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Essays in International Economics

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by

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ABSTRACT OF THE DISSERTATION

Essays in International Economics

by

Yasheng Zhang
Doctor of Philosophy in Economics
University of California, Los Angeles, 2024
Professor Jonathan E Vogel, Chair

This dissertation consists of two chapters that analyze the effects of placed-based policy and fiscal stimulus in international economics, with a focus on the Chinese economy.

In the first chapter, I investigate the causal effects of special economic zones (SEZs) on local economies in China using an instrumental variable (IV) approach, leveraging cultural ties between mainland China and Hong Kong as instruments. By examining data from the 2000s, the study reveals that SEZ status significantly increases GDP and GDP per capita, primarily through enhanced productivity and investment rather than mass labor influx. Unlike traditional OLS estimates, the IV method highlights the nuanced impact of SEZs on various economic factors, providing fresh insights into their long-term effectiveness and policy implications.

In the second chapter, co-authored with Kezhou Xiao, we investigate the disruptive effects on the Chinese state-owned enterprise (SOE) reform process following the Global Financial Crisis and the subsequent fiscal stimulus, which we treat as exogenous to firms. Using a triple difference (DDD) identification strategy, we report several findings: (i) The allocation of the 2008-2010 fiscal stimulus funds inversely correlates with prior levels of privatization and is preferentially channeled through SOEs; (ii) We discover that although SOEs exposed to the stimulus package outperform private firms in the same market, this effect is primarily

driven by SOEs operating in less privatized markets; (iii) Additionally, the stabilization program increased nationalization efforts at the aggregate level. In short, the fiscal stimulus represents a shift in SOE reform strategy, serving to strengthen those operating in less privatized markets, even as macro-level privatization deepens. We discuss the implications of our results using a political economy model with firm dynamics.

The dissertation of Yasheng Zhang is approved.

Margaret Etheridge Peters

Pablo David Fajgelbaum

Ariel Tomas Burstein

Jonathan E Vogel, Committee Chair

University of California, Los Angeles

2024

To Tofu, for being the joy in our family.

To Lucina, for lighting up my life.

To my parents, for always being there for me.

To my wife, for everything.

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VITA

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CHAPTER 1

Place-Based Policies and Long-Term Consequences: A Re-Evaluation of Special Economic Zones in China

1.1 Introduction

Special Economic Zones (SEZs) in China have long been regarded as one of the flagship policies of the Reform and Opening Up era in the 1980s and 1990s. These place-based policies transformed selected locations into special zones dedicated to economic development under a new economic system. Given their significant role in the transformation of the Chinese economy, this paper aims to identify the effects of being granted SEZ status on the local economy.

The SEZs in China have attracted considerable attention in the literature, with numerous empirical studies examining their impact on both the local and broader economy. The challenge in identifying the effects of SEZs primarily arises from the non-random selection of their locations. Early SEZs, being experimental in nature, were likely established in locations with higher chances of success, making their selection endogenous to growth potentials. This complicates comparisons of the long-term effects between SEZs and non-SEZs and hence makes their effects difficult to identify.

Unlike previous studies that use difference-in-differences (DiD) and event study (ES) methods, which attempt to match SEZs in a given period with the most similar non-SEZs to identify the effects of these place-based policies, this paper employs an instrumental variable (IV) approach. This method is motivated by insights from the historical literature on this period, providing a means to identify the causal impact of the SEZs.

The historical literature on the Deng Xiaoping-era reforms, such as Vogel (2011) and Leung (2008), provides detailed accounts of the origins of the SEZ policies. Specifically, the SEZs were initiated to attract foreign investment, which at the time came overwhelmingly from Hong Kong businesspeople, particularly ethnic Chinese. These Hongkongers, many of whom were emigrants or descendants of emigrants from mainland China, were interested in investing in mainland China, especially in areas near their ancestral hometowns. It is therefore likely that to cater to this preference, the central government considered the cultural connections between mainland regions and the Hongkongers when selecting the locations for the SEZs.

Given that mass emigration from mainland China to Hong Kong occurred many years before the Reform and Opening Up, the emigration pattern appeared quasi-random to policymakers in the late 1970s, thereby introducing an element of randomness into the selection of SEZs. Leveraging the IV method, I estimate the effects of SEZs using the cultural connection between a given prefecture in mainland China and Hong Kong as the instrument, based on the assumption that places with closer ties to Hong Kong were more likely to be selected as SEZs.

To find a suitable proxy for the cultural distance to Hong Kong, I use the dialects spoken in mainland China and Hong Kong to construct a cultural distance metric between a given prefecture and Hong Kong. Specifically, I utilize the Language Atlas of China, which contains detailed information on the dialects spoken in each mainland county in the 1980s, and the 1961 Hong Kong Census, which provides data on the dialects spoken in Hong Kong by population. By computing the cultural distance between counties in mainland China and Hong Kong and then aggregating this distance to the prefecture level, I can quantify the cultural ties. The coding scheme of cultural distance follows prior literature that uses Chinese dialects to measure cultural distance between regions within China.

A natural concern that arises with this IV approach is that the cultural connection to Hong Kong may largely reflect geographical proximity to Hong Kong, which is associated with other factors affecting growth potential. To address this concern, I include geographical distance to Hong Kong as a control variable, along with other pre-determined controls. Re-

sults indicate that cultural distance to Hong Kong remains a strong and significant predictor of SEZ status even after controlling for geographical distance to Hong Kong. Furthermore, using provincial-level data, I demonstrate that being culturally closer to Hong Kong is not associated with positive pre-reform outcomes, thereby further validating the instrument.

I run the main IV regressions on economic outcomes using data from selected years in the 2000s. The results reflect positive effects of SEZ status on GDP and GDP per capita, with the effects becoming stronger over time and statistically significant at the 5% level in some years. Compared to the OLS estimates, the effects from the IV results are generally smaller for total GDP but larger for GDP per capita. This suggests that the OLS estimates overstate the SEZ effects on GDP and understate them for GDP per capita.

To further understand the factors driving the differential outcomes of the SEZs, I break down the SEZ effects into labor, capital, productivity, and foreign-related outcomes. Unlike the OLS estimations, the IV approach identifies very small effects on population, employment, and total wage. Additionally, OLS underestimates the effects on capital-related outcomes such as investment and foreign direct investment (FDI). The biggest difference lies in productivity measures. While OLS results show no significant difference in productivity between SEZs and non-SEZs, the IV results reveal that SEZs are, in fact, much more productive. Using firm-level data, IV regressions also show that SEZs have a higher degree of marketization, reflected by a consistently higher share of private output and a greater increase in private firm entry. Finally, I run the IV regressions with slightly different specifications tailored to address various concerns about the robustness of the results and find similar outcomes.

To sum up the main results, the IV estimates present a different narrative from the OLS regarding the success of SEZs. Rather than relying primarily on mass labor and high investment, the SEZs perform better in economic outcomes due to higher productivity in addition to increased investment. Furthermore, the comparison of results on employment and total wages suggests that SEZs may actually suppress wages, although there are signs of improvement over time. Despite the differences from the OLS results, the SEZ story aligns with familiar themes in the broader context of the Chinese economy. With a stalling or even

decreasing population, SEZs (and arguably the entire Chinese economy) rely on high levels of investment to drive higher output. Despite increased productivity (perhaps a result of marketization), they still resort to wage suppression to maintain competitiveness (Klein & Pettis, 2020).

This paper contributes to a long line of empirical research on the effectiveness of China's SEZ policies, with recent advances made by Wang (2013), Alder *et al.* (2016), and Lu *et al.* (2019). Wang (2013) finds that SEZs generate higher FDI and productivity and do not crowd out domestic investment, with later SEZs causing greater distortions in FDI locations compared to earlier zones. Alder *et al.* (2016) also finds that SEZs lead to higher GDP and productivity growth, as well as significant spillover effects across neighboring regions. Lu *et al.* (2019) uses firm-level data to examine the short-term effects of SEZs, finding positive impacts on capital investment, employment, output, productivity, and wages, along with an increased net entry of firms.

While this paper finds broadly similar results to these prior studies, it differs in three main aspects. First, I use an instrumental variable approach to identify the effects of SEZs, differing from the DiD and ES frameworks used in previous research. Second, instead of examining SEZs at all levels, I focus on zones established by central government policies with 1994 as the cutoff date. This ensures that the SEZs in the sample were set up with consistent rationales and are less subject to local political interventions. These SEZs also cover entire prefectures rather than smaller areas within a prefecture, reducing the likelihood of crowding-out effects within the same prefecture. Finally, by focusing on the long-term effects of SEZs, I can break down and analyze the factors driving the differential aggregate effects of SEZs in the long run, offering fresh perspectives to the discussion.

This paper also relates to studies on place-based policies and special economic zones (SEZs) both within and outside China. Zheng *et al.* (2017) examines China's investment in industrial parks aimed at boosting economic growth, finding that the parks' impact on productivity, wages, and employment depends on their human capital, FDI share, and synergy with nearby firms. These parks also stimulate housing and retail growth, leading to the development of suburban consumer cities. Similarly, Schminke & Van Biesebroeck (2013)

investigates two types of preferential regional policies in China's manufacturing sector, finding that firms in Economic and Technological Development Zones (ETDZs) achieve higher export values through increased trade volumes and destinations, while firms in Science and Technology Industrial Parks (STIPs) excel in quality, fetching higher export prices and succeeding in high-income markets. These two types of policies are considered by some as subsets of SEZs. In the context of developing countries, Chaurey (2017) studies a location-based tax incentive scheme in India, finding significant increases in employment, output, fixed capital, and the number of firms, driven by both firm growth and new entries. There is no evidence of firm relocation or spillover effects, suggesting that the policy improves welfare and is cost-effective. In developed countries, Grant (2020) shows that SEZs in the US allow policymakers to selectively lower tariffs for certain manufacturers, driven by a desire to discriminate across buyers to raise seller prices.

Methodologically, this paper is closely related to Faber & Gaubert (2019), which studies the effects of tourism on the Mexican economy. By constructing a measure of beach quality based on specific local natural and cultural characteristics along the Mexican coastline, Faber & Gaubert (2019) instruments for local tourism revenue and finds that tourism causes significant local economic gains, partly driven by positive spillovers on manufacturing. In terms of constructing cultural distance between prefectures in China and Hong Kong, this paper closely follows the methodologies used in Gao & Long (2014) and Liu *et al.* (2015), both of which utilize the Language Atlas of China to compute measures of cultural connections between Chinese regions.

This paper also draws on a body of historical literature on the Deng Xiaoping-era reforms and China's transition to a market economy, such as Vogel (2011) and Coase & Wang (2016). These works provide insights into the political maneuvering during this crucial period in China's economic transition and motivate the instrumental variable approach used in this study.

1.2 Policy Background

This section describes the historical background of the SEZ reforms, focusing on how the SEZs were partially designed to attract investment and how this objective, when put into practice, provides an opportunity to identify the causal effects of SEZ policies.

1.2.1 Historical Context

In the late 1970s, the Chinese economy was in a dire state. The country lacked economic opportunities for young people, leading many to flee to Hong Kong via Guangdong, which became a serious security issue (Vogel, 2011). Additionally, China lacked the foreign currency necessary to import capital goods to restart the economy after a decade of the Cultural Revolution. While numerous significant events occurred between 1977 and 1980 that led to various reformist policies, what is particularly important for this paper is the government's shift toward welcoming overseas businesspeople of Chinese ancestry.

Deng Xiaoping, China's paramount leader in the late 1970s, was particularly interested in inviting ethnic Chinese people overseas to come back and invest in China. As explained in the seminal work on the Deng-era reform by Vogel (2011):

> Beijing sought investments from "overseas Chines" who lived in Southeast Asia, the United States, and elsewhere, but even more from "brethren" (*tongbao*, literally, those from the same womb), those living in territories claimed by China—Taiwan, Macao, and Hong Kong. At the time, not counting Taiwan, officials estimated that some 8.2 million descendants of Guangdong natives and some 5 million descendants of Fujian natives lived outside mainland China. As the two provinces sought investment funds, these descendants would be the primary targets of money-raising efforts, although investments from elsewhere would also be welcome. Those returning to China to visit in the years after 1978 overwhelmingly came through the "southern gate" to their ancestral homes in Guangdong and

Fujian.[1]

The roles that overseas ethnic Chinese played in China's development are explored in Leung (2008), while examples of overseas Chinese returning to their hometowns to invest are provided in Sawada (1998). Overall, businesspeople in Hong Kong and other parts of Southeast Asia were keen on investing in their ancestral hometowns or nearby areas, likely due to their attachment to ancestors and living relatives. However, at the time, completely opening up to foreign investment was out of the question, as many conservative officials were not ready to abandon their prejudices against the capitalist system and free market economy (Vogel, 2011). The reformist wing of the Communist Party found a way to welcome foreign investment while keeping dissent from conservatives manageable—they selected specific locations to use as trial grounds for policy experiments. This approach ensured that successes could lead to further opening up, while failures would have limited impact on the rest of the country. Through this method, the special economic zones were established and tasked with attracting foreign investment, primarily from the Hong Kong business community.

For reformist policymakers, balancing multiple considerations was crucial when selecting the locations of SEZs. They needed to ensure a decent chance of success, make the areas attractive to investors, and prevent failures from leading to uncontrollable consequences. Therefore, the choices were clearly non-random. However, given the historical context, being attractive to foreign investors implied that the locations would likely have certain connections to overseas ethnic Chinese. This suggests that the quasi-randomness of Hong Kong businesspeople's ancestry played a partial role in determining the locations of the SEZs.[2]

After the initial wave of SEZs, the flow between mainland and Hong Kong extended to other parts of China (Vogel, 2011). As part of efforts to deepen reforms, more cities and regions were opened up to become SEZ-style "open areas." Although policymakers' considerations might have evolved, attracting overseas investment remained a major goal of

[1] *Deng Xiaoping and the Transformation of China.* P. 403. Harvard University Press. Kindle Edition.

[2] Discussed in more depth in Section 1.3.3.

establishing SEZs. Consequently, a prefecture's connection to overseas Chinese communities likely remained a factor, whether explicit or implicit, in selecting new SEZ locations.

1.2.2 SEZ Policy

Preferential treatments given to the special economic zones (SEZs) are well-known and mostly consistent across all SEZs considered in this paper. Since the SEZs were created to attract investment from overseas investors, the specific policies accompanying their establishment were designed to benefit foreign investors, representing a significant departure from non-SEZ policies. Based on the summary in Wang (2013), these policies primarily encompass three areas: property rights enforcement, tax incentives, and special land use policies.

In the pre-reform era, the Chinese economic system did not recognize private property. Since the SEZs aimed to promote overseas-invested enterprises and joint ventures, ensuring that investors' assets, profits, and other rights were protected from appropriation and misuse was a high priority. The commitment by the Chinese government to protect private property within the SEZs is significant, considering that private property rights were not constitutionally protected outside the SEZs until the 2004 amendment.

Tax incentives were also established to encourage investment. In the SEZs, foreign investors benefit from a corporate tax rate of 15–24%, depending on the technological advancement of their products, compared to the 33% rate for domestic firms. They also enjoy minimal customs duties, duty-free allowances for production materials, and income tax exemptions for foreign expat employees.

As part of the pre-reform era legacy, all land in China is technically state-owned. However, within the SEZs, foreign investors can lawfully acquire rights to land for industrial and commercial use. They are also permitted to transfer, lease, or mortgage these rights for specified purposes and terms. For state-encouraged projects operating for more than 15 years, investors are exempt from land use fees for the first five years and pay only 50% of the usual fees for the subsequent five years. Guaranteed land use rights are provided for projects

with an investment of at least USD 10 million or those considered technologically advanced with substantial local economic influence.

While these policies are already special compared to non-SEZ areas, what makes the SEZs even more unique is the autonomy they obtained from the central government (Alder *et al.*, 2016). The SEZs were established at the beginning of a drastic economic transition period in China. Most policymakers at the time had no experience managing a market economy, so their choices involved much more than just adjusting tax rates. Many of the policies enacted faced pushback from the conservative wing of the government and other vested interests. As a result, local officials in charge of the SEZs had to be entrepreneurial in policymaking (Xu, 2011) and adept at dealing with both superiors in government and investors from overseas.

The autonomy granted to local leaders was crucial in allowing the SEZs to experiment with deeper structural reforms, setting them apart from non-SEZ areas. This level of autonomy enabled SEZs to implement innovative policies and respond flexibly to challenges, which was essential for their chances of success during China's economic transformation. Therefore, it is appropriate to study the effects of SEZ policies as a whole, rather than just treating them as a combination of tax cuts and reduced capital costs.

1.3 Data

This section describes the data and sources used for the empirical analysis. Additionally, I construct the main instrumental variable that will be used in the later sections.

1.3.1 Special Economic Zones

The term "special economic zone" initially referred to four coastal cities—Shantou, Shenzhen, and Zhuhai in Guangdong, and Xiamen in Fujian—that were opened up around 1980 to attract foreign investment and served as a trial run for broader economic reforms. Since then, it has been used to denote various geographical areas granted special status or treatment as part of the government's place-based policies.

In this paper, I focus on prefectures that received special status from the central government between 1980 and 1994, using the term "special economic zones" (SEZs) to refer to them. These SEZs were established with similar objectives, such as attracting foreign investment and increasing international trade, and they encompass entire prefectures.[3] They also precede most provincial-level special zones and national-level zones with specific purposes (e.g., high-tech zones).

Although interesting in their own right, I do not estimate the effects of the provincial-level SEZs and other special zones with specific purposes for two reasons. First, these zones are not comprehensive reform experiments like the national-level SEZs I consider. Instead, they focus on specific areas of the prefecture, which raises the question of whether they divert economic activities from other parts of the prefecture. Second, their establishment is less likely to follow the same rationale as the national-level SEZs. Locally designated SEZs are more prone to local favoritism and other political objectives of local leaders, whereas national-level SEZs enjoy more autonomy. Consequently, I do not consider the provincial-level special zones and the SEZs I study to be comparable.[4]

According to the seminal work on SEZs by Wang (2013), the SEZs considered in this paper are classified as Open Economic Areas (OEA). I provide the full list of SEZs studied in this paper, along with the relevant policies announcing their establishments, in Section A.1.1 in the Appendix.

[Insert Figure 1.1 here]

Figure 1.1 shows the geographical distribution of SEZs by the end of 1980, 1985, 1991, and 1994. By the end of 1980 (Panel A), there were only four SEZs (mentioned above), located in the southern coastal areas of China. The number of SEZs then gradually increased to 110 by the end of 1994. Although many SEZs were established in inland areas, the vast majority

[3]While foreign investment might be concentrated in specific parts of these cities, the cities themselves are listed as special zones without reference to specific locations within them, unlike later development zones and provincial special zones.

[4]Relevant research including provincial SEZs can be found in Alder *et al.* (2016) and Lu *et al.* (2019).

were concentrated in coastal regions and a few key land border areas. Therefore, I include dummies for whether a prefecture is coastal and whether it is located on the land border as additional controls.

Although the main regression is run with the full set of SEZs identified by the end of 1994, I also use 1985 and 1991 as additional cutoff years to define SEZs, to determine whether earlier SEZs differ substantially from the later ones. These cutoff years are chosen because approximately the same number of incremental SEZs were identified by these dates—30 SEZs by the end of 1985, 70 by the end of 1991, and 110 by the end of 1994. These periods were usually followed by significant waves of new SEZs—38 SEZs were added in 1988 and 33 in 1992. Hence, I believe these two cutoff years are appropriate for dividing the SEZs into different waves and will apply them to conduct robustness checks.

1.3.2 Outcome and Control Variables

The main outcome and control variables at the prefecture and province levels, including GDP, population, employment, other measures of economic activities and geographical features such as coordinates, are obtained from a collection of official statistical yearbooks at the national, provincial, and some prefecture levels. To ensure consistent data quality and to study the long-term effects of the SEZs, I collect data only for the years between 1998 and 2019. I match all prefecture-level administrative units to those of 2019, resulting in 297 consistent prefecture-level observations (including the four provincial level municipalities directly administered by the central government). However, some prefectures have missing data for certain years, leading to a slightly lower number of observations in the regression results.[5]

I also collect firm-level data from the Annual Survey of Industrial Firms (ASIF), a dataset on industrial firms published and maintained by the National Bureau of Statistics in China.

[5]Prefectures with missing data are generally less developed and therefore have fewer capabilities and resources to maintain high-quality statistics. Consequently, given the close association between SEZ status and economic development, any impact this may have on the results of SEZ effects in this paper is more likely to represent a lower bound of the true effects.

The ASIF is the primary panel dataset used for studying firm- and aggregate-level questions about the Chinese economy, covering all SOEs and non-SOEs with annual revenue greater than 5 million RMB. I use data from 1998 to 2007, as these years are considered to have better quality. I clean the data following the methodology outlined by Brandt *et al.* (2014).

Finally, to complement the official statistics, the quality of which is often subject to debate, I collect night-light data as an alternative measure of economic activity from the Prolonged Artificial Nighttime-light Dataset of China (Zhang *et al.*, 2024). This newly developed dataset covers night-light data in China from 1984 to 2020 with reportedly high accuracy. Night-light intensity is matched to the prefecture level to study economic development.

1.3.3 Construction of Instrument

1.3.3.1 Data Sources for Matching Dialects

The main instrument used in this paper is constructed using two main sources: the Language Atlas of China (China Academy of Social Science, 2012) and the Hong Kong 1961 Census Report published by the Census and Statistics Department of Hong Kong Government.

The Language Atlas of China (henceforth *the Atlas*) is a collective effort by the Chinese Academy of Social Science and the Australian Academy of the Humanities to map out Chinese language dialects (for both Han Chinese and ethnic minorities) in the 1980s and first published in 1987. For this paper, I focus only on the Sinitic dialects.

The Atlas codes the dialects in a hierarchy of groupings: supergroup, group, subgroup, clusters and locals. Two supergroups are identified in the Atlas, namely Mandarin and *Min*, along with eight distinct groups (e.g., Cantonese, or *Yue*) that do not belong to the above supergroups. A total of 109 subgroups are identified. For the purpose of constructing the instrument, I use only the first three layers (supergroup to subgroup) to match with the dialects mentioned in the Hong Kong census. At the county level (one level below prefecture), each is assigned a subgroup, with the corresponding group and supergroup attached. I match the Hong Kong census data with each county in mainland China.

[Insert Table 1.1 here]

The Hong Kong 1961 Census Report (henceforth *the Census*) records the dialects spoken by the population, categorized by age and gender groups. An excerpt of the page on dialects is shown in Figure A.1 in the Appendix. I summed the total number of people speaking each Sinitic dialect and then matched these dialects with the groupings in the Atlas. The results are presented in Table 1.1, with details of the matching process for each dialect provided in Section A.1.2 of the Appendix. For groups without a corresponding supergroup, I assigned a supergroup with the same name for presentation purposes.[6]

In 1961, Hong Kong was predominantly Cantonese-speaking, but five out of the ten distinct supergroups and groups had speakers in Hong Kong, indicating a decent level of variation within the population. I exploit this variation for the construction of the instrument, as discussed in the next section.

1.3.3.2 Coding Cultural Distance to Hong Kong

Given that the SEZs were established in part to attract investment from Hong Kong, the home bias of Hong Kong businesspeople played an important role in the location choices of the SEZs, and it is important the instrument captures this home bias. Therefore, I compute a measure of cultural distance between a given mainland prefecture and Hong Kong using the dialects spoken in both places, with the idea being that speaking similar dialects are indicative of closer cultural ties and a higher probably of shared ancestral roots.

To compute the measure of cultural distance, I start with county-level data on dialects. Let $d(c) \in D_m$ denote the dialect (subgroup) d spoken in county c, and D_m is the set of all dialects spoken in mainland China. Then, for each county c, I compute the distance between its dialect, $d(c)$, and a given dialect $d \in D_{HK}$ recorded in the Census, where D_{HK} is the collection of all dialects spoken in Hong Kong in 1961, as follows:

[6]This does not affect the coding of cultural distance.

$$\text{CulturalDistance}(d(c),\, d) = \begin{cases} 0 & \text{if } (d(c),\, d) \text{ are in the same subgroup,} \\ 1 & \text{if } (d(c),\, d) \text{ are in different subgroups but same group,} \\ 2 & \text{if } (d(c),\, d) \text{ are in different groups but same supergroup,} \\ 3 & \text{if } (d(c),\, d) \text{ are in different supergroups.} \end{cases}$$

This coding scheme follows the literature that uses the Atlas to construct cultural distances between different Chinese regions, such as Gao & Long (2014) and Liu *et al.* (2015). It leverages the hierarchical structure of dialect groupings in the Atlas, reflecting the fact that some dialects are closer to each other than others. To compute the cultural distance between county c and Hong Kong, I take the average of the distances between $d(c)$ and all dialects d spoken in Hong Kong, weighted by the share of the population speaking dialect d in Hong Kong in 1961. Formally, let θ_d be the share of Hong Kong population speaking d, I calculate:

$$\text{CulturalDistance}_{c,HK} = \sum_{d \in D_{HK}} \theta_d \text{CulturalDistance}(d(c), d).$$

Given that SEZ status is assigned at the prefecture level, I aggregate the county-level cultural distance to Hong Kong to the prefecture level using the population share of county c in prefecture p as the weight. I use the 1982 population census to obtain county-level population shares since it coincides with the period when linguistic research leading to the compilation of the Atlas was conducted. Additionally, it is close enough to the beginning of the reform era that migration patterns had not yet significantly affected the population distribution. Formally, let ω_c be the population share of county c in prefecture p, I calculate:

$$\text{CulturalDistance}_{p,HK} = \sum_{c \in C_p} \omega_c \text{CulturalDistance}_{c,HK},$$

where C_p is the set of all counties in prefecture p. For prefectures with missing data on cultural distance, I replace it with the within-province average. Details of the matching quality is discussed in Section A.1.3 in the Appendix.

[Insert Figure 1.2 here]

In Figure 1.2, the distribution of cultural distances to Hong Kong is plotted. Panel A shows the full sample, with the sample mean indicated by the red dashed line. Despite the majority of prefectures having a cultural distance greater than 2.9, there are significant variations in the lower part of the distribution. To highlight this variation more clearly, Panel B presents the distribution for a subgroup of prefectures with a cultural distance smaller than that of Beijing. Beijing is chosen as a reference because only Mandarin is spoken there, and thus the cultural distance between Beijing and Hong Kong represents the weighted-average distance between Mandarin and the dialects spoken in Hong Kong. To put the distribution into a geographical context, Figure 1.3 plots the cultural distance to Hong Kong on the map of China. I observe that, although the pattern of cultural distance broadly aligns with geographical distance, it is not a perfectly linear relationship. The variations in cultural distances follow their own unique patterns.

[Insert Figure 1.3 here]

To sum up, cultural distances to Hong Kong are constructed by leveraging the variations in dialects spoken in both Hong Kong and mainland China to capture the likelihood of shared cultural identities and ancestral roots between Hong Kong and each Chinese prefecture. The distribution of cultural distances follows a unique pattern that is not entirely explained by geographical distance alone. Nevertheless, geographical distance to Hong Kong will be controlled for in all subsequent analyses to ensure robustness of the results.

1.4 Empirical Strategy and Results

In this section I use the instrumental variable approach to estimate the long-term effect of gaining the SEZ status by 1994. I estimate the effects on measures of economic output, and then break down the findings into different components in order to see what factors are driving the effects from SEZs.

1.4.1 Reduced-Form Estimation

Given that cultural distance to Hong Kong is an important determinant in the SEZ policymaking process, to motivate the instrumental variable approach, I first run the reduced form regression between cultural distance to Hong Kong and economic outcomes in log.[7] Specifically, I test the following specification:

$$\log(y_p) = \alpha_s + \beta \text{CulturalDistanceHK}_p + \gamma' X_p + \epsilon_p, \tag{1.1}$$

where α_s is the province fixed effects and X_p are the pre-determined controls at the prefecture level. In particular, I control for the geographical distance to Hong Kong to address the concern that cultural and geographical distance to Hong Kong could be highly correlated. Therefore, without controlling for the geographical distance, any effects from cultural distance might simply be picking up the effects from geographical distance. I additionally control for the geographical distance to Beijing as a proxy for the influence from the central government, whether the city is coastal or inland, whether the city is a border city on land, and the longitude and latitude of the prefecture to proxy for fundamental climate conditions. To alleviate the concern that standard errors are auto-correlated within a given province, I cluster the standard errors at the province level. Due to data availability and the focus on long-term effects, I run the reduced form regressions separately for four years (2001, 2006, 2011 and 2016). The results are presented in Table 1.2.

[Insert Table 1.2 here]

The coefficients are negative on GDP and GDP per capita across all years, although the results are only statistically significant at 5% level for GDP per capita in 2011 and 2016. The sign of the coefficients point to the fact that having a closer cultural distance

[7]For consistency, both the reduced-form and IV regressions are run with dependent variables in current prices (where applicable). Alder *et al.* (2016) finds that in a limited sample with city-level inflation data between 1996 and 2010, cities with SEZs have an average yearly inflation rate of 1.8% compared to 2.3% for cities without SEZs. This difference is not statistically significant. Although the definition of SEZs in Alder *et al.* (2016) slightly differs from mine, their results suggest that SEZs do not systematically trigger higher prices.

to Hong Kong results in a higher average level of GDP and (significantly) GDP per capita. Using the GDP per capita of 2016 as an example, a one standard deviation decrease in cultural distance to Hong Kong (about 0.4) is associated with a roughly 10% higher GDP per capita. Furthermore, looking at the coefficients by year, the magnitudes of the effect from cultural distance are gradually increasing in later years. For GDP per capita in particular, this increase is occurring against the backdrop of a decrease in the effects of geographical distance to Hong Kong. Overall, the implication of the reduced-form results is that (a) the effects from cultural distance are magnified in later periods, and (b) cultural distance to Hong Kong is picking up effects on economic outcomes that are not captured by the geographical distance.

1.4.2 IV Estimation

1.4.2.1 First-Stage Results

I now turn to the instrumental variable approach in order to identify the effect of being an SEZ on economic and other related outcomes. As the first stage, I run the following regression to show the relationship between cultural distance to Hong Kong and the status as an SEZ:

$$\text{SEZ}_p = \alpha_s + \beta \text{CulturalDistanceHK}_p + \gamma' X_p + \epsilon_p, \tag{1.2}$$

where SEZ_p is an indicator equal to one if prefecture p is an national level SEZ (defined as in Section 1.3.1). To avoid running the "forbidden regression", I run OLS in the first stage and control for province fixed effects in addition to the other control variables that are also appear in Equation 1.1, although the results without control variables are included for reference. The first stage results are presented in Figure 1.4.

[Insert Figure 1.4 here]

I run the above regression with multiple SEZ cutoff dates (each representing the cumulative total number of SEZs by that time) to check if the results vary based on the establishment year of the SEZs. Specifically, I choose 1985, 1991, and 1994, as these years include roughly

similar numbers of incremental SEZs. Cultural distance to Hong Kong exhibits negative and significant effects (at the 5% level) on the SEZ indicator for all three waves, even after controlling for geographical distance to Hong Kong. The effect of geographical distance to Hong Kong turns positive for later waves, suggesting that closer to 1994, places that are geographically further from Hong Kong are more likely to become SEZs. In contrast, the coefficients for cultural distance remain negative and significant. The first stage results without control variables (except province fixed effects) are included for reference. The regression output table is provided in Table A.1 in the Appendix.

1.4.2.2 Instrument Validity

In my empirical strategy, the validity of the IV approach is based on the assumption that cultural closeness to Hong Kong affects economic outcomes only through the establishment of SEZs. There are a few ways this assumption may be threatened.

First, home bias among Hongkongers may affect a prefecture's outcome not only through their influence on SEZ status, but also through a higher level of import from regions of their ancestral home. This problem is likely to be more severe in the initial year of China's opening up, as the country is slowly returning to the global trade network and export to Hong Kong is particularly important. I partially this concern by using the outcome variables from later years (post-2000), a period in which the export to Hong Kong purely for local consumption (and not for re-export) is likely to be small compared to the overall size of the economy. Moreover, the inclusion of the geographical distance to Hong Kong also addresses this issue. Motivated by the gravity model in trade, the geographical distance is a good proxy for home bias while being a pre-determined variable.[8]

Second, the instrument's validity is threatened if cultural closeness to Hong Kong is related to other unobserved determinants of economic growth. Note that this unobserved determinant has to be orthogonal to the added pre-determined controls. I attempt to min-

[8]Controlling for prefecture-level export to Hong Kong would not address this concern properly since there is no way to separate goods sold purely for home bias and goods sold due to competitiveness, with the latter being endogenous to the SEZ status.

imize this problem by using the earliest census data from Hong Kong available. The 1961 census likely contains migrants who moved to Hong Kong before or shortly after 1949, the year after which it became much more difficult to leave the country. Given that the country went through an overhaul of the economic system after the Communist Party's takeover, whatever element that is driving the migration pattern before 1949 is unlikely to be affecting the growth potential under the new economic system, *after controlling for pre-determined geographical features*.

Nevertheless, I examine whether cultural distance affects pre-SEZ era outcomes. Due to the absence of prefecture-level data before the reform era, I replicate the reduced form analysis regression (Equation 1.1) using provincial data. Given that the dataset includes only 30 provinces, running OLS by year results in an insufficient number of observations per year for propert estimation. Consequently, I pool observations across years, dividing them into pre-reform (1952-1976) and post-reform (1995-2019) periods, each covering 25 years.

I compute the population-weighted average cultural distance to Hong Kong for each province using the 1982 population census data. Province fixed effects are replaced with region and time fixed effects, and standard errors are clustered at the provincial level to account for autocorrelation across years within a province. Regions (*diqu*) refer to six officially defined geographical areas in China, often cited in economic development discussions, though they usually hold no political significance.

Additionally, I replace the coastal region dummy variable with the log value of the provincial average distance to the coastline, acknowledging the geographical diversity within provinces. Some prefectures within a province may be significantly further from the coastline than the average of some non-coastal provinces. The results are presented in Figure 1.5, with the regression output table available in Table A.2 in the Appendix.

[Insert Figure 1.5 here]

For both GDP and GDP per capita, the coefficients are positive for the pre-reform era and negative for the post-reform era. Given that aggregating cultural distance to the provincial level loses valuable information, these results can only provide broad guidance. Although not

statistically significant at the 5% level, the signs of the coefficients suggest that closer cultural distance to Hong Kong does not influence growth in the pre-reform era but is beneficial in the post-reform periods. Thus, if cultural distance captures other elements of growth potential, they are not evident in the pre-reform period.

I acknowledge that certain determinants of growth potential may vary depending on the economic system. However, for such a determinant to pose a significant issue, it would need to be correlated with cultural distance, influence future economic output through channels other than SEZs, and persist after accounting for geographical distance to Hong Kong and other controls. If such a condition exists, the overlap of the SEZ reform with the economic transition could render it unidentifiable, at least with the current dataset.

1.4.2.3 IV Results

1.4.2.3.1 Baseline IV results on GDP and GDP per capita

Having discussed the relevance (first stage) and validity of the instrument, I now proceed to estimate the IV regressions. The regression specification is as follows:

$$\log(y_p) = \alpha_s + \beta \text{SEZ}_p + \gamma' X_p + \epsilon_p, \tag{1.3}$$

where the left-hand side represents the outcome variables in logarithmic form. The right-hand side includes an indicator variable for SEZ status and additional controls, including geographical distance to Hong Kong. The SEZ_p variable can be replaced with SEZ dummy variables from various cutoff dates, such as 1985 or 1991. For the main specification, I include all SEZs in the sample, using 1994 as the cutoff year, and leave other cutoff years for robustness checks.

[Insert Table 1.3 here]

The main results on GDP and GDP per capita are presented in Table 1.3. Similar to the reduced-form analysis, I conducted the regression separately for four different years. For IV

coefficients on GDP (Panel A), the effects of SEZs are smaller than the OLS estimates and are not statistically significant at the 5% level for all years. However, all IV coefficients are positive, suggesting that SEZs still have a positive impact on total economic output, albeit with a lower magnitude and higher variability than what OLS estimates show.

For GDP per capita (Panel B), the IV coefficients are consistently positive across all years, with the 2016 coefficient being statistically significant at the 10% level. Unlike the results for total GDP, the IV estimates show greater magnitudes than the OLS coefficients for all years except 2001. Additionally, the IV coefficients increase steadily over the years, contrasting with the OLS coefficients, which exhibit a slightly downward trend. Overall, the results suggest that the difference in per capita output between SEZs and non-SEZs is growing over time, and this increase is more pronounced for GDP per capita than for total GDP.

The first stage F-statistics reported in Table 1.3 likely raise the question of weak IV. However, this concern is mitigated because the IV regression in Equation 1.3 is just identified, making it approximately median-unbiased (Angrist & Pischke, 2009). In the just-identified case, weak instruments would be problematic only if the first stage were extremely weak. Results from the reduced-form and first-stage analyses indicate that this is not the case, affirming that weak instrument is not a threat to the identification strategy.

Having established the baseline results that being assigned the SEZ status is associated with higher GDP and even GDP per capita, I now move on to study the economic forces driving this difference. Specifically, I explore the impact of SEZs on labor-, capital-, productivity- and foreign-related outcomes at the prefecture level.

1.4.2.3.2 IV results on labor-related outcomes

[Insert Table 1.4 here]

Table 1.4 presents the results of the IV regressions on various labor-related outcomes, including population, employment, and the total wage bill. While all control variables are included

in the regressions, they are omitted from the table to focus on the variable of interest. The results are mixed.

The coefficients on population are negative across all years, contrasting with the positive coefficients from the OLS results. Due to data limitations, the population figures here only include individuals registered under the local *hukou* (household registration) system, excluding seasonal migrant workers. Two potential explanations for this are as follows. First, the *hukou* system imposes significant barriers to internal migration (Ngai *et al.*, 2019), with more developed cities imposing stricter controls to prevent population surges. Consequently, SEZs that thrive may struggle to attract long-term residents due to these barriers. Second, a combination of reduced fertility rates resulting from economic development and the stringent enforcement of the *one-child policy* suggests that wealthier, urban areas are likely to experience lower population growth or greater declines (Whyte *et al.*, 2015). This would be reflected in the registered population figures.

The effects of SEZs on total employment, a variable less affected by the *hukou* system, are positive for three out of the four years examined (Panel B). However, the IV coefficients are much smaller than the OLS counterparts and the corresponding differences in total GDP, indicating that SEZs do not attract as many workers as the GDP differences would suggest.[9] Finally, the coefficients on the total wage bill are negative for the first two years tested and turn positive in the later years (Panel C). Once again, the IV estimates are much smaller than the OLS estimates and even lower than the coefficients on total employment. This suggests relative wage suppression might be happening in SEZs that is not captured by simple OLS.

1.4.2.3.3 IV results on capital-related outcomes

[Insert Table 1.5 here]

Moving on to capital-related outcomes, Table 1.5 presents the IV results for these variables. The coefficients on investment are positive across all years (Panel A). Although they are

[9]For example, the IV results in 2016 suggest gaining the SEZ status by 1994 increases the GDP in 2016 by around 62.9%, while the corresponding increase in employment is only 22.4%.

smaller in magnitude than the OLS estimates, the difference is less pronounced compared to the differences observed for employment.[10] Similar patterns are observed for real estate investment and residential property investment (a sub-category of real estate), with coefficients that are larger in magnitude compared to overall investment (Panels B and C). For capital-related outcomes, the effects of SEZ status are much larger than those on labor-related outcomes and even surpass the effects on total GDP.[11] Therefore, it is much more likely that the positive SEZ effect on total GDP is driven by investment than by employment, and by extension consumption (as reflected by the total wage bill).

1.4.2.3.4 IV results on productivity-related outcomes

[Insert Table 1.6 here]

The results on productivity-related outcomes are shown in Table 1.6. Panel A presents the results on total industrial output, with coefficients that are positive for all years and increase in the later years (2011 and 2016). These coefficients are higher than the OLS counterparts and also surpass the effects on total GDP in the later years, suggesting that over time, the superior GDP associated with SEZs is increasingly driven by higher industrial output.

Panels B and C provide actual measures of productivity. Panel B shows the results for output per labor, where positive and increasing (over time) coefficients are observed, with the effect in 2016 being statistically significant at the 5% level. The coefficients on output per labor align with earlier observations regarding employment and total output—SEZs do not hire more labor compared to non-SEZs but produce more output. In contrast, the OLS estimates show coefficients with much smaller magnitude for 2011 and 2016, both in comparison to the IV estimates and to the OLS estimates earlier.

Finally, Panel C presents the OLS residuals, computed by regressing the log value of

[10] For example, in 2016, the OLS coefficients for both employment and investment are around 0.9, but the IV coefficient for employment is 0.22, much smaller than the corresponding number for investment, which is 0.76.

[11] The coefficients on investment are larger than the corresponding coefficients on total GDP for all four years.

total output on the log values of total wage and capital stock, using the residuals as a proxy for productivity. The effects are positive across all years and significant in the later years. For both output per labor and the OLS residuals, the IV coefficients are larger in magnitude than the OLS counterparts for 2011 and 2016. Combining with earlier results on labor and capital outcomes, this result reflects that the OLS estimates would attribute much of the differential increase in SEZ total output to higher capital and labor, and not much from increase in productivity, whereas the IV results suggest that the SEZ effects are more likely driven by higher investment and productivity, with limited contribution from labor input.

1.4.2.3.5 IV results on foreign-related outcomes

[Insert Table 1.7 here]

Table 1.7 presents the results on foreign-related outcomes. Due to issues with data quality (particularly missing values), I do not include results on import-export and instead focus on foreign direct investment (FDI) and related measures.[12]

In Panel A, the effects on FDI from the IV regression are positive across all years and statistically significant for 2006 and 2011. Except for 2001, the IV coefficients are consistently larger than the OLS estimates. Panels B and C show the results on total output from firms owned by foreign and Hong Kong, Macao, and Taiwan (HMT) owners, two mutually exclusive groups in China's economic statistics. For both measures, the coefficients are positive across all years and statistically significant for HMT-owned output for all years except 2001. The IV coefficients shown in all three panels are much larger in magnitude than the effects on GDP and total investment, suggesting that the heavy use of foreign capital contributes significantly to the superior performance of SEZs.

[12]Insufficient observations exist in my dataset to provide results on exports across the four years as in other analyses. For the year 2017, where export data are more complete, running the IV regression shows positive effects of SEZs with a magnitude greater than 1. These results are omitted here.

1.4.2.3.6 IV results on marketization

[Insert Table 1.8 here]

Given that a major component of SEZs' preferential policies is marketization, i.e., the gradual transition to a market economy, I test whether SEZs perform differently in terms of private versus state economy. To obtain ownership information, I use the Annual Survey of Industrial Firms (ASIF) dataset for the period from 1998 to 2007 to construct three measures of marketization at the prefecture-year level.

First, I aggregate private-firm and state-firm revenues at the prefecture level and compute the share of private revenue to total revenue in the given prefecture. I refer to this measure as "Private Share."[13]

Second, for each firm, I compute the ratio of its private contributed capital (also called paid-in capital) to total capital, and then aggregate these ratios to the prefecture level using each firm's assets as weights. I call this measure "Private Capital Ratio."

Finally, for each prefecture, I compute the number of private firm entries. I define firm i as a private entrant in year t if year t is the first year that firm i appears in a given prefecture and the firm is private. I then sum the total number of private entrants in a given year by prefecture. This way, I am able to capture not only entrants but also firms that have relocated to prefecture i. For the year 1998 (the first year of the data), I define a firm as an entrant if its registered age is equal to 1.

One caveat of using the ASIF data is that not all private firms are included in this dataset. From 1998 to 2007, private firms needed to have a revenue threshold of 5 million RMB to be eligible for inclusion.[14] Therefore, the marketization measures constructed are likely to be a lower bound of the true values. I again run the regression at four years (1998, 2001, 2004, 2007)

[13] I use the firm's registered ownership (state or non-state) to aggregate the revenues.

[14] See Brandt *et al.* (2014) for a detailed discussion.

The results on marketization are presented in Table 1.8. In Panel A, the IV regression coefficients on private share are consistently positive. In Panel B, the results are more mixed; IV coefficients decline across the years and turn negative in 2007. Finally, in Panel C, the effects on private entry are positive for all years except 1998. This finding is consistent with Lu *et al.* (2019). Overall, the results suggest that SEZs generally have a higher degree of marketization. However, signs of state advancement or mixed ownership reforms are evident, as shown by the declining average private capital ratio in SEZs.[15]

1.5 Robustness Checks

In this section I conduct a number of robustness checks.

1.5.1 Alternative Measure of Output

[Insert Table 1.9 here]

The quality of Chinese economic data is often subject to debate, and it has become standard in the literature to use some objective, alternative measures of output.[16] Following this trend, I use night-light data from the Prolonged Artificial Nighttime-light Dataset of China (Zhang *et al.*, 2024) to test the robustness of the IV regressions on GDP and GDP per capita.

Table 1.9, Panel A presents the results using the of average yearly night-light as calculated in Zhang *et al.* (2024) as a proxy for economic output. The IV regressions yield positive coefficients across all years, which are slightly higher than the OLS estimates. In Panel B, I normalize the night-light values by the registered population (those with *hukou* in the prefecture) and use it as a proxy for per capita output. The IV regressions again show positive coefficients for all years, with statistically significant results for 2006, 2011, and 2016. I also include the reduced-form regressions using night-light data as the dependent

[15] A detailed discussion on mixed ownership reform is beyond the scope of this paper. Allen *et al.* (2022) provides an overview of this issue.

[16] For example, Alder *et al.* (2016) uses night-light data to study the effects of SEZs.

variable and find negative and significant effects from cultural distance to Hong Kong. The regression output is presented in Table A.3 in the Appendix. These findings suggest that the main IV results are robust to alternative measures of economic activity.

1.5.2 Alternative Cutoff Dates for SEZs

[Insert Tables 1.10 and 1.11 here]

In the main IV results, I use 1994 as the cutoff to define SEZs, as there are far fewer national-level special economic zones established after that, and those that were established had vastly different rationales. However, the SEZ experiment began much earlier than 1994, and it is plausible that the earlier SEZs may have different outcomes in the 2000s compared to the later ones. For instance, looking back from the year 2000, an SEZ established in 1980 has had 10 more years, or 50% more time, to benefit from SEZ status compared to an SEZ set up in 1990. Moreover, the logic of using cultural distances to Hong Kong as an instrument still applies to earlier waves, and the first stage results are already reported in Figure 1.4.[17] Therefore, I rerun the main IV specification using 1991 and 1985 as alternative cutoff dates for defining SEZs.[18]

Table 1.10 presents the results on GDP and GDP per capita, with the main variable of interest being whether a prefecture is an SEZ by 1991. The coefficients on GDP and GDP per capita are very similar to the main IV results using 1994 as the cutoff date, with the effects on GDP per capita now being statistically significant at the 5% level for 2011 and 2016.[19] The reduced standard errors of the IV coefficients suggest that even with quantitatively similar effects, the pre-1991 SEZs likely have less variability in their output in the 2000s.

Table 1.11 shows the results with 1985 as the cutoff date for defining an SEZ. In contrast

[17] As long as no prefecture delayed obtaining its SEZ status precisely because it is culturally close to Hong Kong, the instrument remains valid.

[18] As aforementioned, these dates are chose because they roughly the same number of SEZs are added by each cutoff.

[19] Given that SEZs established between 1991 and 1994 are now grouped with prefectures that never became SEZs, the difference between pre-1991 SEZs and post-1994 non-SEZs is likely larger than shown here. The same logic applies to the regressions with 1985 as the cutoff date.

to both the main results and those in Table 1.10, the effects of gaining SEZ status pre-1985 are larger for both GDP and GDP per capita, especially in and after 2006. The standard errors are reduced even further, with statistically significant effects observed in 2016 for total GDP and in 2006, 2011, and 2016 for GDP per capita. These differences may be attributed to having SEZ status longer or that the reform policies in the earlier zones are better suited to drive economic growth. In any case, the results show that earlier waves of SEZs have higher total and per capita output with significant effects.

I also re-ran the estimations on measures of labor, capital, productivity, foreign exposure, and marketization using the different cutoff dates. The results are similar to the findings on GDP and GDP per capita, with the 1991 cutoff producing nearly identical effects and the 1985 cutoff yielding larger effects with generally smaller standard errors. These results collectively reaffirm the robustness of the main findings and support the interpretation that the superior performance of SEZs is driven by higher investment (a lot of which is foreign-originated) and productivity, and not by the combination of higher investment and labor as OLS results would suggest. The regression output tables are provided in Section A.3.3 of the Appendix.

1.5.3 Additional Controls

[Insert Table 1.12 here]

In Table 1.12, I control for two additional variables: the log values of the 1982 population and the first available land area. These controls are added to determine whether the IV results are robust when considering agglomeration potentials. The land area is taken from the first available data due to the fact that prefectures can change in size over time as administrative borders are redrawn. Since changes in land area are likely endogenous, I fix it with the first available observation for each prefecture.

The effects on both GDP and GDP per capita remain positive, with the magnitude generally being slightly smaller for GDP and slightly larger for GDP per capita. These results suggest that the IV regressions are robust to the inclusion of controls for growth

potential.

1.5.4 Alternative Years of Observation

[Insert Table 1.13 here]

The main specifications are performed on data from 2001, 2006, 2011, and 2016. Although there is no particular reason for choosing these years, I rerun the IV regressions on a different set of years-2003, 2008, 2013, and 2018-to ensure that the main results are not driven by the choice of years. The results are presented in Table 1.13.

The coefficients from the IV regressions are positive for all years for both GDP and GDP per capita, with the effects on GDP per capita being greater than those on GDP in each corresponding year. Compared to the original results in Table 1.3, the IV coefficients on GDP per capita are no longer increasing linearly over time. Nevertheless, these effects are similar in magnitude and remain larger than the OLS counterparts.

1.5.5 Removing Prefecture with Maximum Cultural Distance

[Insert Table 1.14 here]

Given that cultural distances to Hong Kong are coded from 0 to 3, prefectures with a cultural distance of exactly 3 cannot be further differentiated. To address the potential issue where the coding scheme fails to capture the true cultural distance, I ran the regression on a subset of the sample, excluding prefectures with the maximum distance. Depending on the year, around 27 to 29 prefectures were excluded. The results from the IV regression on this sub-sample are provided in Table 1.14. Compared to Table 1.3, there are no discernible quantitative or qualitative differences.

1.6 Conclusion

This paper examines the effectiveness of China's SEZ policies, highlighting their significant impact on the Chinese economy since the 1980s. Utilizing an instrumental variable (IV) approach inspired by historical literature on China's Reform and Opening Up, this study addresses the challenge of non-random SEZ location selection, providing a clearer understanding of their causal effects. The findings reveal that SEZ status leads to higher GDP levels and even greater increases in GDP per capita compared to non-SEZs in the 2000s. This contrasts with OLS estimates, which tend to overstate the effects on total GDP while understating the impact on GDP per capita.

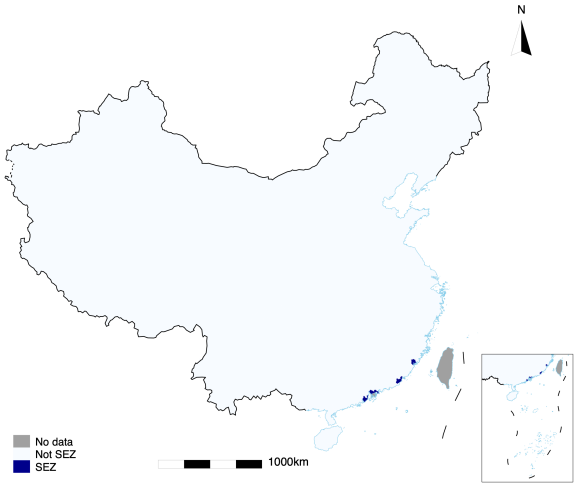
Further analysis uncovers that the differential outcomes of SEZs are driven by various factors, including labor, capital, and productivity outcomes. The IV approach identifies small effects on population, employment, and total wage, but highlights substantial impacts on investment and productivity. Additionally, the study shows that SEZs exhibit a higher degree of marketization, reflected by a greater share of private output and increased private firm entry.

These findings are found to be robust to alternative regression specifications and measures of dependent variables. Notably, the SEZ effects remain consistent when using night-light data as an alternative output measure. Furthermore, SEZs established in earlier years, particularly before 1985, demonstrate larger effects with less variability.

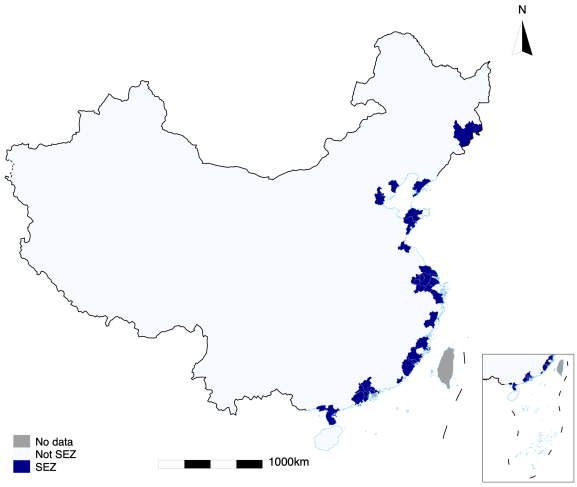
Overall, this paper provides fresh insights into the long-term effectiveness of SEZs and their role in China's economic transformation. The results suggest that the superior performance of SEZs is primarily driven by higher productivity and investment rather than by mass labor influx. This aligns with broader trends in China's economic growth, where higher investment (much of it initially from abroad) and increased productivity (likely due to marketization) drive economic growth against the backdrop of stalling population and wage growth.

Figure 1.1: Distribution of SEZs by Year

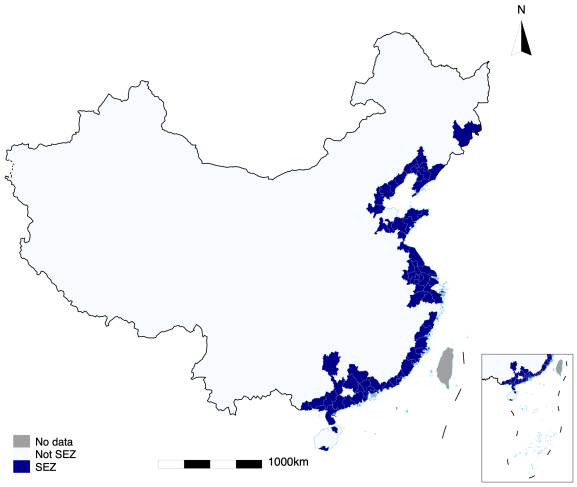
Panel A: SEZs by 1980



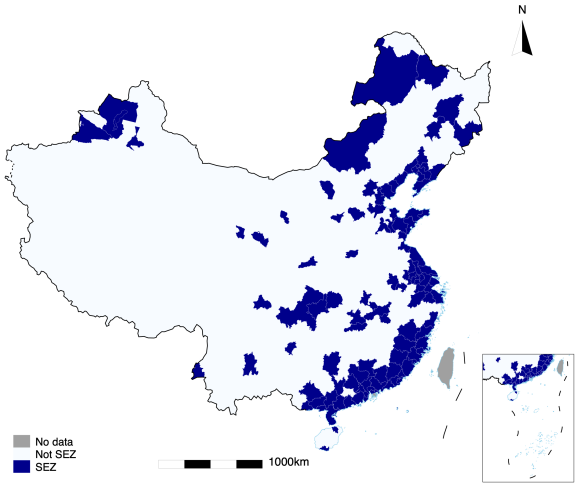
Panel B: SEZs by 1985



Panel C: SEZs by 1991



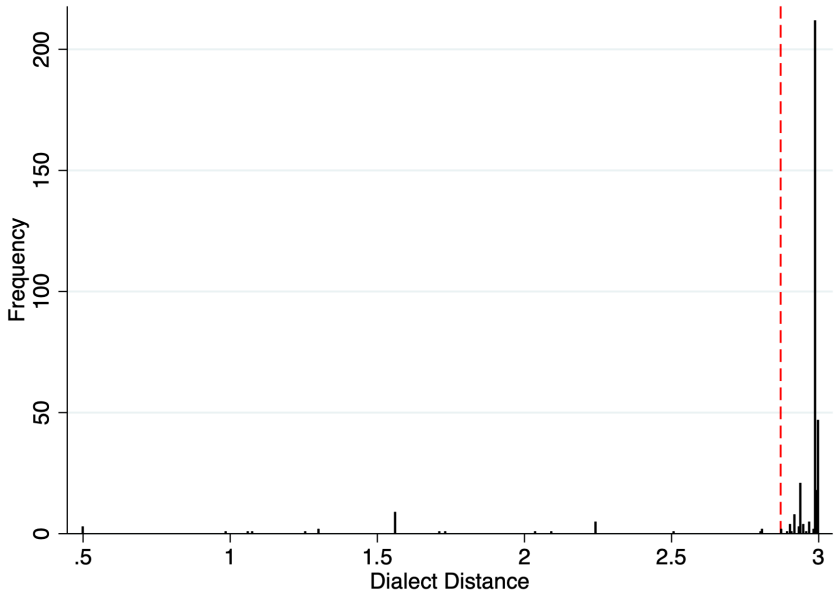
Panel D: SEZs by 1994



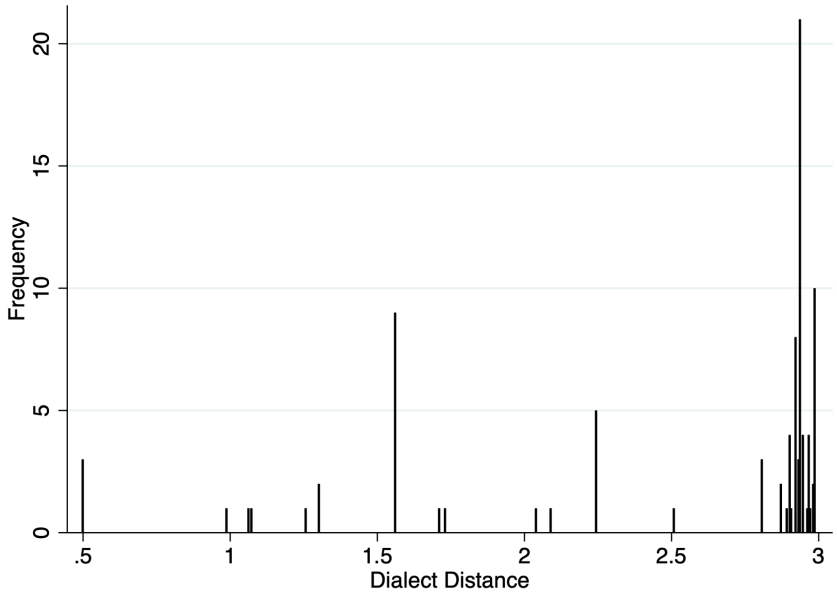
Notes: This figure presents geographical distributions of SEZs at the prefecture level by the end of 1980, 1985, 1991 and 1994. SEZs are marked by dark blue and non-SEZ areas are marked by light blue.

Figure 1.2: Distribution of Cultural Distances to Hong Kong

Panel A: Distribution of Cultural Distances to Hong Kong, Full Sample

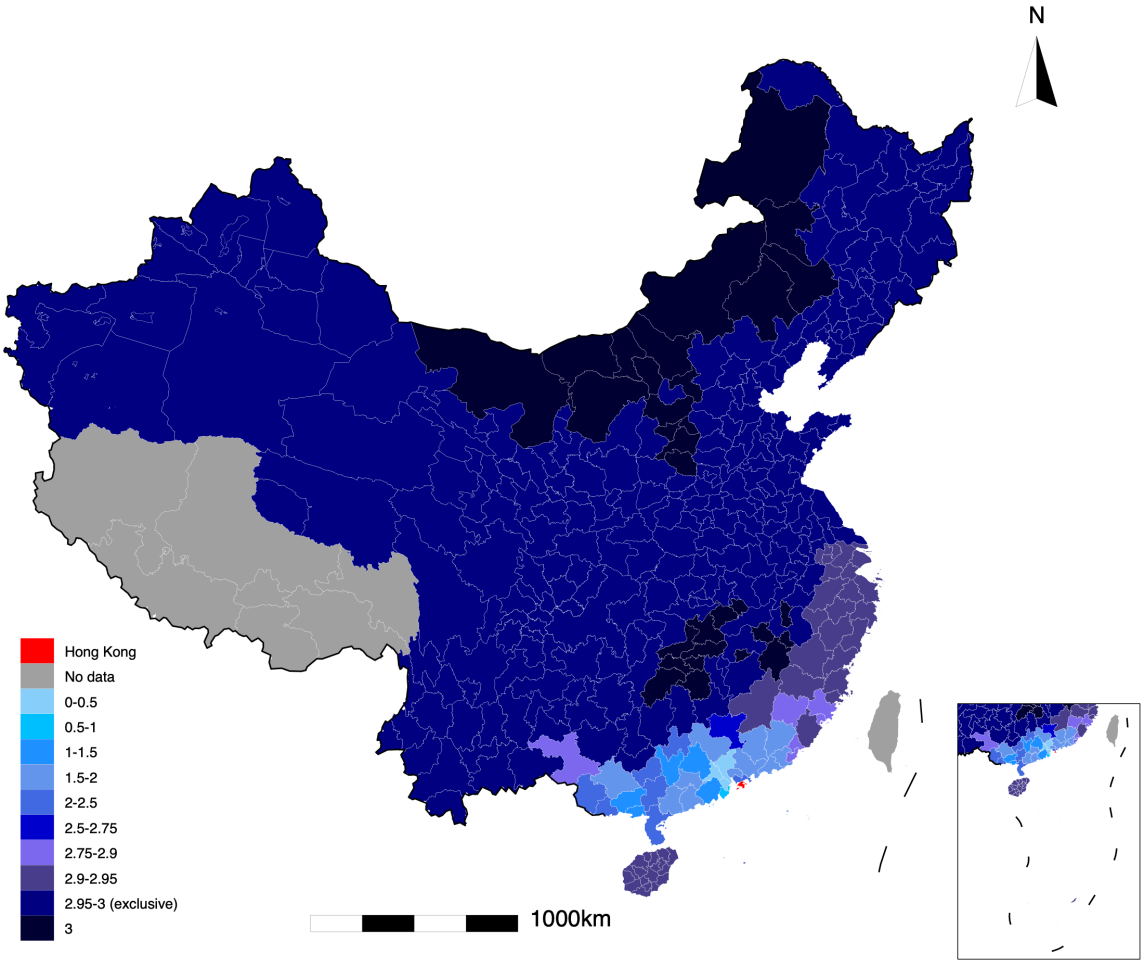


Panel B: Distribution of Cultural Distances to Hong Kong, Distance Smaller than Beijing



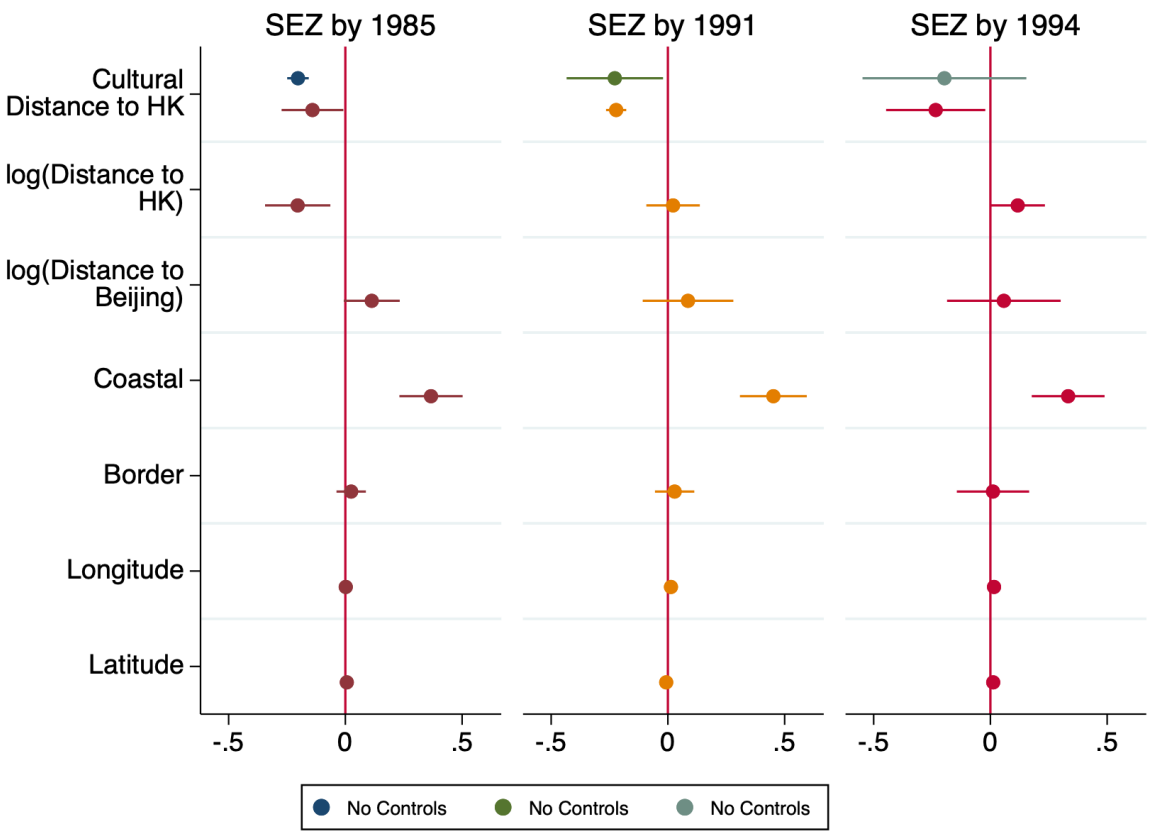
Notes: This figure presents the frequency distribution of cultural distances between Hong Kong and prefectures in China, computed as in Section 1.3.3. Prefectures with missing values receive the within-province average. Panel A presents the distribution with the full sample. Panel B presents the distribution after trimming the prefectures with a cultural distance smaller or equal to that of Beijing. The red dashed line in Panel A represents the mean.

Figure 1.3: Cultural Distance to Hong Kong by Prefecture



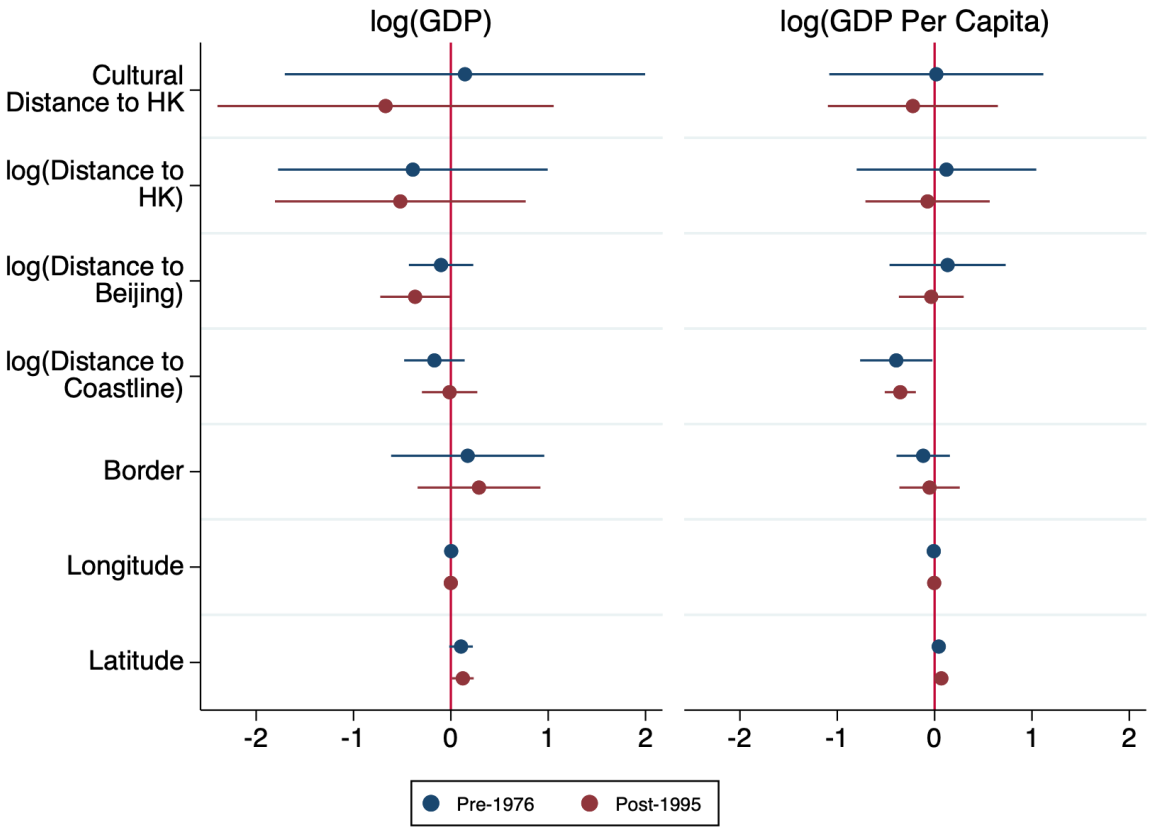
Note: This figure presents the geographical distribution of cultural distances between Hong Kong and prefectures in China, computed as in Section 1.3.3. Prefectures with missing values receive the within-province average.

Figure 1.4: First Stage Results, by Cutoff Date



Note: SEZs by year show the accumulative totals of SEZs by the end of 1985, 1991, and 1994. Cultural distance to Hong Kong is coded as in Section 1.3.3. Results with the "No Controls" label refer to first stage results of SEZ status on cultural distance with only province fixed effects and no additional controls. Province fixed effects are included for all results. Standard errors are clustered at the province level. The 95% confidence intervals are provided.

Figure 1.5: Effect of Cultural Distance on Pre- and Post-Reform Outcomes, Provincial Data



Note: Cultural distance to Hong Kong is computed by taking the population-weighted average cultural distance at the prefecture level. Pre-1976 refers to coefficients from running the regression on the period 1952 to 1976, while post 1995 refers to the period 1995 to 2019. Region and time fixed effects are included for all results. Standard errors are clustered at the province level. The 95% confidence intervals are provided.

Table 1.1: Population of Each Dialect Spoken in Hong Kong and Matching Results with the Language Atlas of China

HK Dialect	Population	Supergroup	Group	Subgroup
Cantonese	2,076,210	Yue	Yue	Guangfu
Hakka	128,432	Hakka	Hakka	Yuetai
Hoklo	164,537	Min	Minnan	Chaoshan
Sze Yap	114,484	Yue	Yue	Siyi
Shanghai	69,523	Wu	Wu	Taihu
Kuo Yu	26,021	Mandarin		
Total	2,579,207			

Note: Detailed matching process can be found in Section A.1.2. For groups without a corresponding supergroup, I assign a supergroup with the same name. *Source: Language Altas of China and Hong Kong 1961 Census Report.*

Table 1.2: Reduced Form Results on GDP

	log(GDP)				log(GDP Per Capita)			
	(1) 2001	(2) 2006	(3) 2011	(4) 2016	(5) 2001	(6) 2006	(7) 2011	(8) 2016
Cultural Distance to HK	-0.0522	-0.0363	-0.124	-0.146	-0.0592	-0.171	-0.237**	-0.242***
	(0.203)	(0.171)	(0.155)	(0.114)	(0.130)	(0.135)	(0.0959)	(0.0695)
log(Distance to HK)	-0.569***	-0.814***	-0.728***	-0.714***	-0.819**	-0.515*	-0.370	-0.354
	(0.0872)	(0.146)	(0.167)	(0.180)	(0.335)	(0.273)	(0.262)	(0.257)
log(Distance to Beijing)	0.00338	0.130	0.0921	-0.0669	0.193	0.377	0.269	0.138
	(0.202)	(0.193)	(0.180)	(0.170)	(0.286)	(0.296)	(0.253)	(0.210)
Coastal	0.0867	0.0934	0.0644	0.105	0.410***	0.286*	0.234*	0.276**
	(0.115)	(0.134)	(0.113)	(0.115)	(0.146)	(0.161)	(0.124)	(0.121)
Border	-0.698**	-0.501**	-0.485**	-0.424**	-0.132	-0.179	-0.132	-0.0813
	(0.281)	(0.186)	(0.180)	(0.170)	(0.143)	(0.121)	(0.133)	(0.121)
Longitude	0.0305	-0.0122	-0.0124	-0.00691	-0.0341	-0.0478	-0.0425	-0.0340
	(0.0304)	(0.0232)	(0.0218)	(0.0234)	(0.0350)	(0.0333)	(0.0292)	(0.0233)
Latitude	-0.0257	0.0343	0.0341	0.00788	0.0364	0.0686	0.0666	0.0429
	(0.0452)	(0.0325)	(0.0323)	(0.0286)	(0.0471)	(0.0628)	(0.0576)	(0.0440)
Observations	264	282	285	288	264	282	285	288
Adj. R-Squared	0.425	0.472	0.451	0.473	0.380	0.345	0.337	0.366
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Cultural distance to Hong Kong is coded as in Section 1.3.3. Standard errors are clustered at the province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 1.3: IV Estimates of the Effects of SEZs on Output

Panel A: Effects on log(GDP)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.913***	0.337	0.949***	0.157	0.940***	0.532	0.997***	0.629
	(0.0875)	(1.268)	(0.0911)	(0.735)	(0.0908)	(0.797)	(0.0934)	(0.664)
log(Distance to HK)	-0.633***	-0.603***	-0.859***	-0.832***	-0.810***	-0.793***	-0.807***	-0.792***
	(0.0746)	(0.123)	(0.198)	(0.198)	(0.217)	(0.225)	(0.217)	(0.220)
log(Distance to Beijing)	-0.0686	-0.0226	0.0363	0.115	0.000179	0.0409	-0.168	-0.130
	(0.211)	(0.204)	(0.199)	(0.185)	(0.191)	(0.176)	(0.169)	(0.156)
Coastal	-0.212*	-0.0242	-0.247	0.0374	-0.272*	-0.126	-0.252*	-0.119
	(0.124)	(0.409)	(0.151)	(0.264)	(0.136)	(0.277)	(0.133)	(0.239)
Border	-0.614***	-0.666**	-0.375**	-0.480***	-0.359**	-0.413***	-0.290*	-0.339**
	(0.214)	(0.282)	(0.143)	(0.185)	(0.148)	(0.160)	(0.143)	(0.146)
Longitude	0.0119	0.0232	-0.0316	-0.0157	-0.0323	-0.0242	-0.0281	-0.0208
	(0.0251)	(0.0380)	(0.0187)	(0.0258)	(0.0196)	(0.0252)	(0.0205)	(0.0234)
Latitude	-0.0369	-0.0296	0.0195	0.0320	0.0203	0.0265	-0.00683	-0.00118
	(0.0394)	(0.0431)	(0.0320)	(0.0299)	(0.0326)	(0.0293)	(0.0262)	(0.0236)
Observations	264	264	282	282	285	285	288	288
Adj. R-Squared	0.579	0.518	0.622	0.517	0.608	0.579	0.639	0.616
First Stage F-stat		11.23		6.095		6.147		6.148
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(GDP Per Capita)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.662***	0.383	0.663***	0.738	0.538***	1.022	0.573***	1.041*
	(0.0715)	(0.822)	(0.0792)	(0.778)	(0.0612)	(0.711)	(0.0527)	(0.603)
log(Distance to HK)	-0.872**	-0.858***	-0.599*	-0.602**	-0.477	-0.496*	-0.464*	-0.482*
	(0.329)	(0.325)	(0.296)	(0.288)	(0.281)	(0.276)	(0.268)	(0.261)
log(Distance to Beijing)	0.142	0.164	0.314	0.307	0.219	0.171	0.0822	0.0336
	(0.319)	(0.287)	(0.300)	(0.283)	(0.262)	(0.259)	(0.215)	(0.215)
Coastal	0.193	0.284	0.0496	0.0226	0.0430	-0.131	0.0722	-0.0959
	(0.139)	(0.280)	(0.147)	(0.292)	(0.109)	(0.258)	(0.0991)	(0.222)
Border	-0.0708	-0.0960	-0.0906	-0.0806	-0.0596	0.00516	-0.00347	0.0596
	(0.132)	(0.126)	(0.138)	(0.142)	(0.147)	(0.159)	(0.139)	(0.148)
Longitude	-0.0478	-0.0423	-0.0627*	-0.0642*	-0.0554*	-0.0651**	-0.0476**	-0.0570**
	(0.0331)	(0.0328)	(0.0320)	(0.0338)	(0.0290)	(0.0318)	(0.0227)	(0.0255)
Latitude	0.0284	0.0320	0.0588	0.0576	0.0594	0.0521	0.0351	0.0279
	(0.0478)	(0.0437)	(0.0627)	(0.0569)	(0.0581)	(0.0539)	(0.0439)	(0.0409)
Observations	264	264	282	282	285	285	288	288
Adj. R-Squared	0.522	0.496	0.487	0.485	0.453	0.351	0.527	0.413
First Stage F-stat		11.23		6.095		6.147		6.148
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1994 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1994. IV results contain the output of instrumenting SEZ by 1994 with the cultural distance to Hong Kong. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 1.4: IV Estimates of the Effects of SEZs on Labor-Related Outcomes

Panel A: Effects on log(Population)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.250***	-0.147	0.264***	-0.311	0.286***	-0.363	0.293***	-0.445**
	(0.0897)	(0.604)	(0.0836)	(0.282)	(0.0874)	(0.222)	(0.0904)	(0.200)
Observations	264	264	283	283	285	285	290	290
Adj. R-Squared	0.364	0.317	0.356	0.258	0.357	0.233	0.410	0.256
First Stage F-stat		11.23		6.133		6.147		5.969
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(Employment)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.680***	0.300	0.794***	-0.0563	0.889***	0.167	0.871***	0.224
	(0.0974)	(1.356)	(0.0979)	(0.503)	(0.102)	(0.541)	(0.118)	(0.596)
Observations	264	264	283	283	285	285	288	288
Adj. R-Squared	0.411	0.369	0.507	0.329	0.537	0.423	0.547	0.464
First Stage F-stat		11.23		6.133		6.147		6.148
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(Total Wage)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.910***	-0.497	0.994***	-0.150	1.012***	0.158	1.050***	0.164
	(0.101)	(1.264)	(0.115)	(0.759)	(0.112)	(0.691)	(0.122)	(0.720)
Observations	264	264	283	283	285	285	288	288
Adj. R-Squared	0.498	0.0570	0.521	0.289	0.564	0.441	0.590	0.466
First Stage F-stat		11.23		6.133		6.147		6.148
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1994 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1994. IV results contain the output of instrumenting SEZ by 1994 with the cultural distance to Hong Kong. Population data contain locally registered people only and can therefore exclude seasonal migrant workers. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 1.5: IV Estimates of the Effects of SEZs on Capital-Related Outcomes

Panel A: Effects on log(Investment)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	1.074***	0.965	0.949***	0.419	0.921***	0.822	0.939***	0.759
	(0.110)	(1.210)	(0.114)	(0.937)	(0.0977)	(0.983)	(0.0891)	(0.607)
Observations	264	264	283	283	285	285	288	288
Adj. R-Squared	0.558	0.556	0.618	0.572	0.546	0.544	0.617	0.611
First Stage F-stat		11.23		6.133		6.147		6.148
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(Real Estate Investment)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	1.611***	1.445	1.468***	1.179	1.398***	1.345	1.432***	1.048
	(0.204)	(2.338)	(0.182)	(1.477)	(0.160)	(1.789)	(0.159)	(1.076)
Observations	263	263	283	283	285	285	287	287
Adj. R-Squared	0.528	0.526	0.558	0.551	0.551	0.551	0.601	0.587
First Stage F-stat		11.14		6.133		6.147		6.132
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(Residential Properties Investment)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	1.225***	0.512	1.452***	1.326	1.424***	1.909	1.422***	1.059
	(0.183)	(1.879)	(0.182)	(1.618)	(0.154)	(1.489)	(0.153)	(1.091)
Observations	263	263	282	282	284	284	287	287
Adj. R-Squared	0.438	0.383	0.546	0.545	0.560	0.535	0.594	0.581
First Stage F-stat		11.55		6.557		6.018		6.132
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1994 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1994. IV results contain the output of instrumenting SEZ by 1994 with the cultural distance to Hong Kong. Residential properties investment is a sub-category of real estate investment. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 1.6: IV Estimates of the Effects of SEZs on Productivity-Related Outcomes

Panel A: Effects on log(Industrial Output)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	1.137***	0.425	1.117***	0.144	0.956***	1.084	0.984***	1.392*
	(0.124)	(2.513)	(0.130)	(1.084)	(0.119)	(1.134)	(0.107)	(0.796)
Observations	263	263	283	283	283	283	287	287
Adj. R-Squared	0.551	0.499	0.608	0.522	0.624	0.623	0.684	0.667
First Stage F-stat		11.17		6.133		6.030		6.113
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(Output Per Labor)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.457***	0.124	0.323***	0.201	0.0655	0.918	0.0875	1.169***
	(0.101)	(1.208)	(0.0917)	(0.607)	(0.0803)	(0.599)	(0.0808)	(0.228)
Observations	263	263	283	283	283	283	287	287
Adj. R-Squared	0.512	0.488	0.473	0.469	0.414	0.196	0.502	0.146
First Stage F-stat		11.17		6.133		6.030		6.113
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(OLS Residual)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	-0.0594	0.732	-0.0719	0.639**	-0.0930	0.828***	-0.0400	0.709***
	(0.0716)	(0.579)	(0.0738)	(0.252)	(0.0635)	(0.136)	(0.0707)	(0.172)
Observations	263	263	283	283	283	283	284	284
Adj. R-Squared	0.407	-0.0503	0.335	-0.0450	0.425	-0.146	0.553	0.323
First Stage F-stat		11.17		6.133		6.030		6.236
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1994 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1994. IV results contain the output of instrumenting SEZ by 1994 with the cultural distance to Hong Kong. OLS residuals are computed by regressing the log value of total output on the log values of total wage and capital stock for each year at the prefecture level, and taking the residuals. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 1.7: IV Estimates of the Effects of SEZs on Foreign-Related Outcomes

Panel A: Effects on log(FDI)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	1.889***	1.429	2.141***	3.293***	1.719***	2.443***	1.833***	2.778
	(0.226)	(2.475)	(0.201)	(1.012)	(0.226)	(0.930)	(0.228)	(1.778)
Observations	257	257	271	271	273	273	266	266
Adj. R-Squared	0.644	0.637	0.631	0.580	0.622	0.594	0.596	0.563
First Stage F-stat		9.697		6.075		5.979		6.818
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(Foreign-Owned Output)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	1.867***	2.659	1.863***	0.629	1.767***	1.839	2.253***	3.666
	(0.249)	(4.218)	(0.225)	(2.526)	(0.213)	(2.607)	(0.189)	(2.243)
Observations	243	243	272	272	267	267	273	273
Adj. R-Squared	0.496	0.475	0.577	0.527	0.586	0.586	0.622	0.558
First Stage F-stat		11.54		6.178		6.476		5.955
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(HMT-Owned Output)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	1.874***	2.253	1.865***	3.552***	1.456***	3.018***	1.753***	2.957***
	(0.218)	(2.773)	(0.203)	(0.284)	(0.258)	(1.083)	(0.240)	(0.692)
Observations	249	249	263	263	267	267	275	275
Adj. R-Squared	0.604	0.599	0.611	0.523	0.610	0.521	0.592	0.543
First Stage F-stat		13.68		6.712		6.404		6.361
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1994 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1994. IV results contain the output of instrumenting SEZ by 1994 with the cultural distance to Hong Kong. Foreign- and HMT-owned output refer to total output from firms owned by foreign and Hong Kong, Macao and Taiwan owners, two mutually exclusive groups in China's economic statistics. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 1.8: IV Estimates of the Effects of SEZs on Marketization

Panel A: Effects on Private Share

	1998		2001		2004		2007	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.0784**	0.230	0.0532	0.257	-0.0163	0.172**	-0.0484**	0.143
	(0.0319)	(0.457)	(0.0360)	(0.383)	(0.0319)	(0.0764)	(0.0237)	(0.141)
Observations	291	291	301	301	319	319	323	323
Adj. R-Squared	0.552	0.502	0.418	0.329	0.337	0.238	0.307	0.182
First Stage F-stat		10.16		10.46		6.146		6.177
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on Private Capital Ratio

	1998		2001		2004		2007	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.101***	0.122	0.119***	0.194	-0.0159	0.0989	-0.0333	-0.222***
	(0.0298)	(0.489)	(0.0416)	(0.343)	(0.0303)	(0.0642)	(0.0225)	(0.0398)
Observations	291	291	301	301	319	319	323	323
Adj. R-Squared	0.507	0.506	0.364	0.350	0.320	0.278	0.205	0.0663
First Stage F-stat		10.16		10.46		6.146		6.177
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(No. of Private Entry)

	1998		2001		2004		2007	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.931***	-0.228	0.931***	0.372	0.882***	0.200	0.749***	0.248
	(0.110)	(1.685)	(0.0948)	(2.103)	(0.106)	(0.951)	(0.101)	(0.756)
Observations	291	291	302	302	319	319	323	323
Adj. R-Squared	0.692	0.571	0.705	0.679	0.735	0.701	0.751	0.731
First Stage F-stat		10.16		10.40		6.146		6.177
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1994 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1994. IV results contain the output of instrumenting SEZ by 1994 with the cultural distance to Hong Kong. Private share is computed by aggregating private-firm and state-firm revenues at the prefecture level and compute the share of private revenue to total revenue. Private capital ratio is computed by taking the ratio of private contributed capital (also called paid-in capital) to total capital, and then aggregate these ratios to the prefecture level using each firm's assets as weights. Number of private entry is computed as follows: define firm i as a private entrant in year t if year t is the first year that firm i appears in a given prefecture and the firm is private. Then sum the total number of private entrants in a given year by prefecture. For the year 1998 (the first year of the data), define a firm as an entrant if its registered age is equal to 1. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 1.9: IV Estimates of the Effects of SEZs on Night-Light

Panel A: Effects on log(Night-Light)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.690***	0.692	0.671***	0.737	0.623***	0.831**	0.604***	0.893*
	(0.138)	(0.481)	(0.127)	(0.466)	(0.120)	(0.418)	(0.124)	(0.540)
log(Distance to HK)	-1.153***	-1.153***	-1.096***	-1.098***	-1.068***	-1.077***	-0.956***	-0.968***
	(0.261)	(0.248)	(0.257)	(0.243)	(0.250)	(0.235)	(0.257)	(0.249)
log(Distance to Beijing)	-0.502	-0.502	-0.435	-0.441	-0.421	-0.441	-0.436	-0.464
	(0.395)	(0.367)	(0.348)	(0.324)	(0.351)	(0.327)	(0.336)	(0.316)
Coastal	0.383*	0.382*	0.307	0.285	0.275	0.206	0.347*	0.251
	(0.222)	(0.231)	(0.208)	(0.225)	(0.200)	(0.212)	(0.197)	(0.246)
Border	-1.066**	-1.066***	-0.897***	-0.899***	-0.848**	-0.855***	-0.876**	-0.886***
	(0.414)	(0.389)	(0.314)	(0.298)	(0.324)	(0.312)	(0.318)	(0.312)
Longitude	0.0263	0.0262	0.0135	0.0124	0.0173	0.0138	0.0139	0.00905
	(0.0384)	(0.0355)	(0.0353)	(0.0320)	(0.0348)	(0.0317)	(0.0360)	(0.0330)
Latitude	0.0248	0.0248	0.0243	0.0233	0.0297	0.0267	0.0121	0.00794
	(0.0894)	(0.0826)	(0.0842)	(0.0779)	(0.0882)	(0.0817)	(0.0775)	(0.0720)
Observations	359	359	360	360	360	360	360	360
Adj. R-Squared	0.618	0.618	0.631	0.631	0.624	0.621	0.610	0.603
First Stage F-stat		5.287		5.292		5.292		5.292
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(Normalized Night-Light)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.371**	1.039	0.376**	1.182**	0.323**	1.327***	0.315**	1.452**
	(0.152)	(0.726)	(0.146)	(0.508)	(0.145)	(0.498)	(0.134)	(0.603)
log(Distance to HK)	-1.219*	-1.256**	-1.032	-1.064*	-0.928	-0.971*	-0.831	-0.880
	(0.689)	(0.591)	(0.648)	(0.584)	(0.615)	(0.552)	(0.592)	(0.538)
log(Distance to Beijing)	-0.166	-0.234	-0.0437	-0.144	-0.0411	-0.168	0.0463	-0.129
	(0.609)	(0.601)	(0.479)	(0.464)	(0.456)	(0.450)	(0.449)	(0.442)
Coastal	0.777***	0.562	0.694**	0.408	0.656**	0.300	0.706***	0.298
	(0.276)	(0.409)	(0.262)	(0.347)	(0.256)	(0.338)	(0.249)	(0.348)
Border	0.0819	0.104	-0.187	-0.115	-0.132	-0.0429	-0.279	-0.149
	(0.203)	(0.246)	(0.332)	(0.338)	(0.329)	(0.344)	(0.323)	(0.329)
Longitude	-0.0979	-0.114*	-0.0716	-0.0923*	-0.0646	-0.0899*	-0.0674	-0.0924*
	(0.0820)	(0.0695)	(0.0629)	(0.0534)	(0.0580)	(0.0495)	(0.0581)	(0.0500)
Latitude	0.0288	0.0204	0.0224	0.00916	0.0159	0.0000972	0.0338	0.0107
	(0.113)	(0.108)	(0.0960)	(0.0870)	(0.0894)	(0.0810)	(0.0840)	(0.0749)
Observations	264	264	283	283	285	285	290	290
Adj. R-Squared	0.386	0.343	0.351	0.281	0.352	0.235	0.333	0.166
First Stage F-stat		12.29		6.503		6.518		6.306
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1994 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1994. IV results contain the output of instrumenting SEZ by 1994 with the cultural distance to Hong Kong. Night-light is obtained from the Prolonged Artificial Nighttime-light Dataset of China project and aggregated to the year level. Normalized night-light is computed by dividing the night-light data by registered population of the same year. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 1.10: IV Estimates of the Effects of SEZs on Output, Using 1991 as Cutoff Date

Panel A: Effects on log(GDP)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	0.715***	0.300	0.704***	0.165	0.674***	0.562	0.683***	0.664
	(0.138)	(0.984)	(0.150)	(0.708)	(0.136)	(0.620)	(0.122)	(0.443)
log(Distance to HK)	-0.553***	-0.572***	-0.784***	-0.817***	-0.732***	-0.739***	-0.726***	-0.727***
	(0.0877)	(0.0695)	(0.197)	(0.145)	(0.218)	(0.172)	(0.219)	(0.185)
log(Distance to	-0.0123	-0.00264	0.0961	0.122	0.0612	0.0667	-0.0975	-0.0966
Beijing)	(0.215)	(0.188)	(0.182)	(0.174)	(0.173)	(0.159)	(0.164)	(0.149)
Coastal	-0.249**	-0.0547	-0.256*	0.0118	-0.269**	-0.214	-0.232*	-0.223
	(0.0963)	(0.461)	(0.127)	(0.346)	(0.122)	(0.305)	(0.118)	(0.230)
Border	-0.723**	-0.708***	-0.508***	-0.502***	-0.492***	-0.491***	-0.431**	-0.431***
	(0.266)	(0.244)	(0.179)	(0.168)	(0.176)	(0.162)	(0.165)	(0.152)
Longitude	0.0172	0.0245	-0.0231	-0.0150	-0.0236	-0.0219	-0.0188	-0.0185
	(0.0287)	(0.0327)	(0.0205)	(0.0233)	(0.0203)	(0.0204)	(0.0228)	(0.0207)
Latitude	-0.0151	-0.0210	0.0411	0.0360	0.0410	0.0399	0.0154	0.0152
	(0.0438)	(0.0419)	(0.0285)	(0.0308)	(0.0301)	(0.0303)	(0.0282)	(0.0280)
Observations	264	264	282	282	285	285	288	288
Adj. R-Squared	0.470	0.455	0.511	0.488	0.489	0.488	0.509	0.509
First Stage F-stat		9.764		105.2		106.2		104.6
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(GDP Per Capita)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	0.600***	0.341	0.590***	0.776	0.455***	1.079***	0.452***	1.101***
	(0.141)	(0.592)	(0.156)	(0.517)	(0.113)	(0.335)	(0.0914)	(0.218)
log(Distance to HK)	-0.811**	-0.823***	-0.541*	-0.530**	-0.428	-0.391	-0.414	-0.375
	(0.338)	(0.307)	(0.298)	(0.258)	(0.283)	(0.254)	(0.271)	(0.250)
log(Distance to	0.181	0.187	0.351	0.342	0.251	0.220	0.120	0.0886
Beijing)	(0.286)	(0.262)	(0.275)	(0.250)	(0.244)	(0.219)	(0.200)	(0.180)
Coastal	0.128	0.249	-0.00532	-0.0975	0.0104	-0.299	0.0543	-0.267*
	(0.174)	(0.288)	(0.209)	(0.284)	(0.166)	(0.203)	(0.155)	(0.155)
Border	-0.153	-0.143	-0.185	-0.187	-0.137	-0.143	-0.0854	-0.0921
	(0.148)	(0.137)	(0.134)	(0.131)	(0.141)	(0.152)	(0.129)	(0.140)
Longitude	-0.0454	-0.0408	-0.0582*	-0.0610**	-0.0515*	-0.0608***	-0.0432*	-0.0532***
	(0.0305)	(0.0290)	(0.0298)	(0.0271)	(0.0272)	(0.0228)	(0.0211)	(0.0169)
Latitude	0.0454	0.0417	0.0748	0.0765	0.0719	0.0778	0.0484	0.0550
	(0.0433)	(0.0422)	(0.0585)	(0.0544)	(0.0555)	(0.0498)	(0.0420)	(0.0375)
Observations	264	264	282	282	285	285	288	288
Adj. R-Squared	0.435	0.425	0.397	0.391	0.370	0.290	0.407	0.301
First Stage F-stat		9.764		105.2		106.2		104.6
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1991 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1991. IV results contain the output of instrumenting SEZ by 1991 with the cultural distance to Hong Kong. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 1.11: IV Estimates of the Effects of SEZs on Output, Using 1985 as Cutoff Date

Panel A: Effects on log(GDP)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	0.837***	0.313	0.928***	0.272	0.880***	0.923	0.839***	1.089***
	(0.163)	(1.019)	(0.174)	(1.043)	(0.175)	(0.607)	(0.176)	(0.281)
log(Distance to HK)	-0.340***	-0.494*	-0.565***	-0.750***	-0.526***	-0.514***	-0.532***	-0.462***
	(0.0905)	(0.276)	(0.129)	(0.188)	(0.147)	(0.104)	(0.153)	(0.130)
log(Distance to Beijing)	-0.0852	-0.0291	0.0381	0.103	0.00669	0.00237	-0.148	-0.172
	(0.200)	(0.207)	(0.176)	(0.193)	(0.162)	(0.153)	(0.159)	(0.138)
Coastal	-0.272**	-0.0479	-0.301**	-0.0219	-0.309**	-0.327	-0.250**	-0.357**
	(0.109)	(0.438)	(0.135)	(0.437)	(0.119)	(0.266)	(0.122)	(0.148)
Border	-0.668**	-0.686**	-0.498**	-0.500***	-0.482**	-0.482***	-0.423**	-0.423***
	(0.284)	(0.267)	(0.189)	(0.171)	(0.183)	(0.168)	(0.173)	(0.160)
Longitude	0.0302	0.0300	-0.0104	-0.0119	-0.0115	-0.0114	-0.00630	-0.00573
	(0.0284)	(0.0270)	(0.0203)	(0.0205)	(0.0191)	(0.0177)	(0.0214)	(0.0196)
Latitude	-0.0352	-0.0290	0.0238	0.0313	0.0246	0.0241	-0.000976	-0.00378
	(0.0436)	(0.0430)	(0.0292)	(0.0292)	(0.0296)	(0.0256)	(0.0266)	(0.0228)
Observations	264	264	282	282	285	285	288	288
Adj. R-Squared	0.474	0.455	0.524	0.498	0.500	0.500	0.513	0.509
First Stage F-stat		8.095		3.196		3.218		3.224
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(GDP Per Capita)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	0.685***	0.355	0.592***	1.278***	0.399***	1.772***	0.373***	1.804***
	(0.144)	(0.609)	(0.140)	(0.361)	(0.123)	(0.387)	(0.108)	(0.558)
log(Distance to HK)	-0.637**	-0.734**	-0.410	-0.217	-0.343	0.0409	-0.336	0.0639
	(0.296)	(0.318)	(0.250)	(0.238)	(0.243)	(0.271)	(0.236)	(0.305)
log(Distance to Beijing)	0.121	0.157	0.321	0.253	0.233	0.0968	0.105	-0.0369
	(0.263)	(0.264)	(0.276)	(0.239)	(0.244)	(0.207)	(0.203)	(0.180)
Coastal	0.116	0.257	0.0358	-0.256	0.0666	-0.518***	0.120	-0.489**
	(0.172)	(0.272)	(0.212)	(0.189)	(0.168)	(0.166)	(0.154)	(0.219)
Border	-0.107	-0.118	-0.177	-0.176	-0.131	-0.127	-0.0803	-0.0786
	(0.155)	(0.128)	(0.123)	(0.118)	(0.132)	(0.134)	(0.120)	(0.124)
Longitude	-0.0345	-0.0346	-0.0480	-0.0465	-0.0437	-0.0406	-0.0353	-0.0320
	(0.0317)	(0.0303)	(0.0320)	(0.0287)	(0.0284)	(0.0251)	(0.0226)	(0.0197)
Latitude	0.0288	0.0326	0.0625	0.0547	0.0631	0.0474	0.0397	0.0236
	(0.0438)	(0.0417)	(0.0613)	(0.0531)	(0.0569)	(0.0506)	(0.0433)	(0.0399)
Observations	264	264	282	282	285	285	288	288
Adj. R-Squared	0.437	0.424	0.383	0.327	0.352	0.0588	0.381	0.00838
First Stage F-stat		8.095		3.196		3.218		3.224
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1985 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1985. IV results contain the output of instrumenting SEZ by 1985 with the cultural distance to Hong Kong. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 1.12: IV Estimates of the Effects of SEZs on Output with Additional Controls

Panel A: Effects on log(GDP)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.791***	0.174	0.810***	0.137	0.797***	0.510	0.875***	0.358
	(0.0862)	(0.651)	(0.0866)	(0.826)	(0.0892)	(0.935)	(0.0891)	(0.706)
log(Distance to HK)	-0.617**	-0.589**	-0.846***	-0.816***	-0.839**	-0.826***	-0.873**	-0.850***
	(0.287)	(0.281)	(0.301)	(0.303)	(0.305)	(0.301)	(0.317)	(0.309)
log(Distance to Beijing)	-0.0922	-0.0619	0.0354	0.0630	-0.0414	-0.0296	-0.291	-0.270
	(0.311)	(0.236)	(0.296)	(0.253)	(0.287)	(0.249)	(0.256)	(0.216)
Coastal	0.00223	0.174	-0.0517	0.137	-0.0957	-0.0149	-0.0528	0.0924
	(0.189)	(0.249)	(0.224)	(0.302)	(0.205)	(0.299)	(0.208)	(0.272)
Border	-0.749***	-0.770***	-0.753***	-0.775***	-0.757***	-0.767***	-0.624***	-0.641***
	(0.214)	(0.236)	(0.223)	(0.261)	(0.215)	(0.206)	(0.138)	(0.146)
Longitude	0.00704	0.0306	-0.00661	0.0193	-0.00727	0.00380	-0.00734	0.0126
	(0.0284)	(0.0331)	(0.0285)	(0.0402)	(0.0291)	(0.0405)	(0.0252)	(0.0324)
Latitude	-0.0504	-0.0348	0.00409	0.0218	0.00190	0.00947	-0.0349	-0.0213
	(0.0624)	(0.0540)	(0.0727)	(0.0667)	(0.0746)	(0.0679)	(0.0625)	(0.0554)
log(Population in 1982)	0.314***	0.393***	0.293***	0.380**	0.281***	0.318**	0.320***	0.387***
	(0.0956)	(0.142)	(0.0905)	(0.154)	(0.0846)	(0.150)	(0.0901)	(0.138)
log(Initial Land Area)	0.107	0.0783	0.0589	0.0294	0.0666	0.0540	0.0778	0.0551
	(0.0909)	(0.0966)	(0.0986)	(0.102)	(0.0975)	(0.101)	(0.103)	(0.101)
Observations	184	184	185	185	185	185	185	185
Adj. R-Squared	0.713	0.639	0.688	0.597	0.667	0.649	0.724	0.673
First Stage F-stat		2.726		2.771		2.771		2.771
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(GDP Per Capita)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.608***	0.425	0.600***	1.158	0.473***	1.456	0.535***	1.241
	(0.0822)	(1.008)	(0.0734)	(1.592)	(0.0713)	(1.507)	(0.0652)	(1.330)
log(Distance to HK)	-0.708***	-0.700***	-0.501***	-0.526***	-0.439**	-0.483***	-0.461***	-0.493***
	(0.235)	(0.238)	(0.170)	(0.190)	(0.162)	(0.177)	(0.162)	(0.172)
log(Distance to Beijing)	0.227	0.236	0.356	0.333	0.212	0.172	0.0286	-0.000335
	(0.348)	(0.299)	(0.334)	(0.331)	(0.292)	(0.325)	(0.224)	(0.252)
Coastal	0.0223	0.0731	-0.121	-0.278	-0.105	-0.381	-0.0456	-0.244
	(0.165)	(0.273)	(0.191)	(0.447)	(0.145)	(0.426)	(0.143)	(0.376)
Border	-0.248	-0.254*	-0.326**	-0.307*	-0.318**	-0.286	-0.229***	-0.206
	(0.157)	(0.147)	(0.152)	(0.172)	(0.141)	(0.225)	(0.0806)	(0.181)
Longitude	-0.0395	-0.0325	-0.0327	-0.0542	-0.0318	-0.0696	-0.0254	-0.0526
	(0.0327)	(0.0500)	(0.0330)	(0.0692)	(0.0313)	(0.0669)	(0.0227)	(0.0555)
Latitude	0.0570	0.0616	0.0779	0.0632	0.0690	0.0432	0.0435	0.0249
	(0.0570)	(0.0573)	(0.0664)	(0.0792)	(0.0640)	(0.0829)	(0.0479)	(0.0638)
log(Population in 1982)	-0.0290	-0.00540	-0.0666	-0.138	-0.0835	-0.210	-0.0569	-0.148
	(0.0597)	(0.148)	(0.0631)	(0.220)	(0.0554)	(0.222)	(0.0391)	(0.185)
log(Initial Land Area)	-0.282***	-0.291***	-0.266***	-0.242**	-0.200**	-0.157	-0.158**	-0.127
	(0.0691)	(0.0869)	(0.0818)	(0.121)	(0.0767)	(0.134)	(0.0606)	(0.107)
Observations	184	184	185	185	185	185	185	185
Adj. R-Squared	0.622	0.610	0.576	0.443	0.494	-0.134	0.574	0.225
First Stage F-stat		2.726		2.771		2.771		2.771
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1994 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1994. IV results contain the output of instrumenting SEZ by 1994 with the cultural distance to Hong Kong. Initial land area is the first available data on land area for the given prefecture. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 1.13: IV Estimates of the Effects of SEZs on Output with Alternative Years

Panel A: Effects on log(GDP)

	2003		2008		2013		2018	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.908***	0.150	0.919***	0.323	0.942***	0.604	1.024***	0.526
	(0.0953)	(0.712)	(0.0898)	(0.728)	(0.0923)	(0.670)	(0.105)	(0.643)
log(Distance to HK)	-0.686***	-0.647***	-0.843***	-0.819***	-0.814***	-0.800***	-0.846***	-0.825***
	(0.148)	(0.168)	(0.208)	(0.213)	(0.204)	(0.211)	(0.234)	(0.236)
log(Distance to Beijing)	-0.0428	-0.0202	-0.00478	0.0695	-0.0848	-0.0404	-0.161	-0.0842
	(0.272)	(0.243)	(0.206)	(0.192)	(0.205)	(0.188)	(0.184)	(0.178)
Coastal	-0.217	0.0509	-0.238	-0.0274	-0.257*	-0.137	-0.219	-0.0404
	(0.143)	(0.251)	(0.141)	(0.259)	(0.136)	(0.232)	(0.131)	(0.238)
Border	-0.415*	-0.496**	-0.422**	-0.475***	-0.361**	-0.392***	-0.327**	-0.384***
	(0.216)	(0.217)	(0.164)	(0.164)	(0.158)	(0.145)	(0.139)	(0.128)
Longitude	-0.0186	-0.00286	-0.0347*	-0.0194	-0.0358	-0.0274	-0.0425**	-0.0315
	(0.0257)	(0.0290)	(0.0200)	(0.0277)	(0.0214)	(0.0262)	(0.0199)	(0.0231)
Latitude	-0.0139	-0.0149	0.0256	0.0354	0.0145	0.0202	0.00108	0.0115
	(0.0443)	(0.0371)	(0.0325)	(0.0297)	(0.0300)	(0.0271)	(0.0273)	(0.0249)
Observations	280	280	283	283	287	287	289	289
Adj. R-Squared	0.594	0.500	0.609	0.550	0.600	0.580	0.640	0.601
First Stage F-stat		6.275		6.503		6.479		6.297
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(GDP Per Capita)

	2003		2008		2013		2018	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.672***	0.925	0.604***	0.934	0.649***	1.414	0.569***	0.912
	(0.0886)	(0.880)	(0.0863)	(0.733)	(0.0899)	(1.035)	(0.0618)	(0.560)
log(Distance to HK)	-0.767**	-0.780**	-0.581*	-0.594**	-0.793**	-0.826**	-0.482*	-0.495*
	(0.316)	(0.318)	(0.311)	(0.303)	(0.366)	(0.364)	(0.277)	(0.269)
log(Distance to Beijing)	0.346	0.339	0.254	0.212	0.205	0.105	0.0633	0.0114
	(0.355)	(0.337)	(0.301)	(0.293)	(0.312)	(0.330)	(0.225)	(0.226)
Coastal	0.122	0.0325	0.0514	-0.0657	0.0877	-0.184	0.0857	-0.0396
	(0.138)	(0.306)	(0.144)	(0.274)	(0.142)	(0.355)	(0.104)	(0.217)
Border	-0.0793	-0.0521	-0.116	-0.0867	-0.0239	0.0465	-0.000200	0.0378
	(0.135)	(0.131)	(0.138)	(0.134)	(0.183)	(0.194)	(0.133)	(0.133)
Longitude	-0.0506*	-0.0559*	-0.0600*	-0.0685**	-0.0683**	-0.0872**	-0.0555**	-0.0629***
	(0.0264)	(0.0309)	(0.0312)	(0.0341)	(0.0307)	(0.0391)	(0.0228)	(0.0243)
Latitude	0.0667	0.0671	0.0624	0.0570	0.0726	0.0596	0.0365	0.0291
	(0.0596)	(0.0546)	(0.0644)	(0.0584)	(0.0609)	(0.0577)	(0.0455)	(0.0413)
Observations	280	280	283	283	287	287	288	288
Adj. R-Squared	0.531	0.511	0.445	0.408	0.468	0.289	0.503	0.443
First Stage F-stat		6.275		6.503		6.479		6.450
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1994 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1994. IV results contain the output of instrumenting SEZ by 1994 with the cultural distance to Hong Kong. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 1.14: IV Estimates of the Effects of SEZs on Output, Remove Prefectures with Max Cultural Distance

Panel A: Effects on log(GDP)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.897***	0.116	0.926***	0.0760	0.936***	0.458	1.010***	0.509
	(0.101)	(1.290)	(0.111)	(0.720)	(0.111)	(0.769)	(0.112)	(0.624)
log(Distance to HK)	-0.601***	-0.554***	-0.854***	-0.819***	-0.817***	-0.795***	-0.816***	-0.793***
	(0.0983)	(0.161)	(0.211)	(0.215)	(0.222)	(0.232)	(0.220)	(0.224)
log(Distance to Beijing)	-0.146	-0.0856	0.104	0.201	0.00150	0.0561	-0.231	-0.171
	(0.342)	(0.277)	(0.286)	(0.247)	(0.302)	(0.257)	(0.301)	(0.246)
Coastal	-0.200	0.0453	-0.229	0.0692	-0.259*	-0.0910	-0.242*	-0.0664
	(0.124)	(0.406)	(0.151)	(0.253)	(0.138)	(0.262)	(0.135)	(0.223)
Border	-0.886***	-0.948***	-0.507***	-0.602***	-0.436**	-0.489***	-0.363*	-0.421***
	(0.0984)	(0.157)	(0.174)	(0.168)	(0.202)	(0.180)	(0.181)	(0.160)
Longitude	-0.00396	0.0176	-0.0448*	-0.0230	-0.0476*	-0.0355	-0.0504**	-0.0378
	(0.0310)	(0.0513)	(0.0253)	(0.0334)	(0.0252)	(0.0316)	(0.0241)	(0.0277)
Latitude	-0.0758	-0.0768*	0.0166	0.0216	0.0164	0.0189	-0.0236	-0.0208
	(0.0537)	(0.0461)	(0.0479)	(0.0430)	(0.0491)	(0.0427)	(0.0421)	(0.0358)
Observations	237	237	253	253	256	256	259	259
Adj. R-Squared	0.581	0.480	0.622	0.516	0.612	0.576	0.649	0.612
First Stage F-stat		12.01		6.526		6.562		6.566
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(GDP Per Capita)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1994	0.642***	0.308	0.640***	0.796	0.519***	1.066	0.571***	1.046*
	(0.0990)	(0.807)	(0.102)	(0.770)	(0.0816)	(0.691)	(0.0635)	(0.575)
log(Distance to HK)	-0.913***	-0.893***	-0.641**	-0.648**	-0.528**	-0.553**	-0.514**	-0.536**
	(0.303)	(0.304)	(0.272)	(0.268)	(0.249)	(0.246)	(0.236)	(0.231)
log(Distance to Beijing)	0.241	0.267	0.492	0.474	0.319	0.256	0.130	0.0729
	(0.444)	(0.386)	(0.397)	(0.374)	(0.351)	(0.345)	(0.291)	(0.290)
Coastal	0.202	0.307	0.0581	0.00328	0.0523	-0.139	0.0770	-0.0895
	(0.140)	(0.268)	(0.148)	(0.280)	(0.109)	(0.246)	(0.1000)	(0.209)
Border	-0.143	-0.170	-0.0598	-0.0424	0.00180	0.0631	0.0565	0.111
	(0.172)	(0.144)	(0.168)	(0.151)	(0.167)	(0.159)	(0.144)	(0.136)
Longitude	-0.0495	-0.0403	-0.0532	-0.0572	-0.0503	-0.0641*	-0.0484*	-0.0604**
	(0.0431)	(0.0421)	(0.0321)	(0.0362)	(0.0305)	(0.0336)	(0.0257)	(0.0276)
Latitude	0.0362	0.0358	0.0929	0.0920	0.0869	0.0841	0.0511	0.0485
	(0.0631)	(0.0557)	(0.0688)	(0.0621)	(0.0652)	(0.0595)	(0.0500)	(0.0459)
Observations	237	237	253	253	256	256	259	259
Adj. R-Squared	0.515	0.484	0.484	0.477	0.451	0.329	0.536	0.423
First Stage F-stat		12.01		6.526		6.562		6.566
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1994 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1994. IV results contain the output of instrumenting SEZ by 1994 with the cultural distance to Hong Kong. Prefectures with a cultural distance of 3 (the maximum value) are removed from the sample. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Appendices

A.1 Additional Notes

A.1.1 List of Special Economic Zones and Relevant Policies

Below is the list of all prefecture-level (by 2019 classification) SEZs considered in this study, organized by year of establishment, along with the relevant policies that announce their creation. Lists within each year are not in any particular order.

1. 1980 (4 in total)
    - Special Economic Zones (*Jingji Tequ*): Shenzhen, Shantou, Zhuhai, Xiamen.
2. 1984 (14 in total)
    - Coastal Open Cities (*Yanhai Kaifang Chengshi*): Dalian, Qinhuangdao, Tianjin, Yantai, Qingdao, Lianyungang, Nantong, Shanghai, Ningbo, Wenzhou, Fuzhou, Guangzhou, Zhanjiang, and Beihai.
3. 1985 (12 in total)
    - Minnan-Triangle Open Areas (*Minnan Sanjiao Jingji Kaifangqu*): Zhangzhou, Quanzhou;
    - Pearl River Delta Open Areas (*Zhusanjiao Jingji Kaifangqu*): Jiangmen, Zhongshan, Dongguan, Foshan;
    - Yangtze River Delta Open Areas (*Changsanjiao Jingji Kaifangqu*): Suzhou, Changzhou, Wuxi, Huzhou, Jiaxing;
    - Autonomy for Self-Governed Ethnic Minority Regions (*Zizhi Tiaoli*): Yanbian.
4. 1988 (38 in total)
    - Enlargement of Coastal Open Cities, Hebei (*Kuoda Yanhai Kaifang Chengshi, Hebei*): Cangzhou, Tangshan;

- Enlargement of Coastal Open Cities, Liaoning (*Kuoda Yanhai Kaifang Chengshi, Liaoning*): Dandong, Huludao, Jinzhou, Liaoyang, Panjin, Shenyang, Anshan, Yingkou;
- Enlargement of Coastal Open Cities, Shandong (*Kuoda Yanhai Kaifang Chengshi, Shandong*): Zibo, Rizhao, Weifang, Weihai;
- Enlargement of Coastal Open Cities, Jiangsu (*Kuoda Yanhai Kaifang Chengshi, Jiangsu*): Yangzhou, Zhenjiang, Taizhou, Nanjing, Yancheng;
- Enlargement of Coastal Open Cities, Zhejiang (*Kuoda Yanhai Kaifang Chengshi, Zhejiang*): Hangzhou, Zhoushan, Shaoxing;
- Enlargement of Coastal Open Cities, Fujian (*Kuoda Yanhai Kaifang Chengshi, Fujian*): Putian, Ningde;
- Enlargement of Coastal Open Cities, Guangdong (*Kuoda Yanhai Kaifang Chengshi, Guangdong*): Yangjiang, Huizhou, Zhaoqing, Jieyang, Maoming, Qingyuan, Chaozhou, Shanwei;
- Enlargement of Coastal Open Cities, Guangxi (*Kuoda Yanhai Kaifang Chengshi, Guangxi*): Qinzhou, Wuzhou, Yulin, Fangchenggang;
- Special Economic Zones (*Jingji Tequ*): Haikou, Sanya.

5. 1990 (1 in total)
    - Enlargement of Coastal Open Cities, Shandong (*Kuoda Yanhai Kaifang Chengshi, Shandong*): Jinan.
6. 1991 (1 in total)
    - Enlargement of Coastal Open Cities, Guangxi (*Kuoda Yanhai Kaifang Chengshi, Guangxi*): Guilin.
7. 1992 (33 in total)
    - Yangtze River Cities (*Changjiang Yanan Chengshi*): Wuhu, Jiujiang, Wuhan, Yueyang, Chongqing

- Provincial Capital Cities (*Shenghui Chengshi*): Hefei, Lanzhou, Guiyang, Shijiazhuang, Changsha, Zhengzhou, Changchun, Hohhot, Nanchang, Yinchuan, Xining, Xi'an, Taiyuan, Chengdu, Harbin, Ürümqi, Beijing;
- Northern Border Open Areas (*Beibu Kaifang Chengshi*): Heihe, Hulunbuir, Xilingol;
- Southern Border Open Areas (*Nanbu Kaifang Chengshi*): Nanning, Kunming, Dehong Dai and Jingpo Autonomous Prefecture;
- Western Border Open Areas (*Xibu Kaifang Chengshi*): Tacheng, Ili Kazakh Autonomous Prefecture;
- Enlargement of Coastal Open Cities, Guangdong (*Kuoda Yanhai Kaifang Chengshi, Guangdong*): Heyuan, Meizhou, Shaoguan.

8. 1993 (5 in total)

- Enlargement of Coastal Open Cities, Shandong (*Kuoda Yanhai Kaifang Chengshi, Shandong*): Dongying;
- Enlargement of Coastal Open Cities, Fujian (*Kuoda Yanhai Kaifang Chengshi, Fujian*): Sanming, Longyan, Nanping;
- Yangtze River Cities (*Changjiang Yanan Chengshi*): Huangshi.

9. 1994 (2 in total)

- Yangtze River Sanxia Economic Areas (*Changjiang Sanxia Jingji Kaifangqu*): Enshi, Yichang.

A.1.2 Matching Hong Kong dialects with Language Atlas of China

The dialects spoken in Hong Kong, as recorded in the 1961 Census, are presented in Table 1.1. Each dialect is matched with a corresponding grouping in the Atlas as follows:

1. Cantonese: Almost synonymous with the Yue language group (supergroup), it is matched with the Yue group. For the subgroup, Cantonese is matched with the Guangfu subgroup, the dialect spoken predominantly in Guangzhou (Canton).

2. Hakka: A well-defined group (supergroup) in the Atlas with the same name. The Hakka spoken in Hong Kong is matched with the Yuetai subgroup, which is geographically closest to Hong Kong.

3. Hoklo: Refers to the Minnan dialect group under the Min supergroup. It is matched with the Chaoshan subgroup, the geographically closest subgroup to Hong Kong.

4. Sez Yap: Directly matches with the Siyi subgroup under the Yue group (supergroup).

5. Shanghai: Although it does not match any group in particular, the Shanghai dialect spoken in Hong Kong is matched with the Taihu subgroup under the Wu group (supergroup), as the whole of Shanghai city speaks this subgroup dialect.

6. Kuo Yu: Literally means the national language, i.e., Mandarin. Given that there are six groups within Mandarin, each with numerous subgroups, it is not matched to avoid unintentional bias. Hence, it is left with only the Mandarin supergroup.

A.1.3 Matching Quality of Prefecture-Level Data

The Language Atlas of China dataset contains 343 unique prefecture-level observations with no missing data for the 1980s. When matched with the 1982 population census data, certain prefecture-level administrative units are merged to align with administrative units in more modern statistical yearbooks. This merging process particularly applies to places that share the same name but are distinguished by an urban suffix (city, or *shi*) and a more suburban/rural suffix (region, or *diqu*).

The 1982 population census data include 272 unique prefecture-level units. After merging the two datasets to compute prefecture-level cultural distances, I obtain 244 unique prefectures across 30 provinces. The remaining units are lost due to changes in administrative borders, administrative rankings, and missing data. Additionally, five more prefectures are excluded when cultural distances are matched to the panels of economic data. Consequently, the final dataset comprises 239 prefectures, which form the basis of the regression analysis against a total of 297 prefectures in the panel data.

For prefectures with missing values in cultural distance, I use the within-province average. Provinces that have no data on dialects at all are excluded from the analysis.

A.2 Additional Figures

A.2.1 Additional Figures for Data Construction

Figure A.1: Population by Dialect, 1961, Hong Kong

131 USUAL LANGUAGE BY SEXES AND QUINQUENNIAL AGE GROUPS.

AGE GROUPS	SEX	ENGLISH	CANTONESE	HAKKA	HOKLO	SZE YAP	SHANGHAI	KUO YU	ALL OTHER E. ASIAN LANGUAGE	PORTUGUESE	ANY OTHER LANGUAGE	TOTAL
5 - 9	M	1,585	181,511	10,536	13,733	6,727	3,777	1,464	388	33	324	220,078
	F	1,629	170,691	9,376	11,275	6,078	3,404	1,394	301	31	320	204,499
	T	3,214	352,202	19,912	25,008	12,805	7,181	2,858	689	64	644	**424,577**
10 - 14	M	1,142	149,281	8,277	13,008	7,180	4,619	1,487	312	36	363	185,705
	F	1,251	134,902	7,064	9,826	6,399	3,706	1,279	231	34	268	164,960
	T	2,393	284,183	15,341	22,834	13,579	8,325	2,766	543	70	631	**350,665**
15 - 19	M	1,037	72,479	3,590	6,014	2,855	2,679	805	506	20	215	90,200
	F	645	60,550	2,992	4,646	2,531	2,276	757	181	32	212	74,822
	T	1,682	133,029	6,582	10,660	5,386	4,955	1,562	687	52	427	**165,022**
20 - 24	M	4,492	87,070	4,988	6,516	4,236	1,874	621	1,008	108	294	111,207
	F	1,429	72,721	4,118	5,250	4,515	1,992	652	477	43	263	91,460
	T	5,921	159,791	9,106	11,766	8,751	3,866	1,273	1,485	151	557	**202,667**
25 - 29	M	2,142	111,999	6,002	7,069	5,562	2,378	804	782	45	395	137,178
	F	1,608	93,498	5,261	6,700	5,351	2,678	815	463	33	332	116,739
	T	3,750	205,497	11,263	13,769	10,913	5,056	1,619	1,245	78	727	**253,917**
30 - 34	M	1,924	111,393	6,653	8,991	5,771	3,383	1,441	780	45	463	140,844
	F	1,608	93,691	5,772	8,831	6,024	4,219	1,328	346	31	364	122,214
	T	3,532	205,084	12,425	17,822	11,795	7,602	2,769	1,126	76	827	**.263,058**
35 - 39	M	1,627	94,886	6,449	8,984	4,815	3,833	1,864	452	29	368	123,307
	F	1,497	84,602	5,430	8,585	5,074	4,281	1,508	250	25	350	111,602
	T	3,124	179,488	11,879	17,569	9,889	8,114	3,372	702	54	718	**234,909**
40 - 44	M	1,338	82,428	5,807	7,317	3,819	4,029	1,869	293	21	377	107,298
	F	1,140	73,141	4,732	6,747	4,627	3,611	1,185	177	33	300	95,693
	T	2,478	155,569	10,539	14,064	8,446	7,640	3,054	470	54	677	**202,991**
45 - 49	M	1,180	65,921	5,067	5,533	3,020	3,526	1,700	228	31	303	86,509
	F	833	62,339	4,094	4,887	4,606	2,609	855	192	26	260	80,701
	T	2,013	128,260	9,161	10,420	7,626	6,135	2,555	420	57	563	**167,210**
50 - 54	M	792	45,693	3,879	3,552	1,925	2,650	1,144	175	37	220	60,067
	F	587	49,084	3,424	3,542	4,445	1,799	524	121	37	225	63,788
	T	1,379	94,777	7,303	7,094	6,370	4,449	1,668	296	74	445	**123,855**
	M	531	27,991	2,497	2,206	1,281	1,689	739	107	19	150	37,210

Note: This is an excerpt from the Hong Kong 1961 Census Report, Volume II, containing the number of people speaking a particular language/dialect by age and gender groups.

A.3 Additional Tables

A.3.1 Additional Tables for IV Estimations

Table A.1: First Stage Results, by Cutoff Date

	SEZ by 1985		SEZ by 1991		SEZ by 1994	
	(1)	(2)	(3)	(4)	(5)	(6)
Cultural Distance to HK	-0.203***	-0.141**	-0.227**	-0.222***	-0.197	-0.234**
	(0.0225)	(0.0647)	(0.101)	(0.0212)	(0.172)	(0.104)
log(Distance to HK)		-0.204***		0.0223		0.117**
		(0.0684)		(0.0560)		(0.0569)
log(Distance to Beijing)		0.113*		0.0863		0.0577
		(0.0586)		(0.0949)		(0.119)
Coastal		0.367***		0.452***		0.333***
		(0.0662)		(0.0700)		(0.0763)
Border		0.0250		0.0290		0.0110
		(0.0307)		(0.0412)		(0.0757)
Longitude		0.00172		0.0133*		0.0157
		(0.00537)		(0.00740)		(0.0101)
Latitude		0.00616		-0.00714		0.0122
		(0.00542)		(0.00789)		(0.00871)
Observations	362	360	362	360	362	360
Adj. R-Squared	0.294	0.446	0.518	0.625	0.343	0.384
Controls	No	Yes	No	Yes	No	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZs by year show the accumulative totals of SEZs by the end of 1985, 1991, and 1994. Cultural distance to Hong Kong is coded as in Section 1.3.3. Standard errors are clustered at the province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table A.2: Effect of Cultural Distance on Pre- and Post-Reform Outcomes, Provincial Data

	log(GDP)		log(GDP Per Capita)	
	(1) Pre-1976	(2) Post-1995	(3) Pre-1976	(4) Post-1995
Cultural Distance to HK	0.145	-0.671	0.0175	-0.223
	(0.902)	(0.844)	(0.536)	(0.427)
log(Distance to HK)	-0.390	-0.519	0.122	-0.0725
	(0.675)	(0.630)	(0.450)	(0.312)
log(Distance to Beijing)	-0.101	-0.367**	0.133	-0.0345
	(0.161)	(0.175)	(0.291)	(0.163)
log(Distance to Coastline)	-0.169	-0.0128	-0.394**	-0.353***
	(0.151)	(0.139)	(0.181)	(0.0781)
Border	0.173	0.289	-0.117	-0.0519
	(0.383)	(0.308)	(0.134)	(0.152)
Longitude	0.00365	-0.000443	-0.00859	-0.00406
	(0.0284)	(0.0292)	(0.0162)	(0.0105)
Latitude	0.105*	0.123**	0.0428	0.0692**
	(0.0590)	(0.0545)	(0.0351)	(0.0289)
Observations	700	750	699	750
Adj. R-Squared	0.691	0.859	0.619	0.926
Region FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Note: Cultural distance to Hong Kong is computed by taking the population-weighted average cultural distance at the prefecture level. Pre-1976 refers to coefficients from running the regression on the period 1952 to 1976, while post 1995 refers to the period 1995 to 2019. Region and time fixed effects are included. Standard errors are clustered at the province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

A.3.2 Additional Tables for Robustness Checks-Alternative Measure of Output

Table A.3: Reduced Form Results on Night-Light

	log(Night-Light)				log(Normalized Night-Light)			
	(1) 2001	(2) 2006	(3) 2011	(4) 2016	(5) 2001	(6) 2006	(7) 2011	(8) 2016
Cultural Distance to HK	-0.165**	-0.175**	-0.198***	-0.212***	-0.166	-0.280*	-0.314**	-0.343***
	(0.0743)	(0.0698)	(0.0617)	(0.0675)	(0.144)	(0.144)	(0.138)	(0.120)
log(Distance to HK)	-1.067***	-1.006***	-0.973***	-0.857***	-1.143	-0.916	-0.801	-0.694
	(0.262)	(0.263)	(0.260)	(0.259)	(0.726)	(0.685)	(0.649)	(0.612)
log(Distance to Beijing)	-0.439	-0.374	-0.365	-0.382	-0.131	-0.00151	-0.00556	0.0892
	(0.387)	(0.344)	(0.349)	(0.332)	(0.583)	(0.463)	(0.441)	(0.432)
Coastal	0.611***	0.528***	0.481***	0.546***	0.898***	0.824***	0.767***	0.816***
	(0.183)	(0.170)	(0.165)	(0.165)	(0.252)	(0.236)	(0.232)	(0.227)
Border	-1.041**	-0.873***	-0.826**	-0.855***	0.0662	-0.220	-0.162	-0.316
	(0.388)	(0.291)	(0.303)	(0.296)	(0.171)	(0.324)	(0.322)	(0.322)
Longitude	0.0391	0.0261	0.0293	0.0257	-0.0867	-0.0595	-0.0535	-0.0574
	(0.0386)	(0.0357)	(0.0356)	(0.0364)	(0.0822)	(0.0627)	(0.0579)	(0.0584)
Latitude	0.0341	0.0331	0.0377	0.0198	0.0323	0.0275	0.0197	0.0388
	(0.0903)	(0.0850)	(0.0889)	(0.0783)	(0.112)	(0.0960)	(0.0892)	(0.0837)
Observations	359	360	360	360	264	283	285	290
Adj. R-Squared	0.589	0.599	0.595	0.579	0.374	0.339	0.344	0.326
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Cultural distance to Hong Kong is coded as in Section 1.3.3. Night-light is obtained from the Prolonged Artificial Nighttime-light Dataset of China project and aggregated to the year level. Normalized night-light is computed by dividing the night-light data by registered population of the same year. Standard errors are clustered at the province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

A.3.3 Additional Tables for Robustness Checks-Alternative Cutoff Dates for SEZs

Table A.4: IV Estimates of the Effects of SEZs on Labor-Related Outcomes, Using 1991 as Cutoff Date

Panel A: Effects on log(Population)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	0.117	-0.131	0.0904	-0.328	0.0904	-0.383	0.0850	-0.470*
	(0.139)	(0.592)	(0.124)	(0.388)	(0.124)	(0.318)	(0.121)	(0.260)
Observations	264	264	283	283	285	285	290	290
Adj. R-Squared	0.347	0.339	0.336	0.312	0.334	0.303	0.386	0.345
First Stage F-stat		9.764		106.3		106.2		105.8
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(Employment)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	0.450***	0.267	0.465***	-0.0594	0.524***	0.176	0.509***	0.236
	(0.163)	(1.076)	(0.110)	(0.554)	(0.115)	(0.503)	(0.133)	(0.538)
Observations	264	264	283	283	285	285	288	288
Adj. R-Squared	0.305	0.301	0.377	0.345	0.392	0.380	0.421	0.414
First Stage F-stat		9.764		106.3		106.2		104.6
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(Total Wage)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	0.490***	-0.443	0.586***	-0.158	0.610***	0.167	0.665***	0.174
	(0.126)	(1.338)	(0.137)	(0.863)	(0.134)	(0.665)	(0.161)	(0.693)
Observations	264	264	283	283	285	285	288	288
Adj. R-Squared	0.339	0.247	0.375	0.328	0.421	0.405	0.450	0.432
First Stage F-stat		9.764		106.3		106.2		104.6
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1991 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1991. IV results contain the output of instrumenting SEZ by 1991 with the cultural distance to Hong Kong. Population data contain locally registered people only and can therefore exclude seasonal migrant workers. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table A.5: IV Estimates of the Effects of SEZs on Capital-Related Outcomes, Using 1991 as Cutoff Date

Panel A: Effects on log(Investment)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	0.450**	0.859	0.522***	0.443	0.564***	0.868	0.623***	0.802**
	(0.173)	(0.660)	(0.129)	(0.814)	(0.106)	(0.696)	(0.125)	(0.339)
Observations	264	264	283	283	285	285	288	288
Adj. R-Squared	0.374	0.361	0.492	0.492	0.394	0.384	0.498	0.495
First Stage F-stat		9.764		106.3		106.2		104.6
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(Real Estate Investment)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	0.807***	1.287	0.814***	1.244	0.849***	1.419	0.795***	1.107
	(0.243)	(1.452)	(0.208)	(1.064)	(0.195)	(1.326)	(0.171)	(0.700)
Observations	263	263	283	283	285	285	287	287
Adj. R-Squared	0.368	0.360	0.404	0.397	0.387	0.371	0.433	0.428
First Stage F-stat		9.736		106.3		106.2		104.4
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(Residential Properties Investment)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	0.560**	0.456	0.803***	1.406	0.864***	2.003***	0.804***	1.118
	(0.246)	(1.452)	(0.201)	(1.166)	(0.151)	(0.766)	(0.166)	(0.714)
Observations	263	263	282	282	284	284	287	287
Adj. R-Squared	0.290	0.290	0.395	0.381	0.387	0.322	0.436	0.431
First Stage F-stat		9.648		103.0		104.1		104.4
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1991 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1991. IV results contain the output of instrumenting SEZ by 1991 with the cultural distance to Hong Kong. Residential properties investment is a sub-category of real estate investment. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table A.6: IV Estimates of the Effects of SEZs on Productivity-Related Outcomes, Using 1991 as Cutoff Date

Panel A: Effects on log(Industrial Output)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	0.966***	0.379	0.925***	0.152	0.788***	1.145	0.748***	1.471***
	(0.209)	(2.058)	(0.217)	(1.085)	(0.185)	(0.742)	(0.159)	(0.283)
Observations	263	263	283	283	283	283	287	287
Adj. R-Squared	0.464	0.447	0.532	0.506	0.557	0.550	0.612	0.587
First Stage F-stat		9.715		106.3		105.8		106.6
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(Output Per Labor)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	0.515***	0.111	0.460***	0.212	0.264*	0.970***	0.201	1.236***
	(0.141)	(1.026)	(0.156)	(0.562)	(0.134)	(0.255)	(0.134)	(0.302)
Observations	263	263	283	283	283	283	287	287
Adj. R-Squared	0.494	0.477	0.472	0.465	0.422	0.351	0.506	0.352
First Stage F-stat		9.715		106.3		105.8		106.6
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(OLS Residual)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	0.168***	0.653***	0.138***	0.674***	0.0827	0.874***	0.0560	0.755
	(0.0410)	(0.202)	(0.0487)	(0.104)	(0.0886)	(0.266)	(0.102)	(0.463)
Observations	263	263	283	283	283	283	284	284
Adj. R-Squared	0.414	0.332	0.338	0.234	0.421	0.220	0.553	0.456
First Stage F-stat		9.715		106.3		105.8		105.2
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1991 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1991. IV results contain the output of instrumenting SEZ by 1991 with the cultural distance to Hong Kong. OLS residuals are computed by regressing the log value of total output on the log values of total wage and capital stock for each year at the prefecture level, and taking the residuals. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table A.7: IV Estimates of the Effects of SEZs on Foreign-Related Outcomes, Using 1991 as Cutoff Date

Panel A: Effects on log(FDI)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	1.729***	1.269	1.593***	3.484***	1.394***	2.574***	1.441***	2.980***
	(0.233)	(1.548)	(0.184)	(0.429)	(0.334)	(0.255)	(0.249)	(0.756)
Observations	257	257	271	271	273	273	266	266
Adj. R-Squared	0.571	0.567	0.504	0.438	0.514	0.478	0.510	0.466
First Stage F-stat		9.266		96.07		77.03		86.10
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(Foreign-Owned Output)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	2.096***	2.453	1.628***	0.668	1.509***	1.956	1.832***	3.697***
	(0.245)	(2.656)	(0.289)	(2.415)	(0.294)	(1.972)	(0.202)	(0.811)
Observations	243	243	272	272	267	267	273	273
Adj. R-Squared	0.451	0.449	0.505	0.490	0.506	0.502	0.512	0.457
First Stage F-stat		8.944		97.65		77.43		94.80
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(HMT-Owned Output)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	1.784***	1.910	1.549***	3.805**	1.065***	3.233***	1.056***	3.131***
	(0.263)	(1.630)	(0.301)	(1.596)	(0.302)	(0.336)	(0.310)	(0.638)
Observations	249	249	263	263	267	267	275	275
Adj. R-Squared	0.540	0.540	0.540	0.462	0.553	0.470	0.505	0.435
First Stage F-stat		9.553		87.23		91.28		94.82
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1991 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1991. IV results contain the output of instrumenting SEZ by 1991 with the cultural distance to Hong Kong. Foreign- and HMT-owned output refer to total output from firms owned by foreign and Hong Kong, Macao and Taiwan owners, two mutually exclusive groups in China's economic statistics. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table A.8: IV Estimates of the Effects of SEZs on Marketization, Using 1991 as Cutoff Date

Panel A: Effects on Private Share

	1998		2001		2004		2007	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	0.113***	0.204	0.134***	0.227	0.0665***	0.186***	0.00927	0.154*
	(0.0348)	(0.306)	(0.0329)	(0.230)	(0.0195)	(0.0439)	(0.0202)	(0.0937)
Observations	291	291	301	301	319	319	323	323
Adj. R-Squared	0.551	0.543	0.429	0.421	0.342	0.324	0.299	0.268
First Stage F-stat		11.06		11.05		107.4		109.3
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on Private Capital Ratio

	1998		2001		2004		2007	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	0.162***	0.108	0.170***	0.172	0.0378	0.107**	0.0157	-0.238**
	(0.0372)	(0.381)	(0.0260)	(0.222)	(0.0265)	(0.0437)	(0.0259)	(0.107)
Observations	291	291	301	301	319	319	323	323
Adj. R-Squared	0.511	0.508	0.360	0.360	0.321	0.314	0.202	0.0897
First Stage F-stat		11.06		11.05		107.4		109.3
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(No. of Private Entry)

	1998		2001		2004		2007	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1991	0.943***	-0.202	0.874***	0.330	0.793***	0.216	0.590***	0.266
	(0.172)	(1.591)	(0.155)	(1.704)	(0.142)	(0.942)	(0.121)	(0.710)
Observations	291	291	302	302	319	319	323	323
Adj. R-Squared	0.650	0.597	0.662	0.651	0.698	0.688	0.719	0.716
First Stage F-stat		11.06		11.08		107.4		109.3
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1991 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1991. IV results contain the output of instrumenting SEZ by 1991 with the cultural distance to Hong Kong. Private share is computed by aggregating private-firm and state-firm revenues at the prefecture level and compute the share of private revenue to total revenue. Private capital ratio is computed by taking the ratio of private contributed capital (also called paid-in capital) to total capital, and then aggregate these ratios to the prefecture level using each firm's assets as weights. Number of private entry is computed as follows: define firm i as a private entrant in year t if year t is the first year that firm i appears in a given prefecture and the firm is private. Then sum the total number of private entrants in a given year by prefecture. For the year 1998 (the first year of the data), define a firm as an entrant if its registered age is equal to 1. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table A.9: IV Estimates of the Effects of SEZs on Labor-Related Outcomes, Using 1985 as Cutoff Date

Panel A: Effects on log(Population)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	0.195	-0.137	0.221	-0.539	0.219	-0.629	0.225	-0.770
	(0.169)	(0.619)	(0.157)	(0.841)	(0.156)	(0.755)	(0.154)	(0.695)
Observations	264	264	283	283	285	285	290	290
Adj. R-Squared	0.350	0.337	0.340	0.279	0.338	0.262	0.390	0.294
First Stage F-stat		8.095		3.211		3.218		3.225
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(Employment)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	0.619***	0.279	0.767***	-0.0976	0.796***	0.290	0.723***	0.388
	(0.191)	(1.115)	(0.164)	(0.953)	(0.168)	(0.697)	(0.207)	(0.711)
Observations	264	264	283	283	285	285	288	288
Adj. R-Squared	0.319	0.306	0.404	0.338	0.413	0.393	0.433	0.425
First Stage F-stat		8.095		3.211		3.218		3.224
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(Total Wage)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	0.835***	-0.461	0.994***	-0.259	0.931***	0.273	0.888***	0.284
	(0.228)	(1.405)	(0.184)	(1.535)	(0.180)	(0.971)	(0.197)	(1.009)
Observations	264	264	283	283	285	285	288	288
Adj. R-Squared	0.372	0.231	0.409	0.309	0.443	0.417	0.460	0.440
First Stage F-stat		8.095		3.211		3.218		3.224
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1985 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1985. IV results contain the output of instrumenting SEZ by 1985 with the cultural distance to Hong Kong. Population data contain locally registered people only and can therefore exclude seasonal migrant workers. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table A.10: IV Estimates of the Effects of SEZs on Capital-Related Outcomes, Using 1985 as Cutoff Date

Panel A: Effects on log(Investment)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	0.875***	0.895	0.848***	0.727	0.754***	1.425***	0.586***	1.315***
	(0.185)	(0.675)	(0.143)	(1.012)	(0.132)	(0.534)	(0.185)	(0.297)
Observations	264	264	283	283	285	285	288	288
Adj. R-Squared	0.408	0.408	0.513	0.512	0.405	0.370	0.486	0.454
First Stage F-stat		8.095		3.211		3.218		3.224
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(Real Estate Investment)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	1.455***	1.341	1.454***	2.043**	1.130***	2.331**	1.124***	1.813***
	(0.350)	(1.485)	(0.290)	(0.835)	(0.175)	(1.153)	(0.260)	(0.395)
Observations	263	263	283	283	285	285	287	287
Adj. R-Squared	0.402	0.402	0.441	0.431	0.399	0.346	0.446	0.430
First Stage F-stat		8.088		3.211		3.218		3.222
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(Residential Properties Investment)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	0.766	0.476	1.381***	2.234***	1.025***	3.296***	1.066***	1.832***
	(0.471)	(1.500)	(0.286)	(0.807)	(0.168)	(0.540)	(0.231)	(0.436)
Observations	263	263	282	282	284	284	287	287
Adj. R-Squared	0.297	0.294	0.425	0.404	0.389	0.196	0.444	0.425
First Stage F-stat		7.326		2.910		3.215		3.222
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1985 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1985. IV results contain the output of instrumenting SEZ by 1985 with the cultural distance to Hong Kong. Residential properties investment is a sub-category of real estate investment. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table A.11: IV Estimates of the Effects of SEZs on Productivity-Related Outcomes, Using 1985 as Cutoff Date

Panel A: Effects on log(Industrial Output)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	1.309***	0.395	1.261***	0.250	0.967***	1.889***	0.792***	2.415***
	(0.244)	(2.135)	(0.209)	(1.670)	(0.179)	(0.394)	(0.147)	(0.773)
Observations	263	263	283	283	283	283	287	287
Adj. R-Squared	0.484	0.452	0.546	0.513	0.561	0.528	0.608	0.513
First Stage F-stat		8.067		3.211		2.940		3.222
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(Output Per Labor)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	0.689***	0.115	0.494***	0.348	0.169	1.600***	0.0691	2.029
	(0.138)	(1.067)	(0.174)	(0.776)	(0.136)	(0.459)	(0.108)	(1.415)
Observations	263	263	283	283	283	283	287	287
Adj. R-Squared	0.506	0.479	0.469	0.467	0.415	0.198	0.500	0.0860
First Stage F-stat		8.067		3.211		2.940		3.222
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(OLS Residual)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	0.00386	0.680***	-0.146**	1.108*	-0.119*	1.442	-0.143*	1.241
	(0.0703)	(0.208)	(0.0546)	(0.590)	(0.0588)	(1.124)	(0.0724)	(1.324)
Observations	263	263	283	283	283	283	284	284
Adj. R-Squared	0.404	0.279	0.337	-0.0888	0.423	-0.160	0.555	0.269
First Stage F-stat		8.067		3.211		2.940		3.220
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1985 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1985. IV results contain the output of instrumenting SEZ by 1985 with the cultural distance to Hong Kong. OLS residuals are computed by regressing the log value of total output on the log values of total wage and capital stock for each year at the prefecture level, and taking the residuals. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table A.12: IV Estimates of the Effects of SEZs on Foreign-Related Outcomes, Using 1985 as Cutoff Date

Panel A: Effects on log(FDI)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	1.640***	1.323	1.687***	5.710*	1.305***	3.879**	1.649***	4.877***
	(0.204)	(1.599)	(0.313)	(3.236)	(0.245)	(1.747)	(0.281)	(1.132)
Observations	257	257	271	271	273	273	266	266
Adj. R-Squared	0.555	0.553	0.497	0.270	0.497	0.365	0.510	0.362
First Stage F-stat		8.026		3.187		3.975		3.159
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on log(Foreign-Owned Output)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	2.132***	2.558	1.879***	1.095	1.722***	2.958	1.432***	6.050***
	(0.380)	(2.740)	(0.358)	(3.468)	(0.294)	(1.884)	(0.248)	(1.647)
Observations	243	243	272	272	267	267	273	273
Adj. R-Squared	0.437	0.435	0.505	0.498	0.506	0.484	0.483	0.232
First Stage F-stat		7.814		3.185		3.955		3.308
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(HMT-Owned Output)

	2001		2006		2011		2016	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	1.881***	2.002	1.888***	6.232	1.607***	5.316*	1.501***	5.122
	(0.205)	(1.680)	(0.181)	(5.391)	(0.236)	(2.714)	(0.287)	(3.287)
Observations	249	249	263	263	267	267	275	275
Adj. R-Squared	0.534	0.533	0.545	0.324	0.567	0.385	0.515	0.354
First Stage F-stat		8.180		3.149		3.132		3.179
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1985 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1985. IV results contain the output of instrumenting SEZ by 1985 with the cultural distance to Hong Kong. Foreign- and HMT-owned output refer to total output from firms owned by foreign and Hong Kong, Macao and Taiwan owners, two mutually exclusive groups in China's economic statistics. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table A.13: IV Estimates of the Effects of SEZs on Marketization, Using 1985 as Cutoff Date

Panel A: Effects on Private Share

	1998		2001		2004		2007	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	0.0914**	0.210	0.0695**	0.233	0.0500**	0.300*	0.0134	0.247***
	(0.0390)	(0.309)	(0.0316)	(0.230)	(0.0207)	(0.165)	(0.0263)	(0.0741)
Observations	291	291	301	301	319	319	323	323
Adj. R-Squared	0.545	0.534	0.415	0.395	0.339	0.280	0.299	0.236
First Stage F-stat		8.376		8.479		3.303		3.353
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Effects on Private Capital Ratio

	1998		2001		2004		2007	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	0.126***	0.111	0.106***	0.176	0.0457*	0.172*	0.000994	-0.383
	(0.0408)	(0.388)	(0.0190)	(0.222)	(0.0233)	(0.0948)	(0.0239)	(0.337)
Observations	291	291	301	301	319	319	323	323
Adj. R-Squared	0.496	0.496	0.338	0.333	0.321	0.304	0.201	0.00560
First Stage F-stat		8.376		8.479		3.303		3.353
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Effects on log(No. of Private Entry)

	1998		2001		2004		2007	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
SEZ by 1985	1.139***	-0.209	1.107***	0.338	1.151***	0.348	0.956***	0.427
	(0.171)	(1.646)	(0.195)	(1.737)	(0.179)	(1.361)	(0.177)	(0.949)
Observations	291	291	302	302	319	319	323	323
Adj. R-Squared	0.656	0.597	0.670	0.652	0.711	0.695	0.731	0.724
First Stage F-stat		8.376		8.487		3.303		3.353
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: SEZ by 1985 is an indicator, equal to 1 if a prefecture has gained the SEZ status by 1985. IV results contain the output of instrumenting SEZ by 1985 with the cultural distance to Hong Kong. Private share is computed by aggregating private-firm and state-firm revenues at the prefecture level and compute the share of private revenue to total revenue. Private capital ratio is computed by taking the ratio of private contributed capital (also called paid-in capital) to total capital, and then aggregate these ratios to the prefecture level using each firm's assets as weights. Number of private entry is computed as follows: define firm i as a private entrant in year t if year t is the first year that firm i appears in a given prefecture and the firm is private. Then sum the total number of private entrants in a given year by prefecture. For the year 1998 (the first year of the data), define a firm as an entrant if its registered age is equal to 1. Controls are included in the regressions but results are omitted. Standard errors are clustered at the provincial level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

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CHAPTER 2

Privatization, State-Owned Enterprises Reform, and State Strategy: Evidence from Post-Crisis Stimulus in China

2.1 Introduction

Immediately after the 2008 Global Financial Crisis (GFC), China's Vice Premier Wang Qishan, in a high-level meeting with a group of international guests,

> (...) quickly made it clear that China had little to learn from the visitors about its financial system. 'Mr Wang said: "This is what you do, and this is what we do," which is what the Chinese always say,' one of the participants recalled. 'But his message was different. It was: "You have your way. We have our way. And our way is right!"' (McGregor, 2010, p.3)

For long-term China observers, the turn toward an aggressive posture by the Vice Premier was interpreted as a policy break from the pre-crisis years. After the fall of the Soviet Union in the 1990s, China's SOE reform program was permeated by the idea of privatization, a cornerstone proposal of the Washington Consensus package of economic policy reform in the 1980s and 90s. The accession of the World Trade Organization (WTO) was widely interpreted as China's further political commitment to continued privatization, which has since increased the share of the private economy in the overall economic output (Xia & Walker, 2015; Hu *et al.*, 2023). In the wake of the Global Financial Crisis, however, the thinking regarding the state-owned economy from China's leading policymakers, showcased in the above statement by Wang Qishan, was about to shift dramatically. Toward the end of

2008, the Chinese government coordinated with its trading partners to introduce an initial 4 trillion RMB fiscal stimulus package, equivalent to about 12.5% of China's GDP at the time (Liu *et al.*, 2018). However, aside from a few economic outcomes (Fardoust *et al.*, 2012; Chen *et al.*, 2019b; Cong *et al.*, 2019; Han *et al.*, 2021), the impact of stimulus on firm dynamics between state-owned enterprises (SOEs) and private firms, as well as its lasting effects on privatization and SOE reform strategies, has not been comprehensively explored. Furthermore, limited attention has been paid to how the design and implementation of the stimulus program relate to the prior degree of market-level privatization and how this affects aggregate-level outcomes. We tackle these questions in this paper.

[Insert Figure 2.1 here]

Figure 2.1 illustrates the political economy context by highlighting a simple stylized fact: the median private revenue share in a predefined market, defined as the ratio of total revenue from private firms to the total revenue of all firms within a sector-province pair, continues to advance forward across three different industrial classification levels, despite the disruption caused by fiscal stimulus between 2008 and 2010. Although the Global Financial Crisis impacted the reform strategy of SOEs and influenced the design of stimulus packages, the general trend toward further marketization persists. What transpired? Did the stimulus package achieve its intended outcomes? What explains the ongoing evolution of market-level privatization?

In this paper, we address an important research vacancy by first proposing a measure of market-level privatization on the basis of the total revenue share of SOEs and private firms and then matching it with the intensity of the stimulus package at the province $\times$ sector level[1]. In doing so, we utilize detailed ownership information on SOEs in the Chinese manufacturing industry from the Annual Survey of Industrial Firms (ASIF),[2] administrated

[1]Our focus on dynamics between SOEs and private firms takes a view of an industry perspective of privatization. This approach differs from firm-level studies, which, for example, investigate the impact of split-share structure reform in converging the interests of the government and private investors (Liao *et al.*, 2014).

[2]Alternatively, names of the same dataset include Annual Survey of Industrial Enterprises.

by the National Bureau of Statistics (NBS) of China. Our sample contains rich information on the ownership structures of approximately 11,000 SOEs and 328,000 private firms over the period of 2004 and 2013, covering a 10-year period around the Global Financial Crisis and the subsequent stimulus program. Combined with official information on the stimulus program in 2008, we are able to disaggregate the fiscal stimulus based on three years of pre-shock revenue share into a province $\times$ sector level consistent with our market-level privatization measure. Since the design of the stabilization package heavily relies on SOEs as channeling agents, we establish, as a stylized fact based on our measures, that the (expected) intensity of the fiscal stimulus in a given market is negatively correlated with the degree of privatization in that market. This factual pattern remains robust even after conditioning on broad sector and province-fixed effects, suggesting that the stimulus policy targets markets with a heavier presence of SOEs in addition to sector and location.

To examine the short-term and long-term effects of the stimulus, we employ a triple-difference (DDD) estimation framework to causally identify the differential impact of the stimulus program on the performance of SOEs compared to private firms. Given the rapid expansion of the Chinese economy around the time of the Global Financial Crisis (GFC), it is unlikely that markets subject to the stimulus package are experiencing parallel trends compared to those not directly affected by the stimulus. Therefore, by introducing private firms as an additional control group, the DDD specification allows us to more accurately identify the effects of the stimulus program on firm-level outcomes for SOEs. Since the divergent outcomes between SOEs and private firms lead to changes in aggregate outcomes, we also examine the effect of the stimulus on aggregate-level outcomes, conditional on the prior level of privatization in a given market. By decomposing the evolution of market-level privatization dynamics, we further investigate the effect of the stimulus on each component driving the long-term upward trend of deepening privatization at the province $\times$ sector level.

Given the large heterogeneity in the pre-shock levels of privatization, SOEs in more privatized markets tend to face tougher competition from private firms than their counterparts in less privatized markets, even with the same intensity of fiscal stimulus. Therefore, beyond simple comparisons between the performance of SOEs and private firms (Dollar &

Wei, 2007; Lardy, 2014), we differentiate SOEs based on their pre-shock competitive environments, categorizing them into groups depending on whether their markets have high or low degrees of privatization. Using a triple-difference estimation framework, we examine the causal impact of stimulus intensity on firm performance between SOEs and private firms across these groups. Benchmarking against previous findings that consistently report SOEs underperforming post-2008 (Chen *et al.*, 2019b; Han *et al.*, 2021; Zou, 2024), we find that SOEs exposed to the stimulus package with varying intensities perform better, with significant effects on revenue and assets. By splitting the samples into two groups based on the median degree of privatization, we discover that although positive effects are present in both samples, only the sample with less privatized markets shows significant effects. Moreover, SOEs in less privatized markets experience greater productivity gains after exposure to the stimulus, compared to negative, insignificant effects for those in more privatized markets. Thus, the fiscal stimulus was strategically designed to act as a government demand shifter, strengthening SOEs in less privatized markets and enhancing their power, even as overall market-level privatization advances.

Our firm-level evidence aligns with aggregate-level results. Using a similar triple-difference (DDD) specification, we interact expected stimulus with a market's prior degree of privatization and a post-2008 dummy to investigate how the stimulus program affects market-level outcomes based on privatization levels. The empirical results reveal a polarizing effect of fiscal stimulus: in less privatized markets, the stimulus benefits SOEs by consolidating their position, while in highly privatized and competitive markets, the stimulus accelerates the exit of private firms and inhibits the elimination of inefficient SOEs. Furthermore, we observe a negative impact of stimulus programs on baseline privatization, which is less pronounced in already highly privatized markets. Consequently, the stimulus exacerbates the disparity between more and less privatized markets. To verify the robustness of our findings, we test several alternative measures of stimulus intensity, examine multiple subsamples, and address various methodological concerns. In the end, these results are broadly consistent with our main findings.

To explore the theoretical mechanisms underpinning our empirical findings, we construct

a political economy model by embedding a politico-economic objective function into a classic framework of heterogeneous firms (Melitz, 2003; Chen *et al.*, 2019a). In contrast to the misallocation literature, which focuses on the unintended costs of the stimulus programs, we argue that China's policymakers pioneered contextually dependent and industry-specific reform packages tailored to different developmental stages. Prior to the Global Financial Crisis, the priority of the strategy was to pressure SOEs with intense market entries from private firms and engineer a mass exiting of SOEs in industries with low strategic values by cutting back subsidies. Following the stimulus package largely channeled by SOEs, the strategic focus shifts into consolidating existing SOEs in industries with high strategic values as the exit rate for remaining SOEs drops. In both periods, policymakers carefully aligned active private firm entries and exits to serve the reform objective of SOEs.

This paper is related to several strands of the literature in macroeconomics and political economy. First, our study makes progress on a growing literature on the determinants and consequences of China's post-Global Financial Crisis stimulus program and how SOEs were purposefully deployed as channeling agents of the party-state's politico-economic objectives in monetary policy transmission (Chen *et al.*, 2019b), housing prices (Han *et al.*, 2021), and unemployment (Zou, 2024). Bai *et al.* (2016) highlights the role of local financing vehicles in supporting fiscal expansion in 2009 and 2010, exacerbating the extent of capital misallocation in favor of connected firms. Similar to our study, Cong *et al.* (2019) documents that China's credit stimulus package as a response to the Global Financial Crisis exacerbated capital allocation to private firms and disproportionately favored SOEs in a misallocation framework *a la* Hsieh & Song (2015). Additionally, Liu *et al.* (2018) finds that bank lending became less responsive to the profitability quality of investment opportunities using data on publicly listed firms.

Our paper also contributes to the literature on private sector development through privatization at the market and firm level in China and worldwide. The privatization project, a central tenet of the Washington Consensus package of economic reforms, spawns large empirical studies in developing countries (Gupta *et al.*, 2008; Roland, 2008; Estrin & Pelletier, 2018). The market transition in China, by and large, fits broadly into this global

picture, where the development of the private sector is conceived as an important source of economic growth, employment, tax revenue, domestic exports, and foreign direct investment in the past decades. The rapid rise in the number of global billionaire entrepreneurs from mainland China, fast catching up to that of the United States, results from deepening privatization and marketization (Xiao, 2023). Moreover, Xiao (2024) also finds that the incidence of SOE restructuring during the career of a billionaire entrepreneur is associated with an increased probability of bad entrepreneurship. However, our present discussion deviates from existing works on privatization in three important aspects. First, we define the process of privatization at two distinct yet interrelated levels: the firm level, where the share of ownership of SOEs is transferred to private firms, and the market level, where the market size is increasingly dominated by private firms. Second, treating fiscal stimulus as an exogenous shock to firms, the stabilization package introduced by China's policymakers can be interpreted as a policy response to the intellectual crisis of neoliberal political economy (Posner, 2009). Third, apart from concentrating on private ownership alone, we stress the importance of firm dynamics between SOEs and private firms as a channeling mechanism in response to both competitive market discipline and the party-state's political objectives.[3]

Our empirical findings and theoretical mechanisms are related to research on firm dynamics and creative destruction in China. In early efforts, Hu *et al.* (2005); Jefferson (2008) have observed the underlying process of creative destruction fundamental to China's economic growth miracle. In an influential study by Brandt *et al.* (2012), the authors report that "Between 1998 and 2007, annual productivity growth was 12.5% for SOEs, compared to 11.3% for private firms and 11.8% for foreign." Using the introduction of a stimulus program as an exogenous shock, we showcase that the process of creative destruction and firm dynamics in China is not only endogenous to purely competitive market pressure but also sensitive to strategic shifts of careful political calculations.

Finally, our study is related to the comparison of SOEs and private enterprises over

[3]Consistent with early findings from Zhang *et al.* (2008); what matters appears to be not privatization and regulation on their own, but the nature of competition in stimulating performance improvements, independent of ownership types and regulatory environments.

the course of the transition to a market economy in China. SOEs have consistently been found to suffer a lower marginal return to capital than their private counterparts (Dollar & Wei, 2007; Lardy, 2014). With the help of firm-level evidence, Bai *et al.* (2006, 2009) offers a multi-task theory of SOE reform, arguing that the need for SOEs to provide social stability can rationalize its inefficiencies. One observation underlying our analysis suggests that SOEs are qualitatively different from private firms, except those large private business groups, echoing with Allen *et al.* (2022a) that "drawing a stark distinction between SOEs and POEs misperceives the reality of the corporate sectors as well as their role in the economy." In a similar vein, Chen *et al.* (2019a) reports that private multinational companies are more likely to undertake FDI but are less productive and smaller than state-owned counterparts.

The remainder of the article is organized as follows. In Section 2.2, we discuss the policy context under which the privatization strategy on SOE reforms is related to the stimulus package. Specifically, we formally introduce our measure of market-level privatization and the method with which we disaggregate official stimulus information into comparable units of analysis. In Section 2.3, using the triple difference estimation, our causal analysis estimates the impact of stimulus on firm-level and aggregate level outcome, with robustness checks in Section 2.4. To rationalize our findings, in Section 2.5, we propose a political economy model to explicate the theoretical channels. Section 2.6 concludes.

2.2 The Policy Context

Since the beginning of China's Reform and Opening-Up policies, SOE reform has always been the cornerstone of the transition program into a "socialist market economy," which, up to 2013, experienced three major waves of reorientation of reform priorities. The first wave, conventionally dated to be between 1978 and the mid-1990s, was focused on the improvement of SOE management and performance through incentive-based measures that involve granting a greater degree of autonomy to the management and allowing firms to retain a greater share of profit. During this period, however, the contentious issue of radically altering SOEs' ownership structure was strategically sidelined.

The second wave of SOE reform was implemented gradually after 1992 when Deng Xiaoping's monumental southern tour green-lighted the political direction of continued marketization. For example, the official decision at the 14^{th} National Congress of the Chinese Communist Party (CCP) in 1992 to transform the Chinese economy into a *socialist market economy* aimed at benchmarking SOEs' operation and performance against market standards, with the lasting implication of accelerating much more fundamental reforms within the state sector. In order to reduce operational inefficiencies and financial losses of SOEs, the experiments during this era included new measures in the form of mass layoffs, debt swaps, and debt reduction, among other proposals. Importantly, ownership reform was introduced as a means to transform SOEs into functioning business entities rather than "state production units" (Lee, 2009). Consequently, the initialization of the corporatization program gradually restructured SOEs into shareholding corporations in the mode of private firms, which greatly simplified the firm-level privatization process and the eventual transfer of ownership. Overlapped with China's accession to the WTO in 2001, which triggered the privatization process for many SOEs due to increased pressures from import competition (Hu *et al.*, 2023), the hallmark of this era is captured by the "grasp the large, let go of the small" slogan, where large SOEs were more carefully turned into competitive businesses, while smaller SOEs were "let go" and either exited the market or became privatized.

By 2003, the establishment of the State-owned Assets Supervision and Administration Commission (SASAC) marked the beginning of the third phase of SOE reforms. The SASAC in Beijing, in conjunction with sub-national SASAC agencies at the local level, provided the party-state with a bureaucratic organ to better oversee large central and local SOEs. This organization facilitated the market-level privatization process while managing the challenges posed by private sector demands. The SASAC consolidated the management system of state-owned assets and constructed a framework to enhance business cooperation between SOEs and private firms in markets with varying degrees of privatization.

Despite efforts to improve economic efficiency, SOEs retained roles essential for the government's non-economic objectives. Concurrently, in the early 2000s, SOEs were assigned a new role: going global. While it might seem natural for large SOEs and private firms to

expand globally following China's accession to the WTO, under state guidance, the global presence of SOEs has been closely tied to the party-state's broader objectives beyond mere economic efficiency.[4] Amid deepening market-level privatization, the economic and political roles that SOEs play, aligned with the party-state's global and domestic policy objectives, remain vital. These roles, in turn, influence policymakers' decisions regarding market and firm-level privatization.

2.2.1 Data

Our main dataset draws from the firm-level information from the Annual Survey of Industrial Firms (ASIF), compiled by the National Bureau of Statistics (NBS) of China from 1998 to 2013.[5] The data from 1998 to 2006 cover all SOEs and non-SOEs with an annual revenue larger than 5 million RMB. Starting in 2007, ownership status was no longer stated as a criterion for inclusion in the dataset, and therefore, having a revenue exceeding 5 million RMB was the only criterion left. Starting in 2011, the minimum revenue threshold was raised to 20 million RMB (Brandt *et al.*, 2023). Limitations of the ASIF dataset aside, we stress that this dataset still provides rich and comprehensive details at the firm level and remains a benchmark for studies on Chinese firms. Although the dataset contains observations going back to 1998, we restrict our sample to the ten years around the GFC, which is the focus of our paper. We also drop any observations that permanently exited the sample before 2008.

[Insert Table 2.1 here]

We follow the literature (Brandt *et al.*, 2014) by cleaning and matching the dataset across years. For key variables, ownership is defined by a firm's registration type according to ASIF records for consistency.[6] Separated by ownership type (SOE versus non-SOE),

[4]For example, SOEs have played significant roles in overseas resource development, such as funding oil exploration in Africa (Lee, 2009).

[5]This dataset is the main dataset used by researchers studying firm-level and even more aggregated-level issues in the Chinese economy. Many seminal works in this field, including Hsieh & Klenow (2009) and Song *et al.* (2011), have used this dataset, so its strengths and weaknesses are well-known.

[6]Recent literature has begun to emphasize the importance of looking past registration types and using

Table 2.1 provides key variables' summary statistics in 2007 and 2013. Note that the ASIF categorizes ownership type not simply by making a distinction between state and private firms but also includes foreign ownership and Hong Kong, Macau, and Taiwan (HMT) firms. We, therefore, use the term non-SOEs to pool all firms that are not registered as state-owned to facilitate comparability and illuminate our analytical perspective.[7] In general, non-SOEs are smaller than SOEs in terms of revenue, assets, profit, and employment, are younger in age, and are greater in number.[8]

2.2.2 Privatization and State-Owned Enterprise Reform

We begin to construct our measure of privatization at the province × sector (market) level, which is our unit of analysis throughout this paper. Utilizing a decomposition technique, we also show how our market-level measure of privatization is linked to the firm-level measure of privatization, where shares of SOEs are sold to private agents. This measure, whose variation just prior to the fiscal stimulus is central to our analysis, gauges the extent to which the market is regarded as having high strategic value, as viewed by China's policymakers. Formally, for each market (p, k) at province p and sector k, we define at the year prior to stimulus:

$$\text{PrivateShare}_{p,k,2007} = \frac{\sum_i \mathbf{1}_{\text{private}_{i,2007}} \times w_{i,p,k,2007}}{\sum_i w_{i,p,k,2007}}, \tag{2.1}$$

where $\mathbf{1}_{\text{private}_i}$ is an indicator function equal to 1 if firm i is private, and $w_{i,p,k}$ is a measure of firm size measured by firm revenue. Previously shown, Figure 2.1 visualizes this measure across our sample years.

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capital information to identify true ownership of Chinese firms (Allen *et al.*, 2022b). Since the information from the ASIF dataset on detailed contributed (or paid-in) capital information is missing for the crucial year of 2009, we use the capital information only as a robustness check. As shown in several figures and tables in this paper, the difference is minimal.

[7] In our analysis, the term "private" (a.k.a. non-state) was broadly used to pool together all domestically private-owned, foreign-owned, and HMT-owned firms.

[8] Table B.1 in the Appendix also provides the summary statistics for other critical years, including 2004 (the first year in our sample) and 2008 (the year of the GFC). Additionally, we use contributed capital to determine if a firm is state-owned by checking if the firm's state-owned capital is at least 50% of the total contributed capital and compute the same statistics. The results show very similar numbers and patterns in Appendix Table B.2.

[Insert Figure 2.2 here]

To better characterize the general trend of the evolution of market-level privatization and relate this measure to firm-level privatization,[9] we conduct a decomposition exercise using the conceptual flow of Figure 2.2, which illustrates the dynamic process of firm-level privatization and nationalization between SOEs and private firms.[10] Specifically, SOEs and private firms can be transformed into one another by privatization and (re)nationalization, and each type of firm experiences respective market entry and exit of its own. As a result, we can decompose the change in the market-level degree of privatization, measured by firm revenue, into the following five components. Formally, the evolution of private revenue share dynamics at a given market measured by specific province-sector pair (p, k) can be written as:[11]

$$\begin{aligned}\Delta\text{Private\_Revenue}_{p,k,t} = &\sum_i \mathbb{1}\{\text{Private Stayers}\}\Delta\text{Revenue}_{i,p,k,t} + \sum_w \mathbb{1}\{\text{Private Entrants}\}\text{Revenue}_{w,p,k,t} \\ &+ \sum_j \mathbb{1}\{\text{Privatized}\}\text{Revenue}_{j,p,k,t} - \sum_g \mathbb{1}\{\text{Nationalized}\}\text{Revenue}_{g,p,k,t-1} \\ &- \sum_q \mathbb{1}\{\text{Private Quitters}\}\text{Revenue}_{q,p,k,t-1}\end{aligned} \tag{2.2}$$

where i, w, j, g, q are indices for firms. The five components are (a) the change in revenue by continuing private firms (e.g., private stayers); (b) the change in private revenue brought by private entrants (e.g., private entrants); (c) the change in private revenue brought by newly privatized SOEs (e.g., privatized); (d) the loss of revenue from existing private firms (e.g., private quitters); and finally (e) the loss of revenue from (re)nationalized firms (e.g., nationalized). Note that Equation 2.2 links the market-level privatization process with that at the firm level. Calibrated within our firm-level data, we could calculate the relative importance of each of the above components. Table 2.2 computes the ratio of each of the

[9] Huang *et al.* (2021), for example, reports that the decisions to re-nationalize private firms are carried by politicians without strong factional and more sensitive to measures of social stability.

[10] We use the ownership data in 2007 only so that each firm is assigned a fixed status, and as thus we do not need to be concerned with it changing status from private to state-owned or *vice versa*.

[11] A similar decomposition can be written for state shares.

above components to the total revenue of the given market, and for each ratio, rank all markets from top to bottom and present the evolution of the median values.[12]

[Insert Table 2.2 here]

The results suggest that private stayers' contribution to total revenue - a major driver of market-level privatization - experienced a mild decline after 2010, while the entry and exit components became active. Contributions from privatization and (re)nationalization appear to be relatively small. In order to further observe the evolution of privatization across markets, we plot the yearly 25^{th}, 50^{th} and 75^{th} quantiles of growth in private share across markets (at the 2-digit level) in Figure 2.3. We observe that post-2008, the growth in private share experiences a higher degree of fluctuation compared to pre-2008, implying that the impact of the GFC and the subsequent stimulus package may not be uniform across private and state sectors.

[Insert Figure 2.3 here]

Overall, the history of SOE reforms and the market-level development of privatization suggests that while the Chinese government has gradually raised the priority of economic efficiency in SOE reforms, with privatization being the key means (sometimes even an end in itself), it has also frequently modified its direction depending on its non-economic objectives and remains prepared to do so should any special occasions arise.

2.2.3 The Fiscal Stimulus Program

In November 2008, when bracing for the impact of the Global Financial Crisis,[13] the government reacted by launching an unprecedented fiscal stimulus program aimed at restoring

[12]For the convenience of interpretation, we use the level instead of the *change* for total revenue and the revenue from remaining firms. Therefore, the contribution of exited and nationalized firms show their relative importance using the revenue from the previous year.

[13]Official statistics show that exports continued to expand in the fourth quarter of 2008 and did not begin to drop until 2009.

economic activities and confidence. This section describes the fiscal stimulus package in detail and presents stylized facts on how its distribution across markets is correlated with the prior degree of privatization. The stimulus program was announced after a State Council meeting in November 2008. The headlining figure, 4 trillion RMB, was equivalent to 12.5% of the total GDP in 2008 (Liu *et al.*, 2018). The program was designed to run from the fourth quarter of 2008 to the end of 2010, representing "a large, unexpected *increment* to the existing plan" (Naughton, 2009). However, the central government committed to fund only 1.18 trillion of the program, leaving the rest to be sourced by local governments. The investment plans are combined with loosening the central bank's monetary policy and the financial constraints imposed by state-owned commercial banks.

[Insert Table 2.3 here]

Although the program's implementation began in the fourth quarter of 2008 with a spending amount of around 100 billion RMB, the National Development and Reform Commission (NDRC) revised the stimulus in March 2009. Table 2.3 outlines the seven main areas covered by the program as described in official documents, without referencing specific sectors. Notably, one-quarter of the stimulus program was allocated to earthquake relief and reconstruction efforts following the 2008 Sichuan Earthquake. Since this component is concentrated in one province and lacks clear sectoral targets, it is excluded from our analysis (we will use fixed effects to account for its impact). The remainder of the program targets broad industrial sectors, despite the absence of direct references to specific industrial codes. Consequently, we have made significant efforts to disaggregate these broad industrial sectors into our units of analysis under the 2003 Chinese Industrial Classification (CIC) system.

We make the following assumptions while conducting our exercise: given the fact that much of the stimulus program required local governments to fund (the central government pledged only 1.18 trillion), many announced their own stimulus programs. Within the first few months of the decision to launch the stimulus program from the State Council, 18 provinces announced their own programs totaling 25 trillion RMB (Naughton, 2009). It can be reasonably deduced that most of the projects proposed were already in the planning

phases even before the GFC, but the stimulus package offered an unexpected opportunity to move certain projects forward. Since the NDRC still needed to approve local projects, we therefore do not consider the specific content of local programs (in any case, most of them do not contain enough details for our analysis) and instead focus on the national program and relegate any unexpected investment from purely local programs to local fixed effects.

In summary, the Chinese government's stimulus program, launched in late 2008, consisted of both fiscal stimulus and a loosening of monetary policy, involving various funding strategies. To operationalize our analysis and construct a treatment variable for the stimulus, we focus on the headline figure of four trillion RMB, which is effectively three trillion RMB after deducting the earthquake relief component, as it represents an *unexpected* increase in investment.[14] We discuss specific measurements and their relation to market-level privatization.

2.2.4 Fiscal Stimulus and Privatization

We now look at how the investment part of the stimulus program is distributed across different sector-province pairs. We do not expect a uniform allocation of investment across relevant markets with heterogeneous prior degrees of privatization as in 2007. As such, the manner in which the stimulus is implemented reveals China's policymakers' preferences and objective functions, one to be rationalized in our theoretical framework. In order to check the relationship between a market's exposure to the stimulus program and its prior degree of privatization, we need to decompose the aggregate stimulus investments into corresponding sector-province pairs. For disaggregated sector, k (i.e., those that can be classified by industry code) that belong to an aggregate sector $\mathcal{K}$ (e.g., rural infrastructure from Table 2.3), consider the following measure:

$$\text{RevenueShare}_{p,k} = \frac{\sum_{i,t\in\mathcal{T}} m_{i,p,k,t}}{\sum_{i,p,k\in\mathcal{K},t\in\mathcal{T}} m_{i,p,k,t}} = \frac{m_{p,k}}{m_{\mathcal{K}}}, \tag{2.3}$$

[14] Additionally, there were secondary stimulus measures, such as those funded by local governments in conjunction with the central bank's loosening of credit policies.

where $m_{i,p,k,t}$ denotes the revenue of firm i at year t in province p, and $\mathcal{T}$ is the set of the three years prior to the GFC (2005 to 2007). In words, $\text{RevenueShare}_{p,k}$ is the share of revenue (summed across 2005 to 2007) in $\mathcal{K}$ going to market (p,k), representing the relative weight of market (p,k) in aggregated sector $\mathcal{K}$ in the three years prior to the stimulus program. We compute the weight using the data from 2005 to 2007 to avoid any idiosyncrasies in 2007 that may skew the result. Therefore, the higher $\text{RevenueShare}_{p,k}$ is for a given (p,k), the more important this market is under the aggregated sector $\mathcal{K}$. Since the official announcement of the stimulus program does not contain a detailed list of disaggregated (coded) sectors, nor does it provide a clear way to match the aggregated sectors to disaggregated sectors, we hand code the disaggregated sectors to match with the aggregated sectors by keywords, at three levels of industrial classifications, CIC 2-, 3- and 4-digit levels.[15]

The key assumption we make in constructing our province $\times$ sector measure on the intensity of the stimulus is that the disaggregate stimulus investment is proportional to the revenue shares from the past three years, as captured by $\text{RevenueShare}_{p,k}$ above. The expected stimulus in each sector-province pair can be computed as:

$$\text{ExpectedStimulus}_{p,k} = \text{Stimulus}_{\mathcal{K}} \times \text{RevenueShare}_{p,k}, \tag{2.4}$$

where $\text{Stimulus}_{\mathcal{K}}$ is the amount of stimulus announced for aggregated sector $\mathcal{K}$ from the numbers provided by Table 2.3.[16]

How is expected stimulus related to the pre-shock market level of privatization? We explore this question using the constructed measures of $\text{ExpectedStimulus}_{p,k}$ and $\text{PrivateShare}_{p,k}$. The clue to this question, we believe, offers a glimpse at China's policymakers' politico-economic preferences. Figure 2.4 shows a negative correlation between the pre-shock market level of privatization and the log value of the expected stimulus at the province $\times$ sector

[15]The details of our data matching are given in Technical Appendix B.1.1.

[16]As an example, let $\mathcal{K}$ be transportation and let (p,k) be railway equipment production in Guangdong province. The amount of stimulus received by transportation that goes to market (p,k) is jointly determined by stimulus received by all transportation sectors ($\text{stimulus}_{\mathcal{K}}$), and the relative size of railway equipment production in Guangdong in all transportation sectors ($\mathcal{K}$) between 2005 to 2007.

based on the CIC 2-, 3- and 4-digit industrial classifications.[17] It suggests that, on average, markets with lower prior privatization levels are more likely to receive a higher amount of (expected) stimulus. To confirm this intuition, we run a simple regression of the log value of expected stimulus on private shares. The results for all three sectoral levels are presented in the first column of all three panels in Table 2.4.

[Insert Figure 2.4 here] and [Insert Table 2.4 here]

We observe negative correlations across all three levels and statistically significant results at the 1% level for 3- and 4-digit level sectors. To check if this relationship is driven entirely by the selection of sectors in the stimulus program, i.e., the degree of privatization does not play a role once we control for sectors, we add a dummy variable for whether or not the given market belongs to one of the aggregate sectors listed in Table 2.3 to the regression, and the results are presented in the second column for all three panels. In addition, we control for province-fixed effects to address the concern that the stimulus program is allocated through a central-provincial bargaining process that absorbs any remaining correlations between expected stimulus and private shares. The results are presented in Column (3), where the negative correlations remain and are statistically significant.

The results in Table 2.4 show that conditional on the aggregated sector and province a market belongs to, on average, the expected stimulus in the given market is negatively correlated with the market's prior privatization degree. The implication is that the money from the stimulus program is more likely channeled through SOEs even after all the sectoral and geographical considerations, a result that is consistent with prior findings. We have hitherto assumed that the allocation of the stimulus funds is proportional to the relative size of the market in the aggregated sector in prior years. As robustness checks, we consider two alternative measures of exposure to stimulus. First, we ignore the actual stimulus amount and treat a market as being exposed to stimulus if its sector component falls under any aggregated sector. Under this definition, we use a violin-style plot to compare the

[17] We use the $\ln(X+1)$ transformation to deal with markets with zero expected stimulus.

distribution of private shares across markets over stimulus status (Appendix Figure B.1). Second, we assume that the funds are allocated only to the "big players" in each aggregated sector. That is, only the markets that are important enough get allocated funds from the stimulus program. Under this definition, we rank markets within their respective aggregated sector by their sizes and define a market as treated (received stimulus) if its relative size within the aggregated sector is above the median. We then plot the distribution of private revenue shares across markets by stimulus status (Appendix Figure B.2), similar to the first alternative measure. For both alternative measures, we find that private shares in markets that are exposed to the stimulus program are less concentrated around the higher end of the spectrum and more evenly spread out, implying that they are, on average, less privatized.

2.3 The Causal Impact of Stimulus Program on Firm Performance and Aggregate Outcomes

2.3.1 Firm-Level Impacts

2.3.1.1 Specification

We now introduce a triple difference (DDD) framework to estimate the causal impact of stimulus on SOEs and private firms. To do so, we compute, for each market at the province $\times$ sector level, the intensity of the stimulus package, measured by $\text{ExpectedStimulus}_{p,k}$ as defined in Equation 2.4. For our main specification, we focus on the most granular level of market (CIC 4-digit) as it offers the highest level of variation. Formally, we apply the following specifications to a set of firm-level outcomes in log terms, $Y_{i,p,k,t}$:

$$\begin{aligned} \ln(Y_{i,p,k,t}) =& \beta \ln(\text{ExpectedStimulus}_{p,k}) * \text{Post}_t * \text{SOE}_{i,2007} \\ &+ \gamma \text{Post}_t * \text{SOE}_{i,2007} + \mu_{p,k,t} + A_{i,t}\rho' + \alpha_i + \epsilon_{i,p,k,t}, \end{aligned} \tag{2.5}$$

where Post_t is an indicator for years after 2008, the year of the Global Financial Crisis when the fiscal stimulus was introduced. $\text{SOE}_{i,2007}$ is a dummy variable denoting the SOE status in

2007, $A_{i,t}$ represents a set of firm-level controls to account for time-varying covariates,[18] $\mu_{p,k,t}$ is a set of sector-year-province fixed effects. We also include α_i as firm fixed effects, and $\epsilon_{i,p,k,t}$ is the error term clustered at the sector-province level. We use a set of outcome variables in log form, including revenue, asset, export, employment, and three standard measures of total factor productivity (TFP): OLS, computed by running OLS on capital, labor, and materials; Olley & Pakes (1996), or OP, computed by following Yasar *et al.* (2008); and Levinsohn & Petrin (2003), or LP, computed by following Petrin *et al.* (2004). In the absence of stimulus, the differential impact between SOEs and private firms would be captured by γ, an estimate of falling demand induced by the Global Financial Crisis. However, the coefficient of our interest, β, measures the average elasticity of firm-level outcomes with respect to the expected intensity of the stimulus, which is the central focus of our theoretical framework.

While the Chinese government at all levels relies heavily on state-sponsored investments to drive economic growth, the central government's stimulus program accelerates investments planned for the future and potentially introduces new areas of investment. The identification assumption is that the allocation of the stimulus program, an explicit and direct response to the 2008 Global Financial Crisis, is an exogenous shock to firms in both the affected and unaffected markets, which is plausible given accounts by Naughton (2009). Moreover, our triple difference estimation strategy relies on a relatively weak common trend assumption. For example, recent advances in the study and application of triple difference estimator, led by Olden & Møen (2022), emphasize that one parallel trend assumption is sufficient for identification, so long as the bias that exists in one set of difference-in-differences is also present in the other set of difference-in-differences.[19] In our setting, the only parallel trend assumption required for the triple difference estimator given in Equation 2.5 to be identified translates into the following statement: The differential outcomes between SOEs and non-SOEs should trend similarly across different treatment statuses ($\text{ExpectedStimulus}_{p,k}$) in the absence of the shock (stimulus program). In other words, we can identify the differential

[18] In practice, we include only the log value of firm age, as other firm-level variables are likely affected by the stimulus program.

[19] For examples on recent literature that applies the results from Olden & Møen (2022), see Arabzadeh *et al.* (2024), Nocito *et al.* (2023) and Nikolov & Hossain (2023).

outcomes of SOEs as long as the relative performance of SOEs over non-SOEs remains the same across different markets without the shock.

In addition, we make the assumption that one of our main variables of interest $\text{ExpectedStimulus}_{p,k}$ is not capturing effects other than those from the stimulus program. To address this concern, we include market-year fixed effects to net out any market-level external shocks.[20] Finally, we may be concerned that the share of SOEs versus private firms post-2008 is driving the effect of the stimulus program. For example, a sudden influx of SOEs post-2008 in a given market might distort the interpretation of our findings for firms that existed in the market before the stimulus. To alleviate this concern, we fixed a firm's ownership type in 2007. The rationale is simple: If a firm was state-owned in 2007 but changed ownership status in 2010, then the stimulus program starting in 2008 will likely affect it in the same way as a state-owned firm that never went private. Consequently, firms appear only in the years after 2008 are dropped from the sample. We shall return to this point by investigating the impact of the stimulus program on privatization/nationalization in the aggregate-level analysis.

2.3.1.2 Results

Table 2.5 presents firm-level estimates. In the absence of stimulus, the post-2008 revenue and assets growths for SOEs, captured by $\hat{\gamma}$, are on average lower by about 7.47% and 15.8% respectively (Columns (1) and (2)) relative to non-SOEs, which suggests that SOEs in province $\times$ sector pairs that are less exposed to the stimulus suffer more from falling market demands induced by the Global Financial Crisis. As a response, these SOEs cut back (relatively) on employment (Column (4)) and experience stronger growth in exports (Column (3)). This implies that private firms are more sensitive to negative demand shocks from external markets (hence SOEs have higher growth in export) but could compensate for this by squeezing the market share of SOEs with limited fiscal support. Therefore, relative TFP growths of SOEs (Columns (5) to (7)) in those weakly supported markets continue to undergo restructuring.

[20] We also attempt to use firm-level export to predict the expected stimulus, and the result is not statistically significant.

[Insert Table 2.5 here]

The picture becomes starkly different once we turn to markets with more exposure to the stimulus. Since SOEs act as the agents channeling the investment funds from the stimulus program into the economy, the elasticity of firm outcomes to expected stimulus, captured by $\hat{\beta}$, is strong and highly significant (Columns (1) to (7)) other than on export and employee growth. For instance, conditional on the fixed effects, a ten percent increase in market-level expected stimulus translates into roughly 0.2% higher growth of assets for SOEs relative to non-SOEs post-2008 (Column (2)). The elasticity of TFP growth gives an estimate of about 0.05% to 0.08% (Columns (5) to (7)) for the same increase in expected stimulus. Altogether, the differential impact of stimulus on SOEs in comparison with private firms is a function of the expected stimulus targeted to a specific province $\times$ sector, which in turn depends on the prior degree of market-level privatization. Take revenue growth as an example. For those markets with an expected intensity above the 60^{th} percentile (conditional on receiving stimulus), the growth for SOEs is larger than that of private firms post-2008.

How does the intensity of expected stimulus affect the degree of firm-level privatization? To answer this important question, we rely on a simple measure, the private share of paid-in capital, $\text{PrivateRatio}_{i,p,k,t} = 1 - \frac{\text{StateCapital}_i}{\text{TotalCapital}_i}$, using available data whenever possible, where StateCapital_i here refers to the capital owned by the state within firm i. The results are presented in Column (8). Two remarkable results were discovered: First, in the absence of stimulus, the impact of falling external demand induces continued privatization of SOEs by a statistically significant rate of 6.46 percentage points, which indicates firm-level privatization of SOEs carries on in those markets that likely have a higher degree of prior privatization. Second, the elasticity of the private share of paid-in capital to the expected stimulus is a small and insignificant estimate. This strongly suggests that, despite political efforts to channel stimulus via SOEs, the package fails to reverse the continued trend in privatization at the firm level.

Earlier discussion suggests that although SOEs serve as the channels for the stimulus program, with markets having a lower degree of prior privatization receiving higher expected

stimulus, the overall impact on SOE outcomes can still vary significantly across markets. To test this intuition, we divide our sample into two groups: markets with a private share above (inclusive) or below the median within the same aggregated sector in 2007. For example, if a market (p, k) represents equipment production in Guangdong province, then all firms in that market are classified into one of two groups based on whether the railway equipment production sector k in Guangdong has a privatization level above or below the median across all markets within the aggregated transportation sector $\mathcal{K}$. We then apply the same empirical specification as in Table 2.5.

[Insert Table 2.6 here]

Table 2.6 presents the results. The estimated impact of falling demand on SOEs, in the absence of stimulus, is similar among those operating in both highly privatized and less privatized markets, with the exception of TFP outcomes (Columns (5) to (7), Panels A and B). However, the magnitudes of the coefficients are smaller in less privatized markets across the non-TFP outcome measures (Columns (1) to (4), Panels A and B). This suggests that SOEs, facing less competition from private firms, are less likely to be adversely affected by negative external demand shocks.

With the presence of stimulus, the results for non-TFP outcome measures remain generally similar, but the outcomes for TFP measures diverge significantly. Although the stimulus induces relative revenue growth for SOEs in both market types, the effect on asset growth is statistically significant only for SOEs in less privatized markets (Column (2)). The elasticity of expected stimulus with respect to export is negative for more privatized markets (though not statistically significant at the 5% level), suggesting that in markets with more exporters,[21] the stimulus program helps private firms recover faster in terms of exports. No such evidence is observed in less privatized markets.

The elasticity of TFP with respect to stimulus intensity shows a positive and significant response for SOEs in less privatized markets, compared to the negative and/or insignificant

[21]Non-SOEs generally export more than SOEs, see Table 2.1.

effects in more privatized markets. This indicates that the stimulus program enhances SOEs' productivity in markets with fewer private competitors, whereas in markets with more private competition, the stimulus does not affect SOEs' productivity.

Finally, although the effects of the stimulus program on firm-level privatization are negative and insignificant for both types of markets, at baseline (absent of stimulus), more privatized markets see a greater relative increase in firm-level privatization compared to less privatized markets. This suggests that privatization advances faster in more privatized markets even after 2008.

In summary, the regression results indicate that although SOEs in both more and less privatized markets generally respond positively to the stimulus program, those in less privatized markets experience relatively faster TFP growth compared to private firms following fiscal stimulus. This occurs despite the overall trend of increasing market-level privatization across all province-by-sector combinations. Consequently, these SOEs emerge as the primary beneficiaries of the stimulus. Essentially, the fiscal stimulus acts as a strategic government demand shifter, strengthening SOEs in less privatized markets and enhancing their power, even as market-level privatization progresses, regardless of policymakers' preferences.

2.3.2 Aggregate-Level Evidence

Since our measure of expected stimulus intensity is defined at the province × sector level, it is of interest to investigate the causal effect of the stimulus program on aggregate level outcomes. Formally, we first consider a continuous different-in-different (DiD) framework, making similar identification assumptions as we did before in investigating firm-level outcomes:

$$Y_{p,k,t} = \beta \ln(\text{ExpectedStimulus}_{p,k}) * \text{Post}_t + \mu_{p,k} + \nu_t + \epsilon_{p,k,t}, \tag{2.6}$$

where the continuous measure of $\text{ExpectedStimulus}_{p,k}$ characterizes the degree of market impacts. Fixed effects at the province × sector level, $\mu_{p,k}$, and year effect ν_t are included to absorb time-unvarying heterogeneity across markets and calendar cycles. The estimated coefficient, $\hat{\beta}$, captures the pooled average effect on aggregate level outcomes, to be defined

below, by changes in expected stimulus intensity.

How does prior market-level privatization affect the transmission effect of the stimulus package? To answer this important question of interest, we turn again to a DDD framework similar to firm-level specification:

$$\begin{aligned} Y_{p,k,t} =& \gamma \ln(\text{ExpectedStimulus}_{p,k}) * \text{Post}_t \\ &+ \beta \ln(\text{ExpectedStimulus}_{p,k}) * \text{PrivateShare}_{p,k,2007} * \text{Post}_t + \mu_{p,k} + \nu_t + \epsilon_{p,k,t}, \end{aligned} \tag{2.7}$$

where γ estimates the average effect of expected stimulus intensity in a purely state-owned market, and β, in this case (Equation 2.7), offers an estimate of the pass-through effect of stimulus via prior market-level privatization on aggregate outcomes.

This exercise takes into account several dependent variables. First, we consider productivity dispersions measured by what we call the performance gap, the TFP ratio based on the OP method between firms at the 99th and 50th percentile within a given market. Next, we check the performance ratio, which is defined as the revenue ratio between the median private firm and SOE for a given pair of (p, k). Then, the ratio of the number of private firms to that of SOEs, called the number ratio, offers another look at the aggregate-level privatization degree. This is followed by the private ratio, defined as the size-weighted average ratio of privately contributed capital across firms within the same market. Since these aggregate-level measures are inevitably affected by the dynamic process of creative destruction (Aghion & Howitt, 1992; Brandt *et al.*, 2012), driven by both SOEs and private firms, which are disrupted by the implementation of fiscal stimulus, we further consider the private (state) entry rate as the number of new private (state) firms over the total number of firms in each period. Finally, the private (state) exit rate is the number of firms in the market in the last period but not in the market in the current period over the total number of firms in the current period.

Note that, as a caveat, in our context, market exit, defined in our sense, does not necessarily imply abandoning the operation altogether. Rather, a firm may simply have switched its operating market by switching to another sector or relocating to a different province, and

it would be counted as an exit in the current market. Furthermore, despite the fact that the ASIF dataset has limitations in capturing entry and exit since its sampling process imposes a minimum threshold with regard to revenue, the comprehensive documentation of firm-level information offers our analysis a good benchmark against which further research may build on should a better dataset become available.

2.3.2.1 Impact of Stimulus on Aggregate Outcomes

Panel A of Table 2.7 reports results from Equation 2.6. The pooled average effects on the performance gap, as well as the performance ratio, turn out to be negative but statistically insignificant (Columns (1) and (2)). The stimulus program generate a narrowing effect in TFP dispersions across all types of firm as well as reduced revenue gaps between private firms and SOEs, indicative of distorted capital allocation.[22] For the ratio of the number of private firms to that of SOEs, a ten percent increase in expected stimulus on average decreases it by roughly 5 percentage points (Column (3)), with the effect being statistically significant at 5%.[23] We also see a slight drop in the weighted-average ratio of private capital (Column (4)).

[Insert Table 2.7 here]

Turning to market dynamics, the transmission of stimulus packages discourages the entry and exit activities of private firms (Columns (5) and (6)), although the magnitude is relatively small - a ten percent increase in expected stimulus decreases private exit rate by 0.2 percentage points. On the other hand, although SOEs have already been outnumbered by private firms, the stimulus packages significantly incentivize SOEs to enter the market (Column (7)). In addition, the number of SOEs is also whittled down (Column (8)), although the magnitudes are also small. Overall, policymakers appear to be effective in not only using

[22]However, in the theoretical analysis, we shall argue that the small price of capital misallocation is perfectly acceptable to policymakers who achieved the policy goal of cleansing inefficient private firms as well as SOEs from the market.

[23]Note that the number ratio is not restricted between 0 and 1.

SOEs channeling agents for the stabilization program but also marginally reshuffling the portfolio of SOEs.

Although the total effect of stimulus works to narrow the performance gap across all firms and between private and state firms (Panel A, Columns (1)-(2)), corresponding estimates in Panel B find that stimulus intensity amplifies existing TFP gap across all firms as well as revenue dispersions between private and state firms in more privatized markets. We interpret this result as follows: because SOEs are comparably inefficient and weak in markets that already experience a high degree of privatization, the injection of stimulus package merely compensates falling external demand with government support, which is captured by efficient private firms in those markets. Consistent with this interpretation, comparably less efficient private firms witness a drop in numbers in a lower degree of privatized markets (Panel B, Column (3)) with declining market shares (Panel B, Column (4)), whereas efficient private firms in a higher degree of privatized markets expand their overall market shares and increases in their numbers. Specifically, we estimate the cutoff level of the prior degree of market-level privatization to lie at 20^{th} percentile of all province $\times$ sector pairs for private revenue share to remain stable before and after the introduction of stimulus. Regarding the ratio of the number of private firms to SOEs, a ten percent increase in expected stimulus in a market with average level of privatization can expect a drop of 0.6 percentage points.[24] However, for markets with a privatization level above 91.4%, increase in expected stimulus will actually increase the relative number of private firms to state firms. Hence, while the stimulus program induces a reduction in the relative number of private firms in less privatized markets, they continue to dominate in already highly privatized markets post-2008. The same is qualitatively true for the ratio of privately contributed capital (Column (4)).

Moving on to entry and exit activities, we observe again that the effect of stimulus is polarized by the degree of market-level privatization. Columns (5) and (6) of Panel B report that private entry and exit rates fall in less privatized markets. Specifically, in markets beyond 18^{th} percentile of the prior degree of privatization, the total effect of the expected

[24]Given the average private share at the 4-digit level across markets in 2007 is 0.8857, conditional on other fixed effects, we calculate $-0.1967 + 0.2153 \times 0.8857 \approx -0.006$.

stimulus on private entry turns positive. Regarding exit rates, however, the total effect of the stimulus program, even for the highest possible degree of market-level privatization, turns out to be negative, suggesting that stimulus support for less competitive SOEs in those markets no more than compensates for falling external demands induced by the Global Financial Crisis. Furthermore, in those provinces × sector pairs that have higher private share, private firms faced increased entry and decreased exit rates that resulted in intensified competition. Falling in line with the above interpretation, the estimated effect on state entry and exit rates witnessed a healthy restructuring in less privatized markets (Panel B, Columns (7) and (8)), while inefficient SOEs in highly privatized markets were likely to degenerate into zombies because of stimulus support.

Taken together with the findings from above, our empirical results indicate that comparing the performance of SOEs and private firms necessitates an inquiry into the dynamic market structure under which these firms strategically interact. Using the pre-crisis market-level privatization as a continuous measure of the market structure, we identify a polarizing effect of fiscal stimulus on the dynamic interaction between SOEs and private firms such that while the stimulus package consolidates SOEs in less privatized markets with positive outcomes, the stabilization program also accelerates cleansing of private firms and hampers creative destruction of inefficient SOEs in markets that are already competitive and highly privatized.

2.3.2.2 Impact of Stimulus on Privatization Dynamics

In the previous section, we have documented the impact of the expected stimulus intensity on a number of aggregate outcomes concerning firm dynamics between SOEs and private firms. The question, however, is how this stabilization package affects the evolution of market-level privatization dynamics. We tackle this question of importance by exploring each component underpinning the change of privatized revenue within each province × sector using the variables defined and computed in the same way as in Section 2.2.2.[25] In

[25]For example, Stayers' $\text{Share}_{p,k,t} = \frac{\sum_i \mathbb{1}\{\text{Private Stayers}\}\text{Revenue}_{i,p,k,t}}{\text{TotalRevenue}_{p,k,t}}$ across all markets (p, k).

addition, we also consider the growth rate of private shares, defined as Private Growth = $\Delta\text{PrivateShare}_t/\text{PrivateShare}_{t-1}$.[26] The specifications we use are similar to Equation (2.6) and (2.7).

[Insert Table 2.8 here]

Panel A of Table 2.8 reports the results using Equation (2.6). As expected, we observe province × sector exposed to stimulus package to experience a weak decline (not statistically significant) in the growth of private revenues (Column (1)), with a ten percent increase in expected stimulus leading to a decrease of roughly 0.74 percentage points in the growth rate of private share. Inspecting Columns (2) to (5), the impact of stimulus does not appear to alter four major components of market-level privatization dynamics: (i) the revenue share of continuing private firms, (ii) the revenue share of entering private firms, (iii) the revenue share of those privatized SOEs, and (iv) the revenue share of exiting private firms. However, we suspect that the non-significant effect masks the polarizing market dynamics as discussed above. Interestingly, the revenue share of nationalized private firms post-stimulus experiences a statistically significant surge by 0.39 percentage points going from zero to the median level expected stimulus.[27] This implies that markets more exposed to the stimulus program either have a higher number of private firms (re)nationalized, or the (re)nationalized firms are larger on average (or both).

To verify the previously conjectured polarization hypothesis, we apply a DDD estimator using Equation 2.7 to investigate the heterogeneous effects of market-level privatization in channeling the stimulus package. Panel B of Table 2.8 reports our findings. Column (1) on private growth verifies our intuition. Specifically, we estimate the cutoff level of the prior degree of market-level privatization to lie at the 20^{th} percentile of all province × sector pairs, above which the effect of the stimulus on private revenue growth becomes positive. Returning

[26] Note that $\Delta\text{PrivateShare}_t = \frac{a_t}{y_t} - \frac{a_{t-1}}{y_{t-1}} = \frac{a_{t-1}}{y_{t-1}}\left[\frac{1+p_t}{1+g_t} - 1\right]$, where a_t is private sector size, y_t is total market size, p_t is the growth rate of private sector (revenue) and g_t is the growth rate of the whole market. Therefore, we can write Private Growth = $\Delta\text{PrivateShare}_t/\text{PrivateShare}_{t-1} = [\frac{1+p_t}{1+g_t} - 1]$.

[27] Going from zero stimulus to the median level increases the share of nationalized firms by $[\ln(91.6+1) - \ln(0+1)] \times 0.000871 \times 100 \approx 0.39$ percentage points.

to the five major components of market-level privatization dynamics above, the polarizing effects are salient with their respective cutoff percentiles at the 15^{th}, 19^{th}, 16^{th}, 13^{th}, and 20^{th} percentiles. This implies that in markets that are already highly privatization before the Global Financial Crisis, the stimulus program fails to reverse the general trend of continuing market-level privatization with increasing revenue share of continuing firms, entering private firms, privatized SOEs, exiting private firms, and decreasing nationalized private firms. The opposite situation holds for less privatized province $\times$ sector pairs. Consistent with earlier findings on polarized aggregate outcomes based on prior degrees of market-level privatization, our results indicate that while some degree of misallocation between SOEs and private firms may exist, firm dynamics play a more fundamental role in driving the evolution of market-level privatization.

2.4 Robustness Checks

We address a number of robustness issues in this section.

2.4.1 Firm-Level Results

[Insert Tables 2.9 and 2.10 here]

Alternative Definition of Exposure to Stimulus In the previous analysis, we implicitly assume that the stimulus program is allocated proportionally to the pre-GFC revenue share of province $\times$ sector pairs. To ensure our results are not biased by this particular way of measuring exposure, We replicate Tables 2.5 and 2.6 using an alternative definition of stimulus. That is, we define stimulus as a dummy and code a market as treated if its expected stimulus is greater than zero. In other words, we ignore the proportionality part of the expected stimulus and instead focus on whether a market is treated or not. Tables 2.9 and 2.10 report our results, showing qualitatively similar results. Under the alternative definition, exposure to stimulus raises all outcome measures. For example, being exposed to the stimulus increases SOE revenue by around 11% relative to private firms post-2008

(Table 2.9, Column (1)). Splitting the sample by prior degree of privatization, we observe similar results (Table 2.10). The stimulus program significantly raises the relative growth of TFP for SOEs in less privatized markets, in stark contrast to the results for more privatized markets (Columns (5) to (7), Panels A and B). These findings suggest that our main results are robust to the alternative definition of stimulus.

[Insert Figure 2.5 here]

Pre-trend Issues Our identifying assumption is that SOEs' relative performance over private firms remains the same across different markets without the stimulus shock. To check this condition, we interact each variable of interest with separate year dummies and plot the coefficients of the triple interaction terms in Figure 2.5.[28] The results are broadly consistent with our expectations.[29]

[Insert Tables 2.11 and 2.12 here]

External Demand Shock The stimulus program was introduced immediately as a direct response to the 2008 Global Financial Crisis, which may be continuously affecting Chinese firms' performance. As such, we might confound the effect of stimulus programs with lasting falling external demand shocks. To address this concern, we split our sample by a given market's (potential) exposure to trade and repeat our analysis on firm-level outcomes. First, we split our sample by whether the market is "coastal" or not. Specifically, we define a given province $\times$ sector as a coastal market if the province contains at least one coastal city. Table 2.11 reports our results. We do not observe a significant difference between the two sets of results other than with the effects on export, which is to be expected. Exports from SOEs located in coastal markets do not benefit from the stimulus, while the opposite

[28]The corresponding regression output table is presented in Table B.3 in the Appendix. We also present the plots for the regressions where we split the samples by prior degree of privatization in Figures B.3 and B.4, along with the regression output tables in Tables B.4 and B.5 in the Appendix. The same are done for the alternative definition of stimulus, with figures presented in Figures B.5, B.6 and B.7 and the regression output tables presented in Tables B.6, B.7 and B.8 in the Appendix.

[29]However, we do see minor pre-trend concerns with the results on revenue and assets—the coefficients seem to have been growing even before 2008, with the result on assets being more problematic.

is true for those in inland areas. Next, focusing on export more directly, we split our sample by export exposure. We rank the markets by their total export value in 2007 and split the sample into those that are above the median (more export-oriented) and below the median (less export-oriented). The results are shown in Tables 2.12. We see similar results with respect to export, but otherwise, there is no discernible difference between the two sets of samples. Overall, these results suggest that our results are not strongly affected by the direct consequences of the GFC, except for exports.

2.4.2 Aggregate-Level Results

Alternative Definition of Exposure to Stimulus Similar to firm-level specification, we explore whether or not our main results stand the test of an alternative definition of exposure to the stimulus. Tables 2.13 and 2.14 report the results using regression frameworks similar to Equation 2.6 and 2.7. Qualitatively, the estimated coefficients are broadly consistent with the key findings of polarized market outcomes in the main specifications. The same applies to results on privatization dynamics, with the general picture being robust by comparing Table 2.8 with Table 2.14.

[Insert Tables 2.13 and 2.14 here]

Parallel Trend Assumption Similar to the firm-level DDD regressions, the parallel trend assumption requires that the differential outcomes between more privatized markets and less privatized markets should trend similarly across different treatment status ($\text{ExpectedStimulus}_{p,k}$) in the absence of stimulus program. To check this condition, we again interact the main variables of interest with year-specific dummies. Figures 2.6 and 2.7 offer the results.[30] Inspecting Figures 2.6 and 2.7, we fail to observe discernible pre-trend issues over our aggregate outcomes of interest. Even in situations where the coefficients in the pre-2008 years seem to be continuously increasing or decreasing, the fact that either the coefficient in 2007 shows a reversal or the pre-trend or the post-2008 coefficients themselves

[30]The regression tables relegated to Section B.3.2.2 in the Appendix.

reverse the trend suggests that trending issues shall not be a concern because the estimated effect of the following years does not continue.

[Insert Figures 2.6 and 2.7 here]

External Demand Shock Similar to firm-level analysis, one worrying concern is the confounding effect of stimulus programs with lasting falling external demand shocks. To address this issue, we again split the sample by (potential) exposure to trade and repeated our analysis on aggregate outcomes. Tables 2.15 and 2.16 present results using a split of our sample into the coastal and inland regions. We do not observe significant changes in either the sign or the magnitude of the coefficients.[31] To further verify that GFC-induced lasting falling external demand shall not alter our key findings, Tables 2.17 and 2.18 report results based on a splitting of our sample by the intensity of how export-oriented the given market is. We again do not find any significant changes in either the sign or the magnitude.[32] Overall, the above results suggest that our aggregate level outcomes are robust to alternative definitions of stimulus, pre-trend concerns, and the potential influence of the GFC itself.

[Insert Tables 2.15, 2.16, 2.17 and 2.18 here]

2.5 Theoretical Mechanisms: A Political Economy Framework

Our empirical findings report a strong presence of the polarized effect of the stimulus program, depending on the prior degree of privatization. To rationalize the empirical evidence and relate our findings to policymakers' politico-economic considerations, in this section, we develop a political economy model to illustrate how the party-state's objective function affects the dynamic interaction between SOEs and private firms. In contrast to the misallocation literature (Bai *et al.*, 2016; Cong *et al.*, 2019), which appears to suggest that China's policymakers fail to take into account "unintended" costs of the stimulus program,

[31] With the minor exception of the effect on private stayers' share for coastal regions (Table 2.16, Panel A, column (2)), the effects are now statistically insignificant.

[32] The only exception is performance ratio in Table 2.17, Panel B, column (2).

our framework builds on models of heterogeneous firms (Melitz, 2003; Chen *et al.*, 2019a), which allows us to explicate the effect of changing fiscal support on the dynamics between SOEs and private firms under varying market structures conditioned by the prior degree of market-level privatization. In doing so, we are capable of deepening our understanding of shifting state strategy regarding SOE reforms.

2.5.1 Model Setups and Assumptions

The representative consumer exhibits a CES utility function over a continuum of goods indexed by ω, i.e., $U = [\int_{\rho\in\Omega} q(\omega)^{\rho} d\omega]^{\frac{1}{\rho}}$. Set Ω represents the mass of available goods. Since $0 < \rho < 1$, the elasticity of substitution between any of two goods is $\sigma = \frac{1}{1-\rho} > 1$. With an aggregate good $Q \equiv U$, the associated aggregate price $P = [\int_{\rho\in\Omega} p(\omega)^{1-\sigma} d\omega]^{\frac{1}{1-\sigma}}$. For each variety ω, the resulting optimal quantity of consumption and expenditure are, respectively, $q(\omega) = Q[\frac{p(\omega)}{P}]^{-\sigma}$ and $r(\omega) = R[\frac{p(\omega)}{P}]^{1-\sigma}$, where $R = PQ = \int_{\rho\in\Omega} r(\omega) d\omega$ represents the aggregate expenditure.

A continuum of firms (SOEs or private firms) choosing to supply a different product variety ω. The only factor of production is labor, inelastically supplied at an aggregate (industrial) level of L. The production technology is a constant marginal cost function with a constant fixed cost F: to produce q, the labor input required is q:$l = q/\phi + F$. We normalize the wage bill to be one. In face of a constant elasticity σ demand curve, a firm with productivity ϕ maximizing profit would leads to the following pricing rule $p(\phi) = (\rho\phi)^{-1}$, revenue $r(\phi) = R(P\rho\phi)^{\sigma-1}$, and profit $\pi(\phi) = r(\phi)/\sigma - F$. The key innovation of the model is to introduce a wedge between fixed costs F incurred by SOEs and private firms once the production process begins. Specifically, we assume that the intensity of government subsidy is $\tau > 0$.[33] As such,

$$F^{SOE} = (1-\tau)F^{POE}. \tag{2.8}$$

[33]We shall discuss that the distinction between a highly supported industry τ^H and a weakly supported industry τ^L where $1 > \tau^H > \tau^L > 0$, which are chosen endogenously via a government's optimization program, translates a difference between low and high degree of market-level privatization, and in turn is correlated with the intensity of expected stimulus intensity.

There is a large potential pool of prospective entrants (SOEs and private firms) into this industry. Before entry, with probability χ, the potential entrant is designated as an SOE; with probability $1-\chi$, a private firm. The inverse of χ, $\frac{1}{\chi}$, can thus be interpreted as a measure of (*ex-ante*) privatization intensity. Connecting with our regression framework, χ can be proxied by the market-level privatization level, $\text{PrivateShare}_{p,k,2007}$.

We consider two plausible scenarios of productivity draw among these two kinds of firms. In the benchmark case, there is no difference in *ex-ante* productivity between SOEs and private firms; the c.d.f and density of productivity for both kinds of firms are $G(\phi)$ and $g(\phi)$. We also consider a case where the productivity of private firms commands an advantage over that of SOEs because the latter need to shoulder more social burdens under multi-tasking incentives (Bai *et al.*, 2006) than private firms.[34] Suppose the c.d.f (density) of productivity of SOE and private firms are $G^{SOE}(\phi)$ ($g^{SOE}(\phi)$), and $G^{POE}(\phi)$ ($g^{POE}(\phi)$), respectively. We assume that the productivity of private firms exhibits a first-order stochastic dominance (FSD) over that of SOEs:

$$\forall \bar{\Phi} \geq 0, \quad G^{POE}(\bar{\Phi}) \leq G^{SOE}(\bar{\Phi}). \tag{2.9}$$

Consider productivity cutoffs for SOE and private firm: $\underline{\phi}^{SOE}$ and $\underline{\phi}^{POE}$ in the benchmark case.[35] Using the conditional distribution of $g(\phi)$ upon entry and (inverse) privatization ratio χ, the average productivity level $\tilde{\phi}$ is function of cutoff level of both $\underline{\phi}^{SOE}$ and $\underline{\phi}^{POE}$:

$$\tilde{\phi}(\underline{\phi}^{SOE}, \underline{\phi}^{POE}) = [\frac{\chi}{1-G(\underline{\phi}^{SOE})} \int_{\underline{\phi}^{SOE}}^{\infty} \phi^{\sigma-1} g(\phi) d\phi + \frac{1-\chi}{1-G(\underline{\phi}^{POE})} \int_{\underline{\phi}^{POE}}^{\infty} \phi^{\sigma-1} g(\phi) d\phi]^{\frac{1}{\sigma-1}}. \tag{2.10}$$

[34] Consistent with empirical findings comparing the performance of SOEs with that of private firms (see Ljungqvist *et al.* (2015) for example), our empirical work also finds support of average performance gaps between SOEs and private firms.

[35] The discussion of FSD case is left in the Appendix.

The revenue of the average productivity firm:

$$r(\tilde{\phi}) = \begin{cases} r^{SOE}(\tilde{\phi}) = [\frac{\tilde{\phi}(\underline{\phi}^{SOE}, \underline{\phi}^{POE})}{\underline{\phi}^{SOE}}]^{\sigma-1} r(\underline{\phi}^{SOE}), & \text{for SOEs;} \\ r^{POE}(\tilde{\phi}) = [\frac{\tilde{\phi}(\underline{\phi}^{SOE}, \underline{\phi}^{POE})}{\underline{\phi}^{POE}}]^{\sigma-1} r(\underline{\phi}^{POE}), & \text{for private firms.} \end{cases}$$

The revenue of the average productivity firm (also pinning down the ZCP conditions):

$$\pi(\tilde{\phi}) = \begin{cases} \pi^{SOE}(\tilde{\phi}) = [\frac{\tilde{\phi}(\underline{\phi}^{SOE}, \underline{\phi}^{POE})}{\underline{\phi}^{SOE}}]^{\sigma-1} \frac{r(\underline{\phi}^{SOE})}{\sigma} - (1-\tau)F, & \text{for SOEs;} \\ \pi^{POE}(\tilde{\phi}) = [\frac{\tilde{\phi}(\underline{\phi}^{SOE}, \underline{\phi}^{POE})}{\underline{\phi}^{POE}}]^{\sigma-1} \frac{r(\underline{\phi}^{POE})}{\sigma} - F, & \text{for private firms.} \end{cases}$$

Since under zero cutoff profit (ZCP) condition, $\pi^{SOE}(\underline{\phi}^{SOE}) = 0$, $\pi^{POE}(\underline{\phi}^{POE}) = 0$. Then $r(\underline{\phi}^{SOE}) = \sigma(1-\tau)F$ and $r(\underline{\phi}^{POE}) = \sigma F$. As such, $\underline{\phi}^{SOE} < \underline{\phi}^{POE}$ when $1 > \tau > 0$. Given a firm of average productivity $\tilde{\phi}$, $\pi^{SOE}(\tilde{\phi}) = (1-\tau)F[(\tilde{\phi}/\underline{\phi}^{SOE})^{\sigma-1} - 1] \equiv (1-\tau)Fk(\underline{\phi}^{SOE})$ for a SOE, and $\pi^{POE}(\tilde{\phi}) = F[(\tilde{\phi}/\underline{\phi}^{POE})^{\sigma-1} - 1] \equiv Fk(\underline{\phi}^{POE})$ for a private firm, where $(\tilde{\phi}/\underline{\phi}^{SOE})^{\sigma-1} - 1 = k(\underline{\phi}^{SOE})$ and $(\tilde{\phi}/\underline{\phi}^{POE})^{\sigma-1} - 1 = k(\underline{\phi}^{POE})$.

In each discrete time period, a potential entrant pays entry cost f_e measured in labor units and realizes the productivity draw. The entering firm stays upon receiving positive profit or exits immediately without production. For continuing firms, the exogenous exit rate is δ.[36] For the sake of simplicity by considering zero time discounting case,[37] the value function of each firm with productivity ϕ is thus determined by $v(\phi) = \max\{0, \sum_{t=0}^{\infty}(1-\delta)^t \pi(\phi)\} = \max\{0, \frac{1}{\delta}\pi(\phi)\}$. Since all SOEs beyond $\underline{\phi}^{SOE}$ and all private firms beyond $\underline{\phi}^{POE}$ are making positive profits, the present value of the average profit flow: $\tilde{v} = \sum_{t=0}^{\infty}(1-\delta)^t \tilde{\pi}$, which depends on cutoff value conditional on firm type. Define the net value of entry as v_e with a

[36] Prior to the Global Financial Crisis, this modeling assumption is plausible for SOEs and private firms. After the stimulus package and deepening privatization, the exit rate for SOEs and private firms cannot be assumed to be the same. We pick this point in the application of our model by discussing the implications when $\delta^{SOE} < \delta^{POE}$.

[37] This does not affect the substance of our argument.

fixed entry cost f_e across SOEs and private firms:

$$v_e = \begin{cases} v_e^{SOE} = [1 - G^{SOE}(\underline{\phi}^{SOE})]\frac{\tilde{\pi}}{\delta} - f_e, & \text{for SOEs;} \\ v_e^{POE} = [1 - G^{POE}(\underline{\phi}^{POE})]\frac{\tilde{\pi}}{\delta} - f_e, & \text{for private firms.} \end{cases} \tag{2.11}$$

Set $v_e = 0$ leads to free entry (FE) conditions for SOEs and private firms: $\pi^{SOE} = \frac{\delta f_e}{[1-G^{SOE}(\underline{\phi}^{SOE})]}$, $\pi^{POE} = \frac{\delta f_e}{[1-G^{POE}(\underline{\phi}^{POE})]}$. We summarize the above discussions as a lemma, which explains why empirical studies (e.g., Lardy (2014)) often report that the productivity level of SOEs is lower than that of private firms due to government support.

Lemma 2.5.1. *(ZCP condition) Given the average productivity level $\tilde{\phi}(\underline{\phi}^{SOE}, \underline{\phi}^{POE})$, the profit of a SOE and a private firm at their respective cutoffs is zero: (i) The ZCP conditions for SOEs and private firms are: $\pi^{SOE} = (1-\tau)Fk(\underline{\phi}^{SOE})$ and $\pi^{POE} = Fk(\underline{\phi}^{POE})$; (ii) The presence of subsidy τ results in a misallocation wedge such that $\underline{\phi}^{SOE} < \underline{\phi}^{POE}$; (iii) As subsidy τ increases, the misallocation wedge $|\underline{\phi}^{POE} - \underline{\phi}^{SOE}|$ intensifies; (iv) Against the benchmark case, FSD condition lowers $\underline{\phi}^{POE}$, and as such the misallocation wedge $|\underline{\phi}^{POE} - \underline{\phi}^{SOE}|$ attenuates.*

Empirically, the above lemma explains the rising trend of market-level privatization (Figure 2.1 and Figure 2.3) as the policymakers encourage neck-to-neck competition between SOEs and private firms by drastically cutting back subsidies in less strategic industries following the accession into the WTO. Consequently, prior to the Global Financial Crisis and the introduction of the stimulus program, the aggregate outcome and each component underpinning the market-level privatization dynamics, thanks to the successful channeling of the party-state's reform strategy (Xiao, 2023).

2.5.2 Equilibrium Solutions

In the (ϕ, π) space, we can characterize the existence and uniqueness of equilibrium by intersecting FE conditions and ZCP conditions for SOEs and private firms.[38] Because the

[38]This can be seen by verifying the monotonicity of FE and ZCP conditions.

differentiated shape of $G^{SOE}(\phi)$ and $G^{POE}(\phi)$ is governed by FSD condition, we further explore two situations: (a) the effect of FSD is either weak or non-existent or (b) the effect of FSD is strong. Figure 2.8 graphically characterized our equilibrium solutions. The FE and ZCP conditions intersect at point A for SOEs in Panel A of the benchmark case. The equilibrium condition for private firms intersects at point B. As the effect of FSD strengthens but still does not yet dominate, the FE condition begins to rotate leftward, reaching a new equilibrium point C for private firms. In either case, $\phi_*^{SOE} < \phi_*^{POE}$. Panel B discusses the case where the effect of FSD is dominating. Notice that the FE condition for private firms swings leftward to such an extent that $\phi_*^{SOE} > \phi_*^{POE}$. In both cases, the average profit in the equilibrium for private firms is higher than that of SOEs.

[Insert Figure 2.8 here]

We discuss how the number of SOEs (M^{SOE}) and private firms (M^{POE}) can be determined endogenously in this specific industry within our equilibrium analysis, where a total number of firms $M = M^{SOE} + M^{POE}$. The economy-wide total revenue R must equal the total payment to label L:

$$R = L = M^{SOE}\sigma[\tilde{\pi}^{SOE} + (1-\tau)F] + M^{POE}\sigma[\tilde{\pi}^{POE} + F]. \tag{2.12}$$

Since the total mass M_e of new entrants in every period needs exactly replaces the mass of δM of incumbents hit by bad luck, a successful entering SOE or private firm replaces another SOE and a private firm: $\delta M^{SOE} = \chi M_e[1-G(\underline{\phi}^{SOE})]$ and $\delta M^{POE} = (1-\chi)M_e[1-G(\underline{\phi}^{POE})]$. Using FC condition for both SOEs and the private firm:

$$\frac{M^{SOE}}{M^{POE}} = \frac{\chi}{1-\chi}\frac{1-G(\underline{\phi}^{SOE})}{1-G(\underline{\phi}^{POE})} = \frac{\chi}{1-\chi}\frac{\tilde{\pi}^{POE}(\phi_*^{POE})}{\tilde{\pi}^{SOE}(\phi_*^{SOE})}. \tag{2.13}$$

This equation implies that the ratio of the number of SOEs to that of private firms depends on the prior degree of privatization intensity and the average productivity profit ratio of SOEs to private firms. From Equations (2.12) and (2.13), the number of SOEs and private

firms, respectively, in this economy:[39]

$$M^* = \begin{cases} M^{SOE} = \frac{L}{\sigma}[\frac{1}{\chi}\tilde{\pi}^{SOE} + F(\frac{1-\chi}{\chi}\frac{\tilde{\pi}^{SOE}}{\tilde{\pi}^{POE}} + (1-\tau))]^{-1}, & \text{for SOEs;} \\ M^{POE} = \frac{L}{\sigma}[\frac{1}{1-\chi}\tilde{\pi}^{POE} + F(\frac{\chi}{1-\chi}\frac{\tilde{\pi}^{POE}}{\tilde{\pi}^{SOE}}(1-\tau) + 1)]^{-1}, & \text{for private firms.} \end{cases} \tag{2.14}$$

Proposition 2.5.1. *(Equilibrium solution and welfare analysis) In steady state equilibrium, both ZCP and FE conditions hold for SOEs and private firms: (i) When the effect of FSD does not dominate,* $\phi_*^{SOE} < \phi_*^{POE}$*; (ii) When the effect of FSD dominates,* $\phi_*^{SOE} > \phi_*^{POE}$*; (iii) An increase in subsidy* τ *increases the ratio of number of SOEs to that of private firms,* $\frac{M^{SOE}}{M^{POE}}$*; (iv) An increase in privatization intensity* $1/\chi$ *decreases the ratio of number of SOEs to that of private firms,* $\frac{M^{SOE}}{M^{POE}}$*.*

As such, we can summarize our discussion in proposition 2.5.1 on the equilibrium cutoff and average profit of SOEs and private firms and how an aggregate number of SOEs and private firms arise endogenously in a steady-state situation. In addition to subsidy τ as an economic instrument guiding firm dynamics across SOEs and private firms, the party-state could, in order to serve its politico-economic objectives, manipulate firm dynamics between SOEs and private firms via targeted state guidance policy on firm entries (e.g., χ) to specific industries. Our previous finding on the evolution of market-level privatization (for example, Table 2.2) speaks for the theoretical mechanisms discussed here.

2.5.3 Applications to Policy Context: The Political Calculation of Economic Stimulus in Relation to SOE Reform

To understand the SOE reform strategy before and after the economic stimulus, we rationalize policymakers' behavior from an optimization program via the politico-economic objective of the party-state. Given a fixed number of industry n,[40] each of which has varied strategic

[39] We also discuss the equilibrium the aggregate price index and aggregate welfare from the determination of aggregate number of firms in the appendix.

[40] We assume each industry operates as the model economy discussed before beside difference in exogenous parameters.

value governed by the equilibrium cutoff productivity level of SOE ϕ_*^{SOE}, the policymaker could choose a variety of instruments $(\tau_i)_{i=1}^n$ within given resource pool, taking into account the economic distortion caused by misallocation of resources between SOEs and private firms. As such, the political economy problem underpinning the SOE reform framework by the party-state is governed by the following optimization program:

$$\max_{(\tau_i)_{i=1}^n} \underbrace{\sum_{i=1}^{n} S_i[\phi_*^{SOE}(\tau_i)]}_{\text{industrial strategic value}} - \lambda \underbrace{\sum_{i=1}^{n} M_i(\tau_i)^{SOE}\tau_i}_{\text{subsidy cost}} - \underbrace{\sum_{i=1}^{n} \mu_i[\phi_*^{POE}(\tau_i) - \phi_*^{SOE}(\tau_i)]}_{\text{misallocation cost}} \tag{2.15}$$

subject to resource constraint $\sum_{i=1}^n M_i(\tau_i)^{SOE}\tau_i = G$.[41] λ denotes the weight on subsidy cost from the policymakers' perspective. The weights $(\mu_i)_{i=1}^n$ on the cutoff gap within each industry, as a measure of misallocation, can be adjusted by the government. From the prior discussion, we know that in a specific industry i under steady-state equilibrium, M_i^{SOE}, ϕ_*^{POE}, ϕ_*^{SOE} can all be endogenously determined. As a result, the sequence of $(\tau_i)_{i=1}^n$ fully characterizes both industry-level firm dynamics and the coexistence of SOEs and private firms within a specific industry from the policymakers' politico-economic preference and maps prior level of ex-ante (inverse) privatization level χ_i, subsidy intensity $(\tau_i)_{i=1}^n$ into industry-level heterogeneity, captured by the prior degree of market-level privatization in our empirical analysis.

From the accession into the WTO until the outbreak of global financial and the introduction of economic stimulus, the policymakers continued an SOE reform strategy known as "grabbing big and loosening the small," which allowed a sharp decline in the ratio of $\frac{M^{SOE}}{M^{POE}}$. Riding on the privatization wave while executing this strategy, the policymaker cut back tariffs by a significant amount (Cui & Li, 2023) and decreased subsidies τ to targeted industries, in addition to radical closures of profit-losing SOEs (e.g., a decrease in χ). As a result, the performance of SOEs catches up with that of private firms, which helps reduce the fiscal burden on the party-state, $\sum_{i=1}^n M_i(\tau_i)^{SOE}$. To see this mechanism within our framework, independent of the effect of FSD, this can be represented by bringing ZPC for SOE closer to

[41] The number of SOEs in industry i, $M_i(\tau_i)^{SOE}$ is endogenous determined by Equation (2.14).

ZPC for POE, increasing the equilibrium cutoff value of ϕ_*^{SOE} while reducing misallocation cost, an obvious objective of the party-state (Panel A, Figure 2.9). However, the equilibrium cutoffs for private firms, depending on the effect of FSD, might be described by either Panel A or B. Due to the party-state's optimization problem, the subsidy intensity shall be lower in a given industry with a more substantial effect of FSD and a low industrial strategic value. Consequently, the reform period before the stimulus witnessed a rapid expansion in the number and revenue share of private firms in a number of industries, particularly those with low strategic values.

[Insert Figure 2.9 here]

Following the Global Financial Crisis, the political calculation of economic stimulus can be represented by a shift in policymaker preference and increased subsidy intensity as the party-state committed more fiscal resources G in stabilizing the economy using SOEs as channeling agents, as supported in Table 2.5 of our empirical analysis. Probabilistically speaking, the policymakers selected SOEs in less privatized industries (larger χ) with higher strategic values (larger S_i) (see Table 2.4 and our discussion on the relationship between market-level privatization and expected stimulus intensity). Moreover, the policymaker realized that after decade-long SOE reforms with radical downsizing and bankruptcy policy, the exit rate δ for exiting SOEs and private firms began to diverge (Columns (5) to (8), Table 2.7).[42] The combined effect of more substantial stimulus and divergent exit rates for existing SOEs helps explain why, in some supported industries, the average productivity of SOEs continued to experience improvement (Panel B, Figure 2.9), which is empirically documented by Columns (5) to (7), Panel B of Table 2.6.

Proposition 2.5.2. *(SOE Reform Strategy and Political Calculation of Economic Stimulus) The party-state's SOE reform strategy is contextual-dependent and industry-specific, which leads to a variation of firm dynamic patterns among SOEs and private firms. The specific reform package and policy outcome are summed up in Table 2.19.*

[42] The endogenous evolution of $\frac{M^{SOE}}{M^{POE}}$ induces a departure from the original assumption that the exogenous exit rate for SOEs and private firms is the same. The policymaker can choose, to some degree, the exit rates for SOEs, δ^{SOE} - another policy instrument of the party-state.

How about an observed decrease in average productivity for private firms within some industries? Traditional analysis tends to focus on the negative externality of stimulus programs and emphasizes the effect of "crowding out".[43] We question this simple explanation by applying the party-state's politico-economic objective function and discussions of equilibrium firm dynamics. Consider an industry with strong FSD among private firms (Panel B of Figure 2.8). Further, assume that policymakers design this industry as having low strategic value (low S_i). In this case, following the policymaker's politico-economic calculation (Equation 2.15), the number of SOEs falls with an increased level of privatization degree. As this industry becomes super-competitive among private firms (e.g., A further leftward shift of FE for private firms), the equilibrium cutoff of ϕ_*^{POE} continues to drop, which might lead to a decline of average productivity within this industry. As a result, the observed pattern of increased privatization and declining average productivity for private firms can be rationalized by vibrant business dynamism and fierce competition in industries with low strategic values (Panel B of Table 2.7).[44]

[Insert Table 2.19 here]

We summarize our discussion of SOE reform strategies concerning economic stimulus in Proposition 2.5.2. Table 2.19 describes China's SOE reform strategies between 2001 and 2011, which are contextually dependent on economic stimulus post-financial crisis and explicitly tailored to the industry level of privatization degree. Before economic stimulus, the SOE reform strategy of enhancing average productivity of ϕ_*^{SOE} depends on cutting back subsidies and consolidating SOEs in several strategically important industries. Following economic stimulus, SOE reform strategy was repackaged into a combination of increasing subsidy and decreasing exit rates, the effect of which resulted in an improved equilibrium ϕ_*^{SOE}.

Comparing proposition 2.5.2 with results from misallocation literature (for example, Cong *et al.* (2019)), we suggest that the misallocation view in the political economy sense appears

[43] See, for example, Bai *et al.* (2016).

[44] See appendix for a more technical discussion.

to imply that the policymakers were making "irrational" policy choices over the design of economic stimulus package by enlarging the unintended efficiency gap between SOEs and private firms. However, in our framework, the policymakers' chosen policy instruments could be rationalized by the structure of the politico-economic objective function as the overall privatization degree deepens (illustrated by Figure 2.2). Rather than ignoring misallocation costs, the policymakers needed to balance that cost generated by the government's policy maneuver as an acceptable political sacrifice in the service of the party state's overall politico-economic objective, which assigns different strategic levels across industries. Xiao (2023), for example, stresses the capability of the party-state to instrumentalize its privatization program in consistent realignment with the overall politico-economic objective function. Furthermore, our analysis shows that even if the government is capable of financing a more considerable amount of support in the economy through sustained growth, the intensity of FSD for private firms' productivity distribution could continue the process of privatization across industries, as measured by the market share of private firms, independent of the policymakers' preference reassignments.

2.6 Discussion and Conclusion

The 2008-9 Global Financial Crisis has made a lasting economic impact on the world economy with remarkable political consequences within which China's policymakers played an influential role, but the nature and outcomes of their stabilization packages and strategic considerations are still under-explored. The absence of rigorous econometric analysis on China's stimulus package's economic and political consequences represents a major vacancy in the literature. Using a firm-level dataset in China between 2004 and 2013, we address this gap by investigating the impact of post-crisis economic stimulus on firm performance between SOEs and private firms conditional on pre-shock market-level privatization. Focusing on aspects of firm dynamics, we also examine the impact of China's stabilization package on the aggregate province-industry level.

Our results show that the allocation of stimulus funds is inversely related to the pre-shock

degree of privatization, clearly driven by political calculations. Using a triple difference (DDD) specification framework, we find that a ten percent increase in market-level expected stimulus translates into roughly 0.2% higher growth of assets for SOEs relative to non-SOEs post-2008. However, conditional on the prior degree of market-level privatization, the total impact on SOEs of the stimulus can be polarized. In markets with a high degree of privatization, the intensity of firm-level privatization of SOEs continues despite the impacts of stimulus programs, whereas SOEs facing lesser competition from private firms are less likely to be squeezed by negative external demand shocks given stimulus support. Furthermore, our empirical specification reveals that firm-level impacts translate into aggregate-level outcomes: the interaction between SOE and private firms experiences a healthy restructuring in less privatized markets, while in highly privatized markets, insufficient SOEs, pressured by dynamic entries from private firms, are likely to degenerate into zombies because of stimulus support. In the end, the strength of our findings passes a number of robustness checks on our empirical strategy.

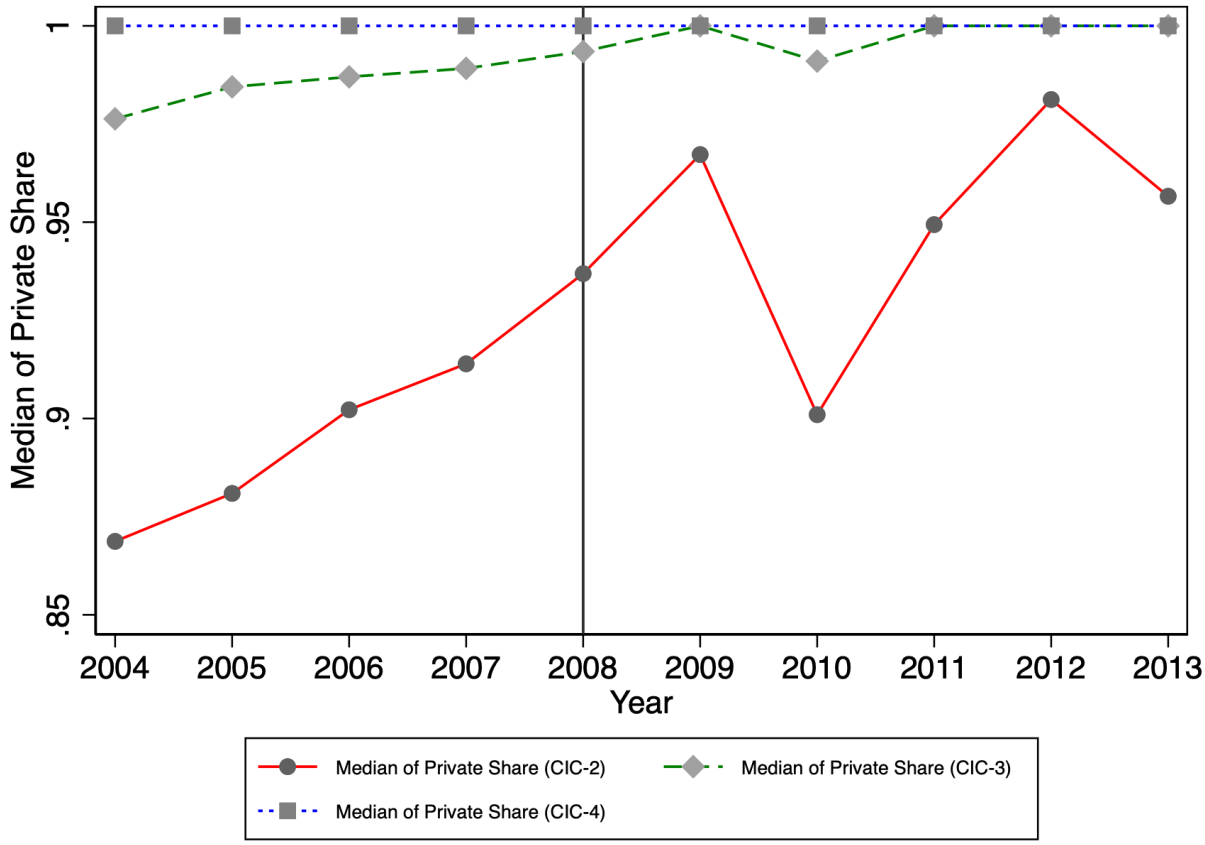
In discussing the theoretical mechanism aided by a political economy model, we argue that the SOE reform strategy underpinning the Chinese model of "socialist market economy" places firm dynamics between SOEs and private firms at the center of market transition, which relates privatization at the firm level with that at the market level. Reflecting on the collapse of the Soviet model of socialism, Stiglitz (1996, p.14) noted, "At the core of the failure of the socialist experiment is not just the lack of property rights. Equally important were the problems arising from lack of incentives and competition, not only in the sphere of economics but also in politics." Equally shocked by the failure of their socialist mentors, China's policymakers - pupils of Marxism and late-comers in the development game - were pioneering alternative political economy experiments within the framework of market socialism, instrumentalizing healthy firm dynamics between SOEs and private firms via Schumpeterian competition to implement the marketization and privatization programs. Whether or not this SOE reform strategy in connection with the idiosyncratic privatization program has led to inefficiency or other unintended consequences, the primary message of our empirical analysis is such that the party-state's development strategy is contextually

dependent and industry-specific with limited bindings of socialist ideology.[45]

Furthermore, the politico-economic objective of China's policymaker aims to score high in advancing both the state and the private economy, "state advances, private economy advances." As such, departing from the misallocation framework, our interpretation also contrasts with analysis based on "state withdraws, private economy advances," which purports that the generation of private wealth, through "privatization as a process of endogenous institutional change" (Lin, 2017, p.5), results not from conscious state strategy but from unintended policy changes of earlier reforms, and therefore have crippled China's governing institutions, leading to its effective transmission of macroeconomic policies due to erosion of state capacities (Pei, 2016). What is more, our framework is also distinct from the traditions of state-capitalism (Naughton & Tsai, 2015; Leutert, 2016; Johansson & Feng, 2016), or the notion of "state advances, private economy withdraws," which stresses a shift in reform strategy following the Third Plenary of the 18^{th} National Congress of Chinese Communist Party (CPC), which is heavily affected by the failure of neoliberal political economy amid Global Financial Crisis, as the party-state began to wield more aggressively its political instruments to toe private firms to the official line. Whether or not China's policy landscape has retreated into either of these two policy corners merits further research.

[45]Drawing evidence from China, our findings deepen the insights from Aghion & Roulet (2014) in several directions: (i) A smart party-state guiding a socialist market economy shall and has indeed placed firm dynamics between SOEs and private firms at the core of the reform strategy and industrial upgrading program; (ii) The design of government's counter-cyclical program during an economic recession not only needs to be more supply-side than the Keynesian aggregate demand management approach but also more Schumpeterian in essence by taking into account of misallocation gaps and the party-state's politico-economic objectives.

Figure 2.1: The Evolution of Degree of Privatization, 2004-2013



Note: Private shares are computed by the ratio of aggregated revenue of private firms in a given sector-province pair over the aggregated revenue of all firms in the same pair. Percentiles are computed by ranking private shares for each year. This process is repeated for three levels of sector classification according to the Chinese Industrial Classification (CIC), at the 2-, 3- and 4-digit level. Ownership of firms are defined by their registered type in the data for a given year. *Source: Authors' own calculations using ASIF data.*

Figure 2.2: Evolution of Privatization Share: A Firm Dynamics Perspective

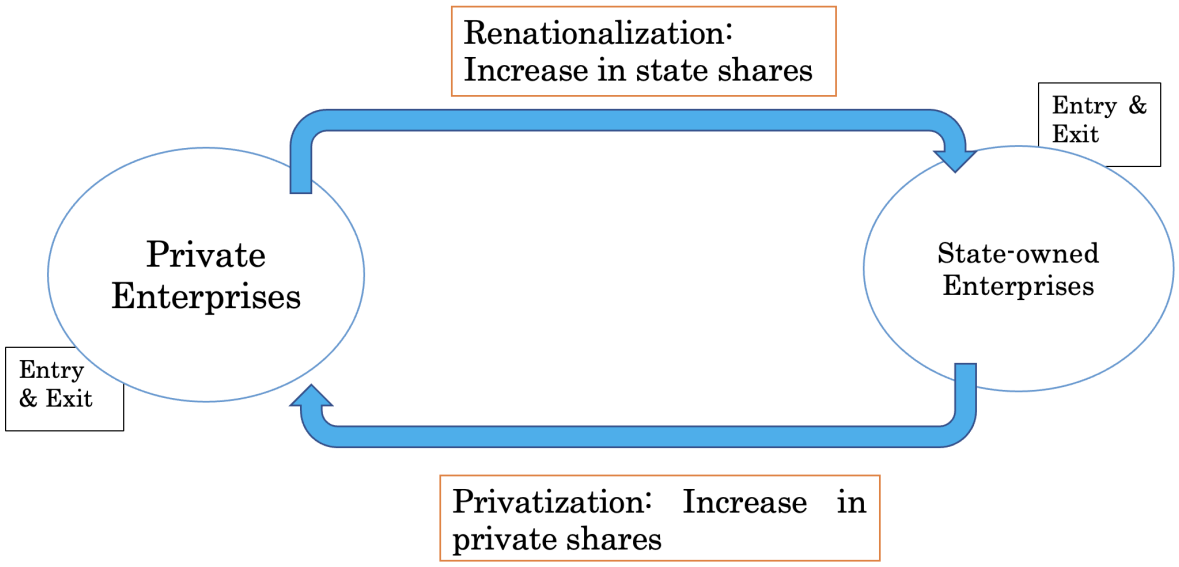
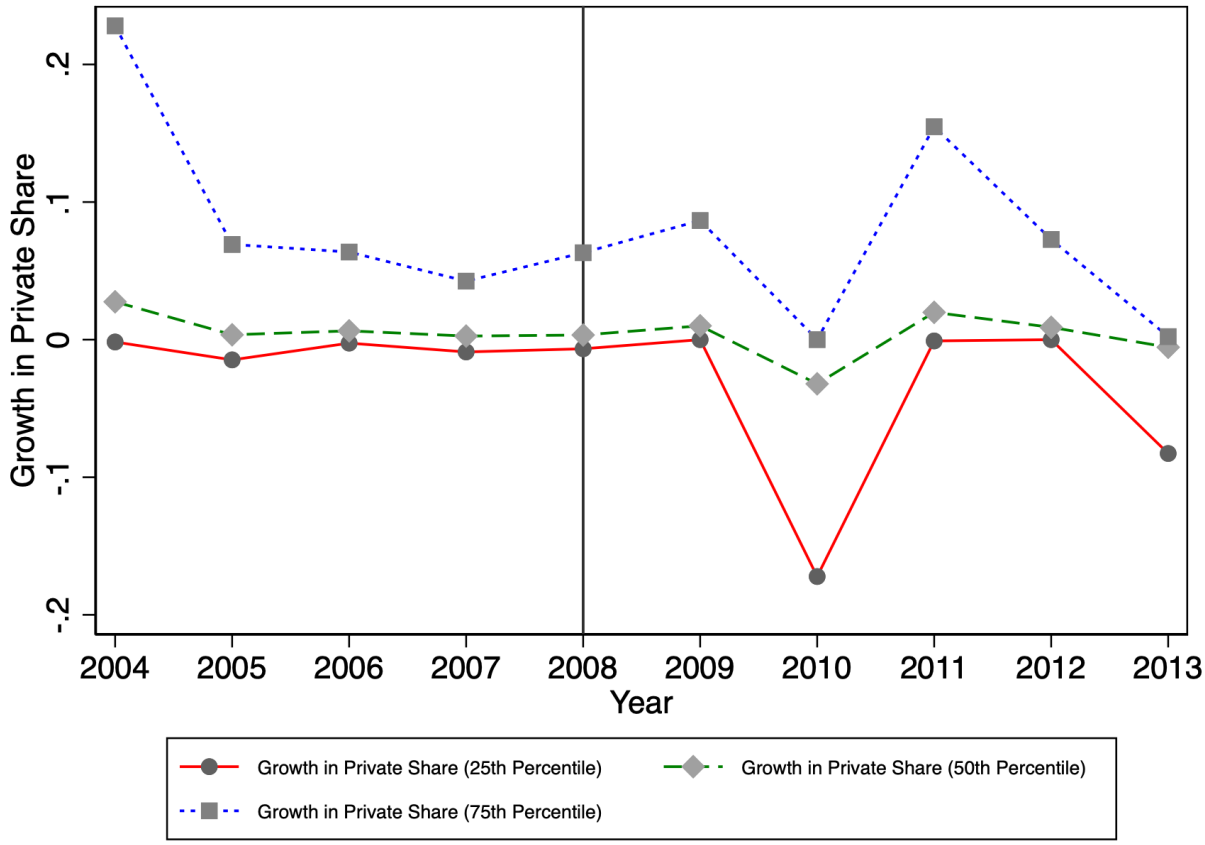
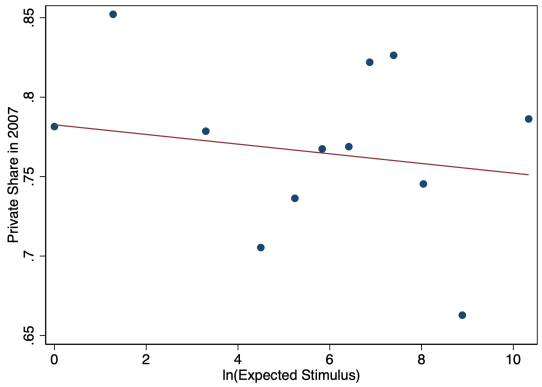


Figure 2.3: Growth in Private Share, by Year

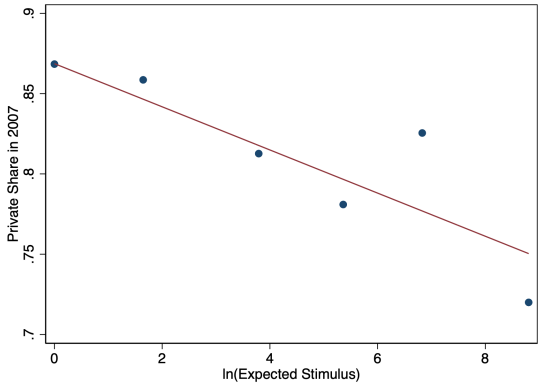


Note: Private shares are computed by aggregating all private revenues and divide it by total revenue in a given sector-province pair. Sectors are recorded at CIC 2-digit level. Firm's ownership is defined by firm's registration type. *Source: Authors' own calculations using ASIF data.*

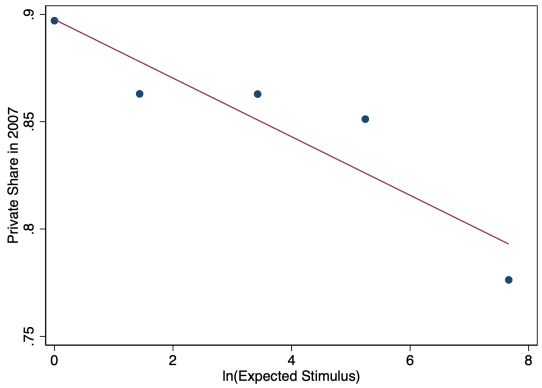
Figure 2.4: Expected Stimulus and Prior Degree of Privatization



Panel A: Market Defined at CIC 2-digit Level



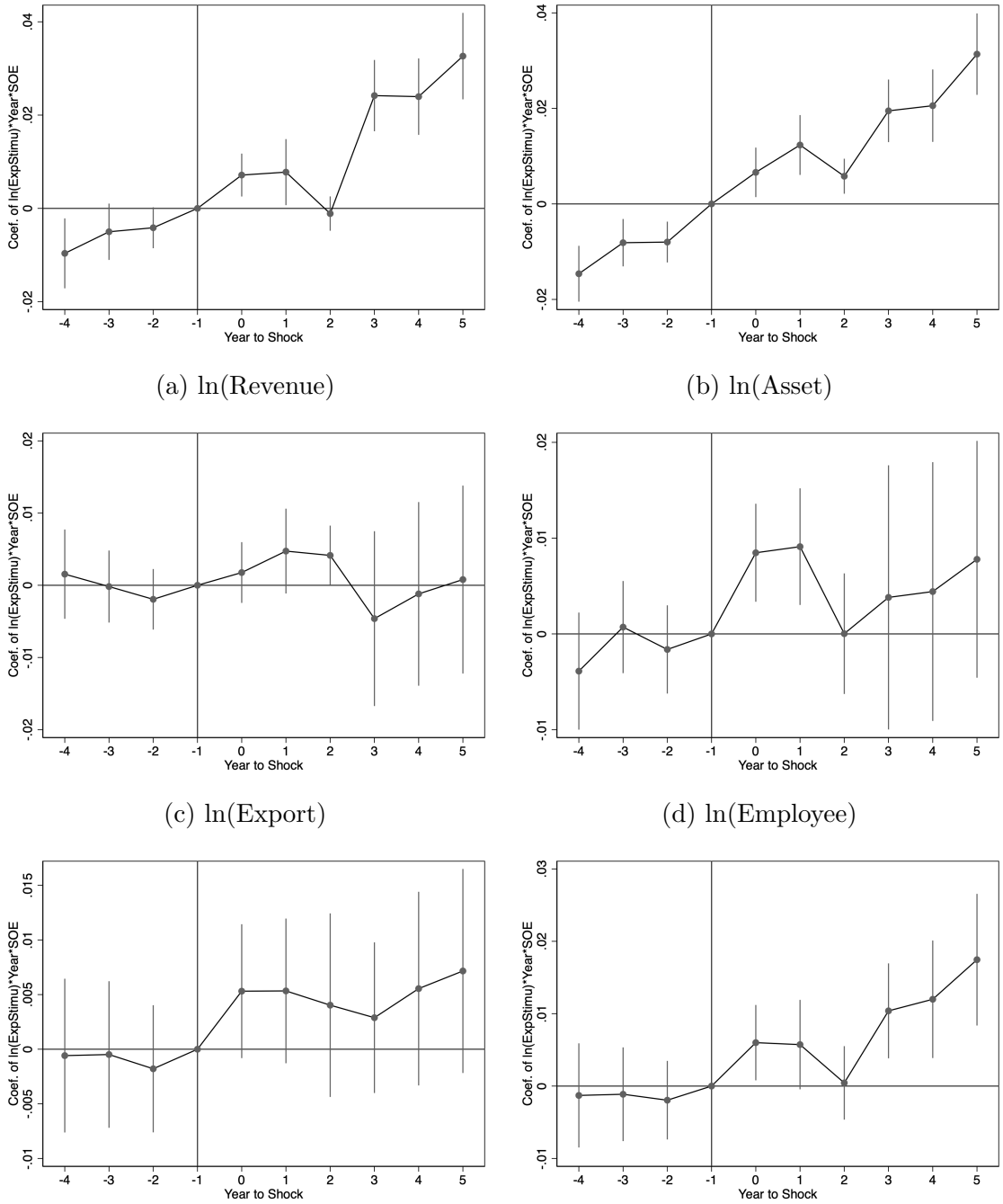
Panel B: Market Defined at CIC 3-digit Level



Panel C: Market Defined at CIC 4-digit Level

Note: The binscatter plot shows the relationship between expected stimulus (in logs) and private shares in 2007. Expected stimulus are divided into multiple bins and the vertical axis shows the average private shares in that bin. Sectors are classified at the CIC 2-, 3- and 4-digit levels. Firm's ownership is defined by firm's registration type. Private shares are computed as in Section 2.2.2.

Figure 2.5: Impact of Stimulus on Firm Performance by Year



(a) ln(Revenue)

(b) ln(Asset)

(c) ln(Export)

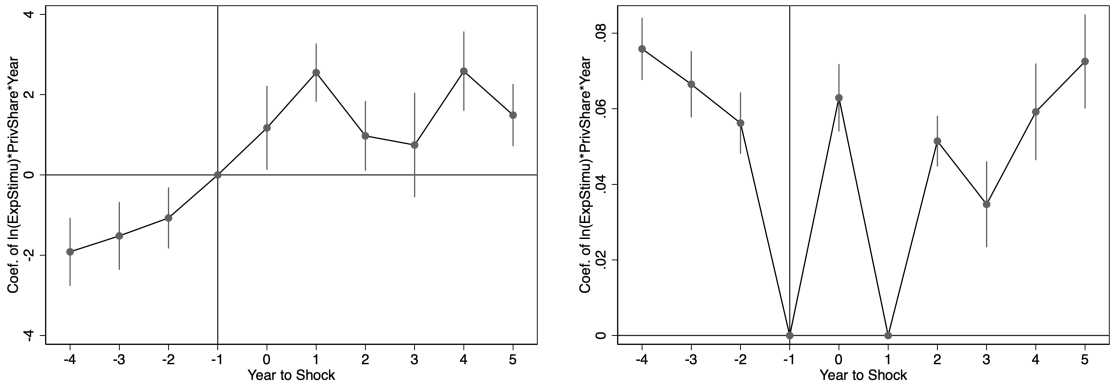
(d) ln(Employee)

(e) ln(TFP_OP)

(f) ln(TFP_LP)

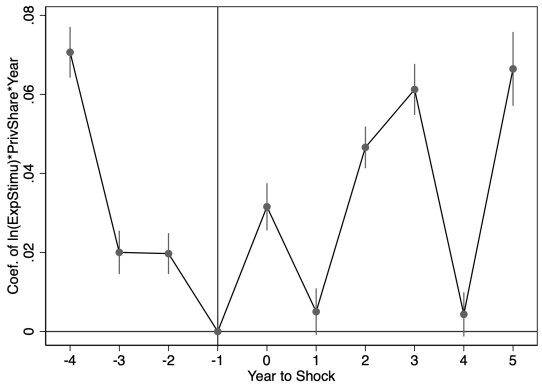
Note: ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Ownership is defined by firm's registration type in year 2007. Standard errors are clustered at the sector-province level.

Figure 2.6: Impact of Stimulus on Aggregate Outcomes by Year

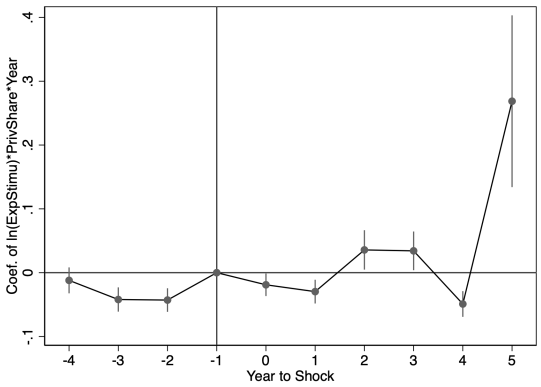


(a) Number Ratio

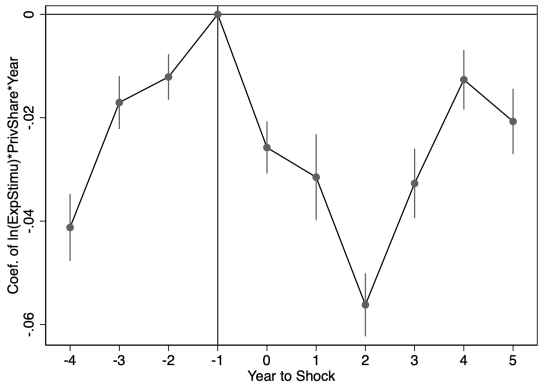
(b) Private Ratio



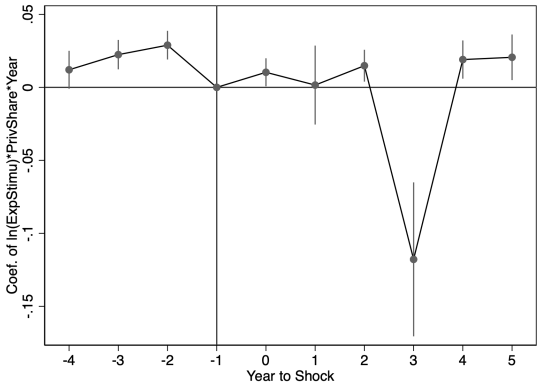
(c) Private Entry Rate



(d) Private Exit Rate



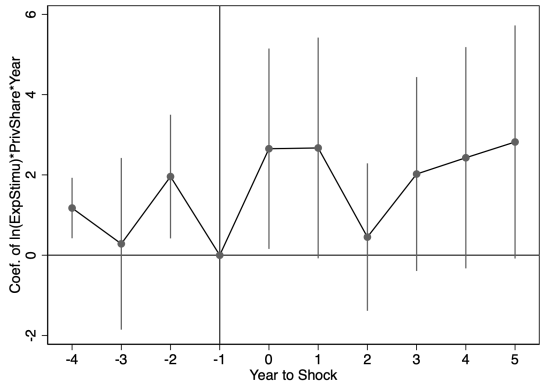
(e) State Entry Rate



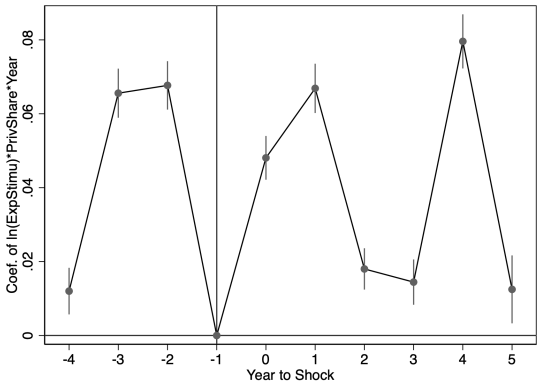
(f) State Exit Rate

Note: ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Variables are defined as aforementioned. Ownership is defined by firm's registration type in year 2007, the base year. Standard errors are clustered at the sector-province level.

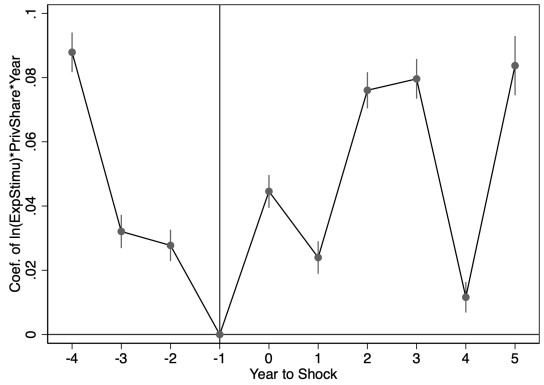
Figure 2.7: Impact of Stimulus on Private Share Components by Year



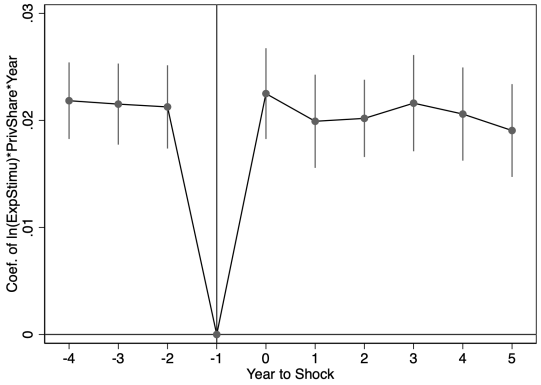
(a) Private Growth



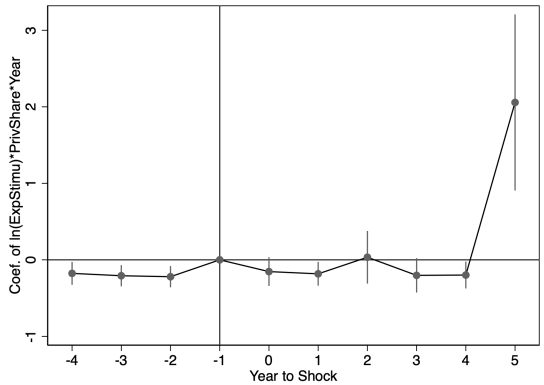
(b) Stayers' Share



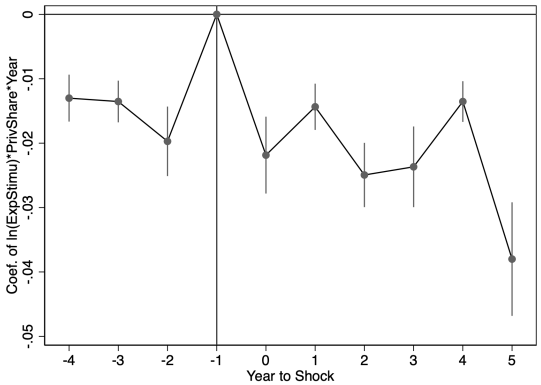
(c) Entrants' Share



(d) Privatized Share



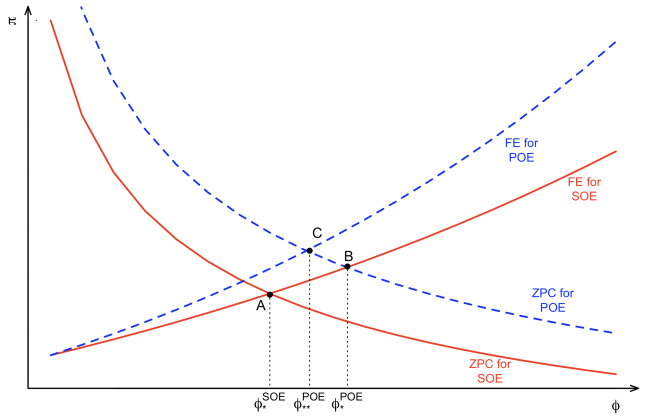
(e) Exits' Share



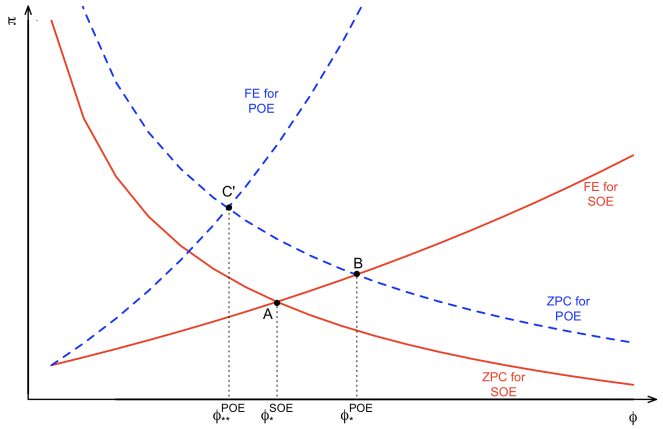
(f) Nationalized Share

Note: ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Variables are defined as aforementioned. Ownership is defined by firm's registration type in year 2007, the base year. Standard errors are clustered at the sector-province level.

Figure 2.8: Determination of the Equilibrium Cutoff ϕ and Average Profit π for SOEs and Private Firms



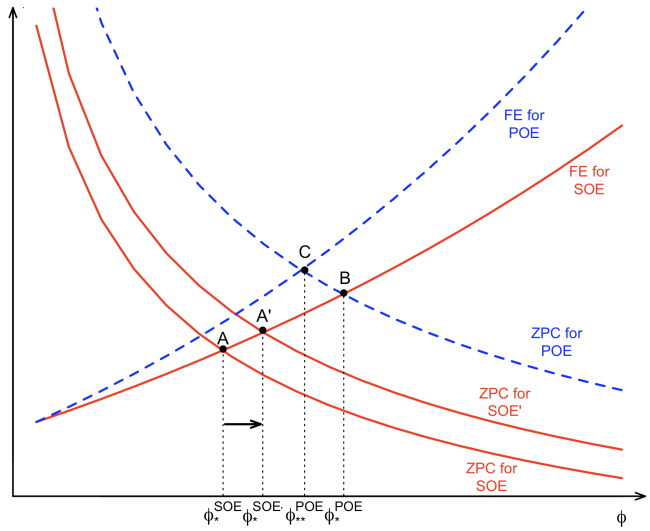
Panel A: Weaker Effect of FSD



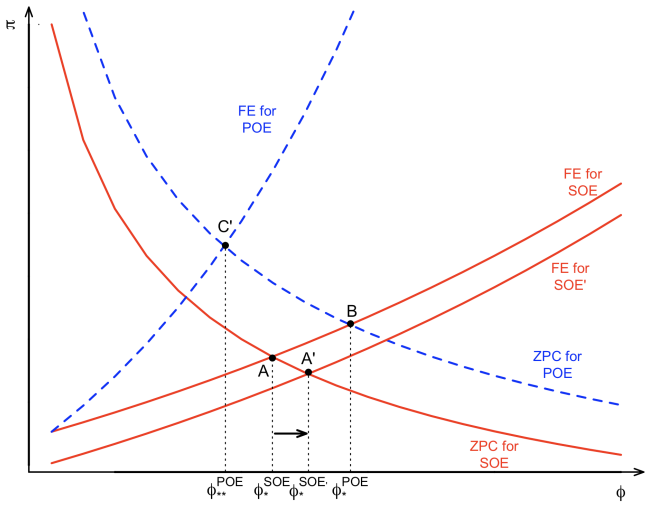
Panel B: Stronger Effect of FSD

Notes: Panel A illustrates the determination of average profit and equilibrium cutoff value for SOEs and private firms under the weak effect of FSD. In the benchmark case, without any effect of FSD, the equilibrium condition of the private firm reaches point B. The introduction of FSD, in this case, drives point B to point C. In either case, $\phi_*^{SOE} < \phi_*^{POE}$. However, Panel B illustrates the determination of average profit and equilibrium cutoff value under the dominating effect of FSD, driving point B to point C', where $\phi_*^{SOE} > \phi_*^{POE}$. In both cases, the average profit in the equilibrium for private firms is higher than that of SOEs.

Figure 2.9: SOE Reform Strategy: Before and After the Stimulus



Panel A: Before Stimulus, Weak FSD



Panel B: After Stimulus, Strong FSD

Notes: Panel A illustrates the party-state's SOE reform strategy prior to stimulus in cutting back subsidies and enforcing exits to close the gap between misallocation wedge $|\underline{\phi}^{POE} - \underline{\phi}^{SOE}|$. We illustrate the case with weak FSD. Panel B, however, shows that the party-state pushes up the performance of SOEs by reducing the exit rate δ^{SOE} as $\frac{M^{SOE}}{M^{POE}}$ falls significantly during the after-implementation of stimulus. We illustrate this with strong FSD without changes in subsidy intensities.

Table 2.1: Summary Statistics for Key Variables, by Ownership

Panel A: Summary Statistics for 2007

	(1) Non-SOE		(2) SOE		(3) Difference	
	Mean	SD	Mean	SD	Mean Diff.	T-Stat
ln(Revenue)	3.43	1.22	3.91	1.87	-0.48***	(-30.90)
ln(Asset)	2.97	1.29	4.31	1.85	-1.33***	(-86.30)
ln(Export)	0.73	1.48	0.38	1.25	0.35***	(32.98)
EBIT	6.88	219.96	40.80	549.03	-33.91***	(-7.46)
ln(Employee)	4.60	1.06	5.42	1.52	-0.83***	(-65.40)
Firm Age	9.31	8.75	27.68	20.17	-18.37***	(-109.97)
Observations	322063		14704		336767	

Panel B: Summary Statistics for 2013

	(1) Non-SOE		(2) SOE		(3) Difference	
	Mean	SD	Mean	SD	Mean Diff.	T-Stat
ln(Revenue)	4.61	1.11	5.30	1.54	-0.70***	(-46.65)
ln(Asset)	3.95	1.30	5.61	1.70	-1.66***	(-100.53)
ln(Export)	1.22	1.93	0.78	1.75	0.45***	(17.79)
EBIT	18.32	245.78	70.66	918.28	-52.33***	(-5.93)
ln(Employee)	5.68	0.80	6.06	1.11	-0.38***	(-35.25)
Firm Age	11.09	8.25	24.53	20.83	-13.44***	(-67.21)
Observations	328077		10898		338975	

Note: Revenue, EBIT, Asset and Export are denominated in millions RMB. All variables except EBIT are log-transformed. Employee is denominated in number of person. Firm Age is denominated in number of year. Ownership is defined by firm's registration type in the corresponding year. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.2: Median Contribution by Component

	Stayers	Entrants	Privatized	Exits	Nationalized
2004	.45	.24	0	.03	0
2005	.71	.06	0	.01	0
2006	.75	.07	0	0	0
2007	.77	.06	0	0	0
2008	.69	.13	0	.08	0
2009	.72	.11	0	.08	0
2010	.44	.27	0	.21	0
2011	.46	.33	0	.2	0
2012	.87	.04	0	.02	0
2013	.28	.16	0	.08	0

Note: This table summarizes the median values across years of the ratio of each component described in Section 2.2.2 to the total revenue of the market, obtained by ranking the each ratio from top to bottom across markets. Sectors are recorded at CIC 2-digit. Firm ownership is defined by its registered type. *Source: Authors' own calculations using ASIF data.*

Table 2.3: Areas of Investment from the Stimulus Program

Areas of Investment	Funds Allocated
Basic housing	400 billion
Rural infrastructure	370 billion
Transportation & urban infrastructure	1500 billion
Medical care, education & culture	150 billion
Environmental & ecological engineering	210 billion
Innovation & structural adjustment	370 billion
Earthquake relief & reconstruction	1000 billion

Source: National Development and Reform Commission

Table 2.4: Relationship between Expected Stimulus and Private Share

Panel A: Market Defined at CIC 2-digit Level

	(1) ln(Expected Stimulus)	(2) ln(Expected Stimulus)	(3) ln(Expected Stimulus)
Private Share of Sector-Province	-0.468	-0.124	-0.508***
	(0.368)	(0.159)	(0.151)
Observations	1135	1135	1135
Adj. R-Squared	0.000544	0.827	0.868
Robust SE	Yes	Yes	Yes
Agg. Sector FE	No	Yes	Yes
Prov. FE	No	No	Yes

Panel B: Market Defined at CIC 3-digit Level

	(1) ln(Expected Stimulus)	(2) ln(Expected Stimulus)	(3) ln(Expected Stimulus)
Private Share of Sector-Province	-1.269***	-0.261***	-0.448***
	(0.163)	(0.0731)	(0.0734)
Observations	4878	4878	4878
Adj. R-Squared	0.0169	0.846	0.857
Robust SE	Yes	Yes	Yes
Agg. Sector FE	No	Yes	Yes
Prov. FE	No	No	Yes

Panel C: Market Defined at CIC 4-digit Level

	(1) ln(Expected Stimulus)	(2) ln(Expected Stimulus)	(3) ln(Expected Stimulus)
Private Share of Sector-Province	-0.910***	-0.245***	-0.371***
	(0.0975)	(0.0485)	(0.0495)
Observations	11142	11142	11142
Adj. R-Squared	0.0123	0.798	0.806
Robust SE	Yes	Yes	Yes
Agg. Sector FE	No	Yes	Yes
Prov. FE	No	No	Yes

Note: Ownership is defined by firm's registration type in year 2007. Private shares are defined by the proportion of revenue going to non-SOE firms in a given 2-, 3- and 4-digit sector-province pair. Firm's ownership is defined by firm's registration type. Expected stimulus is computed as aforementioned. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.5: Impact of Stimulus on Firm Performance

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
Post*SOE	-0.0747***	-0.158***	0.0146*	-0.149***	0.0236**	0.0315***	-0.000368	0.0646***
	(0.00903)	(0.00739)	(0.00841)	(0.00901)	(0.00938)	(0.00930)	(0.00782)	(0.00339)
ln(ExpStimu)*Post*SOE	0.0144***	0.0195***	0.00230	0.00331	0.00634***	0.00534**	0.00765***	-0.00128
	(0.00234)	(0.00233)	(0.00208)	(0.00254)	(0.00237)	(0.00235)	(0.00203)	(0.00100)
Observations	2355344	2354121	2154702	2338382	2270535	2270535	2270535	1799485
Adj. R-Squared	0.846	0.896	0.805	0.798	0.424	0.435	0.653	0.796
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Ownership is defined by firm's registration type in year 2007. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.6: Impact of Stimulus on Firm Performance by Privatization

Panel A: Markets with Pre-Shock Private Share Above Median

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
Post*SOE	-0.120***	-0.182***	0.0998**	-0.206***	-0.0321	-0.0303	-0.0644	0.254***
	(0.0458)	(0.0333)	(0.0389)	(0.0429)	(0.0445)	(0.0440)	(0.0445)	(0.0176)
ln(ExpStimu)*Post*SOE	0.0272*	0.0195	-0.0126	0.0264	-0.00985	-0.00932	0.00119	-0.00517
	(0.0156)	(0.0172)	(0.0246)	(0.0181)	(0.0192)	(0.0190)	(0.0138)	(0.00822)
Observations	694406	693948	638144	690113	669755	669755	669755	525588
Adj. R-Squared	0.859	0.904	0.817	0.809	0.484	0.498	0.700	0.543
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Markets with Pre-Shock Private Share Below Median

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
Post*SOE	-0.0701***	-0.153***	0.00668	-0.138***	0.0237**	0.0318***	0.0000171	0.0563***
	(0.00935)	(0.00767)	(0.00883)	(0.00940)	(0.00987)	(0.00977)	(0.00816)	(0.00348)
ln(ExpStimu)*Post*SOE	0.0139***	0.0195***	0.00259	0.00315	0.00655***	0.00545**	0.00766***	-0.000688
	(0.00238)	(0.00236)	(0.00209)	(0.00260)	(0.00245)	(0.00242)	(0.00209)	(0.00101)
Observations	1627779	1627123	1485892	1615230	1567945	1567945	1567945	1248851
Adj. R-Squared	0.853	0.903	0.809	0.807	0.420	0.431	0.651	0.809
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Sample is split by whether the market a firm operates in has an above or below median level of privatization under its corresponding aggregated sector. Ownership is defined by firm's registration type in year 2007. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.7: Impact on Aggregate Outcomes

Panel A: Impact of Stimulus on Aggregate Outcomes

	(1) Performance Gap	(2) Performance Ratio	(3) Number Ratio	(4) Private Ratio	(5) Private Entry Rate	(6) Private Exit Rate	(7) State Entry Rate	(8) State Exit Rate
ln(ExpStimu)*Post	-0.00227	-2.726	-0.528***	-0.00113*	-0.000572	-0.0243***	0.000973***	0.00495***
	(0.00437)	(3.059)	(0.139)	(0.000575)	(0.000683)	(0.00509)	(0.000327)	(0.00106)
Observations	98518	32652	35575	81248	99520	94990	99520	94990
Adj. R-Squared	0.0505	0.218	0.735	0.603	0.319	0.0134	0.189	0.0758
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Impact of Stimulus and Privatization on Aggregate Outcomes

	(1) Performance Gap	(2) Performance Ratio	(3) Number Ratio	(4) Private Ratio	(5) Private Entry Rate	(6) Private Exit Rate	(7) State Entry Rate	(8) State Exit Rate
ln(ExpStimu)*Post	-0.0142	-5.524	-1.967***	-0.0156***	-0.0157***	-0.0721***	0.0183***	0.0229***
	(0.00999)	(5.185)	(0.107)	(0.00265)	(0.00139)	(0.00713)	(0.00188)	(0.00604)
ln(ExpStimu)*PrivShare*Post	0.0144	3.954	2.153***	0.0177***	0.0185***	0.0581***	-0.0211***	-0.0218***
	(0.0100)	(3.439)	(0.221)	(0.00282)	(0.00165)	(0.00888)	(0.00199)	(0.00653)
Observations	98491	32652	35533	81248	99424	94941	99424	94941
Adj. R-Squared	0.0505	0.218	0.736	0.604	0.320	0.0134	0.195	0.0783
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Performance gap is the ratio of TFP (OP method) between firms at the 99th percentile and the 50th percentile. Performance ratio is the revenue ratio between the median private firm and the median state firm. Number ratio is the ratio of the number of private firms to the number of state firms. Private ratio is the size-weighted average ratio of privately contributed capital across firms within the same market. Private (state) entry rate is the number of new private (state) firms over the total number of firms in a given period. Private (state) exit rate is the number of firms that are in the market last period but are not in the market in the current period over the total number of firms in the current period. Ownership is defined by firm's registration type in year 2007. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.8: Impact on Privatization Dynamics

Panel A: Impact of Stimulus on Private Share Components

	(1) Private Growth	(2) Stayers' Share	(3) Entrants' Share	(4) Privatized Share	(5) Exits' Share	(6) Nationalized Share
ln(ExpStimu)*Post	-0.0738	0.000825	-0.00122	0.000140	0.0365	0.000871***
	(0.0748)	(0.000903)	(0.000781)	(0.000268)	(0.0371)	(0.000209)
Observations	93777	99424	99424	99424	94941	99424
Adj. R-Squared	0.0977	0.408	0.301	0.0670	-0.00510	0.0764
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Impact of Stimulus and Privatization on Private Share Components

	(1) Private Growth	(2) Stayers' Share	(3) Entrants' Share	(4) Privatized Share	(5) Exits' Share	(6) Nationalized Share
ln(ExpStimu)*Post	-1.180*	-0.00963***	-0.0263***	-0.00789***	-0.161***	0.0131***
	(0.683)	(0.00169)	(0.00151)	(0.00109)	(0.0551)	(0.00149)
ln(ExpStimu)*PrivShare*Post	1.336*	0.0128***	0.0306***	0.00980***	0.241***	-0.0149***
	(0.746)	(0.00198)	(0.00184)	(0.00132)	(0.0851)	(0.00162)
Observations	93769	99424	99424	99424	94941	99424
Adj. R-Squared	0.100	0.408	0.303	0.0694	-0.00506	0.0842
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Private growth is computed by recognizing that $\Delta\text{PrivateShare}_t = \frac{a_t}{y_t} - \frac{a_{t-1}}{y_{t-1}} = \frac{a_{t-1}}{y_{t-1}}\left[\frac{1+p_t}{1+g_t} - 1\right]$, where a_t is private sector size, y_t is total market size, p_t is the growth rate of private sector (revenue) and g_t is the growth rate of the whole market. We can therefore write Private Growth $= \Delta\text{PrivateShare}_t/\text{PrivateShare}_{t-1} = \left[\frac{1+p_t}{1+g_t} - 1\right]$. The rest of the variables are defined and computed in the same way as in Section 2.2.2. Ownership is defined by firm's registration type in year 2007. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.9: Impact of Stimulus on Firm Performance, Alternative Definition

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
Post*SOE	-0.0779***	-0.161***	0.0117	-0.153***	0.0204**	0.0283***	-0.00381	0.0654***
	(0.00914)	(0.00745)	(0.00849)	(0.00912)	(0.00946)	(0.00938)	(0.00786)	(0.00342)
Stimulus*Post*SOE	0.112***	0.144***	0.0279**	0.0399**	0.0562***	0.0494***	0.0660***	-0.0121*
	(0.0160)	(0.0157)	(0.0139)	(0.0169)	(0.0177)	(0.0175)	(0.0147)	(0.00718)
Observations	2355344	2354121	2154702	2338382	2270535	2270535	2270535	1799485
Adj. R-Squared	0.846	0.896	0.805	0.798	0.424	0.435	0.653	0.796
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Stimulus is a dummy equal to one if the market has expected stimulus greater than zero. Markets are defined at the CIC 4-digit level with each province. Ownership is defined by firm's registration type in year 2007. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.10: Impact of Stimulus on Firm Performance by Privatization, Alternative Definition

Panel A: Markets with Pre-Shock Private Share Above Median

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
Post*SOE	-0.121***	-0.183***	0.0824**	-0.213***	-0.0233	-0.0219	-0.0559	0.252***
	(0.0467)	(0.0336)	(0.0385)	(0.0435)	(0.0447)	(0.0442)	(0.0449)	(0.0178)
Stimulus*Post*SOE	0.160*	0.118	0.0288	0.192*	-0.106	-0.101	-0.0411	-0.0171
	(0.0930)	(0.0913)	(0.118)	(0.104)	(0.130)	(0.130)	(0.105)	(0.0483)
Observations	694406	693948	638144	690113	669755	669755	669755	525588
Adj. R-Squared	0.859	0.904	0.817	0.809	0.484	0.498	0.700	0.543
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Markets with Pre-Shock Private Share Below Median

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
Post*SOE	-0.0727***	-0.155***	0.00465	-0.142***	0.0199**	0.0280***	-0.00363	0.0573***
	(0.00946)	(0.00773)	(0.00891)	(0.00950)	(0.00995)	(0.00986)	(0.00819)	(0.00351)
Stimulus*Post*SOE	0.106***	0.143***	0.0260*	0.0358**	0.0597***	0.0522***	0.0669***	-0.00907
	(0.0164)	(0.0161)	(0.0140)	(0.0174)	(0.0183)	(0.0181)	(0.0152)	(0.00728)
Observations	1627779	1627123	1485892	1615230	1567945	1567945	1567945	1248851
Adj. R-Squared	0.853	0.903	0.809	0.807	0.420	0.431	0.651	0.809
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Stimulus is a dummy equal to one if the market has expected stimulus greater than zero. Markets are defined at the CIC 4-digit level with each province. Sample is split by whether the market a firm operates in has an above or below median level of privatization under its corresponding aggregated sector. Ownership is defined by firm's registration type in year 2007. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.11: Impact of Stimulus on Firm Performance by Coastal-Inland Split

Panel A: Firms in Coastal Regions

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
Post*SOE	-0.0761***	-0.149***	0.0266**	-0.170***	0.0344***	0.0410***	0.00732	0.0671***
	(0.0114)	(0.00961)	(0.0135)	(0.0125)	(0.0124)	(0.0123)	(0.0113)	(0.00501)
ln(ExpStimu)*Post*SOE	0.0109***	0.0191***	-0.000647	0.00282	0.00687**	0.00591*	0.00542**	-0.00173
	(0.00291)	(0.00291)	(0.00320)	(0.00331)	(0.00324)	(0.00320)	(0.00275)	(0.00140)
Observations	1700810	1699641	1587893	1693112	1645779	1645779	1645779	1322903
Adj. R-Squared	0.846	0.893	0.803	0.791	0.444	0.458	0.672	0.789
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Firms in Inland Regions

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
Post*SOE	-0.0833***	-0.170***	-0.0179*	-0.142***	0.0135	0.0226	-0.0121	0.0611***
	(0.0140)	(0.0111)	(0.00972)	(0.0124)	(0.0141)	(0.0139)	(0.0109)	(0.00459)
ln(ExpStimu)*Post*SOE	0.0185***	0.0194***	0.00575**	0.00420	0.00535	0.00434	0.0102***	-0.000720
	(0.00353)	(0.00373)	(0.00233)	(0.00391)	(0.00348)	(0.00346)	(0.00292)	(0.00142)
Observations	654525	654471	566800	645261	624746	624746	624746	476570
Adj. R-Squared	0.846	0.903	0.774	0.815	0.377	0.384	0.605	0.789
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Coastal regions refer to provinces that include at least one coastal city. ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Sample is split by whether the market a firm operates in has an above or below median level of privatization under its corresponding aggregated sector. Ownership is defined by firm's registration type in year 2007, the base year. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.12: Impact of Stimulus on Firm Performance by Export

Panel A: Firms in More Export-Oriented Regions

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
Post*SOE	-0.0770***	-0.153***	0.0138	-0.159***	0.0164	0.0236**	-0.00622	0.0685***
	(0.0108)	(0.00849)	(0.0110)	(0.0110)	(0.0103)	(0.0102)	(0.00931)	(0.00407)
ln(ExpStimu)*Post*SOE	0.00795***	0.0134***	-0.00107	0.000595	0.00623*	0.00549	0.00483*	-0.00165
	(0.00298)	(0.00268)	(0.00344)	(0.00329)	(0.00352)	(0.00347)	(0.00293)	(0.00126)
Observations	2001190	2000027	1851728	1988209	1932017	1932017	1932017	1546142
Adj. R-Squared	0.848	0.896	0.803	0.799	0.429	0.441	0.660	0.748
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Firms in Less Export-Oriented Regions

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
Post*SOE	-0.0694***	-0.154***	-0.00982	-0.110***	0.0449**	0.0544**	0.0132	0.0504***
	(0.0176)	(0.0152)	(0.00656)	(0.0158)	(0.0218)	(0.0216)	(0.0152)	(0.00610)
ln(ExpStimu)*Post*SOE	0.0193***	0.0257***	0.00199*	0.000850	0.00403	0.00250	0.00843***	-0.000385
	(0.00369)	(0.00411)	(0.00108)	(0.00420)	(0.00388)	(0.00385)	(0.00298)	(0.00168)
Observations	338912	338860	289737	334971	323363	323363	323363	244278
Adj. R-Squared	0.861	0.916	0.587	0.819	0.409	0.421	0.643	0.828
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Export-oriented markets are defined by whether a market's export value is above median in 2007, the pre-shock year. ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Sample is split by whether the market a firm operates in has an above or below median level of privatization under its corresponding aggregated sector. Ownership is defined by firm's registration type in year 2007, the base year. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.13: Impact on Aggregate Outcomes, Alternative Definition

Panel A: Impact of Stimulus on Aggregate Outcomes

	(1) Performance Gap	(2) Performance Ratio	(3) Number Ratio	(4) Private Ratio	(5) Private Entry Rate	(6) Private Exit Rate	(7) State Entry Rate	(8) State Exit Rate
Stimulus*Post	-0.0117	-39.83	-5.196***	-0.000942	-0.00909*	-0.157***	0.000200	0.0216***
	(0.0251)	(41.46)	(0.736)	(0.00321)	(0.00482)	(0.0251)	(0.00186)	(0.00476)
Observations	98518	32652	35575	81248	99520	94990	99520	94990
Adj. R-Squared	0.0505	0.219	0.736	0.603	0.319	0.0134	0.188	0.0755
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Impact of Stimulus and Privatization on Aggregate Outcomes

	(1) Performance Gap	(2) Performance Ratio	(3) Number Ratio	(4) Private Ratio	(5) Private Entry Rate	(6) Private Exit Rate	(7) State Entry Rate	(8) State Exit Rate
Stimulus*Post	-0.0717	-77.69	-12.78***	-0.105***	-0.125***	-0.396***	0.124***	0.137***
	(0.0656)	(71.74)	(0.583)	(0.0165)	(0.00901)	(0.0455)	(0.0122)	(0.0377)
Stimulus*PrivShare*Post	0.0693	53.13	11.65***	0.122***	0.134***	0.277***	-0.143***	-0.134***
	(0.0634)	(44.85)	(1.092)	(0.0174)	(0.0109)	(0.0484)	(0.0127)	(0.0400)
Observations	98491	32652	35533	81248	99424	94941	99424	94941
Adj. R-Squared	0.0505	0.219	0.736	0.605	0.320	0.0134	0.198	0.0783
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Stimulus is a dummy equal to one if the market has expected stimulus greater than zero. Markets are defined at the CIC 4-digit level with each province. Performance gap is the ratio of revenue between firms at the 99th percentile and the 50th percentile. Performance ratio is the revenue ratio between the median private firm and the median state firm. Number ratio is the ratio of the number of private firms to the number of state firms. Private ratio is the size-weighted average ratio of privately contributed capital across firms within the same market. Private (state) entry rate is the number of new private (state) firms over the total number of firms in a given period. Private (state) exit rate is the number of firms that are in the market last period but are not in the market in the current period over the total number of firms in the current period. Ownership is defined by firm's registration type in year 2007. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.14: Impact on Privatization Dynamics, Alternative Definition

Panel A: Impact of Stimulus on Private Share Components

	(1) Private Growth	(2) Stayers' Share	(3) Entrants' Share	(4) Privatized Share	(5) Exits' Share	(6) Nationalized Share
Stimulus*Post	-0.185	0.0158***	-0.0177***	0.00145	0.0407	0.00450***
	(0.226)	(0.00579)	(0.00526)	(0.00141)	(0.156)	(0.00109)
Observations	93777	99424	99424	99424	94941	99424
Adj. R-Squared	0.0976	0.408	0.301	0.0670	-0.00510	0.0764
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Impact of Stimulus and Privatization on Private Share Components

	(1) Private Growth	(2) Stayers' Share	(3) Entrants' Share	(4) Privatized Share	(5) Exits' Share	(6) Nationalized Share
Stimulus*Post	-5.177**	-0.0792***	-0.197***	-0.0514***	-0.596	0.0920***
	(2.576)	(0.0113)	(0.00893)	(0.00570)	(0.420)	(0.00963)
Stimulus*PrivShare*Post	5.781**	0.110***	0.208***	0.0614***	0.741	-0.102***
	(2.779)	(0.0127)	(0.0110)	(0.00678)	(0.471)	(0.0103)
Observations	93769	99424	99424	99424	94941	99424
Adj. R-Squared	0.0991	0.409	0.304	0.0698	-0.00510	0.0874
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Stimulus is a dummy equal to one if the market has expected stimulus greater than zero. Markets are defined at the CIC 4-digit level with each province. Private growth is computed by recognizing that $\Delta\text{PrivateShare}_t = \frac{a_t}{y_t} - \frac{a_{t-1}}{y_{t-1}} = \frac{a_{t-1}}{y_{t-1}}\left[\frac{1+p_t}{1+g_t} - 1\right]$, where a_t is private sector size, y_t is total market size, p_t is the growth rate of private sector (revenue) and g_t is the growth rate of the whole market. We can therefore write Private Growth $= \Delta\text{PrivateShare}_t/\text{PrivateShare}_{t-1} = \left[\frac{1+p_t}{1+g_t} - 1\right]$. The rest of the variables are defined and computed in the same way as in Section 2.2.2. Ownership is defined by firm's registration type in year 2007. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.15: Impact on Aggregate Outcomes by Coastal-Inland Split

Panel A: Impact of Stimulus and Privatization on Aggregate Outcomes, Coastal Regions

	(1) Performance Gap	(2) Performance Ratio	(3) Number Ratio	(4) Private Ratio	(5) Private Entry Rate	(6) Private Exit Rate	(7) State Entry Rate	(8) State Exit Rate
ln(ExpStimu)*Post	0.00116	0.262	-2.515***	-0.0187***	-0.0173***	-0.105***	0.0149***	0.0252*
	(0.00991)	(2.887)	(0.226)	(0.00451)	(0.00230)	(0.0156)	(0.00247)	(0.0140)
ln(ExpStimu)*PrivShare*Post	0.00305	0.487	2.355***	0.0199***	0.0187***	0.0831***	-0.0165***	-0.0253*
	(0.0105)	(1.538)	(0.360)	(0.00467)	(0.00255)	(0.0173)	(0.00256)	(0.0145)
Observations	43553	14939	15856	37156	43794	42640	43794	42640
Adj. R-Squared	0.0970	0.333	0.732	0.613	0.419	0.0191	0.191	0.0987
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Impact of Stimulus and Privatization on Aggregate Outcomes, Inland Regions

	(1) Performance Gap	(2) Performance Ratio	(3) Number Ratio	(4) Private Ratio	(5) Private Entry Rate	(6) Private Exit Rate	(7) State Entry Rate	(8) State Exit Rate
ln(ExpStimu)*Post	-0.0235	-8.705	-1.385***	-0.0145***	-0.0139***	-0.0476***	0.0203***	0.0216***
	(0.0148)	(7.769)	(0.0870)	(0.00327)	(0.00172)	(0.00669)	(0.00259)	(0.00505)
ln(ExpStimu)*PrivShare*Post	0.0187	3.396	1.747***	0.0189***	0.0167***	0.0354***	-0.0251***	-0.0190***
	(0.0147)	(3.306)	(0.259)	(0.00368)	(0.00234)	(0.00920)	(0.00279)	(0.00654)
Observations	54938	17713	19677	44092	55630	52301	55630	52301
Adj. R-Squared	0.0423	0.0414	0.640	0.593	0.262	0.0287	0.195	0.0642
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Coastal regions refer to provinces that include at least one coastal city. ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Performance gap is the ratio of TFP (OP method) between firms at the 99th percentile and the 50th percentile. Performance ratio is the revenue ratio between the median private firm and the median state firm. Number ratio is the ratio of the number of private firms to the number of state firms. Private ratio is the size-weighted average ratio of privately contributed capital across firms within the same market. Private (state) entry rate is the number of new private (state) firms over the total number of firms in a given period. Private (state) exit rate is the number of firms that are in the market last period but are not in the market in the current period over the total number of firms in the current period. Ownership is defined by firm's registration type in year 2007. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.16: Impact on Privatization Dynamics by Coastal-Inland Split

Panel A: Impact of Stimulus and Privatization on Private Share Components, Coastal Regions

	(1) Private Growth	(2) Stayers' Share	(3) Entrants' Share	(4) Privatized Share	(5) Exits' Share	(6) Nationalized Share
ln(ExpStimu)*Post	-0.638*	-0.00236	-0.0293***	-0.00865***	-0.257**	0.0146***
	(0.376)	(0.00271)	(0.00247)	(0.00162)	(0.118)	(0.00267)
ln(ExpStimu)*PrivShare*Post	0.693	0.00389	0.0318***	0.0101***	0.373**	-0.0159***
	(0.442)	(0.00298)	(0.00279)	(0.00186)	(0.167)	(0.00281)
Observations	42282	43794	43794	43794	42640	43794
Adj. R-Squared	0.0347	0.453	0.383	0.0767	0.00655	0.0907
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Impact of Stimulus and Privatization on Private Share Components, Inland Regions

	(1) Private Growth	(2) Stayers' Share	(3) Entrants' Share	(4) Privatized Share	(5) Exits' Share	(6) Nationalized Share
ln(ExpStimu)*Post	-1.499	-0.0156***	-0.0233***	-0.00744***	-0.101*	0.0123***
	(1.056)	(0.00217)	(0.00189)	(0.00144)	(0.0553)	(0.00179)
ln(ExpStimu)*PrivShare*Post	1.798	0.0243***	0.0280***	0.0101***	0.121	-0.0149***
	(1.184)	(0.00275)	(0.00260)	(0.00190)	(0.0772)	(0.00205)
Observations	51487	55630	55630	55630	52301	55630
Adj. R-Squared	0.102	0.384	0.255	0.0640	-0.0132	0.0802
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Coastal regions refer to provinces that include at least one coastal city. ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Private growth is computed by recognizing that $\Delta\text{PrivateShare}_t = \frac{a_t}{y_t} - \frac{a_{t-1}}{y_{t-1}} = \frac{a_{t-1}}{y_{t-1}}\left[\frac{1+p_t}{1+g_t} - 1\right]$, where a_t is private sector size, y_t is total market size, p_t is the growth rate of private sector (revenue) and g_t is the growth rate of the whole market. We can therefore write Private Growth $= \Delta\text{PrivateShare}_t/\text{PrivateShare}_{t-1} = \left[\frac{1+p_t}{1+g_t} - 1\right]$. The rest of the variables are defined and computed in the same way as in Section 2.2.2. Ownership is defined by firm's registration type in year 2007. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.17: Impact on Aggregate Outcomes by Export

Panel A: Impact of Stimulus and Privatization on Aggregate Outcomes, More Export-Oriented Markets

	(1) Performance Gap	(2) Performance Ratio	(3) Number Ratio	(4) Private Ratio	(5) Private Entry Rate	(6) Private Exit Rate	(7) State Entry Rate	(8) State Exit Rate
ln(ExpStimu)*Post	-0.0150*	-18.13	-2.520***	-0.0191***	-0.0152***	-0.0670***	0.0214***	0.0154**
	(0.00890)	(19.06)	(0.195)	(0.00396)	(0.00262)	(0.0146)	(0.00343)	(0.00688)
ln(ExpStimu)*PrivShare*Post	0.0164**	18.57	2.386***	0.0204***	0.0169***	0.0368**	-0.0233***	-0.0141*
	(0.00758)	(17.57)	(0.302)	(0.00411)	(0.00282)	(0.0168)	(0.00354)	(0.00776)
Observations	51705	19961	20849	43977	51883	50838	51883	50838
Adj. R-Squared	0.0504	0.231	0.731	0.581	0.412	0.0249	0.198	0.100
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Impact of Stimulus and Privatization on Aggregate Outcomes, Less Export-Oriented Markets

	(1) Performance Gap	(2) Performance Ratio	(3) Number Ratio	(4) Private Ratio	(5) Private Entry Rate	(6) Private Exit Rate	(7) State Entry Rate	(8) State Exit Rate
ln(ExpStimu)*Post	-0.0127	-3.743	-1.045***	-0.0152***	-0.0140***	-0.0428***	0.0170***	0.0266***
	(0.0136)	(2.494)	(0.0920)	(0.00342)	(0.00163)	(0.00607)	(0.00219)	(0.00835)
ln(ExpStimu)*PrivShare*Post	0.00966	-0.499	1.772***	0.0201***	0.0171***	0.0520***	-0.0220***	-0.0254***
	(0.0148)	(0.571)	(0.386)	(0.00392)	(0.00248)	(0.0141)	(0.00244)	(0.00908)
Observations	46786	12691	14684	37271	47541	44103	47541	44103
Adj. R-Squared	0.0871	0.173	0.633	0.610	0.260	0.0822	0.190	0.0637
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Export-oriented markets are defined by whether a market's export value is above median in 2007, the pre-shock year. ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market (p,k) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Performance gap is the ratio of TFP (OP method) between firms at the 99th percentile and the 50th percentile. Performance ratio is the revenue ratio between the median private firm and the median state firm. Number ratio is the ratio of the number of private firms to the number of state firms. Private ratio is the size-weighted average ratio of privately contributed capital across firms within the same market. Private (state) entry rate is the number of new private (state) firms over the total number of firms in a given period. Private (state) exit rate is the number of firms that are in the market last period but are not in the market in the current period over the total number of firms in the current period. Ownership is defined by firm's registration type in year 2007. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.18: Impact on Privatization Dynamics by Export

Panel A: Impact of Stimulus and Privatization on Private Share Components, More Export-Oriented Markets

	(1) Private Growth	(2) Stayers' Share	(3) Entrants' Share	(4) Privatized Share	(5) Exits' Share	(6) Nationalized Share
ln(ExpStimu)*Post	-0.638***	-0.00874***	-0.0317***	-0.0109***	-0.127	0.0147***
	(0.197)	(0.00296)	(0.00258)	(0.00213)	(0.133)	(0.00270)
ln(ExpStimu)*PrivShare*Post	0.630***	0.0110***	0.0349***	0.0119***	0.181	-0.0160***
	(0.202)	(0.00312)	(0.00285)	(0.00235)	(0.172)	(0.00282)
Observations	50493	51883	51883	51883	50838	51883
Adj. R-Squared	0.0671	0.461	0.373	0.0825	0.00293	0.0961
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Impact of Stimulus and Privatization on Private Share Components, Less Export-Oriented Markets

	(1) Private Growth	(2) Stayers' Share	(3) Entrants' Share	(4) Privatized Share	(5) Exits' Share	(6) Nationalized Share
ln(ExpStimu)*Post	-1.450	-0.0143***	-0.0203***	-0.00645***	-0.0972**	0.0123***
	(1.007)	(0.00208)	(0.00182)	(0.00119)	(0.0460)	(0.00176)
ln(ExpStimu)*PrivShare*Post	1.962	0.0231***	0.0240***	0.0104***	0.210**	-0.0153***
	(1.200)	(0.00294)	(0.00272)	(0.00178)	(0.102)	(0.00208)
Observations	43276	47541	47541	47541	44103	47541
Adj. R-Squared	0.0957	0.368	0.256	0.0586	0.0167	0.0751
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Export-oriented markets are defined by whether a market's export value is above median in 2007, the pre-shock year. ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Private growth is computed by recognizing that $\Delta\text{PrivateShare}_t = \frac{a_t}{y_t} - \frac{a_{t-1}}{y_{t-1}} = \frac{a_{t-1}}{y_{t-1}}\left[\frac{1+p_t}{1+g_t} - 1\right]$, where a_t is private sector size, y_t is total market size, p_t is the growth rate of private sector (revenue) and g_t is the growth rate of the whole market. We can therefore write Private Growth $= \Delta\text{PrivateShare}_t/\text{PrivateShare}_{t-1} = \left[\frac{1+p_t}{1+g_t} - 1\right]$. The rest of the variables are defined and computed in the same way as in Section 2.2.2. Ownership is defined by firm's registration type in year 2007. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table 2.19: Application of the Theoretical Framework: SOE Reform Strategy, Privatization, and Stimulus

	High Strategic Value S_i	Low Strategic Value S_i
Prior to Economic Stimulus (Time Frame: 2001-2007)	Reform Package: • Comparably high subsidy $\tau^H \downarrow$ • Comparably low privatization degree $1/\chi^H \uparrow$ Policy Outcome: • Consolidate existing $M^{SOE} \downarrow$ • Narrowing $\lvert\phi_*^{SOE} - \phi_*^{POE}\rvert \downarrow$	Reform Package: • Comparably low subsidy $\tau^L \downarrow\downarrow$ • Comparably high privatization degree $1/\chi^L \uparrow\uparrow$ Policy Outcome: • Mass Exiting of $M^{SOE} \downarrow\downarrow$ • $\lvert\phi_*^{SOE} - \phi_*^{POE}\rvert$ undetermined depending on FSD
After Economic Stimulus (Time Frame: 2008-2011)	Reform Package: • Comparably high subsidy $\tau^H \uparrow$ • Comparably low privatization degree $1/\chi^H$ small $\uparrow$ • Exit rate $\delta^{SOE} \downarrow$ Policy Outcome: • Consolidate existing M^{SOE} with limited entries • Narrowing $\lvert\phi_*^{SOE} - \phi_*^{POE}\rvert \downarrow$	Reform Package: • Comparably low subsidy τ^L small $\uparrow$ • Comparably high privatization degree $1/\chi^L \uparrow$ • Exit rate $\delta^{SOE} \downarrow$ Policy Outcome: • Consolidate existing M^{SOE} with limited entries • $\lvert\phi_*^{SOE} - \phi_*^{POE}\rvert$ undetermined depending on FSD

Note: This table summarizes the contextually dependent and industry-specific reform packages and their respective firm dynamic outcomes as reported in the empirical section of the paper. We separate our discussion into four reform packages: (i) before versus after economic stimulus; (ii) High versus low industrial strategic values.

Appendices

B.1 Technical Appendix

B.1.1 Exposure to Stimulus and Sectors

We hand coded all sectors that are exposed to the stimulus program. For simplicity, we assign the diaggregated sectors under each aggregated sector in a mutually exclusive way, so that a given sector will not be labelled to have been exposed to the program under two aggregated sectors. We code the sectors at the CIC 2-, 3-, and 4-digit levels using the 2003 CIC system, to which our dataset has been concorded. Details are listed here:

1. Basic housing
   (a) CIC 2-digit: 20, 21, 31-34, 36, 39
   (b) CIC 3-digit: 101, 203, 312, 313, 345, 361
   (c) CIC 4-digit: 1012, 2031, 3124, 3451, 3452, 3459, 3613, 3614, all sectors under CIC 313
2. Rural infrastructure (rural infrastructure)
   (a) CIC 2-digit: 44-46
   (b) CIC 3-digit: 441, 442, 443, 450, 461
   (c) CIC 4-digit: 4420, 4430, 4500, 4610, all sectors under CIC 441
3. Transportation and urban infrastructure
   (a) CIC 2-digit: 35, 37
   (b) CIC 3-digit: 353, 371, 376, 379, 369
   (c) CIC 4-digit: 3530, 3792, 3799, 3696, all sectors under CIC 371, 376
4. Medical care, education and culture
   (a) CIC 2-digit: 23, 24, 26, 27

(b) CIC 3-digit: 295, 241, 415, all sectors under CIC 27

(c) CIC 4-digit: 2950, all sectors under CIC 27, 368, 241, 415

5. Environmental and ecological engineering

(a) CIC 2-digit: 43

(b) CIC 3-digit: 431, 432, 462, 469

(c) CIC 4-digit: 4620, 4690, all sectors under CIC 431, 432

6. Innovation and structural adjustment

(a) CIC 2-digit: 40, 41

(b) CIC 3-digit: all sectors under CIC 40, 41

(c) CIC 4-digit: all sectors under CIC 40, 41

B.1.2 Proof of Lemma 1

Part (i), (ii), (iii) follow from the discussion in the text and the expressions of average industry-level productivity, revenue, and profits for SOEs and private firms. We show that part (iv) hold within our model. Now, consider average productivity under FSD assumption. Re-write the equation of average productivity level $\tilde{\phi}(\underline{\phi}^{SOE}, \underline{\phi}^{POE})$ with type specific productivity distribution function $G^{SOE}(.)$ and $G^{POE}(.)$:

$$\tilde{\phi}^{FSD}(\underline{\phi}^{SOE}, \underline{\phi}^{POE}) = \\ [\frac{\chi}{1-G^{SOE}(\underline{\phi}^{SOE})}\int_{\underline{\phi}^{SOE}}^{\infty}\phi^{\sigma-1}g(\phi)d\phi + \frac{1-\chi}{1-G^{POE}(\underline{\phi}^{POE})}\int_{\underline{\phi}^{POE}}^{\infty}\phi^{\sigma-1}g(\phi)d\phi]^{\frac{1}{\sigma-1}}.$$

Given the definition of FSD, i.e., for fixed pairs of $(\underline{\phi}^{SOE}, \underline{\phi}^{POE})$, $\tilde{\phi}^{FSD} > \tilde{\phi}^{Benchmark}$.

Since $\pi^{POE}(\tilde{\phi}) = [\frac{\tilde{\phi}(\underline{\phi}^{SOE}, \underline{\phi}^{POE})}{\underline{\phi}^{POE}}]^{\sigma-1}\frac{r(\underline{\phi}^{POE})}{\sigma} - F$, for given values of $\underline{\phi}^{SOE}$, $\pi^{POE}(\underline{\phi}^{POE}) = 0$ by definition of ZCP. This holds true only when $\underline{\phi}^{POE}$ under FSD is less than $\underline{\phi}^{POE}$ under the benchmark case. Therefore, more productive private firms actually drives down the cutoff threshold for private firms, resulting in an active business dynamism.

B.1.3 Proof of Proposition 1

As shown in Lemma 1, the presence of FSD condition decreases $\underline{\phi}^{POE}$. The FE condition (discussed in the text) dictates: $\pi^{SOE} = \frac{\delta f_e}{[1-G^{SOE}(\underline{\phi}^{SOE})]}$, $\pi^{POE} = \frac{\delta f_e}{[1-G^{POE}(\underline{\phi}^{POE})]}$. Therefore, we have two situations: when FSD effect is strong and dominating, and when it is not. This leads to part (i) and (ii) of the proposition (as shown in Figure 2.8). Part (iii) and (iv) of the proposition results from inspection of expression for M^{SOE} and M^{POE}.

Let's consider equilibrium aggregate price index and welfare implication of our model. From expression (2.14), the total number of firms:

$$\begin{aligned} M^* &= M^{SOE} + M^{POE} \\ &= \frac{L}{\sigma}\{[\frac{1}{\chi}\tilde{\pi}^{SOE} + F(\frac{1-\chi}{\chi}\frac{\tilde{\pi}^{SOE}}{\tilde{\pi}^{POE}} + (1-\tau))]^{-1} \\ &+ [\frac{1}{1-\chi}\tilde{\pi}^{POE} + F(\frac{\chi}{1-\chi}\frac{\tilde{\pi}^{POE}}{\tilde{\pi}^{SOE}}(1-\tau)+1)]^{-1}\}. \end{aligned}$$

The aggregate price index $P = M^{*\frac{1}{1-\sigma}} p(\tilde{\phi}) = M^{*\frac{1}{1-\sigma}}/\rho\tilde{\phi}$. Welfare measure is the inverse of aggregate price index $W = P^{-1}$. Therefore, by tweaking policy instruments (χ, τ, δ), the government could affect $\underline{\phi}^{SOE}$ and $\underline{\phi}^{POE}$ such that M^{SOE} and M^{POE} could be endogenously determined. As a result, total number of firms M^*, aggregate price level P, and welfare measure W could be pinned at the industry-level.

B.1.4 Proof of Proposition 2

We discuss how our framework could be applied into a discussion of SOE Reform Strategy, taking into account of the variations and heterogeneity of strategic versus non-strategic industries.

FOC of policymakers' objective function with respect to τ_i:

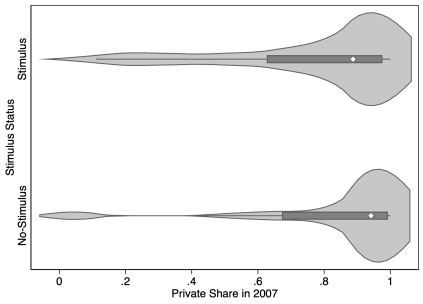
$$\underbrace{S_i' \frac{d\underline{\phi}^{SOE}}{d\tau_i}}_{\substack{\text{Marginal Benefit} \\ \text{of Supporting an Industry}}} \quad \underbrace{-\lambda[\tau_i \frac{dM_i^{SOE}}{d\tau_i} + M_i^{SOE}]}_{\substack{\text{Marginal Cost} \\ \text{of Increasing Subsidies}}} \quad \underbrace{-\mu_i[\frac{d\phi_*^{POE}}{d\tau_i} - \frac{d\phi_*^{SOE}}{d\tau_i}]}_{\substack{\text{Marginal Cost} \\ \text{of Widening Misallocation Gap}}} .$$

From above equation, in connection with stylized facts of SOE reforms in different stages, Proposition 2 follows.

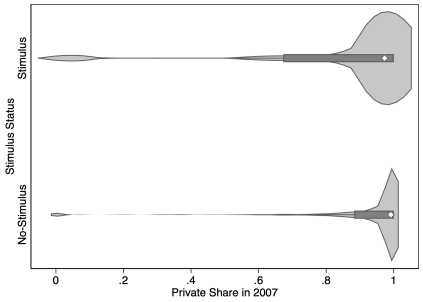
B.2 Additional Figures

B.2.1 Additional Figures for Fiscal Stimulus and Privatization

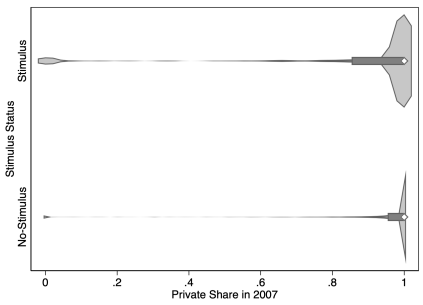
Figure B.1: Exposure to Stimulus and Pre-Shock Private Share (Alternative Measure 1)



Panel A: Market Defined at CIC 2-digit Level



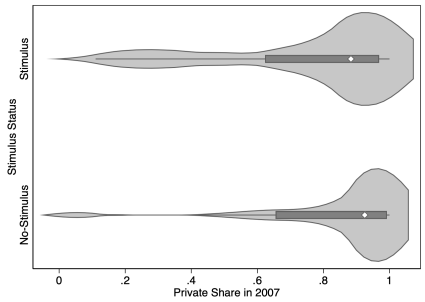
Panel B: Market Defined at CIC 3-digit Level



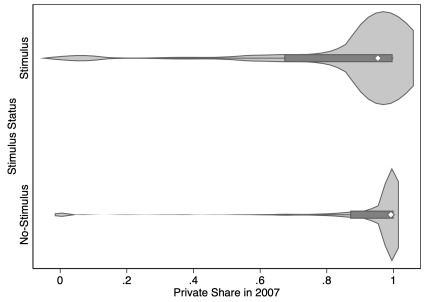
Panel C: Market Defined at CIC 4-digit Level

Notes: Each panel shows the medians (in diamond shape), the first to third quartiles (in rectangular box), the ranges, and the kernel densities of private shares, conditional on status of receiving stimulus. A sector-province pair is defined as treated (Stimulus) if its sector component received stimulus as part of an aggregated sector. Sectors are classified at the CIC 2-, 3-, and 4-digit levels respectively. Firm's ownership is defined by firm's registration type. Private shares are computed as in Section 2.2.2.

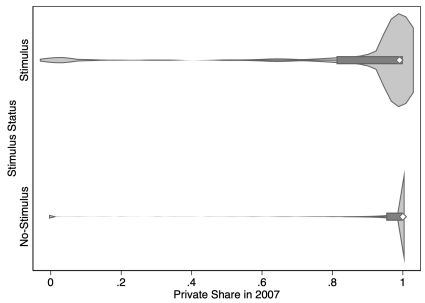
Figure B.2: Exposure to Stimulus and Pre-Shock Private Share (Alternative Measure 2)



Panel A: Market Defined at CIC 2-digit Level



Panel B: Market Defined at CIC 3-digit Level

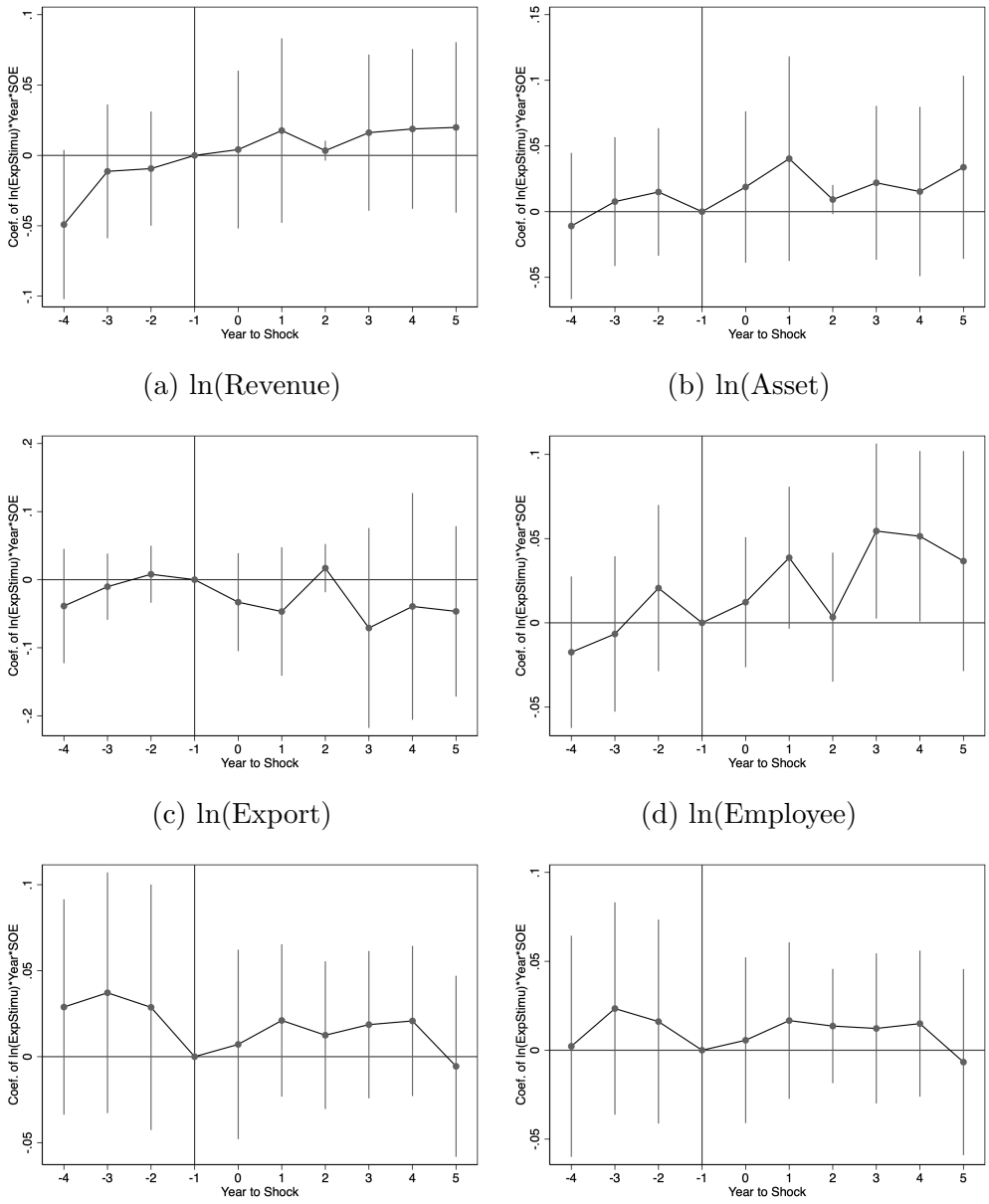


Panel C: Market Defined at CIC 4-digit Level

Notes: Each panel shows the medians (in diamond shape), the first to third quartiles (in rectangular box), the ranges, and the kernel densities of private shares, conditional on status of receiving stimulus. A sector-province pair is defined as treated (Stimulus) if its size ranks above the median within the respective aggregated sector. Sectors are classified at the CIC 2-, 3-, and 4-digit levels respectively. Firm's ownership is defined by firm's registration type. Private shares are computed as in Section 2.2.2.

B.2.2 Additional Figures for Robustness Checks

Figure B.3: Impact of Stimulus on Firm Performance, More Privatized Markets



(a) ln(Revenue)

(b) ln(Asset)

(c) ln(Export)

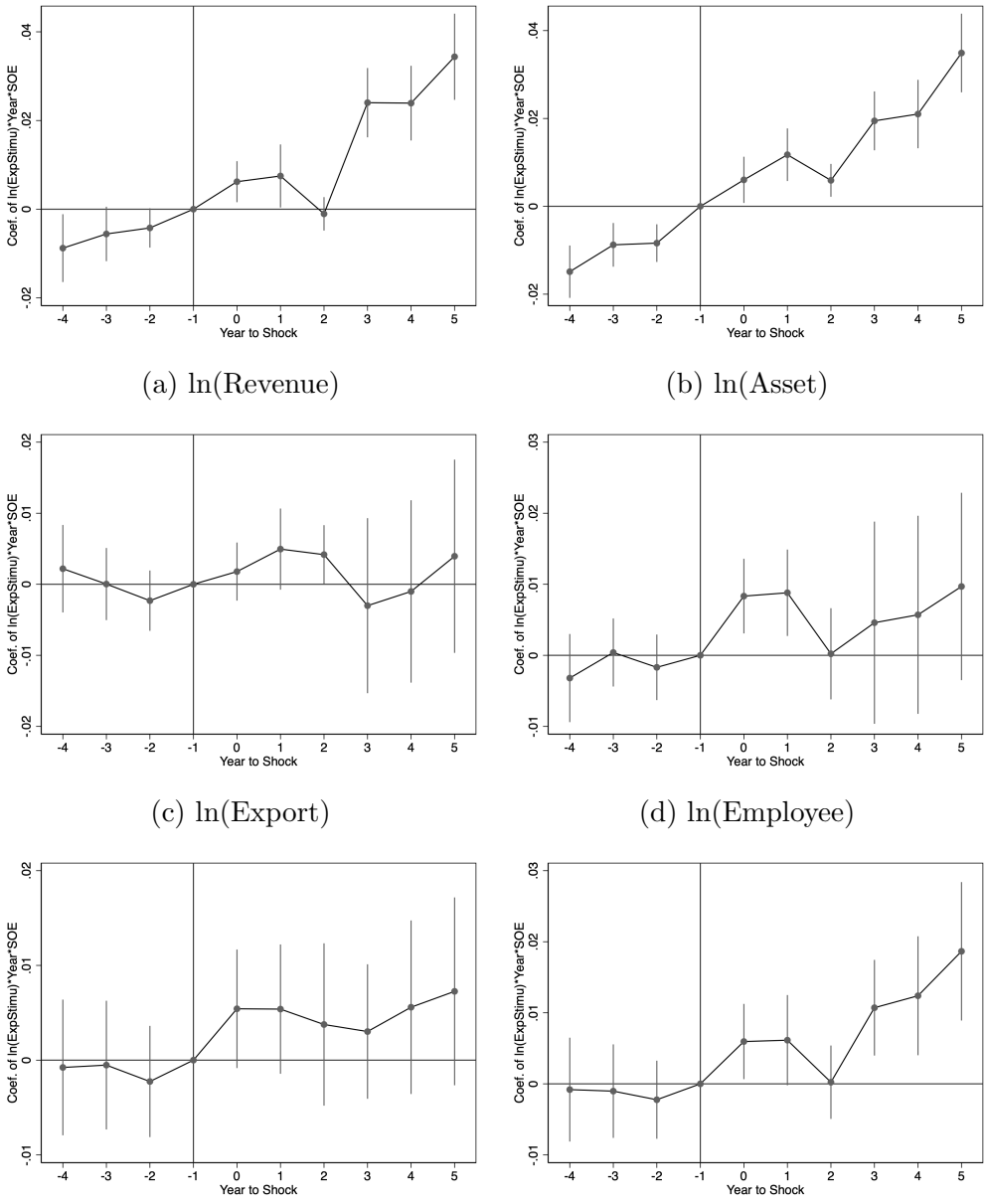
(d) ln(Employee)

(e) ln(TFP_OP)

(f) ln(TFP_LP)

Note: ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Sample is split by whether the market a firm operates in has an above or below median level of privatization under its corresponding aggregated sector. Ownership is defined by firm's registration type in year 2007, the base year. Standard errors are clustered at the sector-province level.

Figure B.4: Impact of Stimulus on Firm Performance, Less Privatized Markets



(a) ln(Revenue)

(b) ln(Asset)

(c) ln(Export)

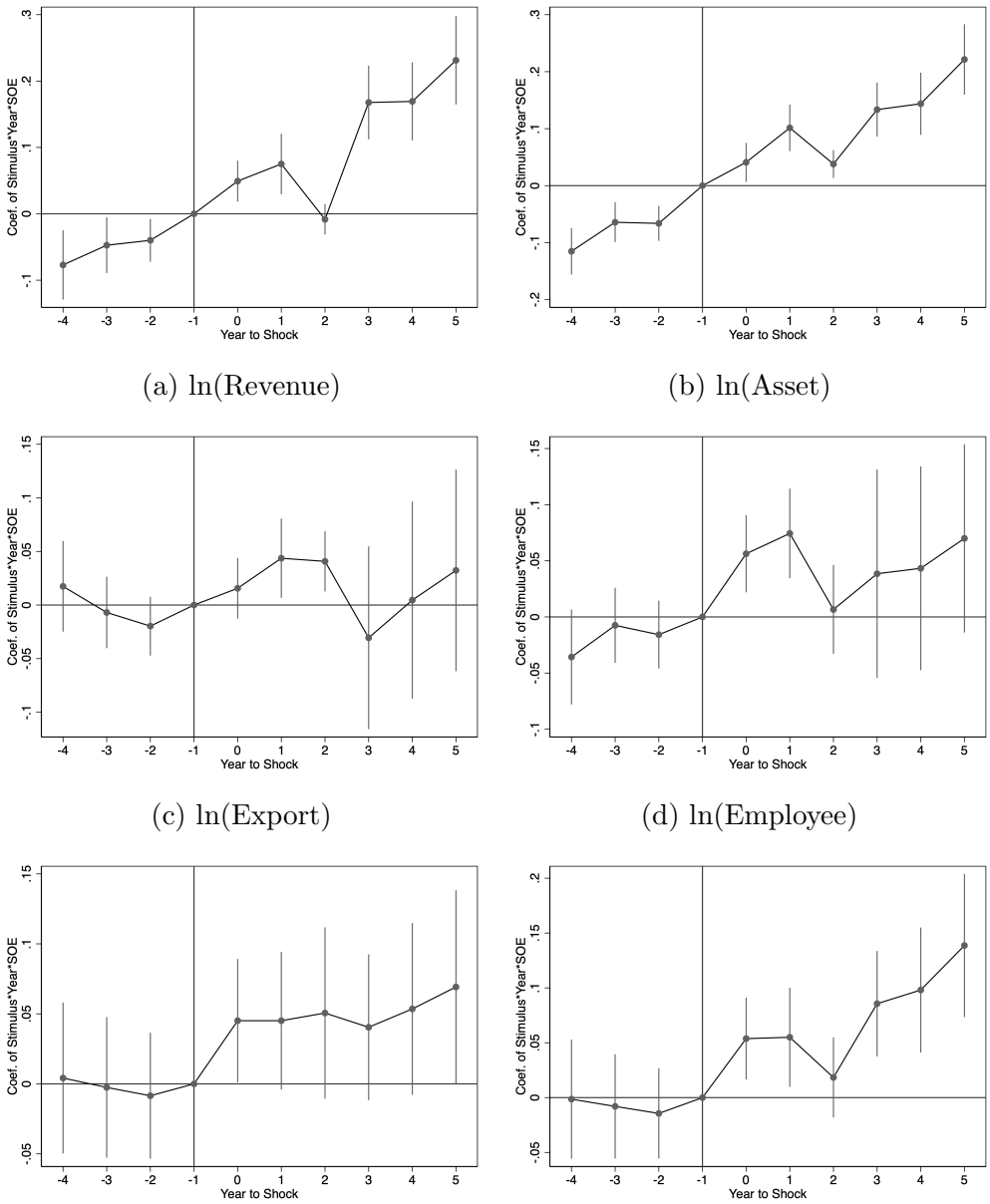
(d) ln(Employee)

(e) ln(TFP_OP)

(f) ln(TFP_LP)

Note: ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Sample is split by whether the market a firm operates in has an above or below median level of privatization under its corresponding aggregated sector. Ownership is defined by firm's registration type in year 2007, the base year. Standard errors are clustered at the sector-province level.

Figure B.5: Impact of Stimulus on Firm Performance, Alternative Definition



(a) ln(Revenue)

(b) ln(Asset)

(c) ln(Export)

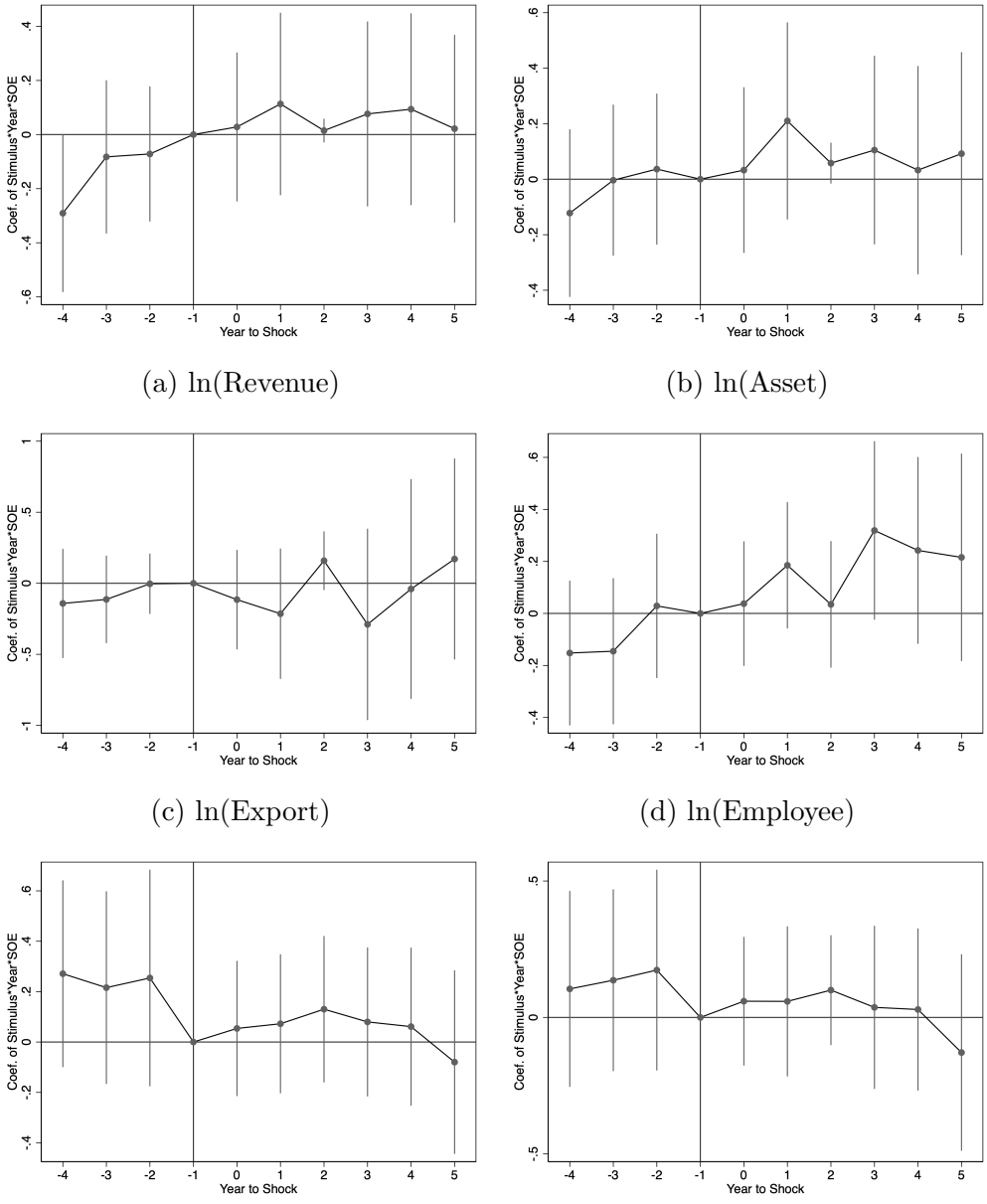
(d) ln(Employee)

(e) ln(TFP_OP)

(f) ln(TFP_LP)

Note: Stimulus is a dummy equal to one if the market has expected stimulus greater than zero. Markets are defined at the CIC 4-digit level with each province. Ownership is defined by firm's registration type in year 2007, the base year. Standard errors are clustered at the sector-province level.

Figure B.6: Impact of Stimulus on Firm Performance, More Privatized Markets, Alternative Definition



(a) ln(Revenue)

(b) ln(Asset)

(c) ln(Export)

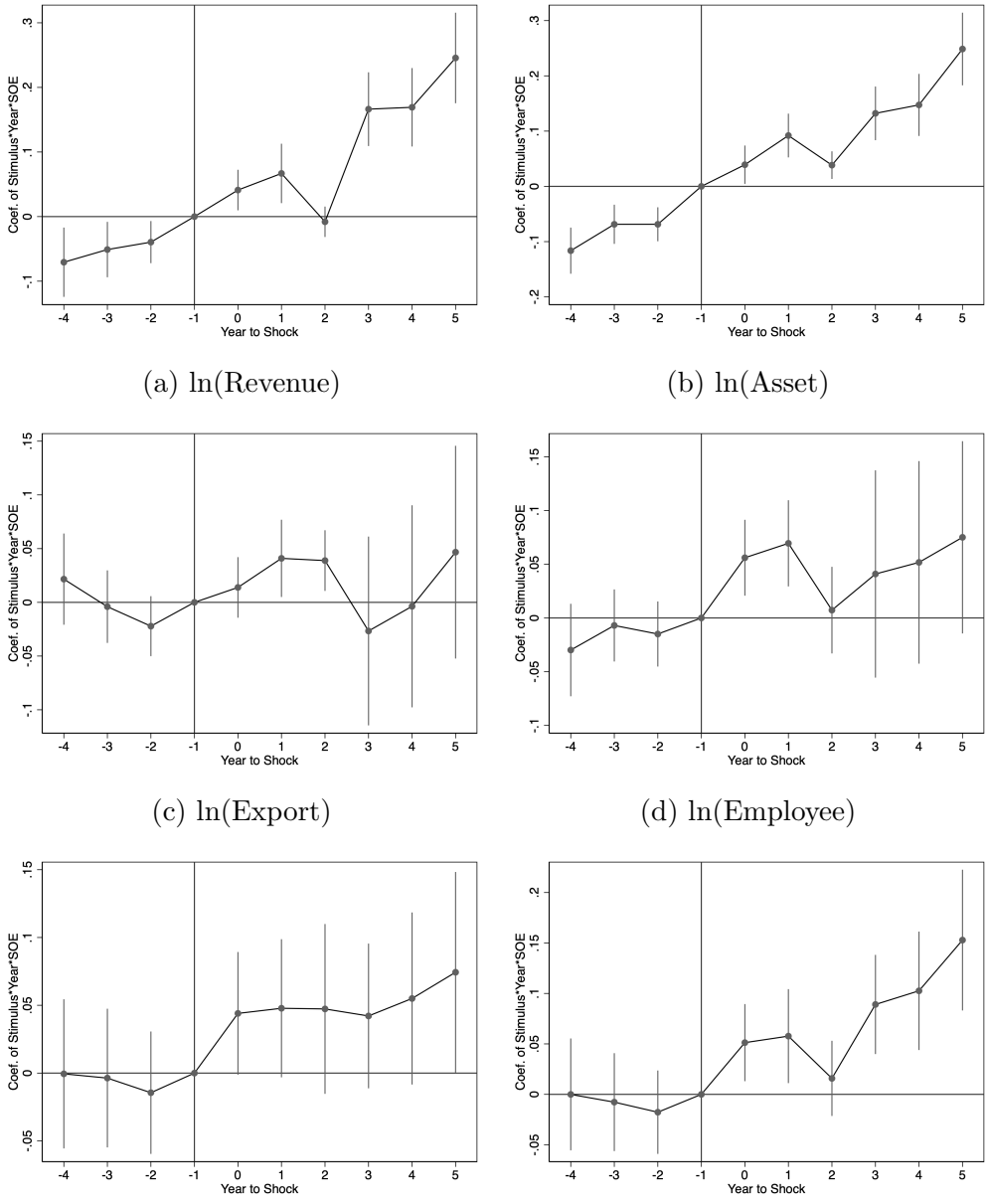
(d) ln(Employee)

(e) ln(TFP_OP)

(f) ln(TFP_LP)

Note: Stimulus is a dummy equal to one if the market has expected stimulus greater than zero. Markets are defined at the CIC 4-digit level with each province. Sample is split by whether the market a firm operates in has an above or below median level of privatization under its corresponding aggregated sector. Ownership is defined by firm's registration type in year 2007, the base year. Standard errors are clustered at the sector-province level.

Figure B.7: Impact of Stimulus on Firm Performance, Less Privatized Markets, Alternative Definition



(a) ln(Revenue)

(b) ln(Asset)

(c) ln(Export)

(d) ln(Employee)

(e) ln(TFP_OP)

(f) ln(TFP_LP)

Note: Stimulus is a dummy equal to one if the market has expected stimulus greater than zero. Markets are defined at the CIC 4-digit level with each province. Sample is split by whether a firm operates in has an above or below median level of privatization under its corresponding aggregated sector. Ownership is defined by firm's registration type in year 2007, the base year. Standard errors are clustered at the sector-province level.

B.3 Additional Tables

B.3.1 Alternative Ownership Definition

Table B.1: Summary Statistics for Key Variables, by Ownership

Panel A: Summary Statistics for 2004

	(1) Non-SOE		(2) SOE		(3) Difference	
	Mean	SD	Mean	SD	Mean Diff.	T-Stat
ln(Revenue)	3.13	1.11	3.46	1.76	-0.33***	(-22.14)
ln(Asset)	2.83	1.27	4.07	1.75	-1.25***	(-82.88)
ln(Export)	0.86	1.47	0.36	1.14	0.50***	(49.03)
EBIT	4.19	170.56	17.45	306.77	-13.26***	(-5.08)
ln(Employee)	4.72	1.06	5.48	1.47	-0.76***	(-60.35)
Firm Age	8.88	9.15	27.04	19.56	-18.16***	(-109.50)
Observations	193522		14135		207657	

Panel B: Summary Statistics for 2007

	(1) Non-SOE		(2) SOE		(3) Difference	
	Mean	SD	Mean	SD	Mean Diff.	T-Stat
ln(Revenue)	3.43	1.22	3.91	1.87	-0.48***	(-30.90)
ln(Asset)	2.97	1.29	4.31	1.85	-1.33***	(-86.30)
ln(Export)	0.73	1.48	0.38	1.25	0.35***	(32.98)
EBIT	6.88	219.96	40.80	549.03	-33.91***	(-7.46)
ln(Employee)	4.60	1.06	5.42	1.52	-0.83***	(-65.40)
Firm Age	9.31	8.75	27.68	20.17	-18.37***	(-109.97)
Observations	322063		14704		336767	

Panel C: Summary Statistics for 2008

	(1) Non-SOE		(2) SOE		(3) Difference	
	Mean	SD	Mean	SD	Mean Diff.	T-Stat
ln(Revenue)	3.40	1.24	4.19	1.78	-0.79***	(-51.26)
ln(Asset)	2.95	1.28	4.46	1.86	-1.50***	(-93.75)
ln(Export)	0.74	1.48	0.39	1.26	0.35***	(31.08)
EBIT	6.67	240.40	37.43	797.75	-30.76***	(-4.51)
ln(Employee)	4.47	1.09	5.39	1.51	-0.92***	(-70.75)
Firm Age	8.97	8.29	25.74	20.07	-16.77***	(-97.74)
Observations	397653		13754		411407	

Panel D: Summary Statistics for 2013

	(1) Non-SOE		(2) SOE		(3) Difference	
	Mean	SD	Mean	SD	Mean Diff.	T-Stat
ln(Revenue)	4.61	1.11	5.30	1.54	-0.70***	(-46.65)
ln(Asset)	3.95	1.30	5.61	1.70	-1.66***	(-100.53)
ln(Export)	1.22	1.93	0.78	1.75	0.45***	(17.79)
EBIT	18.32	245.78	70.66	918.28	-52.33***	(-5.93)
ln(Employee)	5.68	0.80	6.06	1.11	-0.38***	(-35.25)
Firm Age	11.09	8.25	24.53	20.83	-13.44***	(-67.21)
Observations	328077		10898		338975	

Note: Revenue, EBIT, Asset and Export are denominated in millions RMB. All variables except EBIT are log-transformed. Employee is denominated in number of person. Firm Age is denominated in number of year. Ownership is defined by firm's registration type in the corresponding year. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table B.2: Summary Statistics for Key Variables, by Ownership, Alternative Definition

Panel A: Summary Statistics for 2004

	(1) Non-SOE		(2) SOE		(3) Difference	
	Mean	SD	Mean	SD	Mean Diff.	T-Stat
ln(Revenue)	3.13	1.12	3.43	1.71	-0.30***	(-19.84)
ln(Asset)	2.84	1.28	4.04	1.70	-1.20***	(-78.73)
ln(Export)	0.85	1.47	0.38	1.16	0.47***	(44.07)
EBIT	4.21	170.40	18.46	317.76	-14.25***	(-5.05)
ln(Employee)	4.73	1.07	5.44	1.42	-0.71***	(-55.72)
Firm Age	9.06	9.59	25.89	18.91	-16.82***	(-100.30)
Observations	194725		12932		207657	

Panel B: Summary Statistics for 2007

	(1) Non-SOE		(2) SOE		(3) Difference	
	Mean	SD	Mean	SD	Mean Diff.	T-Stat
ln(Revenue)	3.43	1.23	3.86	1.79	-0.43***	(-26.13)
ln(Asset)	2.99	1.31	4.27	1.74	-1.28***	(-80.36)
ln(Export)	0.73	1.48	0.37	1.24	0.36***	(30.93)
EBIT	7.14	220.74	41.00	586.97	-33.86***	(-6.35)
ln(Employee)	4.60	1.08	5.39	1.43	-0.78***	(-59.76)
Firm Age	9.50	9.18	26.55	19.47	-17.06***	(-96.36)
Observations	324570		12197		336767	

Panel C: Summary Statistics for 2008

	(1) Non-SOE		(2) SOE		(3) Difference	
	Mean	SD	Mean	SD	Mean Diff.	T-Stat
ln(Revenue)	3.41	1.24	4.11	1.73	-0.71***	(-44.09)
ln(Asset)	2.96	1.30	4.41	1.76	-1.45***	(-88.49)
ln(Export)	0.74	1.48	0.41	1.29	0.33***	(26.51)
EBIT	6.79	247.13	38.54	787.63	-31.75***	(-4.37)
ln(Employee)	4.48	1.10	5.40	1.37	-0.92***	(-72.07)
Firm Age	9.08	8.59	24.64	19.28	-15.55***	(-87.31)
Observations	399628		11779		411407	

Panel D: Summary Statistics for 2013

	(1) Non-SOE		(2) SOE		(3) Difference	
	Mean	SD	Mean	SD	Mean Diff.	T-Stat
ln(Revenue)	4.61	1.11	5.26	1.52	-0.65***	(-42.52)
ln(Asset)	3.96	1.30	5.54	1.70	-1.58***	(-92.40)
ln(Export)	1.22	1.93	0.87	1.84	0.35***	(12.99)
EBIT	18.51	249.25	68.57	920.54	-50.05***	(-5.46)
ln(Employee)	5.69	0.81	6.02	1.09	-0.33***	(-30.37)
Firm Age	11.18	8.47	22.60	20.03	-11.42***	(-57.20)
Observations	328849		10126		338975	

Note: Revenue, EBIT, Asset and Export are denominated in millions RMB. All variables except EBIT are log-transformed. Employee is denominated in number of person. Firm Age is denominated in number of year. Ownership is defined by firm's contributed capital in the corresponding year. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

B.3.2 Additional Regression Tables

B.3.2.1 Additional Tables for Main Empirical Results

Table B.3: Impact of Stimulus on Firm Performance by Year

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
ln(ExpStimu)*2004*SOE	-0.00964**	-0.0146***	0.00154	-0.00389	-0.00159	-0.000586	-0.00129	0.00644***
	(0.00382)	(0.00298)	(0.00315)	(0.00312)	(0.00360)	(0.00359)	(0.00367)	(0.00189)
ln(ExpStimu)*2005*SOE	-0.00501	-0.00812***	-0.000175	0.000712	-0.00107	-0.000485	-0.00113	0.00198
	(0.00308)	(0.00253)	(0.00254)	(0.00245)	(0.00344)	(0.00342)	(0.00330)	(0.00180)
ln(ExpStimu)*2006*SOE	-0.00415*	-0.00799***	-0.00194	-0.00162	-0.00223	-0.00179	-0.00195	-0.000562
	(0.00224)	(0.00218)	(0.00214)	(0.00235)	(0.00300)	(0.00297)	(0.00277)	(0.00154)
ln(ExpStimu)*2008*SOE	0.00715***	0.00661**	0.00177	0.00847***	0.00552*	0.00531*	0.00600**	-0.000466
	(0.00235)	(0.00264)	(0.00215)	(0.00261)	(0.00316)	(0.00313)	(0.00266)	(0.00164)
ln(ExpStimu)*2009*SOE	0.00777**	0.0123***	0.00474	0.00911***	0.00579*	0.00534	0.00573*	
	(0.00361)	(0.00319)	(0.00300)	(0.00311)	(0.00342)	(0.00338)	(0.00315)	
ln(ExpStimu)*2010*SOE	-0.00111	0.00580***	0.00414**	0.0000235	0.00425	0.00403	0.000439	-0.000361
	(0.00188)	(0.00187)	(0.00210)	(0.00321)	(0.00436)	(0.00429)	(0.00259)	(0.000282)
ln(ExpStimu)*2011*SOE	0.0242***	0.0195***	-0.00462	0.00381	0.00381	0.00289	0.0104***	-0.00451
	(0.00390)	(0.00334)	(0.00618)	(0.00703)	(0.00355)	(0.00352)	(0.00335)	(0.00311)
ln(ExpStimu)*2012*SOE	0.0240***	0.0206***	-0.00119	0.00442	0.00668	0.00555	0.0120***	0.00169
	(0.00418)	(0.00387)	(0.00649)	(0.00689)	(0.00453)	(0.00452)	(0.00414)	(0.00322)
ln(ExpStimu)*2013*SOE	0.0326***	0.0314***	0.000794	0.00779	0.00844*	0.00716	0.0175***	-0.0000908
	(0.00473)	(0.00435)	(0.00664)	(0.00631)	(0.00478)	(0.00476)	(0.00464)	(0.00346)
Observations	2355344	2354121	2154702	2338382	2270535	2270535	2270535	1799485
Adj. R-Squared	0.846	0.896	0.805	0.798	0.424	0.435	0.653	0.798
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Ownership is defined by firm's registration type in year 2007. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table B.4: Impact of Stimulus on Firm Performance, More Privatized Markets

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
ln(ExpStimu)*2004*SOE	-0.0491*	-0.0109	-0.0387	-0.0174	0.0266	0.0289	0.00222	-0.00175
	(0.0270)	(0.0284)	(0.0428)	(0.0229)	(0.0321)	(0.0320)	(0.0317)	(0.0155)
ln(ExpStimu)*2005*SOE	-0.0113	0.00766	-0.0103	-0.00660	0.0384	0.0372	0.0234	-0.00179
	(0.0243)	(0.0250)	(0.0248)	(0.0235)	(0.0357)	(0.0357)	(0.0305)	(0.0144)
ln(ExpStimu)*2006*SOE	-0.00935	0.0150	0.00796	0.0206	0.0296	0.0287	0.0161	-0.00721
	(0.0207)	(0.0248)	(0.0214)	(0.0252)	(0.0364)	(0.0364)	(0.0293)	(0.0124)
ln(ExpStimu)*2008*SOE	0.00417	0.0188	-0.0331	0.0122	0.00855	0.00713	0.00562	-0.00930
	(0.0286)	(0.0294)	(0.0367)	(0.0197)	(0.0278)	(0.0281)	(0.0238)	(0.0123)
ln(ExpStimu)*2009*SOE	0.0177	0.0403	-0.0468	0.0387*	0.0232	0.0211	0.0167	
	(0.0334)	(0.0397)	(0.0481)	(0.0215)	(0.0221)	(0.0226)	(0.0225)	
ln(ExpStimu)*2010*SOE	0.00340	0.00924	0.0170	0.00335	0.0118	0.0125	0.0136	-0.000436
	(0.00360)	(0.00565)	(0.0181)	(0.0195)	(0.0220)	(0.0219)	(0.0164)	(0.000469)
ln(ExpStimu)*2011*SOE	0.0162	0.0220	-0.0710	0.0545**	0.0163	0.0186	0.0123	-0.00136
	(0.0283)	(0.0299)	(0.0749)	(0.0265)	(0.0216)	(0.0218)	(0.0215)	(0.0181)
ln(ExpStimu)*2012*SOE	0.0188	0.0153	-0.0393	0.0514**	0.0185	0.0208	0.0150	-0.0190
	(0.0289)	(0.0329)	(0.0850)	(0.0258)	(0.0218)	(0.0223)	(0.0210)	(0.0207)
ln(ExpStimu)*2013*SOE	0.0199	0.0338	-0.0466	0.0367	-0.00998	-0.00560	-0.00667	-0.0371
	(0.0309)	(0.0356)	(0.0639)	(0.0333)	(0.0265)	(0.0268)	(0.0267)	(0.0349)
Observations	694406	693948	638144	690113	669755	669755	669755	525588
Adj. R-Squared	0.859	0.904	0.817	0.809	0.484	0.498	0.700	0.569
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Sample is split by whether the market a firm operates in has an above or below median level of privatization under its corresponding aggregated sector. Ownership is defined by firm's registration type in year 2007, the base year. Standard errors are clustered at the sector-province level. Capital information is missing in 2009 in the dataset, hence the coefficient for 2009 under PrivateRatio is missing. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table B.5: Impact of Stimulus on Firm Performance, Less Privatized Markets

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
ln(ExpStimu)*2004*SOE	-0.00878**	-0.0149***	0.00219	-0.00320	-0.00181	-0.000770	-0.000813	0.00612***
	(0.00390)	(0.00304)	(0.00314)	(0.00316)	(0.00367)	(0.00365)	(0.00373)	(0.00191)
ln(ExpStimu)*2005*SOE	-0.00559*	-0.00877***	0.0000327	0.000399	-0.00116	-0.000523	-0.00103	0.00119
	(0.00313)	(0.00254)	(0.00259)	(0.00244)	(0.00349)	(0.00346)	(0.00336)	(0.00181)
ln(ExpStimu)*2006*SOE	-0.00422*	-0.00837***	-0.00231	-0.00169	-0.00275	-0.00225	-0.00224	-0.00142
	(0.00226)	(0.00219)	(0.00216)	(0.00235)	(0.00302)	(0.00299)	(0.00280)	(0.00153)
ln(ExpStimu)*2008*SOE	0.00620***	0.00603**	0.00179	0.00834***	0.00563*	0.00542*	0.00595**	-0.000523
	(0.00236)	(0.00268)	(0.00209)	(0.00268)	(0.00322)	(0.00319)	(0.00270)	(0.00169)
ln(ExpStimu)*2009*SOE	0.00749**	0.0118***	0.00495*	0.00881***	0.00595*	0.00539	0.00614*	
	(0.00363)	(0.00305)	(0.00291)	(0.00310)	(0.00352)	(0.00348)	(0.00324)	
ln(ExpStimu)*2010*SOE	-0.00105	0.00592***	0.00417**	0.000215	0.00401	0.00376	0.000229	-0.000391
	(0.00192)	(0.00191)	(0.00212)	(0.00327)	(0.00444)	(0.00437)	(0.00263)	(0.000287)
ln(ExpStimu)*2011*SOE	0.0240***	0.0195***	-0.00301	0.00459	0.00401	0.00302	0.0107***	-0.00519
	(0.00399)	(0.00342)	(0.00628)	(0.00726)	(0.00365)	(0.00362)	(0.00343)	(0.00319)
ln(ExpStimu)*2012*SOE	0.0239***	0.0210***	-0.00101	0.00570	0.00680	0.00559	0.0124***	0.00172
	(0.00430)	(0.00397)	(0.00655)	(0.00711)	(0.00468)	(0.00467)	(0.00427)	(0.00330)
ln(ExpStimu)*2013*SOE	0.0344***	0.0349***	0.00395	0.00969	0.00892*	0.00726	0.0187***	0.00131
	(0.00496)	(0.00456)	(0.00694)	(0.00673)	(0.00507)	(0.00506)	(0.00497)	(0.00373)
Observations	1627779	1627123	1485892	1615230	1567945	1567945	1567945	1248851
Adj. R-Squared	0.853	0.903	0.809	0.808	0.421	0.431	0.651	0.811
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Sample is split by whether the market a firm operates in has an above or below median level of privatization under its corresponding aggregated sector. Ownership is defined by firm's registration type in year 2007, the base year. Standard errors are clustered at the sector-province level. Capital information is missing in 2009 in the dataset, hence the coefficient for 2009 under PrivateRatio is missing. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

B.3.2.2 Additional Tables for Robustness Checks

Table B.6: Impact of Stimulus on Firm Performance, Alternative Definition

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
Stimulus*2004*SOE	-0.0770***	-0.115***	0.0174	-0.0357*	-0.00294	0.00416	-0.00141	0.0488***
	(0.0267)	(0.0208)	(0.0216)	(0.0216)	(0.0276)	(0.0275)	(0.0277)	(0.0130)
Stimulus*2005*SOE	-0.0474**	-0.0643***	-0.00695	-0.00749	-0.00604	-0.00252	-0.00801	0.0196
	(0.0214)	(0.0179)	(0.0170)	(0.0171)	(0.0258)	(0.0256)	(0.0242)	(0.0124)
Stimulus*2006*SOE	-0.0399**	-0.0662***	-0.0197	-0.0158	-0.0112	-0.00847	-0.0144	-0.00242
	(0.0164)	(0.0156)	(0.0140)	(0.0154)	(0.0232)	(0.0230)	(0.0210)	(0.0109)
Stimulus*2008*SOE	0.0493***	0.0410**	0.0156	0.0563***	0.0462**	0.0451**	0.0538***	-0.00224
	(0.0157)	(0.0174)	(0.0145)	(0.0175)	(0.0227)	(0.0225)	(0.0190)	(0.0112)
Stimulus*2009*SOE	0.0750***	0.101***	0.0437**	0.0744***	0.0487*	0.0451*	0.0550**	
	(0.0233)	(0.0209)	(0.0189)	(0.0204)	(0.0253)	(0.0251)	(0.0230)	
Stimulus*2010*SOE	-0.00834	0.0379***	0.0407***	0.00664	0.0524*	0.0506	0.0184	-0.00206
	(0.0116)	(0.0124)	(0.0144)	(0.0202)	(0.0318)	(0.0312)	(0.0186)	(0.00177)
Stimulus*2011*SOE	0.168***	0.133***	-0.0306	0.0385	0.0470*	0.0404	0.0856***	-0.0372*
	(0.0283)	(0.0241)	(0.0435)	(0.0474)	(0.0268)	(0.0266)	(0.0245)	(0.0222)
Stimulus*2012*SOE	0.169***	0.144***	0.00457	0.0433	0.0613*	0.0535*	0.0981***	0.00963
	(0.0300)	(0.0279)	(0.0469)	(0.0463)	(0.0314)	(0.0313)	(0.0291)	(0.0243)
Stimulus*2013*SOE	0.231***	0.221***	0.0323	0.0700	0.0775**	0.0692**	0.139***	0.00277
	(0.0340)	(0.0315)	(0.0480)	(0.0428)	(0.0354)	(0.0352)	(0.0333)	(0.0269)
Observations	2355344	2354121	2154702	2338382	2270535	2270535	2270535	1799485
Adj. R-Squared	0.846	0.896	0.805	0.798	0.424	0.436	0.653	0.798
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Stimulus is a dummy equal to one if the market has expected stimulus greater than zero. Markets are defined at the CIC 4-digit level with each province. Ownership is defined by firm's registration type in year 2007, the base year. Standard errors are clustered at the sector-province level. Capital information is missing in 2009 in the dataset, hence the coefficient for 2009 under PrivateRatio is missing. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table B.7: Impact of Stimulus on Firm Performance, More Privatized Markets, Alternative Definition

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
Stimulus*2004*SOE	-0.291*	-0.122	-0.142	-0.152	0.256	0.271	0.105	-0.0289
	(0.149)	(0.154)	(0.196)	(0.142)	(0.190)	(0.189)	(0.183)	(0.0930)
Stimulus*2005*SOE	-0.0824	-0.00365	-0.113	-0.145	0.224	0.216	0.136	-0.0427
	(0.144)	(0.139)	(0.157)	(0.143)	(0.195)	(0.195)	(0.170)	(0.0865)
Stimulus*2006*SOE	-0.0717	0.0365	-0.00353	0.0289	0.260	0.254	0.173	-0.0717
	(0.127)	(0.139)	(0.108)	(0.141)	(0.219)	(0.219)	(0.188)	(0.0686)
Stimulus*2008*SOE	0.0281	0.0328	-0.115	0.0375	0.0571	0.0539	0.0594	-0.0527
	(0.140)	(0.152)	(0.179)	(0.122)	(0.137)	(0.137)	(0.120)	(0.0688)
Stimulus*2009*SOE	0.113	0.210	-0.214	0.185	0.0831	0.0725	0.0589	
	(0.172)	(0.181)	(0.234)	(0.124)	(0.140)	(0.141)	(0.140)	
Stimulus*2010*SOE	0.0147	0.0580	0.159	0.0348	0.128	0.130	0.100	-0.00255
	(0.0223)	(0.0375)	(0.105)	(0.124)	(0.150)	(0.148)	(0.103)	(0.00303)
Stimulus*2011*SOE	0.0764	0.105	-0.289	0.319*	0.0654	0.0799	0.0369	-0.0376
	(0.174)	(0.173)	(0.343)	(0.175)	(0.150)	(0.151)	(0.152)	(0.102)
Stimulus*2012*SOE	0.0937	0.0327	-0.0402	0.242	0.0413	0.0611	0.0290	-0.114
	(0.181)	(0.191)	(0.394)	(0.183)	(0.158)	(0.160)	(0.151)	(0.123)
Stimulus*2013*SOE	0.0218	0.0921	0.171	0.216	-0.113	-0.0798	-0.129	-0.223
	(0.177)	(0.186)	(0.361)	(0.203)	(0.184)	(0.186)	(0.184)	(0.175)
Observations	694406	693948	638144	690113	669755	669755	669755	525588
Adj. R-Squared	0.859	0.904	0.817	0.809	0.484	0.498	0.700	0.569
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Stimulus is a dummy equal to one if the market has expected stimulus greater than zero. Markets are defined at the CIC 4-digit level with each province. Sample is split by whether the market a firm operates in has an above or below median level of privatization under its corresponding aggregated sector. Ownership is defined by firm's registration type in year 2007, the base year. Standard errors are clustered at the sector-province level. Capital information is missing in 2009 in the dataset, hence the coefficient for 2009 under PrivateRatio is missing. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table B.8: Impact of Stimulus on Firm Performance, Less Privatized Markets, Alternative Definition

	(1) ln(Revenue)	(2) ln(Asset)	(3) ln(Export)	(4) ln(Employee)	(5) ln(TFP_OLS)	(6) ln(TFP_OP)	(7) ln(TFP_LP)	(8) PrivateRatio
Stimulus*2004*SOE	-0.0706***	-0.116***	0.0216	-0.0299	-0.00782	-0.000556	-0.0000542	0.0486***
	(0.0274)	(0.0213)	(0.0216)	(0.0220)	(0.0281)	(0.0281)	(0.0283)	(0.0132)
Stimulus*2005*SOE	-0.0510**	-0.0687***	-0.00400	-0.00697	-0.00760	-0.00368	-0.00770	0.0161
	(0.0219)	(0.0181)	(0.0172)	(0.0171)	(0.0263)	(0.0261)	(0.0247)	(0.0126)
Stimulus*2006*SOE	-0.0395**	-0.0686***	-0.0222	-0.0149	-0.0177	-0.0145	-0.0177	-0.00546
	(0.0166)	(0.0157)	(0.0142)	(0.0154)	(0.0232)	(0.0230)	(0.0211)	(0.0108)
Stimulus*2008*SOE	0.0411**	0.0392**	0.0139	0.0561***	0.0453*	0.0440*	0.0512***	-0.00232
	(0.0160)	(0.0177)	(0.0144)	(0.0180)	(0.0233)	(0.0231)	(0.0195)	(0.0115)
Stimulus*2009*SOE	0.0668***	0.0920***	0.0410**	0.0695***	0.0518**	0.0478*	0.0577**	
	(0.0234)	(0.0202)	(0.0183)	(0.0205)	(0.0262)	(0.0260)	(0.0237)	
Stimulus*2010*SOE	-0.00801	0.0384***	0.0389***	0.00730	0.0493	0.0473	0.0158	-0.00232
	(0.0120)	(0.0127)	(0.0144)	(0.0206)	(0.0325)	(0.0319)	(0.0190)	(0.00181)
Stimulus*2011*SOE	0.166***	0.132***	-0.0267	0.0410	0.0492*	0.0421	0.0891***	-0.0407*
	(0.0291)	(0.0247)	(0.0448)	(0.0492)	(0.0274)	(0.0272)	(0.0251)	(0.0230)
Stimulus*2012*SOE	0.169***	0.147***	-0.00369	0.0518	0.0637**	0.0550*	0.103***	0.0116
	(0.0310)	(0.0287)	(0.0480)	(0.0481)	(0.0324)	(0.0324)	(0.0299)	(0.0251)
Stimulus*2013*SOE	0.246***	0.249***	0.0467	0.0751	0.0854**	0.0744**	0.153***	0.0160
	(0.0357)	(0.0335)	(0.0505)	(0.0456)	(0.0379)	(0.0377)	(0.0356)	(0.0288)
Observations	1627779	1627123	1485892	1615230	1567945	1567945	1567945	1248851
Adj. R-Squared	0.853	0.903	0.809	0.808	0.421	0.431	0.651	0.811
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sec-Prov-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Stimulus is a dummy equal to one if the market has expected stimulus greater than zero. Markets are defined at the CIC 4-digit level with each province. Sample is split by whether the market a firm operates in has an above or below median level of privatization under its corresponding aggregated sector. Ownership is defined by firm's registration type in year 2007, the base year. Standard errors are clustered at the sector-province level. Capital information is missing in 2009 in the dataset, hence the coefficient for 2009 under PrivateRatio is missing. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table B.9: Impact of Stimulus on Aggregate Outcomes by Year

	(1) Performance Gap	(2) Performance Ratio	(3) Number Ratio	(4) Private Ratio	(5) Private Entry Rate	(6) Private Exit Rate	(7) State Entry Rate	(8) State Exit Rate
ln(ExpStimu)*PrivShare*2004	-0.0293***	22.74	-1.915***	0.0758***	0.0707***	-0.0120	-0.0412***	0.0120*
	(0.0113)	(13.97)	(0.433)	(0.00421)	(0.00327)	(0.0104)	(0.00330)	(0.00663)
ln(ExpStimu)*PrivShare*2005	-0.0165	5.748	-1.519***	0.0665***	0.0200***	-0.0420***	-0.0171***	0.0224***
	(0.0130)	(15.58)	(0.430)	(0.00448)	(0.00280)	(0.00970)	(0.00261)	(0.00514)
ln(ExpStimu)*PrivShare*2006	-0.0227*	39.92	-1.072***	0.0562***	0.0197***	-0.0429***	-0.0121***	0.0289***
	(0.0130)	(38.72)	(0.388)	(0.00415)	(0.00264)	(0.00942)	(0.00225)	(0.00500)
ln(ExpStimu)*PrivShare*2007	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
ln(ExpStimu)*PrivShare*2008	-0.00264	15.78	1.171**	0.0629***	0.0316***	-0.0190**	-0.0258***	0.0103**
	(0.0175)	(13.22)	(0.535)	(0.00454)	(0.00304)	(0.00899)	(0.00257)	(0.00489)
ln(ExpStimu)*PrivShare*2009	-0.00971	22.84	2.548***	0	0.00502*	-0.0297***	-0.0315***	0.00157
	(0.0165)	(21.74)	(0.371)	(.)	(0.00300)	(0.00949)	(0.00425)	(0.0138)
ln(ExpStimu)*PrivShare*2010	0.0280**	16.60	0.973**	0.0514***	0.0466***	0.0357**	-0.0562***	0.0148***
	(0.0126)	(13.81)	(0.443)	(0.00343)	(0.00270)	(0.0158)	(0.00311)	(0.00557)
ln(ExpStimu)*PrivShare*2011	-0.0244	13.34	0.745	0.0347***	0.0612***	0.0342**	-0.0327***	-0.118***
	(0.0235)	(12.70)	(0.664)	(0.00580)	(0.00329)	(0.0155)	(0.00344)	(0.0269)
ln(ExpStimu)*PrivShare*2012	0.0222	14.02	2.583***	0.0592***	0.00434	-0.0491***	-0.0126***	0.0190***
	(0.0146)	(13.21)	(0.504)	(0.00653)	(0.00286)	(0.0103)	(0.00294)	(0.00668)
ln(ExpStimu)*PrivShare*2013	0.00848	13.65	1.489***	0.0725***	0.0665***	0.269***	-0.0207***	0.0206***
	(0.0226)	(11.29)	(0.397)	(0.00636)	(0.00477)	(0.0687)	(0.00322)	(0.00797)
Observations	98491	32652	35533	81248	99424	94941	99424	94941
Adj. R-Squared	0.0504	0.218	0.736	0.617	0.325	0.0142	0.213	0.108
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market (p,k) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Variables are defined as aforementioned. Ownership is defined by firm's registration type in year 2007, the base year. Only coefficients of the triple interaction term are shown. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table B.10: Impact of Stimulus on Private Share Components by Year

	(1) Private Growth	(2) Stayers' Share	(3) Entrants' Share	(4) Privatized Share	(5) Exits' Share	(6) Nationalized Share
ln(ExpStimu)*PrivShare*2004	1.175***	0.0120***	0.0879***	0.0218***	-0.177**	-0.0130***
	(0.384)	(0.00322)	(0.00314)	(0.00182)	(0.0764)	(0.00186)
ln(ExpStimu)*PrivShare*2005	0.284	0.0656***	0.0321***	0.0215***	-0.208***	-0.0135***
	(1.091)	(0.00339)	(0.00264)	(0.00193)	(0.0706)	(0.00165)
ln(ExpStimu)*PrivShare*2006	1.959**	0.0677***	0.0277***	0.0213***	-0.221***	-0.0197***
	(0.786)	(0.00334)	(0.00249)	(0.00198)	(0.0709)	(0.00275)
ln(ExpStimu)*PrivShare*2007	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
ln(ExpStimu)*PrivShare*2008	2.652**	0.0481***	0.0446***	0.0225***	-0.154	-0.0219***
	(1.273)	(0.00302)	(0.00261)	(0.00216)	(0.0959)	(0.00304)
ln(ExpStimu)*PrivShare*2009	2.671*	0.0669***	0.0240***	0.0199***	-0.183**	-0.0144***
	(1.402)	(0.00340)	(0.00258)	(0.00222)	(0.0792)	(0.00183)
ln(ExpStimu)*PrivShare*2010	0.452	0.0180***	0.0760***	0.0202***	0.0338	-0.0249***
	(0.937)	(0.00285)	(0.00287)	(0.00184)	(0.176)	(0.00255)
ln(ExpStimu)*PrivShare*2011	2.022	0.0145***	0.0796***	0.0216***	-0.203*	-0.0237***
	(1.232)	(0.00313)	(0.00316)	(0.00229)	(0.115)	(0.00320)
ln(ExpStimu)*PrivShare*2012	2.428*	0.0796***	0.0116***	0.0206***	-0.199**	-0.0135***
	(1.406)	(0.00374)	(0.00243)	(0.00222)	(0.0895)	(0.00161)
ln(ExpStimu)*PrivShare*2013	2.820*	0.0125***	0.0837***	0.0191***	2.057***	-0.0380***
	(1.481)	(0.00469)	(0.00472)	(0.00221)	(0.587)	(0.00450)
Observations	93769	99424	99424	99424	94941	99424
Adj. R-Squared	0.106	0.413	0.310	0.0768	-0.00439	0.0950
Sec-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: ExpStimu is defined as the expected stimulus shock to be received, i.e., the share of revenue in aggregated sector going to market ($p.k$) at CIC 4-digit level between 2005 to 2007, then multiplied by the total stimulus spent on the aggregated sector. Variables are defined as aforementioned. Ownership is defined by firm's registration type in year 2007, the base year. Only coefficients of the triple interaction term are shown. No paid-in capital data is available for 2009. Standard errors are clustered at the sector-province level. *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

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