocation of funds in the Chinese economy. As Hsieh and Klenow (2009) documented, the Chinese economy has suffered significantly from misallocation of economic resources. Recent studies have attempted to explore the institutional reasons of misallocation. For instance, Brandt et al. (2013) found that ownership structure is an important factor because inefficient state-owned firms are favored in the financial market, whereas the more efficient private sector is discriminated against. Lu and Xiang (2016) observed that after 2003, inland-favoring policies and the deterioration of allocative efficiency occurred simultaneously. In this study, we provide a regional perspective on the misallocation of economic resources. Our empirical findings show that development zones are more efficient in coastal China, where the market potential is greater, and identical policies do

not work in inland regions. However, beginning in 2003, development zones were closed in eastern China (where the market potential is high), and the opening of new development zones became biased in favor of inland areas, which deteriorated the interregional allocation efficiency of economic resources. This explains why China's allocative efficiency has worsened since 2003 as it corresponds to when China started to use development zone policies to favor the industrial development of inland China (Lu and Xiang, 2016). We also contribute to the literature on place-based policies by studying whether place-based policies are successful because of the incentives they provide (see the review by Neumark and Simpson (2015)).

The remainder of this paper is arranged as follows: in part 2 we provide the historical background of development zone policies, paying special attention to the 2004-2006 closing of development zones in China; part 3 introduces our data and identification strategies; in part 4 we show the average effect of development zone closures on firms' TFP; part 5 exhibits the analysis on why the effectiveness of development zones differs across regions; and finally, we conclude the study by discussing the implications of place-based policies in part 6.

II. Historical Background

A. Introduction of Development Zones

China's development zones are successors of special economic zones that were first implemented in the 1980s as part of the economic reform and open-door strategy. In 1980, China opened Shenzhen, Zhuhai, Shantou, and Xiamen, and designated these four cities with the status of special economic zones. In 1984, China opened 14 other coastal port cities, which were then given the right to set up economic and technological development zones. In 1985, China opened the Yangtze River Delta, the Min-Zhang-Quan Delta, and Zhuhai to development. In

1988, the entire Hainan province became a special economic zone. In 1990, the State Council approved the opening of Shanghai and started the development of the Pudong New District. Prior to 1990, national-level development zones were only set up in coastal areas. Since 1991, however, the establishment of national-level zones has gradually shifted to the central and western regions (Wang, 2013), which coincides with development zone policies assuming the task of balancing regional economic development.

It is worth noting that, in addition to national-level development zones, there are many provincial-level development zones. Indeed, before 2003 development zones were approved by lower-level governments. The development zones approved by provincial and below-level governments were often created to boost the local economy, but, by law, such development zones were supposed to conform to the land use planning proposed by the central government. In *China Development Zones Audit Announcement Directory* (2006 edition), national ministries admit the legitimacy of only three categories of provincial-level zones: provincial economic development zones; provincial high-tech industrial parks; and provincial special industrial parks.

Development zones attract firms through preferential policies, institutional autonomy, better infrastructures, and government services (Zeng, 2011). The most important preferential policies consist of three categories: tax concessions, cheap land, and banking convenience. Government services provided by zones include (among others): accounting services, legal services, business planning, marketing, import-export assistance, skills training, and management consulting (Zeng, 2011). Some of these conveniences enjoyed by former development zone firms would not be affected by the closure of zones. For example, cheap land and infrastructure, if they do not change their locations, would not be affected. However, other conveniences, such as tax concessions, banking convenience, and government services, would be reduced with the closure of zones.

B. "Zone Fever" and the Closure of Zones in 2004

Ever since the early stages of the open-door strategy, intense competition has existed between local governments in attracting FDI through the building of development zones, leading to so-called "zone fever." In order to attract more investments, local governments competed to construct development zones. This caused a rapid expansion of built-up areas throughout China, and caused all kinds of conflict in the process of land expropriation.

In July 2003, the Ministry of Land and Resources, along with several other relevant departments of the State Council, announced the clearing of development zones. On July 18, the General Office of the State Council released an emergency notice that suspended the approval of all types of development zones. At the end of December, the Development and Reform Commission, the Ministry of Land and Resources, the Ministry of Construction, and the Ministry of Commerce jointly issued a document detailing the rules of how to clear development zones approved by different level of government. According to this document, no matter by which level of government a development zone had been approved, it could be affected in this round of clearing. The document also stated that zones approved by the State Council would not be closed in this phase, but could be impaired in terms of area viability. The zones approved by provincial governments could be closed or reduced in area size. The most affected zones were those approved by branches of the State Council, branches of provincial governments, and branches of government lower than provincial-level governments. Most development zones were closed, while some were promoted to provincial-level zones after being merged with nearby zones. From 2003 to 2006, the number of zones over the entire country was reduced from 6,866 to 1,568, or by 77.2%. Planned areas of all zones were compressed from 38.6 thousand square kilometers to 9,949 square kilometers, or by 74.0%. The most significant number of closures occurred

between the end of 2003 and June 2004, a half year period during which the number of development zones was reduced by 4,813 and planned areas were compressed by 24.6 thousand square kilometers. This accounted for about 70% and 64.5%, respectively, of the total number and area of all development zones, and represented 90.8% and 85.9%, respectively, of the entire reduced number and area size of zones between 2003 and 2006.[1]

The mass closure of zones between 2004 and2006 provides us with an opportunity to identify the precise role of development zones concerning firms' performances. Whether a firm is able to enter a development zone is subject to several selective practices; however, the closure of a development zone is largely exogenous to the firms. Significantly, in the next section of this study we show that affected firms in closed zones and the remaining development zone firms do not differ significantly in terms of TFP. During the same period of time in which development zones began to be closed (2004-2006), development zone policies became biased toward inland areas in an effort to balance regional development.[2] Lu and Xiang (2016) documented that in regions more than 500 km away from major seaports, the share of development zone firms in the national total increased significantly in 2004. The land supply, controlled by the central government through the construction land quota system (under which a centrally-distributed land quota is required for converting agricultural land to non-agricultural uses) also became biased toward inland development (Lu and Xiang, 2016).

[1] Source: the web of the Ministry of National Land and Resources, http://www.mlr.gov.cn/xwdt/jrxw/200411/t20041130_622006.htm.

[2] Also see the official announcement of the central government: http://www.gov.cn/gzdt/2007-04/21/content_590648.htm.

III. Data and Identification

A. Data Sources and Construction of Key Variables

The main data sets used in this study were extracted from China's Annual Survey of industrial firms from 2000 to 2007. The database contains all state-owned and above-scale (sales more than 5 million Yuan) non-state-owned industrial firms. The firms in the data set account for about 90% of all industrial output. The information contained in the database includes basic information such as the firm code, the number of employees, ownership, location, and the main financial indicators included in the balance sheet of the firm. In this study, we attempted to identify how development zones affect firms' TFP. We focused on TFP because it is the key to sustainable growth and competitiveness in the market. Policy-makers' intentions are not only to increase regions' inputs, but to also increase overall efficiency. Therefore, the two basic tasks of our study were: (1) recognition of development zone firms and (2) estimating firm-level TFP.

Identifying Development Zone Firms—The development zones closed in 2004 (representing 70% of the total of all zones), cannot be traced to any official records or documentation. Fortunately, in our database, the firms' address information included keywords which allowed us to distinguish between different development zone firms. In the firm-level database, the detailed location information of a specific firm contains six variables: (1) town (*xiang, zhen*), (2) village or street and doorplate number (*cun, jie, menpaihao*), (3) sub-district office (*jiedaobanshichu*), (4) neighborhood committee (*juweihui*), (5) address (*dizhi*), and (6) street (*jiequ*). We identified whether a firm was located in a development zone by searching through the six variables mentioned above for 17 keywords that indicated the existence of any kind of development zone. Such terms included *kaifa, gaoxin, jingkai, jingji, yuanqu, baoshui, bianjing, kejiyuan,*

chuangyeyuan, huojuyuan, huojuqu, gongyeyuan, chanyeyuan, gongyequ, touziqu, gongyexiaoqu, and *chukoujiagong*.

To ensure that the measurement error was minimized, we compared our results with those from the officially declared change of the development zone policy. Using our definition of development zone firms, for those firms that existed in both 2003 and 2004, the number of development zone firms in 2003 was 16,633, with only 6,148 of those firms remaining in 2004. The other 63% of development zone firms changed to non-development zone firms. The percentage of the firms that lost out on the advantageous policy benefits was very close to the percentage of closed development zones during 2003 and 2004 (which is about 70% and 64.5%, respectively, in terms of total number and area of the development zones).

Next, we calculated the regional distribution of the development zone firms. As Figure 1 shows, the share of development zone firms in the eastern provinces fell sharply in 2004. We also calculated the share of development zone firms within 500 km of major seaports, and again saw a sharp decline in development zone firms in 2004. This finding is consistent with the officially declared policy that development zones be used as policies that favor inland provinces.

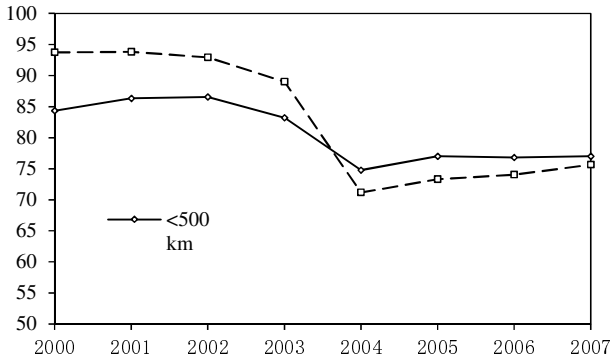

FIGURE 1: SHARE OF COASTAL CHINA IN DEVELOPMENT ZONE FIRMS IN THE ENTIRE COUNTRY.

Note: *<500 km* means the hall of the city a firm located is no more than 500 kilometers away from the nearest one of Shanghai, Tianjian and Hong Kong; *east* mean locations in *Beijing, Tianjin, Hebei, Liaoning, Shanghai, Jiangsu, Zhejiang, Fujian, Shandong, Guangdong* and *Hainan.*

Estimating Firm-Level TFP—Regarding firm productivity, a popular approach to its measurement is to use TFP, which is estimated by using the OP method (Olley and Parks, 1996). This method considers the influence of TFP on firm investment decisions, and the influence of firms' investment decisions and the TFP on their survival probability. Thus, this method resolves the two-way causality and sample selection problems that parametric and non-parametric methods are faced with. Relevant to our estimation of the TFP, two specific points need to be clarified.

First, the output we employed in the estimation of TFP is value-added and calculated by using the input-output method. Our estimation process improves the TFP estimation used by Brandt et al. (2012). For instance, we used officially-reported price deflators, while Brandt et al. (2012) constructed deflators by using the nominal and real output reported by the firms. For the price deflators of inputs, we used input-output tables from 1997, 2002, and 2007, while Brandt et al. (2012) only used a table representing one year, and thus ignored any changes that occurred over time. We also carefully constructed firm-level capital stock (but we choose not to report the lengthy procedure here in order to save space-- an appendix is available upon request).

Second, we estimated the output elasticity of capital, labor, and intermediate inputs for each 2-digit industry separately, thus allowing for variation of output elasticity of inputs among industries. Importantly, this method did not affect our empirical results because all of the regressions provided below control for industry-fixed effects.

B. Identification Strategies

Our strategy to identify the causal effects of development zones on firms' TFP was to use the mass closure of development zones during 2004-2006 as an exogenous shock to firms that had been in development zones at the close of 2003.By studying the mass closure of development zones in this time period, we identified the change in TFP when a firm's status changed from a development zone firm to a non-development-zone firm. We then compared the change in TFP and development zone status to that of development zone firms not affected by development zone closures. This provided a DD (difference in difference) estimation for the average treatment effect on treated firms (ATT) affected by development zone closures. Specifically, our regression model is:

(1) $y_{it} = \alpha + \beta treat_i * after2003 + \gamma X_{it} city_{it} + indus_{it} + T + a_i + prov_{it} * year_t + \varepsilon_{it}$

The subscripts *i* and *t* represent firms and years, respectively. In our main results, the dependent variable, y_{it}, refers to firms' TFP. $treat_i$ is a dummy variable indicating whether at the end of 2003 a firm was in a zone that was soon to be closed. $after2003$ is a time dummy variable that equals 1 when observations occurred between 2004 and 2007. X_{it} refers to a vector of firm-level and city-level control variables. We also included city, industry, year, and firm-fixed effects (denoted as $city_{it}$, $indus_{it}$, T, and a_i, respectively). We also included an interaction term of year and province-fixed effects to control for unobserved provincial specific trends. Essentially, we used a fixed effect model for regression in order to control for firm-level fixed effects.

The definition of treated firms can be divided into three categories. First, as mentioned above, we used key words to identify whether a firm was a

development zone firm in a specific year. Then we searched our sample for firms that existed in both 2003 and 2004. We defined a firm to be treated if it was a development zone firm in 2003 but not in 2004 (given that its location did not change between 2003 and 2004). To exclude the effect of entering and exiting zones for treated firms, we only chose observations that included successful development zone firms (up until 2003), and successful non-development zone firms after 2004. For example, if a treated firm entered a development zone in 2003 and then re-entered a development zone in 2005, then only 2003 and 2004 observations for this firm were included in the regressions.

Besides the DD specification, we controlled the variables that were likely to be correlated to both a development zone's chance to be closed and a firms' TFP. First, we controlled for a firms' age (*age*), which was obtained by using the firms' actual operation years divided by 100.

Second, we controlled for the ownership of firms by using a group of dummy variables (*SOE*, *HMT*, and *FDI*) that represent firms' largest shareholders (government, investors from Hong Kong, Macao, and Taiwan, and investors from foreign countries, respectively). The reference group is local non-SOEs.

An important issue when using a DD specification is the construction of control groups. We used firms that did not change their status before or after 2003 to construct control groups. Therefore, we had three alternative control groups. The first control group consisted of firms that were development zone firms in both 2003 and 2004; the second control group consisted of firms that were non-development-zone firms before and after 2003; the third control group is a combination of the above two groups. However, it is important to note that firms that never entered zones may be systematically different from those that entered development zones. Therefore, for common support consideration, the first control group (consisting of firms that were in development zones throughout 2003 and 2004) is the ideal control group. Another issue concerning common

support consideration is that in our sample, some cities do not have treated firms or control-group firms, meaning that there are no counterparts for comparison within the same city. These observations are excluded from our main empirical analyses.

Neumark and Simpson (2015) summarized the specific econometric challenges of reliably estimating the effects of place-based policies. The first challenge is measuring local areas where policies have been implemented and the subsequent economic outcomes of implementing those policies. The second challenge is the construction of control groups. The third challenge is identifying the effects of specific policies when areas are subject to multiple interventions simultaneously. The fourth challenge is accounting for displacement effects that occur within areas and from other, outside areas. Finally, the last challenge is studying the effects of discretionary policies targeting specific firms. Using the keywords-searching method mentioned above, we were able to overcome these measurement issues. Using the mass closure of development zones in 2004 as a natural experiment, we were also able to alleviate the concerns outlined in the second and third challenges. For the displacement concern, we find that in our sample the trend of TFP of control group does not change before and after 2004, which means that the control group firms are less likely to be affect by spillover effect from closure of zones. Moreover, as mentioned in section 2.1, the fact that former development zone firms will no longer enjoy tax concessions, banking conveniences, and government services, enabled us to separate the effects of those factors from conveniences that will not change after the close of zones (such as cheaper land and better infrastructures).

Commented [k21]: 本来要在这里加一句话说明控制组和处理组 TFP 在清理之前没有差异，但是回去看前面画的图发现并不是这样。只有 500 公里以内样本这句话才成立。后面在做异质性分析的时候画的两张 DID 的图中可以清楚地看到，所以就没有加额外的语句来说明这个问题。

IV. Treatment Effects of Development Zone Closures

A. Treatment Effects of Development Zone Closures on TFP

In Table 1 we report the regression results of the DD estimation. In column 1, we did not control for industry and city-fixed effects, or provincial time trends. All of these factors were controlled in the subsequent four columns of Table 1. In column 3, we controlled for the age of firms and a group of ownership type dummy variables. All three estimations show that the closure of development zones had a negative effect on firms' TFP.

In the course of this study, we became worried that the closure of development zones might be because of systematic differences that exist between the treatment and control groups. Therefore in column 4, we used a matching-DD model to check whether our results were reliable. The matching procedure consisted of two steps. First, we ran a probit model to predict the probability of each firm remaining in a development zone after 2004 (based on the firms' characteristics in 2003). Among these characteristics were: TFP, main sales revenue, profit, accumulated profit (beginning with the first year a firm became a development zone firm), employment, VAT payable, age, number of years in a development zone (until 2003), distance to the nearest major seaports (Shanghai, Hong Kong, and Tianjin), ownership type dummy variables, 2-digit industry dummy variables, and city dummy variables. Second, we did 1-1 matching using the nearest neighbor method in the treatment group for each of the control group firms (without replacement). Column 4 shows the treatment effect is slightly smaller using the 1-1 matched sample. In column 5, we estimated the model using the reduced sample that excludes observations in cities where no treated or no controlled firms were located. The results show that the estimated treatment effect of development zone closures is still significantly negative.

The coefficients of *treat×after2003* are negatively significant in all 5 columns with similar coefficients. That is to say, compared with firms that were not directly affected by the 2004-2006 development zone closures, treated firms suffered from less growth (or larger declines) of TFP. Conversely, the negative effects caused by zone closures means that firms in development zones had experienced positive effects.

In Table 1 and the remaining FE estimation results, the coefficients of the control variables are not informative because most of the control variables (except for the age of the firms) do not have large enough cross-time variations. For example, only a small fraction of firms changed their ownership types. Therefore, we will not report or discuss the coefficients of the control variables.

TABLE 1: TREATMENT EFFECTS OF DEVELOPMENT ZONE CLOSURES

	(1) full sample	(2) full sample	(3) full sample	(4) 1-1matched sample	(5) reduced sample
treat×after2003	-0.0943	-0.0632	-0.0623	-0.0511	-0.0654
	(0.0175)	(0.0198)	(0.0196)	(0.0213)	(0.0208)
age			0.119	0.548	0.204
			(0.120)	(0.264)	(0.174)
_cons	2.952	-201.3	-199.8	-204.3	-201.6
	(0.0240)	(8.769)	(9.037)	(10.09)	(10.91)
Year	Yes	Yes	Yes	Yes	Yes
Industry	No	Yes	Yes	Yes	Yes
Ownership	No	No	Yes	Yes	Yes
City	No	Yes	Yes	Yes	Yes
Prov. year trend	No	Yes	Yes	Yes	Yes
N	89448	89448	89446	39362	59483
R^2 *within*	0.0655	0.114	0.115	0.107	0.106

Notes: All observations are at the firm-year level. Standard errors are in parentheses. Models are estimated using FE. *Year, Industry, Ownership, City* fixed effect are controlled using dummy variables. Provincial year trend is controlled using provincial dummies multiplied with year value. *age* is measured in hundred years.

B. Policy Change and Firm Size Change Along with Development Zone Closures

Because TFP changes are the outcome of firms' input-output changes resulting from zone closures, we wanted to know whether the closure of zones really meant that there would be concrete policy changes and how firms' input-output

decisions changed along with possible policy changes. Development zones in China may offer a bundle of preferential policies for firms inside development zones. Among these policies, cheap land is a one-shot transaction that occurs when a firm enters a development zone. Thus zone closures do not affect firms' land costs. However, subsidies and favorable loans may not be enjoyed as much since firms are no longer regarded as development zone firms. In our data, we observed subsidies and interest expenditures received by firms, which enabled us to examine whether zone closures really changed the preferential policies enjoyed by development zone firms. The results are exhibited in Table 2.

TABLE 2: PREFERENTIAL POLICY CHANGES ALONG WITH ZONE CLOSURES

	(1) subsidized (1=yes)	(2) ln(subsidy)	(3) loan dummy
treat×after2003	-0.0100	-0.0954	-0.00715
	(0.00734)	(0.0347)	(0.00856)
age	-0.0227	-0.205	-0.00253
	(0.0586)	(0.307)	(0.0575)
_cons	-26.87	-158.5	-0.217
	(4.234)	(21.29)	(4.589)
Year	Yes	Yes	Yes
Industry	Yes	Yes	Yes
Ownership	Yes	Yes	Yes
City	Yes	Yes	Yes
Provyear	Yes	Yes	Yes
N	59483	59483	59483
R^2 *within*	0.0168	0.0164	0.00634

Notes: All observations are at the firm-year level. Standard errors are in parentheses. Models are estimated using FE. *Year, Industry, Ownership, City* fixed effect are controlled using dummy variables. Provincial year trend is controlled using provincial dummies multiplied with year value. *age* is measured in hundred years.

In Table 2 we constructed three variables in order to capture preferential policy changes. The first is a dummy variable, *subsidized*, indicating whether a firm was subsidized in a specific year; the second is the log value of subsidies received by firms; the third is also a dummy variable, *loan dummy*, indicating whether a firm borrowed from banks (which equals 1 if a firm's interest expenditure was above zero in a specific year).Unfortunately, we do not know the amount of the loans. The results in Table 2 show that although the possibility of being subsidized and

borrowing from banks does not change significantly, the average amount of subsidies received by development zone firms declined by about 9.5% after the zones were closed. In industrial policy literature, the effects of subsidies are mixed, and whether subsidies improve target firms' performances depends on numerous other conditions (see Harrison and RodrÌguez-Clare, 2009). Here, we argue that if the subsidies can loosen firms' financial constraints, their TFP can be improved through a scale economy. Consequently, the closure of zones may result in a smaller scale and lower efficiency in affected firms.

Subsidies are one of the resources that can affect firms in development zones; however, subsidies are limited compared to the production scale of firms. The sample mean of subsidies is 133 thousand yuan, while that of value-added is more than 28,000 thousand yuan. It is unfortunate that other resources, such as favored loans and services provided by management committees, cannot be observed in our data; however, we can directly examine whether development zone closures have a significant effect on firms' production scales. In Table 3, we used value-added as the measure of production scale. We also used gross output values as a reliability check. Column 1 and column 2 show that zone closures significantly reduced firms' value-added by about 7.7% and reduced firms' gross output value by 4.8%. Columns 3 and 4 show that from the input side, the downsizing of the output scale is realized mainly by the decrease of labor employed. Capital stock was also reduced, but insignificantly.

TABLE 3: EFFECT OF ZONE CLOSURES ON FIRM'S SCALES

	(1)	(2)	(3)	(4)
	lnVA	ln output	lnL	lnK
treat×after2003	-0.0774	-0.0484	-0.0584	-0.0132
	(0.0206)	(0.0163)	(0.0111)	(0.0142)
age	0.0542	0.0767	0.210	0.161
	(0.0954)	(0.0866)	(0.0842)	(0.0827)
_cons	-276.6	-257.6	-121.2	-88.05

	(11.37)	(9.803)	(6.308)	(7.451)
Year	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes
Ownership	Yes	Yes	Yes	Yes
City	Yes	Yes	Yes	Yes
Provyear	Yes	Yes	Yes	Yes
N	59129	59390	59483	59412
R^2 *within*	0.154	0.234	0.0827	0.0447

Notes: All observations are at the firm-year level. Standard errors are in parentheses. Models are estimated using FE. *Year, Industry, Ownership, City* fixed effect are controlled using dummy variables. Provincial year trend is controlled using provincial dummies multiplied with year value. *age* is measured in hundred years.

C. Short-Term Effects of Development Zone Closures

An empirical fallacy in using a long period sample for DD specifications is that the greater the period of time after receiving treatment, the more likely it is that the trend of treated firms and controlled firms will become different. Therefore, (even though our analysis mainly relies on a long period sample) it is necessary to test the short-term effects of zones closures. The results are listed in Table 4.

TABLE 4: DD RESULTS USING A 2003-2004 SUBSAMPLE

	(1)	(2)	(3)	(4)
	TFP	lnVA	lnL	lnK
treat×after2003	-0.0469	-0.0684	-0.0341	-0.0174
	(0.0226)	(0.0217)	(0.00883)	(0.0110)
age	0.210	0.114	0.249	0.161
	(0.257)	(0.145)	(0.123)	(0.110)
_cons	110.3	7.535	15.66	60.28
	(20.72)	(23.24)	(11.50)	(13.99)
Year	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes
Ownership	Yes	Yes	Yes	Yes
City	Yes	Yes	Yes	Yes
Provyear	Yes	Yes	Yes	Yes
N	23296	23172	23296	23260
R^2 *within*	0.0725	0.0427	0.0345	0.0336

Notes: All observations are at the firm-year level. Standard errors are in parentheses. Models are estimated using FE. *Year, Industry, Ownership, City* fixed effect are controlled using dummy variables. Provincial year trend is controlled using provincial dummies multiplied with year value. *age* is measured in hundred years.

In Table 4, two important points become evident when comparing the short-term and long-term effects of zone closures. First, the direction and significance of estimated treatment effects on TFP, value-added, and factor inputs do not change. Second, the estimated treatment effects are smaller in absolute value in Table 4 than in Tables 1 and 3, but the differences are very small. The comparison between results using a long panel and a short panel shows that our estimation using samples from 2000-2007 is reliable.

D. Parallel Trend Test

Here we test whether parallel trends hold if we control for the full set of control variables. In Table 5, we estimated the differences of TFP, value-added, and total output between treatment groups and control groups in each year. The reference year is 2003 (the year before mass zone closures). In terms of TFP, value-added, and total output, the gaps between treatment groups and control groups in 2000, 2001, and 2002 did not significantly differ from those in 2003. Thus the pre-treatment parallel trend assumption holds.

TABLE 5: PARALLEL TREND TEST, CONTROL FOR THE FULL SET OF CONTROL VARIABLES

	(1) TFP	(2) lnVA	(3) lnoutput
treat×year2000	0.0173	0.0337	0.0739
	(0.0499)	(0.0505)	(0.0413)
treat×year2001	-0.0328	-0.0291	0.0330
	(0.0358)	(0.0327)	(0.0294)
treat×year2002	0.00961	0.0280	0.0150
	(0.0341)	(0.0367)	(0.0219)
treat3×after2003	-0.0665	-0.0727	-0.0359
	(0.0230)	(0.0203)	(0.0160)
_cons	-205.0	-282.0	-269.6
	(11.73)	(13.53)	(11.86)
Other controls	Yes	Yes	Yes
N	59483	59129	59390
R^2 *within*	0.106	0.154	0.235

Notes: All observations are at the firm-year level. Standard errors are in parentheses. Models are estimated using FE. *Year, Industry, Ownership, City* fixed effect are controlled using dummy variables. Provincial year trend is controlled using provincial dummies multiplied with year value. Other controls consist of *age*, year fixed effect, 2-digit industry fixed effect and city fixed effect, ownership type, and provincial specific time trend. The reference year is 2003.

Overall, the empirical results provided show that the closure of development zones affected the TFP of firms that had once been part of development zones that were later closed. The underlying logic is that there exists a scale economy in the manufacturing sector, through which lager sizes generate higher productivity.

V. Geographic Heterogeneity, Market Access, and the Effects of Development Zones

The previous section hypothesized that development zones affect TFP through a scale economy. Since China's manufacturing sector is highly export-dependent, the distance to major seaports largely determines a city's international transportation costs. Coastal regions that have more cities and higher population densities also enjoy greater domestic market access compared to inland areas. This section examines the geographic heterogeneity of the effects of development zones, and then explores how geographic heterogeneity is related to market access.

A. Geographic Heterogeneity

We examined the geographically heterogeneous effects of development zones on firms' TFP mainly because of the vast differences that exist between locations with advantageous geography and locations with disadvantageous geography in terms of participation in the global economy. Locational differences result in a huge gap between coastal and inland China in terms of economic agglomeration. From the central planners' perspective, interregional gaps in economic agglomeration justify their efforts to promote the development of lagging inland

areas using policies that were successful in coastal areas. However, as argued by Glaeser and Gottleib (2008), the location of where these policies are implemented is highly significant in terms of the overall success of place-based policies. In China, because of existing differences in geographical conditions and economic agglomeration, the success seen in coastal areas may not necessarily be duplicable in inland areas. Therefore, in this section, we examine the heterogeneous effects of development zones on firms' efficiency in the geographical dimension. We used three different specifications: (1) We split the full sample of firms into two parts according to whether a firm is located in a city within or beyond 500 kilometers from the nearest major seaport. The distance to the seaport also represents the regional heterogeneity in development zone policies before and after 2003, as shown in Figure 1. (2) In order to confirm the reliability of our analysis on geographic heterogeneity, we split our sample into coastal and inland provinces and repeated the regressions. (3) We interacted the location of zones (measured by the distance to the nearest major seaport: Shanghai, Hong Kong, or Tianjin) with the treatment dummy variable and the after 2003 dummy variable.

Before presenting the regression results, we present the contrast of TFP trends between treatment groups and control groups of the above two subsamples (see Figures 2 and 3). In Figure 2, from the subsample of firms located within 500 km of the three major seaports, it is evident that the pre-treatment common trends of TFP hold ideally for the treatment and control groups in terms of DD specification. However, in Figure 3, from the subsample of firms located beyond 500 km of the three major seaports, it is evident that TFP trends of the treatment and control groups show significant between-group differences, both before and after 2003. These two figures jointly show that development zone policies only improve TFP in the "within 500 km" areas.

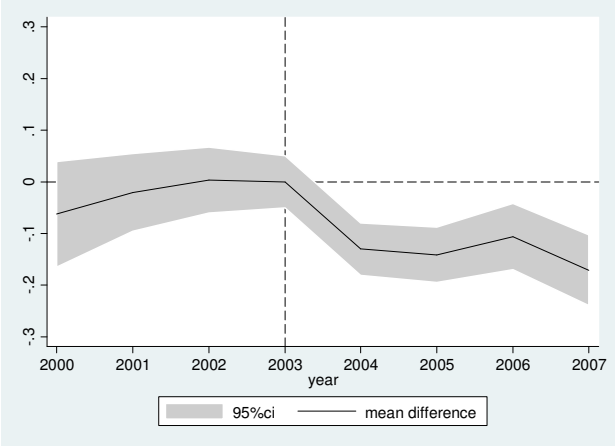

FIGURE 2: TFP DIFFERENCES BETWEEN TREATMENT AND CONTROL GROUPS, "WITHIN 500 KM" SUBSAMPLE.

Note: mean difference denotes the sample mean of TFP of treatment group minus that of the control group

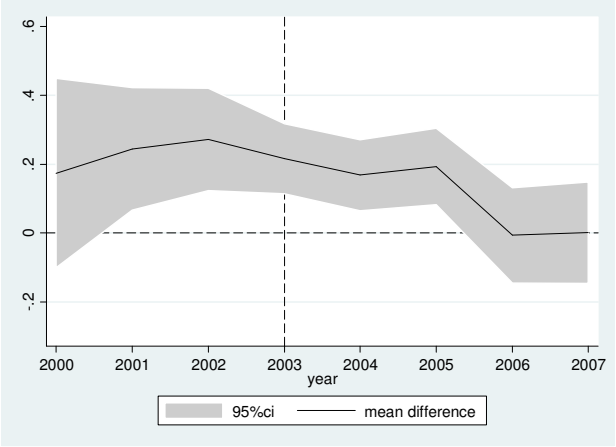

FIGURE 3: TFP DIFFERENCES BETWEEN TREATMENT AND CONTROL GROUPS, "BEYOND 500 KM" SUBSAMPLE.

Note: mean difference denotes the sample mean of TFP of treatment group minus that of the control group

In Table 6, we formally analyzed how development zones' effects on firm-level TFP vary with geography. In columns 1 and 2, we ran subsample regressions for firms in cities within and beyond 500 km of the nearest three major seaports. Columns 3 and 4 repeat the analysis, but divide the samples into eastern and inland groups. The results show that only the coastal areas experience negative effects when zones are closed. In column 5, we interacted the distance of the city to the nearest major seaports (*distport*) with the treatment effect (*treat*) variable, and *after2003* variable. The results show that the negative effects of zone closures become smaller in magnitude as the distance from zone closures increases. Based on the results of column 5, we created Figure 4 in order to demonstrate the marginal effect of zone closures and the accompanying 95% confidence intervals. The coefficient of the treatment effect changes from negative to positive at about 500 km. This justifies our division of subsamples using the cutoff point of 500 km. In column 6, we created a dummy variable, d500, in order to indicate whether or not a city was within 500 km of the nearest major seaports. Then we interacted this variable with the treatment effect variable and the after2003 variable. The coefficient of *treat×after2003×d500* is highly significant, showing that the difference of treatment effects within and beyond 500 km of the major seaports is significant.[3]

TABLE 6: GEOGRAPHIC HETEROGENEITY OF DEVELOPMENT ZONES' EFFECTS ON FIRM-LEVEL TFP

	(1) <500	(2) >500	(3) eastern	(4) inland	(5) full sample	(6) full sample
treat × after2003	-0.0962	0.0473	-0.0725	0.0549	-0.111	-0.0987
	(0.0227)	(0.0399)	(0.0225)	(0.0522)	(0.0278)	(0.0228)
treat×after2003×distport					0.000204	
					(0.0000701)	
treat×after2003×d500						0.165
						(0.0486)
after2003×distport					0.0000654	
					(0.0000637)	

[3] We also examined geographic heterogeneity using only the 2003 and 2004 panels. The results still indicated that only regions close to the major seaports experienced negative effects with the closure of development zones.

after2003×d500						0.0192
						(0.0445)
_cons	-204.7	-191.7	-195.5	-221.5	-196.5	-201.4
	(11.96)	(25.52)	(11.74)	(24.90)	(11.59)	(11.02)
Year	Yes	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes	Yes
City	Yes	Yes	Yes	Yes	Yes	Yes
Provyear	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
N	48091	11392	52489	6994	59483	59483
R^2 *within*	0.110	0.112	0.104	0.129	0.107	0.107

Notes: All observations are at the firm-year level. Standard errors are in parentheses. Models are estimated using FE. *Year, Industry, Ownership, City* fixed effect are controlled using dummy variables. Provincial year trend is controlled using provincial dummies multiplied with year value. *distport* denote the distance of the city a firm located in 2003 to the nearest one of Shanghai, Tianjin, and Hong Kong, measured in kilometers. *d500* is a dummy variable equals one when *distport* is larger than 500. Other controls refer to *age* in column 1~4, *age* and *after2003×distport* in column 5, *age* and *after2003×d500* in column 6.

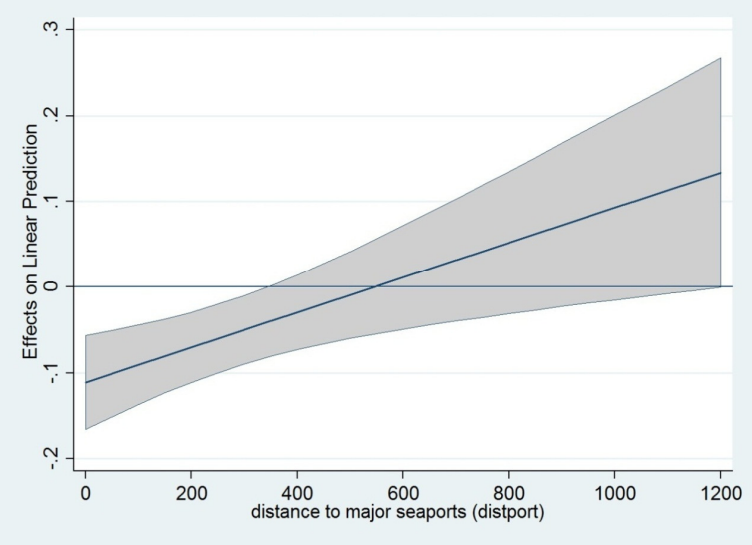

FIGURE 4: TREATMENT EFFECTS WITH RESPECT TO THE DISTANCE TO MAJOR SEAPORTS.

Note: distport denote the distance of the city a firm located in 2003 to the nearest one of Shanghai, Tianjin, and Hong Kong, measured in kilometers.

B. What Causes the Geographic Heterogeneity of Development Zones' Effects?

After identifying the geographically heterogeneous effects of development zones on firms' TFP, the remaining question to be answered is: Why do similar

policy measures have different impacts across regions? When reviewing the literature of place-based policies, one of the most attractive features of such policies is the use of agglomeration externalities (Neumark and Simpson, 2014). In China, while cities in different locations do share a common institutional background, the market conditions and economic opportunities vary significantly. As China's coastline is relatively short compared to the overall size of its territory, and only the eastern portion of the country faces the sea, the locational advantages in participating in the global economy are highly correlated to the distance of cities and regions to major seaports. Moreover, coastal regions also have larger populations which constitute a greater domestic market. As such, we decided to formally test whether the geographic heterogeneity of development zones directly contributes to underlying market condition differences. To do so, we constructed a city-level market potential index as a measure to capture market opportunities of firms in different cities. The market potential index is constructed as follows:

$$mp_c = \sum_{j \neq c} \frac{Y_j}{d_{cj}} + \frac{Y_c}{d_{cc}}$$

where

$$d_{\mathrm{cc}} = \frac{2}{3}\sqrt{\frac{area_c}{\pi}}$$

In constructing market potential, mp_c, Y denotes city-level GDPs that are collected from the *Chinese City Statistical Yearbook*. d_{cj} denotes the distance between city pairs (measured by the distance, in kilometers, between the city halls of each city). $area_c$ denotes the area of a city (measured by its jurisdiction area in squared kilometers).

Coastal China is characterized by greater market potential (obviously), but it is also a region characterized by greater market competition and a larger share of private sector investors. These effects must be controlled in order to determine whether market potential plays a role in geographic heterogeneity. The intensity of city-industry-level competition faced by firms is captured by the Herfindahl-Hirschman Index (HHI).

$$\mathrm{HHI}_{\mathrm{ci}} = \sum_{j=1}^{n} s_j^2$$

The subscripts c and i denote the city and 2-digit-level industry, respectively. s is the market share of a specific firm in the 2-digit-level industry, which is calculated using firms' sales. The importance of the non-SOE sector is captured by the percentage of the number of non-SOEs in the total number of firms at the city-level. Because we want to capture the cross-sectional variances of cities in different locations, all three variables above were constructed using data from 2003. All three variables are divided by sample median, and then placed in logarithmic form.

Table 7 illustrates the correlation matrix between the distances to major seaports and the three variables that capture the differences between cities. As expected, the three variables are correlated with the distance to major seaports. The greater the distance from major seaports, the lower the market potential becomes (along with decreases in the importance of non-state-owned sectors and levels of competition).

TABLE 7: CORRELATION OF COEFFICIENTS BETWEEN LOCATION, MARKET POTENTIAL, AND OTHER MARKET CONDITIONS

	distport	d500	mp	hhi	nonSOEr
distport	1				
d500	0.8070*	1			

mp	-0.5238*	-0.4998*	1		
hhi	0.1098*	0.0162*	-0.1061*	1	
nonSOEr	-0.3633*	-0.2359*	0.1492*	-0.1496*	1

Note: * denotes significance at the 1% level.

In order to empirically test whether the treatment effect of zone closures varies with the three geography-related variables, we interact each of the three variables (*mp*, *hhi*, and *nonSOEr*) with the $treat_i$ and $after2003$ variables. We then estimate Equation 1 Table 8 shows that market potential does matter. In column 1, the coefficient of the interaction term *treat_after2003_mp* is significantly negative. This means that the market potential of a city helps a development zone improve its firms' TFP. In Figure 5, the simulation based on regression results from column 1 also shows that the effect of zone closures on firm-level TFP changes with market potential. Development zones (or their closures) only affect firms' TFP in cities with high market potential. In column 2, we added the interaction terms with *hhi* and *nonSOEr.* Both results were insignificant, with the coefficient of *treat_after2003_mp* remaining almost unchanged.

TABLE 8: MARKET POTENTIAL AND HETEROGENEITY OF ZONES' EFFECTS

	(1) TFP	(2) TFP	(3) TFP	(4) TFP
treat×after2003	-0.0764	-0.0742	-0.0989	-0.102
	(0.0207)	(0.0216)	(0.0296)	(0.0290)
treat×after2003×distport			0.000117	0.000137
			(0.0000902)	(0.0000990)
treat×after2003×mp	-0.0943	-0.0935	-0.0811	-0.0767#
	(0.0379)	(0.0392)	(0.0464)	(0.0469)
treat×after2003×hhi		0.000907		0.000788
		(0.00651)		(0.00640)
treat×after2003×nonSOEr		0.0811		0.148
		(0.139)		(0.152)
_cons	-202.2	-202.7	-193.8	-194.4
	(11.14)	(11.44)	(11.59)	(11.40)
Other controls	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes
City	Yes	Yes	Yes	Yes
Provyear	Yes	Yes	Yes	Yes
N	59458	59458	59458	59458
R^2 *within*	0.107	0.107	0.107	0.107

Notes: All observations are at the firm-year level. Standard errors are in parentheses. Models are estimated using FE. *Year, Industry, Ownership, City* fixed effect are controlled using dummy variables. Provincial year trend is controlled using provincial dummies multiplied with year value. *distport* denote the distance of the city a firm located in 2003 to the nearest one of Shanghai, Tianjin, and Hong Kong, measured in kilometers. *mp*, *hhi* and *nonSOEr* are city-level (city-2 digit industry level for *hhi*) market potential, Herfindahl-Hirschman Index and non-SOE share of firm number, divided by sample median, and then placed in logarithmic form. Note that all the above three variables are at their 2003 level. Other controls refer to *age* and *after2003×mp* in column 1, *age*, *after2003×mp*, *after2003×nonSOEr* and *after2003×hhi* in column 2, *age*, *after2003×distport* and *after2003×mp* in column 3, *age*, *after2003×distport, after2003×mp*, *after2003×nonSOEr* and *after2003×hhi* in column 4, respectively.

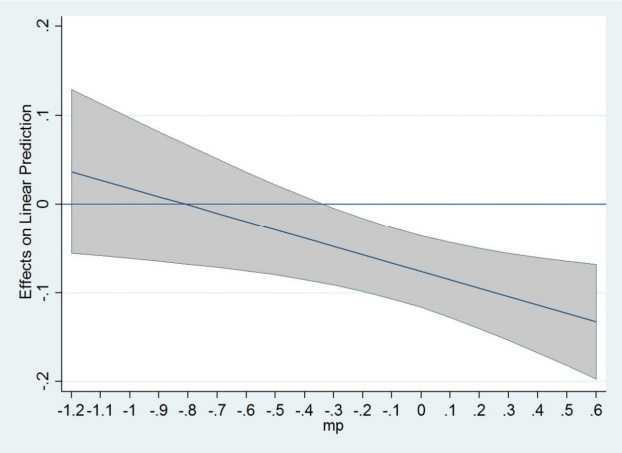

FIGURE 5: MARGINAL TREATMENT EFFECTS WITH RESPECT TO MARKET POTENTIAL.

Note: *mp* is city-level market potential divided by sample median, and then placed in logarithmic form. Note that *mp* is calculated using 2003 city-level data.

Although the results in the first two columns of Table 8 show that market potential itself does affect the role of development zones on firm-level TFP, whether existing market potential differences among regions helps to explain the locational heterogeneity of zones remains a problem. In columns 3 and 4, we controlled for the heterogeneous effect of zone closures with respect to both distance and market potential simultaneously. Compared with column 5 of Table 6, the coefficient of *treat×after2003×distport* is smaller in absolute value and not significant after controlling for zones' heterogeneous effects on firms' TFP (with respect to market potential). Moreover, the results changed little whether or not

we controlled for the heterogeneous effects of zones closures with respect to HHI and non-SOE rates. These two results confirm that market potential constitutes a major factor that helps to explain how location matters in terms of zones' effects on firms' TFP.

C. Geographic Heterogeneity of Development Zone Closures on Firms' Scales

Market potential helps firms increase productivity through a scale economy, which, in turn, constitutes a possible factor for development zones' effects on firms' TFP. If this assumption is taken as true, then the geographic heterogeneity of zones' effects on firm size will be similar to that on firms' TFP, meaning that development zone closures will experience downsized firms in coastal areas but not in inland areas. To be consistent with section 4, we used value-added and factor inputs as dependent variables to determine whether geographic heterogeneity exists. The results are reported in Table 9.

In Table 9, it is evident that the geographic heterogeneity of the effects of zones on firms' scales does exist. In locations that are closer to the three major seaports, the negative effects of zone closures on both firms' value-added and employment are greater in magnitude, regardless of whether we used continuous or dummy variables to measure the distance to major seaports. However, the same pattern does not apply to the results when dependent variables are the real value of firms' fixed assets. In agreement with the results displayed in column 4 of Table 3, the underlying reason for this result may be that it is harder for firms to adjust their fixed assets than labor. Moreover, the geographic heterogeneity of the effect of zones on firms' scales is similar to that on firms' TFP. In columns 1 and 3, the turning points of the marginal treatment effect on value-added and employment (with respect to distance to major seaports) are both around 600 km, which is very

close to the turning point of TFP in Figure 4. The results in columns 4 and 5, which measure the distance to seaports using dummy variables, also show similar patterns to those in Table 6, where we tested the geographic heterogeneity of zone closures on firms' TFP.

TABLE 9: GEOGRAPHIC HETEROGENEITY OF ZONES' EFFECTS ON FIRMS' SCALES

	(1) lnVA	(2) lnL	(3) lnK	(4) lnVA	(5) lnL	(6) lnK
treat×after2003	-0.132	-0.0996	-0.0254	-0.115	-0.0741	-0.0171
	(0.0263)	(0.0154)	(0.0182)	(0.0224)	(0.0126)	(0.0159)
treat×after2003×distport	0.000234	0.000163	0.0000534			
	(0.0000627)	(0.0000329)	(0.0000444)			
treat×after2003×d500				0.186	0.0797	0.0184
				(0.0444)	(0.0220)	(0.0345)
after2003×d500				-0.00533	-0.0520	0.0298
				(0.0449)	(0.0209)	(0.0312)
after2003×distport	0.00000269	-0.000126	0.0000101			
	(0.0000622)	(0.0000323)	(0.0000367)			
_cons	-276.4	-130.9	-87.28	-277.8	-124.3	-86.58
	(12.35)	(6.938)	(7.895)	(11.70)	(6.468)	(7.582)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes	Yes
City	Yes	Yes	Yes	Yes	Yes	Yes
Provyear	Yes	Yes	Yes	Yes	Yes	Yes
N	59129	59483	59412	59129	59483	59412
R^2 *within*	0.155	0.0833	0.0447	0.155	0.0830	0.0448

Notes: All observations are at the firm-year level. Standard errors are in parentheses. Models are estimated using FE. *Year, Industry, Ownership, City* fixed effect are controlled using dummy variables. Provincial year trend is controlled using provincial dummies multiplied with year value. *distport* denote the distance of the city a firm located in 2003 to the nearest one of Shanghai, Tianjin, and Hong Kong, measured in kilometers. *d500* is a dummy variable equals one when *distport* is larger than 500. Other controls refer to *age* and *after2003*×*distport* in column 1~3, *age* and *after2003*×*d500* in column 4~6.

VI. Conclusion

In this study, we used data from the 2000-2007 Chinese Industrial Firms Survey database in order to study the effects of a specific place-based policy (i.e. development zones) on firm-level TFP and its corresponding geographic heterogeneity. To alleviate the possible endogeneities of missing variables and reverse causalities, we made use of the policy shock that occurred between 2004 and 2006, during which more than 70% of development zones were closed. The results (using difference-in-difference specifications) showed that on average the

closure of zones reduced firm-level TFP by 6.5% on treated firms, and that the downsizing of firms can harm the efficiency of scale economies. Moreover, using the distance to the nearest major seaports (Shanghai, Tianjin or Hong Kong) we found that location matters significantly in terms of the efficiency of development zones: the greater the distance from major seaports, the smaller the negative effects of zone closures. By examining our results from an alternative perspective, we found that on average development zones are helpful in terms of firms' efficiency, but this positive effect only exists in regions close to major seaports. Furthermore, we found that market potential differences explain the geographically heterogeneous effects of zone closures. In other words, place-based policies only improve firms' TFP in places with high market potential.

Our empirical findings shed light on the location choices of place-based policies. In locations with low market potential caused by disadvantageous geography, place-based policies are not efficient. Furthermore, the overall allocative efficiency of economic resources is lessened if place-based policies are biased toward regions with lower market potential. Unfortunately, bias in placed-based policies is occurring in China, and explains (from a regional perspective) why China's TFP growth has been slowing down. In a large country like China, if the resources could be re-allocated by market forces across regions, the efficiency of the whole economy would be greatly improved.

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