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

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Abstract

Entry regulation is prevalent across countries, yet there is limited evidence of its implications on industrial dynamics and aggregate efficiency. Leveraging the staggered implementation of the Business Registration Reform in China, this study examines the impact of entry deregulation on firm entry, exit, size and productivity in the manufacturing sector. Based on the administrative firm registration data and annual reports, our difference-in-differences analysis shows that the reform increases firm entry by 25% and firm exit by 8.7%, resulting in higher market turnover. The reform reduces the size of new entrants and raises the size of exiting firms relative to continuing incumbents. Furthermore, entry deregulation improves the efficiency of new entrants mainly through relaxing financial constraints. A decomposition analysis reveals that the reform helps reverse the decreasing trend of average productivity for new entrants.

Keywords: deregulation, entry cost, productivity, market turnover

JEL Codes: L10, L50, L60, O40

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1 Introduction

Entry regulation is an important and prevalent policy tool around the world to screen new firms. It can improve entrants' efficiency by screening unproductive or illegal ones out, but might also entail bureaucratic rent-seeking behavior and escalate entrants' financial stress, thus dampening their productivity. At the same time, entry regulation imposes administrative costs for prospective entrants upon other sunk costs of entry. This additional cost could be a crucial force in shaping firm entry, exit and size distribution. Despite the long-standing policy interests in evaluating entry regulation, a systematic empirical investigation about its implications on industrial dynamics and production efficiency is absent. In this paper, we explore the general equilibrium effects of entry regulation on industrial dynamics, as well as the impacts on entrants' efficiency and aggregate productivity, using variations from China's Business Registration Reform as a natural experiment.

China's Business Registration Reform was implemented by the central government to deregulate firm entry. It substantially reduced the entry cost by simplifying firm registration procedures and relaxing registered capital requirement. The national reform started with a pilot program in Guangdong province, which was implemented in a staggered fashion across cities. Our analysis focuses on this pilot program in Guangdong province in order to take advantage of the variation in implementation timing to identify the causal impacts of entry deregulation.[1] We utilize a rich set of administrative data of the manufacturing sector on firm registration, annual report, entrepreneurial characteristics and shareholding structures in the empirical analysis.

Our empirical analysis proceeds in three steps. In the first step, we adopt a difference-in-differences framework to study the impacts of entry deregulation on market turnovers, i.e., firm entry and exit. Promoting new firms is a key policy objective of the reform, and the impact on firm entry could propagate in market dynamics. Since a greater number of entrants generate more intense competition on incumbent firms, the reform in turn may affect firm exit. In the second step, we evaluate the effects of entry deregulation on firms' size and productivity distribution. We adopt a triple difference strategy to investigate the heterogeneous impacts between entrants (exiters) and continuing incumbents. In particular, we explore the efficiency implications of entry deregulation on entrants. We estimate firm productivity structurally following Aw et al. (2011) and Peters et al. (2017), and also use productivity estimates via Olley and Pakes (1996) (OP) and Levinsohn and Petrin (2003) (LP) as robustness checks. Additionally, we provide a set of evidence and discussion about the mechanisms of our empirical findings. In the final step, we follow Foster et al. (2008) to explore the implications on aggregate productivity.

[1] Guangdong, a coastal province in Southern China with 21 cities, is the most populous province in China with a population of 115 million by the end of 2019. It also has the largest provincial GDP which exceeds that of Spain and Australia, the 13^{th} and 14^{th} 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 key challenge to our identification strategy is the endogenous timing of the reform, as the implementation of the pilot might be correlated with unobserved determinants in firm entry, exit, size and productivity. We address this concern in three ways. First, as firm entry is measured at the monthly level, we exploit the *monthly* variation of the pilot implementation for each city within a year, which is arguably more exogenous. Second, since firm exit, size and productivity are measured annually, we adopt a triple difference strategy to investigate the heterogeneous impacts across industries, or between entrants/exiters and continuing incumbents, while controlling for the endogenous timing of the pilot. Third, throughout the paper, we use an event study design to investigate the pre-treatment effects on various outcomes.

We find that entry deregulation increased firm entry by about 25%. The effect is persistent over time and larger than previous estimates in other countries (Kaplan et al., 2011; Branstetter et al., 2013). Our larger estimate is likely due to the nature of this reform, which is more comprehensive and more strictly implemented. In addition, there is large heterogeneity across industries and firm types: industries with higher degrees of entry deregulation and private firms experience a greater increase in new entrants. Moreover, we find that firm exit increases by 8.7%, likely driven by the more intense competition from massive entry.

Having explored the impacts on the extensive margins (entry and exit), we move on to examine the effects on intensive margins in terms of firm size and efficiency. Compared with continuing incumbents, new entrants become smaller after the reform. The reduction in entry cost allows smaller firms to enter the market and pushes the average size of new entrants downwards. Compared with continuing incumbents, exiting firms are relatively larger after the reform, consistent with the theoretical insight of Hopenhayn (1992). Entry deregulation leads to more intense market competition, which could drive the marginal incumbents out of the market after the reform. It is because the marginal incumbents were larger in size than the average exiters prior to the reform.

Moreover, based on the structurally estimated productivity measures, we find that the efficiency of new entrants improves after the reform. This indicates that the efficiency gain outweighs the weaker screening effect from the lax regulation. Our analysis on the underlying mechanisms points to the reduction in financial constraint after the reform as the main driver. We find that the improvement in efficiency is greater among city and industry pairs where the administrative cost accounts for a larger share of the overall entry cost. In addition, entrepreneurs of a younger age and a lower education level, who are more likely to be in face with financial stress, contribute disproportionately to the new entrants. New entrants are increasingly controlled by individual shareholders (natural persons) instead of corporate shareholders as well. These pieces of evidence altogether point to the financial constraint channel of entry deregulation in promoting entrepreneurship and improving entrants' efficiency. On the contrary, alternative explanations, such as the market competition channel and government rent-seeking channel, do not find much empirical support. To

understand the impact of the entry deregulation on aggregate productivity, we conduct an exercise of productivity decomposition and find that the reform helps reverse the trend of decreasing average productivity of new entrants. New entrants account for 15.4% and 21.2% of the overall productivity improvement in 2013 and 2014, respectively.

Our paper contributes to the following three strands of literature. First, our paper adds to the literature about the impact of entry deregulation on entrepreneurial activities (Djankov et al., 2002, 2006; Klapper et al., 2006; Kaplan et al., 2011; Bruhn, 2011; Branstetter et al., 2013).[2] These studies generally find that entry regulation entails bureaucratic hurdles for new entrants and can greatly hinder entrepreneurship. Thus, entry deregulation tends to encourage firm entry. While most of these studies focus on the number and the characteristics of new entrants, our paper goes beyond that by including firm exit and intensive growth margins (size and productivity). In addition, we examine the the overall impacts on aggregate TFP.

Second, our paper relates to theoretical and empirical studies on the role of entry cost in market structure and industrial dynamics (Hopenhayn, 1992; Dunne et al., 2013; Maican and Orth, 2015, 2018). Different from the structural approach in this literature, our paper uses a quasi-experiment by exploiting the staggered rollout of the reform. A few studies (Barseghyan, 2008; Barseghyan and DiCecio, 2011) have studied the impacts of entry cost on aggregate TFP using cross-country data, which is subjected to omitted variables such as hardly measured institutions and cultural factors. By comparison, our identification strategy is built on regional variations within a country.

Third, our paper contributes to the broad literature on the efficiency implications of deregulation (Olley and Pakes, 1996; Buccirossi et al., 2013; Greenstone et al., 2012). Specific to China's context, there are a few papers from the macro/trade literature Khandelwal et al. (2013) and Tombe and Zhu (2019) explore how institutions such as international trade quotas or internal trade barriers lead to misallocation of resources and efficiency loss of the economy. We complements this literature by providing evidence of how administrative entry barriers bring efficiency loss for new entrants.

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

[2]There is a large literature examining the outcomes of entry (de)regulation. A few of the most influential studies on this topic is based on cross-country dataset (Djankov et al., 2002, 2006; Djankov, 2009). To address the omitted variable problem in cross-country studies, some follow-up studies exploit the variations across industries (Klapper et al., 2006; Ciccone and Papaioannou, 2007; Fisman and Allende, 2010). Furthermore, there is another strand of papers exploiting policy shocks to identify the effects of entry (de)regulation (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). Among these papers, Schivardi and Viviano (2011) studied the impacts 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 firm entry and characteristics of new entrants.

tion. Section 5 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 World Bank, it took 14 procedures, 38 days, and 3.5% of income per capita to start a standardized business in China in 2011.[3] 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 new business.[4]

With increasing pressure to stimulate economic recovery from the global financial crisis in 2008, the State Administration of Industry and Commerce (SAIC) introduced the Business Registration Reform (*Shang Shi Zhi Du Gai Ge*) to deregulate firm registration. The reform was first implemented as a pilot in Guangdong province. The pilot started in two cities, Dongguan and Foshan in 2012. It was then rolled out sequentially in 10 other cities, before being scaled up to the entire province as well as the rest of the nation in March 2014.

Prior to the reform, a start-up had to obtain numerous certificates to receive a business license before operating legally, which was both time consuming and costly. The reform reversed the previous requirement: one can first get the license before securing all the necessary permits. Meanwhile, SAIC moved 108 approval items to post-registration, and reduced the total number of required certificates to 13. Consequently, the reform greatly simplified registration procedures and reduced the time and financial cost to start a new business.

Another key feature of the reform is relaxing the requirement of registered capital. The registered capital is defined as "the amount of contributions actually paid or subscribed to by all shareholders or promoters lawfully registered", which corresponds to the liability of shareholders.[5] According to the New Corporate Law implemented in 2006, the minimum capital requirements varied by company type.[6] In addition, the contribution schedule of registered capital was also regulated. The reform removed the minimum registered capital requirements for all companies as well as

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

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

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

[6]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).

restrictions on the form and contribution schedule.[7] 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 since registration. Instead, the registered capital is the capital subscribed by all the shareholders that can be different from the actual paid-in capital.[8] Firms just have to report the amount of registered capital in local Industry & Commerce 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 annual inspection system requires a firm to fill in inspection form, provide debt statement and income statement, pay inspection and auditing fees, and bring 14 documents to the local SAIC for processing.[9] After the reform, SAIC allowed firms to file their annual reports electronically online themselves. Firms can choose to publicize their annual reports in a newly established firm information system, and SAIC randomly selects a small proportion of firms for onsite inspection. If a firm fails to obtain the necessary permits after operation 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 has set up electronic registration platform, greatly reduced the amount of required paperwork. It also amended a series of laws to support the reform.

According to anecdotal evidence covered in the local media, after the firm registration reform, it takes less than 1 hour to obtain the license and complete all the paperwork. To sum up, the reform has reduced registration cost and lowered bureaucratic barriers faced by potential entrants.

2.2 The Pilot Reform in Guangdong

The Business Registration Reform started with a 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 finally implemented in the remaining 9 cities in Guangdong together with other parts of China in March 2014. Among the 21 cities, five initially started the reform in a small area, i.e. a town/district/county, before scaling it up to the entire city.[10] Since these mini-

[7] 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 restriction 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 was simplified and a company is not required to submit a capital verification report upon registration.

[8] 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.

[9] https://baike.baidu.com/item/%E4%BC%81%E4%B8%9A%E5%B9%B4%E6%A3%80

[10] For example, Foshan started the reform in its Shunde District in May 2012 and then extended the reform to the whole city in September 2012. Dongguan began the first pilot in May 2012 before extending to the whole city in December 2012. Similar phased rollouts occured in Guangzhou, Yangjiang, and Zhuhai.

pilots were only tentative and incomprehensive, we use the start date of the pilot in the whole city in our empirical analysis. We further check the consistency of the start time of the reform based on 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.

Although we could potentially find accurate timing of the pilots, the enforcement varies 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 at all.[11] In addition, those early official documents focused only on relaxing registered capital requirement but ignored other important policy aspects. Therefore, 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 these three cities from the sample, and find that the results are robust. The rollout timing of the reform is summarized in four snapshots in Figure A1.

2.3 Data

To evaluate the impacts of the entry registration reform in multiple dimensions, we compile several administrative data sets, including Business Registry Database and Firm Annual Report Database, in combination with several auxiliary data sets, such as National Tax Survey Database, 2008 Chinese Economic Census, 2018 Enterprise Survey for Innovation and Entrepreneurship in China (ESIEC) Data. In this section, we will focus on our two main data sets exploited in the empirical analysis. Further discussion about other data sets can be referenced in Appendix B.

The Business Registry Database is maintained by SAIC and contains universal firm registration information, including registry date, revoke or cancel date, location, ownership type, industry and registered capital. We use the data during 2009-2016 to calculate the monthly number of new entrants in each city, by industry and ownership type. This database provides a complete picture of firm registration, thus enabling us to directly examine the impacts of entry deregulation on firm extensive margins.

The Firm Annual Report Database from SAIC contains universal firm annual business information, including gross capital, employment, sales, taxes and profits. We get access to Guangdong firm data in 2008-2016. The information enables us to track size distribution and production efficiency. It would be ideal to use the profit data in our empirical analysis, however, a disproportionate amount of firms reported zero profit, greatly limiting us to use this measure. Instead, we will focus on other variables for our main empirical study.

[11] 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 was found until March 2014.

These two data sets have several salient features that make the data particularly useful for our empirical analysis. First, the firm information is universal. Firm Registry Database provides complete information on firm entry of different cities and time periods, while Firm Annual Report Database covers almost all the supposedly active firms. Compared with other extensively used Chinese firm data sets, our data avoids selecting samples either explicitly or implicitly based on the firm size (e.g. the Annual Survey of Industrial Firms and the National Tax Survey Database). This feature is particularly valuable as we aim to investigate the dynamics of the whole industries. Second, our data covers several years both before and after the reform, enabling us to exploit the staggered rollout of the reform. The sample period is long enough for examining the parallel trend assumption using pre-reform data. Third, Firm Annual Report Database provides detailed firm level annual information, which enables us to further explore the distribution and evolution of firm size and productivity, instead of just focusing on the extensive margin, commonly used in previous studies.

Nevertheless, out data sets have two potential caveats. First, there are missing data issues as the reporting rate is much lower in the year of 2013, which might be a result of a chaotic transformation annual firm inspection system to annual report system in that year. However, we don't find a systematic missing pattern for a specific group of industries and ownership types. We further allow very flexible functional form to control for this reporting issue in our regression analysis.[12] Second, the firm employment information is only available post 2013, making it challenging to consistently estimate productivity for the whole sample period. We instead estimate productivity structurally by imposing more model assumptions, and leverage employment information from the shorter sample period in robustness checks.

3 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. We then examine the impacts on the other side of the market turnover, firm exit, as an important outcome of the general equilibrium effect.

For the impacts on firm entry, we leverage the variation in the *monthly* variation in the implementation timing across cities, and employ the difference-in-differences strategy. Furthermore, we explore the heterogeneity of the effects across different industries and ownership types. For the

[12] There are scarce observations for a few cities and industry in certain years. That include Zhuhai after the reform, Shenzhen for the year of 2008-2010, Guangzhou for the year of 2008, and "Repair of Metal products, Machinery and Equipment industry" (2-digit industry code 43) before the reform. We exclude the small numbers of observations from these city-year or industry-year pairs to avoid the potential bias from small reporting groups. Empirical results are also robust whether we exclude them or not.

impacts on firm exit, as it is defined annually, we exploit the *yearly* variation in the implementation timing. To further aid identification, we leverage cross-industry variation to identify the causal impacts on firm exit.

3.1 Descriptive Evidence

We first describe the data pattern before turning to regressions. Guangdong had 250,000 new firms, about 12% of the national total in 2011. The growth rate of the number of new firms is 16.5% on average during 2008-2012 and the entry rate is 14.7%.[13] However, as shown in Figure 1, after the reform, we observe a sharp increase in the number of new entrants. Specifically, the number of new entrants in 2014 nearly doubled that of 2012, and the number of new entrants in 2016 more than tripled. Moreover, the annual growth rate of the number of new firms attains 34.4% in 2012-2016 with average entry rate being 22.7%. The growth rate in 2013 and 2014 exceeded 40%.[14] These data pattern suggests potentially large impacts of the reform on promoting new businesses.

Figure 2 displays the monthly trend by city. Most of the major 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, jumping to 1,521 after the reform took place.[15] Although Foshan and Dongguan, two pilot cities, exhibited a sharp increase in firm entry after March 2014, there was not a clear structural change of new entry in relation to the timing of the policy. This might be due to the nature of trial and error in the early pilots.

3.2 Empirical Strategy

By taking advantage of the staggered rollout of the reform, we adopt the difference-in-differences strategy to estimate the effects of entry deregulation on the number of new firms. The empirical model is shown in (1),

$$y_{cit} = \beta \mathbf{D}_{ct} + \xi_{ci} + \tau_t + \mathbf{Z}_{cit} + \varepsilon_{cit} \tag{1}$$

where c, i and t denotes city, industry and time respectively. The dependent variable y_{cit} is the log monthly number of new entrants in city c and industry i, or log entry rate. Our key explanatory

[13] Entry rate is defined as $Entry\ Rate_{it} = Entry_{it}/Incumbent_{it-1}$.

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

[15] One exception is Zhongshan. According to local gazetteer, though the official pilot program started in November 2013, there had been some local policy changes in 2012 as a precursor to the reform (http://www.zsda.cn/plus/view.php?aid=354830). Indeed, the data show a spike of firm entry in Zhongshan in 2012. In order to be consistent, we use the official reform time in Zhongshan in our main empirical analysis and exclude Zhongshan in one of the robustness checks. The results remain robust.

variable, $\mathbf{D}_{ct}$, 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. The coefficient β is of the key parameter of interest, and it measures the effect of the reform. We include a set of control variables: month-year fixed effects τ_t to capture the common unobservables such as macroeconomic conditions, as well as the city by industry fixed effects ξ_{ci} to rule out the time-invariant confounders across cities and industries.[16] The key identification assumption is that conditional on ξ_{ci} and τ_t, the timing of reform 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.

The most salient threat to our identification assumption is the endogenous timing of the reform, i.e. cities with greater entry potentials are selected as earlier pilots. To address this concern, we control for city by year fixed effects in the main specification, using only the monthly variation of the program implementation within a year. City by year fixed effects largely control for macroeconomic conditions that may be correlated with the reform. Furthermore, to check the parallel trend assumption prior to reform and also to reveal the post-reform effects, we adopt an event study model in (2),

$$y_{cit} = \sum_{q=-12}^{15} \beta_q \mathbf{D}_{cq} + \xi_{ci} + \tau_t + \mathbf{Z}_{cit} + \varepsilon_{cit} \tag{2}$$

This is the same as equation (1), except that we replace the single dummy variable $\mathbf{D}_{ct}$ with a series of indicators $\mathbf{D}_{cq}$. $\mathbf{D}_{cq}$ are dummy variables denoting a certain month relative to the reform during the window of 12 months before and 15 months after the reform in city c. For example, $D_{c,-1}$ is one month prior to the reform, and $D_{c,2}$ is two months post the reform. Two exceptions are $D_{c,-12}$ and $D_{c,15}$, with the former indicates twelve months before the reform or earlier, and the latter represents fifteen months after the reform or even later. We take $D_{c,-1}$, one month before the reform, as the baseline group and set β_{-1} to be 0. β_p are allowed to vary to capture dynamic treatment effects over time.

In addition, we add city by calendar month fixed effects to control for seasonality in firm entry across cities. We also include industry by month of sample fixed effects to control for industry specific trend in robustness checks. Moreover, we weight the regression using the number of incumbents for each city and industry pair at the beginning of sample periods.

To examine firm exit, we use the *yearly* variation in the implementation time as firm exit is defined at the annual level. The regression is at the firm j by year y level, and the basic empirical frameworks replicate (1) and (2).

There are two additional empirical challenges on examining firm exit. First, the revoke data

[16] There are other policy changes taking place in Guangdong in recent years. Value-add tax reform, as well as the establishment of Guangdong Pilot Free Trade Zone and Guangdong-Hong Kong-Macao Greater Bay Area, took place after our sample period. Other policies such as Rural Revitalization Project were rolled out simultaneously across cities and thus largely captured by time fixed effect.

in the Business Registry Database is ill-reported after the reform. The Business Registry Database includes firm exit information of either self cancellation and revocation by SAIC. Both types of "registered exit" were equally important in magnitude before the reform. Nevertheless, firms revocation is largely grounded to a halt 2014 and 2015, introducing a large negative measurement error of firm exit.[17] We use two ways to remedy the concern. First, instead of solely relying in on the Business Registry Database, we construct firm exit measure by the number of incumbents stopping submitting annual reports. The high cost of canceling a business license prevents some firms from officially doing so. These firm normally do not bother to file an annual report. Given the mandatory feature of annual report and the severe penalty of revoking license for violation, this measure is a good proxy for exit and gives plausible overall time trend. Using this alternative measure, we find the aggregate exit rate keeps at 5%-10% both before and after the reform.[18] Second, in 2017-2019, SAIC cleaned up inactive firms, including those which were supposed to be revoked in 2014-2015. We thus use more updated exit information as of 2019 from Firm Registry Database as an alternative exit measure.

As firm exit is defined at an annual level, we could only exploit the *yearly* variation in the implementation time. This is of a great challenge to deal with the problem of endogenous timing. To overcome the challenge, we use the cross-industry variation to identify the causal impacts on firm exit as we discussed in Section 3.3. Conditional on a rich set of controls, we empirically test whether industries with high degrees of deregulation also experience a larger change in the exit rate.

In our main specification, we define the latest year with record in the Firm Annual Report Database as the last year in business. If an incumbent submits annual report until the year of 2016, we call it a continuing firm. The dependent variable used in the regression analysis is a firm level measure $\mathbb{1}\{\text{Exit}_{it} = 1\}$, which takes the value of 1 if firm i is about to exit next year, and 0 if it will remain in operation. Due to the right-censoring nature of the exit measure, we exclude the year of 2016, the last year of our sample, from the analysis.

[17] This is possibly due to local government's incentive to keep up a high firm volume, which is the most important indicator of reform achievement. Suppressing firm revoke is unlikely to have a huge impact on real economy, since those exiting firms could permanently stop operation anyway, but this phenomenon itself is indirectly revealing that the exit rate might be significantly increased after more entrants flood into the market.

[18] 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).

3.3 Firm Entry

Baseline Results

Table 1 reports the regression results based on (1). Panel A shows the estimates on the number of newly registered firms. However, there is a concern about the wide presence of "shell" firms .[19] These firms only exist on paper without any significant operation.[20] The presence of these firms will overestimate the impact on firm entry as the shell firms do not engage in productive activities.

To address this concern, we use two alternative measures of new entrants as robustness checks. Panel B includes only the newly registered firms which also file annual reports during our sample period. Panel C further restrict the sample by excluding firms that never reported a positive capital stock. "Shell" firms are less likely to submit annual reports and even if they submit, they tend to report zeros for most items, including capital stock. If the rise of new firms stems mainly from "shell" firms, we would expect a much lower estimated effect when using the latter two alternative measures. However, we find similar estimates in Panels B and C. One interpretation is that though the number of"shell" firms have significantly increased, its share in new entrants stays rather stable over the whole sample period, including the pre-reform period. After the reform, the cost to register a "shell" firm has declined, so has the cost for active firms. In the manufacturing sector, a "shell" firm can be easily identified if it is not physically present in a registered location. Thereby, "shell" firms might be less of a concern in the manufacturing sector, a focus of our paper, than in the service sector.

Having shown the robustness of our results to alternative exit measures, we focus on the firm entry definition in Panel A as our baseline. In addition to city by industry fixed effects, and time (i.e., month-of-sample) fixed effects, Column (1) of Table 1 also control for city by year fixed effects to address the concern over the macroeconomic confounders and the endogenous timing. The estimated impact of entry deregulation 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 fixed effect to control for industry-level macro time trend. The results are similar across columns. In our preferred specification with the most controls in column (3), entry deregulation is associated with a 24.5% increase in new entrants. Columns (4)-(6) use log entry rate as the dependent variable. 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 city by year fixed effects.

[19] See for example http://amr.sz.gov.cn/xxgk/qt/ztlm/scjgzy/mtbd/content/post_7821580.html

[20] Quoted from Wikipedia https://en.wikipedia.org/wiki/Shell_corporation, shell companies are often registered to achieve anonymity or "serving as a vehicle for business transactions". Sometimes they might be used for tax evasion or tax avoidance behaviors.

Robustness Checks and Effect Dynamics

Table A1 provides more robustness checks by using different sub-samples. 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 columns (1)-(2). The coefficient estimate slightly increases to 27.4% from the baseline estimate of 24.5%. In addition, as Foshan and Dongguan are the cities to start the earliest pilot in 2012 where different reform measures were tried and tested in narrowed areas. The timing may not reflect the larger scale reform as shown in other cities later on. As a robustness check, we exclude them from our sample in columns (3)-(4) and the magnitude further rises to around 37%. Lastly, our main sample covers monthly data from January 2009 to December 2016. In columns (5)-(6), we only keep a shorter sample period up to March 2004, when the national reform was rolling out. In doing so, only the twelve pilot cities are included in the sample. The effect is significant at 29.9%. Overall, Our baseline estimates seem to be more conservative than the results from a series of robustness checks.

The baseline estimates in Table 1 show that the entry deregulation reform increased the number of new firms by about 25%. According to our descriptive evidence, the average annual growth in firm entry increased from 16.5% during 2008-2012 to 34.4% during 2012-2017. Accounting for the time trend prior to the reform, almost all the increase in the growth rate in the aggregate trend can be explained by the entry deregulation itself. 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). The larger effect size in China is likely due to the more comprehensive nature of the reform and stronger enforcement. 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 include 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 that some local resistance to the adoption of the reform in Portugal.

A key underlying assumption for the difference-in-differences strategy is the parallel trend between the treatment and control groups. We check this using the framework of event study outlined in (2). The month before the reform is defined as the baseline group, i.e. β_{-1} is normalized as 0. As demonstrated in Figure 3, none of the coefficient prior to the reform is significantly different from 0, confirming the parallel trend assumption. Right after the reform, there is a sharp increase in the coefficient. The estimates are jointly significant after the reform and the magnitude is still positive

even 15 months post the reform, indicating a persistent increase in the number of new entrants.[21]

Alternative Explanations

Two additional concerns about our baseline results may arise. First, the large impact we document in Table 1 might be mainly driven by the possibility that a significant number of existing individually-operated businesses (*getihu*), which are mostly family-run and small in size, have reclassified their registration status as incorporated firms, to take advantage of the lower registration cost. To address the first concern, we replicate the results in Panel A of Table A1 with the measure of *getihu* entry. If the reclassification of *getihu* drives the result, we would expect a negative impact on the measure. However, Table A1 shows a significant and positive impact on the *getihu* entry, although the effect is smaller at about 60% of the coefficient estimate for new firms. After all, the registration of *getihu* was much easier than that of incorporation in the first place prior to the reform. The result largely rules out the reclassification channel.

Second, the effect may be an artifact of spatial relocation of existing firms from one area to another thanks 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. In specific, we examine if the number of new firms set up by non-local entrepreneurs, whose hometowns are outside the county/city (or province), have exhibited a more rapid increase after the reform.[22] As shown in Table A2, the effect is insignificant for new firms established by entrepreneurs from other counties/cities. The likelihood of entrepreneurs from other provinces even decreases by 3.3%, suggesting that the main impact is unlikely driven by the inflow of entrepreneurs from outside Guangdong province. Instead, entry deregulation promoted local entrepreneurship within the home province.[23]

[21] The DID based on the staggered roll-out could suffer from a well-known problem as dynamic treatment effect: the treatment effect either consistently increases or decreases over time. Our event study of Figure 3 reveals a decreasing treatment effect. According to Goodman-Bacon (2018), the decreasing treatment effect might introduce upward bias to the DID estimate. This is because in the empirical setting where different units receive treatment at different times, earlier-treated group will serve as the control after the treatment of later-treated group begins, and the dynamic treatment effect imposes a differential trend. We try to correct this bias following Goodman-Bacon (2018), but the current decomposition does not accommodate a large set of fixed effects (city by year fixed effects) as in our baseline specification. To remedy this concern, we have done a few robustness checks. First, we only keep 59% of the variation from comparing earlier-treated group (treatment group) and later-treated group (control group), the adjusted effect is 15.5%. Second, only the sample period before the national reform within which the treatment group and never-treated group can be compared. The the adjusted effect is 19.3%, similar to our baseline estimates 21.2%.

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

[23] The other justification is the relatively short reform window. Given the reform was quickly extended to the whole nation in two years, the incentive among firms to relocate is likely small.

Heterogeneity

Administrative entry barriers varied by industry and ownership types. In this section, we explore the heterogeneous effect of entry reform. The registration cost by industry is computed based on the 2018 Enterprise Survey for Innovation and Entrepreneurship in China (ESIEC). The 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 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 questionnaire includes questions on the experience of registration and cost of entry from entrepreneurs. We first calculate the average registration cost at the 2-digit industry level by year of registration. Next we compute the percentage change in the "cost of entry" for each industry in relation to the reform. Finally, a dummy variable $RegCost_i$ is defined as 1 if the industry i is above the median change and 0 otherwise.

We further create a dummy variable $Private_o$, which takes the value of 1 if a firm is registered as privately owned, and 0 if it is in other ownership, such as the state-owned enterprises (SOE) as well as foreign, Hong Kong, Macau and Taiwan invested enterprises (FIE). The registration cost matters little to SOEs and FIEs, as they are less financially constrained than private enterprises.

The estimation results are shown in Table 2. 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 $Private_o$ in Columns (2) and (4). In general, regressions on the two outcome variables yield similar results. As indicated in Column (1) and (3), the reform results in 7.4% more entries in industries which experienced a larger reduction in registration cost after the reform. Column (2) and (4) evaluate the heterogeneity of impacts between different ownership types. The impact of the reform is 15% higher for private enterprises than non-private types, while it's insignificant for the latter group.

3.4 Firm Exit

Classic industrial dynamics literature Hopenhayn (1992) provides theoretical insights about how lower entry cost shifts stationary equilibrium with entry and exit. Therefore, it is important to empirically investigate the impact of the reform on firm exit besides entry.

Table 3 shows the regression results on firm exit. Column (1) controls for fixed effects for city by industry by ownership type, age, and year. The coefficient of the treatment variable is 0.498, significant at 10% level. Given the average exit rate being 5.7% prior to the reform, our estimates indicate that the mean exit rate increase by 8.7% (=0.498/5.7) after the reform. The finding could be due to the market selection effect whereby more intense competition after the reform drives firms

with low profit margin out of the market. The increase in both entry and exit rate implies higher market turnover rate and shorter expected firm lifespan. The effect on firm entry dominates that on firm exit, leading to greater number of firms on the market. In column (2), we further control for firm size, measured by log revenue, which may shape firm exit decisions. We find larger firms are less likely to exit the market, which is consistent with stylized facts of industrial dynamics (Dunne et al., 1988). After controlling for firm size, the effect drops slightly to 0.469.

Similar to the previous analysis on entry, we also conduct event studies on firm exit. . As shown in Figure 4, there is no trend prior to the reform. The effect gets stronger over time after the reform. With increasing number of entries, market competition becomes more intense, resulting in higher exit rate.[24] To further draw 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, meaning that industries with a greater cut in registration cost experience higher exit rate after the reform. This piece of evidence lends us additional confidence in the causality of the main effects.

Another concern of our baseline results is that it might be driven by higher exit rate of new entrants, instead of incumbents. Therefore, in column (4)-(6), we replicate the baseline results in column (1)-(3), but exclude all new entrants. The main effects remain generally robust, with a slightly larger magnitude. This is expected as we have already allowed flexible controls across firm age profiles in our baseline regression. Moreover, we do not find supporting evidence that new entrants are more likely to exit after the entry deregulation. Instead, it is the incumbents that contribute to the baseline results of increased exit rates.

To test the robustness of our baseline estimates, we define 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 between 2017 and 2019. The results are shown in Table A3 and the empirical specification is kept consistent with Table 3. When using the alternative measure, the effect is bigger. The coefficient estimate is 0.820 for incumbents,

[24] Following Footnote 21, our event study of Figure 4 reveals a decreasing treatment effect. According to Goodman-Bacon (2018), the decreasing treatment effect might introduce downward bias to the DID estimate. We try to correct this bias following the decomposition proposed in Goodman-Bacon (2018). We construct a balanced panel at city-industry and year level and replicate the main results. The estimated coefficient is 0.655, larger than 0.498 which might be due to selection on a balanced panel. If we only keep 81% of the variation from comparing earlier-treated group (treatment group) and later-treated group (control group), the adjusted effect is 1.594, 2.433 times as large as the DID estimate without adjustment. If we adjust our baseline result roughly according to this ratio, our estimates indicate a 21.2% increase in the mean exit rate. Given the mean entry rate of 14.7% and exit rate of 5.7% prior to the reform, a 21.2% increase in the exit rate can offset 8.3% out of the 25% increase in the entry rate. Nevertheless, we will interpret our baseline result as a more conservative estimate and the qualitative results of increased exit rate is still robust.

significant at 5% level. Given the average exit rate being 4.8% prior to the reform, our estimates indicate that the mean exit rate increase by 17.1% (=0.820/4.8) after the reform. After controlling for log registered capital, a proxy for firm size, the coefficient slightly increases. In conclusion, we find entry deregulation not only introduces more entrants, but also leads to an increase in the exit rate.

4 Entry Deregulation, Firm Size and Productivity

This section examines the impact of the reform on firm size dynamics and efficiency. Following the theoretical insights of Hopenhayn (1992), entry deregulation cuts down the cost of entry, leading to a massive inflow of new entrants and more intense market competition. Consequently, a group of marginal incumbents that was earning a weak profit margin now quit the market. As these marginal incumbents still have larger firm size and productivity than those previous average exiters, they contribute to the larger average firm size and productivity for exiters after entry deregulation.[25]

Compare with the results on exiters, the forces shaping the size and productivity of new entrants are more complicated. Classical Hopenhayn (1992) is silent towards the prediction along this margin, since entrants are assumed to be ex ante identical. Under the assumption that potential entrants draw their own productivity from a common distribution after paying the entry cost, changes in the entry cost will not alter the size and productivity distribution of new entrants. However, the lower entry cost after the reform enables more smaller firms to enter the market, thereby reducing the average size of entrants.

Regarding the effect on efficiency, two counteracting forces from entry deregulation are at play. First, lax entry regulation implies weaker "*screening channel*", as lower entry barriers enables less efficient firms to enter after the reform, thereby driving down the average productivity of entrants. Second, entry reform may remove some distortions inherent in bureaucratic hurdles and administrative barriers, unleashing previously depressed entrants' efficiency, as documented in previous literature (Khandelwal et al., 2013; Tombe and Zhu, 2019). Moreover, the lowered registration cost associated with the reform relaxes the financial pressure of entrants and enables them to achieve better scale of production, leading to higher productivity. These forces contribute to the "*efficiency channel*". The overall impacts on entrants' productivity remain theoretically ambiguous, depending on which channel dominates.

For the rest of this section, we first explore the impacts of entry deregulation on firm size. Then we estimate firm productivity using a structural model, before investigating the efficiency implica-

[25] An alternative interpretation is that incumbents are making exit decision based on a comparison between their own profitability and the exit threshold. As the profit margins get cannibalized, previous marginal firms find it unprofitable to continue on the market. It raises the exit threshold of profitability and further increases the average firm size and productivity of exiting firms compared with continuing incumbents.

tions. Next, we disentangle the mechanisms underlying the efficiency results and discompose the aggregate productivity.

4.1 Firm Size

Panels (a) and (b) of Figure 5 display the distribution of log firm revenue and capital for the *entrants* before and after the reform. As evident from the figure, there is a clear shift in the size distribution toward the left after entry deregulation. The mean revenue of entrants decreased by 18.6% after the reform, while the mean capital shrank by 21.7%.

We adopt a triple difference strategy to identify causal impacts on firm size of entrants/exiters. The continuing incumbents are set as the baseline group to absorb the effects of all unobserved macroeconomic confounders. The main empirical model is (3). The dependent variable y_{jt} is the size measure of firm j in year t. As firm size is measured annually, we utilize the *yearly* variation in the reform timing. A firm of age 1 is defined as the entrant $\mathbf{E}_{jt}$, and a firm which will exit next year is defined as the exiter $\mathbf{X}_{jt}$.[26] We further interact $\mathbf{E}_{jt}$ and $\mathbf{X}_{jt}$ with the treatment dummy $\mathbf{D}_{ct}$. Therefore, β_1 (β_2) captures the difference between the entrants (exiters) and continuing incumbents before the reform, while β_3 (β_4) captures the impacts on the size difference by entry deregulation. Furthermore, we include city by year fixed effects and city/industry/ownership type triple interactive fixed effects. The key identification assumption is that the difference between the entrants (exiters) and continuing incumbents follow the same trend in treated units and comparison units, if the reform does not take place.

$$y_{jt} = \beta_1 \mathbf{E}_{jt} + \beta_2 \mathbf{X}_{jt} + \beta_3 \mathbf{D}_{ct} \mathbf{E}_{jt} + \beta_4 \mathbf{D}_{ct} \mathbf{X}_{jt} + \mathbf{Z}_{jt} + \xi_{cio} + \varepsilon_{jt} \tag{3}$$

In order to test the identification assumption and explore the dynamics of the effects, we adopt an event study model as (4). Our key interest is in β_{3q} and β_{4q}, which captures the dynamic impacts of entry deregulation of firm size differences. Again, one year before the reform is taken as the baseline group.

$$y_{jt} = \beta_1 \mathbf{E}_{jt} + \beta_2 \mathbf{X}_{jt} + \sum_{q=-4}^{3} \beta_{3q} \mathbf{D}_{cq} \mathbf{E}_{jt} + \sum_{q=-4}^{3} \beta_{4q} \mathbf{D}_{cq} \mathbf{X}_{jt} + \mathbf{Z}_{jt} + \xi_{cio} + \varepsilon_{jt} \tag{4}$$

The main empirical results are shown in Table 4. We measure firm size with log revenue in column (1)-(3), and we use log intermediate input as an alternative firm size measure in column (4)-(6).[27] In column (1), we control city by industry, city by year as well as industry by year fixed

[26] Depending on the registry month, a new entrant might only operate for several months in the registry year. To avoid the heterogeneity, we only keep the sample from the second year on of each firms' operation, and thus all full-year annual information.

[27] Intermediate input is not directly observed in the data. However, we use the revenue and total tax to back out the

effects, which is consistent with our model specification for market turnovers. In column (2) and (3), we further control for the heterogeneity across city, industry as well as ownership types, and time dummies for each industry and ownership type pair. The coefficients in Columns (2) and (3) change little from Column (1). Entrants and exiters are smaller than continuing incumbents before the reform, consistent with stylized facts in industrial dynamics (Dunne et al., 1988). After the reform, entrants becomes smaller, while exiters becomes larger. Although post the reform entrants and exiters are both still smaller than incumbents, the difference between entrants and incumbents has increased by 20.4%, while the difference between exiters and incumbents has shrunk by 27.4%. Different forces are behind these changes. The decline in the size of entrants is a mechanical result of lower entry barriers, while a larger average size of exiters is due to market selection forces, i.e. massive entrants drive profitability cutoff up for firm exit, resulting in larger firms being off the market.

We validate the identification assumptions of triple difference using empirical model (4) in Figure 6. We don't observe significant pre-trends for entrants and exiters, which justifies our empirical strategy. Interestingly, the impacts of entry deregulation on entrants' and exiters' sizes increase over time, though in opposite directions.

4.2 Productivity Estimation

We next evaluate the efficiency implications of entry deregulation. We estimate firm productivity using a simple structural model, following the spirit of Aw et al. (2011) and Peters et al. (2017), but abstracting away the dynamic decision. In addition, we construct the alternative measures of firm productivity following OP and LP as robustness checks. 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 the supply side by firm i's short-term marginal cost

intermediate input, under the assumption that manufacturing firms mainly pay for value added tax, and the tax rate is homogeneous at 17%. If we measure firm size with log tax, the results are robust as well.

function

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

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. m represents a market. Conditional on firm age, the idiosyncratic cost is assumed to come from either firm specific capital stock or its productivity. Additionally, we include W_{mt}, a vector of input prices in market m, and Z_{om}, a vector of common cost shifters shared by the firms with type o in market m. Furthermore, productivity is assumed to follow a Markov process.

$$\varphi_{it} = g(\varphi_{it-1}) + \xi_{it} = \alpha_0 + \alpha_1 \varphi_{it-1} + \alpha_2 \left(\varphi_{it-1}\right)^2 + \alpha_3 \left(\varphi_{it-1}\right)^3 + \xi_{it} \tag{5}$$

where the function $g(.)$ is specified as a cubic function and 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.

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

$$\pi_{it} = \frac{r_{it}\left(\Phi_t, k_{it}, W_{mt}, Z_{om}, \varphi_{it}\right)}{\eta} \tag{6}$$

Identification and Estimation

The key identification issue rising in productivity estimation is the endogeneity problem, 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 to express φ_{it} with the intermediate input M_{it}, which directly responds to the productivity. We construct a flexible function $h(\cdot)$ of intermediate input M_{it} and capital k_{it} following Aw et al. (2011) and Peters et al. (2017).

$$\ln r_{it} = \alpha_0 + (1-\eta)\left(\beta_w W_{mt} + \beta_a a_{it} + \beta_Z Z_{om}\right) + h(k_{it}, M_{it}) + \varepsilon_{it} \tag{7}$$

The identification assumption is that the remaining error term ε_{it}, after flexibly controlling M_{it}, is a random shock and exogenous to the capital stock.

In the empirical revenue model, we define the market as each city and two-digit industry pair. We include year dummies to control for Φ_t. To proxy W_{mt}, we use average annual wage and the number of incumbents for each city and industry.[28] Annual wage is a proxy of labor cost, and the

[28] 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.

number of incumbents captures the competition effect, as a larger number of incumbents lead to more intense market competition and higher input prices.

Furthermore, we include several sets of factors to account for Z_{om}. First, we include city dummies and industry dummies to capture the market-level cost shifters. Second, we add firm ownership type fixed effect to capture their different cost structures. For example, SOEs have access to cheaper credits and are subject to more favorable government supports than their private counterparts. Moreover, we incorporate a cubic function of firm's registered capital to control for potential non-linearity of government procurement, external financing and supplier matching in relation to initial capital size.

Our estimation proceeds in three steps. We first estimate the elasticity of substitution $\hat{\eta}$ in the demand curve by (6) using OLS. Next, we estimate $\hat{h}(\cdot)$ from (7). With estimated $\hat{h}(\cdot)$ and the elasticity of substitution $\hat{\eta}$ in hand, we plug them back to the law of motion for productivity (8) to estimate β_k and the parameters in the productivity dynamics α_{0-3}, via non-linear least squares (NLS).

$$\begin{aligned}\hat{h}_{it} =&\beta_k(1-\hat{\eta})\ln k_{it} - \alpha_0(1-\hat{\eta}) + \alpha_1\left(\hat{h}_{it-1} - \beta_k(1-\hat{\eta})\ln k_{it-1}\right)\\ &-\alpha_2(1-\hat{\eta})^{-1}\left(\hat{h}_{it-1} - \beta_k(1-\hat{\eta})\ln k_{it-1}\right)^2\\ &+\alpha_3(1-\hat{\eta})^{-2}\left(\hat{h}_{it-1} - \beta_k(1-\hat{\eta})\ln k_{it-1}\right)^3 - \xi_{it}(1-\hat{\eta})\end{aligned} \tag{8}$$

Finally, we could back out firm level productivity estimates as

$$\hat{\varphi}_{it} = -\frac{\hat{h}_{it}}{1-\hat{\eta}} + \hat{\beta}_k \ln k_{it} \tag{9}$$

Estimation Results

As we discussed in the Section 2.3, a disproportionate amount of firms reporting almost zero profits restricts us from using firm annual report data to estimate the elasticity of substitution.[29] Alternatively, we use the Guangdong sample of the National Tax Survey Database for a better profit measure. We utilize the revenue and profit data directly to estimate η, and the results are shown in Table A4. We adopt several different model specifications to account for heterogeneity across cities, industries and time, and the elasticity estimates remain robust. 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 $\hat{h}(\cdot)$. Although our data lacks the values of intermediate goods, we

[29] 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 RMB (1,540 USD).

can back out a crude measure of intermediate goods value using firm revenue and tax because the tax rate is rather homogeneous within the manufacturing sector. Given value-added tax (VAT) to be the most important tax imposed on manufacturing firms, we take the prevailing value-added tax rate of 17% and impute the value of intermediate goods as $Intermediate\ Goods\ Value = Revenue - \frac{Tax}{17\%}$.[30] The mean ratio of value-added to total revenue is around 20%.

The estimation results of (7) are shown in Table A5. In column (1), we include city, industry and year interacted FE to control for all unobserved demand and cost shifters on the market. $\hat{h}(\cdot)$ is precisely estimated. There is a non-linear relationship between registered capital on firm revenue. Older firms have on average larger revenues. In column (2), we further include incumbents and wage, and find the coefficients for the two variables are both positive. The positive wage coefficient is likely driven by reverse causality, as more profitable firms tend to enter larger markets and can afford higher wages to workers. To remedy this concern, we use the entry deregulation reform dummy and its interaction with *RegCost* as instruments, given the policy changes were exogenously imposed. As exhibited in Column (3), the coefficients for the number of incumbents and average wage turn negative. The IV estimates of $\hat{h}(\cdot)$ barely change from the OLS estimates. The adjusted R^2 in the three regressions is as high as 0.975, indicating a very good fit of the models.

With the estimates of $\hat{\eta}$ and $\hat{h}(\cdot)$ in hand, we further proceed to estimate (8) via NLS. Allowing for the structural parameters to vary across 2-digit industries, we separately estimate the productivity for each industry. However, for simplicity, we present the structural coefficient estimation by pooling all the industries together in Table 5. β_k is significantly negative, suggesting that firms with larger capital stock enjoy lower marginal cost. α_1 is estimated to be 0.829, indicating high persistence in the productivity evolution. The estimates of α_2 and α_3 also reveal a nonlinear productivity evolution process. With these coefficient estimates, we further back out firm productivity measure via (9). The lower panel of Table 5 summarizes the correlation coefficient of the estimated productivity with other variables in our data. Productivity is positively correlated with revenue, intermediate goods, capital stock, returns on assets (ROA, sales/capital) as well as firm ages, consistent with our expectations. We further validate the productivity measure by plotting its relationships with firm age and size group. As shown Figure A2, estimated productivity is both increasing with firm age and size groups, in accordance with with stylized facts in firm dynamics.

[30] We take an over-simplified approach to back out the value of intermediate goods from revenue and tax data. Two major caveats are worth mentioning. First, firms have to pay for other types of taxes, upon value added tax. Second, the actual tax scheme of value added tax is more complicated than just paying 17% of the value added. However, we discuss later why the simplistic approach we take here is not likely to bias upward our main results.

Discussion

There are some additional concerns about the productivity estimates, including omitted employment size, measurement errors in intermediate inputs as well as selection bias. We discuss each of them in turn.

Labor as the Omitted Variable We only have firm employment information in 2013-2016, which is shorter than the sample period of policy reform. In order to keep the sample period consistent throughout the paper, we rely on a structural model to estimate productivity. However, labor might still be one important omitted variable in ε_{it}, and it may bias our estimate. As a robustness check, we restrict our sample to 2013-2016 and estimate productivity via OP and LP by incorporating labor information.

Measurement Errors in Intermediate Inputs The estimate of intermediate inputs is likely subject to imputation errors. There are two major issues. First, firms have to pay other types of taxes, besides value added tax. Second, the actually paid value added tax may be different from the statutorily defined rate of 17%. Especially, for "small-scale VAT taxpayers", their tax rate is 3% but without input VAT deductible. We address these concerns using three different approaches. First, we utilize returns on asset (ROA, which is defined as revenue asset ratio) as a proxy for productivity to validate our empirical results. Second, we use OP estimates as a robustness check. Since the OP estimates do not use intermediate goods measure, they are not subject to measurement errors in intermediate inputs. Third, we control for a flexible function of firm size in the empirical analysis.

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. According to OP, selection bias tends to be small compared with simultaneity issues. Moreover, the OP estimates as robustness check could largely address this concern.

4.3 Firm Productivity

We first look at the basic data patterns of firm productivity in Figure 5. Panel (c) plots the distribution of firm productivity for new entrants before and after the reform. The distribution has slightly shifted to the right after the reform, indicating an improvement in average productivity of new firms. The mean productivity of entrants increases by 4.2% after entry deregulation. Panel (d) displays the mean productivity with respect to firm size among new entrants. There is a clear improvement in firm productivity after entry deregulation, especially for small firms. Since small

entrants were likely more constrained by entry barriers in the first place prior to the reform, their productivity gains resulting from the reform are naturally more evident.

We follow (3) outlined in 4.1 as our baseline regression model and employ (4) to explore effect dynamics using firm productivity as the outcome variable.

The main empirical results are shown in Table 6. We use our structurally estimated firm productivity as the main outcome in column (1)-(3). Compared with continuing incumbents, entrants and exiters have lower productivity before the reform. However, the average productivity of both entrants and exiters have increased compared with continuing incumbents after entry deregulation. The reform decreased the productivity gap between entrants and continuing incumbents by 19.9%, and that between exiters and continuing incumbents by 44.2%. The latter is consistent with the findings in Table 4 that entry deregulation leads to a larger firm size for exiters.

Interestingly, we find entry deregulation improves entrants' productivity as well. Although lax entry regulation and weaker screening effect allow entrants of lower productivity into the market, it seems that the "*efficiency channel*" effectively dominates the "*selection channel*", leading to higher average productivity for new entrants.

Same as previous excises for firm dynamics and size, we also use the event study to check the effect dynamics. Figure 7 does not reveal a pre-trend for both entrants and exiters. After entry deregulation, the effect grows over time.

In column (4)-(6), returns on asset (ROA, which is defined as the log ratio of revenue to asset) are used as alternative measure of productivity. The main results still hold. To further draw the concern about omitting labor in the productivity estimation, we restrict our sample to 2013-2015 and estimate productivity using OP and LP. OP uses investment as a proxy for productivity, while LP uses intermediate input instead. As shown in Figure A3, the two measures are highly correlated with our baseline estimate. Table 7 repeats Table 6 by using the OP and LP measures as alternative outcomes. The empirical results are qualitatively consistent with our baseline results in Table 6. Entry deregulation increases productivity for both entrants and exiters, and decreases 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).

4.4 Mechanisms of Entrants' Productivity

We try to disentangle three potential channels behind the improvement of entrants' efficiency in this section.

Market Competition

The first channel is market competition. More intense market competition as a result of the reform means lower profitability, which may prevent inefficient potential 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 who entered before the reform and after. The latter group experienced the drop in entry cost, while the former group did not. Let's consider an example of two firms *a* and *b* entered in city *X* with the pilot starting in June 2013. Suppose *a* entered the market in May while *b* registered in July. Both experienced intense competition post entry, but only *b* enjoyed a lower entry barrier. The impacts of entry deregulation on firm *a* hence capture the market competition channel.

The empirical results are shown in Table A6. Compared with continuing incumbents, post entry deregulation, entrants who entered before and after the reform have smaller firm sizes, as both market competition and lower entry barriers drive firm size down. Comparatively, entrants who entered after the reform are even smaller, which results from two effects reinforcing each other. For firm productivity, the main effects we find in Table 6 concentrates in entrants who entered after the reform. More importantly, there is only a marginal impact on the efficiency of entrants who entered before entry deregulation, which indicates a weak effect of market competition. Therefore, market competition might not be the main driving force in improving entrants' efficiency.

Financial Constraint

The second channel is financial constraint. Since entrants save expenditures on administrative entry cost, which alleviates their financial stress, they could have extra resources available to achieve better scale of production, adopt more cost-saving technology and make superior production decisions. We provide three sets of evidence in support of the financial constraint mechanism.

Heterogeneous Effect Table 8 presents the heterogeneous effect of the reform in several dimensions. First, we divide the sample into groups based on the registration cost at the industrial level. As shown in the first two columns of the upper panel, entrants in "more deregulated" industries have experienced a greater productivity gain after the reform with a larger relief in financial stress. Second, as illustrated in Columns (3)-(5) of the upper panel, the effect is larger for private types, especially non-corporate private enterprises, than SOEs and FIEs. As non-corporate private enterprises are more financially constrained compared to corporations and non-private types, this piece of evidence lends support to the financial constraint channel.

Third, we classify city-industry pairs into two groups according to the ratio of fixed asset to revenues, using data from the National Tax Survey Database. The group with higher *Fixed Asset*

indicates heavier ex-ante investment and larger physical sunk cost of entry, which is much larger than the administrative registration cost. Thus the reduction in the administrative cost of entry brought about by the reform matters less financially to this group, which is exactly what we observe the middle panel under *Fixed Asset*.

Fourth, we use *Registered Capital*, the ratio of registered capital to capital stock available from the annual report, to measure the cost of entry. The high paid-in registered capital requirement prior to the reform was a major financial burden to the startups, and the reform removes the requirement. As expected, the group with higher *Registered Capital* has shown greater improvement in efficiency.

Entrepreneurs' Characteristics We next explore how entry deregulation changed the profile of entrepreneurs based on the major characteristics of legal representatives of firms registered in 2009-2015 contained in the Firm Personnel Database. We exploit the same empirical model specification as (1) and present the results n Table 9.

After the reform, the average years of schooling of entrepreneurs drop by 1.9% lower years and the proportion of entrepreneurs with secondary education and above comes down by 6.3%. In addition, the share of entrepreneurs younger than 35 has increased by 9.0% (=0.028/0.310). The gender composition remains stable after the reform.

Here we add a cautious note on the problem of missing information about education and age data. Only 1/3 and 1/4 of the sample include records on education and age. Nevertheless, our findings provide suggestive evidence that entry deregulation induces less educated and younger entrepreneurs to become entrepreneurs. This group of entrepreneurs were likely faced with more financial constraints than those more educated and older entrepreneurs. An increasing number of entry among this group offers suggestive evidence of the financial constraint channel in improving entrants' efficiency.

Shareholding Structure We further investigates the impacts on the shareholding structures in Table 10. Based on the Firm Shareholder Database, we count the number of individual and firm shareholders in our sample. As shown in the first three columns, after the reform, the average number of shareholders remains unchanged, while the ratio of individual sharholders increases by 1.9% and the share held by individuals rises by 2.3%. New sole proprietorship firms (all shares being owned by individuals) goes up by 2.0%. Among them, new firms owned entirely by 2 individual shareholders increase disproportionately. In a word, individual shareholders have played a greater role in new firms after deregulation, both in terms of sheer number of shareholders and the actual share owned. Since individual shareholders are comparatively more financially constrained

than firm shareholders, the rise in the importance of individual shareholders implies that financial constraints facing individual shareholders have become less binding after the reform.

Government Rent-seeking

The third channel is government rent-seeking. The reform cuts bureaucratic hurdles and administrative barriers, which used to give officials opportunities to seek rents from potential entrants, allowing unproductive but favored entrants into the market. A more enabling environment after the reform gives a level playing fields to productive firms.

However, it is of a great challenge to measure the distortions and rent-seeking behavior associated with entry barriers. We exploit some indirect approaches to test this channel. The size of the state sector in local economy is associated with the degree of entry distortions as documented in Brandt et al. (2020). We measure state sector size as the revenue share of SOEs using 2008 Chinese Economics Census. Moreover, government rent-seeking is less feasible with large volumes of entrants, and larger entry distortions are likely to result in a lower entry rate before entry deregulation. We measure the entry rate at the city-industry level using firm registration data in 2008. We further split the sample into two, depending on whether the measure is above the city-industry median. The results are shown in Table 8. We could neither detect any differences in the main effects between sub-samples with different SOE shares, nor do we find larger impacts in the sub-samples with low entry rates. The extent of efficiency improvement among entrants does not depend on state sector sizes, and is higher where entry rate is relatively larger.

Moreover, we explore the impacts of entry deregulation on productivity distribution in other industries. Government rent-seeking might be important for the manufacturing sector, but will be less significant in some service industries where a large number of small firms dominate. We choose four service industries, including wholesale trade, retail trade, accommodation and food Services as well as neighborhood services, for which the incentives of government rent-seeking in entry selection are weaker. As we could not properly construct the productivity measure due to the data limit, we instead proxy productivity using returns on asset. The results are shown in Figure 8. Despite weaker government rent-seeking incentives, we observe a consistent and robust improvement in operation efficiency post deregulation. Therefore, this suggests that government rent-seeking might not be the first-order reason to the efficiency improvement among entrants.

Combining the evidence and discussion above, we interpret that alleviated financial constraint might be the main driver of efficiency improvement of entrants. We do not have strong evidence for other two channels.

4.5 Aggregate Effects

In this section, 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), we explore two different decompositions. The first one follows Baily et al. (1992) and Foster et al. (2001), which is given by

$$\begin{aligned}\Delta TFP_t = &\underbrace{\sum_{i\in C}\theta_{it-1}\Delta tfp_{it} + \sum_{i\in C}(tfp_{it-1} - TFP_{t-1})\Delta\theta_{it} + \sum_{i\in C}\Delta tfp_{it}\Delta\theta_{it}}_{Incumbents} \\ &\underbrace{+\sum_{i\in N}\theta_{it}(tfp_{it} - TFP_{t-1})}_{Entrants} \underbrace{-\sum_{i\in X}\theta_{it-1}(tfp_{it-1} - TFP_{t-1})}_{Exiters}\end{aligned} \tag{10}$$

where TFP_t denotes revenue-weighted average productivity of year t, θ_{it} is the revenue share and tfp_{it} is the productivity of firm i in year t. Furthermore, C denotes continuing incumbent firms, N represents new entrants, while X includes all the exiting firms. The five terms on the LHS of equation (10) represents the within-firm effect, between-firm effect and cross effect for the continuing incumbents, as well as the effects brought by firm entry and exit respectively. We sum the first three terms together and denote that as the contribution brought by incumbents.

The second approach for productivity decomposition follows Griliches and Regev (1995), which is given by

$$\begin{aligned}\Delta TFP_t = &\underbrace{\sum_{i\in C}\bar{\theta}_i\Delta tfp_{it} + \sum_{i\in C}(\overline{tfp_i} - \overline{TFP})\Delta\theta_{it} +}_{Incumbents} \\ &\underbrace{\sum_{i\in N}\theta_{it}\left(tfp_{it} - \overline{TFP}\right)}_{Entrants} \underbrace{-\sum_{i\in X}\theta_{it-1}\left(tfp_{it-1} - \overline{TFP}\right)}_{Exiters}\end{aligned} \tag{11}$$

where the bars over a variable represents the average of the variable between the period t and $t-1$. There are four terms in equation (11), which represents the within-firm effect and between-firm effect for continuing incumbents, as well as the entry effect and the exit effect. The sum of the first two terms stand for the contribution of incumbents.

We conduct two sets of productivity decomposition based on the above two approaches. First, we decompose yearly aggregate productivity based on information from all the cities and industries into three components. Second, we decompose productivity for each city and industry pair and then use the difference-in-differences framework to investigate the impacts of entry deregulation on each productivity component.

The decomposition results of aggregate productivity by year is shown in Table 11. The upper

panel shows the results from equation (10). Since the implementation of entry reform, the contribution of entrants to overall productivity growth turns positive from negative before the reform. Entrants contribute to 15.4% and 21.2% in 2013 and 2014 respectively to the overall productivity change, while the impact of exiters is smaller than entrants across years. The contribution of incumbents does not reveal a clear trend, probably reflecting the fluctuations of the macroeconomic situations. This pattern is robust to the alternative productivity decomposition following Griliches and Regev (1995) as shown in the lower panel.

We further explore the impacts of entry reform on each component of the aggregate productivity change. We calculate the productivity change and decompose it at the city and industry level, and the results are presented in Table A7. Although there is lack of statistical power to precisely estimate the coefficients, the table provides some suggestive evidence that entry reform increases the contribution of entrants in the overall productivity change, while the contribution of incumbents is weakened. The overall impacts on the component contributed by exiters are relatively marginal.

We also conduct a back-of-the-envelope calculation about the overall economic impacts of the reform. Given the mean entry rate of 14.7% and exit rate of 5.7% prior to the reform, a 8.7% increase in the exit rate can only offset 3.4% out of the 25% increase in the entry rate. Therefore, for the average number of new firms being 42,000 in 2009-2012 in Guangdong's manufacturing sector, a 21.6% increase in the net firm entry can be translated into 9,072 new firms annually. New firms on average have 12 employees and 3.5 million RMB annual revenue. Consequently, the reform brought 0.11 million new employment, accounting for 1.0% of the total employment in the manufacturing sector in 2014.[31] The reform generated a total value-added of 6.35 billion RMB if we take the 20% value-added ratio as calculated above. It accounts for 0.23% of the total value-added and 2.9% of the value-added growth in the manufacturing sector in 2013. As the average wage income was reported as 45,824 RMB, the reform brought a total income of 4.98 billion RMB from the manufacturing sector, 0.94% of the growth in GDP.[32] Since our excise includes only the manufacturing sector but not the service sector, the estimate represents a lower bound. Moreover, the back-of-the-envelope calculation focuses only on the contribution of intensive growth margin to GDP growth. If including the intensive growth margin on productivity improvement, the contribution would be even higher.

[31] As missing data issues are severe in 2013, we instead calculate the total employment of the manufacturing sector in 2014 as 10.94 million.

[32] The wage statistics are from http://stats.gd.gov.cn/tjfx/content/post_1435186.html. The value-added and GDP statistics are from http://stats.gd.gov.cn/tjgb/content/post_1430118.html.

5 Conclusion

This paper examines the impacts on industrial dynamics as well as the efficiency implications of entry deregulation. We focus on the Business Registration Reform in China and exploit the staggered rollout pattern of the pilot program across cities in Guangdong province, to identify the causal impacts of entry deregulation.

We find entry deregulation greatly boosts firm entry by around 25%, which is persistent along the time. Firm exit rate also becomes 8.7% higher. Furthermore, we find that compared with continuing incumbents, new entrants become relatively smaller after the reform, while exiting firms are relatively larger. Based on the structurally estimated productivity measure, we find entry deregulation improves entrants' efficiency. We provide a set of evidence and discussion about the main channels, and interpret that alleviated financial constraint might be the main driving force of efficiency improvement for new entrants. After entry deregulation, the contribution of entrants to overall productivity growth turns positive, a sharp change from all negative numbers before the reform. Entrants contribute to 15.4% and 21.2% in 2013 and 2014 respectively to the overall productivity growth. Our back-of-the-envelop calculation suggests that the reform brought 0.11 million new employment, a total value-added of 6.35 billion RMB and a total income of 4.98 billion RMB from the manufacturing sector, which accounts for 0.94% of the growth in the total GDP.

There are several caveats of our paper. First, our empirical evidence focuses solely on the supply side. An important research question about the consumer welfare implications of entry regulation cannot be answered within the current framework. As screening out entrants which supply low-quality products and exert negative externality being one of the major rationales to impose entry regulation, evaluating the impacts of entry deregulation on product quality, market externality and consumer welfare will give a more thorough evaluation of entry regulation. Second, our empirical analysis is silent about incumbents' strategy in accommodating massive entry, except the exit decision. Richer outcomes such as investment/divestment and product re-positioning for incumbents might be important to explore. Third, another trade-off of entry deregulation is the improved market competition and wasted sunk entry cost, as highlighted in Mankiw and Whinston (1986) due to the externality of market cannibalization. However, we could not answer whether the entry on the market is excessive or not compared to social optimal without the aid of a structural model. Therefore, to zoom into an oligopolistic industry and explore the impacts on product quality, incumbents' decision and consumer welfare of reducing entry cost will be interesting future work.

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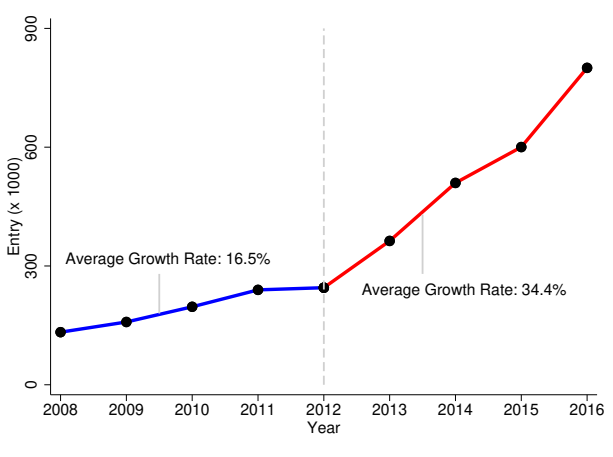
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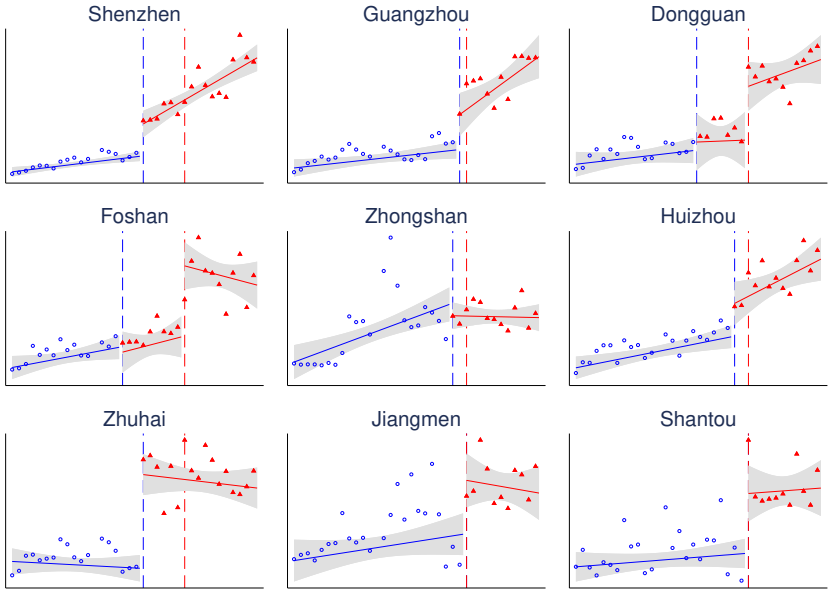
Figures

Figure 1: Number of New Entrants 2008-2016



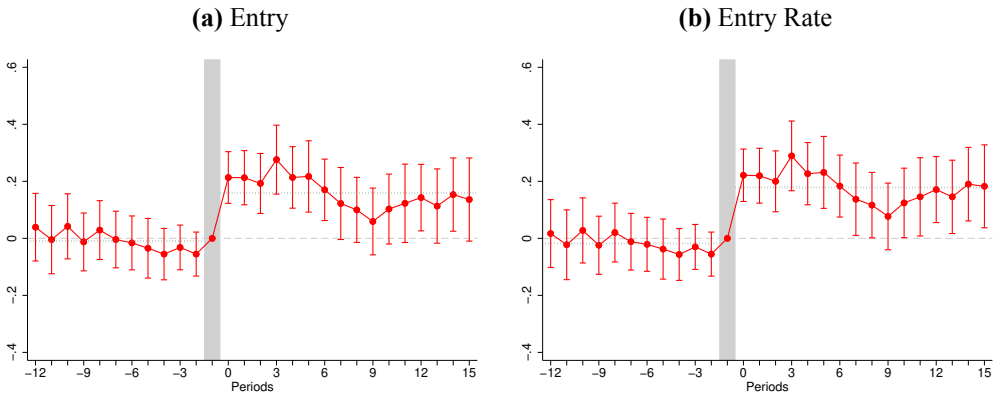
Note: The figure depicts time trend of firm entry of whole industries in Guangdong province tabulated based on Firm Registry Database. The blue segment denotes the series before the reform, and the red segment denotes the series after the reform.

Figure 2: Firm Entry in 2011-2015 (9 Largest Cities)



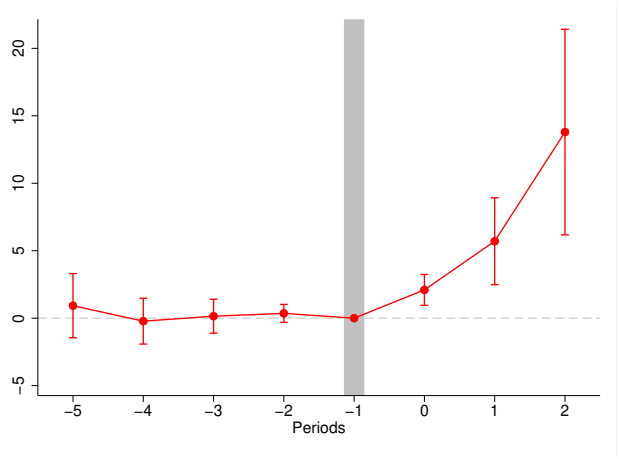
Note: The figure depicts time trend of firm entry of whole industries in Guangdong province for 9 cities with most incumbent firms. The blue dashed vertical line represents the starting month of pilot reform and the red vertical line represents March 2014 when the reform was extended nationwide. The blue and red dots are data points before and after the (pilot) reform respectively. Linear fit as well as 95% confidence intervals are also added.

Figure 3: The Effects on Firm Entry: Event Study



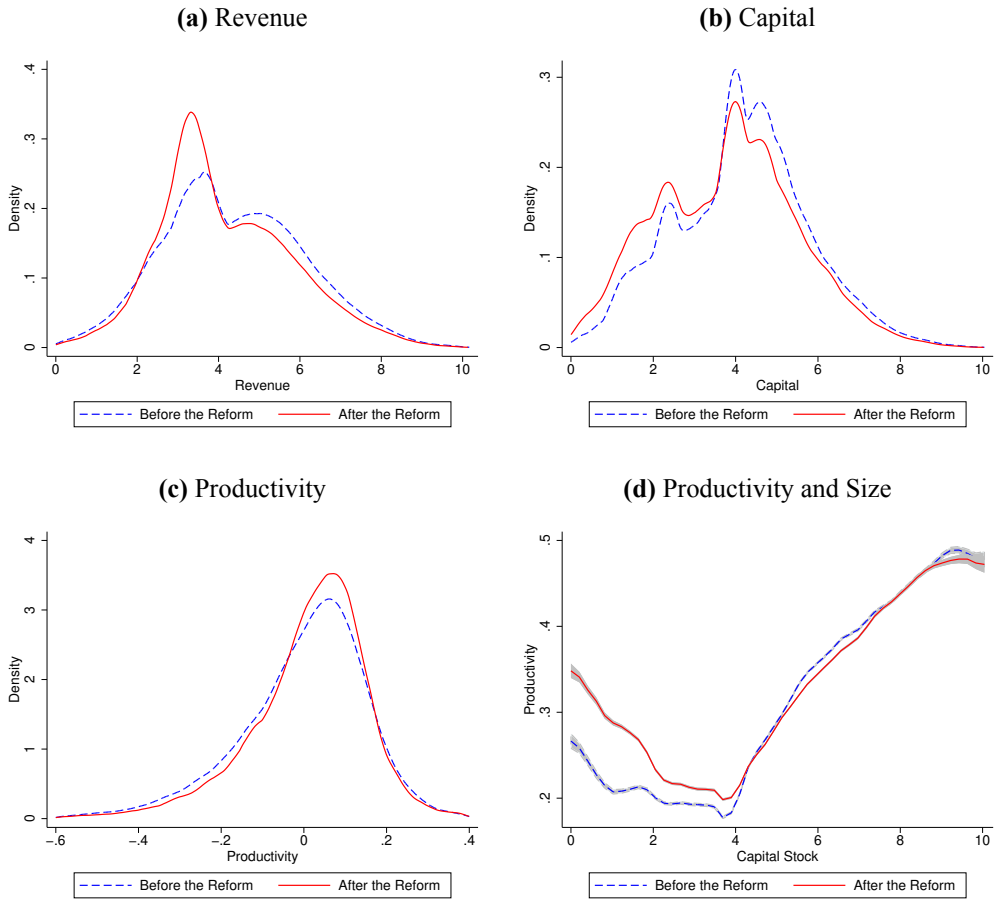
Notes: See equation (2) for specifications. 95% confidence intervals are constructed from robust standard errors, which is clustered at the city and industry level.

Figure 4: The Effects on Firm Exit: Event Study



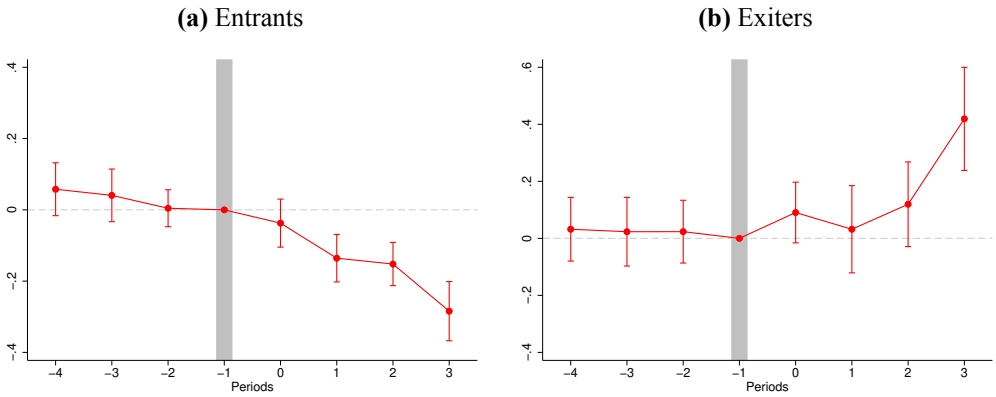
Notes: See equation (2) for specifications. 95% confidence intervals are constructed from robust standard errors, which is clustered at the city and industry level.

Figure 5: Firm Size and Productivity Before/After the Reform



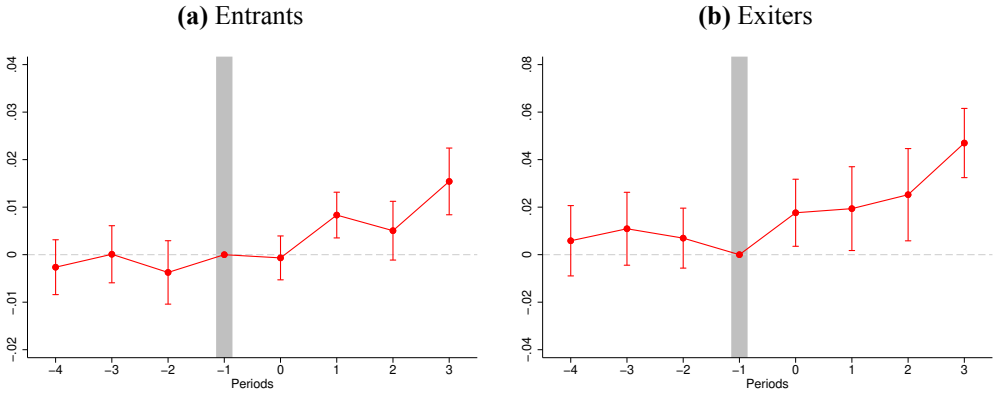
Notes: Panel (a), (b) and (c) show kernel density of entrants' revenue, capital and productivity for the sub-sample before and after the reform respectively. Firm revenue and capital are both log-valued. We partial out a cubic function of capital from productivity to construct Panel (c). Panel (d) shows the local polynomial fit of entrants' productivity with respect to the capital stock, for the sub-sample before and after the reform respectively. 95% confidence intervals are also included.

Figure 6: The Effects on Firm Size: Event Study



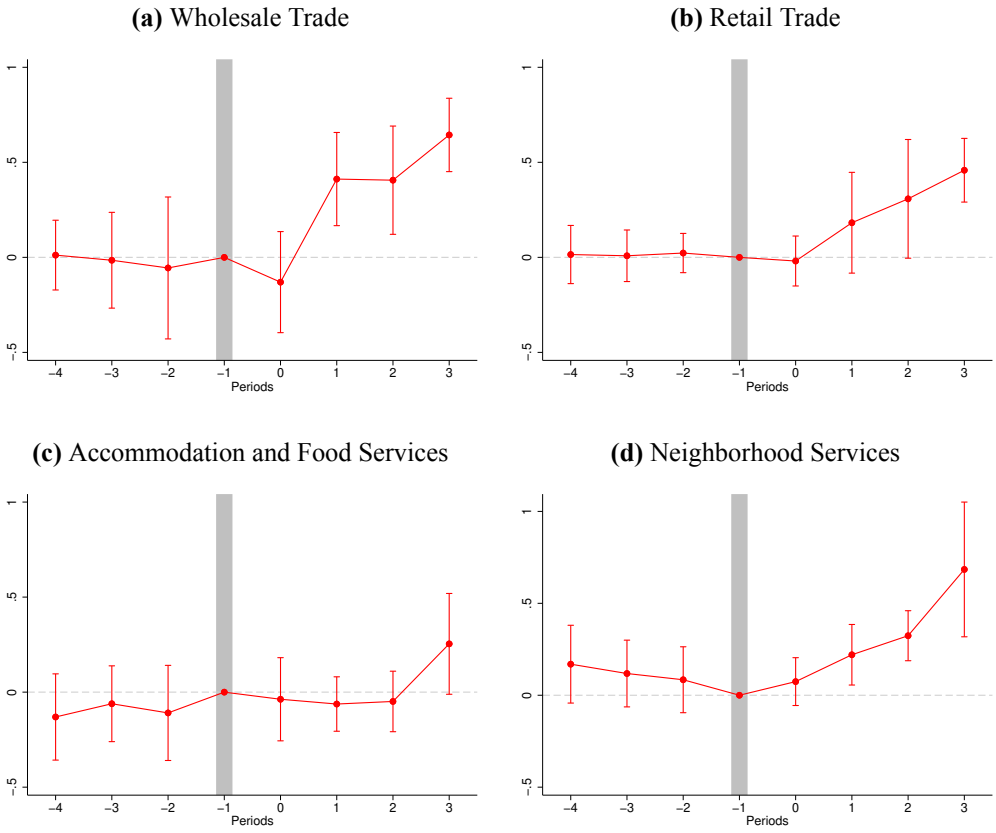
Notes: See equation (4) for specifications. 95% confidence intervals are constructed from robust standard errors, which is clustered at the city and industry level.

Figure 7: The Effects on Firm Productivity: Event Study



Notes: See equation (4) for specifications. 95% confidence intervals are constructed from robust standard errors, which is clustered at the city and industry level.

Figure 8: The Effects on Firm Productivity: Other Industries



Notes: See equation (4) for specifications. 95% confidence intervals are constructed from robust standard errors, which is clustered at the city and industry level.

Tables

Table 1: The Effects on Firm Entry

	Entry			Entry Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Firm Registry						
Treatment	0.212***	0.212***	0.245***	0.221***	0.262***	0.248***
	(0.043)	(0.043)	(0.037)	(0.042)	(0.043)	(0.037)
Observations	53709	53709	53709	53709	53709	53709
Adjusted R^2	0.941	0.941	0.950	0.689	0.699	0.733
Panel B: Firm Entry (Ever Filed Annual Report)						
Treatment	0.236***	0.278***	0.262***	0.236***	0.278***	0.262***
	(0.042)	(0.044)	(0.036)	(0.042)	(0.044)	(0.036)
Observations	51641	51641	51641	51641	51641	51641
Adjusted R^2	0.945	0.947	0.955	0.679	0.689	0.730
Panel C: Firm Entry (Ever Reported Positive Capital Stock)						
Treatment	0.223***	0.271***	0.249***	0.223***	0.271***	0.249***
	(0.047)	(0.048)	(0.036)	(0.047)	(0.048)	(0.036)
Observations	51431	51431	51431	51431	51431	51431
Adjusted R^2	0.944	0.946	0.954	0.702	0.712	0.750
City-Industry, Time FE	✓	✓	✓	✓	✓	✓
City-Year	✓	✓	✓	✓	✓	✓
City-Month FE		✓	✓		✓	✓
Industry-Time FE			✓			✓

Notes: See equation (1) for specifications. The observation is at the city, industry and month of year level, with sample period of 2009M1-2016M12. We only keep the sample of the manufacturing sector. The dependent variable of column (1)-(3) is the log number of new entrants in each city, industry and month. The dependent variable of column (4)-(6) is the entry rate, which is defined as the ratio of entrants to lagged incumbents (%). 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 revoked or cancelled firms. 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. Robust 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: 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	83391	99659	83391	99659
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: The observation is at the city, industry, ownership type (private or non-private) and month of year level, with sample period of 2009M1-2016M12. The dependent variable of column (1)-(2) is the log number of new entrants in each city, industry and month. The dependent variable of column (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 of 1 if the industry experienced high decrease in the reported registration cost. Private is a dummy variable which takes the value of 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 pair 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. Robust standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 3: 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	1392721	1375813	1340428	1290437	1275036	1241527
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: See equation **??** for specifications. The observation is at the firm and year level, with 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. 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 column (1)-(3), we include all firms, while in column (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. Robust standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 4: The Effects on Firm Size

	Revenue			Intermediate Input		
	(1)	(2)	(3)	(4)	(5)	(6)
Entrants	-1.033***	-0.929***	-0.934***	-1.006***	-0.906***	-0.912***
	(0.035)	(0.021)	(0.021)	(0.035)	(0.022)	(0.022)
Exiters	-0.910***	-0.681***	-0.678***	-0.948***	-0.712***	-0.706***
	(0.028)	(0.031)	(0.032)	(0.030)	(0.033)	(0.034)
Treatment × Entrants	-0.151***	-0.209***	-0.191***	-0.132***	-0.191***	-0.173***
	(0.033)	(0.024)	(0.023)	(0.033)	(0.024)	(0.024)
Treatment × Exiters	0.352***	0.187***	0.186***	0.362***	0.189***	0.184***
	(0.062)	(0.052)	(0.053)	(0.061)	(0.052)	(0.054)
Observations	1110702	1110667	1110662	1110702	1110667	1110662
Adjusted R^2	0.101	0.232	0.232	0.096	0.220	0.220
City-Industry FE	✓			✓		
City-Industry-Owner FE		✓	✓		✓	✓
City-Year FE	✓	✓	✓	✓	✓	✓
Industry-Year FE	✓	✓		✓	✓	
Industry-Owner-Year FE			✓			✓

Notes: See equation (3) for specifications. The observation is at the firm and year level, with sample period of 2009-2015. In column (1)-(3), the dependent variable is log revenue, while in column (4)-(6), the dependent variable is log intermediate input. 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 of 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. Robust standard errors are clustered at the city-industry level. $^{*}p<0.10$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 5: Productivity Estimation

Panel A: Parameter Estimates	
	Estimates
β_k	-0.199***
	(0.001)
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
ROA	0.084
Age	0.175

Notes: This table shows the productivity estimation results following Section 4.2. Panel A shows the parameter estimates, while Panel B shows the correlation of estimated productivity with some key variables. Revenue, Capital and Intermediate Goods are all log-valued. ROA is defined as the log of the revenue to capital ratio. $^{*}p<0.10$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 6: The Effects on Firm Productivity

	Productivity			Returns on Asset		
	(1)	(2)	(3)	(4)	(5)	(6)
Entrants	-0.055***	-0.051***	-0.052***	0.042***	0.021	0.018
	(0.002)	(0.002)	(0.002)	(0.016)	(0.013)	(0.013)
Exiters	-0.065***	-0.054***	-0.054***	-0.084***	-0.138***	-0.136***
	(0.003)	(0.004)	(0.004)	(0.028)	(0.026)	(0.026)
Treatment × Entrants	0.011***	0.009***	0.010***	0.212***	0.233***	0.241***
	(0.002)	(0.002)	(0.002)	(0.021)	(0.021)	(0.020)
Treatment × Exiters	0.032***	0.024***	0.024***	0.124***	0.165***	0.163***
	(0.004)	(0.004)	(0.004)	(0.034)	(0.030)	(0.030)
Observations	1110702	1110667	1110662	1110702	1110667	1110662
Adjusted R^2	0.062	0.104	0.105	0.068	0.087	0.087
City-Industry FE	✓			✓		
City-Industry-Owner FE		✓	✓		✓	✓
City-Year FE	✓	✓	✓	✓	✓	✓
Industry-Year FE	✓	✓		✓	✓	
Industry-Owner-Year FE			✓			✓

Notes: See equation (3) for specifications. The observation is at the firm and year level, with sample period of 2009-2015. In column (1)-(3), the dependent variable is the structurally estimated productivity, while in column (4)-(6), the dependent variable is returns on asset, measured as the log of the revenue to capital ratio. 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 of 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. Robust standard errors are clustered at the city-industry level. $^{*}p<0.10$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 7: 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	531322	531293	531289	412544	412489	412476
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: See equation (3) for specifications. The observation is at the firm and year level, with sample period of 2013-2015. In column (1)-(3), the dependent variable is productivity estimated following Levinsohn and Petrin (2003), while in column (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 of 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. Robust standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 8: The Effects on Firm Productivity: Mechanisms

	Productivity				
	RegCost		Owner Type		
	Yes	No	Non-Corporation	Corporation	SOE/FIE
Treatment × Entrants	0.013***	0.006**	0.026***	0.011***	0.005
	(0.004)	(0.003)	(0.003)	(0.002)	(0.010)
Observations	494548	616154	241360	775381	93948
Adjusted R^2	0.058	0.060	0.120	0.062	0.084
	Fixed Asset		Registered Capital		
	Large	Small	Large	Small	
Treatment × Entrants	0.002	0.011***	0.012***	0.002	
	(0.005)	(0.003)	(0.003)	(0.003)	
Observations	291129	729249	435349	532785	
Adjusted R^2	0.081	0.052	0.066	0.042	
	SOE Share		Entry Rate		
	Large	Small	Large	Small	
Treatment × Entrants	0.009***	0.009*	0.010***	0.005	
	(0.003)	(0.004)	(0.003)	(0.004)	
Observations	635592	386174	846053	264648	
Adjusted R^2	0.054	0.061	0.050	0.088	
City-Industry-Owner FE	✓	✓	✓	✓	
City-Year FE	✓	✓	✓	✓	
Industry-Owner-Year FE	✓	✓	✓	✓	

Notes: The observation is at the firm and year level, with 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 of 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 RegCost, Owner Type, Fixed Asset, Registered Capital, SOE share and Entry rate, each of which is a dummy variable. RegCost is defined in Table 2. Owner Type is defined as Non-corporation, corporation or SOE/FIE. Fixed Asset is average fixed asset per employee in each city-industry pair from 2010 National Tax Survey, while Registered Capital is the ratio of registered capital to capital stock for new entrants in each city-industry pair from 2008 Firm Annual Report. SOE share is measured as the revenue share of SOEs in each city-industry pair using 2008 Chinese Industrial Census, and Entry rate is the measured entry rate in 2008 using Firm Registration Database at the city and industry level. We divide the sample to two groups, "Large" and "Small", according to whether each indicator is above the city-industry median or not. We include city-industry-owner FE, city-year FE and industry-owner-year FE across different model specifications. Robust standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 9: The Effects on Entrepreneurial Characteristics of New Firms

	Years of Schooling	Secondary	Post-Secondary	Age	Age Below 35	Female
Treatment	-0.198**	-0.041***	-0.024	-0.234	0.028*	0.001
	(0.095)	(0.014)	(0.018)	(0.329)	(0.017)	(0.007)
Mean of Dep Var	10.557	0.646	0.118	40.066	0.310	0.161
Observations	96867	96867	96867	66289	66289	273839
Adjusted R^2	0.473	0.370	0.764	0.097	0.062	0.037
City-Industry, Time FE	✓	✓	✓	✓	✓	✓
City-Year, City-Month FE	✓	✓	✓	✓	✓	✓
Industry-Time FE	✓	✓	✓	✓	✓	✓

Notes: See Table 1 for model specification and variable definitions. We keep a sample of entrants in 2009-2015 with the major characteristics of their legal representatives constructed from Firm Personnel Database. The dependent variable is years of schooling, whether the education level is above the secondary level or the post-secondary level, the age, whether age is below 35 and whether the entrepreneur is a female. 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. Robust standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table 10: The Effects on Shareholder Structure of New Firms

	# of Shareholders	Ratio of Individuals	Share by Individuals	Sole Proprietorship
Treatment	0.012	0.019***	0.023***	0.020***
	(0.018)	(0.006)	(0.007)	(0.007)
Mean of Dep Var	1.723	0.948	0.904	0.939
Observations	366188	366486	366486	366486
Adjusted R^2	0.087	0.077	0.154	0.071
	One Individual	Two Individuals	Three Individuals	Four and above
Treatment	-0.015	0.025**	-0.006	-0.004
	(0.012)	(0.012)	(0.005)	(0.004)
Mean of Dep Var	0.428	0.467	0.072	0.033
Observations	344241	344241	344241	344241
Adjusted R^2	0.142	0.108	0.024	0.040
City-Industry FE	✓	✓	✓	✓
City-Year, City-Month FE	✓	✓	✓	✓
Industry-Time FE	✓	✓	✓	✓

Notes: See Table 1 for model specification and variable definitions. The observation is at the firm level, with sample period of 2009-2016. We construct the shareholding structure of new entrants from Firm Shareholder Database, and categorize shareholders into two types: individuals (natural persons) and firms. The dependent variables, on the upper panel, are the number of shareholders, the ratio of individual shareholders, the share owned by individual shareholders and whether the firm is the sole proprietorship of individual shareholders. The lower panel only includes samples of sole proprietorship, and the dependent variables are four dummy variables indicating whether there are one, two, three and four and above individual shareholders. We include city-industry FE, city-registry year FE, city- registry month FE as well as industry-registry time FE across different model specifications. Robust standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

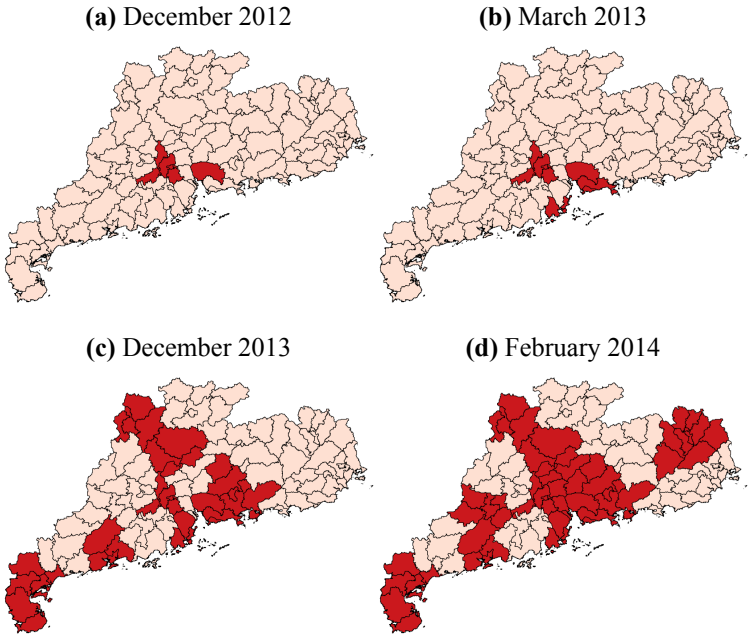
Table 11: Aggregate Productivity and Decomposition

	Incumbents	Entrants	Exiters	Total
Panel A: BHC				
2009	-1.168	-0.177	0.061	-1.284
2010	1.667	-0.054	0.053	1.666
2011	0.254	-0.231	0.036	0.060
2012	-0.417	-0.175	0.010	-0.582
2013	0.709	0.133	-0.001	0.842
2014	0.257	0.068	0.005	0.331
2015	-0.532	0.014	-0.036	-0.554
Panel B: GR				
2009	-1.202	-0.135	-0.045	-1.382
2010	1.798	-0.112	-0.069	1.617
2011	0.215	-0.215	-0.031	-0.031
2012	-0.425	-0.151	-0.001	-0.577
2013	0.499	0.101	-0.023	0.577
2014	0.258	0.068	-0.005	0.321
2015	-0.550	0.024	0.043	-0.483

Notes: This table shows the productivity decomposition results. The numbers are in the unit of percentage points. Panel A shows the results following Baily et al. (1992) according to equation (10), while Panel B shows the results following Griliches and Regev (1995) according to equation (11).

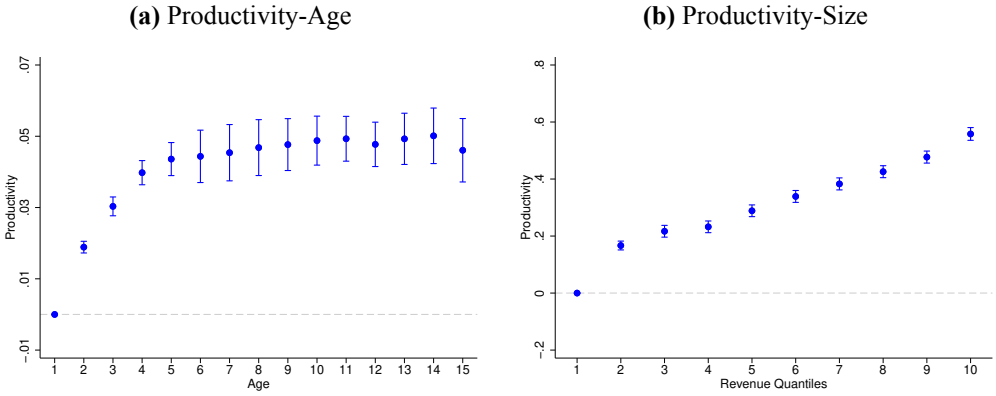
Appendix A Additional Figures/Tables

Figure A1: Four Snapshots of Program Rollout



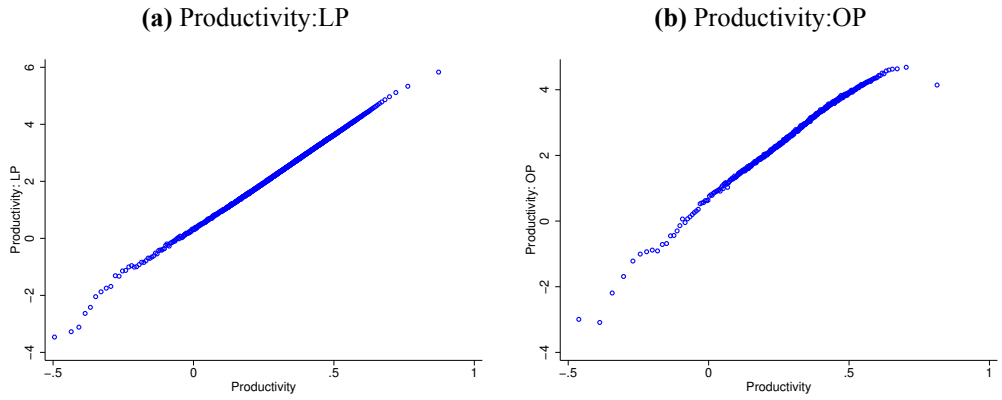
Notes: The figure shows four snapshots of the Guangdong map with the dark red regions being reformed cities.

Figure A2: Productivity, Age and Size



Notes: This figure shows the relationship of productivity and firm age/size. The left panel shows the result for firm age with age equals 1 as the base group, and the right panel shows the result for firm size. Firm size is categorized into 10 groups according to deciles, and the smallest group serve as the base group. Firm level data is used for this figure. 95% confidence intervals are constructed from robust standard errors clustered at the city-industry level.

Figure A3: Comparison of Different Productivity Measures



Notes: Productivity is estimated in Section 4.2, Productivity:LP is estimated following Levinsohn and Petrin (2003), while Productivity:OP is estimated following Olley and Pakes (1996).

Table A1: The Effects on Firm Entry: Robustness Check

	No HJZ		No DF		Before 2014M3	
	Entry	ER	Entry	ER	Entry	ER
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.274***	0.277***	0.369***	0.370***	0.299***	0.301***
	(0.040)	(0.040)	(0.041)	(0.041)	(0.043)	(0.044)
Observations	45936	45936	48525	48525	34619	34619
Adjusted R^2	0.953	0.752	0.951	0.681	0.947	0.752
City-Industry, Time FE	✓	✓	✓	✓	✓	✓
City-Year	✓	✓	✓	✓	✓	✓
City-Month FE	✓	✓	✓	✓	✓	✓
Industry-Time FE	✓	✓	✓	✓	✓	✓

Notes: See Table 1 for model specification and variable definitions. We calculate the number of entrants and incumbents using firm registration information. In column (1)-(2), we exclude Huizhou, Jieyang and Zhaoqing from our sample due to ambiguous starting month. Alternatively, we discard Foshan and Dongguan in column (3)-(4) as they start the pilot reform much earlier than the rest. Finally, in column (5)-(6), we replace the sample period (2009M1-2016M12) with the pilot sample period of 2009M1-2014M2. Robust standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table A2: The Effects on Firm Entry: Alternative Channels

Panel A: Reclassification			
	GTH Entry		
Treatment	0.166***	0.173***	0.159***
	(0.030)	(0.030)	(0.029)
Observations	238680	238680	238680
Adjusted R^2	0.854	0.855	0.942
City-Industry, Time FE	✓	✓	✓
City-Year FE	✓	✓	✓
City-Month FE		✓	✓
Industry-Time FE			✓
Panel B: 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	261097	261097	261097
Adjusted R^2	0.232	0.389	0.237
City-Industry, Time FE	✓	✓	✓
City-Year,City-Month FE	✓	✓	✓
Industry-Time FE	✓	✓	✓

Notes: See Table 1 for model specification and variable definitions. In Panel A, the observation is at city, industry and month level with the dependent variable being log of *Getihu* entry. In Panel B, the observation is at firm level with the dependent variable being whether entrepreneur is from a county, city or province different from the location of the firm, constructed from Firm Personnel Database. Robust standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table A3: 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	3384893	3288350	2728065	2643663
Adjusted R^2	0.012	0.019	0.012	0.021
City-Industry-Owner FE	✓	✓	✓	✓
Age FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Notes: See equation **??** for specifications. The observation is at the firm and year level, with sample period of 2009-2018. The dependent variable is a dummy variable of whether to exit or not, scaled by 100. Exit is defined as firm getting revoked or cancelling itself. 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. We include city-industry-owner FE, age FE and year FE across columns. Robust standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table A4: Estimation of the Elasticity of Substitution

	Profit		
	(1)	(2)	(3)
Revenue	0.130***	0.132***	0.132***
	(0.010)	(0.010)	(0.010)
Observations	149051	149045	149045
Adjusted R^2	0.088	0.126	0.120
City-Industry FE		✓	
Year FE		✓	
City-Industry-Year FE			✓

Notes: We use the sample from National Tax Survey 2010-2015 for 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. Robust standard errors are reported. *p<0.10; **p<0.05; ***p<0.01.

Table A5: 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: The observation is at the firm and year level, with 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 column (1)-(2), and use the entry deregulation 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 A6: The Effects on Firm Size and Productivity: Market Competition

	Revenue	Productivity
	(1)	(2)
Entrants	-0.985***	-0.052***
	(0.034)	(0.002)
Treatment × Entrants Who Entered Before the Reform	-0.184***	0.011***
	(0.034)	(0.003)
Treatment × Entrants Who Entered After the Reform	-0.075**	0.004
	(0.037)	(0.004)
Observations	1110702	1110702
Adjusted R^2	0.096	0.059
City-Industry FE	✓	✓
City-Year FE	✓	✓
Industry-Year FE	✓	✓

Notes: See Table A6 for model specification and variable definitions. 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 of 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 of 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. Robust standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Table A7: Productivity Decomposition

	Incumbents	Entrants	Exiters
Panel A: BHC			
Treatment	-0.039	0.109	-0.006
	(0.303)	(0.081)	(0.012)
Observations	3295	3359	3359
Adjusted R^2	0.035	0.010	0.016
Panel B: GR			
Treatment	-0.141	0.050	0.006
	(0.305)	(0.067)	(0.016)
Observations	3297	3297	3297
Adjusted R^2	-0.020	0.020	0.043
City-Industry FE	✓	✓	✓
Year FE	✓	✓	✓

Notes: This table shows the impacts of entry reform on productivity decomposition. The observation is at the city, industry and year level. The annual change of aggregate productivity for each city and industry pair is decomposed into three components: incumbents, entrants and exiters following Baily et al. (1992) according to equation (10) in Panel A, and following Griliches and Regev (1995) according to equation (11) in Panel B. The dependent variables are in the unit of percentage points. Treatment is defined as in Table 3. We control for city-industry FE as well as Year FE. Robust standard errors are clustered at the city-industry level. *p<0.10; **p<0.05; ***p<0.01.

Appendix B Additional Data

B.1 Further Data Issues

There are two additional data issues. First, as we also discussed extensively in Section 3.3, although Firm Registry Database provides accurate information about firm registration, there are some policy concerns that lowering entry barrier introduced many new "shell" companies which only exist on the paper but are never in operation. Therefore, the impacts of entry deregulation might be exacerbated because those "shell" companies inflate the number of real new entrants. Though identifying those "shell" companies from the data is beyond the scope of this paper, we provide robustness checks by constructing the firm entry measure from Firm Annual Report Database, arguing that "shell" companies are much less likely to report their annual information and thus be recorded in the Firm Annual Report Database and further report positive capital stock. The impacts on firm entry is robust if we examine the number of new reporting firms instead of new registered firms.

Secondly, the original data in Firm Annual Report Database suffers from misreporting issues of units from 10,000 Yuan to Yuan. We carefully correct the units of data by two approaches. One is to benchmark it to registered capital, which is arguably a crude measure for the firm size but free from reporting issue, while the other is to compare numbers reported by the same firm across different years and correct those with very unlikely drastic ups and downs.

B.2 Other Data Sets

We use several other auxiliary data sets in empirical analysis, including Firm Personnel Database, Firm Shareholder Database, *Getihu* Registration Database, National Tax Survey Database, 2008 Chinese Industrial Census, 2018 Enterprise Survey for Innovation and Entrepreneurship in China (ESIEC) Data.

Firm Personnel Database Firm Personnel Database is from SAIC. We use its Guangdong sample in 2009-2015 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 entrants. We categorize shareholders into two types: individuals (natural persons) and firms. 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.

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.

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, at each city and industry level in 2010-2015[33]; (2) estimate elasticity of substitution using the firm-level revenue and profit data; (3) construct a measure of *Fixed Asset* using the firm-level fixed asset and revenue data. More information about the National Tax Survey Database can be found in Liu and Mao (2019).

2008 Chinese Industrial Census 2008 Chinese Industrial Census is from China's National Bureau of Statistics, which covers all firms in the manufacturing sector. We use 2008 Chinese Industrial Census to measure state sector size as the revenue share of SOEs at the city and industry level.

2018 Enterprise Survey for Innovation and Entrepreneurship in China (ESIEC) Data Enterprise Survey for Innovation and Entrepreneurship in China (ESIEC) is conducted by the Center for Enterprise Research in Peking University led by one of the authors[34]. It's an annual survey about innovation and entrepreneurship of Chinese enterprises via random sampling at the province level. The survey asked entrepreneurs to recall the experience of registration, which provides valuable information on the directly reported "cost of entry" from entrepreneurs[35]. We first calculate the means of reported "cost of entry" for firms which entered before and after the reform respectively and across 2-digit industries. Next, we calculate the percentage decrease of the mean reported "cost of entry" for each industry. We construct a dummy variable *RegCost*, which take the value of 1 if the industry is above the median of decreases. To put it simply, *RegCost* equals 1 for the industries experiencing a larger decrease in the average reported registration cost after the reform.

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

[34] https://www.cer.pku.edu.cn/en/index.htm

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