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

Panle Jia Barwick Luming Chen Shanjun Li Xiaobo Zhang

January 1, 2022

Click Here for the Latest Version

Abstract

Entry regulation is ubiquitous across countries, but the empirical evidence of its impacts on firm dynamics and productivity is limited. Leveraging the staggered implementation of a pilot program of entry deregulation in Guangdong, China, this study examines the effects of entry deregulation on firm entry, exit, size distribution, and productivity in the manufacturing sector. Based on detailed administrative data on firm registrations and annual reports, and field survey data, our analysis shows that the reform has increased firm entry by 25% and firm exit by 8.7%. The productivity of post-reform entrants is 1.3% higher than the productivity of pre-reform entrants, likely due to the easing of the financial constraints and more intense market competition. A back-of-the-envelope calculation suggests that the nationwide reform following the pilot program would have increased employment by at least 0.7 million and generated more than ¥35 billion of value added per year.

Keywords: Deregulation, entry cost, productivity, market turnover

JEL Codes: L10, L50, L60, O40

*Barwick: Department of Economics, Cornell University and NBER, email: panle.barwick@cornell.edu; Chen: Department of Economics, Cornell University, email: lc929@cornell.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 our discussants, Mark Roberts and Mo Xiao, and seminar participants at CES Annual Meeting (Virtual), Cornell University, NBER China Meeting, NEUDC, and Peking University for helpful comments.

1 Introduction

New firms are a key source of economic growth (Foster et al., 2001; Brandt et al., 2012; Asturias et al., 2021), but not all entrants are productive. Some may be unproductive and even destructive (Baumol, 1990). Therefore, entry regulation has been introduced across the world to screen potential entrants and steer them toward productive activities. However, entry regulation could yield unintended consequences. The weakening market competition may prevent productive entrepreneurs from setting up their businesses and inefficient incumbents from exiting the market. In addition, entry regulations could lead to rent-seeking behavior to the detriment of the society (Djankov et al., 2002). Despite the long-standing policy interests in entry regulation, there is a lack of comprehensive evaluation of the broad impacts of entry regulation on firm dynamics and productivity. The key empirical challenge in the literature is that variation in entry regulation could respond to changing economic conditions or might be confounded by unobserved institutional factors.

Leveraging the staggered roll-out of China's Business Registration Reform, this paper studies the impacts of entry deregulation on firm entry, exit, size distribution, and productivity. The Business Registration Reform was launched by the Chinese government in 2014 with a goal of encouraging entrepreneurial activities. The reform substantially reduced the cost of firm entry by simplifying firm registration procedures and relaxing registered capital requirement.[1] Prior to scaling up the reform at the national level, a pilot program in Guangdong province was rolled out in a staggered fashion across cities.[2] Our analysis exploits this pilot program and takes advantage of the temporal and regional variations of entry deregulation. We utilize administrative data on universal firm registration records and annual reports, as well as primarily collected enterprise survey data, to evaluate the impacts of entry deregulation on a host of measures related to firm dynamics and productivity. Our baseline analysis focuses on the manufacturing sector, though key results also hold in the service sector.

We start with a stylized theoretical model to illustrate how entry deregulation could shape industrial dynamics and firm productivity. We incorporate the heterogeneity among potential entrants to explore the efficiency implication of the entry reform. Our theoretical model generate three key predictions. First, entry deregulation would boost both firm entry and exit, leading to a higher market turnover rate. Second, entry deregulation reduces the average size of entrants, but raises

[1] Relative to deregulation in other countries, China's reform is unprecedented in both the geographical coverage and the intensity. As a result of this reform, China's ranking for the World Bank's "starting a business" index in the *Doing Business Report* leaped from the 150th among 183 countries in 2011 to the 27th in 2020.

[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 by the end of 2019. It also features the largest provincial GDP among all provinces. Its GDP exceeds that of Spain and Australia, the 13th and 14th largest economy in the world in 2019, respectively. Its major cities include Shenzhen and Guangzhou, among the wealthiest and economically most advanced cities in China.

the average size of exiters as the survival size-threshold is shifted rightward. Third, the impact on entrants' productivity depends crucially on the magnitude of competing forces and is hence ambiguous. We then take these testable predictions to the data.

Our empirical strategy leverages the staggered implementation of the reform across 21 cities in Guangdong province. The key threat to identification is the potentially endogenous timing of the program in that the roll-out across cities might be correlated with time-varying unobserved shocks to the variables of interests. For example, if the government had determined the order of the roll-out in response to economic conditions (e.g., the slowdown of entrepreneurial activity in a given city relative to other cities), this would lead to the endogeneity in the policy variable and violate the parallel trend assumption. We address this concern in three ways. First, as firm entry is measured at the monthly level, we control for unobserved shocks at the annual level and exploit the month-to-month variation of the program implementation for each city within a year. Therefore, our identification relies on a weaker assumption that the exact month (rather than the year) of program implementation is exogenous. Second, as firm exit, size and productivity are measured annually, we adopt a triple difference strategy to investigate the differential impacts across industries, or between entrants/exiters and continuing incumbents. This strategy allows us to control for time-varying and city-specific shocks using city by year fixed effects. Third, throughout the paper, we use an event study design on various outcomes and the results do not suggest any significant trend differences across cities in different treatment waves before the reform.

Our empirical analysis proceeds in four steps to test model predictions about firm entry and exit, size distribution, productivity, and examine aggregate implications of entry deregulation. We first examine the impacts on firm entry and exit using firm registration records and a difference-in-differences (DID) framework. Our analysis finds that entry deregulation increased firm entry by 25%: the effect persists over time and is much larger than previous estimates in other countries (Kaplan et al., 2011; Branstetter et al., 2013). A host of robustness checks suggest that the large increase in firm entry was not driven by the proliferation of "shell" firms, the spin-off of existing incumbents, reclassification of informal businesses, or relocation of firms from other areas. Instead, most of the newly registered firms represented *de novo* initiatives by entrepreneurs. There was large heterogeneity in the impact on the entry rates across firm types and industries: private firms and industries with higher degrees of entry deregulation experienced a greater increase in new entrants. Accompanied with the higher entry rate was an increase of the exit rate by 8.7%, mainly in more deregulated industries. The rise in the exit rate was likely due to the intense competition and lower profit margins from massive firm entry, and more entry and exit together lead to a higher market turnover rate after the reform.

We next evaluate the effect of entry deregulation on firm size distribution based on information from firm annual reports. Our analysis shows that new entrants became smaller in size after the reform, as lower entry barriers allowed smaller firms to enter the market. In contrast, exiting firms became larger after the reform, consistent with our model prediction and the theoretical insight of Hopenhayn (1992). Entry deregulation led to more intense market competition, which in turn drove the marginal incumbents out of the market, and shifted the survival size-threshold rightward.

Another key outcome we examine is firm productivity. Our baseline productivity estimation follows Aw et al. (2011). Alternative productivity measures by Olley and Pakes (1996), Levinsohn and Petrin (2003), as well as the revenue-over-capital ratio (or asset turnover rates) are used in the robustness analysis. As illustrated in our theory model, while entry deregulation could lead to weaker screening and allow unproductive firms to enter, there are two countervailing forces. First, the reform could have introduced more intense competition and driven down the profit margins, preventing the less productive potential entrants from entering the market (i.e., *the market competition effect*). Second, entry deregulation could have changed the composition of new entrants and allowed productive yet financial constrained potential entrepreneurs to enter, who would otherwise be precluded due to their limited resources (i.e., *the composition effect*). Our analysis shows that the productivity of new entrants improved by 1.3% after the reform, suggesting that these two forces outweighed the weaker screening effect from laxer regulations.

We conduct additional analysis to disentangle potential channels underlying the observed improvement in entrants' productivity. For *the market competition effect*, we document that firms that entered *immediately before* the reform also exhibited productivity improvement, though the magnitude is smaller compared with entrants after the reform. These firms were still faced with more stringent regulation at the time of entry, but experienced more intense market competition together with the the new entrants post-reform, thus their productivity improvement is likely due to stronger selection as indicated by *the market competition effect*. Regarding the *the composition effect*, we show that the easing of financial constraints is the main underlying mechanism using three pieces of evidence. First, survey evidence suggests that fewer entrepreneurs borrowed from friends/relatives or obtained loans from financial institutions to fund the start-up costs of new businesses after the reform. Second, entry deregulation opens up opportunities to less educated and younger entrepreneurs, who were more likely to be financially constrained. Moreover, individual shareholders (natural persons), who have fewer financial resources than corporate shareholders (legal persons), played a greater role in the formation of new firms after the reform. Third, the increase in productivity was greater for more deregulated industries, private firms, industries requiring higher registered capital before the reform, and markets with more limited access to finance. Therefore, the productivity gain for entrants stems disproportionately from sub-samples that experience a larger degree of relaxation of financial constraints.

Lastly, we quantify the contribution of entry deregulation to the aggregate economy. Following Foster et al. (2008), we conduct a productivity decomposition to measure the contribution of entrants, exiters, and continuing incumbents to the aggregate productivity growth. We find that entrants have much larger contribution to the annual aggregate productivity growth after entry deregulation, consistent with the insights of Asturias et al. (2021). A back-of-the-envelope calculation suggests that entry deregulation as implemented at the national level after the pilot run in Guangdong would have increased annual employment by 0.7 million and value-added by ¥35 billion nationwide in the manufacturing sector alone.

Our paper contributes to the following three strands of literature. First, our paper directly speaks to the literature on the impact of entry deregulation on entrepreneurial activities (Djankov et al., 2002; Klapper et al., 2006; Kaplan et al., 2011; Bruhn, 2011; Branstetter et al., 2013).[3] The pioneering work of Djankov et al. (2002) twenty years ago motivated a wave of reforms to simplify entry regulation around the world, and these reforms opened up the opportunity to explore the nature and the consequences of entry regulation. The literature generally finds that entry regulation entails bureaucratic hurdles for new entrants and hamper entrepreneurial activities, while entry deregulation tends to encourage firm entry. Different from most of the existing studies that focus on the numbers and the characteristics of new entrants, our paper takes the entry reform as an ideal quasi-experimental shock to the entry cost of the economy, and explore its impacts on firm dynamics covering both extensive growth margins (firm entry and exit) and intensive margins (size). Moreover, we also systematically examine the causal impacts of entry regulation on firm productivity, a margin under-explored in earlier studies.

Second, our paper relates to the literature on the role of entry cost in shaping market structure and industrial dynamics (Hopenhayn, 1992; Dunne et al., 2013; Maican and Orth, 2015, 2018). Different from the structural approach commonly used in this strand of literature, our empirical strategy relies on a natural experiment to provide causal evidence on the direct and general equilibrium effects of decreasing entry cost. Moreover, there is also an active literature trying to understand the implications of entry cost on the aggregate TFP and misallocation (Barseghyan, 2008; Poschke, 2010; Barseghyan and DiCecio, 2011; Herrendorf and Teixeira, 2011; Boedo and Mukoyama, 2012; Hopenhayn, 2014; Restuccia and Rogerson, 2017; Fattal-Jaef, 2021; Asturias et al., 2021). Instead of relying on the cross-country data, our identification strategy exploits regional variation within a province, mitigating concerns over omitted institutional and cultural factors. In addition, previ-

[3] There is a large literature examining the outcomes of entry (de)regulation based on different types of data variation. Some of the influential studies on this topic are based on cross-country variation in entry regulations (Djankov et al., 2002, 2006; Djankov, 2009). To better address omitted variables in cross-country studies, other studies exploit the variations across industries (Klapper et al., 2006; Ciccone and Papaioannou, 2007; Fisman and Allende, 2010). In addition, some studies exploit policy shocks in entry (de)regulation for identification, an approach that our study follows (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 Italian retail trade sector, while Branstetter et al. (2013) evaluated the impact of entry deregulation in Portugal on the number and characteristics of new entrants.

ous papers have highlighted *the market competition effect*, while our empirical results also suggest that *the composition effect* due to the relaxation of financial constraints might be another important source of efficiency gain from entry deregulation.

Third, our paper contributes to the broad literature about the impact of regulation and government policies on TFP and misallocation in an important developing economy. Examples of regulation and government policies in the Chinese context include international trade quotas (Khandelwal et al., 2013), SOE reform (Hsieh et al., 2015), internal trade barriers (Tombe and Zhu, 2019) among others. Two recent papers also highlight the importance of barriers to entry to account for the economic growth in China (Brandt et al., 2020; Jiang et al., 2021). Our paper is the first to use Business Registration Reform as a quasi-experiment to explore the role of entry regulation. We add to this literature by providing causal evidence that reducing entry barriers increases the market turnover rates and improves the productivity of new entrants.

The rest of this paper is organized as follows: Section 2 discusses policy background and the data. Section 3 presents the theory model. Section 4 explores the empirical results on market turnover, including firm entry and exit. Section 5 documents the main effects of entry deregulation on firm size and productivity distribution. Section 6 concludes.

2 Policy Background and Data

2.1 Entry Registration Reform

The business registration system in China originated from the planned economy. According to *Doing Business Report* by the World Bank, it took on average 14 procedures, 38 days, and 3.5% of per-capita income to start a standardized business in China in 2011.[4] In terms of the ease to start a business, China ranked 150th among 183 regions and countries, being one of the most heavily regulated markets for establishing a new business.[5]

The State Administration of Industry and Commerce (SAIC) introduced the Business Registration Reform (*Shangshi Zhidu Gaige*) to deregulate firm registration. The reform was first implemented as a pilot in Guangdong province, which started from two cities, Dongguan and Foshan in 2012. It was then rolled out sequentially in 10 other cities, before being scaled up nationwide in March 2014.

Prior to the reform, a start-up had to obtain numerous certificates to receive a business license before official operation. The process was both time consuming and costly. The reform reversed

[4] For the assumptions about the standardized business, please refer to http://web.archive.org/web/20140607070136/http://www.doingbusiness.org/methodology/starting-a-business.

[5] The data source is http://web.archive.org/web/20120310024541/http://www.doingbusiness.org/data/exploreeconomies/china/.

the previous requirement: one can first get the license to run the business before securing all the necessary permits. Meanwhile, SAIC moved 108 approval items to post-registration, reducing the total number of required certificates to only 13. Consequently, the reform greatly simplified registration procedures, saving both time and financial cost to start a new business.

Another key feature of the reform is to relax the requirement of registered capital. The registered capital is "the amount of contributions actually paid (or subscribed) by all shareholders lawfully registered", which corresponds to the liability of shareholders.[6] The New Corporate Law implemented in 2006 imposed minimum capital requirements upon registration, and regulated the contribution schedule of registered capital.[7] However, the reform removed all these requirements and restrictions.[8] By replacing the previous "capital paid-in system" with the "capital subscription system", a new firm no longer needs to fully pay the registered capital within the first two years after registration.[9] After entry deregulation, firms just have to report the amount of registered capital to the local administration without any verification requirement.

In addition to simplifying numerous procedures and lessening the registered capital requirements, the reform also changed the firm annual inspection system to an annual report system. The old annual inspection system required a firm to fill in inspection forms, provide income and debt statements, pay inspection and auditing fees, and bring multiple documents to the local administration for processing.[10] In contrast, after the reform, SAIC allowed firms to file their annual reports electronically. Firms now can choose to publicize their annual reports in the National Enterprise Credit Publicity System, and SAIC randomly selects a small proportion of firms for onsite inspection. If a firm fails to obtain the necessary permits within grace periods or report false information in their annual reports, the violation records will be displayed in the system and open to the public. Furthermore, the government amended a series of laws to support the reform. Combining all these different measures, the reform has eliminated a huge proportion of registration cost and greatly lowered bureaucratic barriers faced by potential entrants.

[6] It is directly quoted from government official document http://www.lawinfochina.com/display.aspx?lib=law&id=3562&CGid=.

[7] The minimum capital requirements vary by firm types. For limited liability companies with two or more shareholders, the minimum capital requirement was ¥30,000 (around $46,000). For single shareholder limited liability companies, it was set at ¥100,000 (around $15,000) and must be paid in a single installment. For public limited companies, the minimum capital requirement was ¥5 million (around $0.77 million).

[8] The amendment to the Corporate Law involved several aspects about the registered capital. First, the system of paid-in registered capital was transformed to subscribed capital and the provisions on the statutory periods of a company's contribution were cancelled. Second, the restrictions on a company's minimum registered capital, the proportion of initial contributions from shareholders, and the ratio of monetary contribution were all cancelled. Third, the registration procedures were simplified and a company is no longer required to submit a capital verification report upon registration.

[9] For the details about the "capital paid-in system" and the "capital subscription system", please refer to https://www.lexology.com/library/detail.aspx?g=3c67911a-e058-41d8-918a-e38382e259b6.

[10] For more information on firm annual inspection, please refer to https://baike.baidu.com/item/%E4%BC%81%E4%B8%9A%E5%B9%B4%E6%A3%80.

2.2 The Pilot Program in Guangdong

The Business Registration Reform started with a staggered pilot in Guangdong province. It was initiated in 2012 for two cities, Dongguan and Foshan, and was adopted by other 10 cities sequentially during 2013-2014. The reform was then scaled up in the remaining nine cities in Guangdong together with other provinces of China in March 2014. Among the twelve pilot cities, five started the reform in a small area, i.e. a township/district/county, before scaling it up to the entire city.[11] Since these mini-pilots were only tentative and incomprehensive, we use the start date of the pilot in the entire city in our empirical analysis. We collect the start time from two sources: official documents and local news media. Specifically, we take the date of first license issued after the reform from the local news, and compare that with the start date officially stated in the government documents. These two dates are exactly the same in all cases where the former is available.

There is some variation in policy enforcement across cities. For example, early government documents in Zhaoqing, Huizhou, and Jieyang claimed to begin the pilots in 2012, but there were no major reform breakthroughs until March 2014.[12] In addition, these early official documents focused only on relaxing registered capital requirement but ignored other important policy aspects. For these three cities, we use the date from local news as the exact start time of the pilot. As a robustness check, we exclude them from the sample, and find that the results are robust. The rollout timing of the pilot is summarized in four snapshots in Figure A1.

There are other policy changes in Guangdong in recent years. Among them, the value-added tax reform, the establishment of Guangdong Pilot Free Trade Zone, and the development of Guangdong-Hong Kong-Macao Greater Bay Area, all took place after our sample period, thus will not confound our empirical results. Other policies such as Rural Revitalization Project were rolled out simultaneously across cities. We introduce a set of controls including time fixed effects and city by year fixed effects to rule out these contemporaneous reforms as potential confounders.

2.3 Data

To evaluate the impacts of the business registration reform along multiple dimensions, we compile two administrative data sets, Business Registry Database and Firm Annual Report Database, as well as primarily collected Enterprise Survey for Innovation and Entrepreneurship in China (ESIEC) in 2016. In this section, we describe these three main data sets. We further complement the main data sets with National Tax Survey Database among others, and additional data information can be

[11] For example, Foshan started the reform in its Shunde District in May 2012 and then expanded the reform to the whole city in September 2012. Dongguan began the first pilot in May 2012 before scaling up to the whole city in December 2012. Similar staggered rollouts occured within Guangzhou, Yangjiang, and Zhuhai.

[12] Take the case of Jieyang as an example, three official documents announced the reform would start in May 2012. However, no explicit news report and information were found until March 2014.

found in Appendix B.

The first data set, the Business Registry Database, is maintained by SAIC. It contains universal firm registration information, including registry date, revoke or cancel date, location, ownership, industry code, registered capital, entrepreneurs' characteristics (education, age, and gender), and the complete shareholding structures. We calculate the monthly number of new entrants in each city and industry using data from 2009 to 2016. The firm registration records enable us to directly examine the impacts on the extensive margins. Moreover, the information on entrepreneurs' characteristics and shareholding structures allows us to explore some key features of new entrants. Our second data set, the Firm Annual Report Database, is also from SAIC covering universal firm annual business information such as gross capital, employment, sales, and taxes.[13] We have access to the database for Guangdong province for 2008-2016.

These two data sets have several key advantages that make them particularly useful for our empirical analysis. 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 include all the firms regardless of their size. This feature is particularly valuable as we aim to investigate the dynamics of the entire industries and many entrants are small. Second, our data covers several years both before and after the reform, enabling us to exploit the staggered roll-out of the reform. The sample period is long enough for examining the parallel trend assumption using pre-reform data. Third, the Firm Annual Report Database provides detailed firm-level annual information, which allows us to further explore the distribution and evolution of firm size and productivity, instead of just focusing on the extensive margins as in many previous studies.

Nevertheless, out data sets have three potential caveats. First, there are missing data issues in the Firm Annual Report Database as the reporting rate is much lower in the year of 2013, which might be a result of a transition from the annual firm inspection system to the annual report system that year. However, we don't find a systematic missing pattern in relation to industries and ownership types. We further allow very flexible functional forms to control for this reporting issue in our regression analysis. Second, the firm employment information is only available post 2013, making it challenging to consistently estimate productivity for the entire sample period. We instead estimate productivity structurally and leverage employment information from the shorter sample period in robustness checks.

The third data issues is that the number of firms whose licenses were revoked in 2014 and 2015 dropped greatly. The Business Registry Database includes firm exit information of either self cancellation or revocation by SAIC. Both types of "registered exit" were equally important

[13] The database also contains information firm profit but the variable has a lot of missing and likely under-reported values: 2.6% of observations have missing profit information, 17.3% of observations have exactly zero profit and 20% of observations have non-zero profit with absolute values lower than ¥10,000 ($1,540).

in magnitude before the reform. Nevertheless, firm registration revocation was largely grounded to a halt in 2014 and 2015.[14] We use two strategies to mitigate the issue. First, instead of solely relying on the official exit from the Business Registry Database, we define the incumbents stopping submitting annual reports as exiting firms.[15] Given the mandatory nature of filing annual reports and the severe penalty of revoking license for not doing it, this measure is a good proxy for the actual exit. Using this measure, we find the aggregate exit rate to be 5%-10% over time and showing an increase after the reform. Second, SAIC cleaned up inactive firms in 2017-2019, including those which were supposed to be revoked in 2014-2015. We use the updated exit information as of 2019 from Firm Registry Database as an alternative exit measure, and the results are qualitatively similar using either measure.

In order to examine detailed characteristics of new firms and new entrepreneurs, we utilize a third data set from the 2018 Enterprise Survey for Innovation and Entrepreneurship in China (ESIEC), which is conducted by the Center for Enterprise Research at Peking University.[16] It's an annual survey on innovation and entrepreneurship of Chinese enterprises via random sampling at the province level. The 2018 survey covers newly established private firms between 2010 and 2017 in six provinces. Although the sample was supposed to be representative in each province, it turned out that the distribution of the finished sample closely resembles the national distribution at the first-digit-industrial-code level based on China Economic Census 2018. The survey asked entrepreneurs to recall the time and money spent on registration, from which we can directly measure the self-reported "cost of entry".[17] Moreover, we also use the information of past experience of running businesses, risk attitude, firm innovation, as well as sources of starting capital in our empirical analysis.

3 Theoretical Model

In this section we provide a theoretical model to illustrate how entry deregulation shapes industrial dynamics and firm productivity. The model is a static version of entry, exit and market competition following Hopenhayn (1992), Melitz (2003) and Helpman et al. (2004). To keep the model tractable analytically, we do not solve the full equilibrium and abstract away the firm specific uncertainty. Furthermore, instead of assuming potential entrants to be ex ante identical as in previous papers, we incorporate the heterogeneity among potential entrants to explore the efficiency implications of

[14] This is likely due to local government's incentive to keep the total number of existing firms high in order to bolster the achievement of the reform.

[15] Inferring firm exit from the disappearance from consecutive censuses (annual report in our case) is also a common practice in the literature (Dunne et al., 1988).

[16] For more information about the survey, please refer to https://www.cer.pku.edu.cn/.

[17] Question P0201 in the 2018 questionnaire is used.

entry deregulation.[18]

3.1 Model Setup

There is a continuum of potential entrants endowed with a fixed factor Γ and productivity φ. Γ includes firm-level endowment, social capital and initial physical capital, which are higher for those with better financial access and bureaucratic connection. We discretize Γ such that $\Gamma \in \{\Gamma_L, \Gamma_H\}$, $\Gamma_L < \Gamma_H$, and each type of potential entrants is of an equal unit mass. φ is firm-level productivity, which measures managerial ability and entrepreneurial talent, drawn from a common distribution $G(\varphi)$. Before the reform, due to the entry regulation, we assume that only potential entrants with $\Gamma = \Gamma_H$ would enter and operate on the market, while potential entrants with $\Gamma = \Gamma_L$ are only allowed after entry deregulation. This assumption indicates that government regulation selects entrants on the firm size and endowment, favoring those with more financial resources and better bureaucratic connections. We start by analyzing the economy before the reform.

There are N incumbent firms producing a homogeneous product on a competitive market using the fixed factor Γ, as well as a variable input l at the unit cost $w(N)$. We assume w to be an increasing function of N and $w'(N) > 0$: more intense market competition is associated with higher input prices. Thus, the magnitude of w captures the intensity of market competition. The price of the output good is normalized as 1. The production function is $y = \varphi\Gamma^{\alpha}l^{\gamma}$ where $\alpha, \gamma, \alpha + \gamma \in (0, 1)$.

In each period before the reform, the incumbent firms solve the following profit maximization problem,

$$\pi(\varphi, \Gamma_H, w) = \max_{l} \quad \varphi\Gamma_H^{\alpha}l^{\gamma} - wl.$$

The optimal labor demand and profit function are

$$l^*(\varphi, \Gamma_H, w) = (\frac{\gamma\varphi\Gamma_H^{\alpha}}{w})^{\frac{1}{1-\gamma}}, \quad \pi(\varphi, \Gamma_H, w) = A\varphi^{\frac{1}{1-\gamma}}, \quad \text{where} \quad A = (1-\gamma)(\frac{\gamma\Gamma_H^{\alpha}}{w^{\gamma}})^{\frac{1}{1-\gamma}}.$$

Incumbent firms also decide whether to exit or not, denoted as χ. They are faced with a fixed cost c_f of operation and will exit the market if the value of variable profit net fixed cost falls below zero. Since productivity φ is the only heterogeneity among firms with fixed factor Γ_H, equivalently, a firm will exit the market if its φ is below the threshold φ^o:

$$\chi = 1 \Leftrightarrow \pi(\varphi, \Gamma_H, w) - c_f < 0 \Leftrightarrow \varphi < \varphi^o.$$

[18]For example, Hopenhayn (1992) is silent about how entry deregulation affect entrants' sizes and productivity, as all potential entrants are assumed to be ex ante identical. In order to relax this assumption, we introduce heterogeneity among potential entrants in the model.

where the *marginal* exiting firms at φ^o satisfy $\pi(\varphi^o, \Gamma_H, w) = c_f$. Therefore, the value of continuing on the market with the discount factor β is

$$V^c(\varphi, \Gamma_H, w) = \max\{\frac{\pi(\varphi, \Gamma_H, w) - c_f}{1-\beta}, 0\} = (1-\chi)\frac{\pi(\varphi, \Gamma_H, w) - c_f}{1-\beta}.$$

There is a continuum potential entrants who make entry decisions, denoted as E. An entrant will enter the market if the expected entry value exceeds entry cost c_e:

$$E = 1 \Leftrightarrow \frac{\beta(\pi(\varphi, \Gamma_H, w) - c_f)}{1-\beta} \geqslant c_e.$$

The equality holds above for the *marginal* entrants with productivity φ^*. Since we abstract away firm specific uncertainty to ease exposition, $\varphi^* > \varphi^o$, and there is no exit in the long-run equilibrium.[19] However, for the purpose of illustration, we define firms with φ below the threshold φ^o as exiters to investigate the impacts of the reform on firm exit. The total number of firms is

$$N = \int_{\varphi \geqslant \varphi^*} dG(\varphi).$$

To sum up, the competitive equilibrium is defined as follows:

Definition 1 *A competitive equilibrium for the industry consists of labor price w^*, exit threshold φ^o, entry threshold φ^*, total number of firms N and variable input demand $l^*(\varphi, \Gamma, w)$ which satisfy:*

1. *$l^*(\varphi, \Gamma, w)$ maximizes firm's profit.*
2. *φ^o, φ^*, N and w^* satisfies*

$$\pi(\varphi^o, \Gamma_H, w^*) = c_f \qquad \text{(Exit rule)}, \tag{1}$$

$$\frac{\beta(\pi(\varphi^*, \Gamma_H, w^*) - c_f)}{1-\beta} = c_e \qquad \text{(Entry rule)}, \tag{2}$$

$$N = \int_{\varphi \geqslant \varphi^*} dG(\varphi) \qquad \text{(Total number of firms)}, \tag{3}$$

$$w^* = w(N) \qquad \text{(Input market clears)}. \tag{4}$$

[19]In the long-term equilibrium, firms' exit decision is degenerated due to the lack of firm specific uncertainty. Reintroducing the uncertainty to firms' productivity evolution or fixed cost draws will fully close the model so that there are entry and exit in the long-term stationary equilibrium. However, it increases the complexity of the model without providing additional insights for our purpose.

3.2 The Impact of Entry Deregulation

Entry deregulation removes administrative restrictions on new entrants and reduces entry cost. We assume that after the reform, potential entrants with Γ_L are now allowed to enter the market, and that the entry cost is reduced such that $\tilde{c}_e < c_e$ (we use $\tilde{x}$ to denote the post-deregulation value of x). As indicated by Equation (2), $\varphi^* = \varphi(\Gamma_H, w^*, c_e)$ with $\frac{\partial \varphi^*}{\partial \Gamma_H} < 0$, $\frac{\partial \varphi^*}{\partial w^*} > 0$, and $\frac{\partial \varphi^*}{\partial c_e} > 0$. Similarly, we can define the new entry cutoffs of productivity for two types of potential entrants as $\tilde{\varphi}_H^*$ and $\tilde{\varphi}_L^*$ respectively, such that

$$\tilde{\varphi}_H^* = \varphi(\Gamma_H, \tilde{w}^*, \tilde{c}_e), \quad \tilde{\varphi}_L^* = \varphi(\Gamma_L, \tilde{w}^*, \tilde{c}_e)$$

After the entry deregulation, under mild assumptions on G, there will be more firms entering the market:

$$\tilde{N} = \int_{\varphi \geqslant \tilde{\varphi}_H^*} dG(\varphi) + \int_{\varphi \geqslant \tilde{\varphi}_L^*} dG(\varphi) > N.$$

3.2.1 Incumbents

Entry deregulation affects incumbents through the general equilibrium effect of more intense competition. As more firms entering the market, $\tilde{w}^* = w(\tilde{N}) > w(N) = w^*$, the profit $\pi(\varphi)$ will increase more slowly as φ grows. Consequently, previous marginal incumbents will find it unprofitable to stay in the market as shown in Figure 1:

$$0 = \pi(\varphi^o, \Gamma_H, w^*) - c_f > \pi(\varphi^o, \Gamma_H, \tilde{w}^*) - c_f.$$

The new exit threshold for productivity moves rightward $\tilde{\varphi}^o > \varphi^o$, and thus incumbents with $\varphi \in (\varphi^o, \tilde{\varphi}^o)$ will exit the market. The mass of exiting firms increases by $X_2 - X_1$ as labelled in Figure 1. Compared with continuing incumbents, we expect to observe a relative increase in average firm productivity and size among exiting firms, as well as a higher exit rate, given that the exit threshold increases after deregulation. The impacts on firm exit highlight important general equilibrium effects of entry deregulation.

Proposition 1 *Entry deregulation increases the average productivity and size among exiting firms while resulting in a higher exit rate.*

3.2.2 Entrants

Before entry deregulation, only potential entrants with Γ_H could enter the market. Following the entry rule described in Equation (2), the average productivity of new entrants is

$$E_H(\varphi|\varphi \geqslant \varphi^*) = \int\limits_{\varphi \geqslant \varphi^*} \varphi dG(\varphi|\varphi \geqslant \varphi^*) = E_H(\varphi|\varphi \geqslant \varphi(\Gamma_H, w^*, c_e)).$$

After entry deregulation, not only potential entrants with Γ_H but also those with Γ_L could enter the market. The average firm size, if measured by the value of fixed factor, will unambiguously decrease. However, the impact on entrants' productivity is more complicated. There are three sources of variations that lead entrant's productivity to vary: w^* (market competition), Γ (entrants' composition), and c_e (the stringency of entry screening).

For entrants with Γ_H, the average productivity after the reform is denoted by $E_H(\varphi|\varphi \geqslant \varphi(\Gamma_H, \tilde{w}^*, \tilde{c}_e))$. The change in the average productivity after the reform is $\Delta E_H(\varphi)$ defined as

$$\begin{aligned} \Delta E_H(\varphi) &= E_H(\varphi|\varphi \geqslant \varphi(\Gamma_H, \tilde{w}^*, \tilde{c}_e)) - E_H(\varphi|\varphi \geqslant \varphi(\Gamma_H, w^*, c_e)) \\ &= \underbrace{E_H(\varphi|\varphi \geqslant \varphi(\Gamma_H, \tilde{w}^*, \tilde{c}_e)) - E_H(\varphi|\varphi \geqslant \varphi(\Gamma_H, w^*, \tilde{c}_e))}_{\textit{market competition effect } (+)} \\ &+ \underbrace{E_H(\varphi|\varphi \geqslant \varphi(\Gamma_H, w^*, \tilde{c}_e)) - E_H(\varphi|\varphi \geqslant \varphi(\Gamma_H, w^*, c_e))}_{\textit{weak screening effect } (-)}. \end{aligned} \quad (5)$$

As shown in Equation (5), it could further be decomposed into two components. The first component is only from the change in w^*, which we call *the market competition effect*. The second component is only due to the change in c_e, which we call *the weak screening effect*. As $\tilde{w}^* > w^*$ and $\tilde{c}_e < c_e$, we have

$$\varphi(\Gamma_H, \tilde{w}^*, \tilde{c}_e) > \varphi(\Gamma_H, w^*, \tilde{c}_e), \quad \varphi(\Gamma_H, w^*, \tilde{c}_e) < \varphi(\Gamma_H, w^*, c_e)$$

Therefore, *the market competition effect* pushes the average productivity upwards among entrants with Γ_H, as more intense market competition only permits entrants with higher productivity. On the contrary, *the weak screening effect* leads to a decrease in the average productivity, since lower entry cost imposes a weaker screening force and allows less efficient firms into the market. The two counteracting forces are illustrated in Figure 2.

For entrants with Γ_L, the new average productivity is denoted by $E_L(\varphi|\varphi \geqslant \varphi(\Gamma_L, \tilde{w}^*, \tilde{c}_e))$. We denote the difference in the average productivity for entrants with Γ_L compared to previous entrants as $\Delta E_L(\varphi)$, which is defined as the following.

$$\begin{aligned}\Delta E_L(\varphi) &= E_L(\varphi|\varphi \geqslant \varphi(\Gamma_L, \tilde{w}^*, \tilde{c}_e)) - E_H(\varphi|\varphi \geqslant \varphi(\Gamma_H, w^*, c_e)) \\ &= \underbrace{E_L(\varphi|\varphi \geqslant \varphi(\Gamma_L, \tilde{w}^*, \tilde{c}_e)) - E_L(\varphi|\varphi \geqslant \varphi(\Gamma_L, w^*, \tilde{c}_e))}_{\textit{market competition effect } (+)} \\ &+ \underbrace{E_L(\varphi|\varphi \geqslant \varphi(\Gamma_L, w^*, \tilde{c}_e)) - E_L(\varphi|\varphi \geqslant \varphi(\Gamma_L, w^*, c_e))}_{\textit{weak screening effect } (-)} \\ &+ \underbrace{E_L(\varphi|\varphi \geqslant \varphi(\Gamma_L, w^*, c_e)) - E_H(\varphi|\varphi \geqslant \varphi(\Gamma_H, w^*, c_e))}_{\textit{composition effect } (+)}.\end{aligned} \tag{6}$$

As shown in Equation (6), it could further be decomposed into three components. The first two are again from *the market competition effect* because of the change in w^*, and from *the weak screening effect* due to a lower c_e. The third component is unique for entrants with Γ_L, which is from the change in entrants' composition. As $\Gamma_H > \Gamma_L$, we have

$$\varphi(\Gamma_L, w^*, c_e) > \varphi(\Gamma_H, w^*, c_e).$$

We call the third channel *the composition effect*, which stems from a change in the entrants' composition: the productivity of new entrants improves since entrants with a smaller endowment have to be more productive in order to enter the market compared to those with a larger endowment. Moreover, if the endowment allocation is distorted among potential entrants due to financial friction or government rent-seeking, allowing less endowed entrepreneurs to enter the market would correct the distortion and improve the efficiency.[20] Different forces are further illustrated in Figure 2.

Proposition 2 *Entry deregulation decreases the average firm size among new entrants. Among large entrants, the average firm productivity increases if the market competition effect dominates the weak screening effect. Among small entrants, the average firm productivity increases if the sum of the market competition effect and the composition effect dominates the weak screening effect.*

To sum up, the stylized model generates three predictions: 1) entry deregulation boosts both firm entry and exit; 2) entry deregulation increases the average size of exiters, and decreases the average size of entrants; 3) entry deregulation raises the average productivity of exiters, while the impacts on entrants depend on three competing forces. We next take these model predictions to data and test them empirically.

[20] In figure 2, we assume the productivity distributions of potential entrants with Γ_H and Γ_L are identical. However, we could relax this assumption such that the productivity distribution of potential entrants with Γ_L is to the right (left) of that for potential entrants with Γ_H, which leads to an increasing (decreasing) force on $E_L(\varphi|\varphi \geqslant \varphi(\Gamma_L, \tilde{w}^*, \tilde{c}_e))$. Nevertheless, it's challenging to separate this channel from *the composition effect*, thus we label *the composition effect* as a net effect of the two forces.

4 Entry Deregulation and Market Turnover

This section first examines the impacts of entry deregulation on the market turnover. We begin by analyzing the effects on firm entry, the direct policy target of the reform. We then examine the impacts on firm exit as an important outcome of the general equilibrium effect. For the impacts on firm entry, we leverage the *monthly* variation in the implementation timing across cities, and employ the DID strategy. Moreover, we explore the heterogeneity of the effects across different industries and ownership types. For the impacts on firm exit which is measured annually, we exploit the *yearly* variation in the implementation timing, and leverage the cross-industry variation to further aid identification.

4.1 Descriptive Evidence

We first describe the data pattern before turning to regressions. Guangdong had 250,000 new firms in all sectors, about 12% of the national total in 2011. The annual growth rate in the number of new firms is 16.5% and the entry rate averages 14.7% during 2008-2012.[21] Post the reform, the number of new entrants exhibited a sharp increase as shown in Panel A of Figure 3. The number of new entrants in 2014 and 2016 nearly doubled and tripled that of 2012 respectively. Moreover, the annual growth rate in the number of new entrants was 34.4% in 2012-2016 with the average entry rate being 22.7%. The growth rates in 2013 and 2014 both exceeded 40%.[22]

Panel B of Figure 3 displays the monthly trend by city. Most cities experienced significant upward shifts in the number of new entrants *immediately* after the reform. For example, the number of monthly new entrants averaged 910 across all cities in the 12-month window prior to the reform, and jumped to 1,521 after the reform took place. The only exceptions are Zhongshan, which experimented with local policy changes in 2012 as a precursor to the official provincial program, and Foshan and Dongguan, the first two pilot cities with many trials and errors in the policy experimentation.[23] Zhongshan witnessed a spike of firm entry in 2012, while Foshan and Donguan exhibited a sharp increase in firm entry in March 2014 when the reform was expanded nationwide. We use the official reform time for Zhongshan in our main empirical analysis to be consistent with the rest of the sample, though results are robust when Zhongshan is excluded from our analysis.

[21] 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., Entry $\text{Rate}_t = \text{Entry}_t/\text{Incumbent}_{t-1}$.

[22] For the manufacturing sector, new entrants experienced a milder increase, but we still observe significant shifts in both the levels and trends.

[23] See http://www.zsda.cn/plus/view.php?aid=354830.

4.2 Empirical Strategy

We take advantage of the staggered roll-out of the reform and adopt the DID strategy to identify the effects of entry deregulation on the number of new firms. The empirical model is shown in Equation (7),

$$Y_{cit} = \beta D_{ct} + \xi_{ci} + \tau_t + \zeta_{cy(t)} + \epsilon_{cit}, \tag{7}$$

where the dependent variable Y_{cit} is the number of new entrants in logarithm in city c, industry i and month t. Our key explanatory variable, D_{ct}, is a dummy variable, which takes the value 1 if time t is post the reform in city c and 0 prior to the reform. β is the key parameter of interest and measures the effect of the reform. The city by industry fixed effects ξ_{ci} control for the time-invariant differences across cities and industries, while τ_t denotes the province-wide time trend.

The key threat to identification is the potentially endogenous timing of the reform.For example, if the government had determined the order of the roll-out in response to economic conditions (e.g., the slowdown of entrepreneurial activity in a given city relative to other cities), this would lead to the endogeneity in the treatment variable and violate the parallel trend assumption. To address this concern, we control for a variety of time-related fixed effects. Our preferred specification controls for city by year fixed effects $\zeta_{cy(t)}$ that capture city-level macroeconomic shocks. This specification only uses the *monthly* variation of the program implementation within a year, thus our identification relies on a weaker condition that the exact month (rather than the year) of program implementation is exogenous. To further test the robustness, we add city by calendar month fixed effects to control for seasonality in firm entry across cities. We also include industry by time (month and year) fixed effects to control for industry specific trends. The key assumption is that conditional on these controls, the reform's timing is exogenous to the unobserved factors ϵ_{cit}, and thus the treated group would have followed the same trend as that of the control group had the reform not taken place. We also weight the regression using the number of incumbents for each city and industry pair at the beginning of the sample period.[24]

To further check the parallel trend assumption prior to reform and to examine the dynamic effects post the reform, we conduct an event study:

$$Y_{cit} = \sum_{q=-12}^{15} \beta_q D_{ct}^q + \xi_{ci} + \tau_t + \zeta_{cy(t)} + \epsilon_{cit}, \tag{8}$$

where we just replace the dummy variable D_{ct} in Equation (7) 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, and D_{ct}^2 denotes whether t is two months post the

[24] The estimates stay robust if we use city-level regressions without the weights.

reform. Periods earlier than 12 months before the reform and later than 15 months post the reform are grouped as D_{ct}^{-12} and D_{ct}^{15}. We set one month before the reform as the baseline group. We are interested in β_p as they capture the pre-trend (if any) and dynamic treatment effects post the reform.

A recent literature has pointed out that two-way fixed effects model could yield biased estimates when there are heterogeneous treatment effects in the staggered DID setting (De Chaisemartin and d'Haultfoeuille, 2020a,b; Borusyak et al., 2021; Sun and Abraham, 2020; Callaway and Sant'Anna, 2020; Goodman-Bacon, 2021). In the presence of the effect heterogeneity, the "forbidden comparison" between the later-treated units and the comparison groups which are treated earlier might lead to negative weights in estimating the average treatment effects, and contaminate the coefficients of all leads and lags in the event study. We follow the aforementioned literature and replicate our results with several new estimation methods, and find the results to be largely robust as shown in Appendix C. Therefore, we use standard OLS estimates in the main text of our paper.

As discussed in Section 2.3, we use two measures to capture exiters: firms stopping submitting annual reports, and firms cancellation/revocation from updated exit records as of 2019 in the administrative Firm Registry Database. As firm exit is only observed at the annual level, we cannot rely on the within-year variation for identification. Instead, we rely on the cross-industry variation to identify the causal impacts on firm exit. Conditional on a rich set of controls, we empirically test whether industries with higher degrees of deregulation experience a larger increase in the exit rate. The dependent variable is a firm-level measure $\mathbb{1}\{\text{Exit}_{it} = 1\}$, which takes the value 1 if firm i exits in the year $t + 1$ and 0 otherwise. We exclude the last year of our sample (year 2016) for the firm exit analysis due to the right-censoring issues.

4.3 Firm Entry

Baseline Results: the Impacts on Firm Entry

Table 1 reports the regression results based on Equation (7). Panel A shows the estimates on the number of newly registered firms across different manufacturing industries. The dependent variable in Columns (1)-(3) is the number of new firms by month in logarithm at the city-industry level. Column (1) controls for city by year fixed effects to address potential concerns over endogenous reform timing and unobserved macroeconomic shocks at the city level. The estimated impact of the reform on firm entry is about 21.2%. In Columns (2) and (3), we further add city by calendar month fixed effects to control for seasonality across cities, and industry by time (i.e., month of sample) fixed effect to control for industry specific time trend. The estimates are similar across columns with slightly larger magnitudes. In our preferred specification with the most controls in Column (3), entry deregulation is associated with a 24.5% increase in new entrants. Moreover, we use log entry rate as the dependent variable in Columns (4)-(6). The results are similar to those in Columns (1)-(3), because the variation in the number of incumbents (the denominator of entry rate) is largely absorbed by the city and year interacted fixed effects.

The baseline estimates show that the entry deregulation increased the number of new firms by about 25%. The average annual firm entry increased by 51.5% from 2009-2012 to 2013-2016, thus entry deregulation explained around 49% of the total growth. Our estimate is larger than the impacts of entry deregulation documented in other countries, such as 5% in Mexico (Kaplan et al., 2011) and 17% in Portugal (Branstetter et al., 2013). This is likely because China's reform measures were more comprehensive and implemented more rapidly and forcibly. Reforms in Mexico and Portugal were mainly about the simplification of registration procedures to cut down the time and financial costs required to start a new business, while the entry deregulation reform in China included many other measures, such as replacing paid-in capital with the subscribed capital. Furthermore, the pilot program in Guangdong was carried out quickly with little delay and strictly enforced. In comparison, according to Kaplan et al. (2011), although the federal government in Mexico exerted great efforts to persuade municipals to take up the reform, the roll-out schedule was delayed due to local resistance. Similarly, Branstetter et al. (2013) also mentioned local resistance to the adoption of the reform in Portugal.

One concern with the high entry rates post the reform is the presence of more "shell" firms because of the more lax entry regulation after the reform. These shell firms exist on paper without real business activities.[25] An increasing number of "shell" firms would overestimate the impact on firm entry as shell firms are not engaged in productive activities. To address this concern, we use two alternative measures of new entrants that should eliminate or at least minimize potential "shell" firms. Panel B includes only the newly registered firms which have ever filed annual reports during our sample period. Panel C further restricts the sample by excluding firms that always report zero or missing capital stock. As shown in Dai et al. (2020), the sampled firms, which could not be physically found by the enumerators of the ESIEC survey in 2018, are less likely to submit annual reports and even if they submit, they tend to report zeros for most items, including capital stock. Thus these indicators are effective in screening shell companies. If the rise in new firms stems mainly from "shell" firms, we should expect a smaller effect when using the latter two alternative measures.

However, we find similar estimates in both Panel B and C. These results suggest that though the number of "shell" firms may have significantly increased, its share in new entrants stays stable over the sample period. After the reform, the cost to register a "shell" firm has declined, so has the cost to register an active firm. Furthermore, in the manufacturing sector, a "shell" firm can

[25]See for example `http://amr.sz.gov.cn/xxgk/qt/ztlm/scjgzy/mtbd/content/post_7821580.html`. According to Wiki `https://en.wikipedia.org/wiki/Shell_corporation`, shell companies are often registered to achieve anonymity, "serving as a vehicle for business transactions", or for tax evasion or tax avoidance.

be easily identified if it is not physically present in a registered location. Therefore, "shell" firms might be less of a concern in the manufacturing sector, which is a focus of our paper, than in the service sector, as shown in Figure D1. Appendix D presents detailed discussion on "shell" firms.

A key underlying assumption for the DID strategy is the parallel trend between the treatment and control groups. We check this with the event study framework outlined in Equation (8). As demonstrated in the Panel A of Figure 4, the coefficients prior to the reform are all small, insignificant, and hovering closely around zero, confirming the parallel trend assumption. Right after the reform, there is a sharp increase in entry rates. The estimates are jointly significant after the reform and the magnitude persists even 15 months post the reform, indicating a lasting effect.

Recent literature raises concerns over the negative weights in the two-way fixed effects estimates when the treatment timing is staggered and the treatment effects are heterogeneous. We follow the new estimators proposed by Sun and Abraham (2020), Borusyak et al. (2021) and De Chaisemartin and d'Haultfoeuille (2020a) and examine the robustness of our baseline results. The results are very similar to what we obtain via OLS, thus we keep standard OLS estimates for the rest of the paper and leave detailed discussions of our results using alternative estimators in Appendix C.

Robustness Checks: Samples, Expectations, and Spillovers

Table A1 provides a series of robustness checks for our baseline results. Firstly, as discussed in Section 2.3, there are some ambiguities regarding the timing of the reform in Huizhou, Jieyang and Zhaoqing. We exclude these three cities from our sample in Column (1). The coefficient estimate slightly increases to 27.4% from the baseline estimate of 24.5%. Secondly, as Foshan and Dongguan are the cities to start the earliest pilots in 2012 where reform measures implemented may have been different from later cities. Figure 3 also shows that clear shifts occurred only after the national reform took place for these two cities. As a robustness check, we exclude them from our sample in Column (2) and the magnitude further rises to around 37%.

Thirdly, our main sample covers monthly data from January 2009 to December 2016. In Column (3), we only keep a shorter sample period up to March 2014 before the national reform rolled out. Therefore, we only rely on the timing variation for the twelve pilot cities, while the non-pilot cities serve as the comparison group throughout the time window. The estimate is robust and a bit larger at around 30%. Fourthly, as shown in the Panel A of Figure 4, there is a slight dip before the reform, suggesting the possibility of the anticipation effect. We drop the three periods right before the treatment to eliminate the anticipation effect in Column (4). The estimated impact decreases to around 20%, but still within the confidence interval of our baseline estimates.

Lastly, one might be concerned about the geographical spillovers, as the entry deregulation of "early-treated" cities might attract businesses away from the "late-treated" cities, if the product or input markets are integrated within the province. Those geographical spillovers would contaminate our comparison group and bias our baseline results upwards. In Columns (5)-(6), we keep our sample period up to March 2014. Instead of using non-pilot cities in the same province as the comparison group, we replace all non-pilot cities with cities in Jiangsu province and Shandong province respectively. Jiangsu and Shandong, the second and third most developed provinces in China, comparable to Guangdong economically but geographically far away, are less likely to be contaminated by early pilots in Guangdong. The estimated coefficients are around 26%, which should address the concern over geographical spillovers. Overall, our baseline estimates are robust and indeed more conservative than the results from a series of robustness checks.

Are New Entrants *De Novo*?

Although we show that our baseline results are robust, another important question is whether the massive new entrants are *de novo* or not. There are three key alternative explanations for the estimated impact. First, the large impact we document in Table 1 might be mainly driven by the spin-off of existing firms, instead of *de novo* entrants. Even if we observe a large increase in the number of firms, it may only reflect the expansion of existing firms, while the pool of entrepreneurs stays unchanged. We explicitly test this channel by investigating whether the individual investors of these new firms are more likely to be serial entrepreneurs after entry deregulation. Serial entrepreneurs are defined as individual investors who have invested other existing firms. If the spin-off is the major driving force, we would observe a large increase in the frequency of entrants owned by serial entrepreneurs. The results in Panel A of Table 2 contradict this hypothesis. After the reform, the probability of new firms with any (or the largest) individual investor being a serial entrepreneur stays unchanged, indicating that the spin-off of existing firms is unlikely to be the main driver of our baseline result.

Second, one might argue that the higher entry rates after the reform reflect a reclassification of family-operated small businesses (*getihu* or mom-and-pop operations) to formal firms. In the presence of reclassification, the total number of enterprises remains the same despite the increase in the number of newly incorporated firms. To address the concern, we replicate the results in Table 1 with the measure of *getihu* entry. If the reclassification of *getihu* drives the result, we would expect a negative impact of entry deregulation on the *getihu* entry. However, Panel B of Table 2 shows a significant and positive impact on the *getihu* entry. The effect is about 80% of the coefficient estimate for new firms because the registration of *getihu* had been easier than that of firms prior to the reform. Furthermore, we leverage the ESIEC field surveys and examine the past experience for the surveyed entrepreneurs. The ratio of entrepreneurs who previously were *getihu* owners does not show an obvious trend in relation to the timing of the reform, as shown in Figure A3. These results largely rule out the reclassification channel.

Third, the estimated increase in new firms may be an artifact of spatial relocation of existing firms from one area to another due to the lower entry cost associated with the reform. The registration data includes the information about hometown origins of entrepreneurs, allowing us to directly check this out. Specifically, we examine whether the number of new firms set up by non-local entrepreneurs (i.e., whose hometowns are outside the county/city/province) have exhibited a more rapid increase after the reform.[26] As shown in Panel C of Table 2, the effects are insignificant for new firms established by non-local entrepreneurs. The likelihood of entrepreneurs from other provinces even decreases by 3.3% ($= \frac{0.019}{0.573}$), suggesting that the increase in the number of newly established firms are unlikely driven by the entrepreneurs moving to Guangdong from outside the province. Instead, entry deregulation promoted entrepreneurship within the province.

Effect Heterogeneity across Industries and Ownership Types

Our last empirical finding about firm entry is the effect heterogeneity, as administrative entry barriers clearly vary by industry and ownership type. We compute the industry specific registration cost based on the 2018 ESIEC survey data. We first calculate the average self-reported registration cost at the 2-digit-industry-level by the year of registration, and then compute the percentage change in the "cost of entry" for each industry in relation to the reform. Finally, we define a dummy variable $RegCost_i$ as 1 if the industry i is above the median change and 0 otherwise.

Moreover, we create a dummy variable $Private_i$, which takes the value 1 if a firm is registered as privately owned, and 0 otherwise. Other ownership types include the state-owned enterprises (SOE) and foreign, Hong Kong, Macau, and Taiwan invested enterprises (FIE). The registration cost is unlikely a major consideration for the entry of SOEs and FIEs, as they are less financially constrained than private enterprises.

The estimation results are shown in Table 3. The two key outcome variables are the monthly number of entrants and entry rate at the city, industry, and ownership type (private and non-private) level. The variables of interest are the two interaction terms between $Treatment_{ct}$ and $RegCost_i$ in Columns (1) and (3) and between $Treatment_{ct}$ and $Private_i$ in Columns (2) and (4). In general, regressions on the two outcome variables yield similar results. As indicated in Columns (1) and (3), the reform results in 7.4% more entries in industries which experienced a larger reduction in registration cost after the reform. Columns (2) and (4) evaluate the heterogeneity of impacts in ownership. The impact of the reform is 15% higher for private enterprises than for non-private enterprises.

These results provide evidence that entry deregulation greatly promoted the entry of new businesses in the manufacturing sector, especially in more deregulated industries and for private firms.

[26] We define the legal representatives as entrepreneurs of each firm.

Appendix Figure A2 shows that the reform also significantly increased firm entry in the service sector, with even larger impacts. That is, the effect of entry regulation on new businesses goes beyond the manufacturing sector.

4.4 Firm Exit

Our theoretical model highlights how entry deregulation could lead to more intense market competition and a higher exit rate. In this section, we empirically investigate this general equilibrium effect. We leverage two measures of firm exits: one is based on the discontinuation of filing annual reports and the other one is based on license revocation or cancellation as explained in Section 2.3.

Table 4 shows the regression results using the first definition. We keep annual observations for each firm, which allows us to control for firm-level attributes that also affect exit decisions. Results are similar if we collapse data to the city-industry-year level. 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 the average exit rate being 5.7 percentage points prior to the reform, our estimates indicate that the mean exit rate has increased by 8.7% ($= \frac{0.498}{5.7}$) post the reform. The finding is consistent with the theoretical insights that more intense competition after the reform drives firms with low profit margins out of the market, thus raising firm exit rate. The increase in both entry and exit rate implies higher market turnover rate and shorter expected firm lifespan. However, the effect on firm entry still dominates that on firm exit, resulting in a greater number of firms on the market. In Column (2), we further control for firm size, measured by log revenue, which is an important factor in shaping firms' exit decisions. Larger firms are less likely to exit the market, which is consistent with the stylized facts of industrial dynamics (Dunne et al., 1988). After controlling for firm size, the effect drops only slightly to 0.469 percentage points.

Similar to the previous analysis on entry, we also conduct an event study on firm exit. As shown in the Panel B of Figure 4, there is no trend prior to the reform. The effect becomes positive and larger over time after the reform. It's as expected because with more entries, market competition becomes more intense, resulting in a higher exit rate. Similar to firm entry, our results are also robust to the concern of negative weights from heterogeneous treatment effects, as discussed in Appendix C.

To further address the identification concern of the endogenous timing and macroeconomic confounders, we adopt the triple difference strategy. We interact $RegCost_i$ with $Treatment_{cy}$ and control for city by year fixed effects in Column (3). The coefficient for the interaction term is positive and significant, indicating that industries with greater reduction in registration cost experience higher exit rates after the reform. This piece of evidence lends us additional confidence in the causality of the main effect. One might also be concerned that our results are driven by higher exit rates of new entrants that entered after the reform. To address the concern, in Columns (4)-(6), we replicate the baseline results in Columns (1)-(3) but exclude all new entrants. The main effects remain robust, with slightly larger magnitudes.

We repeat our analysis using an alternative exit measure by including the firms revoked by SAIC or having canceled their registrations. As the firm revocation and cancellation records were subject to under-reporting problems in the first couple of years right after the reform, we supplement the data with the updated information until 2019. The results are shown in Table A2 and the empirical specification is kept consistent with Table 4. When using this alternative measure, the coefficient estimate is 0.820 for incumbents, suggesting a larger effect. Given the average exit rate being 4.8% prior to the reform, this estimate indicate that the mean exit rate increases by 17.1% ($= \frac{0.820}{4.8}$) post reform. After controlling for log registered capital, a proxy for firm size, the coefficient slightly increases. Therefore, entry deregulation not only introduces more entrants, but also results in more firm exit, consistent with theoretical predictions.

5 Entry Deregulation, Firm Size and Productivity

This section examines the impacts of the reform on firm size distribution and productivity. As illustrated in Section 3, entry deregulation reduces the cost of entry, inducing a large inflow of new entrants. This in turn results in more intense market competition, leading to a higher exit rate. Market competition further shifts the survival size-threshold rightward and increases average firm size and productivity among exiting firms. Moreover, since entry deregulation enables more firms of smaller size to enter the market, it reduces the average size of entrants.

The impact on entrants' productivity is determined by three counteracting forces. First, lax regulation enables less efficient firms to enter after the reform, therefore driving down the average productivity of entrants (*the weak screening effect*). Second, entry reform introduces more intense competition on the market. Only efficient firms will now find it profitable to enter a more competitive market, thus the average productivity is improved (*the market competition effect*). Third, entry deregulation allows entrepreneurs with limited financial resources to enter the market and changes the composition of potential entrants' pool. These firms with small endowment will have to be more productive in order to enter the market, compared to those with larger endowments. Therefore, a change in the entrants' composition will improve the average efficiency (*the composition effect*). The overall impact on entrants' productivity remains theoretically ambiguous, depending on whether the latter two forces dominate the former.

For the rest of this section, we first explore the impact of the entry deregulation on firm size. Then we estimate firm productivity and investigate the efficiency implications of the reform. Lastly, we disentangle the underlying mechanisms for the productivity gain and decompose the aggregate productivity change.

5.1 Firm Size

We first describe the data pattern of firm size. Panel (a) of Figure 5 displays the distribution of firm revenue in logarithm for the *entrants* before and after the reform. As evident from the figure, the size distribution shifted to the left after entry deregulation. The average revenue of entrants decreased by 18.6% after the reform. Similarly, the average capital also shrank by 21.7%.

Next, we adopt a triple difference strategy to identify causal impacts on the firm size of entrants/exiters, where incumbents are the comparison group to help control for unobserved macroeconomic confounders:

$$y_{jt} = \beta_1 E_{jt} + \beta_2 X_{jt} + \beta_3 D_{ct} E_{jt} + \beta_4 D_{ct} X_{jt} + Z_{jt} + \xi_{cio} + \epsilon_{jt}. \tag{9}$$

The dependent variable y_{jt} is either the log revenue or capital stock of firm j in year t, constructed based on the annual report. This analysis exploits the *yearly* variation in the timing of the reform. We run this analysis at the firm-year level instead of city-industry-period level to control for firm attributes (e.g., ownership type). An entrant E_{jt} is a firm of age one at time t, and an exiter X_{jt} is a firm that exits in the following year.[27] We further interact E_{jt} and X_{jt} with the treatment dummy D_{ct}. Therefore, β_1 (β_2) captures the average size differences between the entrants (exiters) and continuing incumbents before the reform, while β_3 (β_4) captures the impact of entry deregulation on the size differences. We include a rich set of controls, and the key identification assumption is that the size differences between the entrants (exiters) and continuing incumbents would have followed the same trend in treated units and comparison units, had the reform not taken place.

Similar to the analysis on entry/exit, we use event studies to examine the dynamic impacts of entry deregulation on firm size differences:

$$y_{jt} = \beta_1 E_{jt} + \beta_2 X_{jt} + \sum_{q=-4}^{3} \beta_{3q} D_{ct}^q E_{jt} + \sum_{q=-4}^{3} \beta_{4q} D_{ct}^q X_{jt} + Z_{jt} + \xi_{cio} + \epsilon_{jt}, \tag{10}$$

where the year before the reform serves as the baseline group.

Table 5 reports the regression results. The firm size measure is log revenue in Columns (1)-(3) and log capital in Columns (4)-(6). Column (1) controls city by industry, city by year, and industry by year fixed effects, similar to our model specification for firm entry/exit. Column (2) and (3)

[27] A firm's age is 0 in the year when it enters the registration database. We use its size measures in the calendar year immediately after the registration year so that we observe it for an entire year.

take more stringent specifications by further interacting with ownership types and controlling for the time trend in each industry and ownership type. The coefficient estimates in Columns (2) and (3) are qualitatively similar to those in Column (1). Entrants and exiters are smaller than continuing incumbents before the reform, consistent with the stylized facts in industrial dynamics (Dunne et al., 1988). After the reform, entrants becomes smaller, while exiters becomes larger. Although both entrants and exiters are still smaller than incumbents after the reform, the size differences between incumbents and entrants have increased by 15.3% ($=\frac{0.138}{0.900}$), while the size differences between incumbents and exiters have shrunk by 35.9% ($=\frac{0.299}{0.900}$). Compared with continuing incumbents, entry deregulation reduces the size of entrants but increases the size of exiters, which corroborates our model predictions.[28]

Figure 6 illustrates the deregulation's dynamic effect through an event study. There are no significant pre-trends for either entrants or exiters, which justifies our empirical strategy. The effects of the entry deregulation on the size of entrants and exiters are both reinforced over time, though in opposite directions.

Table A3 investigates how the entry deregulation affects firm growth rates. Compared with incumbents, entrants on average have higher growth rates. After the reform, the difference in the growth rate widens by 11.8% ($=\frac{0.013}{0.110}$). Apparently lower entry barriers as a result of the reform enable a group of small yet high potential entrants to enter the market.

5.2 Firm Productivity

We next evaluate the efficiency implications of entry deregulation. We estimate firm productivity using a stylized structural model, following Aw et al. (2011) and Peters et al. (2017).[29] To examine the robustness of our findings to alternative measure of productivity, we use the revenue-over-capital ratio (in logarithm) as a data-driven measure of productivity, and follow Olley and Pakes (1996) (henceforth, OP), Levinsohn and Petrin (2003) (henceforth, LP) as well as Brandt et al. (2021) to construct alternative measures of firm productivity.[30] We include an extended discussion

[28] The aforementioned concern over heterogeneous treatment effects in the staggered DID setting are also present in our triple difference estimates. We check the robustness of our results using two sub-samples where the treatment is 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 group 1 are treated in 2012 while group 3 are never treated during the sample period. We find the results to be similar to those in Table 5. In the second robustness check, we drop group 1 and only keep observations before 2014, thus group 2 are treated in 2013 while group 3 are never treated during the sample period. We find similar effects on entrants, while the effects on exiters are negative but statistically insignificant. The results are consistent with the fact that it may take longer time for the effects on exiters to materialize.

[29] Aw et al. (2011) and Peters et al. (2017) both use dynamic models, but for the purpose of our paper, we abstract away from the dynamic considerations and use the static model to back out firm productivity.

[30] We construct OP/LP productivity estimates with a shorter sample period when we have the labor input information. Brandt et al. (2021) use similar data as ours and exploit different model assumptions to estimate firm productivity.

of model, identification, and estimation for our baseline productivity measure in Appendix E. For detailed information about these alternative productivity measures, please refer to Appendix F.

We summarize key data patterns in Figure 5. Panel (b) illustrates differences in new entrants' productivity pre- and post-reform. The productivity distribution has shifted to the right after the reform, which amounts to a 4.2% increase in the average productivity of new firms. Similar patterns also hold for alternative measures of firm productivity, such as the revenue-over-capital ratio and other measures. Panel (c) of Figure 5 plots productivity against firm size among new entrants. Almost all of the improvement in entrants' productivity after the entry deregulation can be attributed to entrants that are small. Since small entrants were more likely to be constrained by entry barriers in the first place prior to the reform, lowered entry barriers bring about higher productivity gains for them. It's also consistent with the theoretical model that small entrants with less endowment might enjoy larger efficiency improvement because of a change in entrants composition.

We follow the same empirical specification as Equation (9), where y_{it} is either the baseline productivity estimate or log revenue-over-capital ratio. The main empirical results are shown in Table 6. Compared with continuing incumbents, entrants and exiters have lower productivity before the reform. However, the average productivity of both entrants and exiters have increased after the entry deregulation. Entrants exhibit a 1.3% increase in their average productivity, while exiters' productivity rises by 3%. The reform decreased the productivity gap between entrants and continuing incumbents by 26.5% ($= \frac{0.013}{0.049}$), and that between exiters and continuing incumbents by 49.2% ($= \frac{0.030}{0.061}$). Although lax entry regulation and *the weak screening effect* allow entrants of lower productivity into the market, the result reveals that *the market competition effect* and *the composition effect* collectively dominate *the weak screening effect*, leading to higher average productivity for new entrants.[31]

The event study in Panel (c) and (d) of Figure 6 echos the regression results. While there is no pre-trend for either entrants and exiters, there is a significant improvement in entrant productivity after the deregulation and the effect strengthens over time. The productivity among exiters also increases over time, reflecting the stronger competition associated with a larger number of firms after the reform.

We implement a series of robustness checks. Columns (4)-(6) of Table 6 repeat the analysis using the log revenue-over-capital ratio as an alternative measure of productivity which is data-

[31] We conduct similar robustness checks for heterogeneous treatment effects in the staggered DID setting as in Footnote 28. City groups are also similarly defined. In the first robustness check, we drop group 2 and only keep observations before 2014, thus group 1 are treated in 2012 while group 3 are never treated during the sample period. We find exiters become more productive, though the effects on entrants are insignificant. In the second robustness check, we drop group 1 and only keep observations before 2014, thus group 2 are treated in 2013 while group 3 are never treated during the sample period. We find entrants being more productive, while the effects on exiters are small and insignificant. The results are consistent with our prior that it may take longer time for the effects on exiters to materialized.

driven. Entrants after the reform have 21-22% higher revenue-over-capital ratios. Second, we restrict our sample after 2013 and estimate productivity using The OP and LP methods. The OP method uses investment as a proxy for productivity, while the LP method uses intermediate input instead. Table F1 replicates Table 6 using the OP and LP productivity estimates as the alternative outcomes. Entry deregulation increases productivity for both entrants and exiters, therefore narrowing the productivity gap between entrants and continuing incumbents by 19.3% (=21.5% using OP), and that between exiters and continuing incumbents by 48.2% (=57.7% using OP). Third, we follow Brandt et al. (2021) to estimate firm productivity relative to the average of all firms in a province-industry-year cell, which is valid under the assumption that firms within the cell face the same wage rate. The empirical results are robust as shown in Table F2. Fourth, we use dummies of firm capital quintiles to flexibly control for firm size, and our empirical results remain stable.

Having found that entry deregulation improves the productivity for new businesses in the manufacturing sector, we next examine whether the effect persists over time. Figure A4 plots the average revenue and revenue-weighted average productivity for the treated and untreated cohort by firm age. The average firm size of treated cohort is persistently lower than the untreated cohort throughout all three years after the registration year, which is the longest time we could follow an entrant. On the contrary, the productivity of treated cohort is persistently higher, and the efficiency improvement is stable at least up to age three.

Our main analysis limits to the manufacturing industry primarily because we do not have data to reliably estimate productivity for the service industries. However, using the revenue-over-capital ratio as a proxy, Figure A5 shows that the reform also significantly improves entrants' productivity in some major service industries, including the wholesale trade, retail trade, business service, and real estate. These patterns provide suggestive evidence that the effect of entry regulation on entrants' productivity goes beyond the manufacturing sector as well.

Finally, a natural question is how the entry deregulation affects the productivity of continuing incumbents. We focus on the incumbents in the 2010 Annual Report Database so that the sample does not include new entrants, and utilize the within-firm variation to explore how the incumbent's productivity changes after the reform. We also check the alternative sub-samples where incumbents are allowed only to miss one-year annual report or up to three annual reports.[32] Table A4 presents results using different productivity measures and samples. The results do not suggest a clear and strong impact on incumbents. Given the entry rate being around 20% after the reform, it might take a few years for the reform to significantly change the incumbents' pool. As we only observe

[32]We define the full set of annual reports as from the year when firm age is one to the year before firm exit the market. If the number of annual reports is equal to the total number of annual reports the firm needs to file, we define that the firm has no missing annual report. Zeros or missing values in firm revenue and capital, as well as missing values in firm tax will all lead to missing productivity estimate that year, thus are defined as missing annual reports here.

data for 2-3 years after entry deregulation, the sample period is not long enough to detect the slow composition change for incumbent firms.

5.3 Channels of Impacts on Entrants' Productivity

This section aims at disentangling potential channels underlying the improvement of entrants' productivity.

The Market Competition Effect

The first channel is market competition. More intense market competition may lead to lower profitability and deter inefficient firms from entering in the first place. In order to separate the impacts of market competition from other channels directly linked to entry barriers, we categorize entrants into two groups: those that entered immediately before the reform and those that entered after the reform. The latter group experienced the drop in entry cost upon registration, while the former group did not. Let's consider an example of two firms *a* and *b* registering in city X that implemented the entry reform in June 2013. Suppose firm *a* entered the market in May while firm *b* registered in July. In 2014, the performance of both firms reflect the intense competition post entry, but only firm *b* benefited from the lower entry barriers. The coefficient on firm *a* thus solely capture the impact of market competition.

$$y_{jt} = \beta_1 E_{jt} + \beta_2 D_{ct} E_{jt}^{pre} + \beta_3 D_{ct} E_{jt}^{post} + Z_{jt} + \xi_{cio} + \epsilon_{jt}. \tag{11}$$

The model specification is similar to Equation (9), but we replace the entrants dummy E_{jt} with two dummy variables defined according to whether the registration of the entrant occurred before the reform or not. E_{jt}^{pre} is the dummy variable of whether entrant j entered before the reform, and E_{jt}^{post} is the dummy variable indicates whether entrant j entered after the reform. $E_{jt} = E_{jt}^{pre} + E_{jt}^{post}$. β_2 captures only the market competition channel, while β_3 represents the compound effect of three channels aforementioned.

The empirical results are shown in Table 7. Column (1) examines the impact on firm size. The coefficient estimate on entrants that entered before the reform is negative but insignificant. Column (2) examines firm productivity. Both groups of entrants experienced higher productivity, though entrants that registered after the reform have larger gains in productivity. More importantly, the impact on the productivity of entrants who entered before entry deregulation is also positive and significant. This suggests that market competition is one channel through which entry deregulation improves entrants' productivity.

The Composition Effect

Another channel which could improve entrants' productivity is *the composition effect*. Entry deregulation changed entrants' composition, and allowed financially constrained potential entrepreneurs to enter who would otherwise be precluded due to their limited resources. These entrants with smaller endowment have to be more productive than their larger counterparts in order to overcome the entry barriers. As the share of more productive entrants increases, the overall productivity goes up. In this section, we find that *the composition effect* due to the easing of financial constraints is another important mechanism underlying the productivity improvement. We provide three sets of evidence to support it. First, we explore how the source of funds to start the business changes before and after the reform. Second, we examine changes in the composition of new entrants with respect to entrepreneurial characteristics and shareholding structures. Third, we explore the effect heterogeneity on entrants' productivity across industries, ownership types, and locations.

Source of Funds We provide suggestive evidence about the change in the source to fund the start-up costs of new businesses, leveraging the ESIEC survey data from six provinces in 2018. The results are shown in Panel A of Table 8. We find new entrepreneurs are less likely to borrow from friends/relatives and take loans from financial institutions to cover the cost of establishing new businesses after entry deregulation. As all the provinces other than Guangdong carried out the reform simultaneously, we cannot conduct a DID analysis using the ESIEC surveys. However, the before-after comparison provides suggestive evidence that firms are less reliant on external resources to fund the entry cost after the entry deregulation, therefore entry deregulation eases the financial constraints faced by potential entrants.

Entrepreneurs' Characteristics and Shareholding Structures We explore how entry deregulation changed the profile of entrepreneurs based on characteristics of firms' legal representatives, following the same empirical specification in Equation (7). Panel B of Table 8 presents the results. After the reform, the entrepreneurs' average years of schooling drop by 1.9% ($= \frac{0.198}{10.557}$) and the proportion of entrepreneurs with secondary education and above reduces by 6.3% ($= \frac{0.041}{0.646}$). In addition, the share of entrepreneurs younger than 35 increases by 9.0% ($= \frac{0.028}{0.310}$).[33] Our findings provide suggestive evidence that the entry deregulation opens up opportunities to less educated and younger entrepreneurs, who were more likely to be financially constrained.

We next investigates the shareholding structure of new firms. There are two major types of shareholders: individual shareholders (natural persons) and corporate shareholders (legal persons). As shown in Panel C of Table 8, the reform increased the share held by individuals increases by

[33] Note that only 1/3 and 1/4 of the sample report the information of the age and education for their legal representatives.

2.3%. The number of firms with sole-proprietorship (i.e., owned by natural persons) goes up by 2.0%. Therefore, individual shareholders have played a greater role after deregulation in terms of the actual shares owned. Since individual shareholders likely have lower capital than corporate shareholders, the rise in the importance of individual shareholders again implies that entry deregulation has relaxed the financial constraints faced by entrepreneurs and changed the composition of new entrants.

Effect Heterogeneity Our last analysis investigates the heterogeneous effects on entrants' productivity in Table 9. Columns (1)-(2) divide the sample into two groups based on the registration cost reduction at the industry level. "High" reduction group include the industries in which the relative decrease of the registration cost is above the median. The results suggests that entrants in industries with "High" reduction of registration costs experience a greater productivity gain after the reform. Columns (3)-(4) show that the effect is primarily driven by private firms instead of SOEs or foreign invested enterprises. Both regressions suggest that financially constrained entrants are more productive after the reform, and these financially disadvantaged entrepreneurs contribute most to the efficiency gain.

We further explore the effect heterogeneity along other dimensions in Figure 7. We find limited heterogeneity across industries with different ratios of fixed asset to revenues, and the share of SOEs in the industry in 2009. However, the reform has more pronounced impacts in industries with lower export revenue shares and higher ratios of registered capital to capital stock. Domestic firms and those faced with higher registered capital are more likely to be financial constrained by the entry barriers, thus enjoy a larger productivity gain after the reform. Moreover, counties with a lower number of bank branches, which indicates a lack of capital market access, witness a much larger gain in entrants' productivity. Therefore, the productivity gain for entrants stems disproportionately from sub-samples that experience a larger degree of relaxation of financial constraints.

5.4 Macro Implications

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

$$\Delta\Phi_t = \underbrace{\sum_{i\in C}\bar{\theta}_i\,\Delta\varphi_{it} + \sum_{i\in C}(\overline{\varphi_i} - \overline{\Phi})\,\Delta\theta_{it}}_{Incumbents} + \underbrace{\sum_{i\in N}\theta_{it}\left(\varphi_{it} - \overline{\Phi}\right)}_{Entrants} - \underbrace{\sum_{i\in X}\theta_{it-1}\left(\varphi_{it-1} - \overline{\Phi}\right)}_{Exiters}, \tag{12}$$

where φ_{it} denotes the productivity of firm i in year t, and θ_{it} represents the revenue share of firm i in year t in each city-industry-year cell. Φ_t is the aggregate productivity in year t weighted by the revenue share. $\bar{Z}$ represents the average of the variable Z between the period t and $t-1$. Furthermore, C, N and X denote the set of incumbents, entrants and exiters respectively. There are four terms in Equation (12), which represent the within-firm effects and between-firm effects for continuing incumbents, as well as the contribution from entrants and exiters. The sum of the first two terms represent the contribution of incumbents.

We decompose annual aggregate productivity using firms in all cities and industries and plot the entrants' contribution to the aggregate TFP change in Figure 8. The contribution of entrants to the overall productivity growth has turned from being negative before the reform to being positive after the reform. Entrants contribute to 15.1% and 23.8% of the overall productivity change in 2013 and 2014, respectively. That is, entrants become a more powerful source of growth after entry deregulation, which corroborates the insights of Asturias et al. (2021). Moreover, the impact of exiters is smaller than entrants.

Implications for the Aggregate Economy We conduct a back-of-the-envelope calculation on the overall economic benefits of the reform. Prior to the reform, the average entry rate and exit rate is 14.7% and 5.7%, respectively. The reform is associated with a 25% increase in the entry rate and a 8.7% increase in the exit rate. This leads to a net 21.6% increase in the number of firms each year. The annual number of new firms in Guangdong's manufacturing sector is 42,000 in 2009-2012. Given that new firms on average employ 12 workers and generate a revenue of ¥3 million, a 21.6% increase in the net firm entry translates into 9,072 new firms, 0.11 million new employment, 27.2 billion increase in revenue, and ¥5.4 billion of additional value added annually. This is equivalent to 1.8% of the total employment and 1.7% of the total value added in the manufacturing sector in 2014. These estimates capture the extensive margin of the economic growth as a result of the entry regulation.[34] Extrapolating Guangdong's estimates nationwide, the entry deregulation could have potentially created 58.3 thousand new firms annually in the manufacturing sector, leading to 699.6 thousand new jobs and ¥35 billion of value added per year.

6 Conclusion

This paper provides a comprehensive analysis on the impact of entry deregulation on firm turnover, size distribution and productivity in China using the staggered roll-out of entry deregulation in Guangdong province as an identification strategy. Consistent with the predictions from our theo-

[34]Guangdong's manufacturing sector employs 5.97 million workers and creates ¥349.65 billion of value-added in 2014.

retical model, the empirical analysis finds significant impacts of entry deregulation along multiple margins: firm entry, exit, size, and productivity. Entry deregulation boosts firm entry by around 25% and the impact persists over time. Firm exit rate increases by 8.7%. Furthermore, Compared with continuing incumbents, new entrants are relatively smaller after the reform, while exiting firms have become larger. Based on the structurally estimated productivity measure, we find that the productivity of new entrants is higher after the deregulation. As a result, new entrants contribute 15.1% and 23.8% to the overall productivity growth in 2013 and 2014, respectively.

To interpolate the findings from the pilot program in Guangdong province to the nationwide reform in 2014, additional new firms as a result of entry deregulation would increase employment by 0.70 million and value-added by ¥35 billion in the manufacturing sector at the national level per year. In addition, the entry regulation is associated with a 1.3% *persistent* improvement in productivity among new entrants after the reform. Given the high turnover rate among firms, most of the active firms in a few years would be those firms entering the market after the reform.

We conclude with a few directions for future research. First, the weaker screening from entry deregulation could lead to low-quality products and services, which would have negative implications especially in the presence of information asymmetry and spillovers. How entry regulation affects the overall consumer welfare calls for structural analysis and is left for future works. Second, it would be interesting to investigate the important trade-off from entry deregulation between the improved market competition versus wasteful sunk entry costs from market cannibalization as highlighted in Mankiw and Whinston (1986). Lastly, entry deregulation could have impacts on firm investment and R&D decisions due to changes in the competitive landscape. These are important questions to be answered.

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. (2021). Firm entry and exit and aggregate growth. *American Economic Journal: Macroeconomics*. Forthcoming.

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.

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

Barseghyan, L. and DiCecio, R. (2011). Entry costs, industry structure, and cross-country income and tfp differences. *Journal of Economic Theory*, 146(5):1828–1851.

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. (2021). Revisiting event study designs: Robust and efficient estimation. Working paper.

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. (2020). Difference-in-differences with multiple time periods. *Journal of Econometrics*.

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

Dai, R., Liu, X., and Zhang, X. (2020). Detecting shell companies in china. Paper presented at the ASSA Annual Meeting, San Diego, January 4.

De Chaisemartin, C. and d'Haultfoeuille, X. (2020a). Difference-in-differences estimators of intertemporal treatment effects. *Available at SSRN 3731856*.

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

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. (2021). Entry barriers, idiosyncratic distortions, and the firm size distribution. *American Economic Journal: Macroeconomics*. Forthcoming.

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*.

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

Helpman, E., Melitz, M. J., and Yeaple, S. R. (2004). Export versus fdi with heterogeneous firms. *American Economic Review*, 94(1):300–316.

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.

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.

Melitz, M. (2003). The impact of trade on aggregate industry productivity and intra-industry reallocations. *Econometrica*, 71(6):1695–1725.

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.

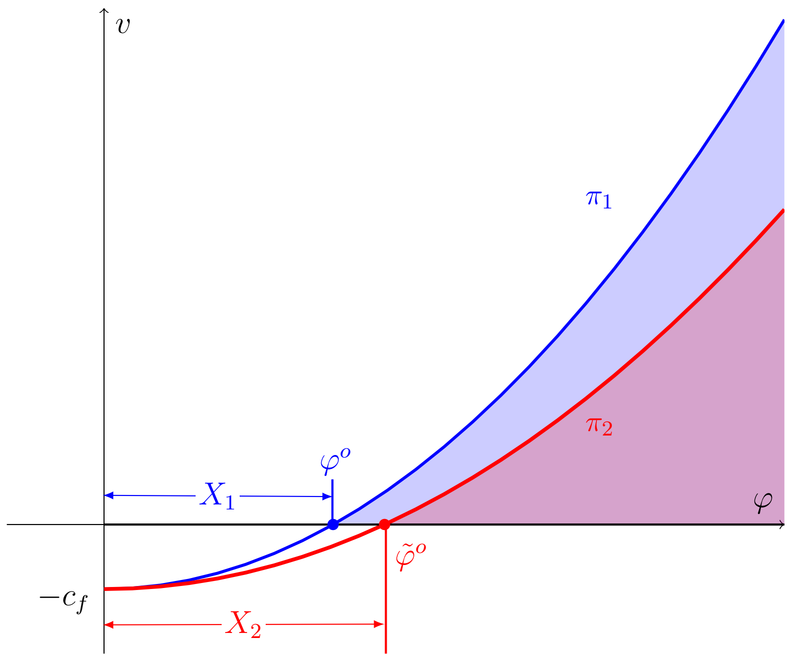
Sun, L. and Abraham, S. (2020). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*.

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

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.

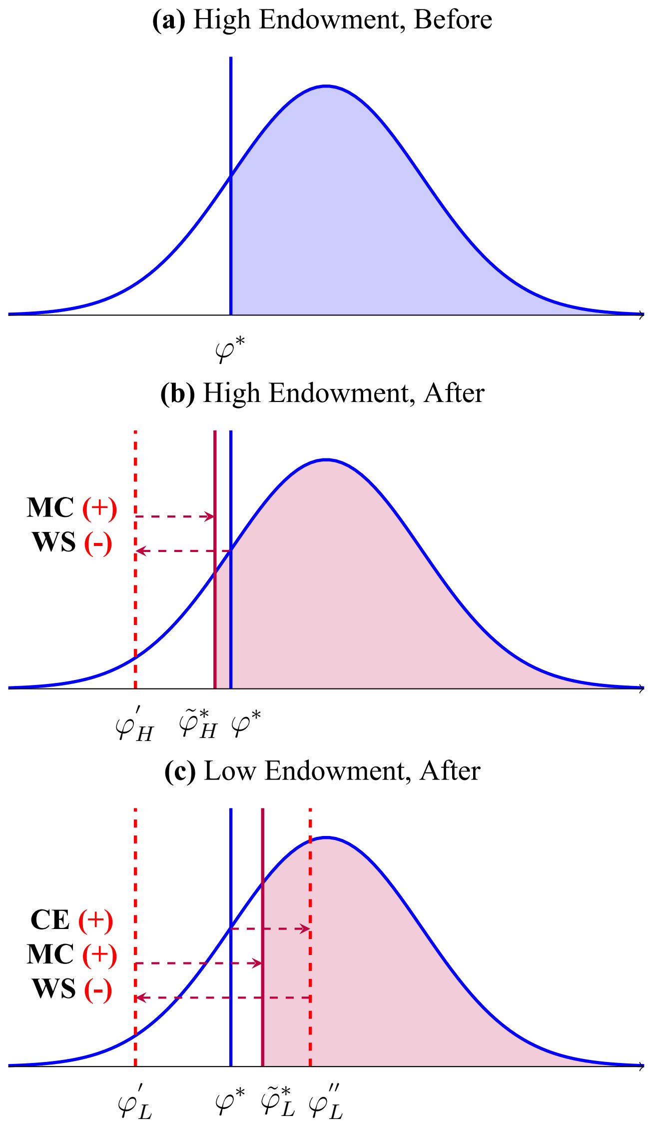
Figures

Figure 1: Theoretical Predictions on Incumbents



Notes: This figure illustrates the theoretical impacts of entry deregulation on incumbents. Please refer to Section 3 for model details. The horizontal axis denotes the productivity φ, while the vertical axis denotes the firm value v. The blue red curves represent the profit function before and after the reform respectively. φ^o and $\tilde{\varphi}^o$ indicate productivity cutoff for firm exit, while X_1 and X_2 indicate the mass of exiting firms. c_f represents the fixed cost to operate in the market. Entry deregulation allows more firms to enter the market. More intense market competition decreases firm values at each productivity level, raises the average productivity and size for exiting firms, and results in more exit.

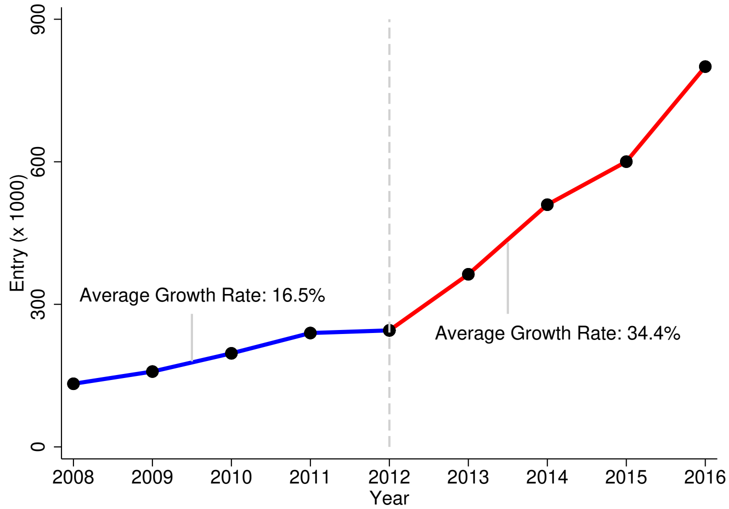
Figure 2: Theoretical Predictions on Entrants



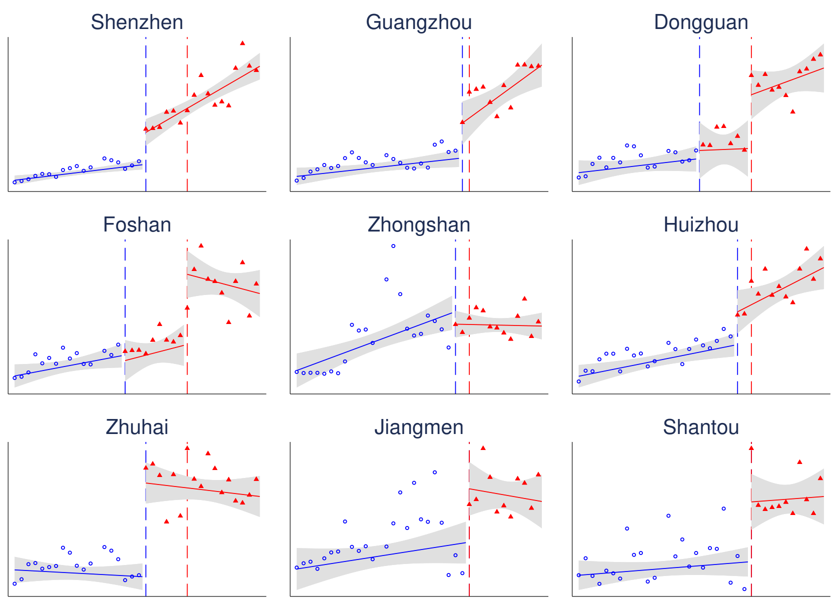
Notes: This figure illustrates the theoretical impacts of entry deregulation on entrants. Please refer to Section 3 for model details. The blue curve shows the productivity distribution of potential entrants. The blue/red solid vertical line represents the productivity threshold before/after the reform, and the red dashed vertical line represents the counterfactual productivity thresholds. φ^* denotes the entry threshold before the reform. $\tilde{\varphi}^*_H$ and $\tilde{\varphi}^*_L$ denote the entry threshold after the reform for entrants with high endowment and low endowment respectively. $\varphi^{'}_H$ and $\varphi^{'}_L$ denote the entry threshold after the reform had the equilibrium wage stayed constant, while $\varphi^{''}_L$ denotes the entry threshold before the reform for the low type had the government allowed them to enter. "WS" represents *the weak screening effect*, "MC" represents *the market competition effect*, and "CE" represents *the composition effect*. For entrants with high endowment, entry deregulation increases the average firm productivity if *the market competition effect* dominates *the weak screening effect*. For entrants with low endowment, the average firm productivity increases if the sum of *the market competition effect* and *the composition effect* dominates *the weak screening effect*.

Figure 3: Number of New Entrants in Guangdong Province

(a) Total



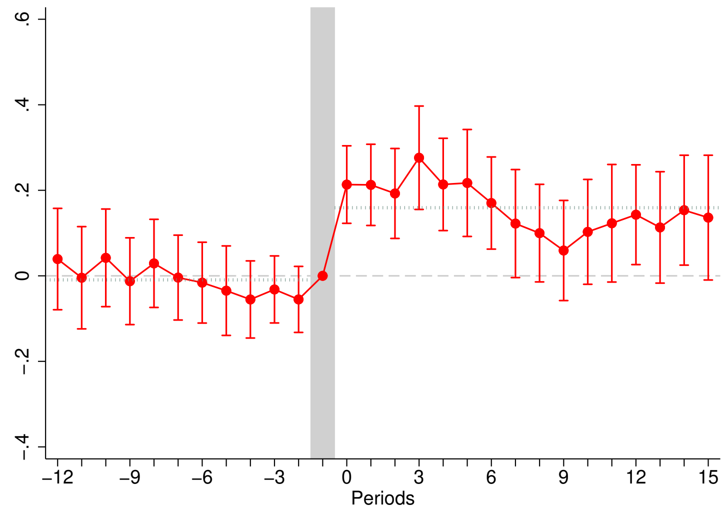
(b) Nine Largest Cities



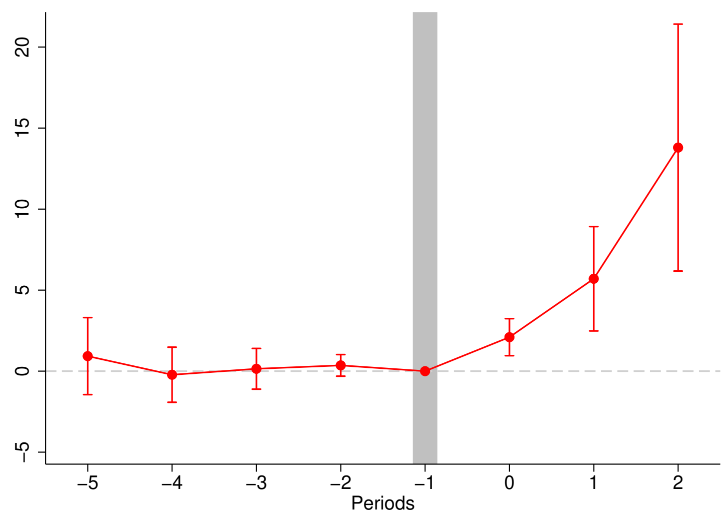
Notes: Panel A shows the time trend of firm entry for the entire Guangdong province. The blue/red segment denotes the series before/after the reform. Panel B depicts monthly trend of firm entry for in nine cities with most incumbent firms. The blue dashed vertical line represents the start month of pilot reform and the red vertical line represents March 2014 when the reform was implemented nationwide. The blue and red dots are data points before and after the (pilot) reform, respectively. Linear fits and 95% confidence intervals are added in Panel (b).

Figure 4: The Effects on Firm Entry and Exit: Event Study

(a) Entry

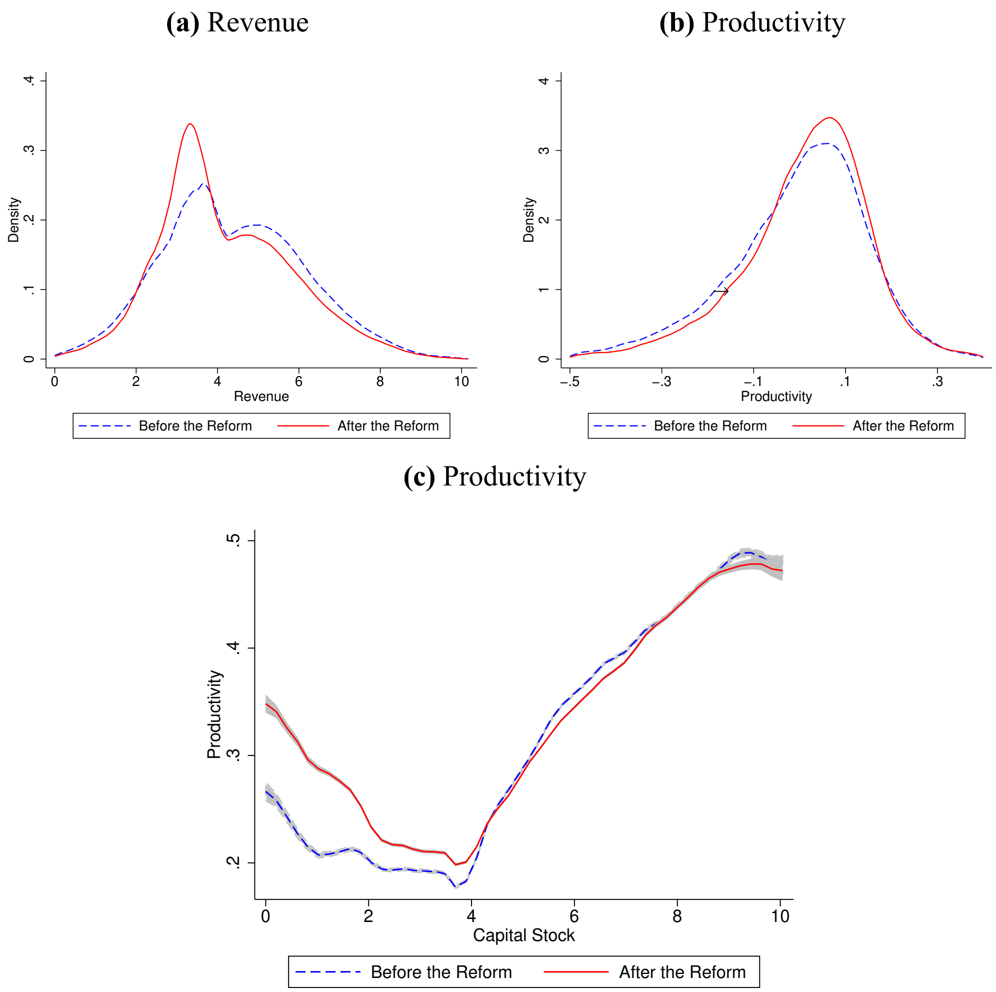


(b) Exit



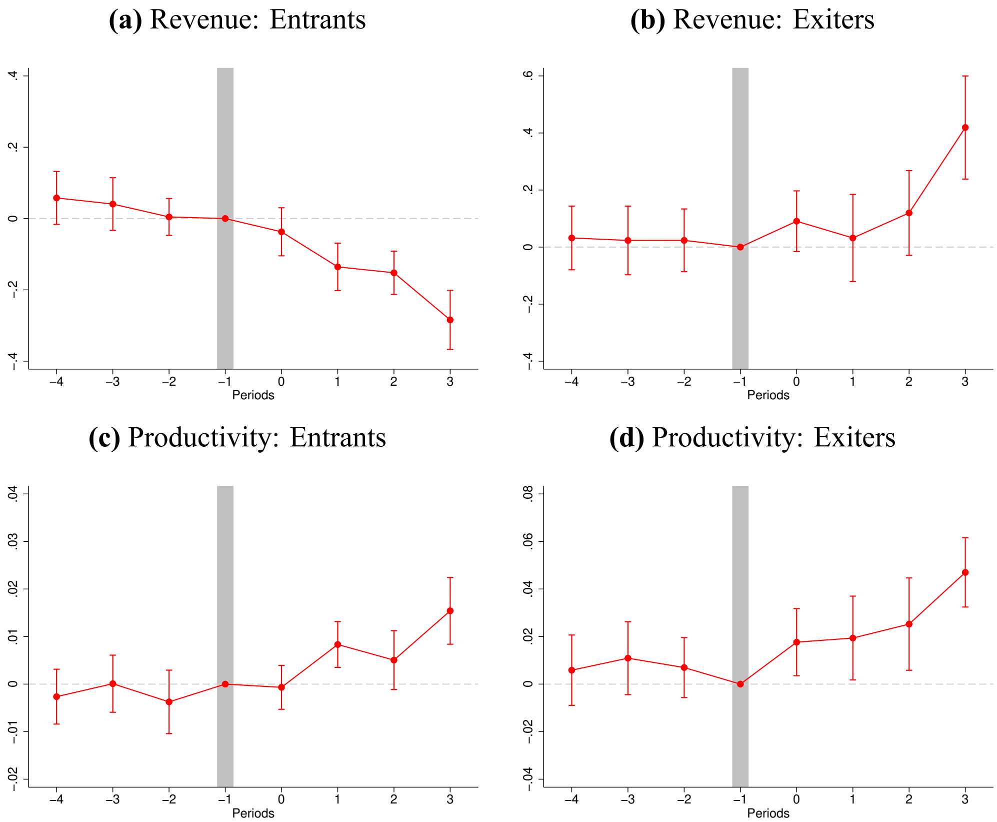
Notes: This figure shows the event study results for firm entry and exit as shown in Equation (8). Panel A shows the event study results on firm entry, where the observation is at the city-2-digit-industry-month level for each two-digit manufacturing industry from Jan 2009 to Dec 2016. The identification relies on the monthly variation in the reform timing. The horizontal axis denotes months before and after the reform, and the vertical axis denotes the DID coefficients. Panel B shows the event study results for firm exit, where the observation is at the firm-year level in 2009-2015. The identification relies on the yearly variation in the reform timing. Firm exit is a dummy variable which equals 1 if the firm stops submitting annual reports, further multiplied by 100. The horizontal axis is in the unit of year, and the vertical axis denotes the DID coefficients. 95% confidence intervals are constructed from clustered standard errors at the city and industry level.

Figure 5: Entrants' Size and Productivity Distribution



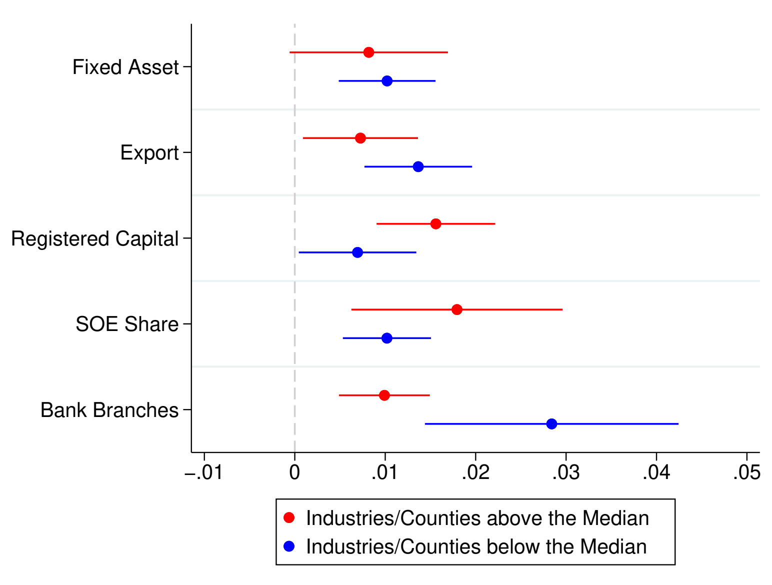
Notes: This figure describes the size and productivity, as well as their relationship for new entrants in 2008-2016. New entrants are defined as firms with age one. Panel (a) and (b) show kernel density of entrants' revenue and productivity for the sub-sample before (dashed blue curve) and after (solid red curve) the reform respectively. Firm revenue is in logarithm. We partial out a cubic function of log capital from productivity to construct Panel (b). Panel (c) shows the local polynomial fit of entrants' productivity with respect to the capital stock, for the sub-sample before (dashed blue curve) and after (solid red curve) the reform respectively. 95% confidence intervals are also included.

Figure 6: The Effects on Firm Size and Productivity: Event Study



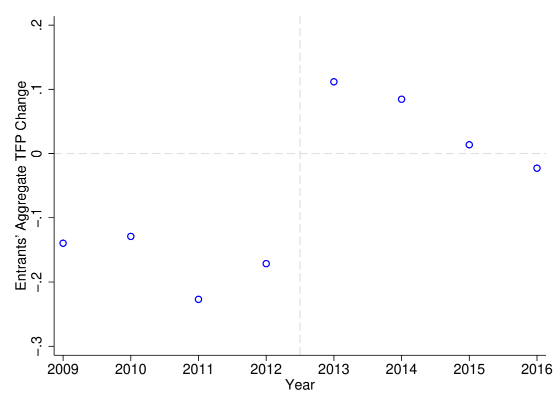
Notes: This figure shows the event study results on firm size and productivity, for entrants and exiters respectively as specified in Equation (10). Entrants are defined as firms with age one, and exiters are defined as firms who exit next year. The observation is at the firm-year level in 2008-2015, and the yearly variation in the reform timing is used for identification. The horizontal axis is in the unit of year, and the vertical axis denotes the triple-difference coefficients. Panel (a) and (b) show the event study results on firm revenue for entrants and exiters, where firm revenue is in logarithm. Panel (c) and (d) show the event study results on firm productivity for entrants and exiters. 95% confidence intervals are constructed from clustered standard errors at the city and industry level.

Figure 7: The Heterogeneous Effects on Entrants' Productivity



Notes: This figure shows the heterogeneous effect on entrants' productivity. We construct five variables: 1) the ratio of fixed asset to revenue at the 2-digit industry level; 2) the export share in the total revenue at the 2-digit industry level; 3) the ratio of registered capital to capital stock at the 2-digit industry level; 4) the SOE share among all incumbents at the county level; 5) the number of bank branches in logarithm at the county level. The first three variables are constructed using the National Tax Survey Database, the fourth from the Business Registry Database, and the fifth from the Branch Database of Chinese Commercial Bank. We further divide the sample into two according to the median (industry-level median for the first three and county-level median for the last two), and run the regression following Equation (9). We plot the coefficient estimates on the triple difference for entrants. 95% confidence intervals are constructed from clustered standard errors at the city and industry level.

Figure 8: Productivity Decomposition: Entrants



Notes: This figure plots the contribution of entrants in the aggregate TFP change. We follow Foster et al. (2008) and Griliches and Regev (1995) as shown in Equation (12) for decomposition. The numbers are scaled by 100.

Tables

Table 1: The Effects on Firm Entry

		Entry			Entry Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Total Entry						
Treatment	0.212***	0.259***	0.245***	0.221***	0.262***	0.248***
	(0.043)	(0.043)	(0.037)	(0.042)	(0.043)	(0.037)
Observations	53,709	53,709	53,709	53,709	53,709	53,709
Adjusted R^2	0.941	0.943	0.950	0.689	0.699	0.733
Panel B: Non-Shell Entry (Ever Filed Annual Report)						
Treatment	0.233***	0.276***	0.260***	0.239***	0.268***	0.253***
	(0.041)	(0.044)	(0.036)	(0.040)	(0.043)	(0.036)
Observations	51,729	51,729	51,729	51,729	51,729	51,729
Adjusted R^2	0.945	0.947	0.955	0.664	0.675	0.716
Panel C: Non-Shell Entry (Ever Reported Positive Capital Stock)						
Treatment	0.223***	0.271***	0.249***	0.229***	0.263***	0.242***
	(0.047)	(0.048)	(0.036)	(0.045)	(0.048)	(0.036)
Observations	51,458	51,458	51,458	51,458	51,458	51,458
Adjusted R^2	0.944	0.946	0.954	0.688	0.699	0.737
City-Industry, Time FE	✓	✓	✓	✓	✓	✓
City-Year FE	✓	✓	✓	✓	✓	✓
City-Month FE		✓	✓		✓	✓
Industry-Time FE			✓			✓

Notes: This table shows the baseline results for the impacts on firm entry as specified in Equation (7). The observation is at the city, 2-digit manufacturing industry, and month level, with the sample period from Jan 2009 to Dec 2016. The dependent variable of Columns (1)-(3) is the number of new entrants in each city, industry, and month in logarithm. The dependent variable of Columns (4)-(6) is the log of entry rate, which is defined as the ratio of entrants to lagged incumbents in logarithm. In panel A, we calculate the number of entrants and incumbents using firm registration information, and define new entrants as those who got newly registered. The lagged incumbents are firms operating until last year, with firm exit defined as being revoked or cancelled. In panel B, we calculate the number of entrants and incumbents using firm annual report data, and define new entrants as newly registered ones who has ever filed annual reports. Firm exit is defined as the incumbents who terminated annual report. In panel C, we calculate the number of entrants and incumbents using firm annual report data, and define new entrants as newly registered ones who has ever reported positive capital stock. Firm exit is defined as the incumbents who terminated annual report. Treatment is a dummy variable, which takes the value 1 if the month is post the reform in that city and 0 prior to the reform. We weight the regression using the volume of incumbents for each city-industry pair at the beginning of sample periods. We include city-industry FE, time FE, city-year FE, city-month FE, and industry-time FE according to different model specifications. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 2: The Effects on Firm Entry: Alternative Channels

Panel A: Spin-off			
	Serial Entrepreneurs		
	Any Ind. Investor		Largest
Treatment	0.004	0.005	0.011
	(0.008)	(0.009)	(0.008)
Mean of Dep Var	0.322	0.322	0.277
Observations	27,639	27,639	27,569
Adjusted R^2	0.275	0.350	0.321
City-Industry, City-Year, Time FE	✓	✓	✓
City-Month, Industry-Time FE		✓	✓
Panel B: Reclassification			
	Getihu Entry		
Treatment	0.166***	0.201***	0.195***
	(0.030)	(0.045)	(0.032)
Observations	49,920	49,920	49,920
Adjusted R^2	0.911	0.913	0.936
City-Industry, City-Year, Time FE	✓	✓	✓
City-Month FE		✓	✓
Industry-Time FE			✓
Panel C: Relocation			
	Entry from		
	Other County	Other City	Other Province
Treatment	-0.002	-0.008	-0.019**
	(0.006)	(0.009)	(0.009)
Mean of Dep Var	0.867	0.761	0.573
Observations	24,232	24,232	24,232
Adjusted R^2	0.232	0.389	0.237
City-Industry, City-Year, Time FE	✓	✓	✓
City-Month, Industry-Time FE	✓	✓	✓

Notes: This table examines three alternative explanations of our baseline results on firm entry, including the spin-off of existing firms, reclassification from existing informal businesses (*getihu*, or mom-and-pop operations), and spatial relocation of existing firms from other areas. Please see Table 1 for the model specification and variable definitions. In Panel A, the observation is at city, industry, and month level with the dependent variable being the frequency of any (or the largest) individual investor had registered another firm before (i.e., the investor is a serial entrepreneur), constructed from firms' shareholding structure data in 2009-2016. In Panel B, the observation is at city, industry, and month level with the dependent variable being log of *Getihu* entry in 2009-2016. In Panel C, the observation is at city, industry, and month level, and the dependent variables are the frequencies of the entrepreneur from a county, city or province different from the location of the firm. The dependent variables are constructed from firms' entrepreneurial characteristics information in Jan 2009-May 2015. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 3: The Effects on Firm Entry: Heterogeneity

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

Notes: This table examines the effect heterogeneity on firm entry. The observation is at the city, industry, ownership type (private or non-private), and month of year level, with the sample period from Jan 2009 to Dec 2016. The dependent variable of Columns (1)-(2) is the log number of new entrants in each city, industry, and month. The dependent variable of Columns (3)-(4) is the log entry rate, which is defined as the ratio of entrants to lagged incumbents (%). Treatment is a dummy variable, which takes the value 1 if the month is post the reform in that city and 0 prior to the reform. RegCost is constructed from 2018 ESIEC survey data, which is the dummy variable taking the value 1 if the industry experienced high decrease in the reported registration cost. Private is a dummy variable which takes the value 1 if firm is registered as private-owned, 0 if it's an FIE or SOE. We weight the regression using the volume of incumbents for each city-industry-owner cell at the beginning of sample periods. We include city-industry-owner FE, time FE, city-year FE, city-month FE, and industry-owner-time FE in different columns. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 4: The Effects on Firm Exit

	Exit Rate (× 100)					
	All Firms			Incumbents at Reform		
	(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)
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	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓		✓	✓	
City-Year FE			✓			✓

Notes: This table shows the baseline results for the impacts on firm exit as specified in Equation (7). The observation is at the firm and year level, with the sample period of 2009-2015. The dependent variable is a dummy variable of whether to exit or not in the next year, scaled by 100. We define the exit of firms as they stop submitting annual reports following our discussion in Section 4.2. The average exit rate was 5.7% prior to the reform. Treatment is a dummy variable, which takes the value 1 if the year is post the reform in that city and 0 prior to the reform. RegCost is constructed from 2018 ESIEC survey data, which is the dummy variable taking the value of 1 if the industry experienced high decrease in the reported registration cost. Revenue is the log sales. In Columns (1)-(3), we include all firms, while in Columns (4)-(6), we exclude all new entrants. We include city-industry-owner FE, age FE, year FE, and city-year FE across different model specifications. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 5: The Effects on Firm Size

	Revenue			Capital		
	(1)	(2)	(3)	(4)	(5)	(6)
Entrants	-1.033***	-0.899***	-0.900***	-1.075***	-0.918***	-0.919***
	(0.035)	(0.027)	(0.028)	(0.041)	(0.034)	(0.035)
Exiters	-0.910***	-0.831***	-0.832***	-0.826***	-0.732***	-0.733***
	(0.028)	(0.026)	(0.026)	(0.031)	(0.026)	(0.026)
Treatment $\times$ Entrants	-0.151***	-0.142***	-0.138***	-0.363***	-0.359***	-0.358***
	(0.033)	(0.027)	(0.028)	(0.041)	(0.035)	(0.037)
Treatment $\times$ Exiters	0.352***	0.296***	0.299***	0.228***	0.160**	0.161**
	(0.062)	(0.059)	(0.059)	(0.076)	(0.072)	(0.072)
Observations	1,110,662	1,110,634	1,110,629	1,110,662	1,110,634	1,110,629
Adjusted R^2	0.101	0.157	0.157	0.132	0.207	0.207
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 the impacts on firm size as specified in Equation (9). The observation is at the firm and year level, with the sample period of 2009-2015. In Columns (1)-(3), the dependent variable is log revenue, while in Columns (4)-(6), the dependent variable is log capital. Treatment is a dummy variable, which takes the value 1 if the year is post the reform in that city and 0 prior to the reform. The dummy variable Entrant and Exiters take the value 1 if the firm is the new entrant (of the age one) and the exiting firm (in the last year of operation and age greater than one) respectively. We include city-industry FE, city-industry-owner FE, city-year FE, industry-year FE and industry-owner-year FE across different model specifications. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 6: The Effects on Firm Productivity

	Productivity			Revenue-Capital Ratio		
	(1)	(2)	(3)	(4)	(5)	(6)
Entrants	-0.055***	-0.049***	-0.049***	0.042***	0.019	0.018
	(0.002)	(0.002)	(0.002)	(0.016)	(0.015)	(0.015)
Exiters	-0.064***	-0.061***	-0.061***	-0.084***	-0.099***	-0.099***
	(0.003)	(0.003)	(0.003)	(0.028)	(0.028)	(0.028)
Treatment × Entrants	0.012***	0.013***	0.013***	0.212***	0.217***	0.220***
	(0.002)	(0.002)	(0.002)	(0.021)	(0.020)	(0.020)
Treatment × Exiters	0.032***	0.030***	0.030***	0.124***	0.136***	0.137***
	(0.004)	(0.004)	(0.004)	(0.034)	(0.033)	(0.033)
Observations	1,110,662	1,110,634	1,110,629	1,110,662	1,110,634	1,110,629
Adjusted R^2	0.071	0.088	0.088	0.068	0.075	0.075
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 the impacts on firm productivity as specified in Equation (9). The observation is at the firm and year level, with the sample period of 2009-2015. In Columns (1)-(3), the dependent variable is the structurally estimated productivity, while in Columns (4)-(6), the dependent variable is the revenue-over-capital ratio, measured as the log ratio of the revenue to capital. Treatment is a dummy variable, which takes the value 1 if the year is post the reform in that city and 0 prior to the reform. The dummy variable Entrant and Exiters take the value 1 if the firm is the new entrant (of the age one) and the exiting firm (in the last year of operation and age greater than one) respectively. We include city-industry FE, city-industry-owner FE, city-year FE, industry-year FE and industry-owner-year FE across different model specifications. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 7: The Effects on Entrants' Size and Productivity: Market Competition

	Revenue	Productivity
	(1)	(2)
Entrants	-0.855***	-0.046***
	(0.028)	(0.002)
Treatment × Entrants Who Entered Before the Reform	-0.045	0.006**
	(0.030)	(0.003)
Treatment × Entrants Who Entered After the Reform	-0.173***	0.012***
	(0.030)	(0.002)
Observations	1,110,629	1,110,629
Adjusted R^2	0.152	0.085
City-Industry-Owner FE	✓	✓
City-Year FE	✓	✓
Industry-Owner-Year FE	✓	✓

Notes: This table explores the market competition channel in shaping entrants' productivity as specified in Equation (11). The observation is at the firm and year level, with the sample period of 2009-2015. In Column (1), the dependent variable is log revenue, while in Column (2), the dependent variable is productivity. The dummy variable Entrant Who Entered Before the Reform takes the value 1 if the firm is the new entrant (of the age 1) and entered before the pilot was implemented in that city, and Entrant Who Entered After the Reform takes the value 1 if the firm is the new entrant (of the age 1) and entered after the pilot was implemented in that city. We include city-industry FE, city-year FE and industry-year FE in different columns. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 8: Evidence for the Composition Effects

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, Time FE	✓	✓
City-Year, City-Month FE	✓	✓
Industry-Time 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 FE	✓	✓
City-Year, City-Month FE	✓	✓
Industry-Time FE	✓	✓

Notes: This table shows the evidence of the composition effects. In Panel A, we explore the source to fund the start-up costs of new businesses. The observation is at the firm level from 2018 ESIEC survey data in six province and the whole industries. The dependent variables are whether the entrepreneur borrowed from friends/relatives in order to establish the business, and whether the entrepreneur loaned from financial institutions in order to establish the business. Treatment is a dummy variable, which takes the value 1 if the year is post the reform in that city and 0 prior to the reform. We include city FE and one-digit industry FE across different model specifications. In Panel B, we explore the change of entrepreneurial characteristics of new firms. We keep a sample of entrants from Jan 2009 to May 2015 with the major characteristics of their legal representatives constructed from the Business Registry Database. The dependent variables are years of schooling and whether the entrepreneur's age is below 35. In Panel C, we investigate the change of shareholding structure of new firms. The observation is at the firm level, with the sample period of 2009-2016. We construct the shareholding structure of new entrants using the Business Registry Database, and categorize shareholders into two types: individuals (natural persons) and firms. The dependent variables are the share owned by individual shareholders and whether the firm is the sole proprietorship of individual shareholders. In both Panel B and Panel C, we include city-industry FE, firm registry time FE, city-registry year FE and city- registry month FE as well as industry-registry time FE across different model specifications. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 9: The Effects on Entrants' Productivity: Heterogeneity

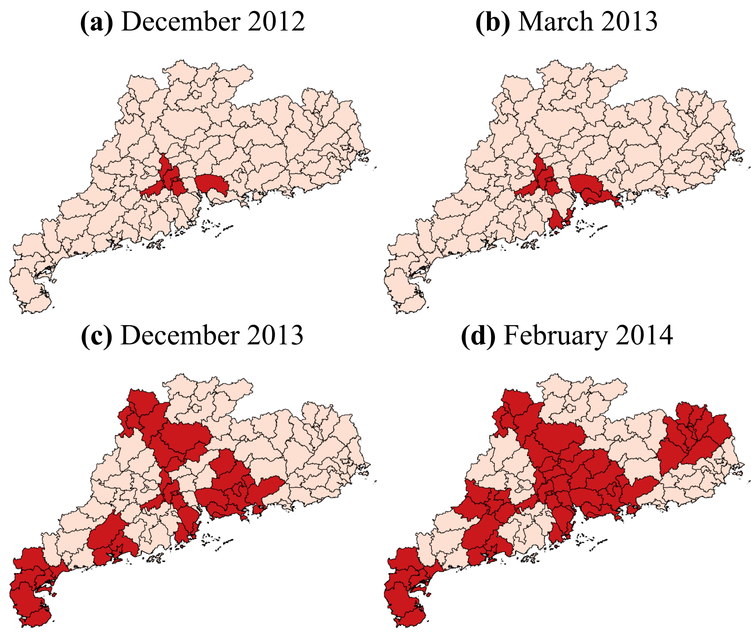
	Productivity			
	RegCost		Owner Type	
	High	Low	Private	SOE/FIE
Treatment × Entrants	0.014***	0.007***	0.011***	0.005
	(0.004)	(0.003)	(0.002)	(0.010)
Observations	494,535	616,127	1,016,711	93,945
Adjusted R^2	0.066	0.069	0.067	0.101
City-Industry-Owner FE	✓	✓	✓	✓
City-Year FE	✓	✓	✓	✓
Industry-Owner-Year 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 2009-2015. The dependent variable is productivity. Treatment is a dummy variable, which takes the value 1 if the year is post the reform in that city and 0 prior to the reform. The dummy variable Entrant take the value 1 if the firm is the new entrant (of the age 1). We only include the dummy variable Entrant and its interaction with Treatment in this regression, while the coefficient of the former is suppressed. We run separate regressions for subgroups, according to two dummy variables RegCost and Owner Type. RegCost is constructed from 2018 ESIEC survey data, which is the dummy variable taking the value of 1 if the industry experienced high decrease in the reported registration cost. Owner Type is defined as private firms or SOE/FIE. We include city-industry-owner FE, city-year FE and industry-owner-year FE across different model specifications. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Online Appendix (Not for Publication)

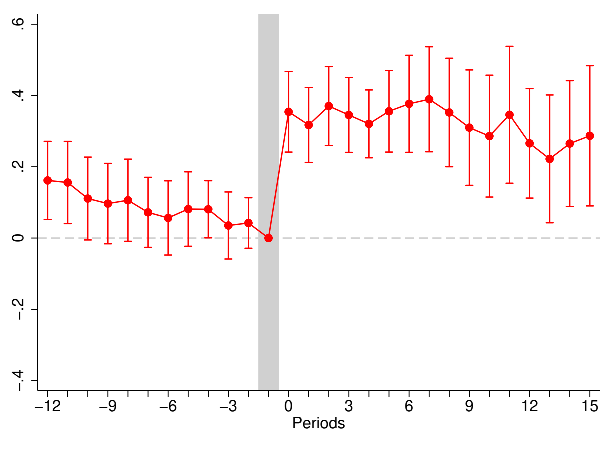
A Additional Figures/Tables

Figure A1: Four Snapshots of Program Rollout



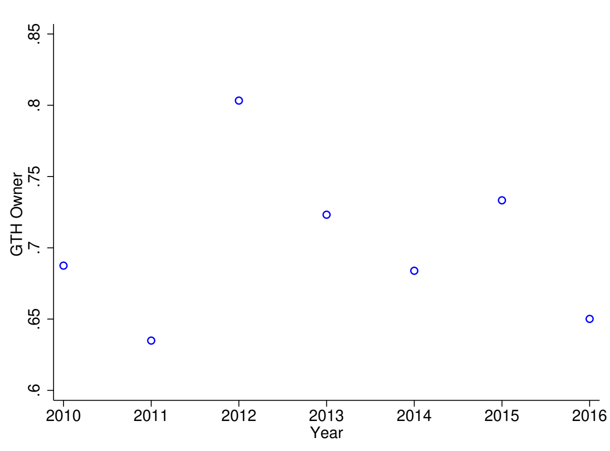
Notes: The figure shows four snapshots of the Guangdong map with the dark red regions being reformed cities.

Figure A2: The Effects on Firm Entry: Event Study, Service Sector



Notes: This figure shows the event study results on firm entry for the service sector as specified in Equation (8). The observation is at the city-2-digit-industry-month level for each two-digit service industry from Jan 2009 to Dec 2016, and we use the monthly variation in the reform timing. The horizontal axis is in the unit of month, and the vertical axis denotes the DID coefficients. 95% confidence intervals are constructed from robust standard errors, which is clustered at the city and industry level.

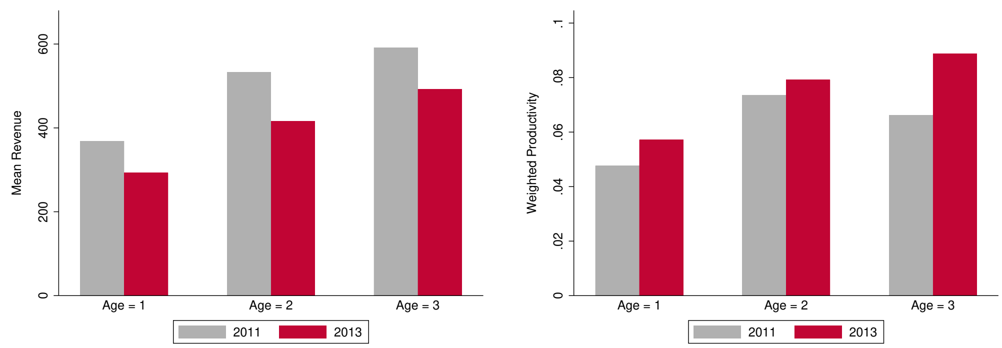
Figure A3: The Ratio of Entrepreneurs as Previous *Getihu* Owner



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

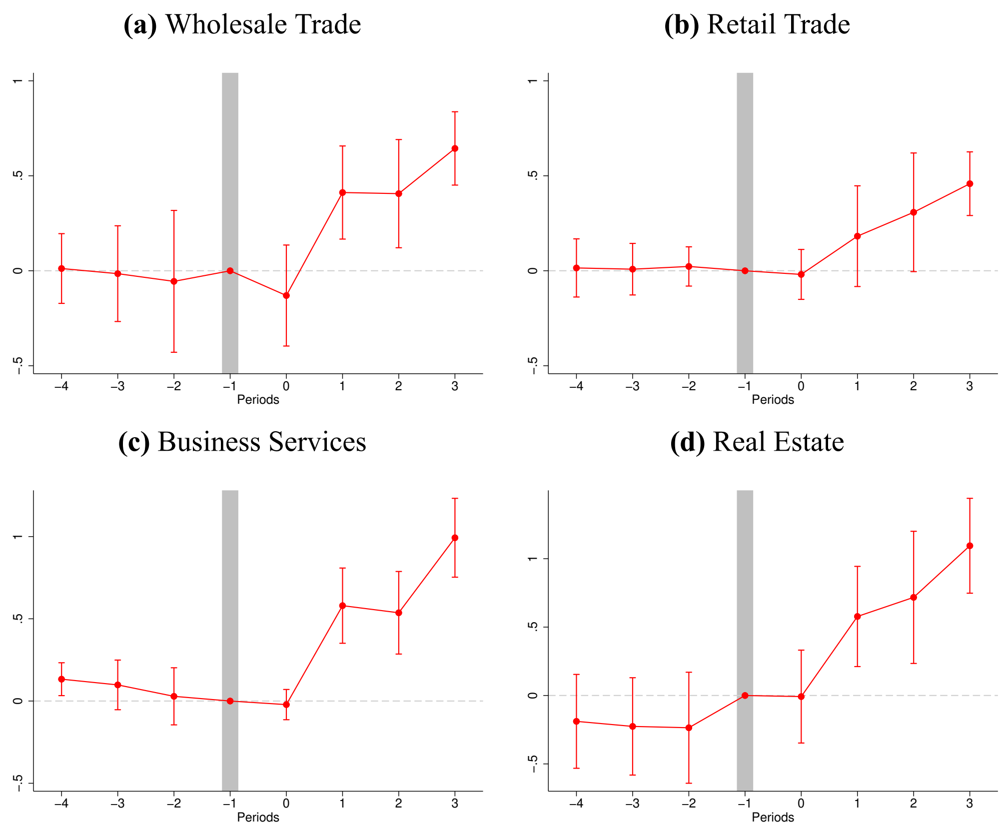
Figure A4: Entrants' Size and Productivity Across Age Groups

(a) Size-Age | **(b)** Productivity-Age



Notes: This figure shows the average revenue and weighted aggregate productivity for firms which entered in 2011 and 2013 when their ages are one, two and three. We residualize firm productivity on city by industry fixed effects, and then we weight each firm using its revenue share in city-industry-cohort-age cell. In order to make two years comparable, we only keep firms which do not miss annual reports, and those cities which implemented the reform up until the end of 2013. Therefore, the cohort 2013 entered after the reform, while the cohort 2011 entered before the reform.

Figure A5: The Effects on Firm Productivity: Service Sector, Entrants



Notes: This figure shows the event study results on entrants' log revenue-over-capital ratio in four 2-digit service industries, including wholesale trade, retail trade, business services and real estate as specified in Equation (10). Entrants are defined as firms with age one. The observation is at the firm-year level in 2008-2015, and we use the yearly variation in the reform timing. The horizontal axis is in the unit of year, and the vertical axis denotes the triple-difference coefficients. 95% confidence intervals are constructed from robust standard errors, which is clustered at the city and industry level.

Table A1: The Effects on Firm Entry: Robustness Check

	Entry					
	No HJZ	No DF	Pre 2014M3	No Anticip.	Jiangsu	Shandong
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.274***	0.369***	0.299***	0.194***	0.261***	0.265***
	(0.040)	(0.041)	(0.043)	(0.048)	(0.044)	(0.042)
Observations	45,936	48,525	34,619	52,037	36,546	48,161
Adjusted R^2	0.953	0.951	0.947	0.951	0.928	0.933
City-Industry, Time FE	✓	✓	✓	✓	✓	✓
City-Year, City-Month FE	✓	✓	✓	✓	✓	✓
Industry-Time FE	✓	✓	✓	✓	✓	✓

Notes: This table shows the robustness checks for the impacts on firm entry as specified in Equation (7). The observation is at the city, 2-digit industry and month of year level. We only keep the sample of the manufacturing sector. The dependent variable is the log number of new entrants in each city, industry and month, and we calculate the number of entrants using firm registration information, and define new entrants as those who got newly registered. Treatment is a dummy variable, which takes the value 1 if the month is post the reform in that city and 0 prior to the reform. We weight the regression using the volume of incumbents for each city-industry pair at the beginning of sample periods. In Column (1), we exclude Huizhou, Jieyang and Zhaoqing from our sample due to ambiguous starting month. Alternatively, we discard Foshan and Dongguan in Column (2) as they start the pilot reform much earlier than the rest, thus reform measures might be different from later pilots. In Column (3), we replace the sample period (Jan 2009 - Dec 2016) with the pilot sample period from Jan 2009 to February 2014 to alleviate bias from dynamic treatment effects. In Column (4), we drop three periods before the treatment to eliminate the anticipation effect. In Columns (5)-(6), we keep our sample period up to March 2014. Instead of using "late-treated" cities as the comparison group, we replace them with cities in Jiangsu province and Shandong province respectively, in order to address the concern of geographical spillovers. We include city-industry FE, time FE, city-year FE, city-month FE and industry-time FE according to different model specifications. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table A2: The Effects on Firm Exit

	Exit Rate (× 100)			
	All Firms		Incumbents at Reform	
	(1)	(2)	(3)	(4)
Treatment	1.019***	1.059***	0.820**	0.837**
	(0.365)	(0.385)	(0.383)	(0.387)
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 impacts on firm exit using firm revocation/cancellation as an alternative exit measure as specified in Equation (7). The observation is at the firm and year level, with the sample period of 2009-2018. The dependent variable is a dummy variable of whether to exit or not in the next year, scaled by 100. We define the exit as a firm got revoked or cancelled itself following our discussion in Section 4.2. The average exit rate was 4.8% prior to the reform. Treatment is a dummy variable, which takes the value 1 if the year is post the reform in that city and 0 prior to the reform. Registered capital is log valued. In Columns (1)-(2), we include all firms, while in Columns (3)-(4), we exclude all new entrants. We include city-industry-owner FE, age FE, year FE, and city-year FE across different model specifications. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table A3: The Effects on Entrants Growth Rates

	Growth Rate		
	(1)	(2)	(3)
Entrants	0.112***	0.110***	0.110***
	(0.004)	(0.004)	(0.004)
Treatment × Entrants	0.014**	0.013**	0.013**
	(0.006)	(0.006)	(0.006)
Observations	771,298	771,261	771,243
Adjusted R^2	0.026	0.027	0.026
Size Quartiles	✓	✓	✓
City-Industry FE	✓		
City-Industry-Owner FE		✓	✓
City-Year FE	✓	✓	✓
Industry-Year FE	✓	✓	
Industry-Owner-Year FE			✓

Notes: This table shows the checks for the impacts on firm growth rate as specified in Equation (9). The observation is at the firm and year level, with the sample period of 2009-2015. The dependent variable is firms' revenue growth rate. Treatment is a dummy variable, which takes the value 1 if the year is post the reform in that city and 0 prior to the reform. The dummy variable Entrant takes the value 1 if the firm is the new entrant (of the age one). We include size quintile dummies, which are defined over the capital stock. We further include city-industry FE, city-industry-owner FE, city-year FE, industry-year FE and industry-owner-year FE across different model specifications. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table A4: The Effects on Incumbents' Productivity

	Productivity			Productivity: Alternative		
	After 2010	One Missing	Three Missings	After 2010	One Missing	Three Missings
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.004*	0.001	-0.002	-0.005	-0.004	-0.005
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.003)
Observations	307,041	159,746	260,945	371,463	267,893	351,256
Adjusted R^2	0.687	0.673	0.674	0.309	0.310	0.309
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 2010-2015. In Columns (1)-(3), the dependent variable is the structurally estimated productivity, while in Columns (4)-(6), the dependent variable is the alternative productivity measure following Brandt et al. (2021). In Columns (1) and (4), we keep all firms which submitted annual report in 2010. In Columns (2) and (5), we keep all firms which submitted annual report in 2010 and only missed one annual report. In Columns (3) and (6), we keep all firms which submitted annual report in 2010 and missed at most three annual reports. Treatment is a dummy variable, which takes the value 1 if the year is post the reform in that city and 0 prior to the reform. We include city-industry FE, city-industry-owner FE, city-year FE, industry-year FE and industry-owner-year FE across different model specifications. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

B Additional Information about Data

B.1 Business Registry Database

Business Registry Database includes several different modules. Apart from the Firm Registry Database, we also use Firm Personnel Database, Firm Shareholder Database and *Getihu* Registration Database for robustness checks and channel tests.

Firm Personnel Database Firm Personnel Database is from SAIC. We use its Guangdong sample from January 2009 to May 2015 to construct entrepreneurial characteristics, including education, gender, birth place, and age. We keep the information of legal representatives of the entrants.

Firm Shareholder Database Firm Shareholder Database is from SAIC. We use its Guangdong sample in 2009-2016 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 total number of shareholders, the ratio of individual shareholders, the share held by individual shareholders as well as 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 *Getihu* Registration Database is from SAIC. We use its Guangdong sample in 2009-2016 to measure the number of new *getihu* in each city, industry, and month to check the impacts of entry deregulation on firm's reclassification incentives.

B.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 in 2010-2015 to (1) calculate average wage in each city, industry, and year as the ratio of total payroll over total employment in 2010-2015[35]; (2) estimate elasticity of substitution using the firm-level revenue and profit data; (3) construct two industry-level measures: the ratio of fixed asset to the revenue and the export share in the total revenue; (4) replicate the baseline productivity estimation to 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).

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

C Robustness Checks for 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 the weight could be negative if the treatment effects vary across groups or periods. 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 interrogate 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.

C.1 Sun and Abraham (2020)

We follow Sun and Abraham (2020) and estimate dynamic treatment effects in the following steps. The estimation is accomplished using the stata command `eventstudyinteract` published by the authors.[36]

1. We define groups of cities by the months when they implemented the reform. 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 E_c denotes the starting month of the reform in each city and e is the group index for different cities. D^q_{ct} is an indicator for city c being q months away from initial treatment in month 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 $\hat{w}_{eq}$for δ_{eq} as the sample share of group e among cities for which q months after the their initial treatment are covered by our sample periods. The weights sum up to one and are non-negative.

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

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 C1. 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.

C.2 Borusyak et al. (2021)

Borusyak et al. (2021) 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.[37]

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

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.

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

where $\omega_{cit} = \mathbb{1}\{t - E_{ci} = q\}/ \mid \Omega_q \mid$ 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.

[37] 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 the observations in 2013.

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 C2. 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. (2021).

C.3 De Chaisemartin and d'Haultfoeuille (2020a)

We follow De Chaisemartin and d'Haultfoeuille (2020a) 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 boostrap replications to estimate standard errors. The estimators are numerically equivalent to Callaway and Sant'Anna (2020) if there are no covariates in the estimation and the not-yet-treated are used as the comparison groups.

1. Let q indicates the number of periods since the implementation of the treatment. We form a DID estimator for every t and q, by comparing the outcome evolution from $t-q-1$ to t in groups treated for the first time in $t-q$ and in groups untreated between period 1 and t, accounting for city by year fixed effects $\zeta_{cy(t)}$.

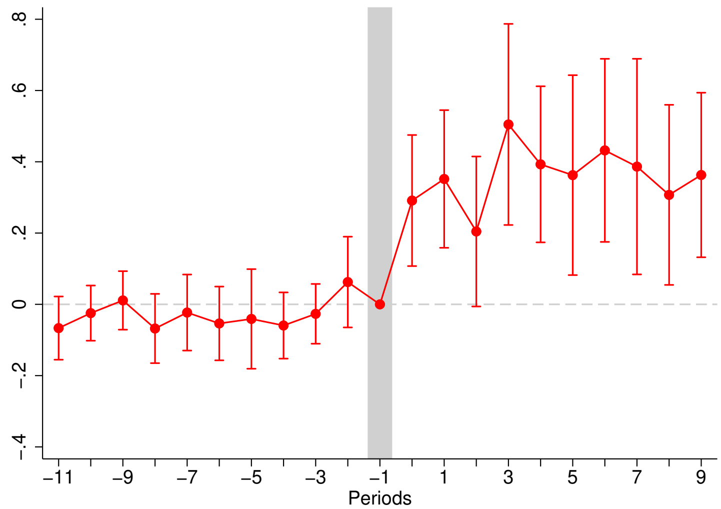
2. We weight the DID estimators across t with their sample shares.

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.

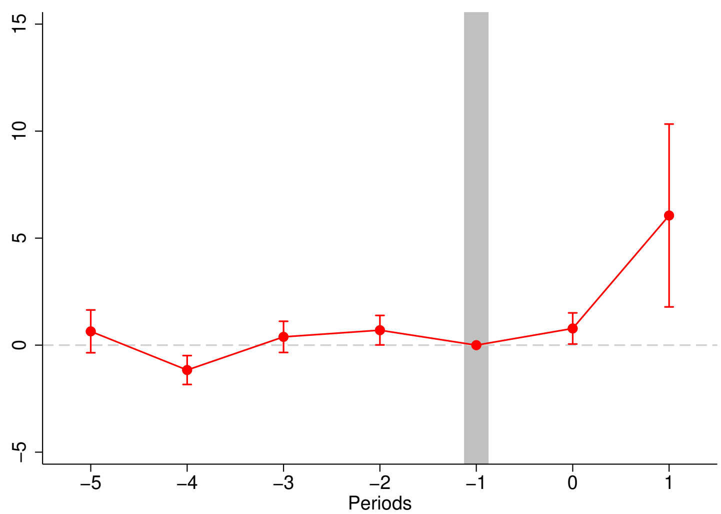
The estimation results are shown in Figure C3 and are robust compared our baseline OLS results.

Figure C1: Dynamic Treatment Effects: Sun and Abraham (2020)

(a) Entry



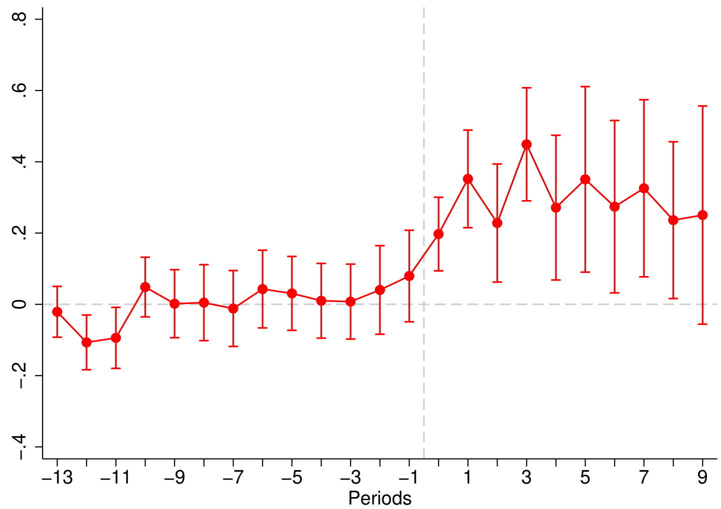
(b) Exit



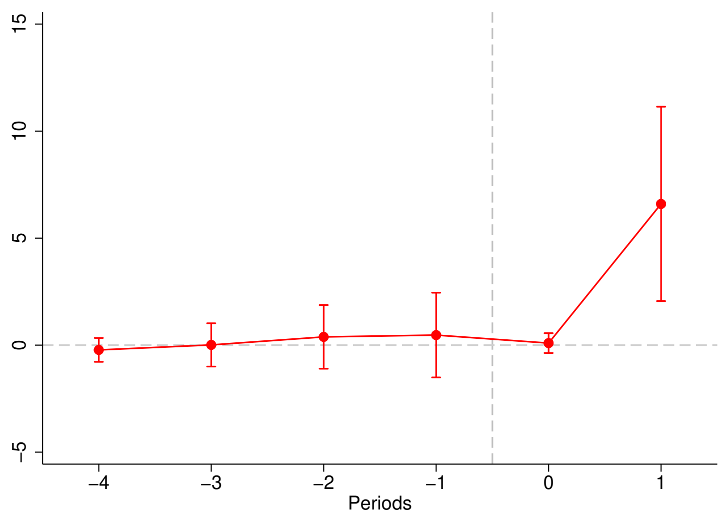
Notes: This figure shows the event study results on firm entry and exit following Sun and Abraham (2020). We define groups of cities according to the months when they implemented the reform. Furthermore, we exclude observations after the national reform was carried out and use non-pilot cities as the comparison group. Panel A shows the event study results on firm entry, where the observation is at the city-2-digit-industry-month level for each two-digit manufacturing industry from Jan 2009 to Feb 2014, and we use the monthly variation in the reform timing. The horizontal axis is in the unit of month, and the vertical axis denotes the estimates of dynamic treatment effects. Panel B shows the event study results for firm exit, where the observation is at the firm-year level in 2009-2014 and we use the yearly variation in the reform timing. Firm exit is a dummy variable which equals 1 if the firm stops submitting annual reports, further multiplied by 100. The horizontal axis is in the unit of year, and the vertical axis denotes the estimates of dynamic treatment effects. 95% confidence intervals are constructed from robust standard errors, which is clustered at the city and industry level.

Figure C2: Dynamic Treatment Effects: Borusyak et al. (2021)

(a) Entry



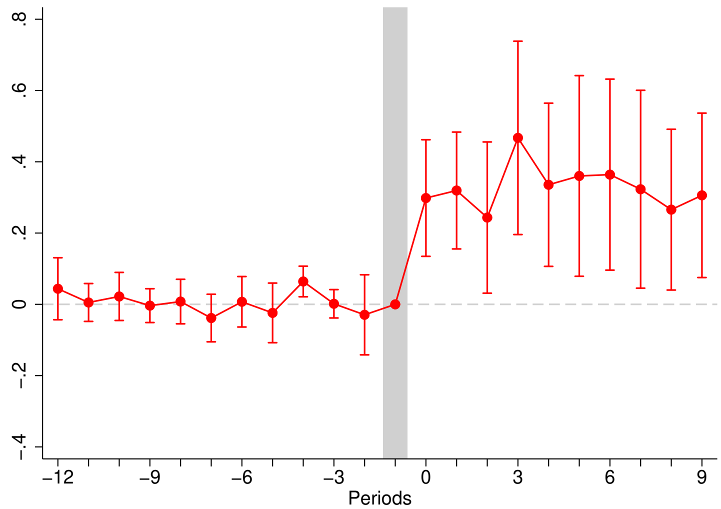
(b) Exit



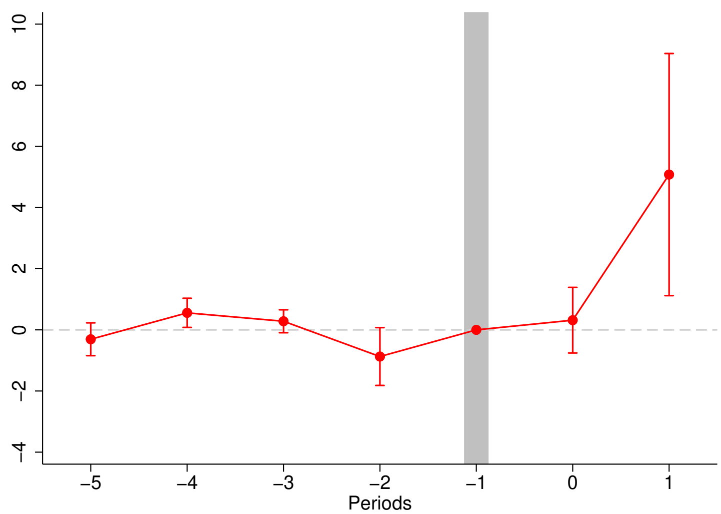
Notes: This figure shows the event study results on firm entry and exit following Borusyak et al. (2021). Panel A shows the event study results on firm entry, where the observation is at the city-2-digit-industry-month level for each two-digit manufacturing industry from Jan 2009 to Feb 2014, and we use the monthly variation in the reform timing. The horizontal axis is in the unit of month, and the vertical axis denotes the estimates of dynamic treatment effects. 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. Panel B shows the event study results for firm exit, where the observation is at the firm-year level in 2009-2014 and we use the yearly variation in the reform timing. Firm exit is a dummy variable which equals 1 if the firm stops submitting annual reports, further multiplied by 100. The horizontal axis is in the unit of year, and the vertical axis denotes the estimates of dynamic treatment effects. Five and more periods before the treatment time serve as the reference group in order to rule out the possible contamination of the treatment anticipation. 95% confidence intervals are constructed from robust standard errors, which is clustered at the city and industry level.

Figure C3: Dynamic Treatment Effects: De Chaisemartin and d'Haultfoeuille (2020a)

(a) Entry



(b) Exit



Notes: This figure shows the event study results on firm entry and exit following De Chaisemartin and d'Haultfoeuille (2020a). Panel A shows the event study results on firm entry, where the observation is at the city-2-digit-industry-month level for each two-digit manufacturing industry from Jan 2009 to Feb 2014, and we use the monthly variation in the reform timing. The horizontal axis is in the unit of month, and the vertical axis denotes the estimates of dynamic treatment effects. Panel B shows the event study results for firm exit, where the observation is at the firm-year level in 2009-2014 and we use the yearly variation in the reform timing. Firm exit is a dummy variable which equals 1 if the firm stops submitting annual reports, further multiplied by 100. The horizontal axis is in the unit of year, and the vertical axis denotes the estimates of dynamic treatment effects. 95% confidence intervals are constructed from robust standard errors, which is clustered at the city and industry level.

D Shell Firms

There is a concern that the increase in firm entry after the reform might be mainly due to a surge of "shell" firms. Shell firms are registered to achieve anonymity, for business transactions, or for tax evasion/avoidance behaviors.[38] As shell firms do not engage in productive activities, the wide presence of these firms will overestimate the impact on firm entry. We describe the basic pattern of "shell" firms as well as how entry deregulation affects them in this section.

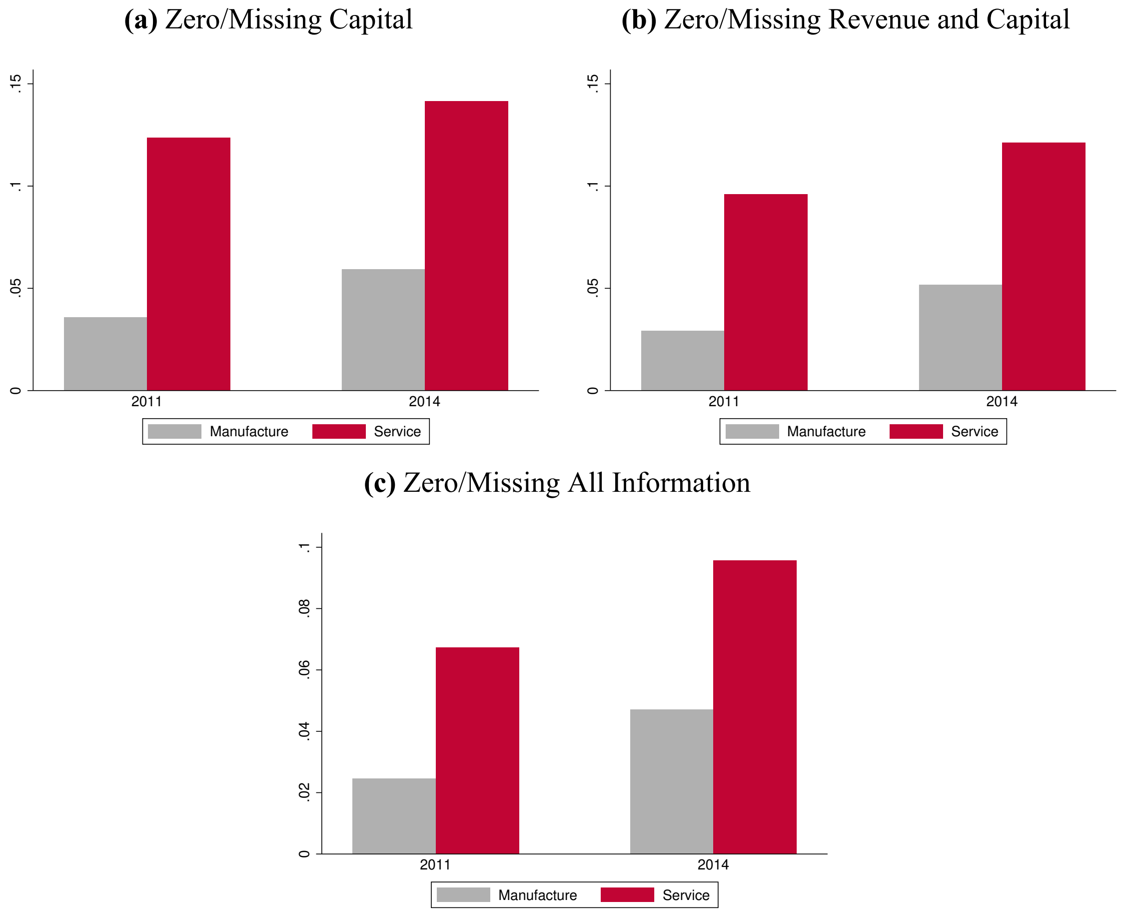
Direct identification of "shell" firms is challenging and cannot be accurate without on-site visit or government inspection. We construct an alternative measure of "shell" firms which are indirectly inferred from the Firm Annual Report Database. Under the assumption that "shell" firms are less likely to submit annual reports and even if they submit, they tend to report zeros or missing values for most items, we construct three different criteria of "shell" firms: 1) having zero or missing capital throughout the sample period; 2) having zero or missing capital and revenue throughout the sample period; 3) having zero or missing for all 5 variables (revenue, capital, tax, profit, net income) throughout the sample period.

The proportion of "shell" firms under three different criteria are shown in Figure D1 for manufacturing and service sector separately. Having zero or missing capital throughout the sample period is the most demanding definition. It categorizes around 14.1% new firms in the service sector as "shell" firms in 2014, while this ratio is 12.4% in 2011. For the manufacturing sector, this ratio increased from 3.6% in 2011 to 5.9% in 2014. Therefore, although the presence of "shell" firms is widespread, it's a much smaller problem in the manufacturing sector. More importantly, entry deregulation does not introduce a large increase in the "shell" firms.

We further explore the impacts of entry deregulation on the ratio of "shell" firms as shown in Table D1. Under the first definition, we see a marginally significant increase in the ratio of "shell" firms, while the estimates are even smaller under two other definitions. The estimated increase in the ratio of "shell" firms is also small compared with 25% rise in the total entry volume. Therefore, a surge of "shell" firms is unlikely the main driver of the increase in firm entry in the manufacturing sector. Although the cost to register a "shell" firm has declined, so has the cost to register an active firm after the reform. Moreover, "shell" firm are easier to identify in the manufacturing sector if it is not physically present in the registered location.

[38] https://en.wikipedia.org/wiki/Shell_corporation

Figure D1: Detection of Shell Firms



Notes: This figure shows the ratio of "shell firms" among entrants in our sample in 2011 and 2014 for manufacturing sector and service sector respectively. The "shell firms" is defined according to three different criteria: 1) having zero or missing capital throughout the sample period; 2) having zero or missing capital and revenue throughout the sample period; 3) having zero or missing for all 5 variables (revenue, capital, tax, profit, net income) throughout the sample period.

Table D1: The Impacts on Shell Firm Ratio

	Zero/Missing K	Zero/Missing Y/K	Zero/Missing All
Treatment	0.010*	0.007	0.006
	(0.006)	(0.005)	(0.005)
Mean of Dep Var	0.066	0.057	0.054
Observations	54,048	54,048	54,048
Adjusted R^2	0.485	0.522	0.523
City-Industry, Time FE	✓	✓	✓
City-Year, City-Month FE	✓	✓	✓
Industry-Time FE	✓	✓	✓

Notes: This table shows the impact of entry reform on the ratio of "shell firms" and total entrants as specified in Equation (7). The observation is at the city, 2-digit industry, and month of year level. We only keep the sample of the manufacturing sector. The "shell firms" is defined according to four different criteria: 1) having zero or missing capital throughout the sample period; 2) having zero or missing capital and revenue throughout the sample period; 3) having zero or missing for all 5 variables (revenue, capital, tax, profit, net income) throughout the sample period. Treatment is a dummy variable, which takes the value 1 if the month is post the reform in that city and 0 prior to the reform. We weight the regression using the volume of incumbents for each city-industry pair at the beginning of sample periods. We include city-industry FE, time FE, city-year FE, city-month FE, and industry-time FE across different model specifications. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

E Productivity Estimation

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:

$$\ln c_{it} = \ln c\left(k_{it}, a_{it}, \boldsymbol{W_{mt}}, \boldsymbol{Z_{om}}\right) - \varphi_{it} = \beta_0 + \beta_k \ln k_{it} + \beta_a a_{it} + \boldsymbol{\beta_w} \boldsymbol{W_{mt}} + \boldsymbol{\beta_Z} \boldsymbol{Z_{om}} - \varphi_{it} \quad (13)$$

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:

$$\varphi_{it} = g(\varphi_{it-1}) + \xi_{it} = \alpha_0 + \alpha_1 \varphi_{it-1} + \alpha_2 \left(\varphi_{it-1}\right)^2 + \alpha_3 \left(\varphi_{it-1}\right)^3 + \xi_{it} \quad (14)$$

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:

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

$$\pi_{it} = \frac{r_{it}(\Phi_t, k_{it}, \boldsymbol{W_{mt}}, \boldsymbol{Z_{om}}, \varphi_{it})}{\eta} \quad (15)$$

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 OP and LP, 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}:

$$\ln r_{it} = \gamma_t + (1-\eta)\left(\boldsymbol{\beta_w W_{mt}} + \beta_a a_{it} + \boldsymbol{\beta_Z Z_{om}}\right) + h(k_{it}, M_{it}) + \epsilon_{it} \tag{16}$$

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 (16), 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 $\boldsymbol{W_{mt}}$.[39] 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 effect 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 (15); b) back out $\hat{h}(\cdot)$ using equation (16); 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:

$$\begin{aligned}\hat{h}_{it} =& \beta_k(1-\hat{\eta})\ln k_{it} - \alpha_0(1-\hat{\eta}) + \alpha_1\left(\hat{h}_{it-1} - \beta_k(1-\hat{\eta})\ln k_{it-1}\right) \\ &-\alpha_2(1-\hat{\eta})^{-1}\left(\hat{h}_{it-1} - \beta_k(1-\hat{\eta})\ln k_{it-1}\right)^2 \\ &+\alpha_3(1-\hat{\eta})^{-2}\left(\hat{h}_{it-1} - \beta_k(1-\hat{\eta})\ln k_{it-1}\right)^3 - \xi_{it}(1-\hat{\eta})\end{aligned} \tag{17}$$

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

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

$$\hat{\varphi}_{it} = -\frac{\hat{h}_{it}}{1-\hat{\eta}} + \hat{\beta}_k \ln k_{it} \tag{18}$$

Estimation Results

We use the revenue and profit information in the National Tax Survey Database to estimate the demand elasticity η and report the results in Table E1. 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 of 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) from the Taiwanese electronics industry.

We next proceed to estimate $\hat{h}(\cdot)$. We use the value-added tax (VAT) reported in the National Tax Survey and the prevailing value-added tax rate of 17% to impute the value of intermediate goods as $\textit{Intermediate Goods Value} = \textit{Revenue} - \frac{\textit{Tax}}{17\%}$.[40] The average ratio of value-added to total revenue is around 20%.

Table E2 reports estimates of the revenue function as in equation (16). Column (1) includes city, industry, and year triple interactions to control for market-industry level unobserved demand and cost shifters. Firm revenue is non-linear in registered capital and older firms on average have higher revenues. Column (2) further includes the number of incumbents and market average wage. The coefficients for both variables are positive, partly driven by confounding factors. For example, markets with higher demand shocks attract more firms and at the same time, leading to higher profit and higher wages. When we instrument these two variables using the entry reform dummy and its interaction with $RegCost$ (which are valid IVs since the policy changes were top-down and exogenously implemented) in Column (3), both coefficients turn negative, as expected. The IV estimates of other parameters, especially $\hat{h}(\cdot)$, barely change from the OLS estimates. The adjusted R^2s in all three regressions exceeds 0.97, indicating a very good fit of the revenue model.

With the estimates of $\hat{\eta}$ and $\hat{h}(\cdot)$ in hand, we proceed to estimate equation (17) via NLS. We allow the structural parameters to vary across 2-digit industries and estimate the productivity for each industry separately. However, for ease of illustration, we present in Table E3 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 cost. 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.

[40]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 17% of the amount of value-added. However, we discuss later why this approach is unlikely to bias upward our main results.

In the final step, we back out the firm productivity via equation (18). The lower panel of Table E3 summarizes the correlation coefficient 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 E1 plots productivity against firm age and revenue. Our estimated productivity increases in both firm age and revenue groups, in accordance with stylized facts in firm dynamics.

Discussion

Labor as the Omitted Variable The annual reports only contain firm employment information in 2013-2016, 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.[41]

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. In other words, we first use the structural model to estimate productivity without labor, and then repeat the exercise by incorporating labor input in (16) 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 as shown in Table E2.

Thirdly, 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 F 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 four different approaches.

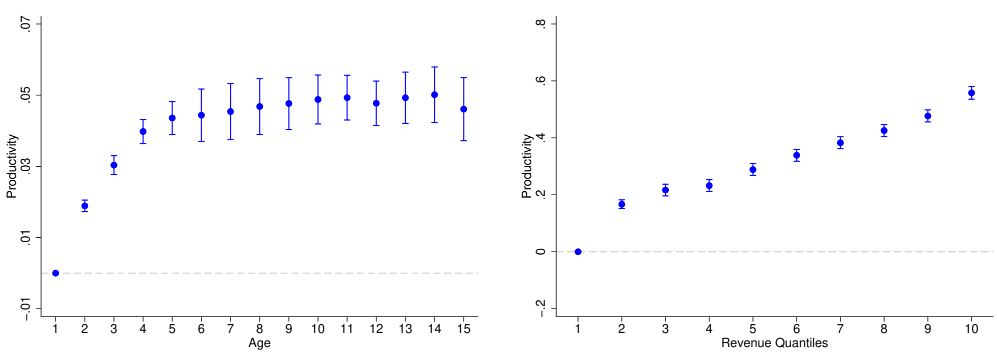
[41] A 1% increase in the OP and LP productivity estimates will raise firms' revenue by 1%, while a 1% increase in our baseline productivity estimates will reduce firms' marginal cost by 1%. This is because we directly measure the cost efficiency using our productivity measure following Aw et al. (2011). According to the model, OP and LP productivity estimates should be on average $\hat{\eta}-1$ (6.5) times as large as our baseline estimates, which is also corroborated in Figure F1.

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 ameliorate the concern. Lastly, we incorporate a flexible function of firm size in the empirical analysis, which might compensate for the limitations in the input measure.

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 (16), and the productivity estimates are robust.

Figure E1: Productivity, Age and Revenue: All Firms

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



Notes: This figure shows the relationship of productivity and firm age/revenue. The observation is at the firm and year level in 2009-2016. 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.

Table E1: Estimation of the Elasticity of Substitution

	Profit		
	(1)	(2)	(3)
Revenue	0.130***	0.132***	0.132***
	(0.010)	(0.010)	(0.010)
Observations	149,051	149,045	149,045
Adjusted R^2	0.088	0.126	0.120
City-Industry FE		✓	
Year FE		✓	
City-Industry-Year FE			✓

Notes: This table shows the estimation results of elasticity of substitution. We use the sample from National Tax Survey 2010-2015 in Guangdong manufacturing industries. The dependent variable is firm profit, and the key independent variable is firm revenue. We control city-industry FE, year FE and city-industry-year FE across different specifications. Standard errors are reported. *p<0.10; **p<0.05; ***p<0.01.

Table E2: Estimation of the Revenue Function

	Revenue		
	OLS		*IV*
	(1)	(2)	(3)
Capital	0.134***	0.135***	0.135***
	(0.001)	(0.001)	(0.001)
Capital^2	0.008***	0.008***	0.007***
	(0.0001)	(0.0001)	(0.0001)
Capital^3	−0.0002***	−0.0002***	−0.0002***
	(0.00001)	(0.00001)	(0.00001)
Intermediate Inputs	0.873***	0.873***	0.874***
	(0.001)	(0.001)	(0.001)
Intermediate Inputs^2	0.045***	0.045***	0.045***
	(0.0001)	(0.0001)	(0.0001)
Intermediate Inputs^3	−0.004***	−0.004***	−0.004***
	(0.00001)	(0.00001)	(0.00001)
Capital × Intermediate Inputs	−0.055***	−0.055***	−0.055***
	(0.0001)	(0.0001)	(0.0001)
Capital × Intermediate Inputs^2	0.004***	0.004***	0.005***
	(0.00002)	(0.00002)	(0.00002)
Capital^2 × Intermediate Inputs	0.0003***	0.0003***	0.0003***
	(0.00002)	(0.00002)	(0.00002)
Age	0.008***	0.008***	0.007***
	(0.0001)	(0.0001)	(0.0002)
Registered Capital	−0.022***	−0.024***	−0.025***
	(0.001)	(0.001)	(0.001)
Registered Capital^2	0.002***	0.002***	0.002***
	(0.0001)	(0.0001)	(0.0001)
Incumbents		0.001**	−0.120***
		(0.0004)	(0.019)
Wage		0.0003*	−0.004***
		(0.0001)	(0.001)
Observations	1,309,353	1,309,353	1,309,353
Adjusted R^2	0.975	0.975	0.973
City-Industry-Year FE	✓		
City, Industry, Type, Year FE		✓	✓

Notes: This table shows the estimation results of the revenue function (16). The observation is at the firm and year level, with the sample period of 2008-2016. The dependent variable is log revenue, and the independent variables include a cubic function of log capital and log intermediate inputs, as well as a quadratic function of log registered capital, age, number of incumbents on the market (city-industry pair) and the average wage. We use OLS in Columns (1)-(2), and use the entry reform dummy, as well as its interaction with $RegCost$ to instrument for Incumbents and Wage in Column (3). We include city-industry-year FE, city FE, industry FE, year FE and ownership type FE across different model specifications. Standard errors are reported. *p<0.10; **p<0.05; ***p<0.01.

Table E3: Productivity Estimation

Panel A: Parameter Estimates		
		Estimates
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
Revenue		0.815
Capital		0.436
Intermediate Goods		0.839
Revenue-Capital Ratio		0.084
Age		0.175

Notes: This table shows the productivity estimation results following Appendix E. 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 (13). 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 (17). Revenue, capital, intermediate goods and revenue-over-capital ratio are all log-valued. *p<0.10; **p<0.05; ***p<0.01.

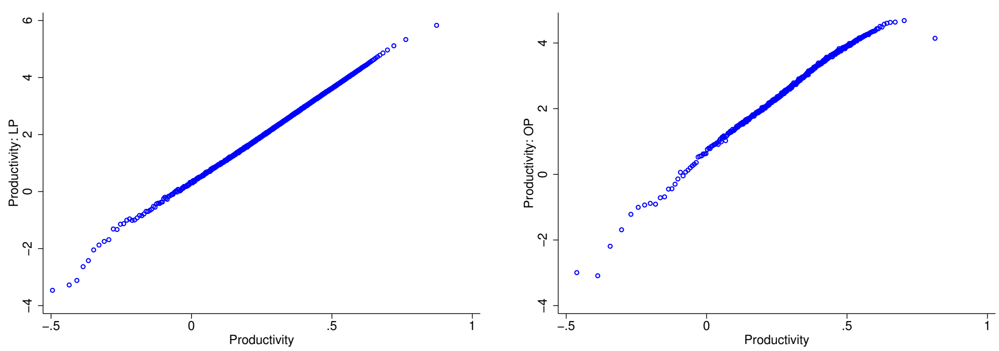
F Alternative Productivity Measures

F.1 Olley and Pakes (1996) and Levinsohn and Petrin (2003)

We estimate productivity via OP and 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. A comparison of different productivity measures is shown in Figure F1. 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 exiters compared with incumbents are robust under both alternative productivity measures, as shown in Table F1.

Figure F1: Comparison of Different Productivity Measures

(a) Productivity:LP **(b)** Productivity:OP



Notes: This figure shows the relationship of the baseline productivity estimates and estimates via LP and OP. The observation is at the firm and year level, with the sample period after 2013. Productivity is estimated in Section E, Productivity:LP is estimated following Levinsohn and Petrin (2003), while Productivity:OP is estimated following Olley and Pakes (1996).

Table F1: The Effects on Firm Productivity: LP and OP

	Productivity: LP			Productivity: OP		
	(1)	(2)	(3)	(4)	(5)	(6)
Entrants	-0.402***	-0.368***	-0.369***	-0.530***	-0.490***	-0.493***
	(0.018)	(0.017)	(0.017)	(0.020)	(0.020)	(0.020)
Exiters	-0.421***	-0.413***	-0.413***	-0.638***	-0.626***	-0.627***
	(0.059)	(0.058)	(0.058)	(0.050)	(0.050)	(0.050)
Treatment × Entrants	0.056**	0.071***	0.071***	0.102***	0.103***	0.106***
	(0.024)	(0.021)	(0.021)	(0.023)	(0.022)	(0.022)
Treatment × Exiters	0.199***	0.199***	0.199***	0.366***	0.360***	0.362***
	(0.061)	(0.059)	(0.059)	(0.063)	(0.062)	(0.062)
Observations	531,322	531,293	531,289	412,544	412,489	412,476
Adjusted R^2	0.047	0.064	0.063	0.050	0.061	0.061
City-Industry FE	✓			✓		
City-Industry-Owner FE		✓	✓		✓	✓
City-Year FE	✓	✓	✓	✓	✓	✓
Industry-Year FE	✓	✓		✓	✓	
Industry-Owner-Year FE			✓			✓

Notes: This table shows the result for the impacts on firm productivity estimated via the LP and OP methods as specified in Equation (9). The observation is at the firm and year level, with the sample period after 2013. In Columns (1)-(3), the dependent variable is productivity estimated following Levinsohn and Petrin (2003), while in Columns (4)-(6), the dependent variable is productivity estimated following Olley and Pakes (1996). Treatment is a dummy variable, which takes the value 1 if the year is post the reform in that city and 0 prior to the reform. The dummy variable Entrant and Exiters take the value 1 if the firm is the new entrant (of the age one) and the exiting firm (in the last year of operation and age greater than one) respectively. We include city-industry FE, city-industry-owner FE, city-year FE, industry-year FE and industry-owner-year FE across different model specifications. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

F.2 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,

$$z_i = \beta^{-\beta} y_i^{1-\beta} k_i^{-\alpha} \omega^{\beta} \tag{19}$$

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 value-added share w_i.

$$\bar{z}_c = \sum_{i \in I_c} w_i \beta^{-\beta} y_i^{1-\beta} k_i^{-\alpha} \omega^{\beta} \tag{20}$$

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

Combining equation (19) and (20), the relative productivity of firm i with respect to the average productivity in cell c is

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

As we control for city-industry-year FE in the regression as shown in Table 17, this relative productivity measure is much similar to our data-driven measure, revenue-over-capital ratio. We estimate $\alpha = 0.240$ and $\beta = 0.631$ via OP, and we could then back out the productivity measure. We find our baseline results robust as shown in Table F2.

Table F2: The Effects on Firm Productivity: Brandt et al. (2021)

	Productivity		
	(1)	(2)	(3)
Entrants	-0.008***	-0.008***	-0.008***
	(0.001)	(0.001)	(0.001)
Exiters	-0.006***	-0.006***	-0.006***
	(0.001)	(0.001)	(0.001)
Treatment × Entrants	0.006***	0.006***	0.006***
	(0.001)	(0.001)	(0.001)
Treatment × Exiters	0.006***	0.006***	0.006***
	(0.001)	(0.001)	(0.001)
Observations	1,469,147	1,469,118	1,469,109
Adjusted R^2	0.078	0.088	0.089
City-Industry FE	✓		
City-Industry-Owner FE		✓	✓
City-Year FE	✓	✓	✓
Industry-Year FE	✓	✓	
Industry-Owner-Year FE			✓

Notes: This table shows the result for the impacts on firm productivity estimated following Brandt et al. (2021) as specified in Equation (9). The observation is at the firm and year level, with the sample period of 2009-2015. The dependent variable is productivity estimated following Brandt et al. (2021). Treatment is a dummy variable, which takes the value 1 if the year is post the reform in that city and 0 prior to the reform. The dummy variable Entrant and Exiters take the value 1 if the firm is the new entrant (of the age 1) and the exiting firm (in the last year of operation and age greater than 1) respectively. We include city-industry FE, city-industry-owner FE, city-year FE, industry-year FE and industry-owner-year FE across different model specifications. Standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.