Productivity Gains from Trade: Bunching Estimates from Trading Rights in China*

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Abstract

This paper identifies productivity gains from trade by studying the manipulation behavior of firms in response to regulatory policies on international trade in China. Bunching estimates show that participation in international trade increases firm productivity. The productivity gains increase over time, indicating dynamic learning from trading. Further exploration shows no effects on R&D investment, product rationalization and markup. Young firms and nonstate-owned firms (non-SOEs) gain more from participating in trade. Workers share productivity gains through increased wages but not from increased employment.

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1 Introduction

A large body of literature on empirical trade has been devoted to identifying productivity gains from trade using firm-level data. The results of this work have important implications for policy designs, especially in recent times with anti-globalization voices. However, academic studies provide mixed findings. Some studies show that the productivity premium enjoyed by exporters stems mainly from the selection of productive firms into foreign markets (e.g., Bernard et al., 1995; Bernard & Jensen, 1999; Pavcnik, 2002), which motivates the development of firm heterogeneity trade models (e.g., Eaton & Kortum, 2002; Meltiz, 2003). For learning by exporting, while some work documents evidence of significant learning effect (e.g., De Loecker, 2007; Harrison & Rodríguez-Clare, 2010; Garcia-Marin & Voigtländer, 2019), others (for recent reviews of the literature, see Syverson, 2011; Bernard et al., 2012) find no effect. A key empirical challenge in identifying the causal effect of exporting on firm productivity lies in finding credible estimation strategies, especially in the presence of potential selection into exporting. A recent study by Atkin, Khandelwal, & Osman (2017) provides a good analysis. The authors conduct an experiment in Egypt, involving randomly providing rug manufacturers access to the U.S. market to export handmade carpets. With effective controls for endogeneity issues, the authors find that treated firms show significant improvements in production efficiency, quality, and profits.

We revisit the evidence on productivity gains from trade by making three contributions to the literature. First, we use a new identification strategy (i.e., the bunching method) widely used in the public and labor economics literature. Specifically, when facing regulatory constraints on international trade, some firms may adjust their measures to be qualified for trade permission, which creates an excessive mass at the policy threshold. We exploit this adjustment behavior to construct a counterfactual distribution if adjustment were forbidden. The effect of trade participation on firm productivity can be identified by calculating the difference between the treated outcome and counterfactual outcome of the responsive firms. Second, the productivity gains increase over time, indicating that learning takes time. Further exploration of mechanisms indicates that there is no evidence that firms adjust their R&D investment, product bundles, or markups, suggesting that learning by trade is the main channel. Third, we study the distribution of productivity gains within and across firms. The former sheds light on the sharing between workers and firms, whereas the latter provides a picture of the evolution of industrial structures and the heterogeneous effects across firm age and ownership.

China provides a good research setting for studying international trade and firm productivity. In recent decades, China has experienced tremendous growth in international trade. When China first opened up to international trade in 1978, the country's total trade value was US$20.6 billion, representing less than 1 percent of global trade. In 2018, China had total trade value of US$4.63 trillion and was the country with the largest exports and the country with the second largest imports in the world. These dynamics provide an opportunity to study the role of international trade in shaping firm performance. Meanwhile, China has adopted active export-promoting strategies, as advised by international organizations such as the United Nations and World Bank. China has many things in common with other developing countries, including low worker productivity, abundant labor endowment, and low value-added in the global production chain. Finding evidence of productivity gains from trade in China can thus provide an example of the exporting-oriented development strategy (Munch & Schaur, 2018).

Our empirical analyses draw on a trade regulatory policy adopted in China in the early 2000s. Specifically, participation in international trade required trading rights during that period, which hinged on firms' minimum registered capital. In response, firms may adjust their registered capital to obtain the rights for international trade. Such adjustment creates bunching at the policy threshold. Using the bunching method developed in the public economics literature (e.g., Saez, 2010; Chetty et al., 2011; Kleven & Waseem, 2013; Diamon & Persson, 2017;), we can infer the counterfactual distribution of firms in the state in the absence of registered capital adjustment. This approach is based on the premise that the variable of interest and registered capital would be smooth if there were no adjustment. The difference between the counterfactual and the treated values for responsive firms illuminates the causal effects. We apply this methodology to manually combined firm-level data covering all manufacturing sectors with trade and production information.

We obtain four sets of results. First, the graphical evidence confirms that firms adjust their registered capital to meet the threshold to be qualified for trade permission. That is, there is exccessive mass of firms at the policy cutoff point. We find that this bunching at the cutoff cannot be explained by the cutoff being a reference point. Meanwhile, several placebo tests using samples without the policy in place show no bunching at the cutoff, further indicating that the bunching behavior is not a spurious correlation due to other unobserved factors.

Second, upon comparing responsive firms in the treated and counterfactual states, we find that qualifying for trading rights increases participation in international trade by 14.4 percent. Meanwhile, firm productivity increases by 9.9 percentage points, which implies that participation in international trade increases firm productivity (based on quantity production function estimation) as a logarithm by 0.688 on average contemporarily. These results are robust to a series of sensitivity checks, including the examination of the exclusion restriction condition, checks of excluded region and polynomial orders, the exclusion of processing traders, and alternative measures of trade participation. Our estimate is comparable to that in Atkin, Khandelwal, & Osman (2017), who find that the treatment of access to foreign markets leads to a contemporary TFP gain of 0.94 percentage points, but is higher than those obtained from matching difference-in-differences (DD) analyses (i.e., De Loecker, 2007; Garcia-Marin & Voigtländer, 2019).

Third, we conduct a series of analyses to explore the mechanism. Specifically, we find that productivity gains increase over time but stabilize after the removal of the policy. The fact that learning takes time is consistent with the learning by international trading hypothesis. In addition, the results show no responses by firms in terms of increasing R&D investment, adjusting product bundles, or changing markups.

Fourth, we find distributional effects within and across firms. Specifically, firms share portions of their gains from trade with workers by increasing the average wage rates but without increasing total employment. Meanwhile, young firms and nonstate-owned firms (non-SOEs) gain more from trade participation than old firms and state-owned firms (SOEs), consistent with the learning effect.

Our work is related to a substantial empirical literature that seeks to identify within-plant productivity gains from trade. That more productive firms are better able to export (Bernard et al., 1995; Bernard & Jensen, 1999; Pavcnik, 2002) poses an identification challenge. A critical part of the challenge lies in the estimation strategy. Earlier work, such as Bernard & Jensen (1999), Clerides et al.(1998), and Aw et al. (2000) document no empirical support for learning by exporting. Applying the matching difference-in-difference (DD) method, De Loecker (2007) and Garcia-Marin & Voigtlander (2019) document evidence of within-plant productivity gains. Moreover, De Loecker (2007) shows that productivity improvements are greater for manufacturing firms that start exporting to high income countries. From a randomize experiment (RCT) on rug producers in Egypt, Atkin, Khandelwal, & Osman (2017) finds that treated firms show significant improvements in production efficiency and quality. We contribute to the literature by applying a new identification strategy, the bunching method, based on the quasi-experimental variation in firms' access to international trade. The sample used, which is the universe of all manufacturing firms in a larger developing country that has experienced tremendous growth in trade and GDP, lends further credence to the results. We find that firms that begin to trade internationally become more productive. Moreover, the productivity gains increase over time, a pattern consistent with the "learning by trade" mechanism.

Further, the paper is related to a recent body of literature that uses bunching methods to estimate behavioral responses to the discontinuities in incentives created by kinked or notched policies (Kleven, 2016). Originating in the tax setting (Saez, 2010; Kleven & Waseem, 2013; Best at al., 2015; etc.), the bunching method has been deployed in many fields such as labor supply (Chetty et al., 2011), investment (Zwick & Mahon, 2017; Xu & Zwick, 2018; Chen et al., 2019;), housing and mortgage markets (Best & Kleven, 2018; Best et al., 2020; Cloyne et al., 2019; etc.), education (Diamon & Persson, 2017;), health (Gruber, Hoe, & Stoye, 2018), R&D (Chen et al., 2021), and behavioral economics (Allen et al., 2017; Rees-Jones, 2018; Zwick, 2018). We apply the bunching method in the international trade setting, focusing on the policy under which only firms with registered capital above the statutory cutoffs are allowed to trade. Following Diamond & Persson (2017), we adapt the method to infer the treated and counterfactual outcomes (without adjustment) of firms that adjust their registered capital upwards (i.e., compliers). The causal effect is identified from the difference between the treated outcome and the counterfactual outcome of compliers.

Our work is also related to the literature on trade barriers and misallocation. For example, Khandelwal, Schott, & Wei (2013) shows that the elimination of quota misallocation, instead of the removal of the quota itself, is the main reason for aggregate productivity gains. In our setting, trading rights policy that hinges on firms' registered capital acts like a trade barrier. This prevents firms with high productivity but low registered capital from entering the foreign market. However, instead of focusing on policy impact evaluation, we use the trading rights policy to construct a quasi-experimental design and study whether trade participation leads to productive gains.

The trading rights policy in China was studied by Bai, Krishna, & Ma (2017), who built a dynamic discrete choice model of export modes. The authors find that had China not liberalized its direct trading rights, its export participation would have been lower. The present work differs from this study by focusing on within-firm productivity gains from trade and using the bunching method based on the trading rights policy as a means of empirical identification.

The rest of this paper is organized as follows. Section 2 discusses the policy background and the data used. Section 3 presents the graphical evidence of policy compliance. Section 4 outlines our estimation strategy. Section 5 presents our empirical findings: first, we present the effects on trade and productivity; then, we discuss exclusion restriction and potential mechanisms; finally, we explore distributional effects within and across firms. Section 6 concludes.

2 Background, Data, and Summary Statistics

2.1 Restrictions on International Trade

China started its reform and opening up policy in 1978 to promote economic development. During the 1990s, the Chinese government emphasized two goals regarding international trade: (i) the healthy development of international trade (i.e., proper growth rates in export value) and (ii) the accumulation of foreign exchange reserves. To this end, the Chinese government maintained strict regulations on direct international trade for domestic-owned enterprises (DEs) until July 2004. Initially, trading rights for international trade were concentrated among a few foreign-trade corporations and their subsidiaries under the control of the central government. In 1992, trading rights were given to large or medium-sized manufacturing state-owned enterprises (SOEs) if their export value in two continuous years had passed a statutory threshold.

In the late 1990s, restrictions on obtaining trading rights were further reduced. Specifically, in 1999, large-scale industrial enterprises (certified by six different central government bodies), 120 large-sized pilot enterprise groups, and 1000 identified key enterprises could obtain trading rights upon registration. Meanwhile, private-invested enterprises (PIEs) were also given trading rights conditional on their registered capital, net assets, revenues and value of exports meeting the corresponding statutory thresholds for two continuous years. SOEs were subject to a lower standard, the only requirement being that their registered capital should be in excess of the statutory threshold since 2000.

As part of the accession agreement for joining the WTO, China gradually relaxed its restrictions on trading rights. Specifically, since January 2001, both SOEs and PIEs have been subject to the same requirements. Specifically, domestic-owned enterprises (DEs) with registered capital exceeding a specified threshold were entitled to trading rights for exporting and importing, whereas firms with registered capital below the threshold were prohibited from engaging in international trade. Between July 2001 and August 2003, the minimum requirements on registered capital for trading rights were reduced to 3 million RMB in eastern China, 2 million RMB in central and western China, and 1 million RMB for the mechanical & electronic sectors and high-technology firms. In September 2003, the requirements were further reduced to 0.5 million RMB in all sectors and regions. The restrictions on international trade were fully abolished after the foreign trade law was enacted in June 2004. By contrast, foreign-invested enterprises (FIEs) were given full rights to directly engage in international trade throughout the whole period.

Table 1 and Figure 1 report the historical changes of the threshold for trading rights. Figure 2 shows the regional distribution of the minimum requirements of registered capital for trading rights in 2002.

[Insert Table 1 here]

[Insert Figure 1 here]

[Insert Figure 2 here]

2.2 Background on Registered Capital

Registered capital (i.e., *Zhu Ce Zi Ben* in Chinese) must be equal to paid-in capital (i.e., *Shi Shou Zi Ben* in Chinese) by law. It is the full value of personal property that shareholders have given a firm in exchange for equity. This personal property could be in the form of cash, physical assets, and intangible assets (such as industrial property rights, patents, non-patented technology, and land use rights). Firm owners and shareholders can increase firms' registered capital by transferring the ownership of additional personal properties to firms. There is a restriction on the composition of registered capital; that is, the value of intangible assets should not account for more than 20% of the overall value of registered capital.

When changing their registered capital, firms must hire a certified public accountant (CPA) to verify the (updated) value of firms' registered capital. Firms inflating the value of registered capital and capital verification institutions issuing a false certificate are subject to penalties. After obtaining an affirmative certificate from the CPA firm, firms need to report their (updated) registered capital to the State Administration for Industry and Commerce of China. Meanwhile, additional requirements are imposed when firms decrease their registered capital. These include notifying firms' creditors (who have the right to ask firms to return the debt immediately or provide an additional guarantee), publishing an announcement in a newspaper regarding the decrease in firms' registered capital at least three times, compiling new balance sheets, and so on.

Note that registered capital differs from capital equipment. Specifically, the former is one component of equity, while the latter is a type of asset. With the adjustment of their registered capital, firms may not necessarily change capital equipment. For example, the lumpy nature of capital equipment investment implies that firms will only buy additional capital equipment when the adjustment in registered capital is large enough. Meanwhile, if the adjustment in registered capital is due to "cooking the books", there would be no actual firm responses such as capital equipment investment.[1]

2.3 Data and Summary Statistics

For our empirical analysis, we match three Chinese firm-level datasets. The first is Chinese customs transaction-level data for the 2000-2006 period. The dataset covers a universe of transactions made by Chinese firms that participated directly in international trade. The data include basic firm information, the type of trade (ordinary and processing trade), the trading partner, and the quantity and value of trade for each product. From the customs data, we define a firm's participation in international trade as 1 if it has any positive value of exports or imports and as 0 otherwise. Our baseline analysis includes firms that engage in ordinary trade or processing trade as they are both required to obtain trading rights (under the same registered capital requirement) for international trade.[2] To further alleviate the concern that firms engaging in processing trade (denoted as processing traders hereafter) may behave differently from firms that engage in ordinary trade (denoted as ordinary traders hereafter), we include a robustness check in the Appendix where we exclude

[1]We conduct a series of checks on the responses of firms' balance sheets to the increase in registered capital in section 5.3. The results indicate that firms did not buy additional capital equipment.

[2]Processing trading has played an important role in China's opening and development. Processing traders are allowed to import duty free all or part of their intermediate inputs but are required to export final goods (after local processing or assembly). This trade arrangement helps firms accumulate production experience and skills and also allows the government to increase foreign exchange reserves.

all processing traders from the analysis to examine whether our findings are driven by the special features of processing trade.

The second dataset is the Annual Surveys of Industrial Firms (ASIFs) from 1998 to 2007, conducted by the National Bureau of Statistics (NBS). The dataset covers all SOEs and non-SOEs with annual sales above 5 million RMB. The data contains information on firms' industries of production, locations, ownership, paid-in capital, employment, payroll, capital stock, output, value of exported goods, etc. In China, registered capital was required to be the same as paid-in capital by law before 2006. Hence, we use paid-in capital in the ASIF data to construct each firm's registered capital.[3]

The third dataset is product-level data from the National Bureau of Statistics of China for the years 2000 to 2006, including information on the price and physical quantity of each product (defined at the five-digit product level) produced by a firm. As the product-level data and ASIF data share the same firm identity, we can easily match the two datasets.

For the measure of firm productivity, we estimate quantity-based total factor productivity (TFPQ) for each 2-digit industry using the control function approach by Ackerberg, Caves, & Frazer (2015). To address a series of concerns regarding production function estimation, we follow the approach laid out by De Loecker et al. (2016) and applied to the Chinese setting by Lu & Yu (2015). Specifically, we first use the output in physical terms from the merged dataset to avoid omitted output price bias in production function estimation (see Klette & Griliches (1996), where the resulting revenue-based TFP may contain markup variations).[4] Second, we focus on a group

[3]Every year, accounting and audit firms check whether registered capital equals paid-in capital, and fines are imposed if this requirement is violated.

[4]Using the quantity data also helps avoid the imperfect inflation accounting issue of the inventories in the conventional revenue-based TFP estimation; that is, recovering the output value from

of single-product firms in the estimation of production function to avoid the potential bias caused by the multiproduct producer issue. With the estimated production function parameters and the assumption of multiproduct firms having the same production technology as single-product firms in the same industry, we obtain the TFPQ for all firms in the sample. Third, production function estimation requires inputs in physical quantity terms. While we have employment to capture labor in physical terms, our data only contain capital and materials in value terms. To back out the physical quantity of capital and materials, we deflate them with the 4-digit industry-level price indices constructed by Brandt et al. (Brandt, Van Biesebroeck, & Zhang, 2012; Brandt et al., 2017). To further alleviate the potential bias from the omitted firm-specific input prices, we use a control function approach developed by De Loecker et al. (2016); that is, we assume input prices are a function of output prices, market shares and trade participation. Fourth, to control for demand and supply shocks in the production function estimation (as shown by De Loecker (2011)), we include output prices, five-digit product dummies, city dummies, year dummies, product market shares, trade participation status. Also, as in De Loecker (2013), the Markov process for TFPQ includes a trade participation dummy and registered capital, where the latter is to directly contain the potential influence of registered capital on productivity evolution.

The estimated production functions for each 2-digit industry are shown in Table A1 in the Appendix. The output elasticities of inputs are similar to those in Lu & Yu (2015) (using the same data but slightly different controls for the production function estimation). The patterns found mimic the elasticities found by De Loecker et al. (2016) using Indian data; that is, the elasticities of materials are the highest, followed by those of labor and then those of capital, though the elasticities

total sales adjusted by the changes in inventories. We thank the referee for pointing out this issue. Meanwhile, the quantity data used for production function estimation corresponds to products manufactured in each period, which is equivalent to products sold plus the change in inventories.

of materials in China are higher than those in India. To alleviate concerns about influential outliers, we exclude the top and bottom 1% of estimated TFPQ in the full sample.

Panel A in Table 2 reports the descriptive statistics of manufacturing firms. Our data include approximately 2 million observations from 1998 to 2007, with approximately 200,000 firms for each year. Approximately 21% of firms engaged in international trade, of which 19% (15%) of firms exported (imported). The 25th, 50th, and 75th percentiles of firms' registered capital are 1 million, 3 million, and 9.3 million RMB, respectively.

[Insert Table 2 here]

3 Bunching Evidence

To identify the effect of international trade on firm productivity, we use China's regulations on international trade applied before 2004, as elaborated in the previous section. Specifically, in response to the regulation, firms may adjust their registered capital to obtain trading rights for international trade. Before we lay out the framework that uses this behavioral response for identification, we first check whether firms respond to the policy; that is, whether firms adjust their registered capital to meet the threshold to qualify for international trade permission.

Our empirical analysis focuses on the year 2002 for two reasons. First, this is the only year in our sample period not involving any policy changes to the trading rights threshold in the middle of the year. As our data are recorded annually, we can then identify the policy effect over an entire year. Second, the policy threshold was reduced to 0.5 million in August 2003, leaving us with insufficient neighborhood data for bunching estimation. Nonetheless, we also draw bunching evidence for the year 2001, when the threshold for trading rights was reduced from 5 million to 3 million in July 2001, leaving firms with half a year to respond to the policy and to show the performance effects.

Panel B in Table 2 reports summary statistics for nonmechanical and nonelectronic firms in eastern China (denoted as East China hereafter), nonmechanical and nonelectronic firms in central and western China (denoted as Central and West China hereafter), and mechanical and electronic firms (denoted as M&E hereafter),[5] respectively, in 2002. Approximately 24.2% of firms in East China engaged in international trade, whereas 7.3% of firms did so in Central and West China. Meanwhile, firms in East China exhibited, on average, higher productivity and registered capital than their counterparts in Central and West China.

In the year 2002, the minimum requirement on registered capital for trading rights was 300 (unit: 10, 000 hereafter) RMB in the East China, 200 in the Central and West China, and 100 for M&E firms, respectively. One complication for Central and West China is that the policy cutoff between January and June 2001 was 300. Given that there are some adjustment costs of changing the registered capital (e.g., auditing and notifying the public and creditors), firms may have continued to remain at 300 even if the policy cutoff were reduced to 200 in the year 2002. In other words, bunching at 300 may include effects from both the previous policy and the reference point (as 300 is a round number). This could bias our estimation of the counterfactual distribution and causal effect of international trade, because usually reference effect at 300 is adopted to control

[5]We classify M&E sectors according to the policy regulations for these sectors. The policy regulations include canceling import restrictions for the following M&E products
in 2002 http://www.customs.gov.cn/customs/302249/302266/302267/356476/index.html,
in 2003 http://www.customs.gov.cn/customs/302249/302266/302267/356681/index.html,
in 2004 http://www.customs.gov.cn/customs/302249/302266/302267/356680/index.html,
and regulations of imports for M&E in 2008
http://www.gov.cn/flfg/2008-05/04/content_960775.htm

for reference effect at the policy cutoff 200. Similarly, the previous policy (threshold of 200) prevents us from identifying the policy effect for M&E firms. Moreover, M&E firms were subject to import restrictions, which could further bias our identification of the policy effect of trading rights. To this end, we focus on the sample of firms in East China in the baseline analysis, and use the sample of firms in Central and West China and the sample of M&E firms as supplementary evidence in the Appendix.

We draw the density distribution of registered capital around the threshold 300 (unit of 10,000 RMB) from 80 to 470 (covering the 21th percentile to the 60th percentile of the distribution) with a bin size of 10 in Figure 3. We find excess bunching at the threshold 300, suggesting evidence of firms responding to the regulations on trading rights. However, we also find significant bunching of firms at 200 and 400 as well as at other multiplies of 50 in the distribution. Given that there is no policy at these registered capital levels, these results suggest that firms have a tendency to choose their registered capital at certain reference points.

[Insert Figure 3 here]

To corroborate this argument, we plot the density distribution of registered capital for firms in East China in 2000 in the same locality (i.e., 80-470) in Figure A1a in the Appendix. We continue to find significant bunching at 200, 300, 400, and other reference points. Note that the minimum requirements for trading rights in 2000 were 500 for SOEs and 850 for PIEs (with additional requirements for net assets, revenue, and exported goods), implying no firms eligible for trading rights in our window of registered capital. These results suggest the existence of reference points in the distribution of registered capital. Meanwhile, upon comparing Figure 3 to Figure A1a, we visually find that bunching at 300 is much higher in the former than in the latter, which suggests the response to the policy. In Figure A1b, we draw the distribution for firms in East China in 2001 in the same locality, when the policy threshold was reduced to 300 for both SOEs and PIEs in the middle of the year. Compared to Figure 3 and Figure A1a, similar patterns of bunching at reference points are found in Figure A1b. Meanwhile, bunching at the policy threshold 300 becomes larger in 2001 than that in 2000, but is lower than that in 2002. Given that firms had only half a year in 2001 to respond to the policy, these findings further suggest that firms indeed adjusted their registered capital to the threshold to be qualified for trade permission.

To formally control for rounding at the reference points and confirm that bunching at the policy threshold reflects firms' responses to the policy, we construct the counterfactual density distribution (i.e., the distribution without our focal policy) using two approaches. The first one adopts a nonparametric comparison in the spirit of difference-in-differences (DD) approach. Specifically, using 2000 as the pretreatment period, we conduct the following regression: $h_{jt} = \lambda_j + Post_t + \sum_{rc_j \geq rc_{lb}}^{rc_j \leq rc_{ub}} \beta_j * Post_t * Treat_j + \varepsilon_{jt}$, where $Post_t = 1$ if $t = 2002$, $Post_t = 0$ if $t = 2000$; $[rc_{lb}, rc_{ub}]$ denotes the excluded region (i.e., the adjustment region; the determination of their values are explained in section 4).[6] Counterfactual density distribution in 2002 is then calculated as $\hat{h}_{jt}^0 = \hat{\lambda}_j + \widehat{Post_t}$. Figure 4a shows the dashed and solid lines for the estimated counterfactual and observed distributions, respectively, whereas Figure 4b shows the difference between observed and counterfactual distributions (which eliminates the reference point effects). We find that the counterfactual distribution matches the observed distribution at the reference points (such as 100, 200, 400 and other multiples of 50). Meanwhile, after controlling for reference point effects, we continue to find a significant and sharp jump at the policy threshold of 300, suggesting responses

[6]The nonparametric approach essentially uses time variation in the budget set and cross-sectional variation from the policy threshold, alleviating the concern of only using the cross-sectional variation from the kink in identifying the extent of bunching (Blomquist et al., 2021). We thank the referee for pointing out this issue.

by firms to the policy.

[Insert Figures 4a-4b here]

In Figure A2 in the Appendix, the solid line shows clear bunching at the policy cutoff of 200 for Central and West China in 2002 as well as at other reference points such as 100 and 300. The dash line denoting the counterfactual distribution (constrcuted in spirit of differen-in-differences) well matches the observed distribution at reference points while leaving excess bunching at the policy threshold of 200. This confirms behavioral changes by firms in response to the regulations on trading rights. A similar pattern is found for M&E firms in Figure A3 in the Appendix, for which the policy threshold is 100.

One concern with the nonparametric comparison is that there were many entries between 2001 and 2002, which may compromise the comparability between the two distributions. To alleviate this concern, we draw the observed density distribution for our focal sample in the year 2002 while excluding new entries between 2001 and 2002. We then compare it to the estimated counterfactual distribution using the aforementioned nonparametric approach. The results are present in Figure A4 in the Appendix. We continue to find a similar pattern: good fit at the other distribution bins but a significant jump at the policy threshold. These results suggest that new entries may not introduce significant bias into our comparison.

Our second approach estimates the counterfactual distribution using the parametric method developed by Kleven & Waseem (2013). Rounding is approximated by a quadratic trend at multiplies of round numbers 50 and 100, i.e., $\sum_{r=50,100} I_{\{\frac{rc_j}{r}\in\mathbb{N}\}}\left(\rho_{0r}+\rho_{1r}rc_j+\rho_{2r}rc_j^2\right)$, where rc_j is the registered capital level in bin j from 80 to 470 and N is the set of natural numbers.[7] The dashed curve in Figure 4c shows the counterfactual distribution of registered capital, and for ease of com-

[7]Note that within our sample window, we have bins of 100, 200 and 400 to exactly identify the three parameters (ρ_{0r}, ρ_{1r}, ρ_{2r}) of the reference effects of 100 multiplies.

parison, the solid line represents the observed distribution of registered capital. Figure 4d shows the density distribution of registered capital net of reference point effects. The dashed vertical lines represent the excluded region. We continue to find a significant jump at the policy threshold of 300, further confirming the strong policy response by firms.

[Insert Figures 4c-4d here]

The crucial identifying assumption for the parametric method is that within the excluded region, our selected polynomial function can well approximate the counterfactual distribution. To verify our chosen excluded region and polynomial order, we impute the counterfactual distribution using the same function for unaffected samples, and compare it to the observed distribution. Specifically, as no policy was in effect in 2000 within the excluded region window, the density distribution of firms in East China in 2000 can be used to examine the fitness of our baseline specification. Meanwhile, given that FIEs were granted trading rights for international trade throughout the sample period and firms in Central and West China in 2002 were subject to the policy cutoff of 200, these samples of firms can also be used to study the accuracy of our interpolation in the exclusion range window. Estimation results are reported in Figure 5. For ease of comparison, we report the distribution for our focal sample (i.e., firms in East China in 2002) in panel a, and the distributions for the three unaffected samples are shown in panels b-d. We find the counterfactual distributions from our baseline specification match the actual distributions of the three unaffected samples.[8] These results lend further support to our specification choice in the parametric estima-

[8]Note that for foreign-invested firms, there is rounding at integers in RMB and USD. Hence, in addition to using the quadratic function for reference points in RMB used in the main analysis, we control for multiples of 5, 10, 20, 25, and 50 (units: 10, 000) USD for foreign-invested firms.

tion of the counterfactual distribution.

[Insert Figure 5 here]

Combined, our analyses show that in response to the policy, firms adjust their registered capital upwards and bunch at the threshold to obtain trading rights.

4 Empirical Strategy

Our identification of the effect of international trade on firm productivity hinges on the adjustment of registered capital by firms responding to the regulation policy, which increases international trade and consequently changes firm productivity. To apply this argument to the data, we follow the methodology proposed by Diamon & Persson (2017). It is based on the premise that the relationship between our variables of interest (such as firm productivity) and registered capital would be smooth if there were no adjustment of registered capital. Hence, we can use the sample of firms outside excluded region to estimate the relationship between the variables of interest and registered capital. Then, applying the same relationship to the excluded region, we can infer their counterfactual values of the variables of interest. The counterfactual and observed values within the excluded region (adjusted by their density) can be used to recover the treated outcome and the counterfactual outcome of the responsive firms. The causal effect is identified from the difference between the treated and the counterfactual outcome of the responsive firms.

To illustrate the estimation strategy, consider first the counterfactual state under which there were regulations on international trade (i.e., minimum requirement of registered capital for trading rights), but firms were not allowed to adjust their registered capital. Firms differ in their initial registered capital (according to various factors that are beyond the scope of this research). We henceforth denote the distribution of initial registered capital as the counterfactual density distribution $h^0(.)$ and the initial registered capital as rc^0.

Consider now that firms can adjust their initial registered capital. Denote the observed registered capital (the one with adjustment) in the data as rc and its density distribution as $h(.)$. In response to the policy, some firms with initial registered capital below the policy threshold (denoted as $\overline{rc}$) may increase their registered capital to go beyond $\overline{rc}$ to obtain trading rights for international trade. Specifically, denote the adjustment region of registered capital as $[rc_{lb}, rc_{ub}]$, where rc_{lb} and rc_{ub} are, respectively, the lower and upper bounds (the values are determined later), and $rc_{lb} < \overline{rc} < rc_{ub}$. Firms with initial registered capital $rc^0 \in [rc_{lb}, \overline{rc})$ may increase their registered capital and move to $rc \in [\overline{rc}, rc_{ub}]$. Figure 6a illustrate the change in the density distribution. Specifically, with the freedom of registered capital adjustment, firms to the left of the cutoff (in the shaded area marked by vertical dashed lines) would adjust their register capital upward (to the shaded area marked by horizontal dashed lines) within the adjustment region (i.e., the region between the two dashed vertical lines). Meanwhile, firms outside the adjustment region do not respond.

[Insert Figures 6a-6b here]

Hence, under the observed state where adjustment is allowed, region $[\overline{rc}, rc_{ub}]$ contains two groups of firms: (1) firms whose initial registered capital is $rc^0 \in [rc_{lb}, \overline{rc})$ and moves to this region as a response to the policy (i.e., $rc \in [\overline{rc}, rc_{ub}]$), which are then referred to as *compliers*; and (2) firms whose initial registered capital is in this region and remain in this region (i.e., $rc = rc^0 \in [\overline{rc}, rc_{ub}]$), which are then referred to as *always takers*. Meanwhile, in the observed state, region $[rc_{lb}, \overline{rc})$ contains the groups of firms whose initial registered capital is in this region and remain in this region (i.e., $rc = rc^0 \in [rc_{lb}, \overline{rc})$), which are then referred to as *never takers*.[9]

[9]Here, we assume away the possibility that firms, in response to the policy, would reduce their registered capital from above to below the cutoff $\overline{rc}$.

Figure 6b illustrates the identification strategy. The dashed and solid curves represent the counterfactual and observed relations between outcome variable and registered capital, respectively. Out of the adjustment region (i.e., the region between the two dashed vertical lines; $rc_j < rc_{lb}$ or $rc_j > rc_{ub}$), these two curves are the same, as firms do not change their registered capital. From the relation between variables of outcomes and registered capital out of the adjustment region, we then impute the counterfactual values of outcome variables in the adjustment region (i.e., the values without adjustment of registered capital). Specifically, in the counterfactual state, region $[\overline{rc}, rc_{ub}]$ only contains always takers, and region $[rc_{lb}, \overline{rc})$ includes both compliers and never takers, which are presented by the extended dashed curve. The difference between the observed (the hypothetical solid curve) and counterfactual values in region $[\overline{rc}, rc_{ub}]$ allows us to infer the average value for compliers in the treatment state (i.e., with the adjustment of initial registered capital to obtain international trade rights). The difference between the counterfactual and observed values in region $[rc_{lb}, \overline{rc})$ allows us to infer the average value for compliers in the control state (i.e., without adjustment of initial registered capital). The difference between average treated outcome and average counterfactual outcome of the compliers identifies the causal effect.

We implement the above strategy in three steps. First, we follow the empirical framework proposed by Chetty et al. (2011) to estimate the counterfactual density distribution of registered capital $h^0(.)$ from the observed one. The rationale is similar: the density distribution of registered capital would be smooth in the counterfactual state, and hence, the observed density distribution out the excluded region can be used to approximate the counterfactual distribution in the exclusion region. Specifically, we estimate the following density equation

$$h_j = \sum_{i=0}^{p} \beta_i (rc_j)^i + \sum_{i=rc_{lb}}^{rc_{ub}} \gamma_i I_{\{rc_j = i\}} + \sum_{r=50,100} I_{\{\frac{rc_j}{r} \in \mathbb{N}\}} \left(\rho_{0r} + \rho_{1r} rc_j + \rho_{2r} rc_j^2\right) + \varepsilon_j, \tag{1}$$

where h_j is the number of firms in bin j; $I_{\{rc_j = i\}}$ is an indicator function for the excluded region (i.e., $rc_{lb} < rc_j < rc_{ub}$); and ε_j is the error term. We choose optimal polynomial order p and excluded region $[rc_{lb}, rc_{ub}]$ via 5-fold cross validation. Specifically, this approach is implemented by splitting the sample into 5 subsamples. We minimize the mean-squared error over 4 of the subsamples and predict out of the sample using the estimated parameters on the 5th "hold out" sample and calculate the out-of-sample mean squared error. We perform this procedure for each of the 5 subsamples and sum up each of the 5 out-of-sample mean squared errors. We select the order of the polynomial and excluded region that minimizes this out-of-sample mean-squared error and satisfies the condition that the excess bunching equals missing mass ($B = M$). We also add a penalty for a higher order polynomial. Specifically, of the combination of the polynomial order and excluded region that satisfies $B = M$ and has a similar mean-squared error, the one of lower polynomial order is chosen.[10]

After obtaining the estimates from equation (1), we calculate the counterfactual density distribution as $h_j^0 = \sum_{i=0}^{p} \hat{\beta}_i (rc_j)^i + \sum_{r=50,100} I_{\{\frac{rc_j}{r} \in \mathbb{N}\}} \left(\hat{\rho}_0 + \hat{\rho}_1 rc_j + \hat{\rho}_2 rc_j^2\right)$. Excess bunching is defined as $\widehat{B} = \sum_{i=\overline{rc}}^{rc_{ub}} (h_j - h_j^0)$, and the missing mass is defined as $\widehat{M} = \sum_{i=rc_{lb}}^{\overline{rc}-1} (h_j^0 - h_j)$,where $\overline{rc}$ is the policy cutoff.

Second, we estimate the relation between outcome variables and registered capital using the sample out of the excluded regions. Specifically, we group firms based on their registered capital into bins of 10 units (one unit is 10,000 RMB). We then use a second-order polynomial function as our baseline estimation equation, i.e.,

$$\begin{aligned} y_{fj} &= \sum_{i=0}^{2} \theta_i (rc_j)^i + \alpha I_{\{rc_j \geq \overline{rc}\}} + \sum_{r=50,100} I_{\{\frac{rc_j}{r} \in \mathbb{N}\}} \left(\rho_{0r} + \rho_{1r} rc_j + \rho_{2r} rc_j^2\right) + \gamma_s + \varepsilon_{fj}, \\ \text{where } rc_j &< rc_{lb} \cup rc_j > rc_{ub} \end{aligned} \tag{2}$$

[10]In Appendix B, as a robustness check, we estimate causal effects based on alternative excluded region and polynomial order selections with the second or third lowest mean-squared error and also satisfying $B = M$.

where y_{fj} is the outcome of firm f in bin j; rc_j is the value of registered capital of bin j; and ε_j is the error term. We allow for a discrete jump in the outcome at the policy cutoff (i.e., $I_{\{rc_j \geq \overline{rc}\}}$ is the indicator function for bins above the policy cutoff $\overline{rc}$), which captures the effect from obtaining trading rights when there is no adjustment of registered capital. We also control for reference point effects $\sum_{r=50,100} I_{\{\frac{rc_j}{r} \in \mathbb{N}\}} \left(\rho_{0r} + \rho_{1r} rc_j + \rho_{2r} rc_j^2\right)$, and sector fixed effects γ_s.

With the estimated coefficients from equation (2), we calculate the counterfactual values of firm-level outcomes $y_{fj}^0 = \sum_{i=0}^{2} \hat{\theta}_i (rc_j)^i + \hat{\alpha} I_{\{rc_j \geq \overline{rc}\}} + \sum_{r=50,100} I_{\{\frac{rc_j}{r} \in \mathbb{N}\}} \left(\hat{\rho}_0 + \hat{\rho}_1 rc_j + \hat{\rho}_2 rc_j^2\right) + \hat{\gamma}_s$ and then take a simple average to obtain the average counterfactual values of outcome variables y_j^0 for bin j in the adjustment region ($rc_j \in [rc_{lb}, rc_{ub}]$).

Third, we calculate the average values of outcomes for the compliers in the treatment and control states separately, and their differences constitute the average treatment effects on the compliers. Specifically, for region $[rc_{lb}, \bar{rc})$, the observed average outcome for bin j corresponds to the average for the group of never takers, i.e., $y_j = y_j^{NT}$; whereas the counterfactual average outcome for bin j corresponds to the weighted average for the groups of never takers and compliers, i.e., $y_j^0 = \left(\frac{h_j}{h_j^0}\right) y_j^{NT} + \left(1 - \frac{h_j}{h_j^0}\right) y_j^{CP,0}$, where $y_j^{CP,0}$ denotes the average outcome for compliers in the control state. Hence, we can infer $y_j^{CP,0} = \frac{y_j^0 \times h_j^0 - y_j^{NT} \times h_j}{h_j^0 - h_j}$.

For region $[\overline{rc}, rc_{ub}]$, the counterfactual data correspond to the average of always takers, i.e., $y_j^0 = y_j^{AT}$, whereas the observed data measure the weighted average of compliers and always takers, i.e., $y_j = \left(\frac{h_j^0}{h_j}\right) y_j^{AT} + \left(1 - \frac{h_j^0}{h_j}\right) y_j^{CP}$, where y_j^{CP} denotes the average outcome for compliers in the treatment state. Hence, we can compute $y_j^{CP} = \frac{y_j \times h_j - y_j^0 \times h_j^0}{h_j - h_j^0}$.

Then, the average treatment effect on the compliers can be calculated as

$$\begin{aligned}\tau &= \frac{\sum_{rc_j \in [\overline{rc}, rc_{ub}]} y_j^{CP}\left(h_j - h_j^0\right)}{\sum_{rc_j \in [\overline{rc}, rc_{ub}]}\left(h_j - h_j^0\right)} - \frac{\sum_{rc_j \in [rc_{lb}, \overline{rc})} y_j^{CP,0}\left(h_j^0 - h_j\right)}{\sum_{rc_j \in [rc_{lb}, \overline{rc})}\left(h_j^0 - h_j\right)} \\ &= \frac{1}{N^{CP}}\left[\sum_{rc_j \in [\overline{rc}, rc_{ub}]} y_j^{CP}\left(h_j - h_j^0\right) - \sum_{rc_j \in [rc_{lb}, \overline{rc})} y_j^{CP,0}\left(h_j^0 - h_j\right)\right],\end{aligned} \tag{3}$$

where $N^{CP} \equiv \sum_{rc_j \in [\overline{rc}, rc_{ub}]}\left(h_j - h_j^0\right) = \sum_{rc_j \in [rc_{lb}, \overline{rc})}\left(h_j^0 - h_j\right)$ is the number of compliers. We estimate the standard error using a parametric bootstrap procedure (for the same practice, see Chetty et al., 2011). Specifically, we redraw the estimated vector of errors ε_{fj} in equation (2) with replacement to generate a new sample and calculate a new estimate (3). We repeat this procedure 500 times and obtain the standard error of the estimate as the standard deviation of the 500 new estimates.

5 Empirical Findings

5.1 Trade Effect

We start with the analysis of whether compliers increase their international trade when adjusting their registered capital beyond the minimum requirements of trade rights. Using the estimation framework, we can infer the average status of international trade for compliers in both the treatment and control states. Specifically, Figure 7a shows the observed distribution of trade participation against registered capital for 2000 and 2002. The vertical line denotes the policy cutoff. We find a significant jump in trade participation at the policy threshold. In Figure 7b, we compare the observed (denoted by dots) and estimated counterfactual function between registered capital and trade participation (i.e., the relationship when the adjustment of registered capital was not allowed denoted by the dashed line) net of all the controls (including fixed effects and rounding effects) in 2002. In Figure 7c, we focus on the comparison of compliers between the counterfactual and treatment states. Specifically, the dots denote the percentage of compliers engaging in international trade for each bin in 2002 with a larger size indicating a larger number of compliers; the vertical line denotes the policy cutoff; the dashed line to the left of the cutoff presents the average value of compliers in the control state;[11] and the solid line to the right of the cutoff reports the average value of compliers in the treatment state. The participation of compliers in international trade clearly increased from the control to the treatment state, indicating that obtaining trading rights with the minimum required registered capital indeed increases firms' participation in international trade.

[Insert Figures 7a-7c here]

To obtain the average treatment effect on compliers, we report the estimation results via the framework elaborated in the previous section in column 1 of Table 3. The estimate is positive

[11]Note that there are firms participating in international trade in the control state for several reasons. First, high-tech firms and research institutes are subject to a lower cutoff, i.e., 1 million RMB in 2002. Second, the government issued trading rights without requirements to certain specified large industrial enterprises in 1997 and to 120 enterprise groups and 1000 key enterprises in 1998. Third, some firms that held trading rights before 1998 went through reconstruction and became new firms. In 2000, the government issued a policy stipulating that these new firms could inherit trading rights without requiring minimum registered capital. As shown in Table A2 in the Appendix, firms with registered capital below the policy threshold of 300 that participated in international trade were more productive than firms that did not trade regardless of whether their registered capital was below or above the threshold. However, this does not affect the validity of our method, because these firms would not respond to the trading rights policy.

and statistically significant, confirming the pattern shown in Figure 7c.[12] Regarding the economic magnitude, obtaining trading rights increases participation in international trade by 14.4 percentage points.[13]

[Insert Table 3 here]

To alleviate the concern that the jump at the cutoff may be due to other factors (on top of the reference point), we conduct two placebo tests using samples without that policy cutoff. First, we use the data on DEs in East China in 2000, for which the minimum requirement of registered capital for trading rights is 500 (unit: 10,000 RMB). Hence, we should not find a significant effect at the cutoff of 300 (unit: 10,000 RMB). Estimation results are reported in column 2 of Table 3. Indeed, we find a statistically and economically insignificant coefficient. Second, we use the sample of FIEs in 2002. As FIEs had full access to international trade regardless of their registered capital, we do not expect to find any significant change at the cutoff of 300. The estimation results are

[12]In Appendix B, as a robustness check, we define trade participation based on whether firms have positive export value in the ASIF data. The compromise of this definition is that it may include both direct and indirect exporters (i.e., firms exporting through trade intermediaries). To further address the concern that indirect exporters may behave differently from ordinary direct exporters, we conduct a robustness check by excluding indirect exporters.

[13]Note that not all compliers participate in international trade once they obtain trading rights. A primary reason for this is that for certain products, in addition to trading rights, international trade was subject to additional requirements on export license and export quotas, as well as import restrictions. In addition, it takes time for firms to find international trade partners and establish networks. Hence, the estimate shown in column 1 largely captures the immediate effect on trade participation. Over time, the trade effect increases, with approximately 40% of firms participating in international trade as shown in Table A3 in the Appendix.

reported in column 3 of Table 3. Consistently, we find a statistically and economically insignificant coefficient. Combined, these results indicate that the evidence given in Figure 6c and in column 1 of Table 3 is not spurious.

5.2 Productivity Effect

Figure 8a shows the distribution of productivity against registered capital for 2000, 2002 and 2004. There is no clear increase of firm productivity at the policy threshold in 2000 (when the policy was not in place), but the trend becomes visually significant in 2002 when the policy was implemented, and productivity improvements increase further in 2004.

[Insert Figures 8a-8c here]

Figure 8b shows the observed (denoted by dots) and estimated counterfactual function between productivity and registered capital (denoted by the dashed line) net of all the controls (including fixed effects and rounding effects) in 2002. The two dashed vertical lines represent the excluded region (i.e., the subsample of firms that respond to the focal policy) and the solid vertical line is the policy threshold. Out of the excluded region, firms at their optimal choices would not respond to the policy, which means that the observed distribution is the counterfactual one. Indeed, we find that observed and counterfactual distributions out of the excluded region are closely matched, lending support to our estimation framework.[14]

[14] Upon comparing the observed and counterfactual distributions in figure 7b, we could also analyze whether compliers have a different ex ante productivity compared to never-takers. Specifically, within the exclusion range, to the left of the policy threshold, firms in the counterfactual state include both compliers and never takers as elaborated in the previous section, whereas firms in the observed distribution only include never takers. As shown in the figure, the counterfactual

More importantly, at the policy threshold, there is a visible jump of the counterfactual productivity level as shown in Figure 8b, indicating that passing the threshold for trading rights increases firms' productivity level. To further illustrate the policy effect on compliers, we compute the counterfactual and treatment values for compliers and report them in Figure 8c. Recall the method shown in the previous section. Specifically, comparing the values of observed distribution with the counterfactual distribution in the excluded region to the left of the policy cutoff, we recover the productivity levels of compliers in the counterfactual state (i.e., without registered capital adjustment and obtaining trading rights). Meanwhile, comparing the values of observed distribution with the counterfactual distribution in the excluded region to the right of the policy cutoff, we recover the productivity levels of compliers in the treated state (i.e., after registered capital adjustment and obtaining trading rights). There is a clear increase from the dashed line to the solid line in figure 8c, indicating that obtaining trading rights leads to higher productivity for compliers.

Estimation results of the reduced form of the effects of trading rights on firm productivity are presented in column 1 of Table 4. We find a positive and statistically significant coefficient, corroborating the pattern shown in Figure 8c. Turning to economic magnitude, obtaining trade rights increases firm productivity by 9.9%.

[Insert Table 4 here]

To verify that our estimates of the productivity effect are not biased due to confounders at the cutoff, we also use two samples without the policy effect at the cutoff to conduct two placebo

distribution lies below the observed one, indicating that never takers has higher productivity levels than compliers in the counterfactual state. Similarly, to the right of the policy threshold, the counterfactual distribution only has always takers, whereas the observed distribution contains both compliers and always takers. These two distributions are similar, indicating that compliers after affected by the policy have similar productivity levels as always takers.

exercises. First, we use the sample of DEs in East China in 2000, for which the threshold for trading rights is far from the focal region of registered capital. Estimation results are reported in column 2 of Table 4. As expected, we do not find a statistically significant and economically meaningful coefficient. Second, we examine the data for FIEs, which had full access to foreign trade regardless of their registered capital, for 2000-2002. Hence, we expect to find smoothness in the TFP at the cutoff for FIEs. The estimated results are reported in column 3 of Table 4. Consistent with our expectations, we find small and statistically insignificant estimated coefficients.

Note that the analyses so far investigate the relation between registered capital and firm productivity, essentially reflecting an intent-to-treat (ITT) effect. To obtain the average treatment effect of international trade on firm productivity, we divide the estimated coefficient in column 1 of Table 4 by that in column 1 of Table 3 and estimate the standard error using the bootstrap method. The estimated coefficient is reported in column 4 of Table 4. It is positive and statistically significant, confirming that international trade increases firm productivity.

In terms of economic magnitude, participation in international trade increases firm TFP (as a logarithm) by 0.688 contemporaneously on average. Our estimate is comparable to that given by Atkin, Khandelwal, & Osman, (2017), which is based on randomized-control experiments for rug producers in Egypt. In their regression with the specification fixed effect, the authors find that the treatment of the access to foreign markets leads to a contemporary TFP gain of 0.94 percentage points. While their results are based on one industry, we find a similar average effect from a universe of manufacturing industries. Our estimate is larger than those obtained from their matching based difference-in-differences (DD) analyses. Specifically, De Loecker (2007), from a DD analysis of matched data, shows that the productivity gains are 0.283 after two years of exporting and 0.434 after three years. By applying an event study and DD to a matched sample, Garcia-Marin & Voigtländer (2019) find that the efficiency gains are 0.151 after two years of exporting and 0.339 after three years of exporting.

5.3 Discussion of Exclusion Restriction

We have shown that after increasing registered capital to obtain trading rights, firms experience higher participation in international trade and thus higher productivity, indicating that access to trade leads to within-firm productivity gains. However, a potential concern is that the adjustment of registered capital may directly affect firm productivity; for example, the injected new money can be invested in physical and human capital to boost productivity. This would violate the exclusion restriction, creating bias in the estimation of the trade effect on firm productivity.

Note that the increase in registered capital could reflect a real response (i.e., the injection of properties into firms) or the manipulation of the registered capital verification. If the latter is the main reason that firms bunch at the threshold, the temporary behavior should not directly affect firm performance. However, the increase of registered capital across the threshold for trading rights can lead firms to engage in international trade, which leads to gains in firm productivity. Hence, the exclusion restriction is satisfied in this scenario and the estimation of trade effect on firm productivity is unbiased.

To examine whether our research setting satisfies the exclusion restriction condition, we conduct two sets of exercises. First, we directly check whether the firm's balance sheet (i.e., assets and liabilities) changes in response to the increased registered capital to shed light on the nature of the increase in registered capital. Second, we conduct a robustness check by considering a scenario where registered capital remains unchanged but trading rights are obtained. This can allows us to directly examine the effect of the participation in international trade (through being granted trading rights) on firm productivity without the potential compounding concerns from registered capital.

5.3.1 Balance Sheet Responses

In the case of a real response, the increased registered capital should affect the firm's balance sheet; that is, assets and liabilities (including debts and equities) should change. To detect the balance sheet responses, we follow the strategy by Chen et al. (2021). As shown in Figure A5a-A5c in the Appendix, there is no visually evident increase in total assets, total debts and equities around the threshold of registered capital. To provide rigorous estimates, we use the same methodology discussed in section 4, and separately examine assets and liabilities responses. Regression estimates are reported in Table A4 in the Appendix. The estimate for total assets is negative but statistically insignificant with a magnitude of 8% relative to the mean. The reduction in assets is due to the fall in total debts whereas equities marginally increase. However, both the estimated responses by debts and equities are statistically insignificant. Combined, these results indicate limited responses by the balance sheet to the increase in registered capital.

To further understand the balance sheet responses, we examine the detailed categories of assets and debts. Specifically, in panel A of Table A5 in the Appendix, we look at the short term and long term debts. We find a statistically and economically insignificant change of the short term debts, but a significant reduction in the long term debts. These results indicate that firms use increased registered capital to pay off long term debts. However, given that the firm's debt structure is overwhelmingly dominated by the short term debts, the overall amount of debts experience limited changes.

Panel B of Table A5 reports the results for current assets, fixed assets, long-run investment, and intangible assets. We find limited changes of current assets, fixed assets and long-run investment, but significant reduction in the intangible assets. But given that intangible assets account for a small proportion of the total assets, the overall response of the assets is limited. In panel C of Table A5, we further decompose current assets into inventory, receivables and other current assets. All the estimates are statistically insignificant and small in the magnitude, indicating that the increase in registered capital does not lead firms to change the total amount and composition of the current assets.

In summary, our aforementioned analyses show that there are limited responses of the balance sheet; that is, there are marginal reductions in the total assets and total debts and marginal increase in total equities in response to the increase in registered capital. There are some compositional changes; that is, firms use increased registered capital to pay off long term debts and reduce the intangible assets. The limited responses of the balance sheet imply that the change in registered capital may not cast significant direct effects on firm operation[15], but it could affect firm productivity by granting firms the trading rights.

To gain knowledge regarding why the increase in registered capital has limited impact on the balance sheet, we consulted several chief financial officers from Chinese manufacturers and certified public accountants from specialized accounting and auditing firms. There are three possible ways that firms can increase their registered capital without significantly changing the balance sheet. First, instead of injecting additional properties into firms to increase the registered capital, firms can cook the books by manipulating the registered capital verification. For example, a firm owner may borrow money from friends to boost the firm's registered capital. After the auditing process, the owner withdraws the money from the firm and returns the money to his friend. However, this leads to an unbalanced account due to the outflow of cash without justification. To balance the account, the firm needs to record the cash outflow as other receivables or advance payments first,

[15]It is possible that adjustment in registered capital might affect firm productivity through channels that are not captured by the balance sheet, such as the number of suppliers and customers. Although we don't have detailed data on these measures, given the limited responses of the balance sheet, we believe the potential impact from such channels is modest.

and then make fake transactions to write off the cash outflow gradually.

Second, firm owners can increase the registered capital by transferring the ownership of personal intangible asset to the firm. Different from fixed assets, the value of intangible asset is relatively subjective, and even specialized auditing companies can not precisely estimate the value. Therefore, firm owners could inflate the value of intangible asset to boost the registered capital. After the verification of registered capital, firms can write off the inflated value later by recording it as asset impairment loss, resulting in both decreases in assets and equities.

However, these two types of manipulation could be identified by the tax bureau and company registration authority as fraudulent investment and illegally withdrawal registered capital after verification, with the fine being approximately 5-15% of the fraudulent investment. A third alternative approach is more or less legitimate. That is, firms can convert the capital reserve, surplus reserve or unallocated profits into registered capital. Such practice leaves the overall value of equity unchanged, and also limited changes of total assets and debts.

5.3.2 Scenario without Registered Capital

To further examine the exclusion restriction condition, we consider a scenario in which registered capital is not adjusted but trading rights are granted. Hence, the change in firm productivity can then be inferred from the participation in international trade, instead of the effect from the adjustment of registered capital.[16] Specifically, in our research setting, *always takers* (i.e., firms with registered capital above 3 million in 2000) did not have trading rights as the threshold was 5 million in 2000, but they were automatically eligible for trading rights in 2002 as the threshold was lowered to 3 million. In other words, these firms were granted trading rights without any adjustment of registered capital. Hence, the comparison of firm productivity for *always takers* between

[16]We thank the referee for suggesting this robustness check.

2000 and 2002 implies a trade effect on productivity.

Estimation results are presented in Table A6 in the Appendix. As shown in column 1, we find a statistically significant and positive coefficient, which is consistent with the main results given in Table 4. This result further confirms that participation in international trade improves firm productivity. However, the magnitude listed in column 1 of Table A6 is smaller than that given in column 1 of Table 4. Note that these estimates represent the ITT effects. To obtain the average treatment effect, we first estimate the trade effect of obtaining trading rights in column 2 of Table A6, and the ratio of the productivity effect (in column 1) and trade effect (in column 2) of trading rights generates the average treatment effect of international trade on productivity for *always takers* in column 3. We find that participation in international trade increases firm TFP (as a logarithm) by 1.104 contemporaneously on average for the group of *always takers*, which is larger than that for our focal group of *compliers* in Table 4. As *always takers* have more registered capital than *compliers*, the heterogeneity in the treatment effect suggests that the effect of trade on firm productivity may increase with firm size.

5.4 Mechanism

Learning by international trading. Our matched firm data span the years 2000 to 2006, which allows us to trace the effects up to 4 years after trade participation in 2002. However, a concern with the dynamic analysis is that the minimum registered capital requirement for trading rights decreased from 3 million RMB to 0.5 million RMB in September 2003. Hence, control firms in the regions of our focal analysis became eligible for trading rights and participation in international trade. Meanwhile, the regulations on trading rights were fully abolished in July 2004. Hence, the analysis using the information from 2002 to 2006 cautions us that the dynamic effects may be limited to a few years of international trade. The estimation results are reported in Table 5. We find that the effects on TFP increased from 2002 to 2004 and then became established in 2005 and 2006. This dynamic pattern is consistent with trends shown in the literature, e.g., De Loecker (2007), Atkin, Khandelwal, & Osman (2017) and Garcia-Marin & Voigtländer (2019). The evolution of the treatment effect on productivity with international trade indicates that learning processes take time. It is different from selection into exporting, where productivity differences may be stable. Additionally, it is different from pure movements along the production frontier, where productivity should immediately jump and then remain unchanged.

[Insert Table 5 here]

R&D investment. By expanding the size of the market, trade encourages firms to invest in R&D, which could subsequently increase productivity (e.g., Lileeva & Trefler, 2010; Bustos, 2011; Meltiz & Trefler, 2012). To examine this potential channel, we estimate whether bunching firms increase R&D investment. Specifically, we consider the following indicators of R&D investment: (i) whether firms have positive R&D investment and (ii) the value of R&D investment (by assigning a value of 0 to firms without R&D investment). The results listed in column 1-2 of Table 6 show no effect on R&D investment.

[Insert Table 6 here]

Product rationalization. Firms may respond to trade participation by developing new products and/or dropping their least successful products (i.e., production rationalization according to Bernard, Redding, & Schott, 2011). The results listed in columns 3-5 of Table 6 show no effect on the number of products (measured at the 5-digit level) that firms manufacture and no effect on the number or outputs of new products (if any).

Markup. Firms might change their markups in response to access to international trade.

While we adopt quantity-based productivity estimation, the estimated productivity gains are not confounded by changes in markups (if any). Nevertheless, we examine whether bunching firms change their markups, which are calculated following De Loecker & Warzynski (2012), De Loecker et al. (2016), and Lu & Yu (2015). As shown in column 6 of Table 6, there is no impact on markups.

In summary, the aforementioned analyses on potential mechanism suggest that learning by trade plays an important role in productivity gains. We do not find supporting evidence for other channels, such as R&D investment, product rationalization and markup in our context.

5.5 Distributional Effects

We have documented significant productivity gains from trade. It is then interesting to determine how these gains are distributed among different agents. For example, firms may share parts of their revenue gains with workers by increasing employment and/or wage rates. Within industries, firms with different characteristics (such as firm age and ownership) may respond to trade differently, which causes structural changes in industrial composition. In this subsection, we investigate these dimensions to shed light on the distributional effects of gains from trade.

Firms vs. workers. To examine the sharing of productivity gains between firms and workers, we study the policy effects on average wage rates, employee benefits and total employment. The estimation results are reported in Table 7. The wage rate increases by 22 percent with access to international trade, whereas employee benefits and total employment remain unchanged. These results suggest that firms share portions of their gains from trade with workers by increasing the average wage rates but keeping the total employment unchanged.

[Insert Table 7 here]

Young vs. old firms. Next, we investigate whether young and old firms behave similarly in terms of trade participation. To this end, we divide firms into two groups based on their year of establishment. Specifically, given the complications of firm mergers and entries, we follow Haltiwanger, Jarmin, & Miranda (2013) in assigning firms an age in the first year the new firm identity is observed in the data and then trace this firm identity over time. Young firms are referred to those with ages below the sample median, whereas old firms are classified as those with ages above the sample median.[17] The estimation results are reported in columns 1-2 of Table 8, with panel A showing trade effects and panel B showing productivity effects. We find that both groups have statistically significant trade effects, with old firms showing a greater magnitude than young ones. Regarding productivity effects, there is a statistically and economically significant effect of productivity increases for young firms but no significant effect for old firms. These results indicate that while both firms increase trade participation upon the receipt of trading rights, the gains from trade occur only for young firms but not for old firms.[18] Our results are consistent with Haltiwanger, Jarmin, & Miranda (2013), who show that new firms contribute substantially to the job growth rate.

[Insert Table 8 here]

Non-SOEs vs. SOEs. In China, SOEs are more politically connected than non-SOEs, granting the former access to more resources. It is thus interesting to understand how trade participation changes the two groups of firms differently, and hence, the evolution of the industrial structure.

[17]To avoid outliers, following Haltiwanger, Jarmin, & Miranda (2013), when defining the median value of firm age, we winsorize the top 5% (firms that are more than 44 years old).

[18]As pointed out by Foster, Haltiwanger, & Syverson (2008), young firms charge lower prices than incumbents, which leads potential bias when comparing revenue-based productivity. To alleviate this concern, we have adopted the quantity-based productivity (i.e., physical productivity) in the estimation.

The estimation results are reported in columns 3-4 of Table 8, with panel A showing the trade effect and panel B showing the productivity effect. We find that having trading rights increases the participation in international trade for both non-SOEs and SOEs. However, regarding the effects on TFP, we find that the effect for non-SOEs is significant, while that for SOEs is insignificant and small in magnitude. These results indicate that gains from trade benefit non-SOEs more than SOEs. As SOEs tend to be less productive than non-SOEs, this implies an increase in production efficiency dispersion among firms.

6 Conclusion

This article applies the bunching method to identify productivity gains from trade. It is based on a regulatory policy in China stating that only firms with registered capital above statutory cutoffs are allowed to trade.

Graphical evidence confirms significant compliance with the policy. Comparing the compliant firms in the observed and counterfactual states, we find that obtaining trading rights increases participation in international trade and productivity. We also find that participation in international trade increases firm TFP (as a logarithm) by approximately 0.688. In terms of mechanisms, we show that firms do not adjust their R&D investment, product composition (at the 5-digit level) or markup. Dynamic effects of productivity gains indicate that learning by trade is an important channel in this context. In terms of distributional effects, we find that firms share their gains from trade with workers by increasing the average wage rates. Meanwhile, young firms and non-SOEs experience greater productivity gains than old firms and SOEs.

Governments from more and more developing countries are adopting the export-oriented development strategy and removing barriers to help firms become successful exporters. China provides a good research setting for studying this phenomenon. Similar to other developing countries, China was characterized by low worker productivity, abundant labor endowment, and low value added in the global production chain in the early 2000s, but experienced a dramatic increase in trade and GDP under an active export-promoting strategy. Finding evidence of productivity gains from trade in China can support the validity of applying export-oriented development strategies in other developing countries.

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Tables and Figures

Table 1: Historical Changes of the Threshold for Trading Rights

Time	1999∼2000	2001∼6/2001	7/2001∼8/2003	9/2003∼6/2004	7/2004∼
Panel A: Domestic-owned firms					
East	500[a]	500	300	50	0
Central and West	300[a]	300	200		
M&E, HTE	200[a]	200	100		
SEZ	200 before 2002, 100 afterwards				
Panel B: Foreign-invested firms					
All	0				

Notes: The unit for registered capital is 10,000 RMB. East means nonmechanical & nonelectronic firms in East China, Central and West denotes nonmechanical & nonelectronic firms in central and western China, M&E means mechanical and electronic firms, and HTE means high-technology enterprise. SEZ denotes firms in special economic zones which are subject to a lower threshold, including Shenzhen, Zhuhai, Shantou, Xiamen, Hainan initially and Pudong district in Shanghai later. Firms in Pudong were subject to the same policy as others, except that during 2002∼Aug 2003 the cutoff was reduced to 50. FIE denotes foreign-invested firms. The subscript "a" denotes that, during 1999∼2000 the threshold of registered capital only applies to state-owned firms, while additional requirements on net asset, revenue, value of exports and a higher threshold (at 850) of registered capital were imposed on private-invested enterprises.

Table 2: Summary statistics for manufacturing firms

Panel A: during 1998∼2007

	Obs	Trade Status			TFP	Registered Capital		
		Trade	Export	Import		p25	p50	p75
1998	149,422					73	236	768
1999	146,842					80	251	819
2000	147,998	16.1%	13.9%	12.8%	1.26	87	268	864
2001	156,510	17.6%	15.2%	13.6%	1.28	91	272	866
2002	166,671	19.6%	17.0%	14.6%	1.30	99	291	890
2003	181,067	21.4%	18.8%	15.3%	1.35	100	300	950
2004	256,595	22.4%	19.7%	15.6%	1.39	98	290	863
2005	248,588	23.6%	21.3%	15.7%	1.43	100	300	1000
2006	278,727	23.0%	20.9%	15.0%	1.47	100	305	1000
2007	312,364					100	321	1000
98∼07	2,044,784	21.1%	18.7%	14.8%	1.37	100	300	934

Panel B: across regions in 2002

	Obs	Trade Status			TFP	Registered Capital		
		Trade	Export	Import		p25	p50	p75
East	61,192	24.1%	21.0%	17.8%	1.65	94	280	826
Central and West	39,642	7.3%	6.1%	3.6%	1.36	82	250	777

Notes: the unit of registered capital is 10,000 RMB, and the unit of trade participation, export participation and import participation is 1.

Table 3. Estimates of Causal Effects on Trade Participation

	Trade Participation		
	Focal	Placebo	Placebo
	DE in 2002	DE in 2000	FIE in 2002
	(1)	(2)	(3)
Causal Effect	0.144***	0.005	-0.032
	(0.026)	(0.062)	(0.135)
Dep Variable Mean	0.074	0.068	0.583
Observations	19091	15923	5932

Significance: *.10; **.05; ***.01.

Notes: Column (1) report the impact on trade participation for our focal group, i.e. DE in East China in 2002, while column (2) and (3) reports the placebo estimates for samples without policy effect, i.e. DE in East China in 2000 and FIE in East China in 2002. The causal estimator is defined in equation 3 on page 23, in which it uses equations 1 and 2 laid out on pages 21-22 and the calculation procedures on page 23. See Section 4 for details on the estimation. The unit is 1. Standard errors are computed via bootstrap.

Table 4 Estimates of Causal Effects on Productivity

	Productivity			Productivity to Trade
	Focal	Placebo	Placebo	Focal
	DE_2002	DE_2000	FIE_2002	DE_2002
	(1)	(2)	(3)	(4)
Causal Effect	0.099***	-0.034	-0.002	0.688***
	(0.019)	(0.180)	(0.189)	(0.058)
Observations	18510	15372	5761	18510

Significance: *.10; **.05; ***.01.

Notes: Column (1) reports the impact on productivity for our focal group, while column (2) and (3) report the placebo estimates for samples without policy effect. The causal estimator is defined in equation 3 on page 23, in which it uses equations 1 and 2 laid out on pages 21-22 and the calculation procedures on page 23. Column (4) reports the causal effect of international trade on firm productivity, by dividing the estimated coefficient in column 1 of Table 4 by that in column 1 of Table 3. Standard errors are computed via bootstrap.

Table 5. Potential Mechanism – Learning by Exporting

	2000	2002	2003	2004	2005	2006
	(1)	(2)	(3)	(4)	(5)	(6)
Causal Effect	-0.034	0.099***	0.105***	0.173***	0.156***	0.157***
	(0.189)	(0.019)	(0.030)	(0.043)	(0.045)	(0.046)
Observations	15372	18510	15290	11117	10400	9565

Significance: *.10; **.05; ***.01.

Notes: Table 5 reports the dynamic effects on productivity. The causal estimator is defined in equation 3 on page 23, in which it uses equations 1 and 2 laid out on pages 21-22 and the calculation procedures on page 23. See Section 4 for details on the estimation. Standard errors are computed via bootstrap.

Table 6. Potential Mechanism – R&D, Product Scope, Markup

	R&D		Product Scope			Markup
	R&D>0	R&D	Num. of Products	New>0	Output of New Product	
	(1)	(2)	(3)	(4)	(5)	(6)
Causal Effect	-0.032	-2.309	-0.019	-0.002	-107.793	-0.016
	(0.031)	(13.060)	(0.102)	(0.021)	(465.298)	(0.026)
Y_ct	0.103	15.810	1.545	0.034	565.124	1.104
Observations	19091	19091	19091	19091	19091	18494

Significance: *.10; **.05; ***.01.

Notes: Column (1) and (2) report the estimates on participation in R&D investment and the value of R&D investment respectively. Column (3), (4) and (5) report the effect on number of products, number of new products and output of new products. Column (6) reports the effect on markup, which is calculated following De Loecker & Warzynski (2012), De Loecker et al. (2016), and Lu & Yu (2015). The causal estimator is defined in equation 3 on page 23, in which it uses equations 1 and 2 laid out on pages 21-22 and the calculation procedures on page 23. See Section 4 for details on the estimation. See Section 4 for details on the estimation. Standard errors are computed via bootstrap.

Table 7. Distributional Effects – Workers

	Employment	Wage	Employee Benefits
	(1)	(2)	(3)
Causal Effect	-0.099	0.220***	-0.056
	(0.102)	(0.064)	(0.142)
Y_ct	4.659	2.049	1.276
Observations	19091	19091	15457

Significance: *.10; **.05; ***.01.

Notes: Column (1), (2) and (3) report the estimates on employement (in logarithm), wage (in logarithm) and employee benefits (unit: 1,000 RMB) respectively. The causal estimator is defined in equation 3 on page 23, in which it uses equations 1 and 2 laid out on pages 21-22 and the calculation procedures on page 23. See Section 4 for details on the estimation. Standard errors are computed via bootstrap.

Table 8. Distributional Effects – Young vs Old, SOE vs Private

	Firm Age		Ownership	
ITT	Young Firms	Old Firms	non-SOEs	SOEs
	(1)	(2)	(3)	(4)
Trade Participation	0.121***	0.206***	0.128***	0.120**
	(0.026)	(0.054)	(0.025)	(0.061)
Y_ct	0.081	0.066	0.090	0.053
Observations	10828	8238	10956	8135

	Firm Age		Ownership	
ITT	Young Firms	Old Firms	non-SOEs	SOEs
	(1)	(2)	(3)	(4)
Productivity	0.115***	0.055	0.075***	0.049
	(0.019)	(0.049)	(0.018)	(0.068)
Y_ct	1.552	1.565	1.576	1.551
Observations	10522	7980	10682	7828

Significance: *.10; **.05; ***.01.

Note: Please refer to the next page.

Note: Column (1) and (2) report the estimates for old firms and young firms. We divide firms into two groups based on the years of establishment: young firms are referred to those with ages below the sample median, whereas old ones are classified as those with ages above the sample median. Column (3) and (4) report the estimates for SOEs and non-SOEs. The causal estimator is defined in equation 3 on page 23, in which it uses equations 1 and 2 laid out on pages 21-22 and the calculation procedures on page 23. Standard errors are computed via bootstrap.

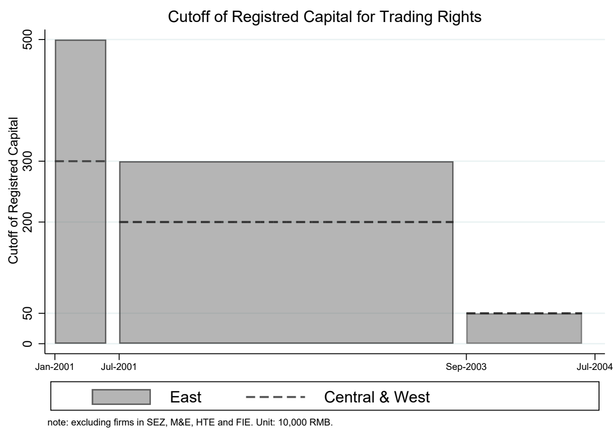


Figure 1. Historical Changes of the Threshold for Trading Rights for Domestic-Owned Firms

Notes: The unit is 10,000 RMB. Firms in SEZs are omitted in the figure to avoid overlapping. Details are shown in Table 1.

Figure 2. Regional Distribution on the Minimum Requirements on Registered Capital for Trading Rights in 2002

Notes: We exclude SEZ, HTE and firms in M&E sector in the figure.

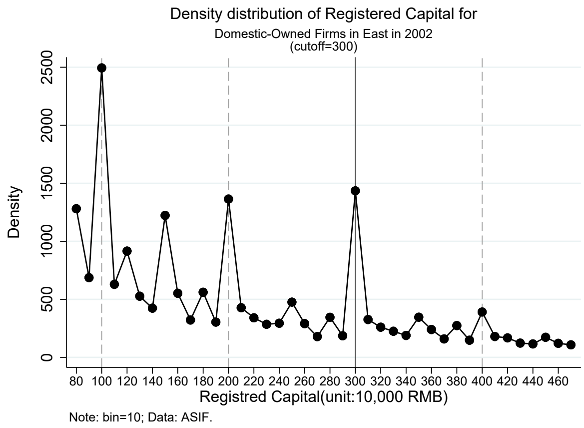
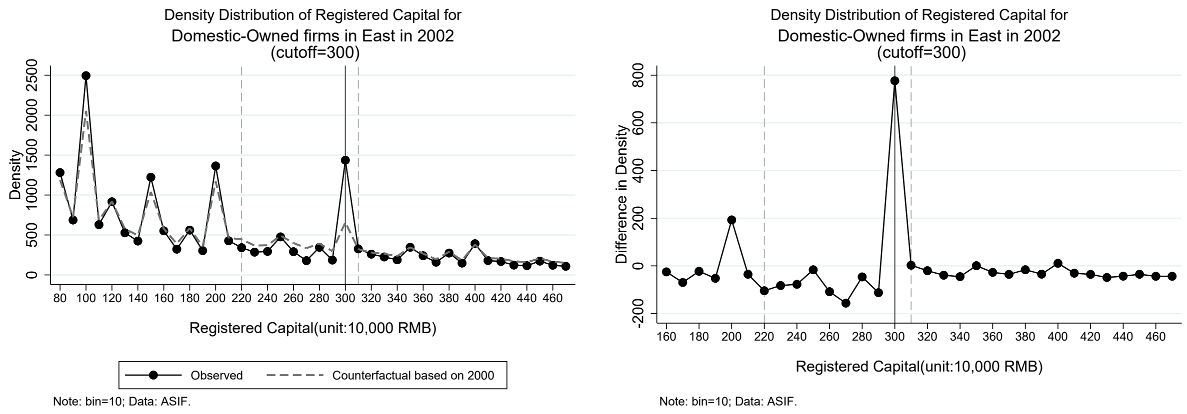


Figure 3 Density Distribution of Registered Capital at Threshold 300 for DOE East in 2002

Note: The minimum requirement on registered capital for trading rights was 300 (unit: 10,000 RMB) in 2002 and was at 500 for SOEs and 850 for PIEs (with other requirements) in 2000. Upon comparing Figure 3 and Figure A1a, we find that bunching at 300 is much higher in the former than in the latter, suggesting that firms indeed adjust their registered capital to the threshold to obtain trading rights.

a. Observed vs Counterfactual (nonparametric) b. Observed − Counterfactual (nonparametric)



c. Observed vs Counterfactual (parametric) d. Net of rounding: Observed vs Counterfactual (parametric)

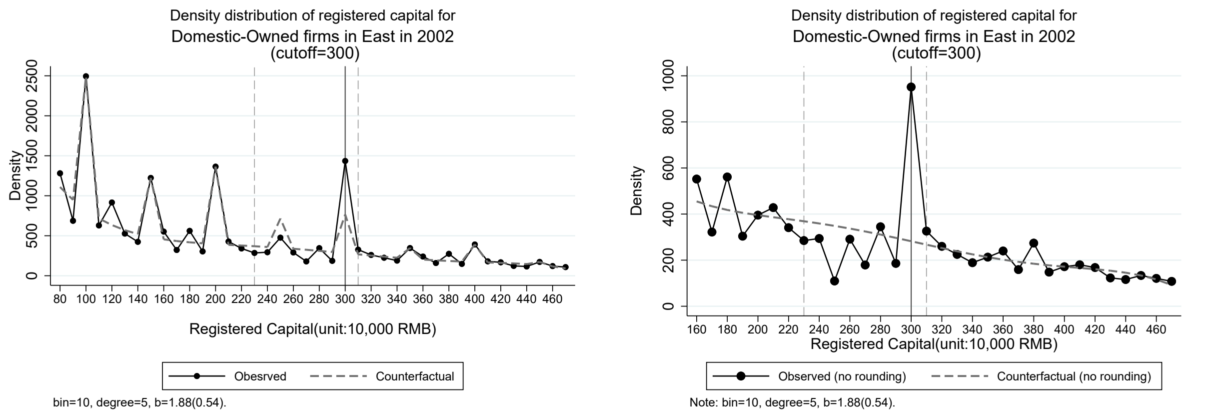
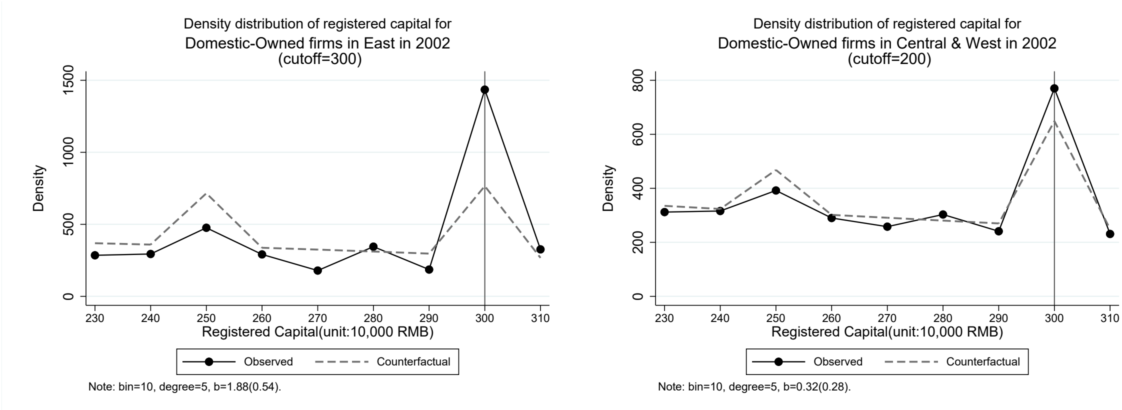


Figure 4 Density Distribution of Registered Capital at Threshold 300 for DOE East in 2002: Observed and Counterfactual

Notes: Please refer to the next page.

Notes: In Figure 4a, the solid and dashed lines show the observed and estimated counterfactual distributions using the nonparametric approach. In Figure 4b, the solid line shows the difference between the observed and the counterfactual distributions using the nonparametric approach. In Figure 4c, the solid and dashed lines show the observed and estimated counterfactual distributions using the parametric approach. In Figure 4d, the solid and dashed lines show the observed distribution (net of reference point effects) and the estimated counterfactual distribution (net of reference point effects) using the parametric approach. After controlling for reference point effects, there is extra bunching at the policy threshold 300, suggesting responses by firms to the policy.

a. DE East in 2002; b. DE Central and West in 2002



c. DE East in 2000; d. FIE East in 2002

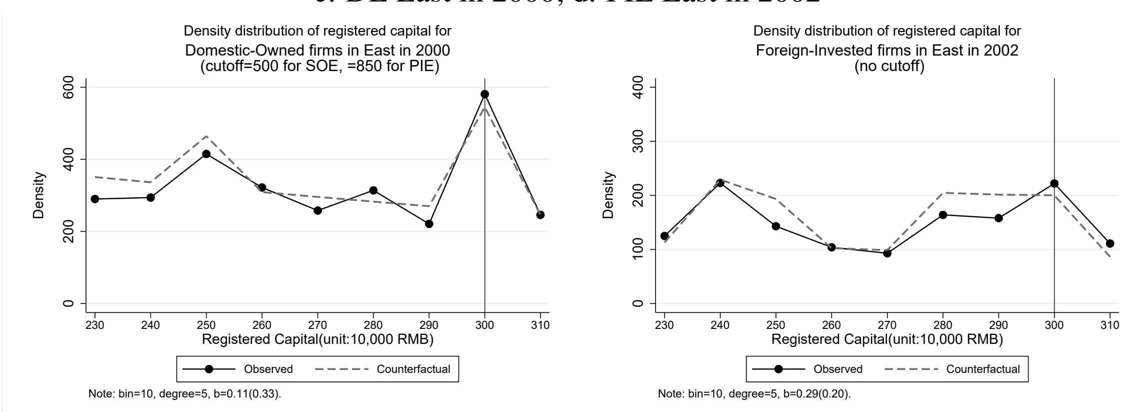
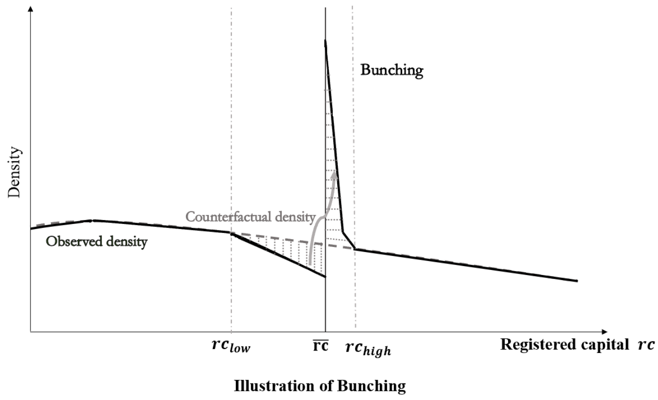


Figure 5 Density Distribution within excluded region for Focal Group and Placebo Groups

Notes: To verify our chosen excluded region and polynomial order, we impute the counterfactual distribution using the same function for unaffected samples, and compare it to the observed distribution. Panel a shows excess bunching at the cutoff 300 for DE in East (focal group) in 2002. Panel b shows bunching but to a much less degree for DE in Central and West (placebo group) in 2002. This is due to the fact that 300 was the cutoff in 2000 for these firms and they continue to stay there when facing high adjustment cost from reducing their registered capital. Panel c and d show no bunching for DE in East in 2000 (placebo group) and FIE in East in 2002 (placebo group). The counterfactual distributions well match the actual distributions in the unaffected samples, lending further support to our speicification choice in the parametric estimation of the counterfactual distribution.

a. Change in Density Distribution



b. Change in Outcome Distribution

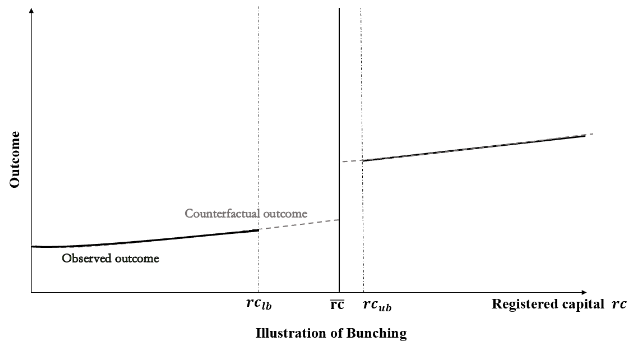


Figure 6. Illustration of Bunching Estimator

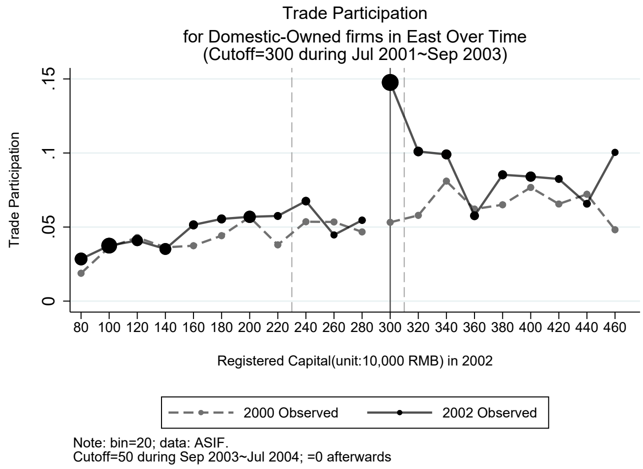
Notes: Please refer to the next page.

Notes: Figure 6a shows the change in density distribution. Within the adjustment region, some firms below the cutoff (in the shaded area marked by vertical dashed lines) would adjust their registered capital upward (to the shaded area marked by horizontal dashed lines). Denote them as compliers. Firms outside the excluded region do not respond.

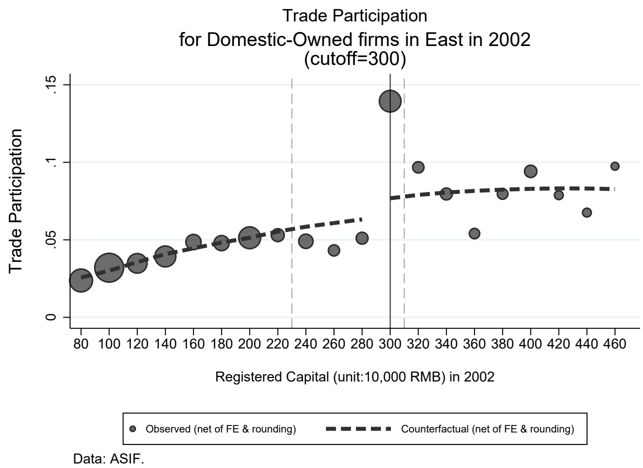
Figure 6b illustrates change in outcome distribution. Out of the adjustment region, the observed and counterfactual distributions match. Within the adjustment region, the two distributions may differ. Specifically, for region $[rc_{lb}, \overline{rc})$, the observed distribution corresponds to the outcome of never-takers, while the counterfactual distribution corresponds to the weighted average outcome of never-takers and compliers. Therefore, we can infer the *average counterfactual outcome of compliers*. For region $[\overline{rc}, rc_{ub}]$, the observed distribution corresponds to the weighted average of always-takers and compliers, while the counterfactual distribution corresponds to that of always-takers. Therefore, we can compute the *average treated outcome of compliers*. Difference between the computed *average treated outcome of compliers* and the computed *average counterfactual outcome of compliers* identifies the causal effect.

In addition, compliers may have higher or lower treated outcome than always takers, depending on the datsets. Therefore, in illustrative figure 6b, we omit the observed distribution for region $[\overline{rc}, rc_{ub}]$. Similarly practice is done for region $[rc_{lb}, \overline{rc})$.

a. 2002 v.s. 2000



b. 2002: Observed and Counterfactual



c. 2002 Compliers: Observed and Counterfactual

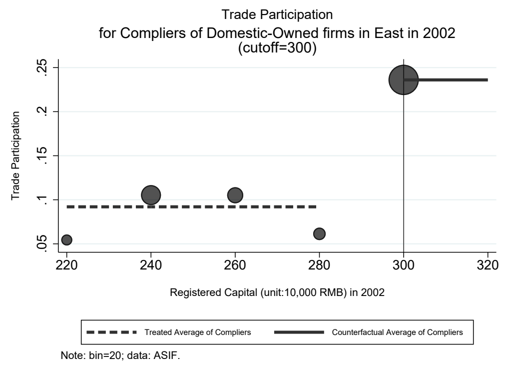
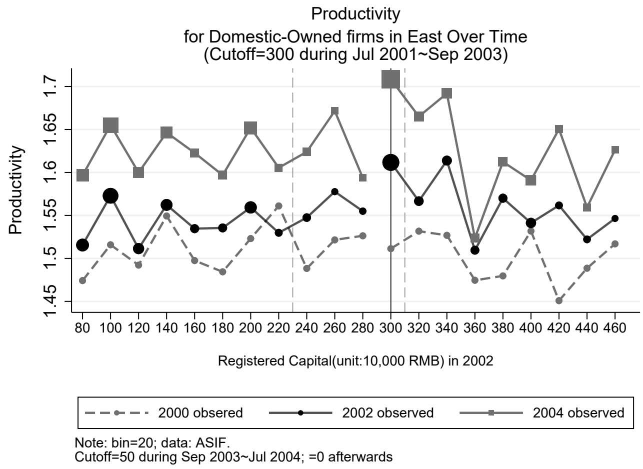


Figure 7. Distribution of Trade Participation: Observed and Counterfactual

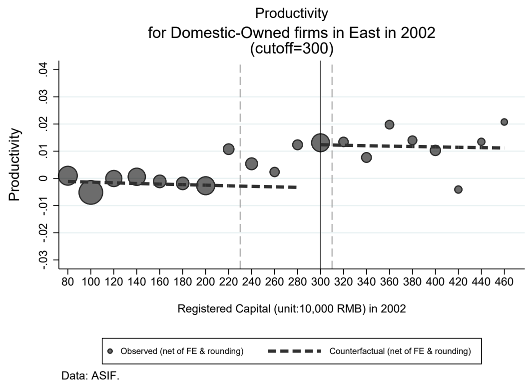
Notes: Please refer to the next page.

Notes: Figure 7a shows the observed distribution of trade participation for 2000 and 2002. Figure 7b shows the observed (denoted by dots) and estimated counterfactual (i.e., without adjustment of registered capital, denoted by dashed line) distributions of trade participation net of all the controls (including fixed effects and rounding effects) in 2002. The two dashed vertical lines represent the excluded region and the solid vertical line is the policy cutoff. Figure 7c focuses on the comparison of compliers between treated state and control state. Specifically, the dots denote the percentage of compliers engaging in international trade for each bin in 2002, with a larger size indicating a larger number of compliers; the dash line to the left of the cutoff presents the average value of compliers in the control state; the solid line to the right of the cutoff reports the average value of compliers in the treatment state. The average participation in trade of compliers increased from the control to the treatment state, indicating that obtaining trading rights indeed increases firms' participation in international trade.

a. 2002, 2004 and 2000



b. 2002: Observed and Counterfactual



c. 2002 Compliers: Observed and Counterfactual

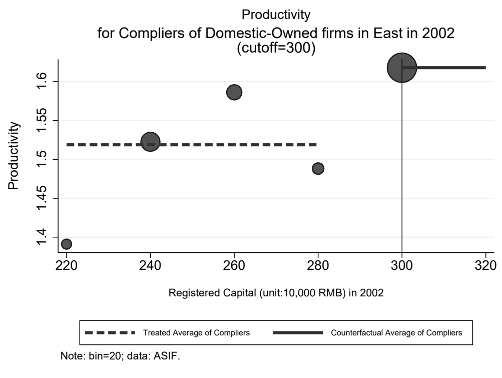


Figure 8. Distribution of Productivity: Observed and Counterfactual

Notes: Please refer to the next page.

Notes: Figure 8a shows the observed distribution of productivity for 2000, 2002 and 2004. Figure 8b shows the observed (denoted by dots) and counterfactual (denoted by the dashed line) distributions of productivity net of all the controls (including fixed effects and rounding effects) in 2002. The two dashed vertical lines represent the excluded region and the solid vertical line is the policy cutoff. Figure 8c focuses on the comparison of compliers between treated state and control state, using the same layouts as Figure 7c. Specifically, comparing the values of the counterfactual distribution with the observed distribution within the excluded region to the left of the cutoff, we recover the productivity levels of compliers in the counterfactual state. Meanwhile, comparing the values of the observed with the counterfactual distributions within the excluded region to the right of the cutoff, we recover the productivity levels of compliers in the treated state (after registered capital adjustment and obtaining trading rights). Difference between avergae counterfactual values of compliers (denoted by dashed line in 8c) and average treated values of compliers (denoted by solid line in 8c) indicates that obtaining trading rights increases firm productivity.