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ABSTRACT

Using the mass closure of development zones in 2004 as a natural experiment, we examine the causal effect of development zones on firm level TFP in China. The difference-in-difference estimator shows that on average, loss of development zone policies results in 6.5% loss of firms' TFP. Locational heterogeneity is important. Within 500 kilometers from the three major seaports in China, closure of zones reduced firm-level TFP by 9.62%, whereas closure of zones farther away did not show significant effects. Market potential and local within-industry spillover effects can explain much of this locational heterogeneity. We conclude that China's strategy of using development zones as a place-based policy to encourage inland development may have led to spatial misallocation.

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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. In China, the government also launched similar place-based policies in different regions, which allows us to focus on interregional differences that determine the effectiveness of place-based policies. If place-based policies do not help improve productivity in lagging regions that are geographically disadvantaged, moving resources to there may cause spatial misallocation for the whole country.

From the 1980s onward, more and more development zones (*kaifaqu*) with similar preferential policies were implemented all over China, and their role changed from a development and reform experiment to promoting regional economic growth. This is typically the objective of these place-based polices. As Wang (2013) demonstrates, 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, locations targeted by new place-based policies may become substitutes for 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 locations with high productivity and high elasticity of productivity with respect to agglomeration. However, in China, the location of development zones is strongly 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 areas is straightforward: they hoped to promote the economic development of inland areas by replicating the policies used in coastal areas, and thus achieve national balance in regional development. However, there are major concerns about whether governments are able to pick the best areas in which to implement place-based policies (Glaeser and Gottleib, 2008). Competition between local governments can improve the efficiency of these 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 between 2004 and 2006 as a natural experiment to identify the causal effects of development zone policies on manufacturing firms' total factor productivities (TFP). In that movement, about 70% of the development zones (mostly in the coastal area) were closed, and development zones were more used to encourage inland growth. Our empirical analysis shows that the average effect of development zone closures on treated firms' TFP is negative. Moreover, we find 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 disappear. Our analysis shows that cities closer to the sea enjoy greater market potential, which helps firms exposed to preferential policies improve TFP through scale economies. We also provide evidence that firms enjoy local spillover effects from other firms in the same industry and city. Thus, when coastal areas lose output after development zone closure, the effect on TFP is magnified because of local spillover effects. In a nutshell, place-based policies lead to spatial misallocation, since preferential policies do not work as well in geographically

disadvantaged areas as they do in the coastal region. This also explains why more recently established development zones, opened in inland areas, have not been as successful as have those opened previously.

There are several econometric challenges to reliably estimating the effects of place-based policies (Neumark and Simpson, 2015). Our contributions in this study are three-fold. First, we use a natural experiment based on development zone closures in China to obtain a difference-in-difference (DD) estimate of the effects of development zones on firm-level TFP. This addresses the possible endogeneity of development zone policies, which poses a significant challenge. The location of development zones and whether a firm is in a zone may be correlated to either regional or firm-level characteristics. Our DD estimation using the natural experiment has identified within-city and within-industry effects of development zones that is more convincing than other existing estimations.

Second, most of the existing studies have examined the efficiency of development zone policies using city-level data[1], but have neglected to explore important forms of heterogeneity – particularly how the policies could be successful from a regional perspective (e.g., Wang, 2013; Alder et al., 2013). Other empirical studies of place-based policies have not explored why different place-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, Mayneris and Loriane, 2017) or why these effects may have been insignificant (Crozet et al., 2004; Bronzini and de Balsio, 2006; Elvery, 2009; Neumark and Kolko, 2010; Hanson, 2009; Hanson and Rohlin, 2013). There are, however, several exceptions. Besides Wang (2013) who found the sequential

[1] An exceptional firm-level study is conducted by Schminke and Van Biesebroeck (2013), who studied the effects of development zones on firms' exports.

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 or may not work; however, existing literature lacks evidence on the specific conditions that make place-based policies successful. Although Briant et al. (2015) found that zones that are more isolated show less positive impact, 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 in a whole country. Our finding is quite cynical and disappointing. In particular, the lagging areas that the place-based policies aim to help may lack precisely what is needed for the policies to be successful: market access and local spillover effects.

Third, we add a regional perspective to the literature on the misallocation literature. As Hsieh and Klenow (2009) documented, both the Chinese and Indian economies have 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 faces discrimination. Other studies focus on frictions from the regional level, such as overall heterogeneity in city level characteristics (Desmet and Rossi-Hansberg, 2013; Behrens et al., 2017), or particular individual frictions such as city level constraints on housing supply (Hsieh and Moretti, 2019) and heterogeneity in state tax rates (Fajgelbaum et al., 2019), as sources of spatial misallocation. We assume that discretional place-based policies may contribute an important source to spatial misallocation. 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 spatial

perspective on the misallocation of economic resources. Our empirical findings show that development zones are more efficient in coastal China. However, beginning in 2003, development zones were closed in eastern China, 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).

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, and in part 4 we measure the average effect of development zone closures on firms' TFP. Part 5 describes the analysis of why the effectiveness of development zones differs across regions. Part 6 discusses some concerns and implications of our findings. Finally, we conclude the study by discussing the implications of place-based policies in part 7.

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 could be 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, infrastructure would not be affected if they do not change their locations. 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, a document was issued detailing the rules of how to clear development zones approved by different levels 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, 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 or lower-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%, while 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 of zones between 2003 and 2006.[2]

The mass closure of zones between 2004 and 2006 provides us with an opportunity to identify the precise role of development zones in affecting firm performance. Whether or not 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 located therein. 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 TFP, and that they followed parallel trends in TFP before the closure movement. 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.[3] 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; Liang, Lu, and Zhang, 2016; Han and Lu, 2017).

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

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

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

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. Therefore, the two basic tasks of our study were: (1) identification of development zone firms and (2) estimation of firm-level TFP.

Identifying Development Zone Firms—The development zones that were closed in 2004 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 zones. 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 *yuanqu,* a Chinese word which means "zones" and may be in the addresses of any kinds of Chinese development zones; a group of words including *kaifa, jingkai* and *jingji,* which mean "development" or "economic development" and indicate that the firms may be located in national-level Economic and Technological Development Zones or provincial and lower-levels of development or economic development zones; a group of words including *gaoxin, kejiyuan, chuangyeyuan, touziqu, huojuyuan* and *huojuqu*, which are often used to describe zones where high/new-technology or newly-invested firms are supposed to located; another group of words including *gongyeyuan, chanyeyuan, gongyequ* and

gongyexiaoqu, which are used to name industrial zones; and the final group of words including *baoshui, bianjing and chukoujiagong*, which mean "bonded zones", "border (economic cooperation zone)" and "export processing zones" respectively.

To ensure that the measurement error was minimized, we compared our results with the official documents. 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 development zone policies was very close to the officially declared 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). Considering that the closed development zones were relatively smaller, our definition of development zones is plausible.

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 a major seaport, and again saw a sharp decline in development zone firms in 2004. This finding is consistent with the officially declared policy that development zones would be used as policies that favor inland provinces.

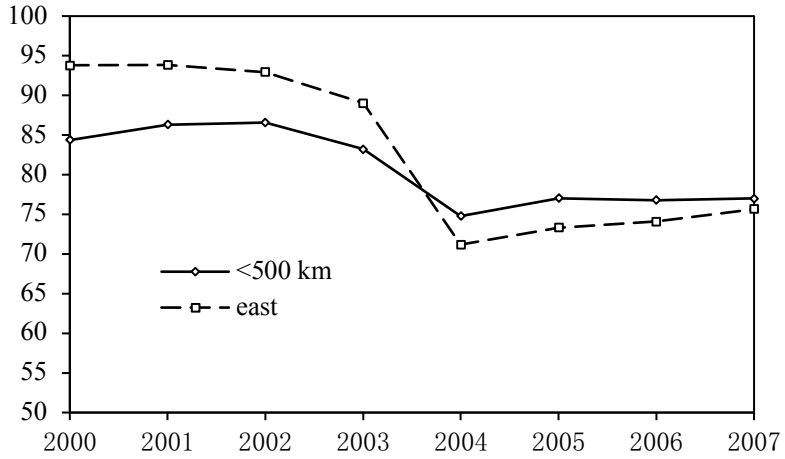

FIGURE 1: SHARE OF COASTAL CHINA IN DEVELOPMENT ZONE FIRMS IN THE ENTIRE COUNTRY.

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

Estimating Firm-Level TFP—Regarding firm productivity, a popular measurement is TFP, which is estimated by using the OP method (Olley and Pakes, 1996). This method considers the influence of TFP on firm investment decisions, and the influence of firms' investment decisions and 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 TFP, two specific points need to be clarified. First, the output we employed in the estimation of TFP is value-added and it is calculated by using the input-output method. Our estimation process builds upon 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.[4] 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's was to use the mass closure of development zones during 2004-2006 as an exogenous shock to firms that had been in development zones. 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, compared to the change in TFP of development zone firms not affected by the shock. 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) $y_{it} = \alpha + \beta treat_i * after2003 + \gamma X_{it} + city + indus + T + a_i + prov * year + \varepsilon_{it}$

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

[4] We do not report the lengthy procedure here in the interest of brevity. An appendix is available upon request.

(denoted as $city$, $indus$, T, and a_i, respectively). An interaction term of year and province-fixed effects is controlled 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 keywords 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 included observations of firms that were continuously in a development zone up until 2003, and then continuously no longer in a development zone 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.[5]

Besides the DD specification, we controlled the variables that were likely to be correlated with both a development zone's likelihood of closure 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.

[5] We find in our data that nearly half (7248 in 14793) of the firms that left the development zones reentered zones in 2006. The reason may be that after the 3-year zones clearing process, local governments, with strong economic growth incentives, relaxed the expansion of development zones. This assumption coincides with the fact that in 2006, the number of development zone firms in our database is more than doubled as in 2003, while both in 2004 and 2005, the number of zone firms is smaller than in 2003.

An important issue when using a DD specification is the construction of control groups, for which we used firms that did not change their status before or after 2003. 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 is more ideal, although we checked the robustness of our results using alternative reference groups. 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 as a robustness check.

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 simultaneous interventions. The fourth challenge is accounting for displacement effects: place-based policies may attract factor in-flow from or create positive spillover to non-targeted entities or areas, which will increase the difficulty of constructing an unaffected control group. 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 the first measurement issues. Using the mass closure of development zones in 2004 as a natural experiment, we can 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 the control group does not change before and after 2004, which means that the control group firms are hardly affected by the 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 those factors from conveniences that will not change after the closure of zones (such as better infrastructure).

IV. Treatment Effects of Development Zone Closures

A. Treatment Effects of Development Zone Closures on TFP

Table 1 reports 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 there were no treatment or control firms.

The coefficients on *treat×after2003* are significantly negative 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 exhibit large enough temporal variation. 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	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: Standard errors are in parentheses.

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 the firms located inside. Among these policies, 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
Prov.year	Yes	Yes	Yes
N	59483	59483	59483
R^2 *within*	0.0168	0.0164	0.00634

Notes: Standard errors are in parentheses.

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 logarithm of the 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 is not significantly reduced, 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 relax 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.

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

	(1)	(2)	(3)	(4)
	ln(VA)	ln(output)	ln(L)	ln(K)
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
Prov.year	Yes	Yes	Yes	Yes
N	59129	59390	59483	59412
R^2 *within*	0.154	0.234	0.0827	0.0447

Notes: Standard errors are in parentheses.

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 mainly associated with the decrease of labor employed. Capital stock was also reduced, but insignificantly. Overall, after the closure of development zones, the treated firms suffered more value-added loss than input reduction, thus lower TFP growth.

C. Short-Term Effects of Development Zone Closures

An empirical fallacy in using a long period sample for DD estimation is that the longer the period of time after receiving treatment, the more likely that the treated and controlled groups trend differently. Therefore, it is useful 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	ln(VA)	ln(L)	ln(K)
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
Prov.year	Yes	Yes	Yes	Yes
N	23296	23172	23296	23260
R^2 *within*	0.0725	0.0427	0.0345	0.0336

Notes: Standard errors are in parentheses.

In Table 4, two points are noteworthy 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 change little. 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.

D. Parallel Trend Test

Here we test whether parallel trends hold if controlling 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) ln(VA)	(3) ln(output)
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: Standard errors are in parentheses. The reference year is 2003.

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

TFP is essentially a measurement of input-output efficiency. If scale economies exist, firms' outputs may grow more rapidly than their inputs when they benefit from development zone policies. 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 international and domestic market access compared to inland areas. 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 inland areas using policies that have been 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. This section examines the geographic heterogeneity of the effects of development zones, and then explores the mechanisms.

A. Geographic Heterogeneity

We examine the heterogeneous effects of development zones on firms' efficiency by 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 seaports 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

repeat the regressions. (3) We interact the the distance to the nearest major seaport with the treatment dummy variable and the post-2003 dummy variable.

Before presenting the regression results, we show the trend of TFP difference between the 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 from the three major seaports, it is evident that the pre-treatment common trends of TFP hold ideally for the treatment and control groups. However, in Figure 3, from the subsample of firms located beyond 500 km from the three major seaports, it is evident that the 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.

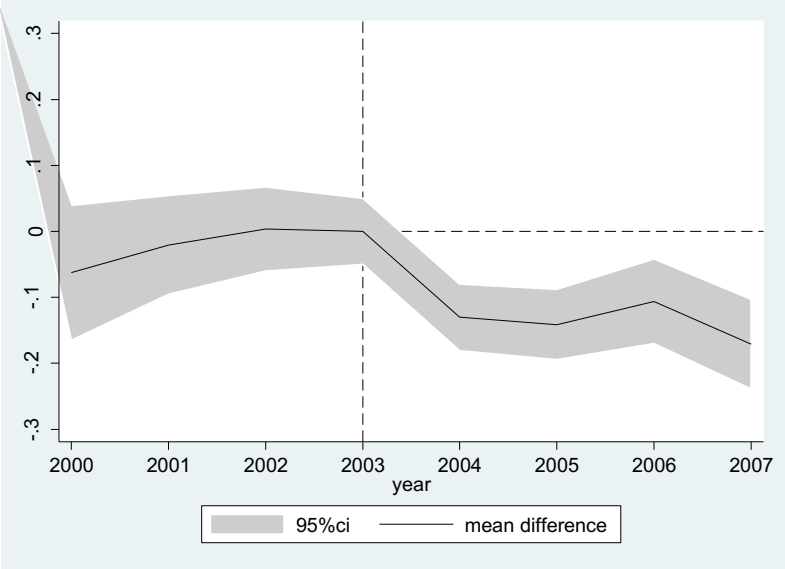

FIGURE 2: TFP DIFFERENCES BETWEEN TREATMENT AND CONTROL GROUPS, "WITHIN 500 KM" SUBSAMPLE.

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

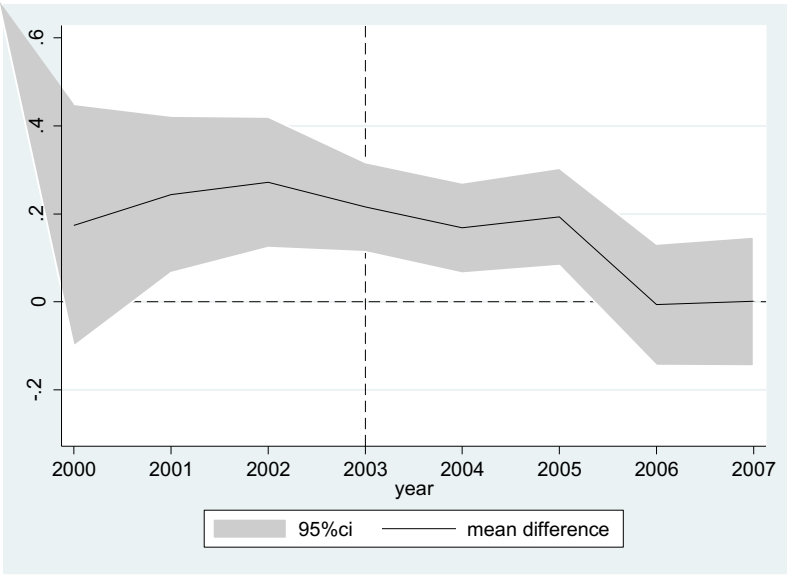

FIGURE 3: TFP DIFFERENCES BETWEEN TREATMENT AND CONTROL GROUPS, "BEYOND 500 KM" SUBSAMPLE.

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 from 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 experienced TFP loss when zones were closed. In column 5, we interacted the distance from the city to the nearest major seaports (*distport*) with the treatment effect (*treat*) variable, and *after2003* dummy. The results show that the negative effects of zone closures become smaller in magnitude as the distance from major seaports increases.

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)	
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: Standard errors are in parentheses.

Based on the results of column 5, we drew Figure 4 to demonstrate the marginal effect of zone closures and the 95% confidence intervals. The coefficient of the treatment effect changes from negative to positive at about 500 km from the major seaports. 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 is within 500 km from the nearest major seaports. Then we interacted this variable with the treatment effect variable and the after2003 dummy. The coefficient of *treat×after2003×d500* is highly significant, showing that the difference of treatment effects within and beyond 500 km from the major seaports is significant.[6]

[6] 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.

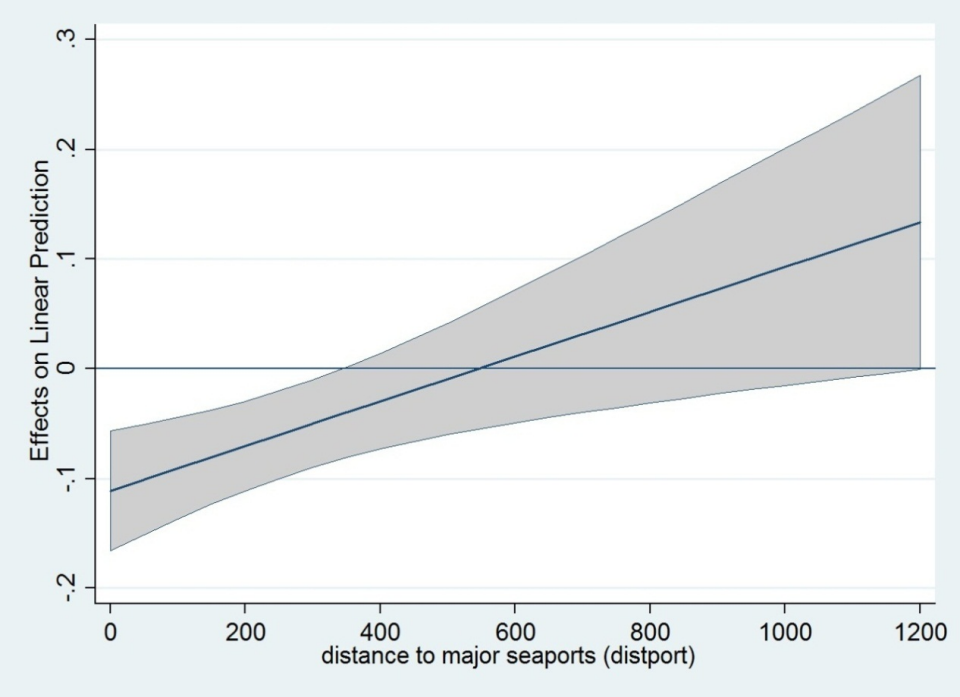

FIGURE 4: TREATMENT EFFECTS WITH RESPECT TO THE DISTANCE TO MAJOR SEAPORTS.

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 exploitation 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 from participating in the global economy are highly correlated with the distance of cities and regions to the major seaports. Moreover, coastal regions also have larger populations which constitute a greater domestic market. As such, we 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 firms. These effects must be controlled in order to determine whether market potential plays an independent role in geographic heterogeneity. The intensity of city-industry-level competition faced by firms is captured by the Herfindahl-Hirschman Index (HHI).

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

where 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, along with decreases in the share 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 $after2003$ dummies. 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: Standard errors are in parentheses.

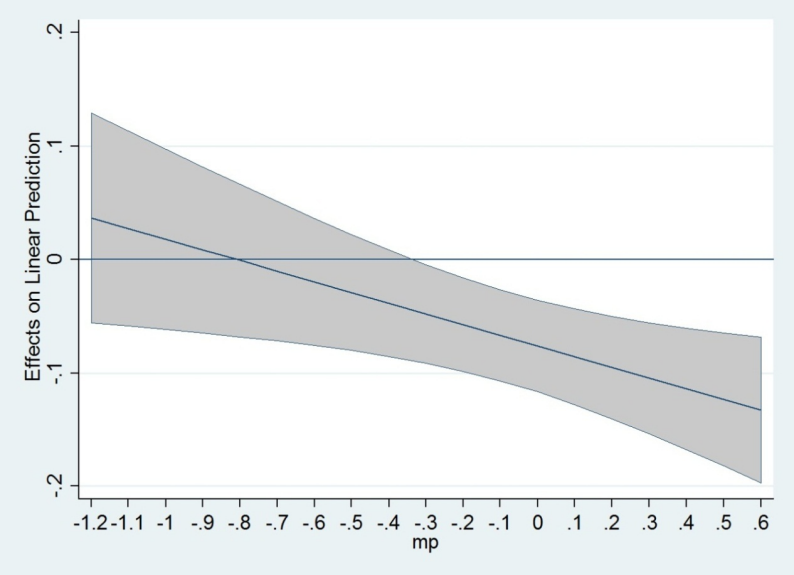

FIGURE 5: MARGINAL TREATMENT EFFECTS WITH RESPECT TO MARKET POTENTIAL.

Note: *mp* is city-level market potential divided by sample median, and then placed in logarithmic form. *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 effects 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 control 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 market potential. Moreover, the results change little whether or not we controlled for 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 scale economies, which, in turn, constitute a possible mechanism for development zones' closure to affect firms' TFP. If this is 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 use 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 use 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 patterns similar to those in Table 6, where we tested the geographic heterogeneity of zone closures' effects on firms' TFP.

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

	(1) ln(VA)	(2) ln(L)	(3) ln(K)	(4) ln(VA)	(5) ln(L)	(6) ln(K)
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: Standard errors are in parentheses.

If the coastal area lost in production significantly after zones' closure, while firms enjoy within-city and within-industry spillover effects, the coastal area's loss in TFP can be further explained.

VI. Discussion

A. Whether taking away a development zone is the same thing as the opposite of putting one in

There may be concern that a successful development zone may affect long term performance of firms, and after the removal of preferential policies, the positive effect will remain instead of fading away. If this were the case, then the negative effect of the closure of zones may indicate the overall failure of China's development zone policy, not the opposite.

To address this concern, we need to clarify the channels of how DZs affect firm level TFP. We assume the main channels through which we find the negative effects of zone closures are cost reductions and scale economies. Zones may reduce firms' costs through subsidies/tax holidays, financial support, cheap land, better government service, and better infrastructure, the last of which is ruled out in the settings of this paper because infrastructure improvement is already done when development zones were put in and will not go away. Except for infrastructure improvement, other channels of cost reduction are bound with DZ-firm identity, and will be taken away when development zones are closed. Although the physical investments are not reduced by former DZ-firms after the closure of zones (as showed in Table 4 and Table 9), probably because of high adjustment costs, former DZ-firms may reduce their scale of output and adjust their employment accordingly because of raised costs caused by closure of zones, reducing TFP through the scale economy mechanism.

Among the possible channels of cost reduction of DZs, we found evidence of a negative effect of DZ closure on the amount of subsidy received by former DZ-firms. The effects of DZ closure on the chances of getting loans from banks or receiving subsidies from the government, and on income tax burden turned out to be insignificant (in Table 2 and Table 10). However, we fail to find any method to

examine the channel of cheap land and better government service of zones, which may be more important in encouraging firms to grow larger.

TABLE 10: TESTING FOR THE CHANNEL OF SUBSIDIES, BANK LOANS AND INCOME TAX BURDEN

	(1) subsidized	(2) lnsubsidy	(3) loan dummy	(4) income tax over profit	(5) TFP
treat3_after2003	-0.0100	-0.0954	-0.00715	-0.00358	-0.0588
	(0.00734)	(0.0347)	(0.00856)	(0.0184)	(0.0198)
ln(sub)					0.00684
					(0.00188)
Loan dummy					-0.0165
					(0.00839)
taxoverprofit					0.00370
					(0.00250)
_cons	-26.87	-158.5	-0.217	-8.079	-197.9
	(4.234)	(21.29)	(4.589)	(5.365)	(10.35)
Other controls	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes
City	Yes	Yes	Yes	Yes	Yes
Provyear	Yes	Yes	Yes	Yes	Yes
N	59483	59483	59483	58611	58611
R^2 *within*	0.0168	0.0164	0.00634	0.000547	0.114

Notes: Standard errors are in parentheses.

For the importance of scale economy, we found that shutting down zones results in smaller output of former DZ-firms measured using value-added or gross output value (in Table 3 and Table 9), and after controlling for value-added, the effect of DZ closure on TFP turned to be insignificant (in Table 11). Furthermore, we found that closing zones does not change firm-level administrative expenses in spite of the output reduction. Thus, on average, closing zones increases general and administrative expenses per unit output (in Table 12).

TABLE 11: TESTING FOR THE CHANNEL OF SCALE ECONOMY

	(1) TFP	(2) TFP	(3) TFP	(4) TFP
treat3_after2003	0.00491	-0.00132	-0.0226	-0.0394
	(0.00956)	(0.0109)	(0.0139)	(0.0157)
treat3_after2003_d500		0.0308		0.0836
		(0.0228)		(0.0319)
after2003_d500		0.00154		-0.00117
		(0.0218)		(0.0338)
ln(VA)	0.757	0.757		
	(0.00537)	(0.00537)		
ln(output)			0.697	0.696
			(0.00824)	(0.00827)
_cons	10.61	10.48	-20.52	-21.24
	(6.372)	(6.484)	(8.349)	(8.397)
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	59129	59129	59390	59390
R^2 *within*	0.664	0.664	0.344	0.344

Notes: Standard errors are in parentheses.

TABLE 12: TESTING HOW DZ CLOSURE AFFECTS GENERAL & ADMINISTRATIVE EXPENSES

	(1) Total G&A expenses (log form)	(2) Total G&A expenses (log form)	(3) G&A expenses over value added	(4) G&A expenses over gross output
treat3_after2003	0.0239	0.0154	0.0508	0.00585
	(0.0151)	(0.0154)	(0.00944)	(0.00121)
treat3_after2003_d500		0.0402	-0.0339	-0.00101
		(0.0412)	(0.0170)	(0.00234)
after2003_d500		0.0701	0.0214	0.000509
		(0.0385)	(0.0163)	(0.00224)
_cons	-232.5	-229.0	21.28	2.654
	(7.707)	(8.053)	(3.667)	(0.465)
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	59212	59212	59364	59390
R^2 *within*	0.176	0.176	0.0116	0.0147

Notes: Standard errors are in parentheses.

B. What is the implication for relocation of zones based on the empirical findings?

Under the assumption that the negative effect of closing zones implies a positive effect of development zones more generally, the pattern of geographic heterogeneities of zone closure we found in Section V may suggest that it is worthwhile to relocate more zones to coastal China than to inland China. However, there are still two concerns. First, zones may have spillover effects on non-development zone firms, so that zones in coastal China may not definitively surpass inland zones even if they do have larger positive effect on development zone firms. Second, there may be other market failures – e.g., labor mobility costs preventing labor from leaving the interior and moving to the coast – that makes it necessary to sacrifice efficiency to gain equality.

In response to the concern over spillover effects, we examined the effect of zone removal on local non-DZ firms. The results show that removal of both coastal and inland zones positively affect the number, total employment, fixed assets and total value-added of non-DZ firms at the county/district-industry (2-digit) level (see Table 13). We also examined how zone removal affects the weighted average TFP of local firms. The results show that zone removal does not have a significant effect on non-DZ firms' TFP for coastal regions, but does have a positive effect for inland regions (see Table 14).

Recognizing the policy goal of regional equality through establishment of more development zones in inland China, we argue that it is necessary to consider the trade-off between enhancing labor mobility and using place-based polices to promote the development of left-behind regions. Considering the institutional obstacles to free labor mobility in China – i.e., the *hukou* system, which is costly both in terms of efficiency and equity – we argue from a long-term development perspective that it is far from necessary to use policy tools that will promote equity at the price of efficiency.

TABLE 13: SPILLOVER EFFECT OF ZONES REMOVAL ON COUNTY/DISTRICT-INDUSTRY SCALE

	(1) ln(# of firms)	(2) ln(# of firms)	(3) ln(total employment of non-DZ firms)	(4) ln(total employment of non-DZ firms)	(5) ln(total fixed assets of non-DZ firms)	(6) ln(total fixed assets of non-DZ firms)	(7) ln(total fixed assets of non-DZ firms)	(8) ln(total fixed assets of non-DZ firms)
treatci_after2003	0.668	0.776	0.789	0.957	0.841	1.045	0.736	0.816
	(0.0424)	(0.0543)	(0.0578)	(0.0730)	(0.0789)	(0.0945)	(0.0740)	(0.0957)
treatci_after2003_d500		-0.369		-0.561		-0.662		-0.271
		(0.0731)		(0.101)		(0.153)		(0.138)
_cons	-110.6	-118.0	-55.10	-59.13	-89.58	-94.42	-358.6	-363.2
	(15.54)	(15.18)	(19.35)	(18.58)	(21.43)	(20.80)	(22.99)	(22.41)
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Provyear	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	127122	127122	126175	126175	127217	127217	124975	124975
R^2 *within*	0.174	0.175	0.0442	0.0449	0.0504	0.0509	0.258	0.258

Notes: Standard errors are in parentheses. *treatci* is defined at the county/district-industry and measured using the ratio of the number of treat firms over the total number of firms in 2003 for each county/district-industry cell. after2003_d500 is controlled but not shown in the table to save space.

TABLE 14: SPILLOVER EFFECT OF ZONE REMOVAL ON COUNTY/DISTRICT-INDUSTRY LEVEL TFP

	(1) Non-DZ firms	(2) Non-DZ firms	(3) All firms	(4) All firms
treatci_after2003	0.112	-0.0409	-0.0607	-0.0984
	(0.0613)	(0.0715)	(0.0388)	(0.0492)
treatci_after2003_d500		0.470		0.0946
		(0.122)		(0.0767)
_cons	-295.3	-298.9	-296.0	-299.5
	(9.621)	(10.72)	(9.489)	(10.54)
Year	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes
Provyear	Yes	Yes	Yes	Yes
N	127122	127122	129332	129332
R^2 *within*	0.143	0.143	0.143	0.143

Notes: Standard errors are in parentheses. *treatci* is defined at the county/district-industry and measured using the ratio of the number of treat firms over the total number of firms in 2003 for each county/district-industry cell. after2003_d500 is controlled but not shown in the table to save space.

VII. 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 a 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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