Entry Deregulation, Market Turnover, and Efficiency: China's Business Registration Reform*

Panle Jia Barwick Luming Chen Shanjun Li Xiaobo Zhang

August 2024

Abstract

Although entry regulation is ubiquitous across countries, comprehensive evaluations on how such regulations affect firm dynamics and productivity are lacking. We examine a 2012-2014 pilot program in Guangdong (which later became a national policy) that was designed to reduce firm registration costs and encourage entrepreneurial activities. Using administrative data on firms' business registrations and annual reports, our analysis shows that the reform increased firm entry by 25% and firm exit by 8.7% in the manufacturing sector. The productivity of post-reform entrants was 1.1% higher than the productivity of pre-reform entrants, likely due to relaxed financial constraints and more intense competition.

Keywords: Entry deregulation, productivity, market turnover

JEL Codes: L10, L50, L60, O40

*Barwick: Department of Economics, University of Wisconsin-Madison and NBER, email: pbarwick@wisc.edu; Chen: Stanford University, email: luming.chen@stanford.edu; Li: Dyson School of Applied Economics and Management, Cornell University and NBER, email: sl2448@cornell.edu; Zhang: Guanghua School of Management, Peking University and IFPRI, email: x.zhang@gsm.pku.edu.cn. We thank the editor and anonymous referees whose comments significantly improved the paper. We are grateful for the helpful discussions from Mark Roberts, Mo Xiao, and Daniel Yi Xu, and comments from Levon Barseghyan, Julieta Caunedo, Kaiji Chen, Hanming Fang, Chang-Tai Hsieh, Yusuke Kuroishi, Qihong Liu, Nancy Qian, Seth Sanders, Michael Song, Bernard Yeung, Xiaodong Zhu, and seminar/conference participants at ASSA Annual Meeting, CES Annual Meeting, Cornell University, NBER China Meeting, NEUDC, PKU GSM, and PKU NSD, as well as numerous data supports provided by the Center for Enterprise Research of Peking University. All errors remain our own.

1 Introduction

New firms are pivotal for economic growth (Foster et al., 2001; Brandt et al., 2012; Asturias et al., 2023), yet not all new entrants contribute positively. Some can be unproductive and even detrimental to the economy (Baumol, 1990). To address this, countries around the world have implemented entry regulations to screen potential entrants and steer them toward productive activities. However, these regulations can have unintended consequences. Distortive regulatory restrictions may deter productive entrepreneurs from starting businesses, while weakened market competition due to regulation-induced entry barriers can protect inefficient incumbents. In addition, entry regulation can encourage rent-seeking behavior to the detriment of society (Djankov et al., 2002). Despite the long-standing policy interests in entry regulation, there is a lack of comprehensive evaluation of its broad impacts on firm dynamics and productivity. This gap is largely due to empirical challenges, as entry regulations are rarely randomly designed and are often shaped by unobserved factors that also matter for firm dynamics and performance.

This paper fills the gap by examining China's 2014 Business Registration Reform, which was designed to reduce firm registration costs and encourage entrepreneurial activities.[1] The reform greatly simplified firm registration procedures and significantly relaxed registered capital requirements. Before implementing the reform nationwide, the government conducted a staggered pilot program across cities in Guangdong province.[2] Our analysis leverages the staggered rollout of this pilot program and takes advantage of the temporal and regional variations in the deregulation. We draw on administrative data from firm registration records and annual reports, as well as field enterprise surveys, to evaluate the impact of entry deregulation on firm entry, exit, size distribution, and productivity.

We first examine how the reform affects entry and exit using firm registration records and an event study framework. The key threat to identification is the potentially endogenous timing of

[1]We use "reform" and "entry deregulation" interchangeably in this paper.

[2]Guangdong is a coastal province in Southern China with 21 cities. It is the most populous province in China, with a population of 115 million as of 2019. It is also the wealthiest province, with a GDP surpassing that of Spain (the 13$^{\text{th}}$-largest economy in 2019) and Australia (the 14$^{\text{th}}$-largest economy). Major cities in Guangdong include Shenzhen and Guangzhou, which are among the most economically advanced in China.

the reform, because the rollout across cities might have been correlated with unobserved time-varying shocks to the outcome variables of interests. We address this concern in three ways. First, because firm entry is measured at the monthly level, we use city-by-year fixed effects to control for temporal city-specific shocks and exploit month-to-month variation within a city-year pair. Therefore, our identification relies on a weaker assumption: the exact month (rather than the year) of the program's implementation is exogenous. This assumption is supported by the fact that there is no clear relationship between city characteristics and the rollout orders. Second, we adopt a triple difference strategy to investigate the heterogeneous impacts across industries with differential degrees of deregulation and exploit within city-year across-industry variations. Third, we conduct event studies and provide evidence that the outcome variables were stable before the reform.

Our findings are quite striking. China's entry deregulation increased firm entry by 25%. The effect persists over time and is much larger than previous estimates from other countries (Kaplan et al., 2011; Branstetter et al., 2013). A host of robustness checks suggests that the large increase in firm entry was not driven by the proliferation of shell companies, the spin-off of existing incumbents, the reclassification of informal businesses, or the relocation of firms from other regions. Instead, most of the newly registered firms represented *de novo* initiatives by entrepreneurs. The reform led to heterogeneous impacts across firm types and industries: private firms and industries with higher degrees of deregulation experienced a greater increase in new entrants. Along with higher entry rates, exit rates went up by 8.7%, mainly in more deregulated industries. The combination of rising entry and exit created a higher market turnover rate overall.

We next evaluate the effect of entry deregulation on firm size. The size of new entrants declined in the wake of the reform because the deregulation relaxed previously stringent capital requirements, allowing smaller firms to enter. In contrast, exiting firms were also larger post reform, consistent with the theoretical insights of Hopenhayn (1992). Entry deregulation intensified market competition, which in turn drove the marginal incumbents out of business and shifted rightward the survival threshold.

The third outcome we examine is firm productivity. Our baseline productivity estimation follows Aw et al. (2011), whose data structure is similar to ours. We conduct robustness analyses using alternative productivity measures, i.e., those by Olley and Pakes (1996), Levinsohn and Petrin (2003), Brandt et al. (2021), as well as the revenue-over-capital ratios (asset turnover rates). While entry deregulation could lead to weaker screening and allow unproductive firms to enter (i.e., the *weaker-screening* effect), there are two countervailing forces in our context. First, the reform introduces more intense competition and drives down the profit margins, preventing the less productive potential entrants from entering the market (i.e., the *market-competition* effect). Second, entry deregulation can potentially change the composition of new entrants and allow productive yet financially constrained entrepreneurs to establish new businesses, who would otherwise be precluded due to their limited resources (i.e., the *composition* effect). Our analysis shows that entrant productivity improved by 1.1% after the reform, suggesting that the *market-competition* effect and the *composition* effect outweighed the *weaker-screening* effect from more lax regulations.[3]

We next examine the underlying channels driving the observed efficiency gains in entrants' productivity. Our analysis provides strong evidence of the *composition* effect in that the alleviation of financial constraints led to significant changes in entrant composition. First, survey data suggests that fewer entrepreneurs borrowed from friends, relatives, or financial institutions to finance startup costs post-reform. Second, entry deregulation opened up opportunities for less-educated and younger entrepreneurs, who were more likely to be financially constrained. Individual shareholders (natural persons), with fewer financial resources than corporate shareholders (legal persons), played a greater role in forming new firms after the reform. Third, the productivity improvement was greater for small firms, private firms, and those in industries that had previously required higher levels of registered capital and in markets with more limited access to finance. Therefore, the productivity gains among entrants stem disproportionately from the subset of firms that experienced a more pronounced easing of financial constraints.

[3] The magnitude of productivity gains is comparable to other studies that have examined China's recent large-scale policy changes. For example, Yu (2015) estimates that a 1 percentage point decrease in output (input) tariffs leads to a 0.92% (0.51)% productivity gain in China's manufacturing sector in 2000-2006. Chen et al. (2021) find that increasing the real R&D by 10% improves firm productivity by 0.9% in China's manufacturing sector in 2008-2011. Liu and Mao (2019) show that China's recent sweeping Value-Added Tax (VAT) reform that allowed firms to deduct purchases of fixed assets from the VAT bases improved firm TFP by 8.9%.

We also find evidence for the *market-competition* effect. To isolate the competition effect from the other two channels (the *weaker-screening* effect and the *composition* effect), we focus on firms that entered *immediately before* the announcement of the reform. These firms still faced stringent regulations at the time of entry, thus shutting down the screening effect and the composition effect, but experienced more intense market competition after the reform. Consequently, their productivity improvements relative to the earlier entry cohorts, if any, would reflect the *market-competition* effect. Indeed, these firms also exhibited productivity improvements, though the magnitude was smaller than the gains exhibited by firms that entered after the reform.

Lastly, we quantify the contribution of entry deregulation to the aggregate economy. Following Foster et al. (2008), we conduct a decomposition exercise to measure the relative contribution to aggregate productivity growth by entrants, exiting firms, and continuing incumbents. After the reform, entrants made a much larger contribution to the annual aggregate productivity growth, consistent with the insights of Asturias et al. (2023). For example, entrants contributed to 15.1% and 23.8% of the overall productivity change in 2013 and 2014, respectively. At the extensive margin, these deregulation-induced newly registered firms increased Guangdong's total employment and revenue in the manufacturing sector by 2.5% and 1.8% during the first three years post-reform and are projected to generate 14.4% additional employment and 11.1% additional revenue in ten years.

Our paper contributes to the following three strands of literature. First, our paper directly speaks to the literature on how entry deregulation affects entrepreneurial activities (Djankov et al., 2002; Klapper et al., 2006; Kaplan et al., 2011; Bruhn, 2011; Branstetter et al., 2013).[4] The literature generally finds that entry regulation entails bureaucratic hurdles that discourage would-be entrants and hamper entrepreneurial activities, and that entry deregulation tends to encourage firm entry.

[4] There is a large literature examining the outcomes of entry (de)regulation based on different types of data variation. Some influential studies are based on cross-country variation in entry regulations (Djankov et al., 2002, 2006; Djankov, 2009). To better address omitted variables in cross-country analyses, other papers exploit cross-industry variations within a country (Klapper et al., 2006; Ciccone and Papaioannou, 2007; Fisman and Allende, 2010). We follow the literature that exploits policy shocks in entry (de)regulation for identification (Bertrand and Kramarz, 2002; Aghion et al., 2008; Schivardi and Viviano, 2011; Kaplan et al., 2011; Bruhn, 2011; Branstetter et al., 2013; Yakovlev and Zhuravskaya, 2013; Alfaro and Chari, 2014). Among these papers, Schivardi and Viviano (2011) studied the impact of entry barriers on sectoral performance and incumbents' productivity in the Italian retail trade sector and Branstetter et al. (2013) evaluated the impact of entry deregulation in Portugal on the number and characteristics of new entrants.

Most existing studies focus on the number and characteristics of new entrants. By comparison, the impacts on market turnover and firm productivity have been under-explored in earlier studies. Our paper fills in the gap by examining the impacts on both the extensive margins (firm entry and exit) and the intensive margins (size and productivity).

Second, our paper relates to the literature that uses structural approaches to study how entry costs shape market structures and industry dynamics (Hopenhayn, 1992; Dunne et al., 2013; Maican and Orth, 2015, 2018). Different from this literature that commonly uses modeling assumptions to address policy endogeneity, our empirical strategy relies on a quasi-experiment to draw causal conclusions on the direct and general equilibrium effects of policy changes that reduce entry costs. There is also an active literature evaluating the impact of entry costs on the aggregate TFP (Barseghyan, 2008; Poschke, 2010; Herrendorf and Teixeira, 2011; Boedo and Mukoyama, 2012; Hopenhayn, 2014; Restuccia and Rogerson, 2017; Fattal-Jaef, 2022; Asturias et al., 2023). These studies are often based on cross-country data and potentially suffer from the omitted variable problem. In contrast, our identification strategy exploits city-industry-level variations within a single province that share the same institutions and culture.

Third, our paper contributes to the broad literature that examines how regulations and reforms affect the TFP in China, such as removing international trade quotas (Khandelwal et al., 2013), reforming state-owned enterprises (Hsieh et al., 2015), and reducing internal trade barriers (Tombe and Zhu, 2019), among others. Two recent articles highlight the importance of entry barriers to aggregate economic growth in China (Brandt et al., 2020; Jiang et al., 2021). Different from these two studies that structurally estimate entry barriers (Brandt et al. (2020)) or measure entry barriers using the external World Bank report (Jiang et al., 2021), our paper is the first one to directly exploit *observed* changes in firm behavior that are induced by a massive entry deregulation reform in China. Instead of focusing on aggregate economic growth, our paper provides richer firm-level evidence on entry, exit, size, and productivity.

The rest of this paper is organized as follows: Section 2 describes the policy background on entry deregulation and discusses data sources. Section 3 presents the empirical model. Section 4 discusses results on firm entry, exit, size, and productivity and summarizes the robustness checks. Section 5 explores the underlying channels and the aggregate implications of entrants' productivity improvement. Section 6 concludes.

2 Policy Background and Data

2.1 Entry Registration Reform

The common rationale behind entry regulations is to screen firms, allow only qualified ones to enter, and direct them toward productive activities. However, in developing countries, entry regulations often impose significant obstacles for potential entrepreneurs. China's business registration system originated from its era of planned economy. In 2011, the country was regarded as one of the world's most heavily regulated markets, ranking the 150th among 183 regions and countries for the relative ease of starting a business.[5] A 2012 news article highlighted the case of a Chinese woman who struggled for over a year to start her business as a result of burdensome paperwork, lack of bureaucratic coordination, and administrative red tape.[6] The widespread public frustration with the arduous firm registration process drew attention of party leaders, leading to the initiation of an entry deregulation pilot program in Guangdong Province in 2012 and subsequent nationwide reform in 2014.[7]

Both the scope and the intensity of the 2012-2014 entry deregulation were unprecedented. The reform streamlined the entry registration process and lowered entry barriers in five major aspects. First, the regulations on **registered capital** were largely lifted.[8] Prior to the reform, there were mandated minimum capital requirements for different types of company registration. For example, the required minimum registered capital for a single-shareholder limited liability company was ¥100,000 (around $15,000), nearly twice Guangdong's GDP per capita in 2012.[9] In addition, the

[5] According to the World Bank's 2011 *Doing Business Report*, starting a business in China required 14 procedures, 38 days, and 3.5% of per-capita income on average. See http://www.doingbusiness.org/data/exploreeconomies/china/.

[6] See http://news.sina.com.cn/c/2012-03-28/082924186244.shtml.

[7] See http://inews.ifeng.com/50489255/news.shtml?&back.

[8] Registered capital is "the total amount of capital injected in full by the investors into the company and registered with the Chinese authorities." See https://www.ptl-group.com/guides/calculating-the-registered-capital.

[9] Minimum capital requirements varied by firm types: ¥300,000 (around $46,000) for limited liability companies

"capital paid-in system" required firms to *deposit* the registered capital in a state-owned bank for the entirety of the verification process, which could last for several months, before firms were legally allowed to register. The entry reform replaced the "capital paid-in system" with a "capital subscription system," where shareholders have sole discretion in determining the amount of registered capital, and verification by an accounting firm is no longer mandatory. This effectively reduced the cost of registered capital to nearly zero.[10]

Second, the requirements for **pre-registration certificates** were significantly relaxed. Previously, business owners had to secure numerous certificates before they could obtain a business license. While the exact number varied across sectors, the total added up to 121 certificates. The reform only mandated 13 certificates for the initial business license, and the remaining 108 certificates could be acquired post-registration.[11]

Third, the reform replaced the cumbersome annual inspection system with a streamlined **annual report** system. Annual inspections were time-consuming and financially burdensome, especially for small businesses.[12] Following the reform, the government established an online platform that allowed firms to file annual reports electronically at significantly reduced costs. These reports are published in the National Enterprise Credit Publicity System (NECPS) and are accessible to the public. To encourage compliance, the State Administration for Industry and Commerce (SAIC) randomly selects some firms for onsite inspections and publishes the names of firms that fail to obtain the necessary permits within a grace period or submit false information in their annual reports. The consequences of these violations are severe. Firms with violation records are excluded from government procurement and subsidies, experience credit downgrades, and face significantly higher obstacles when seeking financial loans. Moreover, the legal representatives of these firms

with two or more shareholders, ¥100,000 (around $15,000) for single-shareholder limited liability companies, and ¥5 million (around $770,000) for public limited companies. In contrast, many developed countries, including the U.S., do not impose any minimum capital requirements.

[10]For more information about the subscription system, see https://www.jonesday.com/en/insights/2014/05/recent-changes-in-the-registered-capital-system-in-china.

[11]A detailed list of these certificates can be accessed at http://www.gd.gov.cn/gkmlpt/content/0/142/post_142958.html#7.

[12]Firms were required to fill out inspections in paper forms, provide financial statements, pay inspection fees, and physically deliver documents to government offices. See https://baike.baidu.com/item/%E4%BC%81%E4%B8%9A%E5%B9%B4%E6%A3%80.

are prohibited from registering new firms, traveling abroad, and applying for mortgages.[13]

Finally, the reform eliminated the stringent requirements for a **business address** and enhanced **administrative services** for businesses. Previously, obtaining a business address required verifying numerous documents, including property ownership or lease contracts. Additionally, firm owners had to visit multiple agencies in person for each certificate and procedure requiring government approval. After the reform, firms can register with any type of address, and multiple firms can share the same address. Instead of making onsite visits to various agencies, businesses can complete the entire registration process through a one-stop online portal.

In summary, the entry reform includes not only policies that reduce firms' registration costs but also a robust system for post-entry supervision and economic monitoring through NECPS. As a result of this reform, China's ranking in the World Bank's "Starting a Business" index in the *Doing Business Report* leaped from the 150$^{\text{th}}$ place in 2011 to the 27$^{\text{th}}$ in 2020. Starting a business in Shanghai in 2020 only required 4 procedures, 9 days, and 1.4% of per-capita income on average.[14] The entry barriers faced by a potential entrepreneur are lower than an average OECD country.

2.2 The Pilot Program in Guangdong

The Business Registration Reform started with a staggered pilot in Guangdong province. It was initiated in 2012 in Dongguan and Foshan and sequentially adopted by ten other cities in 2013 and early 2014. In March 2014, the reform expanded to include the remaining nine cities in Guangdong and other provinces of China. The rollout timing of the pilot is summarized in Figure A1. There is no apparent relationship between city characteristics and the rollout orders (Figure A2), and the rollout order appeared to be determined by random factors other than economic considerations. For example, the biggest city, Shenzhen, started the pilot in early 2013, while the second-largest and capital city, Guangzhou, did not implement the reform until right before the national expansion.

We use two sources to determine the policy implementation date: the date stated in official government documents and the date reported in local news media. These two dates are the same

[13] See https://www.sohu.com/a/156012919_269228.

[14] See https://archive.doingbusiness.org/en/data/exploreeconomies/china.

in all cases where a local media report is available. It typically took several months between the announcement and implementation of a pilot program, and we examine the robustness of our results to the anticipation effect in Sections 4.[15]

There have been other policy changes in Guangdong in recent years. They include the establishment of Financial Reform Pilot Zones and Guangdong Pilot Free Trade Zone, the development of the Guangdong-Hong Kong-Macao Greater Bay Area, the Value-added Tax Reform, as well as the Rural Revitalization Planning. We summarize the timing as well as the goals of these reforms in Table A1. Except for the Financial Reform Pilot Zones that were created in 2012 in the Pearl River Delta area, all other policies took place after our sample period and thus should not confound our empirical results. To capture the effect of the financial reform and other city-year macro shocks, we incorporate a rich set of spatial and temporal controls, such as city-year fixed effects, industry-year fixed effects, city-industry fixed effects, etc.

2.3 Data

Our main data sets come from three sources: the Business Registry Database from January 2009 to December 2016, the Firm Annual Report Database from 2008 to 2016, and the 2018 Enterprise Survey for Innovation and Entrepreneurship in China (ESIEC). Appendix A discusses auxiliary data sets.

The Business Registry Database is maintained by SAIC. It contains information related to the registration process, such as a firm's initial registry date, location of the business, the relevant industry code, the amount of registered capital, the ownership type, the education, age, and gender of the entrepreneur, and the firms' complete shareholding structures. If applicable, the database also contains the date when a license is revoked or canceled. These registration records enable us to directly measure firm entry by city, industry, and month and construct firm attributes. Our second data set, the Firm Annual Report Database, is also maintained by SAIC. This data set includes

[15] For example, Shenzhen announced the pilot program four months before the implementation (http://www.sz.gov.cn/qykbfwzt/dj/zcfg/201302/t20130225_2109647.htm), and Zhuhai announced the pilot program three months before the implementation (http://ssgs.zhuhai.gov.cn/ssdjtl/201302/t20130226_363792.html).

firms' balance sheet information at the annual level, such as the gross capital, employment, sales, and taxes paid.[16]

We extract information from the Business Registry Database and the Firm Annual Report Database for all firms registered in the Guangdong province, covering all 21 cities. These two data sets offer several key advantages, making them particularly useful for our purposes. First, the coverage is universal. Compared with other widely used Chinese firm data sets, such as the Annual Survey of Industrial Firms and the National Tax Survey Database, our data includes all firms regardless of size. This feature is particularly valuable because many entrants are small (and hence often omitted in other data sets). Second, our data cover several years before and after the reform, enabling us to exploit the staggered rollout of the reform. The sample period is long enough to examine the parallel trend assumption. Third, the Firm Annual Report Database covers key balance-sheet variables that allow us to analyze the evolution of firm size and productivity. In contrast, many previous studies have focused solely on the extensive margins (entry and exit) due to data limitations.

Our third data set comes from the 2018 Enterprise Survey for Innovation and Entrepreneurship (ESIEC), which was conducted by the Center for Enterprise Research at Peking University.[17] The survey was designed to be representative of private firms registered between 2010 and 2017 in six provinces (Guangdong, Gansu, Henan, Liaoning, Shanghai, and Zhejiang). ESIEC firms' average employment size and revenues at the one-digit-industry level (as well as the empirical distributions) closely resemble those of the 2018 China Economic Census. The questionnaires include hundreds of questions on the registration procedure, legal representatives' backgrounds and past entrepreneurial experiences, and sources of start-up capital. We use the ESIEC survey to measure the cost of setting up a business, the extent of deregulation across industries, and entrants' financial constraints, etc.

[16] The database also contains information on firm profit, but this variable likely suffers from under-reporting. For example, 2.6% of observations have missing values, 17.3% of observations have exactly zero profit, and 20% of observations have non-zero profits whose absolute value is below ¥10,000 ($1,540). We only use the reported profit for supporting evidence. The annual reports are mostly missing for firms in Zhuhai after the reform. We only include firms registered in Zhuhai for entry regressions and exclude them for the analyses on firm exit, size, and productivity.

[17] See https://www.cer.pku.edu.cn/ for more information.

We note a few caveats. First, a lower fraction of firms filed their annual reports in 2013 during the transition from the inspection system to the annual report system. However, the data missing patterns across industries or ownership types in 2013 are similar to those in 2009 and do not seem to pose identification threats. Second, firms' employment is unavailable until 2013, preventing us from using Olley and Pakes (1996) or Levinsohn and Petrin (2003) to estimate firm productivity prior to 2013. We follow Aw et al. (2011) to estimate productivity without using employment and report productivity estimates via Olley and Pakes (1996) and Levinsohn and Petrin (2003) for the period when employment information is available in Section 4.4. Third, one way to identify exiting firms is through license cancellation or revocations. However, license revocations were much fewer in 2014 and largely ground to a halt in 2015. We implement two solution. In the first solution, we define exiting firms as those that stop submitting annual reports. This approach is analogous to the common practice in the literature (Dunne et al., 1988) that defines exiting firms as those that disappear from consecutive censuses (which are equivalent to annual reports in our setting). Given the mandatory requirement for filing annual reports and the severe penalties for non-compliance (license revocation), firms that failed to file annual reports over consecutive years have likely shut down. Using this measure, the aggregate exit rate varies between 5% and 10% over time, is similar before the reform, and slightly higher after the reform. Second, the SAIC rectified missing revocations in 2017 and 2018. We extend the sample to 2018 and repeat the exit analyses using firm exit based on rectified license cancellations and revocations. These two approaches deliver qualitatively similar results (Section 4.4).

2.4 Descriptive Evidence

From 2008 to 2012, the number of new firms in Guangdong grew at an annual rate of 16.5%, and the overall entry rate averaged 14.7%.[18] In 2011, the year before the reform, Guangdong had 250,000 new firms in all sectors, representing about 12% of the national total. In the wake of the reform, the number of new entrants sharply increased, as shown in Panel (a) of Figure 1. The number of

[18] The growth rate in the number of new firms is defined as $(\text{Entry}_t - \text{Entry}_{t-1})/\text{Entry}_{t-1}$. The entry rate is defined as the ratio of new entrants to lagged number of incumbents, i.e., $\text{Entry Rate}_t = \text{Entry}_t/\text{Incumbent}_{t-1}$.

new entrants in 2014 nearly doubled that in 2012, and 2016 nearly tripled that in 2012. The annual growth rate in the number of new entrants was 34.4% from 2012 to 2016, almost doubling that pre-reform.

Panel (b) of Figure 1 displays the monthly trend for the nine largest cities in Guangdong. Most cities experienced significant upward shifts in the number of new entrants *immediately* after the reform. Foshan and Dongguan experienced some delay and entry increased sharply in March 2014 when the reform was expanded nationwide. This is likely due to the fact that they are the first pilot cities and experienced many trials and errors in the policy-experimentation phase. The only city that did not experience a jump in new firm entries is Zhongshan. This is partly because Zhongshan experienced a significant *increasing* trend in the number of entrant prior to the reform, which made the effect of the reform less pronounced.[19] Results are robust when Zhongshan is excluded from our analysis.

3 Empirical Framework

We begin by analyzing firm entry, the direct policy target of the reform. The analysis leverages the *monthly* variation in implementation time across cities and employs a DID strategy. We then examine firm exit, size, and productivity. Since these outcomes are updated annually, we exploit the variation in implementation time across years. Whenever feasible, we also evaluate the heterogeneous impacts of deregulation across industries and firm types using a triple difference design. These analyses serve dual purposes: demonstrating which subsectors and subsets of firms are most affected by the entry deregulation and providing identification support (because the triple difference design is valid under weaker assumptions than DID). We focus on manufacturing firms in Guangdong province due to the lack of reliable productivity estimates for non-manufacturing firms and present supporting evidence for the retail, wholesale, and service sectors.

[19] See http://www.zsdag.cn/view?id=8845 for local policy experimentation in Zhongshan in 2012.

3.1 Empirical Specification

We take advantage of the staggered rollout of the reform and adopt the DID strategy to identify the effects of entry deregulation on the number of new firms:

$$Y_{cit} = \beta D_{ct} + \xi_{ci} + \xi_{cy(t)} + \xi_t + \epsilon_{cit}, \tag{1}$$

where the dependent variable Y_{cit} is the log number of new entrants in city c, industry i, and month t. The key explanatory variable, D_{ct}, is binary and takes value one if city c has implemented the reform at time t and zero otherwise. Its coefficient β is the key parameter of interest that measures the effect of the reform. City-by-industry fixed effects ξ_{ci} control for time-invariant differences across cities and industries, while city-year fixed effects $\xi_{cy(t)}$ and month-of-sample fixed effects ξ_t absorb city-specific local shocks and aggregate macro shocks. All regressions control for these fixed effects, with some specifications including additional temporal and local fixed effects. We use data from all 21 cities in Guangdong from January 2009 to December 2016 to estimate the effect on firm entry.

Identification Threat and Solutions The key identification threat is the potentially endogenous timing of the reform. For example, had the government determined the order of the rollout according to certain economic conditions, such as entrepreneurial activities, this would have made the treatment variable endogenous (Wang and Yang, 2021). To address this concern, we control for a variety of temporal fixed effects. All specifications control for city-by-year fixed effects $\zeta_{cy(t)}$ to account for city-level macroeconomic shocks, in addition to city-by-industry fixed effects and month-of-sample fixed effects. By comparing differences in firm entry in the months before and after the program implementation within a year, our identification relies on the weaker assumption that the exact month, rather than the year, of the program implementation is exogenous. This condition is plausible given that there is no apparent relationship between city characteristics and the rollout orders, as shown in Figure A2. In more demanding specifications, we add city-by-calendar-month fixed effects to control for seasonality in firm entry across cities. We further include industry-by-month-of-sample fixed effects to control for industry-wide macro shocks. Our key identification assumption is that conditional on these controls, the reform's timing is exogenous to the unobserved factors ϵ_{cit}. These entry regressions are weighted by the number of firms in each city and industry pair at the beginning of the sample period so that the parameter estimates capture the average effect across cities and industries.

To bolster the confidence that our analysis uncovers the causal impact, we conduct two more empirical exercises. First, we use an event study to validate the parallel-trend assumption and examine the dynamic effects of the reform:

$$Y_{cit} = \sum_{\substack{q=-12 \\ q\neq -1}}^{15} \beta_q D_{ct}^q + \xi_{ci} + \xi_{cy(t)} + \xi_t + \epsilon_{cit}, \tag{2}$$

where the dummy variable D_{ct} in Equation (1) is replaced with monthly event dummies D_{ct}^q during the window of 12 months before and 15 months after the reform in city c. For example, D_{ct}^{-1} equals one if t is one month prior to the reform. Periods earlier than 12 months before the reform and later than 15 months after the reform are grouped with D_{ct}^{-12} and D_{ct}^{15}. The month before the reform is the baseline group. The event study coefficients β_p's capture the pre-trend (if any) and dynamic treatment effects.

Second, we adopt a triple difference strategy to investigate the differential impacts across industries that experienced varying degrees of deregulation. We compare firms in industries that experienced a larger reduction in entry cost with those in industries that are exposed to a smaller reduction in entry cost within each city and year. This strategy exploits within-city-year and across-industry variation, thus helping alleviate concerns that the timing of the reform might be endogenous.

3.2 Other Outcomes of Interest

Firm Exit The analysis on firm exit is similar to entry, except that we use firm-year level observations (as firm exit is reported annually instead of monthly):

$$Y_{jt} = \beta D_{ct} + \mathbf{Z}_{jt} + \xi_{ci} + \xi_t + \epsilon_{jt}, \tag{3}$$

where the dependent variable Y_{jt} takes value one if firm j exits in year $t+1$ and zero otherwise. Firm-level regressions allow us to flexibly control for firm attributes $\mathbf{Z}_{jt}$ that also affect exit decisions. We exclude the last year of the sample (year 2016) due to right-censoring. The baseline specifications exploit cross-industry variations to identify the causal impacts on firm exit. We also test empirically whether industries with higher degrees of deregulation experience larger exit rates, conditional on a rich set of controls.

Firm Size and Productivity Our sample consists of three types of firms: entrants, incumbents, and exiting firms. Entry deregulation likely affects these firms differently, stimulating entry while potentially increasing exit due to heightened competition. A simple difference-in-difference comparison of firm size and productivity would mix these heterogeneous effects on different firm types. To separately identify the differential impacts of entry deregulation on entrants and exiting firms, we adopt a triple-difference design that uses incumbent firms as a comparison group. Using incumbents as a control group also implicitly absorbs unobserved macroeconomic confounders. The specification we run is as follows:

$$y_{jt} = \beta_1 E_{jt} + \beta_2 \mathcal{X}_{jt} + \beta_3 D_{ct} \times E_{jt} + \beta_4 D_{ct} \times \mathcal{X}_{jt} + \mathbf{Z}_{jt} + \xi_{ci} + \xi_{ct} + \xi_{it} + \epsilon_{jt}, \tag{4}$$

where y_{jt} is the size or productivity of firm j in year t. The binary variable E_{jt} takes value one if firm j is of age one at time t, and $\mathcal{X}_{jt}$ takes value one if firm j exits in year $t+1$. We separately interact E_{jt} and $\mathcal{X}_{jt}$ with the reform treatment dummy D_{ct}, where c denotes a city. All regressions include firm attributes $\mathbf{Z}_{jt}$ (such as fixed effects for firm ownership type and their interaction with time dummies) and a rich set of spatial and temporal controls (such as city-industry fixed effects ξ_{ci}, city-year fixed effects ξ_{ct}, and industry-year fixed effects ξ_{it}). Coefficients β_1 and β_2 capture the average differences in the outcome variable (firm size or productivity) between entrants and incumbents and between exiting firms and incumbents before the reform. Coefficients β_3 and β_4 capture how these differences *change* after the reform. The key identification assumption is that the size and productivity trends for entrants relative to incumbents and for exiting firms relative to incumbents would have been parallel in treated and comparison units had the reform not taken place.

It is worth emphasizing that in the analyses of firm size and productivity, entrants are defined as firms in their first calendar year of operation immediately following their registration year. This allows us to observe the entrant firm's revenue for an entire year. This definition, combined with the triple difference design in Equation (4), enables us to examine the policy impacts across different cohorts while controlling for age effects, which are correlated with firm revenue and productivity. Essentially, we compare the productivity gap between one-year-old entrants and incumbents for cohorts that entered before the reform and those that entered after the reform.

4 Empirical Results

We have conducted a large number of analyses on entry, exit, firm size, and productivity. We present results for the baseline specifications in Section 4.1 to Section 4.3, provide a high-level summary of the robustness analyses in Section 4.4, and relegate all details on robustness checks to Appendix B.

4.1 Firm Entry

Table 1 reports the regression results on the number of newly registered firms across different manufacturing industries, based on Equation (1). The dependent variable in Columns (1)-(3) is the log number of new firms at the city-industry-month level. Column (1) controls for city-by-year fixed effects to address concerns over potentially endogenous reform timing and unobserved macroeconomic shocks at the city level, in addition to city-by-industry fixed effects and month-of-sample fixed effects. The reform increases firm entry by about 21.1%. Columns (2) and (3) further add city-by-calendar-month fixed effects to control for seasonality across cities and industry-by-month-of-sample fixed effects to control for industry-specific time trends. The estimates are similar across columns. In our preferred specification with the richest set of controls in Column (3), entry deregulation leads to a 24.5% increase in new entrants. Columns (4)-(6) use log entry rate as the dependent variable. Results are similar to those in Columns (1)-(3) as the variation in the number of incumbents (the denominator of entry rate) is largely absorbed by the city-by-year fixed effects.

The average annual growth rate of entrants between the pre-reform period and post-reform period, measured by $\frac{\text{average annual number of entrants in 2013-2016}}{\text{average annual number of entrants in 2009-2012}} - 1$, is 51.5%. Our preferred estimate suggests that entry deregulation accounts for 48% of the growth ($\frac{0.245}{0.515}$). This effect size is larger than the impact of entry deregulation documented in other countries, such as 5% in Mexico (Kaplan et al., 2011) and 17% in Portugal (Branstetter et al., 2013). The differences are likely due to China's more comprehensive reform measures that greatly simplified the registration procedure and reduced financial barriers, rapid implementation and rigorous enforcement (Wei and Sanchez Ortega, 2022). In contrast, reforms in Mexico and Portugal primarily focused on simplifying registration procedures. Moreover, the reform in Mexico faced significant local resistance, which negatively affected its implementation.

A key underlying assumption for the DID strategy is the parallel trend between the treatment and control groups. Panel (a) of Figure 2 plots the event study coefficients for firm entry. The coefficients prior to the reform are all small and insignificant and hover closely around zero, confirming the parallel trend assumption. The entry rate increases sharply after the reform, and the post-reform estimates are jointly significant. The elevated entry rate persists till the end of our sample period, indicating a lasting effect. One might be concerned that the reform was announced a few months prior to the implementation, and firms could postpone the registration process until after the reform. The effect size is smaller but remains in line with the baseline specification when excluding the four months before the policy implementation, as shown in Table A9.

Effect Heterogeneity

We focus on two types of heterogeneity: ownership type and reform-induced changes in (monetary) registration costs. We create the dummy variable $Private_i$ to flag firms that are registered as privately owned. The other ownership types include state-owned enterprises (SOEs) and foreign, Hong Kong, Macau, and Taiwan-invested enterprises (FIEs). The registration red tape prior to the reform was unlikely to be a major constraint for SOEs and FIEs, which had better access to the capital market. The 2018 ESIEC survey enables us to calculate both the average self-reported registration cost at the two-digit-industry level and its percentage change before and after the reform. We define a dummy variable $RegCost_i$ that is equal to one if the percentage change in industry i is above the median over all industries and zero otherwise.

The results for the triple DID are shown in Table 2. The outcome variables are the monthly number of entrants (Columns (1) and (2)) and entry rates (Columns (3) and (4)) at the city, industry, month, and ownership-type level.[20] Following the reform, the percentage increase in new registrations by private enterprises is 15 percentage points higher than that of non-private enterprises, consistent with literature findings that private firms face more financial constraints (Long and Zhang, 2011). Furthermore, industries that experienced a larger reduction in registration costs saw a 7.4 percentage point higher increase in newly registered firms compared to industries with a smaller reduction in costs. This finding is intuitive and lends additional credibility to our results.

4.2 Firm Exit

Entry deregulation intensifies market competition, which in turn could drive the marginal incumbents out of business and increase exit rates. In this section, we empirically investigate this general equilibrium effect. We define exiting firms as those that stop filing annual reports and examine alternative definitions in Section 4.4.

Table 3 reports the regression results. The unit of observation is a firm-year, which allows us to flexibly control for firm attributes. Column (1) controls for city-industry-ownership-type interacted fixed effects, firm-age fixed effects, and year fixed effects. The coefficient estimate of the treatment variable is 0.498 percentage points. Given that the average exit rate was 5.7 percentage points prior to the reform, our estimate suggests an 8.7% increase ($= \frac{0.498}{5.7}$) in the average exit rate post-reform as a result of more intense competition. The increase in both the entry and exit rates leads to higher

[20] The specification in Table 2 is similar to our preferred specification of Column (3) in Table 1, except that we control for city-industry-ownership type and industry-ownership type-month of sample fixed effects instead of city-industry and industry-month of sample fixed effects. All columns use the city-industry-month-ownership type level observations to ensure a comparable number of observations. Results for the triple DID regressions using $RegCost$ are similar if we use city-industry-month level observations instead.

market turnover rates and shorter firm lifespans. However, the effect on firm entry dominates that on firm exit, leading to a net increase in the number of firms in the market. Column (2) further controls for firm size that is measured by log revenue. The coefficient of log revenue is negative and both statistically and economically significant, consistent with the stylized facts of industrial dynamics (Dunne et al., 1988) that larger firms are less likely to exit the market. After accounting for firm size, the effect of the entry reform drops slightly to 0.469 percentage points.

We conduct an event study on firm exit analogous to firm entry and report the results in Panel (b) of Figure 2. There is no visible trend prior to the reform. The exit rate ticked up during the year when the reform was implemented and rose significantly higher one year afterward. This is what one would expect, as intensified market competition from new entrants leads to a higher exit rate.

Column (3) of Table 3 implements the triple-difference design by interacting $RegCost_i$ with $Treatment_{ct}$ and controlling for city-by-year fixed effects (which absorb the treatment dummy.) The triple DID strategy helps alleviate the concern of endogenous timing and macroeconomic confounders. The coefficient for the interaction term is positive and 3.7 times as large as the baseline estimate of 0.469 in Column (2), indicating that industries with greater reductions in registration costs experienced much higher exit rates after the reform.[21]

One might be concerned that the higher exit rate is driven by some new entrants that could not survive the intense competition post the reform. To address this concern, Columns (4)-(6) replicate the analysis but exclude all establishments that entered after the reform. Results remain robust: the coefficient estimate is 0.544 percentage points in Column (5) compared to 0.469 percentage points in Column (2). Altogether, these findings suggest that entry deregulation not only induces more entrants but also leads to more firm exit.

4.3 Firm Size and Productivity

Entry deregulation allows smaller firms to enter the market, thereby mechanically reducing the average size of entrants. At the same time, increased competition from deregulation raises the

[21]Results from the triple-DID that interacts $Private_i$ with $Treatment_{ct}$ are mixed, as more intense competition from increased entry appears to affect all ownership types, not just private firms.

survival threshold for incumbents, potentially increasing the average size of exiting firms. The effect of entry deregulation on productivity is more complex, operating through opposing channels. We discuss how deregulation influences firm size and productivity in this section and explore the underlying channels for productivity changes in Section 5.

Firm Size Panel (a) of Figure 3 displays the distribution of log revenue, a measure of firm size, for *entrants* before and after the reform. As evident from the figure, the size distribution shifted to the left after entry deregulation. The average annual revenue of entrants decreased from ¥3.64 million before the reform to ¥2.98 million after the reform, an 18.1% drop. Similarly, the average capital levels shrank by 20.2%.

Table 4 reports results from Equation (4) on firm size (log revenue) in Columns (1)-(2).[22] Column (1) includes city-by-industry, city-by-year, and industry-by-year fixed effects. Column (2) further interacts city-by-industry fixed effects and industry-by-year fixed effects with firm ownership types, allowing for heterogeneous levels across ownership types and controlling for differential time trends across industries and ownership types. Coefficient estimates are qualitatively similar across these Columns. Before the reform, entrants and exiting firms were smaller than continuing incumbents. Although they remain smaller post-reform, the size gap between incumbents and entrants widened by 20.5% ($=\frac{0.191}{0.933}$), while the size gap between incumbents and exiting firms narrowed by 27.9% ($=\frac{0.191}{0.684}$), based on the estimates in Column (2). Entry deregulation reduced the size of entrants but raised the survival threshold for exiting firms.

Panels (a) and (b) of Figure 4 illustrate the deregulation's dynamic effect on firm size through an event study. There are no significant pre-trends for either entrants or exiting firms, which justifies our empirical strategy.[23] The effects of the entry deregulation on the size of entrants and exiting firms become noticeable post the reform and strengthen over time, though in opposite directions.

Firm Productivity We estimate firm productivity using a static version of Aw et al. (2011) and

[22] We only have employment data after 2013. Using employment to measure firm size leads to similar findings.

[23] There is a modest, though insignificant, downward trend in entrant size prior to the reform. This is primarily driven by under-reporting among small firms in the early years of the sample. For example, the reported revenues of entrants were 8.04% lower in 2011 compared to that in 2009.

Peters et al. (2017). A detailed description of the identification and estimation of this baseline productivity measure is provided in Appendix C.1. Since this estimate is log-valued, we label it as log(productivity) in figures and tables. We discuss alternative productivity measures in Section 4.4 and Appendix C.2.

Panel (b) of Figure 3 illustrates differences in new entrants' productivity before and after the reform, showing a rightward shift in the productivity distribution post-reform that entails a 0.9% increase in new firms' average productivity. Similar patterns hold with alternative measures of firm productivity, such as the revenue-over-capital ratio.

Columns (3) and (4) of Table 4 report the effect of entry reform on productivity using the same empirical specification as the firm-size analysis (Equation (4)), where the dependent variable is the baseline productivity estimate. Before the reform, entrants and exiting firms had lower productivity compared to incumbents. After deregulation, both entrants and exiting firms became more productive. According to our preferred specification in Column (4), entrants exhibited a 1.1% increase in their average productivity, while exiting firms saw a 2.6% rise. This implies that the reform narrowed the productivity gap between entrants and incumbents by 20.0% ($= \frac{0.011}{0.055}$) and the gap between exiting firms and incumbents by 44.8% ($= \frac{0.026}{0.058}$). The event studies in Panels (c) and (d) of Figure 4 echo the regression results. While there is no pre-trend for either entrants or exiting firms, their productivity exhibits significant improvement after the deregulation, with the effect strengthening over time.[24]

Our finding that the entry reform increased entrants' productivity by 1.1% is consistent with other studies on China's recent large-scale policy interventions. For example, Yu (2015) estimates that a one percentage point decrease in output (input) tariffs would lead to a productivity gain of 0.92% (0.51)% in China's manufacturing sector in 2000-2006. Chen et al. (2021) find that increasing the real R&D by 10% would improve firm productivity by 0.9% in China's manufacturing sector in 2008-2011. Liu and Mao (2019) show that China's recent sweeping Value-Added Tax

[24] The pattern that the productivity gains of new entrants appear to lag behind those of exiting firms by one period in Figure 4 is primarily due to the absence of many firms' annual reports in 2013, coinciding with the government's transition from the inspection system to the annual report system. If firms with missing data in 2013 are excluded from the analysis, both entrants and exiting firms show a (statistically insignificant) productivity increase in period 0.

(VAT) reform, which allowed firms to deduct fixed asset purchases from the VAT base, improved firm TFP by 8.9%.

Table A2 explores the heterogeneity of the reform's effect on entrants' productivity by registration costs (Columns (1) and (2)) and ownership types (Columns (3) and (4)). Entrants in industries with high percentage-reductions in registration costs experienced greater productivity gains after the reform, as did privately-owned firms. These findings indicate that financially disadvantaged entrepreneurs and firms – especially those in industries with more red tape or those that are privately owned – contribute most to the reform's efficiency gains.

4.4 Robustness Analyses

Staggered DID Recent literature has pointed out that two-way fixed effects models could yield biased estimates when there are heterogeneous treatment effects in the staggered DID setting (de Chaisemartin and d'Haultfoeuille, 2020, 2024; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Borusyak et al., 2024). We follow the aforementioned literature and replicate our results with frontier estimation methods. Results on all firm outcomes – entry, exit, size, and productivity – are robust to the corrections that allow for heterogeneous treatment effects in staggered DID. See Appendix B.1 for more details.

Firm Entry The high entry rates after the reform could be driven by a proliferation of shell companies, which exist only on paper but conduct no real business activities.[25] We exploit the fact that shell companies are much less likely to submit annual reports and often report zero financial information, including capital stock. Panel A of Table A3 includes only newly registered firms that have ever filed annual reports during our sample period. Panel B further restricts the sample to firms that have reported positive capital stock. Estimates of the effect on firm entry in Table A3 are similar to, and slightly higher than, those reported in Table 1. Although the number of shell companies may have increased post the reform, their share among entrants in the manufacturing

[25] See for example http://amr.sz.gov.cn/xxgk/qt/ztlm/scjgzy/mtbd/content/post_7821580.html. According to https://www.law.cornell.edu/cfr/text/17/240.12b-2, shell companies have no nominal operations. They are often registered to achieve anonymity, "serving as a vehicle for business transactions," or for tax evasion or tax avoidance.

sector appeared stable over the sample period. While the cost of registering a shell company has declined, so has the cost of registering a real firm. Additionally, the manufacturing sectors often require a physical presence at a registered location, making shell companies less of a concern in our context.

We conduct a series of other robustness checks regarding the sample selection, anticipation effect, and geographical spillovers. We further examine whether new entrants are *de novo*. While not the focus of this paper (as we cannot reliably measure productivity in non-manufacturing sectors), the reform also significantly increased firm entry in retail, wholesale, and service sectors, with even greater impacts than that in the manufacturing sector. See Appendix B.2 for more details.

Firm Exit and Size Table A4 replicates the exit analysis using firms whose licenses were revoked or canceled. The coefficient estimates are slightly larger than but qualitatively similar to those in Table 3, where exiting firms are defined as those that stop submitting annual reports. Additionally, results using log capital as the firm size measure are consistent with those using those obtained using log revenue, as shown in Table A5.

Firm Productivity We have constructed several alternative measures of firm productivity. First, we replicate the analysis using productivity estimates following Olley and Pakes (1996) (OP) and Levinsohn and Petrin (2003) (LP). One limitation of the OP/LP estimates is that we only observe employment data since 2013, resulting in a significant loss of observations. Nonetheless, the OP/LP productivity estimates are highly correlated with our baseline productivity measures for periods with employment data, and using OP/LP in Equation (4) delivers similar patterns for the productivity gains of entrants and exiting firms post-reform, as shown in Table A6. Second, we follow Brandt et al. (2021) to estimate firm productivity relative to all firms within a province-industry-year cell, assuming that firms within a cell face the same wage rate. The empirical results remain robust, as shown in Table A7.

Third, using the log of revenue-over-capital ratio as a data-driven measure of productivity yields results similar to those obtained with the baseline productivity estimate (Table A5). Compared to firms that entered before the reform, the revenue-over-capital ratios are 21% to 24% higher for post-reform entrants.[26] Moreover, the productivity gains, proxied by increases in the revenue-over-capital ratio, are stable over time, extend beyond the manufacturing sector, and apply to many segments of the aggregate economy. However, there is no noticeable effect on the productivity of incumbent firms. Appendix B.3 presents more details.

5 Channels of Productivity Gains and Aggregate Implications

Entry deregulation affects productivity through multiple channels, which we illustrate with a simple model in Appendix D. First, more intense competition boosts entrant productivity since only efficient firms find it profitable to enter (the *market-competition effect*). Second, reduced entry barriers may change the entrepreneur composition, allowing the financially constrained but productive entrepreneurs to enter the market (the *composition* effect). Conversely, entry deregulation eliminates preexisting size restrictions and thus permits less productive firms to enter (the *weaker-screening* effect). Results in Section 4.3 suggest that the *market-competition* effect and the *composition* effect collectively dominate the *weaker-screening* effect, leading to higher average productivity for new entrants. This section examines various underlying channels. We do not focus on the channels that led to productivity changes in exiting firms, which are relatively straightforward and driven by more intense market competition forcing out marginal firms. We conclude this section by quantifying the aggregate efficiency implications of entry deregulation.

We begin by examining productivity changes across the firm size distribution. Figure 5 plots productivity against firm size as measured by capital stock for entrants before and after the reform. Post the reform, productivity was higher among entrants with capital stock less than ¥540k (which falls between the required minimum registered capital for limited liability companies and public limited companies under the old system), but the comparison reversed for entrants with higher capital stock. Furthermore, the productivity improvement was more pronounced for firms with less capital, a pattern that we revisit below. These findings suggest that almost all productivity

[26]The 1.1% productivity gain and the 21-24% increase in revenue-over-capital are consistent: based on production parameter estimates, changes in productivity are on the order of 5-10% of changes in the revenue-over-capital ratio.

gains resulting from the entry deregulation can be attributed to small entrants, which is perhaps intuitive as the entry barriers are more constraining for small firms.

5.1 The *Market-competition Effect*

More intense market competition deter inefficient firms from entering. To isolate the market-competition effect from other reform-induced channels, we drop firms that entered *after* the reform and only keep those that entered immediately *before* the reform together with incumbents, which serve as the reference group. Firms that entered immediately *before* the reform were not directly affected by the entry deregulation and hence not subject to the screening effect or composition effect. In other words, changes in these firms' productivity should be attributed to the *market-competition effect* only.[27] We repeat the analyses of Equation (4) but exclude establishments registered after the reform.

The empirical results are shown in Table 5, controlling for the same set of fixed effects as in Columns (2) and (4) of Table 4. Column (1) examines firm size. Consistent with Table 4, entrants are smaller than incumbents. However, the coefficient estimate on treatment-entrant interaction is close to zero and insignificant. This is not surprising since firms that entered just before the reform were subject to the same set of rules (such as the minimum capital requirements) as earlier entrants. Column (2) examines firm productivity. While entrants are less efficient than incumbents in general, the treatment-entrant interaction is positive and significant. In other words, firms that entered immediately before the reform and operated in a more competitive environment reported significant productivity gains relative to earlier entrant cohorts, which were not subject to more intense competition upon their entry. Although the coefficient (0.007 in Column 2 of Table 5) is only 64% of the baseline estimate (0.011 in Column (4) of Table 4), it nevertheless suggests that market competition is an important channel through which entry deregulation improves entrants' productivity. Finally, since there is a few months' gap between policy announcements and imple-

[27]For example, Shenzhen implemented the reform in March 2013. Consider firms that entered in January 2013 (group 1) vs. those that entered in May 2013 (group 2). Group 1's productivity and efficiency performance in 2014 (the calendar year after registration) captures the effect of market competition. In contrast, group 2's productivity in 2014 is affected by all three channels: market competition, composition changes, and weaker screening.

mentation, we repeat Table 5 but further drop entrants that registered after policy announcements and obtained similar results.

5.2 The *Composition* Effect

Entry deregulation reduces administrative entry barriers and could attract entrepreneurs who may be more productive but have less access to the capital market and fewer government connections. We provide three pieces of supporting evidence that the *composition effect* as a result of easing financial constraints is another important mechanism underlying the productivity improvements.

Source of Funds We use the 2018 ESIEC survey to examine sources of funds that entrepreneurs use when starting a new business (Panel A of Table 6). After entry deregulation, entrepreneurs are 13.7% ($= \frac{0.042}{0.306}$) less likely to borrow from friends or relatives to cover the cost of opening a new firm. They are also less likely to take loans from financial institutions, though the estimate is insignificant. Since all provinces other than Guangdong implemented the reform simultaneously, we cannot conduct a DID analysis using the ESIEC survey. However, the before-and-after comparison provides suggestive evidence that entry deregulation eased the financial constraints faced by entrants, and firms became less reliant on external resources to fund set-up costs. As small entrants were more likely to be constrained by bureaucratic entry barriers, reducing these impediments leads to higher productivity gains for them, as demonstrated in Figure 5.

Entrepreneurs' Characteristics and Shareholding Structures We next exploit the characteristics of firms' legal representatives and examine entrepreneur profiles. We follow the same empirical specification as in Equation (1) and present results in Panel B of Table 6.[28] After the reform, entrepreneurs' average years of schooling dropped by 1.9% ($= \frac{0.198}{10.557}$), and 6.3% fewer entrepreneurs have eleven or more years of schooling (64.6% of entrepreneurs have 11+ years of schooling). In the mean time, the share of entrepreneurs younger than 35 increased by 9.0% ($= \frac{0.028}{0.310}$).

We next investigate the shareholding structure of new firms. There are two major types of shareholders: individual shareholders (natural persons) and corporate shareholders (legal persons).

[28] Only one-third (one-fourth) of the sample reported age (education levels) of the legal representatives, hence the sample size in Panel B of Table 6 is much smaller than that in Panel C.

The share of companies that are held by individual shareholders increased by 2.5% ($= \frac{0.023}{0.904}$) after the reform (Panel C of Table 6). The number of firms with sole-proprietorship (i.e., 100% owned by natural persons) rose by 2%. Therefore, individual shareholders (natural persons), with fewer financial resources than corporate shareholders (legal persons), played a greater role in forming new firms after the reform. Overall, these findings in Table 6 suggest that the entry deregulation changed the composition of entrepreneurs and opened up opportunities to less-educated and younger entrepreneurs as well as individual shareholders.

Additional Evidence In Figure A3, we examine differences in productivity gains across other dimensions. First, entrants in industries with above-median shares of SOE companies exhibit higher productivity gains. Additionally, the reform had more pronounced impacts in industries with lower export revenue shares, where firms tend to be smaller compared to those in export-oriented industries. Third, firms in industries with higher registered capital requirements (proxied by the registered-capital over real-capital-stock ratio) were more likely to be financially constrained and reported larger productivity gains than those in industries with lower registered capital requirements. Finally, counties with fewer bank branches – an indicator of limited access to capital markets – experienced significantly larger productivity gains among entrants. Unlike the first three sets of comparisons where the differences in productivity gains are intuitively signed but insignificant, the differences between counties with below-median vs. above-median numbers of bank branches are both statistically and economically significant. These findings, along with results in Table A2 examining industries with high vs. low registration costs, consistently show that productivity gains among entrants are disproportionately from firms that experienced greater relaxation of financial constraints upon registration.

5.3 Macroeconomic Implications

Finally, we explore the macroeconomic implications of entry deregulation and quantify the relative contributions of entrants, exiting firms, and incumbents to the aggregate productivity growth. Following Foster et al. (2008) and Griliches and Regev (1995), we decompose the aggregate productivity as follows:

$$\Delta\Phi_t = \underbrace{\sum_{j\in C}\bar{\theta}_j\,\Delta\varphi_{jt} + \sum_{j\in C}(\overline{\varphi_j} - \overline{\Phi})\,\Delta\theta_{jt}}_{\text{Incumbents}} \;+\; \underbrace{\sum_{j\in E}\theta_{jt}\left(\varphi_{jt} - \overline{\Phi}\right)}_{\text{Entrants}} \;-\; \underbrace{\sum_{j\in X}\theta_{jt-1}\left(\varphi_{jt-1} - \overline{\Phi}\right)}_{\text{Exiting firms}}, \tag{5}$$

where φ_{jt} denotes the productivity of firm j in year t, θ_{jt} represents the revenue share of firm j in year t in each city-industry-year cell, and Φ_t is the aggregate productivity in year t weighted by the revenue share. $\overline{\text{Variable}}$ represents the average of a variable between t and $t-1$, while Δ denotes the difference between t and $t-1$. Finally, C, E, and X denote the set of incumbents, entrants, and exiting firms, respectively. The four terms in Equation (5) represent the within-firm and between-firm differences for continuing incumbents, the contribution from entrants, and the contribution from exiting firms.

We plot the entrants' contribution to the aggregate TFP change in Figure A4. Following the reform, the entrants' contribution to the overall productivity growth switched from being negative to being positive. For example, in 2013 and 2014, entrants contributed 15.1% and 23.8% to the overall productivity change, respectively. Corroborating the insights from the macro literature (e.g., Asturias et al. (2023)), entrants become a more powerful source of growth after entry deregulation. In comparison, the contribution of exiting firms is smaller and varies between 9% to 16% of entrants' contribution across years. The decomposition results are similar if we instead follow the methodologies of Baily et al. (1992) and Foster et al. (2001).

Implications for the Aggregate Economy During the 2009-2012 period, Guangdong's manufacturing sector saw an average of 42,000 new firms annually, while 16,345 firms exited the market each year. Our baseline estimates indicate that the reform led to a 24.5% increase in firm entry and an 8.7% increase in firm exit. Therefore, entry deregulation brought about 34,402 additional firms during the 2014-2016 period.[29] Based on the average revenue and employment by year and firm age, the newly registered firms, as a result of the entry deregulation, increased total employment and revenue in Guangdong's manufacturing sector by 2.5% and 1.8%, respectively, during the first

[29] The increases in firm entry and exit rates are compounded year over year when calculating the net increase in firm numbers.

three years following the reform. Since the reform has led to a permanent increase in the entry rate, these reform-induced entrants will contribute an increasing share of aggregate economic activities over time. In ten years, these reform-induced entrants are projected to generate 14.4% additional employment and 11.1% additional revenue for Guangdong's manufacturing sector. Note that these estimates are likely to be conservative, as they only account for the increase in economic activities at the extensive margin and do not incorporate efficiency gains among new firms that we reported above.

6 Conclusion

This paper evaluates how China's business registration reform affects firm dynamics and productivity. Our analysis leverages a quasi-natural experiment in which the entry deregulation reform that eased the cost and time needed to formally register a business was rolled out in different cities over time. The empirical analyses find significant impacts of entry deregulation along multiple margins. As a result of the reform, both entry and exit rates have increased. New entrants have become more productive and have made greater contribution to overall productivity growth.

We conclude with a few suggestions for future research. First, the *weaker-screening* effect from entry deregulation could lead to low-quality products and services (or shell companies), which in turn could affect the overall consumer welfare. Second, entry (de)regulations involve an important trade-off between improved market competitions and wasteful sunk entry costs as highlighted in Mankiw and Whinston (1986). Lastly, entry deregulation could affect long-run firm investment and R&D decisions due to changes in the competitive landscape. These are open questions for future research.

References

Aghion, P., Burgess, R., Redding, S. J., and Zilibotti, F. (2008). The unequal effects of liberalization: Evidence from dismantling the license raj in india. *American Economic Review*, 98(4):1397–1412.

Alfaro, L. and Chari, A. (2014). Deregulation, misallocation, and size: Evidence from india. *The Journal of Law and Economics*, 57(4):897–936.

Asturias, J., Hur, S., Kehoe, T. J., and Ruhl, K. J. (2023). Firm entry and exit and aggregate growth. *American Economic Journal: Macroeconomics*, 15(1):48–105.

Aw, B. Y., Roberts, M. J., and Xu, D. Y. (2011). R&d investment, exporting, and productivity dynamics. *American Economic Review*, 101(4):1312–44.

Baily, M. N., Hulten, C., Campbell, D., Bresnahan, T., and Caves, R. E. (1992). Productivity dynamics in manufacturing plants. *Brookings Papers on Economic Activity. Microeconomics*, 1992:187–267.

Barseghyan, L. (2008). Entry costs and cross-country differences in productivity and output. *Journal of Economic Growth*, 13(2):145–167.

Baumol, W. J. (1990). Entrepreneurship: Productive, unproductive, and destructive. *Journal of Political Economy*, 98(5, Part 1):893–921.

Bertrand, M. and Kramarz, F. (2002). Does entry regulation hinder job creation? evidence from the french retail industry. *The Quarterly Journal of Economics*, 117(4):1369–1413.

Boedo, H. J. M. and Mukoyama, T. (2012). Evaluating the effects of entry regulations and firing costs on international income differences. *Journal of Economic Growth*, 17(2):143–170.

Borusyak, K., Jaravel, X., and Spiess, J. (2024). Revisiting event-study designs: robust and efficient estimation. *Review of Economic Studies*, pages 1–33.

Brandt, L., Dai, R., Kambourov, G., Storesletten, K., and Zhang, X. (2021). Serial entrepreneurship in china. Working paper.

Brandt, L., Kambourov, G., and Storesletten, K. (2020). Barriers to entry and regional economic growth in china. CEPR Discussion Paper No. DP14965.

Brandt, L., Van Biesebroeck, J., and Zhang, Y. (2012). Creative accounting or creative destruction? firm-level productivity growth in chinese manufacturing. *Journal of Development Economics*, 97(2):339–351.

Branstetter, L., Lima, F., Taylor, L. J., and Venâncio, A. (2013). Do entry regulations deter entrepreneurship and job creation? evidence from recent reforms in portugal. *The Economic Journal*, 124(577):805–832.

Bruhn, M. (2011). License to sell: The effect of business registration reform on entrepreneurial activity in mexico. *The Review of Economics and Statistics*, 93(1):382–386.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

Chen, Z., Liu, Z., Suárez Serrato, J. C., and Xu, D. Y. (2021). Notching r&d investment with corporate income tax cuts in china. *American Economic Review*, 111(7):2065–2100.

Ciccone, A. and Papaioannou, E. (2007). Red tape and delayed entry. *Journal of the European Economic Association*, 5(2-3):444–458.

de Chaisemartin, C. and d'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, 110(9):2964–96.

de Chaisemartin, C. and d'Haultfoeuille, X. (2024). Difference-in-differences estimators of intertemporal treatment effects. *Review of Economics and Statistics*, pages 1–45.

Djankov, S. (2009). The regulation of entry: A survey. *The World Bank Research Observer*, 24(2):183–203.

Djankov, S., La Porta, R., Lopez-de Silanes, F., and Shleifer, A. (2002). The regulation of entry. *The Quarterly Journal of Economics*, 117(1):1–37.

Djankov, S., McLiesh, C., and Ramalho, R. M. (2006). Regulation and growth. *Economics Letters*, 92(3):395–401.

Dunne, T., Klimek, S. D., Roberts, M. J., and Xu, D. Y. (2013). Entry, exit, and the determinants of market structure. *The RAND Journal of Economics*, 44(3):462–487.

Dunne, T., Roberts, M. J., and Samuelson, L. (1988). Patterns of firm entry and exit in us manufacturing industries. *The RAND Journal of Economics*, pages 495–515.

Fattal-Jaef, R. N. (2022). Entry barriers, idiosyncratic distortions, and the firm size distribution. *American Economic Journal: Macroeconomics*, 14(2):416–468.

Fisman, R. and Allende, V. S. (2010). Regulation of entry and the distortion of industrial organization. *Journal of Applied Economics*, 13(1):91–111.

Foster, L., Haltiwanger, J., and Syverson, C. (2008). Reallocation, firm turnover, and efficiency: selection on productivity or profitability? *American Economic Review*, 98(1):394–425.

Foster, L., Haltiwanger, J. C., and Krizan, C. J. (2001). Aggregate productivity growth: lessons from microeconomic evidence. In *New Developments in Productivity Analysis*, pages 303–372. University of Chicago Press.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.

Griliches, Z. and Regev, H. (1995). Firm productivity in israeli industry 1979–1988. *Journal of Econometrics*, 65(1):175–203.

Herrendorf, B. and Teixeira, A. (2011). Barriers to entry and development. *International Economic Review*, 52(2):573–602.

Hopenhayn, H. A. (1992). Entry, exit, and firm dynamics in long run equilibrium. *Econometrica: Journal of the Econometric Society*, pages 1127–1150.

Hopenhayn, H. A. (2014). Firms, misallocation, and aggregate productivity: A review. *Annual Review of Economics*, 6(1):735–770.

Hsieh, C.-T. et al. (2015). Grasp the large, let go of the small: The transformation of the state sector in china. *Brookings Papers on Economic Activity*, page 295.

Jiang, H., Zheng, Y., and Zhu, L. (2021). Growing through competition: The reduction of entry barriers among chinese manufacturing firms. CEPR Discussion Paper No. DP15763.

Kaplan, D. S., Piedra, E., and Seira, E. (2011). Entry regulation and business start-ups: Evidence from mexico. *Journal of Public Economics*, 11(95):1501–1515.

Khandelwal, A. K., Schott, P. K., and Wei, S.-J. (2013). Trade liberalization and embedded institutional reform: Evidence from chinese exporters. *American Economic Review*, 103(6):2169–95.

Klapper, L., Laeven, L., and Rajan, R. (2006). Entry regulation as a barrier to entrepreneurship. *Journal of Financial Economics*, 82(3):591–629.

Levinsohn, J. and Petrin, A. (2003). Estimating production functions using inputs to control for unobservables. *The Review of Economic Studies*, 70(2):317–341.

Liu, Y. and Mao, J. (2019). How do tax incentives affect investment and productivity? firm-level evidence from china. *American Economic Journal: Economic Policy*, 11(3):261–91.

Long, C. and Zhang, X. (2011). Cluster-based industrialization in china: Financing and performance. *Journal of International Economics*, 84(1):112–123.

Maican, F. and Orth, M. (2015). A dynamic analysis of entry regulations and productivity in retail trade. *International Journal of Industrial Organization*, 40:67–80.

Maican, F. G. and Orth, M. (2018). Entry regulations, welfare, and determinants of market structure. *International Economic Review*, 59(2):727–756.

Mankiw, N. G. and Whinston, M. D. (1986). Free entry and social inefficiency. *The RAND Journal of Economics*, pages 48–58.

McChesney, F. S. (1987). Rent extraction and rent creation in the economic theory of regulation. *The Journal of Legal Studies*, 16(1):101–118.

Olley, G. S. and Pakes, A. (1996). The dynamics of productivity in the telecommunications equipment industry. *Econometrica: Journal of the Econometric Society*, pages 1263–1297.

Peters, B., Roberts, M. J., Vuong, V. A., and Fryges, H. (2017). Estimating dynamic r&d choice: an analysis of costs and long-run benefits. *The RAND Journal of Economics*, 48(2):409–437.

Poschke, M. (2010). The regulation of entry and aggregate productivity. *The Economic Journal*, 120(549):1175–1200.

Restuccia, D. and Rogerson, R. (2017). The causes and costs of misallocation. *Journal of Economic Perspectives*, 31(3):151–74.

Schivardi, F. and Viviano, E. (2011). Entry barriers in retail trade. *The Economic Journal*, 121(551):145–170.

Shleifer, A. and Vishny, R. W. (1998). *The grabbing hand: Government pathologies and their cures*. Harvard University Press.

Stigler, G. J. (1971). The theory of economic regulation. *The Bell Journal of Economics and Management Science*, 2(1):3.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

Tombe, T. and Zhu, X. (2019). Trade, migration, and productivity: A quantitative analysis of china. *American Economic Review*, 109(5):1843–72.

Wang, S. and Yang, D. Y. (2021). Policy experimentation in china: The political economy of policy learning. Technical report, National Bureau of Economic Research.

Wei, W. and Sanchez Ortega, L. A. (2022). Business registration reforms in china. Technical report, World Bank, Washington, DC.

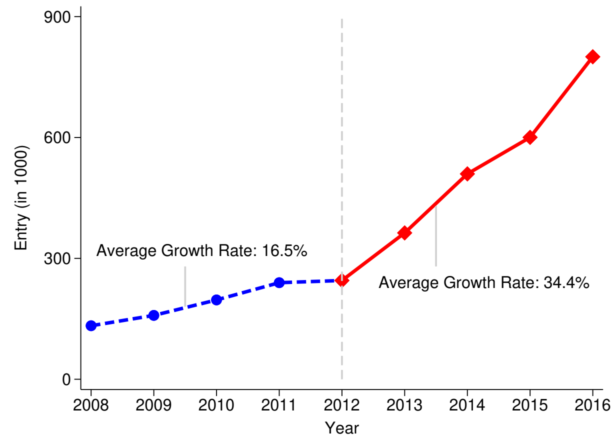
Yakovlev, E. and Zhuravskaya, E. (2013). The unequal enforcement of liberalization: evidence from russia's reform of business regulation. *Journal of the European Economic Association*, 11(4):808–838.

Yu, M. (2015). Processing trade, tariff reductions and firm productivity: Evidence from chinese firms. *The Economic Journal*, 125(585):943–988.

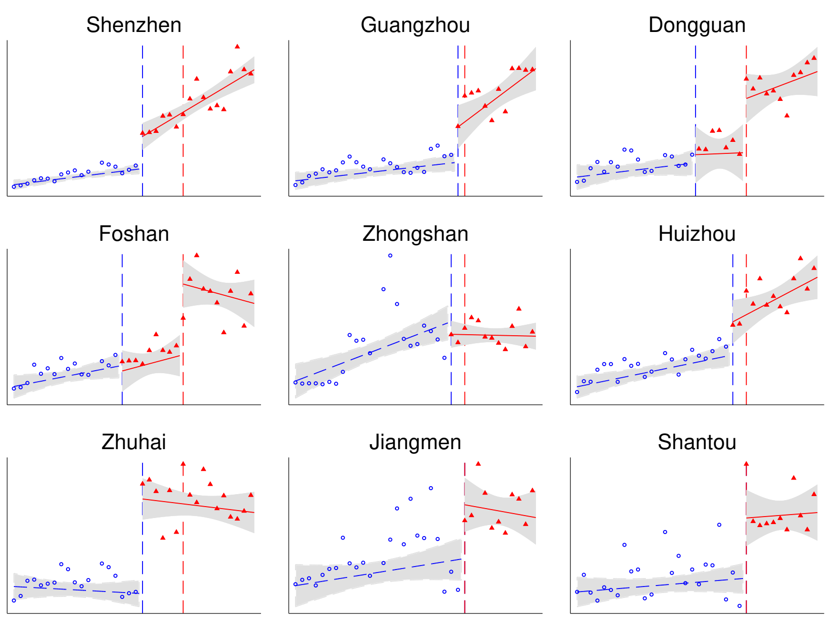
Figures

Figure 1: Number of New Entrants in Guangdong Province

(a) Total Number of Entrants



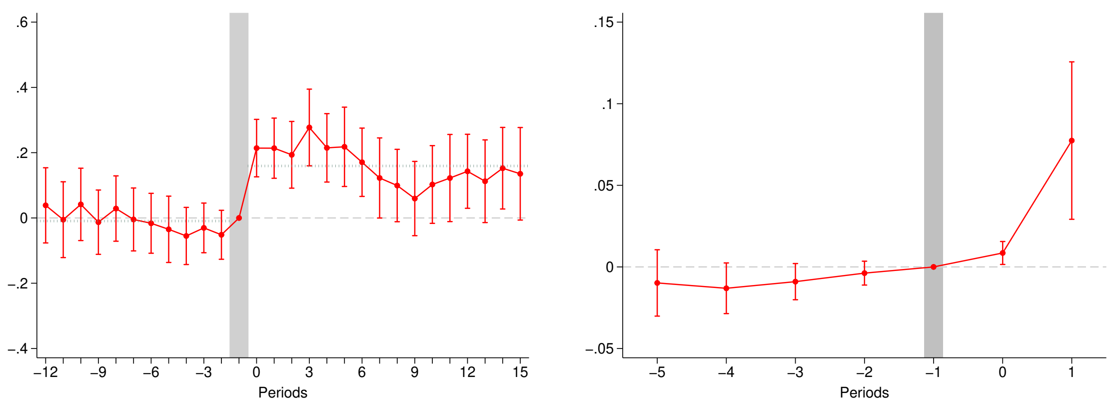
(b) Number of Entrants by City



Notes: This figure plots the number of new entrants in Guangdong province. Panel (a) shows the annual trend of firm entry (in 1,000 of firms) for the entire Guangdong province. The blue/red dots denote raw data time series before/after the reform. Panel (b) shows the monthly trend of firm entries for the nine cities with the highest number of registered firms, listed in descending order. The blue dashed vertical line represents the start month of a city's pilot program and the red vertical line represents March 2014 when the reform was implemented nationwide. Linear fits and 95% confidence intervals (in grey) are added in Panel (b).

Figure 2: The Effect of Entry Deregulation on Firm Entry and Exit: Event Study

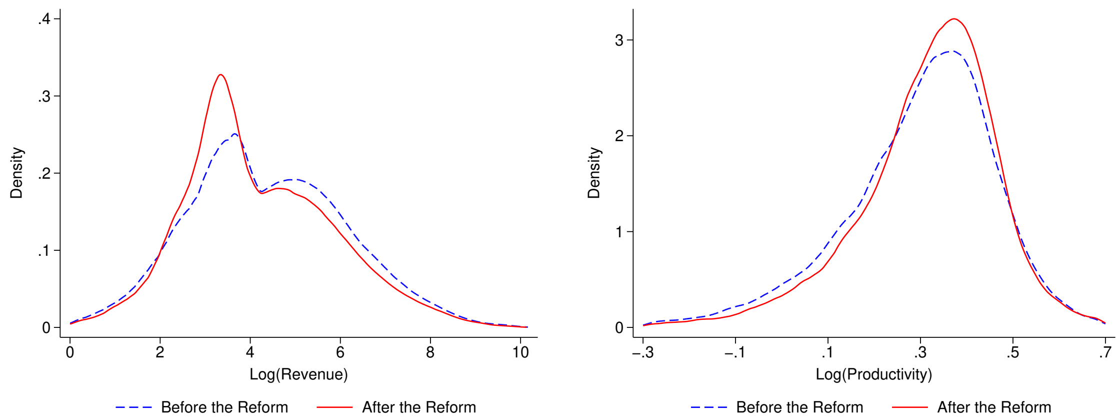
(a) Entry **(b)** Exit



Notes: This figure shows the event study coefficient estimates for firm entry and exit following Equation (2). Panel (a) reports results on firm entry (measured by log number of new firms) at the city, two-digit industry, and month level for the manufacturing sector from Jan 2009 to Dec 2016. The horizontal axis denotes months before and after the reform. Panel (b) reports results for firm exit, where the dependent variable is whether firm i exits (stops submitting annual reports) in year $t+1$. The sample is from 2009 to 2015 (we drop 2016 since a firm's exit status is unknown for 2016). The horizontal axis denotes the years before and after the reform. For both Panels (a) and (b), the 95% confidence intervals are constructed from clustered standard errors at the city and industry level.

Figure 3: Entrants' Size and Productivity Distribution

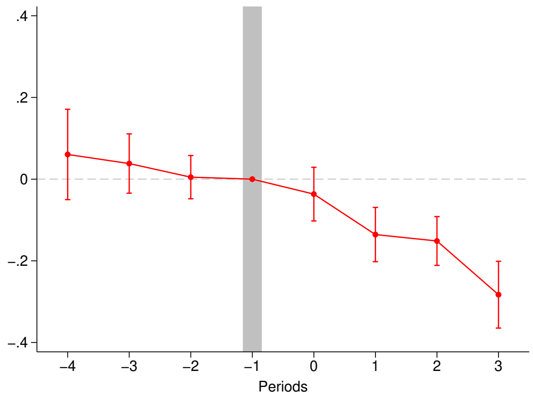
(a) Log(Revenue) **(b)** Log(Productivity)



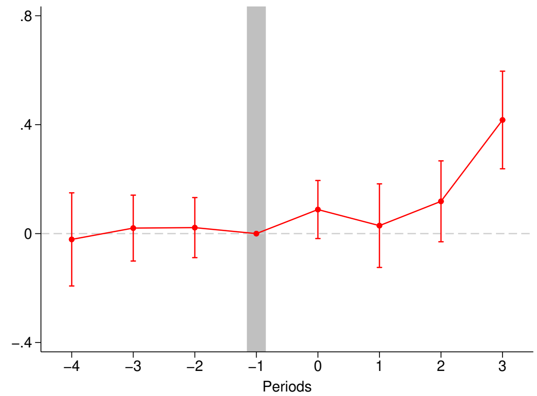
Notes: This figure describes the distribution of firm size (measured by log revenue) and productivity for new entrants from 2009 to 2016. New entrants are defined as firms at age one, or firms in their first calendar year of operation immediately following their registration year. Dashed blue curves (solid red curves) denote the cohorts that entered before (after) the reform. Panels (a) and (b) present the kernel density of entrants' log revenue and log productivity, respectively. Panel (b) partials out a cubic function of log capital from productivity to control for size differences between pre- and post-reform cohorts.

Figure 4: The Effect of Entry Deregulation on Firm Size and Productivity: Event Study

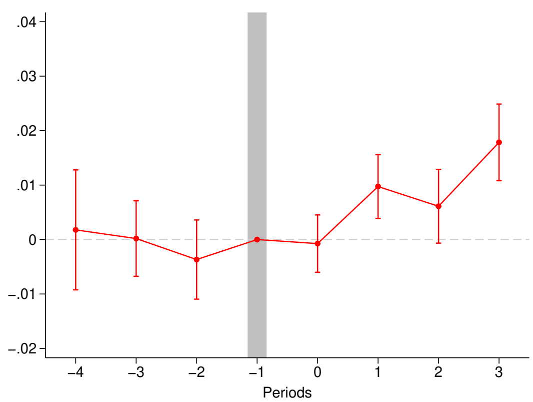
(a) Log(Revenue): Entrants



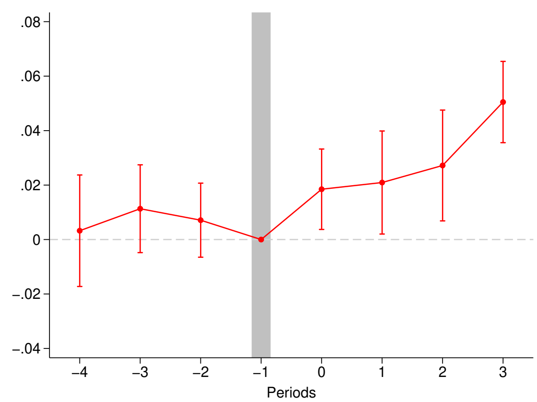
(b) Log(Revenue): Exiting Firms



(c) Log(Productivity): Entrants

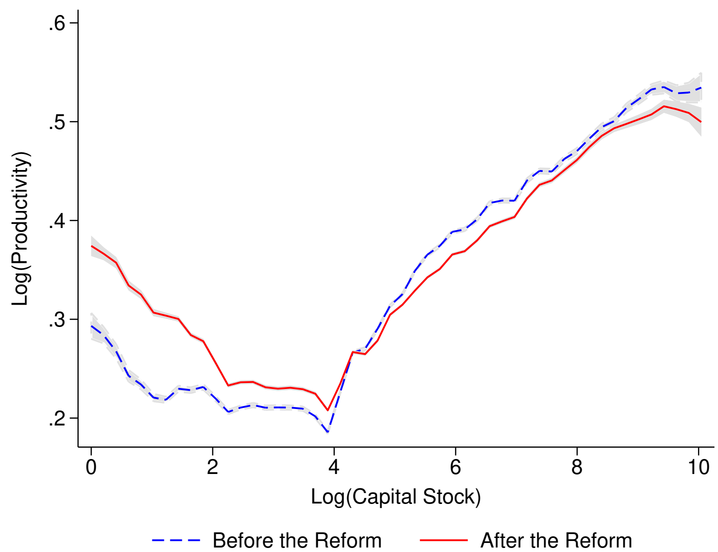


(d) Log(Productivity): Exiting Firms



Notes: This figure presents event study coefficient estimates on firm size (measured by log revenue) and productivity (log-valued), for entrants and exiting firms respectively. Entrants are defined as those at age one; exiting firms are those that exit the following year. The observation is at the firm-year level from 2009 to 2015. The horizontal axis denotes years before and after the reform. The 95% confidence intervals are constructed from clustered standard errors at the city and industry level.

Figure 5: Relationship between Entrant's Productivity and Size



Notes: This figure describes the relationship between firm size (measured by log revenue) and productivity for new entrants from 2009 to 2016. New entrants are defined as firms at age one. Dashed blue curves (solid red curves) denote the cohorts that entered before (after) the reform. The figure displays the local polynomial fit of entrants' log productivity with respect to the log capital stock. The grey area represents 95% confidence intervals.

Tables

Table 1: The Effect of Entry Deregulation on Firm Entry

	Log(Entry)			Log(Entry Rate)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.211***	0.259***	0.245***	0.221***	0.262***	0.248***
	(0.043)	(0.043)	(0.037)	(0.042)	(0.043)	(0.037)
Observations	54,336	54,336	54,336	53,709	53,709	53,709
Adjusted R^2	0.941	0.943	0.950	0.689	0.699	0.733
City-Industry, City-Year FE	✓	✓	✓	✓	✓	✓
Month-of-Sample FE	✓	✓		✓	✓	
City-Calendar-Month FE		✓	✓		✓	✓
Industry-Month-of-Sample FE			✓			✓

Notes: This table shows the baseline results for the deregulation's impact on firm entry following Equation (1). The unit of observation is a city, two-digit manufacturing industry, and month, with the sample period from January 2009 to December 2016. "Treatment" is a dummy variable that takes value one for months during and post a city's reform implementation date and zero otherwise. The dependent variable of Columns (1)-(3) is the log number of new entrants, while the dependent variable of Columns (4)-(6) is the log entry rate (the ratio of entrants to lagged incumbents). The numbers of observations are smaller in Columns (4)-(6) because entry rate is undefined when the lagged incumbent number is zero. All specifications control for city-by-year fixed effects, city-by-industry fixed effects, and month-of-sample fixed effects. We add city-by-calendar-month fixed effects in Columns (2) to (3) and (5) to (6) and additionally replace month-of-sample fixed effects with industry-by-month-of-sample fixed effects in Columns (3) and (6). All regressions are weighted by number of firms in each city-industry pair at the beginning of the sample period. Standard errors are clustered at the city-industry level. $^{*}p<0.10$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 2: The Effect of Entry Deregulation on Firm Entry: Heterogeneity

	Log(Entry)		Log(Entry Rate)	
	(1)	(2)	(3)	(4)
Treatment	0.031	0.107***	0.025	0.107***
	(0.032)	(0.034)	(0.032)	(0.032)
Treatment × Private	0.150***		0.156***	
	(0.038)		(0.037)	
Treatment × RegCost		0.074*		0.070*
		(0.043)		(0.040)
Observations	99,659	83,391	99,659	83,391
Adjusted R^2	0.956	0.956	0.823	0.817
City-Industry-Owner, City-Year FE	✓	✓	✓	✓
City-Calendar-Month FE	✓	✓	✓	✓
Industry-Owner-Month-of-Sample FE	✓	✓	✓	✓

Notes: This table examines heterogeneity in the entry deregulation's effect on firm entry. The observation is at the city, 2-digit manufacturing industry, ownership type (private or non-private), and month level from January 2009 to December 2016. The number of observations roughly doubles that of Table 1 as the data is disaggregated at the ownership-type level. The dependent variable is the log number of new entrants in Columns (1)-(2) and log entry rate in Columns (3)-(4). "Treatment" is a dummy variable that takes value one for months during and post a city's reform implementation date and zero otherwise. "Private" is a dummy variable that takes value one for privately-owned firms and zero otherwise. "RegCost" is constructed from 2018 ESIEC survey data and takes value one if the industry experienced a above-median decrease in the reported registration costs and zero otherwise. The numbers of observations are smaller in Columns (2) and (4) due to missing "RegCost" values. All regressions are weighted by number of firms in each city-industry pair at the beginning of the sample period. We include the same set of fixed effects as in Columns (3) and (6) in Table 1 except that we further interact city-by-industry fixed effects and industry-by-month-of-sample fixed effects with ownership types. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 3: The Effect of Entry Deregulation on Firm Exit

	Exit Dummy (× 100)					
	All Firms			Exclude Post-Reform Entrants		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.498*	0.469**		0.543*	0.544**	
	(0.288)	(0.224)		(0.291)	(0.230)	
Treatment × RegCost			1.738**			1.516**
			(0.793)			(0.651)
Log(Revenue)		-0.521***	-0.531***		-0.525***	-0.532***
		(0.013)	(0.014)		(0.012)	(0.013)
Observations	1,392,721	1,375,813	1,340,428	1,290,437	1,275,036	1,241,527
Adjusted R^2	0.034	0.041	0.065	0.028	0.036	0.059
City-Industry-Owner FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓		✓	✓	
City-Year FE			✓			✓

Notes: This table shows the baseline results for the deregulation's impact on firm exit following Equation (3), using firm-year observations. All columns control for firm age and ownership type fixed effects. The dependent variable is a dummy variable of whether a firm exits (stop submitting annual reports) in the next year. The coefficients are scaled by 100 for ease of interpretation. For example, the coefficient of 0.498 in Column (1) implies that the exit rate is 0.498 percentage points higher post the reform. The average exit rate was 5.7% prior to the reform. "Treatment" is a dummy variable that takes value one in years during and after a city's reform implementation date and zero otherwise. "RegCost" flags industries that experienced above-median reductions in the reported registration cost. Log revenue is added as a control variable (results are similar using lagged log sales). Columns (1)-(3) include all firms, while Columns (4)-(6) exclude firms that entered after the reform. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 4: The Effect of Entry Deregulation on Firm Size and Productivity

	Log(Revenue)		Log(Productivity)	
	(1)	(2)	(3)	(4)
Entrants	-1.035***	-0.933***	-0.059***	-0.055***
	(0.035)	(0.023)	(0.003)	(0.002)
Exiting Firms	-0.906***	-0.684***	-0.069***	-0.058***
	(0.030)	(0.034)	(0.004)	(0.004)
Treatment × Entrants	-0.149***	-0.191***	0.012***	0.011***
	(0.034)	(0.023)	(0.003)	(0.002)
Treatment × Exiting Firms	0.345***	0.191***	0.034***	0.026***
	(0.062)	(0.054)	(0.005)	(0.005)
Observations	1,042,425	1,042,385	1,042,425	1,042,385
Adjusted R^2	0.101	0.229	0.071	0.112
City-Industry FE	✓		✓	
City-Industry-Owner FE		✓		✓
City-Year FE	✓	✓	✓	✓
Industry-Year FE	✓		✓	
Industry-Owner-Year FE		✓		✓

Notes: This table shows the baseline results for deregulation's impact on firm size and productivity as specified in Equation (4). The observation is at firm-year level. The number of the observation is smaller than in Table 3 due to missing values. The dependent variables are log revenue in Columns (1)-(2) and log-valued structurally estimated productivity following Aw et al. (2011) and Peters et al. (2017) in Columns (3)-(4). "Treatment" is a dummy variable that takes value one in years during and after a city's reform implementation date and zero otherwise. The dummy variable "Entrants" flags firms at age one and "Exiting Firms" takes value one if a firm is in its last year of operation and with age greater than one. We control for city-by-industry fixed effects, city-by-year fixed effects, and industry-by-year fixed effects in Columns (1) and (3). We add ownership-type fixed effects and interact city-by-industry fixed effects and industry-by-year fixed effects with ownership types in Columns (2) and (4). Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 5: Evidence for the *Market-Competition Effect*

	Log(Revenue)	Log(Productivity)
	(1)	(2)
Entrants Who Entered Before the Reform	-0.896***	-0.051***
	(0.022)	(0.002)
Treatment × Entrants Who Entered Before the Reform	-0.000	0.007**
	(0.029)	(0.003)
Observations	991,401	991,401
Adjusted R^2	0.223	0.113
City-Industry-Owner FE	✓	✓
City-Year FE	✓	✓
Industry-Owner-Year FE	✓	✓

Notes: This table explores the importance of the *market-competition effect* in explaining entrants' productivity gains. We repeat the analyses of Equation (4) but exclude establishments registered after the reform. The observation is at firm-year level. The dependent variable is log revenue in Column (1) and estimated productivity (log-valued) following Aw et al. (2011) and Peters et al. (2017) in Column (2). "Treatment" takes value one for years during and post a city's reform implementation date and zero otherwise. "Entrants Who Entered Before the Reform" takes value one if a firm was at age one and entered before the reform. We control for the same set of fixed effects as those in Columns (3) and (6) of Table 4. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 6: Evidence that Entrant Composition Changed Post Reform

Panel A: Source of Funds		
	Borrowing	Loan
Treatment	-0.042*	-0.023
	(0.022)	(0.020)
Mean of Dep Var	0.306	0.210
Observations	3,092	3,093
City FE	✓	✓
Industry (1-digit) FE	✓	✓

Panel B: Entrepreneurial Characteristics		
	Years of Schooling	Age Below 35
Treatment	-0.198**	0.028*
	(0.095)	(0.017)
Mean of Dep Var	10.557	0.310
Observations	96,867	66,289
City-Industry, City-Year FE	✓	✓
City-Calendar-Month FE	✓	✓
Industry-Month-of-Sample FE	✓	✓

Panel C: Shareholding Structures		
	Share by Individuals	Sole Proprietorship
Treatment	0.023***	0.020***
	(0.007)	(0.007)
Mean of Dep Var	0.904	0.939
Observations	366,486	366,486
City-Industry, City-Year FE	✓	✓
City-Calendar-Month FE	✓	✓
Industry-Month-of-Sample FE	✓	✓

Notes: This table presents evidence of the *composition* effect. Panel A explores entrepreneurs' sources of funds when starting a new business. The sample consists of firm-level observations in six provinces from the 2018 ESIEC survey. The dependent variables are whether the entrepreneur borrowed from friends/relatives in order to establish a business and whether the entrepreneur loaned from financial institutions. Panel B examines the entrepreneurial characteristics of new firms (available from Jan 2009 to May 2015). The dependent variables are years of schooling and whether the entrepreneur's age is below 35. Panel C investigates the shareholding structure of new firms. The observation is at the firm level for entrants registered between Jan 2009 and Dec 2016. We categorize shareholders into two types: individuals (natural persons) and corporations (legal persons). The dependent variables are the share of registered capital owned by individual shareholders and whether the firm is a sole proprietorship (i.e., 100% owned by natural persons). "Treatment" is a dummy variable that takes value one for years during and post a city's reform implementation date and zero otherwise. We control for city fixed effects and one-digit-industry fixed effects in Panel A. Panels B and C include the same set of fixed effects as in Column (2) of Table 1. Standard errors are clustered at the city-industry level. $^{*}p<0.10$; $^{**}p<0.05$; $^{***}p<0.01$.

Online Appendix for Entry Deregulation, Market Turnover, and Efficiency: China's Business Registration Reform

Panle Jia Barwick Luming Chen Shanjun Li Xiaobo Zhang

August 2024

A Additional Information about Data

A.1 Business Registry Database

The Business Registry Database includes several different modules. We use the Firm Registry Database for our main empirical analysis, and also use the Firm Personnel Database, Firm Shareholder Database, and *Getihu* Registration Database for robustness checks and channel tests.

Firm Personnel Database The Firm Personnel Database is from the State Administration for Industry and Commerce (SAIC). We use its Guangdong sample from January 2009 to May 2015 to construct entrepreneurial characteristics, including education, birth place, and age. We keep the information of legal representatives of the entrants.

Firm Shareholder Database The Firm Shareholder Database is from SAIC. We use its Guangdong sample from 2009 to 2016 to measure shareholding structures of each entrant. We categorize shareholders into two types: individual shareholders (natural persons) and corporate shareholders (legal persons). We construct measures of the share held by individual shareholders and the ratio of firms owned entirely by individual shareholders. We also match it with Firm Registry Database to construct the measure of whether any (or the largest) individual shareholder of the firm is also the owner of another existing firms, i.e., whether any (or the largest) individual shareholder is a serial entrepreneur or not.

***Getihu* Registration Database** The *Getihu* Registration Database is from SAIC. We use its Guangdong sample from 2009 to 2016 to measure the number of new *getihu* in each city, industry, and month to check the reclassification channel of firm entry.

A.2 National Tax Survey Database

National Tax Survey Database is from the State Administration of Taxation and the Ministry of Finance. We use its Guangdong sample of manufacturing from 2010 to 2015 for the following purposes: (1) to calculate average wage in each city, industry, and year as the ratio of total payroll over total employment from 2010 to 2015;[30] (2) to estimate elasticity of substitution using the firm-level revenue and profit data; (3) to construct the industry-level export share in the total revenue; (4) to replicate the baseline productivity estimation and test whether omitting labor information results in large bias or not. More information about the National Tax Survey Database can be found in Liu and Mao (2019).

A.3 Samples of Firm-level Data

We use the Firm Annual Report Database to construct samples at the firm-year level to examine the impacts of entry deregulation on firm exit, size, and productivity. We present a summary of the number of entrants, incumbents, and total firms in 2009-2015, as shown in Table A8. When examining firm exit (Table 3), we include all firms from the Firm Annual Report Database. However, when examining firm size and productivity (Table 4), we keep the sample where productivity estimates are available to make the results comparable. Therefore, the samples used for firm size and productivity are smaller, as key variables are missing for some observations in productivity estimation. Alternatively, we could also use the full sample to examine the impacts on firm size and the revenue-capital ratio, and the results are very robust.

[30] We further impute the wage data in 2008, 2009, and 2016, assuming a constant average wage growth rate within each city-industry pair.

B Robustness Analyses

B.1 Robustness Checks on Dynamic Treatment Effects

Recent studies discuss important pitfalls of estimating the average treatment effects and dynamic treatment effects via two-way fixed effects model in the staggered DID setting. Two-way fixed effects regression yields estimates as weighted sums of the average treatment effects of each group and period, and some of the weights could be negative. The main issue comes from the "forbidden comparison" where the already-treated group serves as the comparison group for the late-treated units. Furthermore, the estimated coefficient on a specific lead or lag is a linear combination of the effects from all leads and lags, thus pre-treatment estimates could also be contaminated by the bias in other periods.

To address the concern in the OLS estimates, we follow different approaches in the literature to replicate the event study results on firm entry and exit. For each method, we first provide a brief summary of the proposed estimation steps, and then discuss the results.

Sun and Abraham (2021) We follow Sun and Abraham (2021) and estimate dynamic treatment effects in the following steps. The estimation is accomplished using the stata command `eventstudyinteract` published by the authors.[31]

1. We define groups of cities by the months when they implemented the reform. We denote the month when city c implemented the reform as E_c, and use e as the group index. For each group of cities, we estimate the average treatment effects using a linear two-way fixed effects regression.

$$Y_{cit} = \xi_{ci} + \tau_t + \sum_{e \notin C} \sum_{q \neq -1} \delta_{eq}(\mathbb{1}\{E_c = e\} \times D^q_{ct}) + \zeta_{cy(t)} + \epsilon_{cit}$$

where i, t and $y(t)$ represent industry, month of sample, and year respectively. D^q_{ct} is an indicator for city c being q months away from initial treatment in month t. Following the

[31] We present the steps for estimating the effects on firm entry only. The steps for firm exit are similar.

baseline model in Equation (1), we further control for the city-by-industry fixed effects ξ_{ci}, the month-of-sample fixed effects τ_t, and the city-by-year fixed effects $\zeta_{cy(t)}$. As there is no never-treated cities, we exclude observations after the national reform was carried out and use non-pilot cities as the comparison group. Therefore, we set $C = \{\max\{E_c\}\}$.

2. We estimate the weight w_{eq} for δ_{eq} as the sample share of group e among cities for which q months after the their initial treatment are within our sample periods. The weights sum up to one and are non-negative.

3. We take the group average treatment effects from step 1 and weights from step 2 to form the estimator.

$$\beta_q = \sum_e \hat{w}_{eq}\hat{\delta}_{eq}$$

The estimation results are shown in Figure A5. The point estimates of dynamic treatment effects on firm entry are larger than those estimated via OLS, but the OLS estimates fall within the confidence intervals we obtain here. Meanwhile, the point estimates of dynamic treatment effects on firm exit are very similar to the results via OLS. More importantly, we observe sharp increases in both the firm entry and exit following entry deregulation, which further validates our baseline results.

Borusyak et al. (2024) Borusyak et al. (2024) proposes an imputation-based estimator. We follow their method and estimate dynamic treatment effects in the following steps. The estimation is accomplished using the stata command `did_imputation` published by the authors.

1. We denote $Y_{cit}(0)$ as the potential outcome of the city c and industry i in month t if it's never treated. We keep all the untreated observations for which $Y_{cit} = Y_{cit}(0)$, and estimate ξ_{ci}, τ_t and $\zeta_{cy(t)}$ by OLS in the following regression.[32]

$$Y_{cit} = Y_{cit}(0) = \xi_{ci} + \tau_t + \zeta_{cy(t)} + \epsilon_{cit}$$

[32] In order to back out the full set of $\zeta_{cy(t)}$, we have to restrict our sample such that it only covers years before 2014. Moreover, for two early-treated cities Dongguan and Foshan, we have to drop their observations in 2013.

2. For each treated observation, the imputed potential outcome if it is never treated can be expressed as the follows.

$$\hat{Y}_{cit}(0) = \hat{\xi}_{ci} + \hat{\tau}_t + \hat{\zeta}_{cy(t)}$$

The estimated treated effect is thus $\hat{\delta}_{cit} = Y_{cit} - \hat{Y}_{cit}(0)$.

3. We estimate the dynamic treatment effects by a weighted sum.

$$\hat{\beta}_q = \sum_{cit \in \Omega_q} \hat{w}_{cit} \hat{\delta}_{cit}$$

where $w_{cit} = \mathbb{1}\{t - E_{ci} = q\} / \mid \Omega_q \mid$ for $\Omega_q = \{cit : t - E_{ci} = q\}$. E_{ci} is the starting time of the reform for city c and industry i, and q denotes the number of months between the initial implementation of the reform and month t.

4. We test the parallel trends assumption separately. We keep all the untreated observations and estimate the following model by OLS, where $q \leqslant -14$ serve as the reference group in order to rule out the possible contamination of the treatment anticipation.

$$Y_{cit}(0) = \xi_{ci} + \tau_t + \zeta_{cy(t)} + \sum_{1 \leqslant q \leqslant 13} \delta_q D_{ct}^{-q} + \epsilon_{cit}$$

The estimation results are shown in Figure A5. The estimates of dynamic treatment effects are similar to our baseline results via OLS. We again observe sharp increases on both firm entry and exit, thus our baseline results are robust via the imputation-based estimator proposed by Borusyak et al. (2024).

de Chaisemartin and d'Haultfoeuille (2024) We follow de Chaisemartin and d'Haultfoeuille (2024) and estimate dynamic treatment effects in the following steps. The estimation is accomplished using the stata command `did_multiplegt` published by the authors. We use 50 bootstrap replications to estimate standard errors. The estimators are numerically equivalent to Callaway and

Sant'Anna (2021) if there are no covariates in the estimation and the not-yet-treated are used as the comparison groups.

1. We define groups of cities by the months when they implemented the reform. We denote the month when city c implemented the reform as E_c, and use e as the group index. Moreover, we use q to indicate the number of periods since the implementation of the pilot. For each group of cities e in period q, we estimate the average treatment effects by comparing the outcomes of group e with the the outcomes of not-yet-treated cities in time $E_c + q$ and $E_c - 1$.

2. We weight the DID estimators with their sample shares.

3. We estimate the coefficients of the lags (placebo estimators) separately, by comparing the outcome evolution between the same groups as mentioned above, but q months before groups switch their treatment status.

The estimation results are shown in Figure A5 and are robust compared our baseline OLS results.

Triple Difference Estimates The concerns over heterogeneous treatment effects in the staggered DID setting are also present in our triple difference estimates in Table 4. We check the robustness of our results using two subsamples in which the treatment was not staggered. We categorize cities into three group according to the year when the reform was implemented. Groups 1, 2, and 3 started entry deregulation in 2012, 2013, and 2014, respectively. In the first robustness check, we drop Group 2 and only keep observations before 2014; thus, only those firms in Group 1 were treated in 2012, and those in Group 3 were never treated during the sample period. We find that entrants and exiting firms both become larger, consistent with results in Table 4. Moreover, we find exiting firms more productive, though the effects on entrants are small and insignificant.

In the second robustness check, we drop firms in Group 1 and only keep observations before 2014; thus, firms in Group 2 were treated in 2013, and firms in Group 3 were never treated during the sample period. We find entrants are smaller; the effects on exiting firms are negative but statistically insignificant. Furthermore, we find both entrants and exiting firms are more productive after the reform, consistent with results in Table 4, although the impact on exiting firms is small. The results are consistent with the fact that it may take longer time for the effects on exiting firms to materialize.

B.2 Robustness Checks on Firm Entry

This section examines the robustness of the empirical results on firm entry, regarding the concerns of sample selection, anticipation effect, and geographical spillovers. One might question whether the massive new entries are *de novo*, and we demonstrate it by ruling out alternative explanations. Moreover, while not the focus of this paper (as we cannot reliably measure productivity in non-manufacturing sectors), we also examine the impacts of entry deregulaton on firm entry in retail, wholesale, and the service sector.

Sample Selection, Anticipation Effect, and Geographical Spillover To examine the robustness of the baseline results, we conduct a battery of checks in Table A9. First, as discussed in Section 2.2, because Foshan and Dongguan were the earliest to experiment with the entry deregulation, the measures implemented may have been different from those implemented in other cities in later periods. Indeed, these two early pilot cities did not experience an increase in the number of new firms until after the national reform began (Figure 1). Column (1) excludes Foshan and Dongguan, and the estimated effect rises to 37%.

Following recommendations from the recent literature (Goodman-Bacon, 2021), Column (2) drops observations after March 2014 and uses a shorter sample (Jan 2009 to Feb 2014) such that the non-pilot cities were never treated in this time window. The estimate is robust and a bit larger than the baseline estimate at 30%.

In practice, it took several months between the reform's announcement date and implementation date. One might be concerned that upon receiving the policy announcement, firms could in principal postpone the registration process in anticipation of the reform. Indeed, Panel (a) of Figure 2 reports a slight though insignificant dip before the reform, suggesting the possibility of the anticipation effect. Column (3) further drops the four months right before the policy implementation date to mitigate the anticipation effect. The estimated coefficient decreases to around 20%, but is still within the confidence interval of our baseline estimates.

Another potential confounding factor is geographical spillovers – firms from nearby cities or regions might register in reforming cities to take advantage of the entry deregulation. Those geographical spillovers would contaminate both the control group and the treatment group and bias results upwards. Our baseline regressions only use firm registration data from the Guangdong province. To address the issue of geographical spillovers, we expand our regression sample and replace non-pilot cities in Guangdong province with cities in Jiangsu province (Column 4) and Shandong province (Column 5), respectively, while restricting the sample to February 2014 as in Columns (2) and (3). Jiangsu and Shandong have the second- and third-highest provincial GDP in China. They are comparable to Guangdong in economic terms but about 2,000km away in geographic distance, so they are less likely to be "contaminated".[33] The estimated coefficients vary from 26.1% to 26.5%, similar to the baseline estimate. Overall, results in Table A9 indicate that our finding of a significant increase in newly registered firms is robust to different subsamples, the anticipation effect, and potential geographical spillovers.

Are New Entrants *De Novo*? Although Table A9 provides evidence that the baseline results are robust, one might question whether the massive new entries are *de novo*. There are three alternative explanations. First, the newly registered firms could be spin-offs of existing firms, instead of *de novo* entrants. That is, the large increase in the number of firms in the wake of reforms reflects the expansion of existing firms, not the growth in the entrepreneur pool. We test this channel by excluding new firms established by serial entrepreneurs, defined as individual investors who have invested in other existing firms (Brandt et al., 2021). If spin-offs are important, we would observe a

[33] As discussed below in Table A10, the contribution of entrepreneurs who are from other cities and provinces but register in the treated pilot cities to the increase in new firms is modest and statistically insignificant.

much smaller increase in the volume of entrants once those owned by serial entrepreneurs are taken out. Panel A of Table A10 contradicts this hypothesis. The estimates remain robust after excluding new firms by serial entrepreneurs, suggesting that spin-offs from existing firms are unlikely to be the main driver of our baseline results.

Second, the higher entry rates post the reforms might reflect a reclassification of small, family-operated businesses (*getihu* or mom-and-pop operations) into formal firm establishments. In the presence of reclassification, the total number of business entities remains the same despite the increase in newly incorporated firms. To address the concern, we replicate Table 1 but replace the dependent variable with the log number of *getihu* entry. If the reclassification of mom-and-pop operations plays an important role so that *getihu* owners register their businesses as firms, we would expect a negative (or a small positive) estimate. On the contrary, Panel B of Table A10 reports that the entry reform led to a 27.6% to 31.5% increase in the number of registered *getihu* entities, in line with (and slightly larger than) the baseline findings for new firms. The increase in registered *getihu* reflects the simplified registration process across the board for *all* types of business entities post the reform. To provide direct evidence on this, we leverage the ESIEC field surveys and examine past business experience for surveyed entrepreneurs. If the reclassification from *getihu* to firms is the main channel underlying the surge in newly established firms, we would expect a larger proportion of entrepreneurs who were interviewed post the reform to be *getihu* owners in the past. However, the fraction of used-to-own-*getihu* entrepreneurs does not exhibit any obvious trend in relation to the timing of the reform, as shown in Figure A6. These results largely rule out the reclassification channel.[34]

Third, the increase of new firms may be an artifact of existing firms from other regions relocating to cities in Guangdong that implemented the reform ahead of the rest of the country. Fortunately, the registration data include the hometown origins (the birth place) of entrepreneurs, allowing us to directly assess this channel. We examine whether the number of new firms set up by non-local entrepreneurs (i.e., those with hometowns outside a city/province) disproportionately increases af-

[34] Regressions with log newly established *getihu* + firms as the dependent variable deliver very similar results.

ter the reform. The possibility that entrepreneurs are from the same city as the location of the firm but a different district rises by 2.4% (the dependent variable mean is 68.9%), reflecting the increased mobility of entrepreneurs within a city (Panel C of Table A10, Column 1). However, there is no evidence that more entrepreneurs are from cities or provinces different from the firm location (Panel C, Columns 2 and 3). Therefore, spatial relocation is not a primary factor. Instead, the entry deregulation in Guangdong promoted local entrepreneurship *within* the province.

Other Sectors While not the focus of this paper (as we cannot reliably measure productivity in non-manufacturing sectors), the reform also significantly increased firm entry in retail, wholesale, and service sectors, with even larger impacts than those in manufacturing (Figure A7). Therefore, the effect of entry deregulation on new businesses appears to be broad-based and is not limited to the manufacturing sector.

B.3 Robustness Checks on Firm Productivity

We have implemented a series of robustness checks. Having established that entry deregulation improves the productivity for new businesses in the manufacturing sector, we examine whether the effect persists over time. Figure A8 plots the average revenue (a measure of firm size) and the revenue-weighted productivity for the treated and untreated cohorts by firm age. The average firm size of the treated cohort is smaller than that of the untreated cohort throughout all three years after the registration year – the longest period we could follow an entrant. In contrast, the productivity of the treated cohort is persistently higher, and the productivity gains (or efficiency improvement) are stable (and slightly stronger) over time.

Our main analysis is limited to the manufacturing sector because we do not have data to reliably estimate productivity for other sectors, such as the service sector. Using the revenue-over-capital ratio as a proxy (which is subject to various caveats), we document that the reform also significantly improved entrants' productivity in retail trade, wholesale trade, and service industries, as shown in Figure A7. These patterns provide suggestive evidence that the effect of entry regulation on entrants' productivity goes beyond the manufacturing sector and extends to many segments of the aggregate economy.

Finally, a natural question is whether the entry deregulation affects the productivity of incumbents. We focus on the 2010 incumbent cohort and utilize the within-firm variation to explore the policy's impact on incumbents. Table A11 presents results using different sub-samples (firms that file annual reports in all years, all but one year, etc.). Overall, there is no noticeable effect on incumbents. This should not be surprising. Given the post-reform entry rate of roughly 20%, it takes many years before the reform significantly changes the incumbent pool. Our sample period (which covers two to three years post the reform) is not long enough to detect composition changes that emerge more slowly among incumbent firms.

C Productivity Estimation

C.1 Productivity Estimation Used in the Main Analyses

We estimate firm productivity using a simple structural model, following Aw et al. (2011) and Peters et al. (2017), but abstracting away the dynamic considerations. We first introduce our simple static structural model for productivity estimation.

Model *Demand* We model the demand as in the Dixit-Stiglitz form. The demand curve faced by firm i in year t is

$$q_{it} = Q_t \left(p_{it}/P_t\right)^{-\eta} = \Phi_t \left(p_{it}\right)^{-\eta}$$

where q_{it} and p_{it} represents the quantity and price, and Q_t and P_t are aggregate quantity and price from the CES aggregation, which are further combined into Φ_t. η is the constant elasticity of substitution among goods produced by different firms.

Supply and Static Equilibrium We specify firm i's short-term marginal cost function as

$$\ln c_{it} = \ln c\left(k_{it}, a_{it}, \boldsymbol{W_{mt}}, \boldsymbol{Z_{om}}\right) - \varphi_{it} = \beta_0 + \beta_k \ln k_{it} + \beta_a a_{it} + \boldsymbol{\beta_w W_{mt}} + \boldsymbol{\beta_Z Z_{om}} - \varphi_{it} \quad (6)$$

where c_{it} is the marginal cost, k_{it} is the capital stock, a_{it} is the firm age, and φ_{it} denotes the firm productivity. Additionally, we control for $\boldsymbol{W_{mt}}$, which measures input prices in market m, and $\boldsymbol{Z_{om}}$, which captures common cost shifters shared by firms of the same ownership-type o in market m. Furthermore, productivity follows a Markov process

$$\varphi_{it} = g(\varphi_{it-1}) + \xi_{it} = \alpha_0 + \alpha_1 \varphi_{it-1} + \alpha_2 \left(\varphi_{it-1}\right)^2 + \alpha_3 \left(\varphi_{it-1}\right)^3 + \xi_{it} \quad (7)$$

where ξ_{it} is an *iid* shock of mean zero.

Combining both the demand and supply side, we derive the revenue and profit function as follows:

$$\ln r_{it} = (1-\eta)\ln\left(\frac{\eta}{\eta-1}\right) + \ln \Phi_t + (1-\eta)\left(\beta_0 + \beta_k \ln k_{it} + \boldsymbol{\beta_w W_{mt}} + \beta_a a_{it} + \boldsymbol{\beta_Z Z_{om}} - \varphi_{it}\right).$$

$$\pi_{it} = \frac{r_{it}(\Phi_t, k_{it}, \boldsymbol{W_{mt}}, \boldsymbol{Z_{om}}, \varphi_{it})}{\eta}. \quad (8)$$

Identification and Estimation The key challenge in productivity estimation is that capital is endogenous, as φ_{it} is unobserved and correlated with capital stock k_{it}. In the same spirit of Olley and Pakes (1996) and Levinsohn and Petrin (2003), we use the control function approach and a flexible function of the intermediate input M_{it} and capital to control for the productivity shock φ_{it}:

$$\ln r_{it} = \gamma_t + (1-\eta)\left(\boldsymbol{\beta_w W_{mt}} + \beta_a a_{it} + \boldsymbol{\beta_Z Z_{om}}\right) + h(k_{it}, M_{it}) + \epsilon_{it} \quad (9)$$

where $\gamma_t = (1-\eta)\ln\left(\frac{\eta}{\eta-1}\right) + \ln \Phi_t$ and $h(k_{it}, M_{it})$ is the control function. The identification assumption is that the remaining error term ϵ_{it} is a random shock and exogenous to the capital stock.

To estimate equation (9), we define a market as a city and two-digit industry combination. We include year dummies to control for γ_t and use average annual wage and the number of incumbents for each city-industry pair to proxy $\boldsymbol{W_{mt}}$.[35] The average annual wage measures the labor cost and the number of incumbents captures the intensity of competition, with a larger number of incumbents leading to more intense market competition and higher input prices.

There are several sets of controls in $\boldsymbol{Z_{om}}$. City dummies and industry dummies absorb the market-level cost shifters. Firm-ownership-type fixed effects capture cost structure differences across private firms, SOEs, and FIEs. For example, SOEs have access to cheaper credits and are subject to more favorable government supports than their private counterparts. Lastly, we incorporate a cubic function of firm's registered capital, as higher registered capital increases the probability of getting government procurement projects, obtaining external financing, and achieving better supplier matching, which could further decrease costs.

Our estimation proceeds in three steps: a) recover the elasticity of substitution $\hat{\eta}$ in the demand curve using Equation (8); b) back out $\hat{h}(\cdot)$ using Equation (9); c) with the estimated $\hat{h}(\cdot)$ and the elasticity of substitution $\hat{\eta}$ at hand, we estimate β_k and the parameters in the productivity dynamics $\alpha_0, ..., \alpha_3$ via the following equation:

$$
\begin{aligned}
\hat{h}_{it} =& \beta_k(1-\hat{\eta}) \ln k_{it} - \alpha_0(1-\hat{\eta}) + \alpha_1 \left(\hat{h}_{it-1} - \beta_k(1-\hat{\eta}) \ln k_{it-1}\right) \\
&-\alpha_2(1-\hat{\eta})^{-1} \left(\hat{h}_{it-1} - \beta_k(1-\hat{\eta}) \ln k_{it-1}\right)^2 \\
&+\alpha_3(1-\hat{\eta})^{-2} \left(\hat{h}_{it-1} - \beta_k(1-\hat{\eta}) \ln k_{it-1}\right)^3 - \xi_{it}(1-\hat{\eta}).
\end{aligned}
\tag{10}
$$

Finally, we back out the firm-level productivity estimates as

$$
\hat{\varphi}_{it} = -\frac{\hat{h}_{it}}{1-\hat{\eta}} + \hat{\beta}_k \ln k_{it}. \tag{11}
$$

[35] Average wage is calculated from National Tax Survey Database, as the ratio of total payroll over total employment, at each city and industry level from 2010 to 2015. We further impute the wage data in 2008, 2009, and 2016, assuming a constant average wage growth rate within each city-industry pair.

Estimation Results We use the revenue and profit information in the National Tax Survey Database to estimate the elasticity of substitution η and report the results in Table A12. The elasticity estimate remains robust across different specifications that account for heterogeneity across cities, industries, and time. Based on our preferred specification in Column (3), which focuses on the variation within a city-industry pair in a specific year, the profit margin is estimated to be 13.2%. The elasticity of substitution is hence 7.58, which is close to 6.38 estimated in Aw et al. (2011) in the Taiwanese electronics industry.

We next proceed to estimate $\hat{h}(\cdot)$. We use the gross tax and the prevailing value-added tax rate of 17% to impute the value of intermediate goods as $\textit{Intermediate Goods Value} = \textit{Revenue} - \frac{\textit{Tax}}{17\%}$.[36] The average ratio of value added to total revenue is around 20%.

We first estimate the revenue function as in Equation (9). With the estimates of $\hat{\eta}$ and $\hat{h}(\cdot)$ in hand, we proceed to estimate Equation (10) via NLS. We allow the structural parameters to vary across two-digit industries and estimate the productivity for each industry separately. However, for ease of illustration, we present in Table A13 the structural coefficient estimates that pool together all industries. The significantly negative β_k estimate suggests that firms with larger capital stocks enjoy lower marginal costs. Productivity is highly persistent over time, as reflected by the large α_1 estimate of 0.829. The estimates of α_2 and α_3 also reveal a nonlinear productivity evolution process.

In the final step, we back out the firm productivity via Equation (11). The lower panel of Table A13 summarizes the correlation coefficients of the estimated productivity with other variables in our data. Productivity is positively correlated with revenue, intermediate goods, capital stock, revenue-over-capital ratio as well as firm ages, consistent with our priors. Figure A9 plots productivity against firm age and revenue. Our estimated productivity increases in both firm age and revenue, in accordance with stylized facts in firm dynamics.

[36] Two major caveats are worth mentioning. First, firms pay other types of taxes in addition to the value-added tax. Second, the actual tax scheme of VAT is more complicated than a single value-added tax rate at 17%. However, we discuss later why this approach is unlikely to introduce a large bias to our main result.

Discussion *Labor as the Omitted Variable* The annual reports only contain firm employment information from 2013 to 2016, which is shorter than our sample period. In our baseline specification, we rely on a structural model to estimate productivity following Aw et al. (2011). However, omitting labor input may bias our estimates. We conduct three sets of robustness checks. First, we re-estimate productivity via OP and LP by incorporating labor information using the 2013-2016 sample, and find that the OP and LP productivity estimates are highly correlated with our baseline estimates.

Second, we replicate this exercise using data from the National Tax Survey Database, which is not the universe of firms but covers 80-90% of provincial GDPs. We first use the structural model to estimate productivity without labor, and then repeat the exercise by incorporating labor input in (9) and implement the estimation steps again. The two sets of productivity estimates are again highly correlated. This is perhaps not surprising given that capital stock, together with a rich set of fixed effects, appears to explain most of the variation in firm revenues in Equation (9).

Third, Brandt et al. (2021) also use the Annual Report Database and face the same empirical challenge as ours. We use the productivity measure proposed in their paper as a robustness check. Specifically, we assume firms are faced with the same wage rate within a city-industry-year cell. We could then calculate the relative firm productivity with respect to the weighted average of all firms in that cell, by only using the information on firm capital and value-added, and the results are robust. Appendix C.2 provides more details.

Measurement Errors in Intermediate Inputs Our constructed measure of intermediate inputs is subject to imputation errors. First, firms pay other types of taxes (e.g., corporate income tax), but we only observe the total amount of paid taxes. Second, the effective VAT rate may be different from the statutorily defined rate of 17%. We address these concerns using three different approaches. First, we use the revenue-over-capital ratio as another proxy for productivity and validate our empirical results. Second, we also provide OP estimates that do not rely on the intermediate goods measure. Third, the robustness check following Brandt et al. (2021) which does not use intermediate goods also helps address the concern.

Selection Bias We have paid more attention to simultaneity bias in productivity estimation, but leave selection bias untreated. The exit of less productive firms may create a sample selection problem. As we incorporate firm exit information in the OP estimates, selection bias issue has been largely addressed. We further flexibly controls for exit probability when estimating the revenue function (9), and the productivity estimates are robust.

C.2 Alternative Productivity Measures

Olley and Pakes (1996) and Levinsohn and Petrin (2003) We estimate productivity via Olley and Pakes (1996) (OP) and Levinsohn and Petrin (2003) (LP) with the sample period after 2013 for which we have employment information. OP uses investment as a proxy for productivity, while LP uses intermediate input instead. We also properly address the selection issue via OP. Therefore, both estimates help alleviate the concern that labor input is an omitted variable, since both of them incorporate employment information. OP estimates further address the issue of the measurement errors in intermediate goods, as we do not use the tax information at all. Productivity estimates via LP and OP are positively correlated with our baseline estimates. More importantly, the findings that entry deregulation increases productivity for both entrants and exiting firms compared with incumbents are robust under both alternative productivity measures, as shown in Table A6.

Brandt et al. (2021) We follow Brandt et al. (2021) for an alternative measure of productivity. Assume firm i has the production function $y_i = z_i k_i^{\alpha} l_i^{\beta}$, where y_i, k_i, l_i, and z_i denote the output, capital input, labor input, and productivity respectively, and $\alpha + \beta < 1$. As firm i is faced with wage ω, combining the first order condition with respect to the labor, and the production function, we could derive

$$y_i = z_i k_i^{\alpha} (\frac{\beta y_i}{\omega})^{\beta}.$$

Furthermore, we could express firm productivity z_i as a function of firm output, capital, and wage,

$$z_i = \beta^{-\beta} y_i^{1-\beta} k_i^{-\alpha} \omega^{\beta}. \tag{12}$$

If we further impose the assumption that the labor market is local, such that wage ω is homogeneous within a city-industry-year cell c, the mean productivity for each cell, $\bar{z}_c$, is averaged across firms in c weighted by the revenue share w_i.

$$\bar{z}_c = \sum_{i \in I_c} w_i \beta^{-\beta} y_i^{1-\beta} k_i^{-\alpha} \omega^{\beta} \tag{13}$$

where I_c denotes all firms in the city-industry-year cell c.

Combining Equations (12) and (13), the relative productivity of firm i with respect to the average productivity in cell c is

$$z_i = \frac{y_i^{1-\beta} k_i^{-\alpha}}{\sum\limits_{i \in I_c} w_i y_i^{1-\beta} k_i^{-\alpha}}$$

If we control for city-industry-year FE in the regression, this relative productivity measure is similar to our data-driven measure, revenue-over-capital ratio. We find our baseline results qualitatively similar to the results in Table A7.

D Theoretical Model

To understand the channels through which the entry reform shapes industrial dynamics and firm productivity, we present a simple two-period model adapted from Hopenhayn (1992). There is a continuum of heterogeneous firms in a perfectly competitive market. In the first period, potential entrants make entry decisions and choose their initial firm size subject to entry regulation. In the second period, incumbents optimize the static profits and decide whether to exit.

D.1 Incumbents

Incumbents produce a homogeneous product with a single input k (though the model can be easily extended to multiple inputs under the standard homotheticity assumption as noted by Hopenhayn (1992)). They differ only in productivity φ, which captures firms' efficiency level. The production function is $y = \varphi k^{\alpha}$ where $\alpha \in (0, 1)$. The input market is assumed to be perfect competitive and the unit cost of capital is r. The price of the output good is p and we assume p is *decreasing* with respect to the number of firms due to competition: more intense market competition is associated with lower output prices. An incumbent solves the following profit maximization problem,

$$\pi^*(\varphi, p) = \max_k \quad p\varphi k^{\alpha} - rk.$$

Incumbent firms also decide whether to exit or not. They incur a constant fixed cost f. Incumbents will exit the market if the static profit net the fixed cost falls below zero.

$$V(\varphi, p) = \max\{\pi^*(\varphi, p) - f, 0\}. \tag{14}$$

Incumbents' exit function is $\chi(\varphi, p) = \mathbb{1}(\pi^*(\varphi, p) < f)$. Let $\varphi^*(p)$ denote the exit threshold, such that incumbents with productivity φ below the threshold exit the market:

$$\chi(\varphi, p) = 1 \Leftrightarrow \varphi < \varphi^*(p).$$

D.2 Potential Entrants

There is a continuous measure N of potential entrants which are endowed with initial productivity φ drawn from distribution $G(\varphi)$. After observing productivity, potential entrants decide whether to enter the market or not. They face an entry cost, and their optimal input choice is subject to a minimum size requirement $k_e \geqslant \zeta$ (we use the subscript e to denote entrants). This is consistent with the institutional background that the entry regulation prior to the reform imposed minimum registered capital requirements upon registration. Moreover, ζ serves as a *screening* device: a higher ζ brings a larger operation cost and excludes entrants with low productivity draws from operating in the market.

In the first period, potential entrants make the entry decision by comparing the ex-ante value of operating a business and the entry cost. We assume that the entry cost is $c + \lambda\varphi$ with both c and λ being positive. c captures the level of entry cost, which includes administrative cost imposed by entry regulations to obtain pre-registration approvals and finance registered capital. λ denote the distortions embedded in the entry regulation. This is motivated by the *tollbooth* view of entry regulation (McChesney, 1987; Shleifer and Vishny, 1998) and the public choice theory (Stigler, 1971): less productive firms have larger incentives to seek lower entry barriers under heavy regulation through rents to the local government.

The key decision by potential entrants can be expressed as the follows.

$$
\begin{aligned}
V_e(\varphi, p) = \max\{\max_{k_e}\; & p\varphi k_e^{\alpha} - rk_e + \beta E_{\varphi'}[V(p,\varphi')|\varphi] - (c + \lambda\varphi), 0\}. \\
\text{s.t.} \quad & k_e(\varphi, p) \geqslant \zeta
\end{aligned}
\tag{15}
$$

We define the productivity threshold of being a successful entrant as $\varphi_e^*(p, \lambda, c, \zeta)$.

D.3 Entry Deregulation

The entry deregulation combines two major policy tools. The deregulation simplified or eliminated numerous procedures previously required to register a new business. It also eliminated the registered capital requirements. Without the loss of generality, we assume both λ and ζ are zero after the reform, and c is much smaller.

The reform naturally permits more firms to enter the market, which contributes to a more competitive market environment and drives output price down. As a result, the survival threshold of firm productivity shifts rightward. Therefore, the reform raises the exit rate of incumbents as well as the average productivity of exiting firms.

Proposition 1 *Entry deregulation increases firm entry and exit rates, i.e. market turnover rate, and the average productivity of exiting firms.*

Three underlying channels affect the average productivity of successful entrants. First, entry deregulation intensifies market competition and lowers output price p, thus shifting the survival-productivity threshold rightward for potential entrants. We call this the *market-competition* effect. Second, the elimination of distortive entry barriers reduces the disproportionately higher entry barriers faced by productive potential entrants, which changes the composition of successful entrants. We call this the *composition* effect. Third, entry deregulation removes the size restrictions for potential entrants. Less productive firms, which were previously bound by the size constraints, are more likely to become successful entrants after the reform. We call this the *weaker-screening* effect. Intuitively, the *market-competition* effect and the *composition* effect raise entrant productivity, while the *weaker-screening* effect counteracts the first two. We use c_0, λ_0 and ζ_0 to characterize the policy environment before the reform. We use c_1, $\lambda_1 = 0$ and $\zeta_1 = 0$ to characterize the policy environment after the reform, where $c_0 > c_1$. Let Φ_e denote the average productivity of successful entrants. We decompose the productivity difference before and after the reform into three corresponding terms:

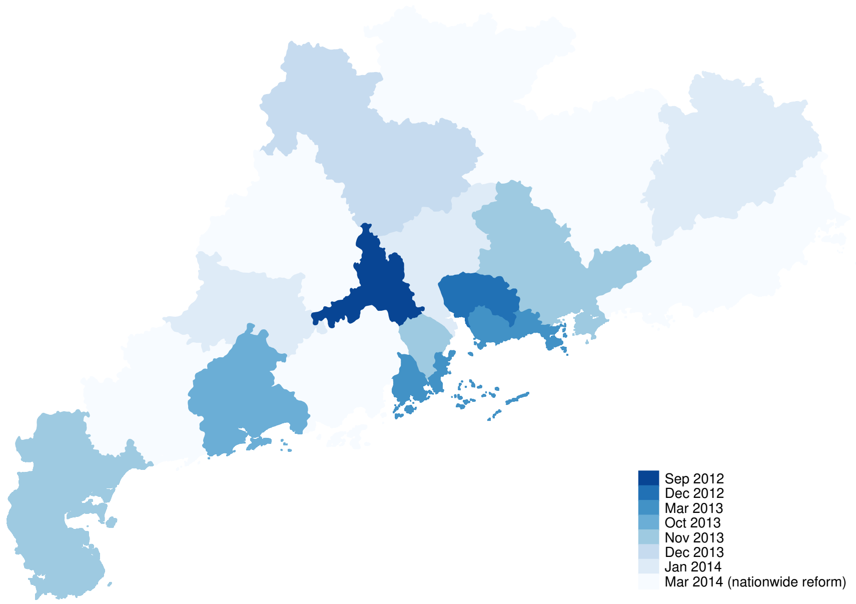
$$\begin{aligned}\Phi_e(p_1, \lambda_1, c_1, \zeta_1) - \Phi_e(p_0, \lambda_0, c_0, \zeta_0) = & \underbrace{\Phi_e(p_1, \lambda_1, c_1, \zeta_1) - \Phi_e(p_0, \lambda_1, c_1, \zeta_1)}_{\text{the } \textit{market-competition} effect \text{ (+)}} \\ & + \underbrace{\Phi_e(p_0, \lambda_1, c_1, \zeta_1) - \Phi_e(p_0, \lambda_0, c_1, \zeta_1)}_{\text{the } \textit{composition} effect \text{ (+)}} \\ & + \underbrace{\Phi_e(p_0, \lambda_0, c_1, \zeta_1) - \Phi_e(p_0, \lambda_0, c_0, \zeta_0)}_{\text{the } \textit{weaker-screening} effect \text{ (-)}}.\end{aligned}$$

We have the following proposition.

Proposition 2 *Entry deregulation increases entrants' average productivity if the market-competition effect and composition effect dominate the weaker-screening effect.*

Additional Figures

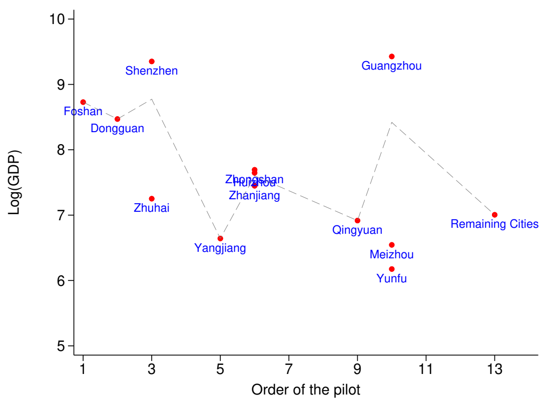
Figure A1: Pilot Rollout Map



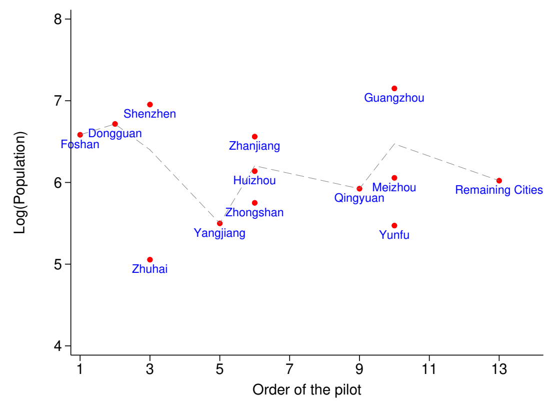
Notes: The figure shows the pilot rollout timing in the Guangdong city map. The darker blue color represents cities that implemented the pilot earlier.

Figure A2: Pilot Rollout Patterns

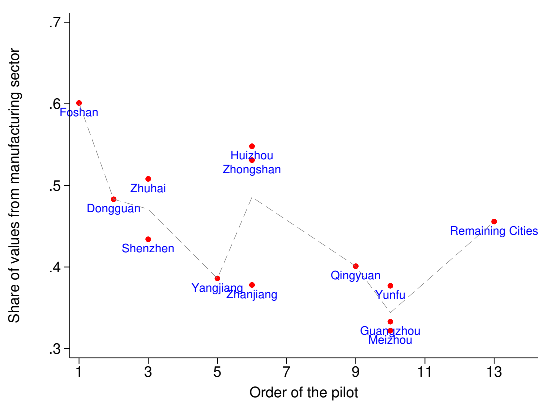
(a) Log(GDP)



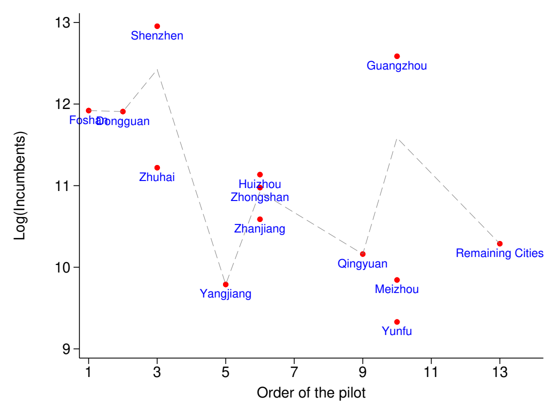
(b) Log(Population)



(c) Share of Values in Manufacturing

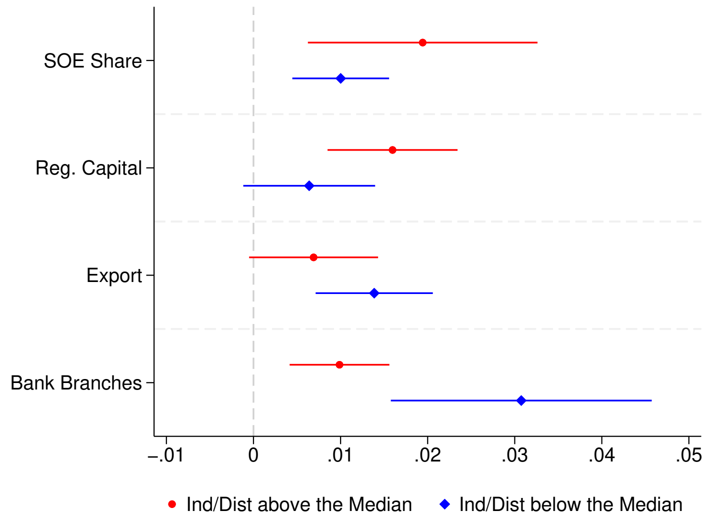


(d) Log(Incumbents)



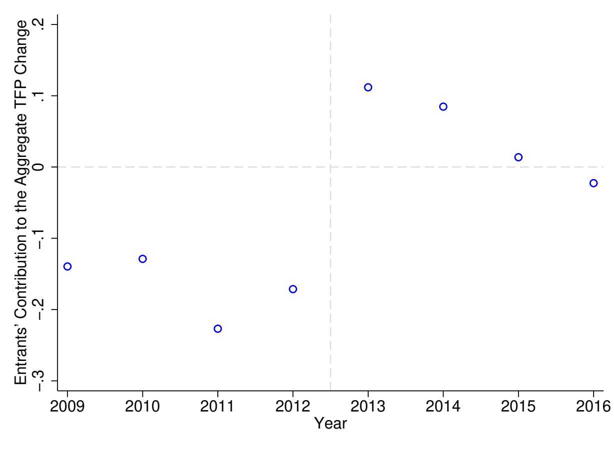
Notes: This figure shows the relationship between the key city-level economic statistics in 2011 and the order of the pilots. The dashed line connects the mean statistics among cities with the same pilot timing, and the averages are taken for all non-pilot cities. GDP, population, and share of values from manufacturing sector are from China City Statistical Yearbook 2011, and the numbers of incumbents at the end of 2011 are from the Business Registry Database.

Figure A3: Heterogeneity in Entry Deregulation's Effects on Entrant Productivity



Notes: This figure shows the reform's heterogeneous effect on entrants' productivity. We construct four variables: (1) the SOE share among all incumbents at the city-district level in 2008; (2) the ratio of registered capital to capital stock at the two-digit industry level in 2008; (3) the export share in total revenue at the two-digit industry level in 2010; (4) the log number of bank branches at the city-district level. Variables (1) and (3) are constructed using the National Tax Survey Database, variable (2) from the Business Registry Database, and variable (4) from the Chinese Commercial Bank Branch Database. We divide the sample into two according to the medians (industry-level medians or district-level medians) and run regressions following Equation (4). The dots denote the triple-difference coefficient estimates for entrants. The 95% confidence intervals are constructed from clustered standard errors at the city and industry level.

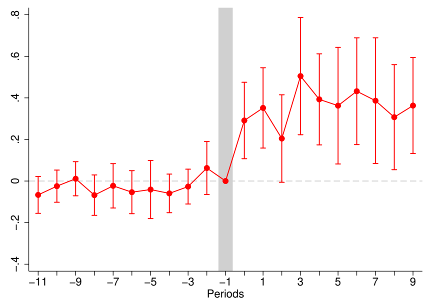
Figure A4: Productivity Decomposition: Entrants' Contribution to Aggregate TFP Change



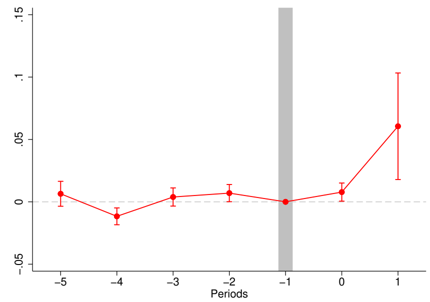
Notes: This figure plots entrants' contribution to the aggregate TFP change. We follow Foster et al. (2008) and Griliches and Regev (1995) for productivity decomposition, as shown in Equation (5).

Figure A5: The Effect of Entry Deregulation on Firm Entry and Exit: Dynamic Difference-in-Differences Estimators

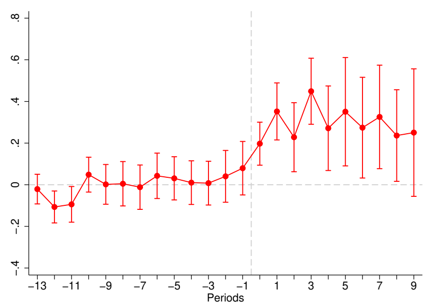
(a) Entry: SA Method



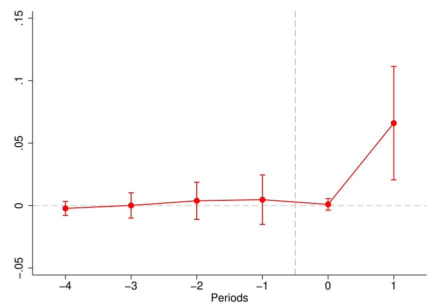
(b) Exit: SA Method



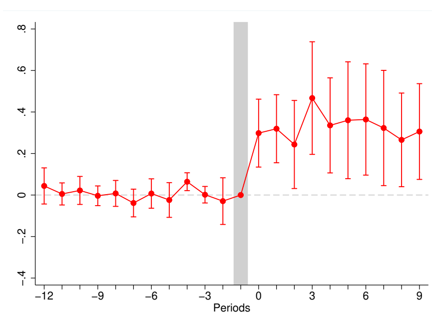
(c) Entry: BJS Method



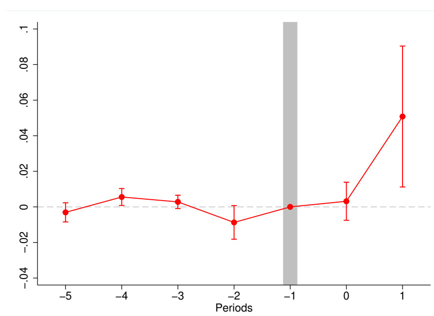
(d) Exit: BJS Method



(e) Entry: DD Method

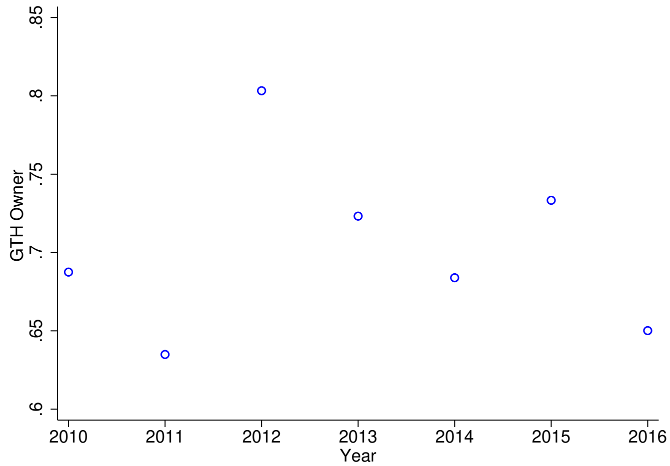


(f) Exit: DD Method



Notes: This figure shows the event study coefficient estimates for firm entry and exit following Sun and Abraham (2021) (labeled as the "SA Method"), Borusyak et al. (2024) (labeled as the "BJS Method"), and de Chaisemartin and d'Haultfoeuille (2024) (labeled as the "DD Method"). Panel (a), (c), and (e) reports results on firm entry (measured by log number of new firms) at the city, two-digit industry, and month level for the manufacturing sector from Jan 2009 to Feb 2014. The horizontal axis denotes months before and after the reform. Panel (b), (d), and (f) report results for firm exit, where the dependent variable is whether firm i exits (stops submitting annual reports) in year $t + 1$. The sample is from 2009 to 2014. The horizontal axis denotes years before and after the reform. Fourteen and more periods before the treatment time serve as the reference group in order to rule out the possible contamination of the treatment anticipation in Panel (c), while five and more periods before the treatment time serve as the reference group in Panel (d). The 95% confidence intervals are constructed from clustered standard errors at the city and industry level.

Figure A6: The Ratio of Entrepreneurs as Previous *Getihu* Owners

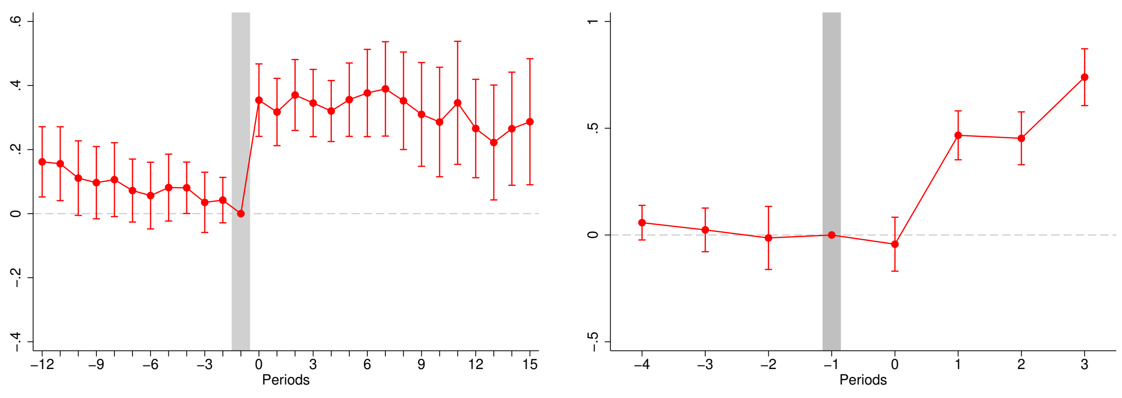


Notes: This figure shows the ratio of entrepreneurs with their first business being a *getihu* (i.e., a mom-and-pop operation or a small family-owned business), among all the entrepreneurs who had owned other businesses (i.e., serial entrepreneurs). It is calculated from 2018 ESIEC survey data. The horizontal axis denotes the entry year of firms.

Figure A7: The Effect of Entry Deregulation on Firm Entry and Productivity: Retail, Wholesale, and Service Sectors

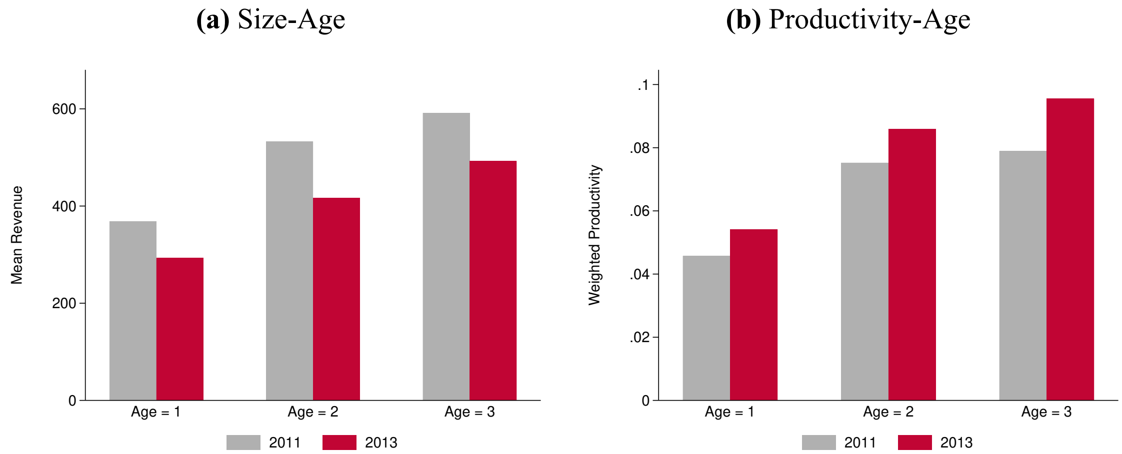
(a) Entry

(b) log(Revenue-Capital Ratio) for Entrants



Notes: This figure shows the event study coefficient estimates for firm entry and the log of the revenue-over-capital ratio for entrants in the retail, wholesale, and service sectors. Panel (a) reports results on firm entry (measured by log number of new firms) at the city, two-digit industry, and month level for the the retail, wholesale, and service sectors from Jan 2009 to Dec 2016. The horizontal axis denotes months before and after the reform. Panel (b) presents event study coefficient estimates on log of the revenue-over-capital ratio for entrants. Entrants are defined as those at age one. The observation is at the firm-year level from 2009 to 2015. The horizontal axis denotes years before and after the reform. The 95% confidence intervals are constructed from clustered standard errors at the city and industry level.

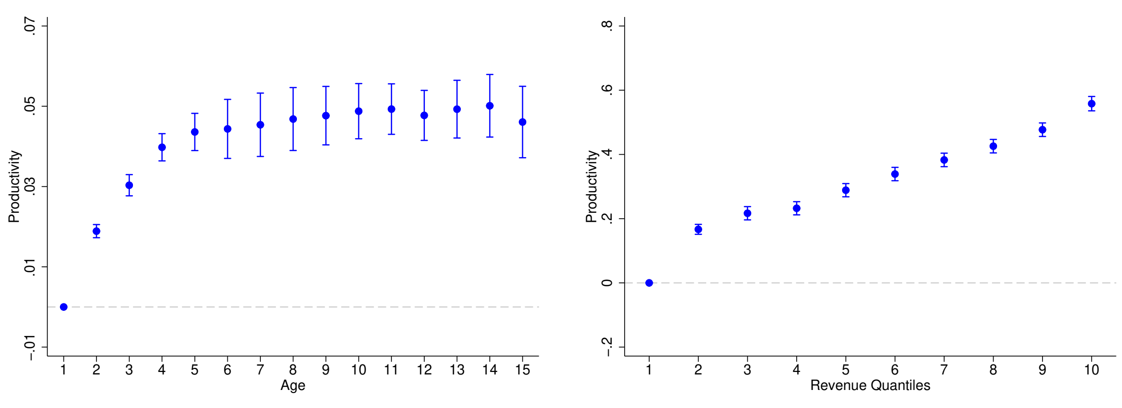
Figure A8: Entrants' Size and Productivity across Age Groups



Notes: This figure shows the average revenue and revenue-weighted average productivity for firms which entered in 2011 and 2013 when their ages were one, two, and three. We residualize firm productivity on city-by-industry fixed effects, and then weight each firm using its revenue share in the city-industry-cohort-age cell in Panel (b). We only keep firms which do not miss annual reports to make these two entry cohorts comparable. We keep cities that have implemented the reform before the end of 2013, so the cohort 2013 entered after the reform, while the cohort 2011 entered before the reform.

Figure A9: Productivity, Age, and Revenue: All Firms

(a) Log(Productivity)-Age **(b)** Log(Productivity)-Revenue Quantiles



Notes: This figure shows the relationship of productivity and firm age/revenue. The observation is at the firm and year level from 2009 to 2016. Panel (a) shows the result for firm productivity and age, with firms of age one serving as the base group. Panel (b) shows the result for firm productivity and revenue. Firm revenue is categorized into 10 groups according to deciles, and the smallest group serve as the base group. 95% confidence intervals are constructed from robust standard errors clustered at the city-industry level.

Additional Tables

Table A1: Other Important Policies

Policy	Time initiated	Details
Financial Reform Pilot Zones	July 25, 2012	to boost financial industry and facilitate the financial integration of cities in Pearl River Delta
Guangdong Pilot Free Trade Zone	April 21, 2015	to lift restrictions on the service provision between Hong Kong, Macau and Guangdong to further open up the financial services sector
Value-added Tax Reform	July 1, 2017	to reduce the value-added tax rates
Guangdong-Hong Kong-Macao Greater Bay Area	July 1, 2017	to economically integrate nine cities in Pearl River Delta with Hong Kong and Macau to form a world-class business hub
Rural Revitalization Planning	September 26, 2018	to revitalize the economy of rural areas

Notes: This table summarizes the timing and the goals of other important policies in Guangdong between 2010 and 2020. The detailed reference can be accessed through the hyperlink to the name of each policy.

Table A2: The Effect of Entry Deregulation on Entrants' Productivity: Heterogeneity

	Log(Productivity)			
	RegCost		Owner Type	
	High	Low	Private	SOE/FIE
Entrants	-0.056***	-0.048***	-0.051***	-0.053***
	(0.003)	(0.003)	(0.002)	(0.005)
Treatment × Entrants	0.013***	0.007***	0.010***	0.001
	(0.004)	(0.003)	(0.002)	(0.011)
Observations	462,405	579,980	956,656	85,729
Adjusted R^2	0.113	0.105	0.094	0.119
City-Industry-Owner FE	✓	✓	✓	✓
City-Year FE	✓	✓	✓	✓
Industry-Owner-Year FE	✓	✓	✓	✓

Notes: This table examines heterogeneity in deregulation's impact on entrants' productivity in subsamples. The observation is at firm-year level. The dependent variable is structurally estimated productivity (log-valued) following Aw et al. (2011) and Peters et al. (2017). "Treatment" is a dummy variable that takes value one for years during and post a city's reform implementation date and zero otherwise. The dummy variable "Entrants" flags firms at age one in year t. Columns (1)-(2) divide the sample based on whether an industry's percentage reduction in registration costs is above (High) or below (Low) the median. Columns (3)-(4) divide the sample into private firms and SOEs/FIEs. Standard errors are clustered at the city-industry level. $^{*}p<0.10$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A3: The Effect of Entry Deregulation on Firm Entry, Excluding Shell Companies

	Log(Entry)			Log(Entry Rate)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Ever Filed Annual Report						
Treatment	0.234***	0.277***	0.261***	0.238***	0.267***	0.252***
	(0.042)	(0.044)	(0.036)	(0.040)	(0.043)	(0.036)
Observations	54,048	54,048	54,048	53,579	53,579	53,579
Adjusted R^2	0.945	0.947	0.954	0.666	0.677	0.718
Panel B: Ever Reported Positive Capital						
Treatment	0.222***	0.270***	0.248***	0.225***	0.260***	0.239***
	(0.047)	(0.048)	(0.036)	(0.045)	(0.047)	(0.036)
Observations	53,952	53,952	53,952	53,512	53,512	53,512
Adjusted R^2	0.944	0.946	0.954	0.690	0.701	0.739
City-Industry, City-Year FE	✓	✓	✓	✓	✓	✓
Month-of-Sample FE	✓	✓	✓	✓	✓	✓
City-Calendar-Month FE		✓	✓		✓	✓
Industry-Month-of-Sample FE			✓			✓

Notes: This table examines the robustness of the results in Table 1 with respect to the concerns about the shell companies. Same variable definition, model specifications, and sample size as in Table 1. Panel A only includes new entrants that have ever filed annual reports. Panel B limits to new entrants that have ever reported positive capital stock. All regressions are weighted by number of firms in each city-industry pair at the beginning of the sample period. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table A4: The Effect of Entry Deregulation on Firm Exit: Alternative Measure

	Exit Dummy ($\times$ 100)			
	All Firms		Exclude Post-Reform Entrants	
	(1)	(2)	(3)	(4)
Treatment	1.019***	1.059***	0.820**	0.837**
	(0.365)	(0.385)	(0.383)	(0.387)
Log(Registered Capital)		-1.080***		-1.163***
		(0.036)		(0.034)
Observations	3,384,893	3,288,350	2,728,065	2,643,663
Adjusted R^2	0.012	0.019	0.012	0.021
City-Industry-Owner FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Notes: This table shows the results for the deregulation's impacts on firm exit using firm revocation/cancellation as an alternative exit measure following Equation (3). We use firm-year observations in 2009-2018 which allows us to control for firm attributes. The coefficients are scaled by 100 for ease of interpretation. The average exit rate was 4.8% prior to the reform. "Treatment" is a dummy variable, which takes the value of one if the year is after the reform in that city, and zero prior to the reform. Columns (1)-(2) include all firms, while Columns (3)-(4) exclude firms that entered after the reform. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table A5: The Effect of Entry Deregulation on Firm Size and Productivity: Robustness Check

	Log(Capital)		Log(Revenue-Capital Ratio)	
	(1)	(2)	(3)	(4)
Entrants	-1.081***	-0.954***	0.047***	0.021
	(0.044)	(0.028)	(0.018)	(0.014)
Exiting Firms	-0.822***	-0.550***	-0.085***	-0.134***
	(0.033)	(0.024)	(0.031)	(0.028)
Treatment × Entrants	-0.356***	-0.428***	0.207***	0.237***
	(0.043)	(0.033)	(0.022)	(0.021)
Treatment × Exiting Firms	0.221***	0.030	0.124***	0.160***
	(0.076)	(0.056)	(0.036)	(0.032)
Observations	1,042,425	1,042,385	1,042,425	1,042,385
Adjusted R^2	0.132	0.312	0.067	0.087
City-Industry FE	✓		✓	
City-Industry-Owner FE		✓		✓
City-Year FE	✓	✓	✓	✓
Industry-Year FE	✓		✓	
Industry-Owner-Year FE		✓		✓

Notes: This table shows the baseline results for deregulation's impact on firm productivity as specified in Equation (4). Same variable definition, model specifications, and sample size as in Table 4. The dependent variable is structurally estimated productivity (log-valued) following Aw et al. (2011) and Peters et al. (2017) in Columns (1)-(3) and log of the revenue-over-capital ratio in Columns (4)-(6). Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table A6: The Effect of Entry Deregulation on Firm Productivity: LP and OP

	Log(Productivity): LP			Log(Productivity): OP		
	(1)	(2)	(3)	(4)	(5)	(6)
Entrants	-0.404***	-0.370***	-0.371***	-0.177***	-0.195***	-0.196***
	(0.017)	(0.017)	(0.017)	(0.036)	(0.034)	(0.034)
Exiting Firms	-0.428***	-0.420***	-0.421***	-0.023	-0.030	-0.031
	(0.059)	(0.057)	(0.057)	(0.047)	(0.048)	(0.048)
Treatment × Entrants	0.057**	0.071***	0.072***	0.098***	0.101***	0.102***
	(0.024)	(0.020)	(0.020)	(0.034)	(0.032)	(0.032)
Treatment × Exiting Firms	0.209***	0.208***	0.209***	0.028	0.032	0.033
	(0.060)	(0.058)	(0.059)	(0.045)	(0.046)	(0.046)
Observations	530,260	530,231	530,227	529,527	529,499	529,495
Adjusted R^2	0.050	0.069	0.069	0.051	0.056	0.056
City-Industry FE	✓			✓		
City-Industry-Owner FE		✓	✓		✓	✓
City-Year FE	✓	✓	✓	✓	✓	✓
Industry-Year FE	✓	✓		✓	✓	
Industry-Owner-Year FE			✓			✓

Notes: This table shows the deregulation's impact on firm productivity estimated via the LP and OP methods, and the empirical model is specified in Equation (4). The observation is at firm-year level, and the sample period is 2013-2015. Shorter sample period is the main reason why the number of observations is much smaller than that of Table 4. The dependent variable is productivity estimated following Levinsohn and Petrin (2003) in Columns (1)-(3) and productivity estimated following Olley and Pakes (1996) in Columns (4)-(6). "Treatment" is a dummy variable that takes value one in years during and after a city's reform implementation date and zero otherwise. The dummy variable "Entrants" flags firms at age one in year t and "Exiting Firms" takes value one if firm i is in its last year of operation and with age greater than one in year t. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table A7: The Effect of Entry Deregulation on Firm Productivity: Brandt et al. (2021)

	Log(Productivity)		
	(1)	(2)	(3)
Entrants	0.011	0.000	-0.002
	(0.009)	(0.009)	(0.008)
Exiting Firms	-0.065***	-0.070***	-0.071***
	(0.014)	(0.013)	(0.013)
Treatment × Entrants	0.059***	0.063***	0.068***
	(0.012)	(0.012)	(0.011)
Treatment × Exiting Firms	0.025	0.030	0.032
	(0.025)	(0.025)	(0.024)
Observations	1,129,338	1,129,309	1,129,305
Adjusted R^2	0.495	0.500	0.501
City-Industry FE	✓		
City-Industry-Owner FE		✓	✓
City-Year FE	✓	✓	✓
Industry-Year FE	✓	✓	
Industry-Owner-Year FE			✓

Notes: This table shows the deregulation's impact on firm productivity estimated following Brandt et al. (2021), and the empirical model is specified in Equation (4). The observation is at the firm-year level, and the sample period is 2009-2015. The number of observations is larger than that of Table 4 because missing key variables is not an issue to estimate productivity here. The dependent variable is log productivity estimated following Brandt et al. (2021). "Treatment" is a dummy variable that takes value one in years during and after a city's reform implementation date and zero otherwise. The dummy variable "Entrants" flags firms at age one in year t and "Exiting Firms" takes value one if firm i is in its last year of operation and with age greater than one in year t. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table A8: Number of Entrants and Incumbents

Year	Entrants	Incumbents	Total Firms
Panel A: Sample used for Table 3			
2009	13,012	77,304	90,316
2010	21,086	104,892	125,978
2011	34,166	160,432	194,598
2012	35,688	180,170	215,858
2013	24,256	119,638	143,894
2014	43,468	252,897	296,365
2015	51,863	273,912	325,775
Total Obs.	223,539	1,169,245	1,392,784
Panel B: Sample used for Table 4			
2009	10,241	61,553	71,794
2010	17,784	87,287	105,071
2011	28,638	131,554	160,192
2012	29,768	149,081	178,849
2013	16,377	78,450	94,827
2014	28,513	174,003	202,516
2015	34,176	194,999	229,175
Total Obs.	165,497	876,927	1,042,424

Notes: This table shows the numbers of entrants and incumbents as well as total numbers of firms for samples that are used in Tables 3 and 4. The numbers of firms in Table 4 are smaller mainly because of missing values in some key variables for productivity estimation.

Table A9: The Effect of Entry Deregulation on Firm Entry: Robustness Check

	Entry				
	No DF	Pre 2014M3	No Anticip.	Jiangsu	Shandong
	(1)	(2)	(3)	(4)	(5)
Treatment	0.367***	0.299***	0.191***	0.261***	0.265***
	(0.041)	(0.043)	(0.047)	(0.044)	(0.042)
Observations	49,152	35,092	32,828	36,546	48,161
Adjusted R^2	0.950	0.947	0.948	0.928	0.933
City-Industry, City-Year FE	✓	✓	✓	✓	✓
City-Calendar-Month FE	✓	✓	✓	✓	✓
Industry-Month-of-Sample FE	✓	✓	✓	✓	✓

Notes: This table shows the robustness checks for the impacts on firm entry following Equation (1). The observation is at the city, two-digit manufacturing industry, and month level. The dependent variable is the log number of newly registered firms and we calculate the number of entrants using firm registration information. "Treatment" is a dummy variable that takes value one for months during and post a city's reform implementation date and zero otherwise. Column (1) excludes Foshan and Dongguan as they started the pilot program much earlier than the rest, and thus reform measures might be different from later pilots. Column (2) replaces the sample period (Jan 2009 - Dec 2016) with the pilot sample period from Jan 2009 to February 2014 such that the non-pilot cities were never treated in this time window. Column (3) further drops four periods before the treatment to eliminate the anticipation effect. Columns (4)-(5) keep our sample period up to February 2014, and replace 9 non-pilot cities with 10 cities in Jiangsu province (excluding 3 cities with pilots) and 17 cities in Shandong province respectively, in order to address the concern of geographical spillovers. We include the same set of fixed effects as in Table 1. All regressions are weighted by number of firms in each city-industry pair at the beginning of the sample period. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table A10: The Effect of Entry Deregulation on *De Novo* Entry

Panel A: Spin-off			
	Entry Excluding Firms by Serial Entrepreneurs		
	Any Individual Investor		Largest
Treatment	0.204***	0.235***	0.233***
	(0.048)	(0.044)	(0.044)
Observations	54,240	54,240	54,240
Adjusted R^2	0.898	0.934	0.936
City-Industry, City-Year, Month-of-Sample FE	✓	✓	✓
City-Calendar-Month FE		✓	✓
Industry-Month-of-Sample FE		✓	✓
Panel B: Reclassification			
	Getihu Entry		
Treatment	0.276***	0.316***	0.315***
	(0.056)	(0.069)	(0.058)
Observations	52,416	52,416	52,416
Adjusted R^2	0.905	0.908	0.930
City-Industry, City-Year, Month-of-Sample FE	✓	✓	✓
City-Calendar-Month FE		✓	✓
Industry-Month-of-Sample FE			✓
Panel C: Relocation			
	Entry from		
	Same City, Different District	Other City	Other Province
Treatment	0.024**	0.005	0.006
	(0.011)	(0.015)	(0.016)
Mean of Dep Var	0.689	0.495	0.324
Observations	24,011	24,011	24,011
Adjusted R^2	0.500	0.729	0.690
City-Industry, City-Year FE	✓	✓	✓
City-Calendar-Month FE	✓	✓	✓
Industry-Month-of-Sample FE	✓	✓	✓

Notes: This table examines whether the regulation led to *de novo* entry and evaluates three alternative explanations: the spin-off of existing firms, the reclassification from existing informal businesses (*getihu*, or mom-and-pop operations), and the spatial relocation of existing firms from other regions. "Treatment" is a dummy variable that takes value one for months during and post a city's reform implementation date and zero otherwise. Panel A excludes new firms whose (largest) individual investor is a serial entrepreneur. Panel B uses log number of newly registered *getihu* (instead of firms as in Table 1). The dependent variables in Panel C are the probability that the entrepreneur is from the same city but a different district as the firm location, or from a different city or province. Panel C has fewer observations due to missing data in some city-industry pairs. Using a balanced panel at city and industry level doesn't change the results qualitatively. All regressions are weighted by number of firms in each city-industry pair at the beginning of the sample period. Standard errors are clustered at the city-industry level. $^{*}p<0.10$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A11: The Effect of Entry Deregulation on Incumbents' Productivity

	Log(Productivity)		
	After 2010	One Missing	Three Missings
	(1)	(2)	(3)
Treatment	-0.004*	0.002	-0.002
	(0.002)	(0.003)	(0.002)
Observations	306,051	67,966	247,213
Adjusted R^2	0.687	0.632	0.674
Size Quintiles	✓	✓	✓
Firm FE	✓	✓	✓
Year FE	✓	✓	✓
Age FE	✓	✓	✓

Notes: This table shows the results for the impacts on incumbents' productivity. The observation is at the firm and year level, with the sample period of 2010 to 2015. The dependent variable is the structurally estimated productivity (log-valued). Column (1) keeps all firms which submitted annual report in 2010, Column (2) keeps all firms which submitted annual report in 2010 and only missed one annual report afterwards, and Column (3) keeps all firms which submitted annual report in 2010 and missed at most three annual reports afterwards. "Treatment" is a dummy variable that takes value one in years during and after a city's reform implementation date and zero otherwise. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table A12: Estimation of the Elasticity of Substitution

	Profit		
	(1)	(2)	(3)
Revenue	0.139***	0.141***	0.141***
	(0.001)	(0.001)	(0.001)
Observations	125,083	125,075	125,075
Adjusted R^2	0.278	0.305	0.290
City-Industry FE		✓	
Year FE		✓	
City-Industry-Year FE			✓

Notes: This table shows the estimation results of the elasticity of substitution. We use the sample from National Tax Survey of 2010 to 2015 in Guangdong manufacturing industries. The dependent variable is firm profit, and the key independent variable is firm revenue. Standard errors are reported. $^{*}p<0.10$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A13: Productivity Estimation

Panel A: Parameter Estimates			
		Estimates	S.E.
Cost Function	β_k	-0.199***	0.001
Productivity Evolution	α_0	0.018***	0.001
	α_1	0.829***	0.002
	α_2	0.154***	0.001
	α_3	-0.010***	0.001
Panel B: Correlation with Productivity			
		Correlation Coefficients	
Log(Revenue)		0.815	
log(Capital)		0.436	
Log(Intermediate Goods)		0.839	
Log(Revenue-Capital Ratio)		0.084	
Age		0.175	

Notes: This table shows the productivity estimation results following Appendix C.1. Panel A shows the parameter estimates, while Panel B shows the correlation of estimated productivity with some key variables. β_k denotes the cost elasticity with respect to the capital input in the cost function (6). α_0, α_1, α_2 and α_3 represent the coefficients of constant term, linear term, quadratic term, and cubic term in the productivity evolution equation (10). *p<0.10; **p<0.05; ***p<0.01.