Free Market Initiatives: Benefits and Distortions from Political Power[*]

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Abstract

Political power can influence resource allocation among individual businesses, disrupting the function of free markets. This study presents the most comprehensive analysis to date of the impact of China's Free Trade Zone (FTZ) initiative, a nationwide market-oriented reform, on the performance of resident firms, and how this impact varies depending on political power. Using a difference-in-differences framework, we employ two distinct identification strategies to enhance comparability between treatment and control firms and mitigate selection bias. Our results demonstrate that FTZs improve firm performance across key indicators such as return on assets, total factor productivity, and exports; however, FTZ benefits are disproportionately skewed towards businesses with state ownership or with political connections. This imbalance is driven by firms that receive political power achieving lower effective tax rates and reduced financial expenses, leveraging ambiguous legal provisions and unequal access to credit support. These findings highlight the critical role of political power in market-oriented environments, particularly in emerging markets.

Keywords: market liberalization, political power, firm performance

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"Our emphasis on the virtues of free, competitive markets and exogenous technical change can distract us from the importance of power … in influencing politics to change the rules of the game. Without an analysis of power, it is hard to understand inequality or much else."

—Angus Deaton, Rethinking My Economics, Mar. 2024

1. Introduction

Classical economic theories argue that market liberalizations foster healthy economies by minimizing government intervention, thereby promoting competitive markets with efficient allocation of resources and equal distribution of benefit (Mises, 1927; Friedman, 1962; Buchanan, 1975; Edmonds and Pavcnik, 2006; MacLeod, 2013). However, since the 2008 financial crisis, scholars have increasingly questioned this belief. They suggest that, in a market-driven environment, the concentration of power in the hands of a few may distort the distribution of benefits, leading to inequities in both opportunities and results, especially in emerging economies lacking robust institutional frameworks (Rausser et al., 2011; Cruz and Graham, 2022; Su and Zhang, 2023).

This disparity between the goals of market liberalization and the skewed distribution of benefits is particularly evident in China's Free Trade Zone (FTZ) initiative, launched in 2013 and later expanded across multiple provinces. Intended to foster liberalized markets, FTZs offer a set of preferential policies such as reduce in corporate taxes, exemptions from import duties, streamlined customs clearance procedures, and better access to logistics and transportation (The State Council Information Office of the PRC, 2023; Huang et al., 2017). While official Chinese media emphasize significant improvements in firm performance within FTZs (People's Daily Overseas Edition, 2021), more independent outlets, such as Reuters, have reported instance where firms have scaled back or withdrawn their investments from these zones. These firms chose to disengage from beneficial FTZ policies despite having no substantial differences in industry or core business operations compared to firms that remained (Tham et al., 2019).[1] Although media reports may carry biases, while, to the best of our knowledge, the lack of rigorous academic studies

[1] People's Daily Overseas Edition. 2021. "Zhongguo (Shanghai) ziyoumaoyi shiyanqu—dadan shi dadan chua-ng zizhu gai [China (Shanghai)'s Pilot Free Trade Zone—to try boldly, to break bravely, and to change independent-ly]." April 18. https://export.shobserver.com/baijiahao/html/359612.html.

Tham, Engen, and Cheng Leng. 2019. "Bankers' exits and zombie accounts: China's Shanghai free trade zone sputters." Reuters, September 1. https://www.reuters.com/article/us-china-shanghai-ftz/bankers-exits-and-zombie-accounts-chinas-shanghai-free-trade-zone-sputters-idUSKCN1VN01V.

investigating these diverging firm outcomes highlights a unneglectable gap in the current literature.

Our study addresses this gap by providing a rigorous causal identification analysis of FTZ influence on firm performance and productivity. We leverage the FTZs as an exogenous economic shock impacting firms within the zone area (Zhang et al., 2023; Yang et al., 2024) and contribute novel insights into how political power may influence economic efficiency and equity for individual market players, a dimension often overlooked in research due to limited empirical evidence and endogeneity concern of firm behaviors (Alesina, and Angeletos, 2005; MacLeod, 2013). The exogeneity of the FTZs is supported by two key factors: (1) the central government's unpredictable designation of FTZ locations (see section 2.1), and (2) the absence of significant pre-existing trends in firm performance prior to FTZ implementation (see section 2.3).

To assess the impact of FTZs on firm performance, we construct an extensive firm-level panel dataset that covers all 21 provinces that implemented FTZs in sequence, spanning from 2007 to 2023, integrating public annual financial reports of listed firms from the stock exchanges with the China industrial enterprises database maintained by the National Bureau of Statistics. This dataset includes a broad array of firm-specific variables, such as export performance, return on assets, total factor productivity, ownership structure, political connections, industry classification, founding date, and geographic location. It represents one of the most comprehensive datasets for analyzing the effects of FTZ policies on firms.

Our primary performance indicators are return on assets, total factor productivity, and exports, which are key indicators commonly used in literature to assess firm performance (Bloom and Van Reenen, 2007; Demir and Su, 2016; Su and Zhang, 2023).[2] Total factor productivity is estimated using the Levinsohn-Petrin (2003) method for baseline specifications, while the Olley-Pakes (1996) approach is employed as a robustness check.[3] A firm is defined as having government ownership if government institutions, agencies, public entities, or state-owned enterprises hold more than 5% of the shares, rank among top ten shareholders, or appear in the list of the top ten shareholders of unrestricted tradable shares. Political connections are captured using a dummy variable indicating

[2] We do not include imports as outcome variables in this study because China's economic growth model has been largely driven by exports, and the focus of China's FTZs and broader economic strategy is primarily on enhancing export competitiveness (Rodrik, 2006).

[3] We used both the Levinsohn-Petrin (2003) and Olley-Pakes (1996) approaches to estimate the total factor productivity, and the results were consistent across both methods. The primary difference between the two methods lies in the choice of proxy for unobserved productivity shocks: Olley-Pakes uses investment, while Levinsohn-Petrin uses intermediate inputs, each with its own implications for the estimation process. The Levinsohn-Petrin approach improves on the Olley-Pakes method by enhancing the accuracy and applicability of TFP estimation, proving more successful in practice (Van Beveren, 2012).

whether a member of the firm's management holds or held a position in governmental or political organizations (Zhang et al., 2013; Luo and Liu, 2019). As a robustness test, we construct a standardized index based on the highest political positions held by the firm's management.

A major challenge in identifying the causal effect of FTZ policies is the potential for selection and sorting bias. For instance, export-oriented firms or those with a comparative advantage in international trade may have already been concentrated in FTZ regions, which often have geographical and infrastructure advantages, such as proximity to seaports, airports, or bonded zones. In addition, firms may have strategically relocated to FTZs after the policy announcement to benefit from the favorable terms offered, further complicating causal attribution.

To mitigate these concerns, we adopt a matching approach, where the control group consists of firms never benefiting from FTZ policies during the sample period. We match firms in the control group to each FTZ beneficiary firm based on geographical proximity and firm characteristics in history, industry, ownership status, political connection, and financial metrics. This method assumes that, after controlling for these observable characteristics, the performance trajectories of FTZ beneficiary and non-beneficiary firms in terms of return on assets, total factor productivity, and export performance would have been similar in the absence of the policy. This assumption implies that the benefits of FTZs are effectively randomly distributed among firms that receive such benefits. We conduct event study analysis to test for the presence of parallel trends in performance outcomes between FTZ beneficiary firms and the respective control groups prior to the implementation of the FTZ policy. Our results show no significant pre-trend existence and provide evidence of consistent significant positive effects of FTZs on each outcome variable following FTZ implementation.

In addition, we use an alternative difference-in-differences strategy that exploits the staggered rollout of FTZs across regions over time, including only firms benefiting from FTZs at different time points. This method differs from the matching approach by assuming that firms benefiting from FTZs later in the sample period can serve as a counterfactual for firms benefiting earlier.

To mitigate potential sorting issues, we define FTZ beneficiaries based on their headquarters location one year before the establishment of the local FTZ, as well as their ownership and political connection status at the same time. We exclude firms that relocated their headquarters to FTZ regions after 2012, a year before the Shanghai FTZ pilot began, which helps avoid reverse causality. Our empirical analysis finds that the establishment of FTZs leads to a statistically significant

increase of 1.5 percentage points in return on assets, 3.7 percentage points in total factor productivity, and CNY 546.8 thousand in exports for firms within the zones, relative to their counterparts in the control group. These findings are consistent with the FTZ's objectives of promoting international trade, economic growth, and productivity improvements.[4]

However, we propose that the benefits of the FTZ policy may not be among firms benefiting from the FTZs due to the influence of political power, which could lead to the observed disparities in the performance of firms benefiting from the policy. To test this hypothesis, we first examine the differential effects based on state ownership, a key factor of political power in China's economy (Fan et al., 2007). Employing a triple-difference approach that leverages variations in policy the timing of policy implementation across provinces and ownership types, our results show significant heterogeneity in the FTZ's impact across all three outcome variables. Specifically, firms with state ownership experience a disproportionate benefit, with an additional 3.1 percentage point increase in return on assets, a 13.2 percentage point increase in total factor productivity, and a CNY 2,333.5 thousand increase in exports compared to firms without state ownership.

Although these results might suggest that the FTZ policy is intentionally skewed in favor of firms with state ownership, the heterogeneous effects likely stem from the broader influence of political power, which extends beyond state ownership. Two crucial dimensions for operationalizing political power are government ownership stakes and the political connections of a firm's managerial team (Besley and Persson, 2011; Acemoglu and Robinson, 2012). Firms often cultivate relationships with the government to leverage political power, which may grant them preferential access to resources and policy advantages (Faccio, 2006).[5] Therefore, we further explore whether the FTZs disproportionately benefit firms with political connections, regardless of their ownership structure. Notably, firms with political connections do not always have state ownership; private firms can forge political connections through various means, such as appointing government-appointed executives (Qian, 2000; Hsieh and Song, 2015) or actively engaging in

[4] Return on asset and total factor productivity are key performance indicators to measure firm profitability and productivity (Bloom and Reenen, 2007; Demir and Su, 2016; Su and Zhang, 2023). We estimate TFP using the Levinsohn and Petrin (2003) approach as the baseline method, and employ the Olley and Pakes (1996) approach for robustness checks. Both methods yield consistent results.

[5] We operationalize government ownership and political connections within a firm's managerial team using the measurement methodologies by Fan et al. (2007), Zhang et al. (2013), and Luo and Liu (2019). Following their approach, we focus on the chairman and CEO to represent the firm's managerial team.

political networks and events (Hu, 2006; Morton et al., 2020).[6]

Our results from the triple-difference approach provide compelling evidence that firms with political connections experience significantly greater performance gains from the FTZ policy compared to their counterparts without political connections, regardless of ownership structure. Specifically, firms with political connections show a 1.7 percentage point increase in return on assets and a 19.3 percentage point increase in total factor productivity. These findings suggest that, rather than being solely designed to privilege state-owned enterprises, the FTZs disproportionately benefit firms receiving political power, diverging from the policy's original goal of establishing a market environment independent of external political forces.

To ensure the robustness of our findings, we conduct a series of tests that address key methodological concerns. Specifically, we employ the Goodman-Bacon (2021) decomposition to assess the impact of staggered treatment timing, perform permutation placebo tests to validate the statistical significance of our findings, and examine the confounding effects of concurrent policies, such as the value-added tax reform and the One Belt One Road initiatives. Additionally, we apply alternative measurements for key variables, including a hierarchical measure of political connections based on the highest level of government positions, and the Olley-Pakes (1996) method to measure total factor productivity. To further test the robustness, we rescale the geographical analysis to the regional level and assess the impact of different policy implementation timings to determine if the results persist. The consistency of our results across all these tests reinforces the reliability of our main conclusions.

In addition to verifying robustness, we explore the mechanisms driving the uneven distribution of benefits from FTZs. Our analysis identifies two key channels through which political power magnifies these gains: preferential tax treatment and enhanced access to financial credit. Firms within FTZs experience significant reductions in both their effective tax rates and financial expenses compared to non-FTZ firms. Specifically, state-owned firms benefit from a 5.7 percentage point reduction in the effective tax rate and a 2.3 percentage point reduction in financial expenses. Furthermore, politically connected firms enjoy an additional 3.4 percentage point

[6] Government appoints and determines executive personnel to retain authority for some non-state-owned enterprises, which were original small-sized, low-profit, non-core industry state-owned enterprises during corporatization (Hsieh and Song, 2015). Private entrepreneurs typically forge political connections through participation in the central or local People's Congress or Political Consultative Conference, where decisions on legislation and personnel assignments are made (Hu, 2006; Luo and Liu, 2009). More information is provided in section 2.2.

decrease in their effective tax rate and a 1.8 percentage point reduction in financial expenses. These advantages likely stem from regulatory ambiguities in FTZ guidelines, which allow for flexible interpretation and selective enforcement.

Our research contributes the literature in five aspects. First, our research deepens the comprehension of FTZs as powerful instruments for economic liberalization. Traditional studies have emphasized FTZs' role in promoting export-led growth, enhancing productivity, and fostering foreign direct investment (Hamada, 1974; Feenstra, 1998; Aitken and Harrison, 1999). We expand on these findings by showing that FTZs not only facilitate market liberalization but also increase firm performance across several dimensions, such as profitability, productivity, and exports, contributing to the growing literature on the economic impacts of FTZs (Wei, 2009).

Second, while classical economic theories assume that market liberalization policies such as FTZs function independently of political influence, our study reveals how political power can skew the distribution of benefits. This underscores the importance of power dynamics in free markets, a topic that has become increasingly pertinent post-2008 as many economies attempt to balance market-oriented reforms with governance and regulatory frameworks (Besley and Persson, 2011; Acemoglu and Robinson, 2012). Our findings emphasize the need for deeper investigation into how political power distorts the competitive benefits intended by such policies.

Third, while previous research has largely focused on macroeconomic indicators such as GDP and industrial output to assess the effects of market liberalization (Bhagwati, 1993; Bown and Crowley, 2013; Pierce and Schott, 2016; Jaumotte and Papageorgiou, 2020), our study contributes by shifting the focus to the firm level. By leveraging a plausibly exogenous policy shock—the staggered implementation of FTZs—we minimize concerns of endogeneity, addressing a key limitation of earlier studies that predominantly rely on national-level aggregates (Fisman and Wei, 2009; Acemoglu and Robinson, 2012; Morton et al., 2022; Su and Zhang, 2023).This micro-level perspective offers new insights into the mechanisms through which political power influences firm outcomes in an emerging market.

Fourth, this research contributes to the literature on government intervention for firms. While previous studies typically argue that such intervention inherently detriments firm performance due to principal-agent problems and the irreconcilability between welfare-maximization and profit-maximization goals (Shleifer and Vishny, 1994, 1998; Acemoglu and Johnson, 2005; Zhang et al., 2020), we document that firms with political connections or state ownership experience significant

gains in profitability, productivity, and export performance, demonstrating that the impact of government intervention can be positive when political power is factored into the analysis (Besley and Ghatak, 2003; Megginson and Netter, 2003; Li and Zheng, 2017).

Finally, we contribute to the literature on the economic consequences of institutional factors by showing how imperfect legal and regulatory frameworks enable firms with political power to extract disproportionate benefits from policies such as FTZs (North, 1990; Rodrik et al., 2004). In developing economies, where market-oriented reforms are often incomplete, political influence can create an uneven playing field, undermining the competitive fairness that such reforms aim to establish (Djankov et al., 2008). Our findings are particularly relevant for policymakers in these contexts, as they highlight the risks of allowing political power to interfere with market liberalization efforts and point to the need for more precise regulatory frameworks to prevent such distortions (Acemoglu and Johnson, 2005).

The rest of the paper is structured as follows. Section 2 describes the FTZs and political connections of enterprises and discusses the exogeneity of the policy. Sections 3 and 4 discuss the data and the empirical strategy, respectively. Section 5 presents the results of the main estimation and robustness checks. Section 6 discusses mechanisms, and section 7 concludes.

2. Background

We start by introducing China's Free Trade Zone (FTZ) policy, followed by an overview of the institutional background of political connections. We then examine the relationship between FTZs and residential firms' attributes.

2.1. Objectives and development of China's Free Trade Zone

Free Trade Zones (FTZs) are specially designated areas within a country where businesses receive from preferential treatments such as tariff exemptions and relaxed trade supervision (Papadopoulos, 1987). These zones typically offer a more favorable business environment compared to non-FTZ areas by providing flexible labor regulations and proactive institutional frameworks. In China, FTZs also extend benefits such as import tax exemptions, streamlined customs procedures, more flexible currency exchange mechanisms, reduced corporate tax rates, and expedited value-added tax refunds, creating incentives for both domestic and international firms to operate within these zones.

The establishment of China's FTZs began in 2013 with a pilot zone in Shanghai, part of a broader central government effort to promote economic reform and market liberalization (Yao and

Whalley, 2016). This initiative expanded to Fujian, Guangdong, and Tianjin in 2015, followed by the inclusion of more provinces and municipalities in 2017 and 2019. By 2020, China had established 21 FTZs, covering diverse industries such as international trade, finance, oil and gas, aviation equipment, biotechnology, tourism, e-commerce, and logistics (Zeng et al., 2023).[7] As shown in Figure 1 and summarized in Table A1, the phased implementation of FTZs reflects the evolving scope of the policy across different regions.

[Insert Figure 1 here]

China's FTZs were introduced within the context of the central government's institutionalized approach to policy experimentation. The rollout, which began shortly after the Xi-Li Administration took office in March 2013, was characterized by central government decisions regarding the timing, content, and location of the zones. This top-down approach made the FTZ rollout unpredictable for local governments and businesses. For example, the sudden establishment of the Shanghai FTZ, despite public debates on the potential exacerbation of regional disparities due to preferential policies, exemplifies this unpredictability. The unexpected nature of the rollout restricted local firms from making preemptive adjustments, underscoring the exogenous nature of the policy introduction (Yao and Whalley, 2016).

The staggered and unpredictable implementation of FTZs across various regions and years provides a strong basis for a multi-period difference-in-differences research design (see Section 2.3). The central government's unilateral control over the timing and location of FTZs limits the influence of local firm behavior on the policy, reducing concerns about reverse causality and making the policy changes suitable for assessing their impact on firm performance (Bertrand and Mullainathan, 2003).

Although official endorsements from the Chinese Communist Party (CCP) portraying FTZs as successful models for market liberalization, skepticism remains regarding the full scope of their benefits. Independent reports, such as those from Reuters, have highlighted cases of businesses withdrawing investments or closing operations within FTZs, raising concerns about the uneven distribution of benefits (Tham and Leng, 2019). These reports suggest that while FTZs create opportunities for some firms, the associated risks and costs may outweigh the benefits for others, challenging the notion of universal gains from the policy.

[7] The FTZ continues expansion beyond 2022. However, we limit our discussion to subjects affected up to 2022 due to data constrains.

2.2. Institutional background of firm connections to the government

Entrepreneurs in emerging markets often face institutional voids, such as limited access to bank loans and weak enforcement of contracts and property rights (Frye and Zhuravskaia, 2000; McMillan and Woodruff, 2003). These gaps create significant hurdles for firms attempting to operate efficiently and competitively. Political connections, although intangible and not formally recognized on financial statements, can be instrumental in overcoming these barriers. Firms with strong political connections can secure favorable regulatory conditions and resources, such as more lenient loan approvals, larger credit limits, lower interest rates, and advantageous legal interpretations, all of which enhance their competitive positioning (Faccio, 2006). Therefore, establishing connections with government entities becomes a strategic necessity for businesses to navigate both market and bureaucratic challenges (Li et al., 2008).

Firms in China establish political connections typically by hiring individuals with past or current roles in government or related agencies to senior management positions. This practice is rooted in historical developments. During China's economic reforms in the 1980s, decision-making authority of state-owned enterprises was decentralized from the central-government level to the local-government level to stimulate market-driven growth (Keller and Shiue, 2007). As part of this process, many smaller, less profitable, and non-core state-owned enterprises underwent "corporatization," transforming into non-state-owned enterprises while maintaining governmental oversight in key areas, such as the appointment of executives and the determination of their compensation (Hsieh and Song, 2015). The subsequent partial privatization of state-owned enterprises in the 1990s further entrenched these connections, allowing government-appointed executives to maintain their political connections. This helps firms gain privileged access to industry permits, bank loans, and protection from local government interference, along with safeguarding property rights (Qian, 2000; Zhao, 2002; Hu, 2006; Luo and Liu, 2009; Islam et al., 2021).

Many private enterprises, even without a background in corporatization, also establish political connections. Under the framework of the Chinese Communist Party's (CCP) constitutional "united front (*Tongyi zhanxian*)," private individuals who make significant contributions to society—such as generating substantial employment, paying large amounts of tax, or donating to education and social welfare—are allowed to participate in political processes. These individuals can be appointed to the People's Congress or the Chinese People's Political

Consultative Conference (CPPCC). The People's Congress plays a key role in legislative functions and personnel appointments, and the CPPCC serves as a consultative body composed of representatives from non-Communist parties and other organizations, under the leadership of the Communist Party.

By joining these bodies, private entrepreneurs gain political identities that enhance their influence and standing in both political and business circles. These political positions help them form connections with government officials, enabling their firms to access regulatory benefits and favorable treatment (Hu, 2006; Luo and Liu, 2009). Such connections are crucial for navigating China's institutional landscape, where formal legal systems may not always fully protect private enterprise interests.

2.3. Correlation of the Free Trade Zone with firm performance-related indices

Although the central government unilaterally designates FTZ regions across the country and local governments has limited capacity to affect geographical delineation (Yao and Whalley, 2016). it is possible that policymakers may choose underdeveloped regions to grant preferential policies for local businesses, or they may select areas concentrated with competitive enterprises advantageous to leverage benefitting policies to showcase successful policy outcomes. Such possibilities can raise endogeneity concerns.

To mitigate potential endogeneity concerns and assess the exogeneity of FTZ establishment, we examine the correlation between a firm's subsequent designation as an FTZ beneficiary and its pre-treatment levels of performance-related indices. Specifically, we regress firms' performance variables on indicators of FTZ inclusion, spanning one to seven years (due to data restriction) prior to the introduction of local FTZ implementation waves. The performance variables include return on assets, total factor productivity (measured using both Levinsohn-Petrin and Olley-Pakes methods), exports, effective tax rate, and financial expense rate, invest, revenue, firm size (asset), and financial leverage.[8] We also include a set of fixed effects and time trends to control for unobserved heterogeneity across industries, provinces, and time periods.

The results show consistently non-significant coefficients of these firm indicators and their future inclusion into FTZ beneficiaries at the 10% level (Table A2), indicating no significant correlation between pre-existing firm performance indicators and subsequent FTZ policy implementation. These findings provide evidence to preclude the presence of systematic selection

[8] Refer to Footnotes 3 and 13 for further details on the differences between the Levinsohn-Petrin and Olley-Pakes methods.

bias in the designation of FTZ regions based on firm performance history. They support the plausibility of leveraging the FTZs as an exogenous shock to firm performance.

3. Data Construction and Summary Statistics

In this section, we explain how we construct the sample, the treatment group, and the control group, as well as measurement for key variables. We also present an overview of summary statistics.

3.1. Sample

We compile a firm-level data set covering the 1998–2022 period by combining both annual financial records of companies listed on the Shanghai Stock Exchange and Shenzhen Stock Exchange, and China industrial enterprises database from the annual surveys conducted by the National Bureau of Statistics.[9] We access profiles of chairman and CEO from firms' financial reports. The raw data set includes all 9,370 individual firms with records within the two data sources across the 21 provinces that implemented the FTZs, and 1,728 individuals in chairman or CEO positions.

Table 1 summarizes the statistics of firms in treatment and the matched control groups across various FTZ waves and provinces. Our unbalanced panel data set comprises 117,275 firm-year observations spanning 1998 to 2023.

[Insert Table 1 here]

This data set comprehensively includes firm attributes such as industry classification, ownership structure, establishment year, and financial indicators. We categorize firms between treatment and control groups according to the timing and geographic scope of each FTZ wave (The State Council Information Office of the PRC, 2023) and headquarters' locations of firms one year prior to local FTZ implementation. We exclude firms that relocated their headquarters into FTZ regions after 2012, one year prior to the announcement of the FTZ initiatives, to mitigate potential sorting issues.

[9] There are three stock exchanges in mainland China: Shanghai Stock Exchange, founded on November 26, 1990; Shenzhen Stock Exchange, founded on December 1, 1990; and Beijing Stock Exchange, founded on September 3, 2021. We exclude the Hong Kong Exchanges and Clearing Market because its accounting standards and financial reporting deadlines differ from those of the mainland stock market. We include companies in the A-share market because equities in the other B-share market do not accept Chinese *yuan* as a deposit and follow different accounting standards and financial reporting deadlines. We exclude companies listed on the newly established Beijing Exchange because it was founded after the FTZ policy was implemented, and its listed companies, with the exception of the 10 IPOs, are shifted from the existing stock exchanges' New Third Board. The China industrial enterprises database (1998–2015) includes all industrial firms that are either state-owned or non-state firms with sales exceeding CNY 5 million.

3.2. Dependent variables

We employ accounting-based metrics to assess company profitability and productivity, including return on assets (ROA), total factor productivity (TFP), and exports, along with effective tax rate (ETR) and financial expense rate (FER) to analyze potential underlying mechanisms.

We calculate the ROA by dividing net profit by total assets. For TFP estimation, we employ the Levinsohn-Petrin (2003) approach as the baseline method and the Olley-Pakes (1996) method as a robustness check. These methods require data on output, labor, capital, intermediate inputs, investment, and an exit variable, which are contained in our data set.[10]

To ensure the accuracy of average effect estimates, we follow Chen and Roth's (2024) recommendation to avoid "log-like" transformations for outcomes that are weakly positive or can be zero, such as earnings and exports. They suggest using the original outcome data to provide a more accurate interpretation of average treatment effects. Therefore, we use raw export data instead of its logarithmic transformation, allowing us to interpret the results directly in terms of percentage increases (Chen and Roth, 2024). In accounting literature, ETR is calculated per this equation:

$$ETR = \frac{TE - DTP}{EBIT},$$

where TE represents tax expense, DTP represents deferred tax provision, and $EBIT$ represents earnings from the operation of the firm's resources before payments to creditors, shareholders, and the government (Porcano, 1986).[11]

We compute financial expense rates by dividing financial expenses by total revenue.

[10] Both Levinsohn-Petrin (2003) and Olley-Pakes (1996) methods require data on output, labor, capital, intermediate inputs, investment, and an exit variable. Output is assessed by the firm's value-added, encompassing accrued payroll, cash paid to and for employees, accumulated depreciation, operating profit, taxes and fees paid, and refunds of tax and fees received. Labor is represented by the number of employees, capital by net fixed assets, intermediate inputs by cash paid for commodities or labor services, and investment by cash paid for the acquisition of fixed assets, intangible assets, and other long-term investments. Instances of bankruptcy, restructuring, or changes in stock name or main business line are considered exits from the market.

The Olley-Pakes method is used to estimate production functions while correcting for the simultaneity bias, where input choices are correlated with unobserved productivity shocks. This method relies on investment as a proxy to control for the unobserved productivity shocks. Specifically, it uses a firm's investment decision to infer its productivity. However, the method requires that firms with zero investment be excluded from the estimation, which could lead to sample selection bias.

The Levinsohn-Petrin method extends the Olley-Pakes framework by addressing the limitation of using investment as a proxy, which might not be ideal when many firms report zero investment. Instead of investment, it uses intermediate inputs (e.g., materials, energy) as a proxy for productivity. This method is advantageous because intermediate inputs are typically non-zero for most firms, reducing the risk of sample selection bias.

[11] This measure aligns with public opinion about taxable income and is considered the most accurate measurement of firm revenues in the accounting literature (Liu and Cao, 2007).

Financial expenses refer to the costs related to procuring funds for production and operational activities, including net interest expenses, foreign exchange losses, and fees charged by financial institutions.

3.3. Independent variables

We establish a variable for state ownership using the data from the 8,674 companies in the China industrial enterprises database. For the 696 listed companies, we classify companies as having state ownership if government institutions, agencies, public entities, or state-owned enterprises hold more than 5% of the shares, rank among the top ten shareholders, or appear in the list of the top ten unrestricted circulating shareholders, as disclosed in the "Shareholding Changes and Shareholders Information (*Gufen biandong ji gudong qingkuang*)" section of company annual reports. To mitigate sorting issues, a dummy variable, "state ownership," is assigned the value of 1 if the ownership structure meets this criterion in the year prior to the local FTZ implementation, and 0 otherwise. Given the significant role of the Chinese government in business, the political connections of the chairman or CEO serve as a proxy for political influence (Wu et al., 2009; Zhang et al., 2013; Luo and Liu, 2019). In cases where firms are in a leadership transition, annual reports may list both the outgoing and incoming chairperson or CEO, resulting in more than two individuals being considered in the analysis. To evaluate these political connections, we employ web crawling and natural language processing techniques to extract relevant information from company documents. Specifically, we use web crawling to collect data from the "Work Experience (*renzhi qingkuang*)" section in the "Directors, Supervisors, Senior Managers, and Employees (*dongshi, jianshi, gaoji guanli renyuan, he yuangong qingkuang*)" part of annual financial reports.

The web crawler and text analysis technique systematically identify and extract specific textual patterns regarding managerial positions and ownership structures. For state ownership, this involved parsing ownership percentages and stockholder identities. For political connections, natural language processing techniques were used to detect mentions of current or former government roles held by chairman and CEO (total of 696 listed companies and 1,728 individuals). Text mining algorithms identified key terms and phrases related to employment in central or local governments, courts, procuratorates, People's Congress, or CPPCC. Firms were then classified as having political connections if their chairman or CEO had relevant governmental experience (Faccio, 2006; Fan et al., 2007). A dummy variable, "political connections", is assigned based on the firm's political connection status one year prior to the local FTZ implementation. We also

collected a variable for "political connection levels" for robustness checks, which will be explained in more detail in Section 5.5.4. After the text analysis automatic processing, we also manually verify the extracted content.

4. Empirical Strategy

We first examine the impact of the FTZs on key performance indicators using the staggered difference-in-differences (DID) framework and then explore the heterogeneity of this impact depending on state ownership and political connections using the triple-difference approach.

4.1. The impact of Free Trade Zone on firm performance

We examine the impact of FTZ implementation on firm performance using the DID strategy, an approach commonly used in policy analysis literature (Bertrand et al., 2004; Imbens and Wooldridge, 2009). We focus on three key outcomes of firm performance: return on assets (ROA), total factor productivity (TFP), and exports. While random assignment of FTZ benefits to equivalent firms would be ideal for causal inference, it may not be practical due to potential strategic behavior by firms, such as relocating to FTZ regions, introducing state ownership, or establishing political connections to take advantage of FTZ preferential measures. To address the potential sorting issues, we categorize firms into the treatment group based on each firm's headquarters location one year prior to the local FTZ establishment, and we only include firms that did not relocate into FTZ regions after 2012 in our sample. In order to enhance comparability between the treatment and control groups, we employ two methods under the DID framework: (1) matching-based DID and (2) DID using later FTZ-beneficiary firms as controls for earlier FTZ-beneficiary ones. The specification is as follows:

$$Y_{ipdt} = \beta_0 + \beta FTZ_Treat_{it} + X_{it}{}' \cdot \gamma + \varphi_i + \tau_t + \omega_p + \pi_d + \pi_d \times \tau_t + \varepsilon_{ipt} \quad (1)$$

where the dependent variable, Y_{ipdt}, represents ROA, TFP, or exports of firm i, whose main industry category is industry d, in year t at province p. The binary variable FTZ_Treat_{it} equals 1 if company i in year t benefits from the FTZ policy and 0 otherwise. The coefficient β captures the average treatment effect of the FTZs on the three performance outcomes. The vector X_{it} includes control variables accounting for potential time-varying factors influencing firm performance, such as firm i's total assets, years since establishment, and its quadratic form.

We include firm fixed effects φ_i, residential province fixed effects ω_p, industry fixed effects π_d, and time fixed effects τ_t to control for unobservable time-invariant firm characteristics that

may influence the outcomes and time trends. We also include the interaction term of industry and time to further account for potential heterogeneity across industries and time periods. The random error term ε_{ipt} captures the idiosyncratic variation associated with each firm over time. We cluster the standard errors at the firm level to address potential serial correlation issues (Bertrand et al., 2004).

For our staggered DID model to be valid, the dependent variables—ROA, TFP, and exports—of the control firms must accurately represent the counterfactual outcomes for the treated firms, had they not benefited from the FTZs. A key challenge in achieving identification lies in ensuring the comparability between treated and control groups. For instance, firms with specific characteristics, such as those engaged in international trade, may choose to relocate their headquarters within the FTZs, thereby complicating direct comparisons.

To address this issue, we first construct a control group using matching techniques. The assumption behind this method is that non-beneficiary firms, when matched on observable characteristics, serve as a valid counterfactual for treated firms. Specifically, we assume that, after controlling for observable characteristics such as firm age, primary industry, total assets, financial leverage, state ownership, and political connections, the outcomes for the matched non-beneficiary firms reflect what would have happened to the treated firms in the absence of FTZ benefits. This approach aligns with established methods in the matching literature (Heckman et al., 1997; Abadie and Imbens, 2006; Iacus et al., 2019; Blundell and Wilson, 2023).

To implement the matching, we apply Mahalanobis distance matching, which pairs firms based on the similarity of their observable characteristics.[12] Specifically, firm j is identified as the m-th closest match to treated firm i by minimizing the Mahalanobis distance over the set of covariates, or, if it solves the following equation for the covariate set $X_{i,\bar{t}}$ of treated firm i at time $\bar{t}$, prior to the establishment of the FTZs:

$$\sum_{l \in L} 1\left\{\left\|X_{l,\bar{t}} - X_{i,\bar{t}}\right\| \leq \left\|X_{j,\bar{t}} - X_{i,\bar{t}}\right\|\right\} = m \tag{2}$$

where L is the set of non-beneficiary firms in period $\bar{t}$, l represents these non-beneficiary firms,

[12] Mahalanobis distance matching pairs units based on a distance measure known as the Mahalanobis distance—a scale-free Euclidean distance. A Mahalanobis distance of zero between two units indicates identical covariate values, while larger distances reflect greater differences in covariates. The goal is to match control units closely with treated units, ensuring that the covariates of both groups are similar. This approach improves covariate balance in the matched sample. Mahalanobis distance matching is particularly advantageous compared to propensity score matching because it directly pairs units with similar covariate values, whereas propensity score matching may not always achieve this level of closeness, even though both methods can produce balanced samples (King and Nielsen, 2019).

and the indicator function $1(\cdot)$ evaluates 1 if the condition is met.

We limit the set of potential matches to non-beneficiary firms headquartered in the same or adjacent provinces to control for geographic factors that may influence performance. By matching firms based on observable characteristics from 2012 or the closest available year prior to the FTZ implementation, we reduce selection bias and improve comparability between the treatment and control groups. Using Mahalanobis distance matching ($m = 1$), we generate a control group consisting of 4,865 non-beneficiary firms, each matched to one of the 4,865 FTZ-beneficiary firms.Columns (1) to (3) and (4) to (6) of Table 1 compare selected variables between FTZ beneficiary and matched non-beneficiary firms, with columns (7) and (8) displaying the differences. The results indicate that the matched firms closely resemble the FTZ-beneficiary firms. Furthermore, the parallel trend graphs in section 5.1 reinforce the validity of this matched control group.

Second, we adopt another staggered DID approach in which firms benefiting later from FTZ implementation are served as the control group for those benefiting earlier. This staggered design leverages the variation in the timing of FTZ establishment to estimate the causal impact by comparing firms at different time points. It assumes that, in the absence of treatment, outcomes of firms benefiting from the FTZs in the later waves reflect counterfactual trends of those of firms benefiting in the earlier waves, if the latter had not benefited in any wave. This approach allows us to examine the main effect while enhancing the comparability of treatment and control groups under the DID framework in a new perspective. Under this approach, our sample limits to the set of 4,865 firms that sequentially benefited during the sample period.

Potential bias may exist in staggered-DID average treatment effect estimates due to treatment effects in different time spots (Goodman-Bacon, 2021; Athey and Imbens, 2022; Baker et al., 2022). To address this, we conduct the Goodman-Bacon decomposition (Goodman-Bacon, 2021) as a robustness check, which decomposes the sources of variation in staggered designs and helps identify potential biases (section 5.5.1). Additionally, we perform an event study analysis, which are less susceptible to concerns associated with staggered DID treatment timing (Goodman-Bacon, 2021) to estimate the treatment effect for each year after FTZ implementation and can also detect the existence of pre-trend differences of outcomes between FTZ-benefiting firms and non-beneficiaries across time (section 5.1).

4.2. Ownership heterogeneity

Since state ownership often provides firms with greater and more accessible resources, potentially boosting their performance (Johnson and Mitton, 2003; Hsieh and Song, 2015), we incorporate firm ownership structure into our analysis to explore the heterogeneity in the effect of FTZs on firm performance associated with political power. We introduce a binary variable, $State_Ownership_i$, to denote whether firm i possesses state ownership in the year preceding the implementation of the local FTZs. We employ this time-invariant indicator to mitigate potential endogeneity concerns arising from firms altering their ownership structures in response to the implementation of the FTZs. $State_Ownership_i$ equals one if government institutions, agencies, public institutions, and state-owned enterprises hold a stake, and zero otherwise (see details in section 3.3). We modify equation (1) of the baseline setting as follows:

$$Y_{ipdt} = \beta State_Ownership_i \times FTZ_Treat_{it} + \gamma State_Ownership_i + \delta FTZ_Treat_{it} + \varphi_i + \tau_t + \omega_p + \pi_d + \pi_d \times \tau_t + \varepsilon_{ipdt} \quad (3)$$

Examining the coefficient β provides insights into the heterogeneity of FTZ impact on firm performance depending on firms' state-ownership status. Consistent signs for β and γ suggest a greater impact of the FTZs on firms with state ownership, opposite signs indicate a lesser impact, and a non-significant β suggests that the FTZs has no differential effects on firms with or without state ownership.

4.3. Political connection heterogeneity

To account for potential preference towards specific ownership characteristics in the FTZ design, we explore the influence of political connections on the policy's effects. Political connections, similar to state ownership, can provide additional advantages to enhance a company's profitability and productivity (Fisman, 2001; Faccio, 2006; Adhikari et al., 2006).

We therefore introduce another binary variable, PC_i, to denote whether firm i has political connections in the year preceding the implementation of the local FTZs (details in section 3.3). Similar to the previous section, this time-invariant indicator helps mitigate potential endogeneity concerns arising from firms changing their political connections in response to local FTZ implementation. We modify equation (1) by including the interaction term between PC_i and $Policy_Treat_{it}$, yielding the following model:

$$Y_{ipdt} = \beta PC_i \times FTZ_Treat_{it} + \gamma FTZ_Treat_{it} + \delta PC_i + \varphi_i + \tau_t + \omega_p + \pi_d + \pi_d \times \tau_t + \varepsilon_{ipdt} \quad (4)$$

Similar to section 4.2, we focus on the coefficient β and the signs for β and γ estimates. In

addition, by comparing the β estimate in equations (3) and (4), we can assess whether the FTZs is designed to facilitate firms with specific ownership characteristics. Consistent signs of the β estimate in both equations suggest that the FTZ influence on firm performance is associated with firms' political power.

5. Results

In this section, we first examine the FTZ influence on ROA, TFP, and exports using an event study with the matched control group, which helps to validate our adoption of the DID framework. We then present the results from our main estimation and policy heterogeneity analysis, employing both matched and staggered sample comprising methods. Finally, we examine the robustness of our main findings across various alternative specifications and sampling strategies.

5.1. Event study

We conduct an event study to examine the separate yearly effect of FTZ impact on firms' ROA, TFP, and export outcomes before and after the FTZ implementation. This event study examines potential pre-trend differences between FTZ beneficiaries and non-beneficiaries before the FTZ establishment, to determine the variation of yearly effect of the FTZs on firm outcomes in ROA, TFP, and exports, and to mitigate concerns on staggered DID estimates (Goodman-Bacon, 2021; more details in section 5.5.1). The event study model is as follows:

$$Y_{ipdt} = \sum_{s=-5, s\neq 0}^{5} \beta_s FTZ_Treat_{it} \times I(t = s) + \varphi_i + \tau_t + \pi_d + \omega_p \times \tau_t + \pi_d \times \tau_t + \omega_p \times \tau_t + \varepsilon_{ipdt} \quad (5)$$

Here, the coefficient β_s denotes the outcome difference in ROA, TFP, and exports, respectively, between FTZ-beneficiaries and their matched control firms in year s. We set $\beta_0 = 0$ as the baseline. We include firm, year, province, industry, and year-by-industry fixed effects, denoted by φ_i, τ_t, ω_p, π_d, $\omega_p \times \tau_t$, and $\pi_d \times \tau_t$. ε_{ipdt} represents unobservable idiosyncratic shocks to the outcome of firm i in year t.

Figure 2 illustrates the estimated coefficients of β_s for each of our three primary outcomes: ROA, TFP, and exports. Overall, the estimates obtained from our event study analysis indicate that we observe significantly positive effects of FTZs on firms' ROA, TFP, and exports. Figures 2a, 2b and 2c demonstrate the absence of a time trend prior to FTZ implementation, followed by a significant increase in ROA, TFP, and exports shortly after the policy was enacted.

[Insert Figure 2 here]

The results from the event study support the parallel trends assumption underlying our DID

models (Table A3). As depicted in Figure 2, we find non-significant difference in the pre-FTZ trends of all three outcomes between FTZ-beneficiary firms and non FTZ-beneficiaries. Moreover, the post-FTZ period treatment effects are consistently positive with the same magnitude, suggesting no potential bias in staggered-DID average treatment effect estimates (Goodman-Bacon, 2021).

5.2. Regression results

We present the baseline results of equation (1) in Table 2. Columns (1) to (3) show that the FTZs consistently significantly increases firms' ROA, TFP, and exports by an average of 1.5 percentage points (16.7% in magnitude), 3.7 percentage points, and CNY 546.8 thousand respectively, relative to controls. For firms engaged in export activities prior to benefiting from the FTZs, the difference in exports amounts to a 53.2% increase, while for all firms, it rises by 90.3%. This discrepancy in the increase in exports between different firm samples is due to the inclusion of firms that previously did not emphasize exports, but were incentivized to do more exports by the FTZ introduction, which inflates the value of the increase in exports. If only firms already oriented towards exports were considered, this value would be lower. Given the observed parallel trends depicted in section 5.1, we contend that the results effectively capture the causal impact of the FTZs on ROA, TFP, and export.

[Insert Table 2 here]

Columns (4) to (6) report the outcomes from our second DID strategy, where we narrow the sample to exclusively include firms sequentially benefiting from the FTZs. The results indicate an increase of 1.6 percentage points (17.8% in magnitude), 10.7 percentage points, and CNY 332.6 thousand (representing a 32.3% increase in exports for firms engaged in export activities prior to benefiting from the FTZs, or 54.9% for all firms) in ROA, TFP, and exports, respectively, exhibiting qualitative consistency with those obtained in our previous matching strategy analysis.

Consequently, across different models and sample groups, the results reported in Table 2 compellingly show that the FTZs indeed contribute to a substantial improvement in firms' performance indicators such as ROA, TFP, and exports.

5.3. The heterogeneous effect of firms' state ownership

The estimation results for equation (3), presented in Table 3show the heterogeneous impact of FTZs based on firms' state-ownership status. The results of the matching technique suggest a significantly greater benefit from the FTZs for firms with state ownership, with a 3.1 percentage

point increase in ROA (column 1), a 13.2 percentage point increase in TFP (column 2), and an additional CNY 2,333.5 thousand in exports (column 3), compared to firms without state ownership.

[Insert Table 3 here]

In the staggered setting, where the sample is restricted to firms sequentially benefiting from the FTZs between 2013 to 2023, the estimated FTZ effect is 2.8 percentage point increase in ROA (column 4), an 18.1 percentage point increase in TFP (column 5), and an additional CNY 2,200.4 thousand in exports (column 6), compared to firms without state ownership These findings are consistent with those in the matching setting.

5.4. The heterogenous effect of firms' political connections

Table 4 presents the estimation outcomes for equation (4). The results indicate that firms with political connections experience greater benefits from FTZ policies relative to those without political connections, showing a 1.7 percentage point higher ROA (column 1) and a 19.3 percentage point increase in TFP (column 2). In the staggered setting, where the sample is restricted to firms sequentially benefiting from the FTZs, the effect increases to a 1.9 percentage point rise in ROA (column 4) and a 20.9 percentage point rise in TFP (column 5). The consistency of results across both settings under the staggered-DID framework strengthens the validity of these findings.

[Insert Table 4 here]

It is worth noting that *for the outcome of exports*, the coefficient of the interaction term *between* FTZ treatment and *political connection dummy* is non-significant in both the matching and staggered settings (columns 3 and 6), suggesting that firms with political connections do not exhibit significantly different export behaviors compared to those without such connections. This finding is plausible, as political connections are primarily relevant to domestic resources and policy utilization, which may not directly influence export-related decisions, *largely* driven by international market factors (Khwaja and Mian, 2005; Su and Zhang, 2023).

5.5. Robustness checks

Previous sections present evidence that the FTZs positively impacts firms' ROA, TFP, and exports. To ensure the robustness of our findings, we conduct a number of robustness checks and report their results in this section.

5.5.1. Goodman-Bacon decomposition

Recent research has highlighted that staggered DID estimates may suffer from bias, due to specific comparisons between the treatment and control group at different time points, leading to concerns on issues such as negative weighting.[13] Essentially, the staggered DID estimate is a weighted average of all possible combinations of treatment and control groups across two time periods, each combination assigned a varying weight based on its timing of treatment within the sample period (Goodman-Bacon, 2021; Baker et al., 2022). In our context, this suggests that our main DID estimator, which is derived from the matching approach, is determined by comparing among firms benefiting from the FTZs at different time intervals and by comparing beneficiary firms with those that never benefited, with some comparisons being assigned higher weights than others.To validate our findings, we follow Goodman-Bacon's (2021) methodology and depict all two-by-two comparisons in Figure A1. The results in Table A4 demonstrate that both comparisons between treated firms benefiting from the FTZs at different times (row 1) and comparisons between FTZ beneficiaries and never-benefiting companies (row 2) yield DID estimates with the same direction to our primary estimates (row 4) across all three outcome variables. Moreover, these results show that comparisons between treated firms and those never treated, which take up the highest weighting, closely mirror the overall DID estimate for all three outcomes (rows 2 and 4), implying that our results are not likely influenced by outlier DID comparisons that receive disproportionate weighting (Goodman-Bacon, 2021). Additionally, the Goodman-Bacon decomposition results show that the within covariates estimate, which is identified by differences in covariates among observations with the same treatment timing, contributes little (0.2%, 1.2%, and 0.6% for ROA, TFP, and exports, respectively; row 3) to the overall estimate, suggesting that excluding this identification when we construct the overall DID estimate will produce close results to our main findings. Therefore, our main findings are robust.

5.5.2. Permutation test

We perform a permutation test to validate the DID design. By randomly shuffling the FTZ treatment variable in the sample and running baseline regressions with a thousand draws, we observe placebo coefficients at the 5th and 95th quantiles and find no significant coefficients

[13] Negative weighting can occur in staggered DID when the treatment effect varies over time or when the control group is treated in both periods. When the average treatment effect varies over time, negative weights can arise. This can happen when already-treated units are used as controls, and changes in their outcomes are subtracted. Additionally, when the control group is treated in both periods, the treatment effect at the second period is differenced out by the DID estimator, resulting in negative weights. Negative weights can lead to misleading estimates of the treatment effect, where the average effect appears negative despite the actual positive impact of the intervention (Goodman-Bacon, 2021).

(Table A5).

5.5.3. Alternative contemporary policies

We investigate the potential influence of two contemporary policies, namely the replacement of business taxes with value-added taxes and the One Belt One Road (*Yi dai yi lu*) initiative, on the effects of FTZs on firm performance.

Since January 1st, 2012, the central government has implemented a value-add tax reform (*ying gai zeng*), replacing business taxes with value-added taxes. Before then, business taxes imposed full taxation on each good and service provided by a firm, regardless of whether they were raw materials, intermediate goods, or final products. This taxation method often resulted in double taxation during the circulation of goods and provided little room for tax deduction or refund. In contrast, value-added tax is imposed on the value increment at each production stage. For general taxpayers, the actual value-added tax equals the disparity between output tax and input tax. This taxation method effectively prevents double taxation while allowing for deductions and refunds. The value-added tax reform aims to levy value-added taxes on both upstream and downstream industries, creating a comprehensive deduction chain. This reform is essentially a tax reduction policy, which can effectively alleviate the tax burden on businesses (Shanghai Municipal Tax Service, 2018).

In 2012, the value-added tax reform program was piloted in Shanghai, Beijing, Jiangsu, Anhui, Fujian, Guangdong, Tianjin, Zhejiang, and Hubei provinces, targeting industries such as transportation, modern service, and radio, film, and television. The reform gradually expanded across the country, with different industries being incorporated in stages. By 2013, the reform included broadcasting and television sectors. In 2014, it was further extended to cover railway transportation, postal services, and telecommunications. Finally, on May 1st, 2016, the value-added tax reform was fully implemented, encompassing all businesses, including construction, real estate, finance, and personal services (The State Council. 2017).

Another contemporary policy, the One Belt One Road initiative, was launched by President Xi Jinping in 2013. It aims to integrate China's economy with the global economy by investing substantially in infrastructure construction for countries along the belt and road, such as Turkey, Saudi Arabia, India, Nepal, Kazakhstan, and others, surpassing traditional free trade agreements.[14]

[14] The name "Belt and Road" draws inspiration from the ancient Silk Road, which generally refers to the trade routes across northern Eurasia. It was established during the Western Han Dynasty (202 BC – AD 8) when Zhang

This initiative seeks to enhance economic capacity and connectivity, particularly in China's western regions. Additionally, it aims to reduce socioeconomic disparities by fostering growth in less developed provinces, promoting exports, accessing natural resources, and supporting key industries (The State Council, 2015).

According to the plan authorized by the State Council in 2015, the timeline for the participation of core provinces in the One Belt One Road initiative is as follows: Inner Mongolia took the lead in 2014, followed by Qinghai, Gansu, Ningxia, and Fujian in 2015. Xinjiang, Shandong, and Sichuan were included in 2016, Chongqing, Guangxi, Hainan, and Jiangsu in 2017, and Zhejiang was included in 2018 (The State Council. 2015).

We sequentially incorporate the two programs separately and collectively to the right-hand side of baseline regressions and pinpoint the treatment and control groups to see whether these contemporary policies have an impact on the main and heterogeneous effects of the FTZs on firms' ROA, TFP, and exports. Table A6 displays the estimates, which are consistent with our main findings, indicating that the impact of the FTZs is not influenced by these contemporary policies.

5.5.4. Alternative measurements

We also explore the robustness of our primary estimates through alternative measurements. Firstly, we measure the level of political connections using an ordered qualitative variable, referred to as "political connection levels" (Luo and Liu, 2019), in order to determine whether the impact of the FTZs on firm performance differs based on the level of political connections. This variable is defined by the highest administrative position held by a firm's chairman or CEO within government institutions, Party committees, the People's Congress, or the CPPCC. It ranges from 0, indicating no administrative or political levels, to 4. a score of 1 signifies section-head level (*ke ji*) leadership or district-, county-level, or lower non-leadership (*qu xian ji*) positions. a score of 2 denotes division-head level (*chu ji*) leadership or city-level non-leadership (*shi ji*) positions a score of 3 corresponds to bureau-director level (*ting ju ji*) leadership or provincial-level non-leadership (*sheng ji*) positions. Finally, a score of 4 represents provincial-ministerial level (*sheng bu ji*)

Qian and later Ban Chao in the Eastern Han Dynasty were sent as envoys to the Western Regions. This route, starting from Luoyang (though some say Xi'an), passed through Gansu and Xinjiang, reaching Central Asia, West Asia, and connecting to Mediterranean countries. Among the goods exchanged along this vast network, Chinese silk was the most iconic, giving the "Silk Road" its name. Since the opening of this route, commercial exchanges between China, Central Asia, and Europe had been expanded rapidly. The Belt and Road Initiative borrows from this long-standing trade route concept.

leadership or national-level non-leadership (*guojia ji*) positions.

The results show that when employing this measurement, the estimates align closely with those obtained when using the binary indicator for political connections (Table A7). Notably, firms with stronger political ties tend to experience more pronounced positive effects. These findings reinforce that the influence of the policy is shaped by the political influence wielded by firms, rather than being tailored to benefit firms with specific characteristics like state ownership.

Secondly, we use the Olley-Pakes (1996) method to re-estimate TFP for each company. The results with the new measurements consistently support baseline estimation of FTZ impact on the three outcomes across different specifications and samples (columns 1 and 2, Table A8). Moreover, the FTZs demonstrates similar heterogeneous policy effects across the outcomes concerning state ownership (columns 3 and 4) and political connections (columns 5 and 6) as our baseline FTZ effect heterogeneity.

The alignment between results of these alternative measurements and those of the baseline supports the robustness of our main findings.

5.5.5. Heterogeneity across policy waves and geographical locations

The FTZs are large-scale national initiative, implemented in multiple phases and across most provinces in China. To assess whether the FTZ effect persists over implementation phases and across different regions, we analyze the impact based on geographical locations (Eastern, Central, and Western regions, each characterized by distinct socioeconomic conditions and natural environments) and the timing of policy implementation.[15] Specifically, we examine whether the main effect holds across different phases of the policy rollout, ensuring that the findings are not region- or time-specific. Here, we employ the matching DID sample setting.

The results (Figure A2) demonstrate a consistently positive FTZ effect on the three outcomes across different policy initiation points and geographical locations, supporting the robustness and validity of our main findings.

6. Mechanism

We examine two potential channels through which the FTZ policy affects firm performance. The first channel is that firms residing in FTZ regions can use lower effective tax rates than those

[15]The eastern region includes Beijing, Tianjin, Hebei, Liaoning, Jilin, Heilongjiang, Shanghai, Jiangsu, Zhejiang, Fujian, Shandong, Guangdong, and Hainan. The central region comprises Shanxi, Anhui, Jiangxi, Henan, Hubei, and Hunan. The western region consists of Inner Mongolia, Guangxi, Chongqing, Sichuan, Guizhou, Yunnan, Tibet, Shaanxi, Gansu, Qinghai, Ningxia, and Xinjiang.

outside FTZ regions, which not only boosts their profitability but also enhances their export competitiveness. The second channel relates the potential variations in financial expense rates between FTZ beneficiary and non-beneficiary firms, as the FTZs offer a set of preferential measures to facilitate firm operation and enhance financial liberalization and capital access (refer to section 2.1 for more details).

6.1. The tax channel

Governments often use taxation as a tool to regulate regional and industrial economies, a strategy also consistently employed by the CCP over the past seven decades (Adhikari et al., 2006; Wu et al., 2012). China's central government has been adjusting tax policies based on various strategic aims. Since 2002, it has decided to share corporate income taxes with local governments to promote and balance regional and industrial growth. A key example of this is the preferential tax rates applied in special economic zones (Wu and Yue, 2009). However, despite ambitious goals to comprehensively regulate and control economies, many tax regulations, especially those concerning tax rebate, deferral, and credit are relatively imprecise, vague, and lacking in detail. Sometimes, the central government only provides framework-style regulations, while local governments fail to formulate operational rules, leaving considerable ambiguity in policy implementation (Cai and Liu, 2009). Consequently, government-business relations can influence favorable tax statuses in a resilient room (Wu et al., 2012).

The FTZs have introduced specialized taxation regulations intended to benefit resident firms, but since these regulations suffer from operational ambiguity, there is room for manipulation. A clear example is found in the Shanghai FTZ tax regulations. While the central government issued a general scheme covering income tax, import-linked tax, and export tax rebate, specific criteria and benchmarks for implementation are absent. For instance, the FTZ guideline states that enterprises registered in the Shanghai FTZ may pay income tax in installments on the value-added component of their asset appraisal following asset restructuring, with a payment period of up to five years.[16] However, no specific criteria or numerical benchmarks are provided regarding the conditions under which these installment payments should be authorized, the rates applicable, or the assessment criteria for asset restructuring.

Beside elevating profit and productivity, reduced tax burdens also allow firms in FTZ regions

[16] The State Council. 2013. "Guanyu yinfa Zhongguo (Shanghai) ziyou maoyi shiyanqu zongti fangan de tongzhi (Notice on the issuance of the general scheme of the China (Shanghai) Pilot Free Trade Zone)]." September 27. http://www.gov.cn/zhengce/content/2013-09/27/content_4036.htm.

to allocate more resources toward expanding production, improving efficiency, and engaging in international markets, ultimately boosting export volumes (Zeng, 2016). This advantage is particularly important for industries highly integrated with global trade. However, the lack of precision in FTZ tax regulations, particularly regarding import value-added tax and export tax rebates, creates room for manipulation. For instance, firms can import machinery and goods with tax exemptions, and customs duties for domestic goods sold from the FTZ may be based on either the value of imported materials or inspection status (The State Council Information Office of the PRC, 2023). The vagueness in these policies—such as installment payment durations and asset appraisal methods—grants individual officials excessive discretion, which opens the door to political influence. This lack of clarity undermines the fairness of FTZ taxation, allowing politically connected firms to gain undue advantages and distorting market competition.

We therefore investigate whether firms operating within FTZ regions enjoy lower effective tax rates (ETRs) compared to those outside as a result of the FTZ implementation, and whether variations in ETRs reflect the influence of political power, given that ETRs can comprehensively measure the cumulative effects of all tax system components (U.S. Government Accountability Office, 2008). A firm's ETR is calculated by dividing income tax payments by pretax income. We employ a triple-difference method. The estimation specifications resemble equations (1) and (3), with ETR as the dependent variable and additional control variables included. These control variables, such as financial leverage and return on assets, address correlations with ETR and potential nonlinear relationships (Fernández-Rodrguez and Martnez-Arias, 2012; Wu et al., 2012).

The results in Table 5 show a consistent decreasing effect of the FTZs in firms' ETRs, on average, using the matched control group. Furthermore, the decreasing effect is more pronounced among firms with state ownership or political connections. Specifically, firms with state ownership experience an additional 5.8 percentage point reduction in ETR compared to those without state ownership (column 1), and firms with political connections observe a larger decrease of 3.3 percentage points in ETR compared to those without political connections (column 2). These reductions translate to 38.7% and 22.0% of the corporate income tax rate (15%) within the FTZ regions, respectively, a non-neglected large portion. Hence, these findings suggest that FTZs improve firm performance through reducing ETR, and highlight the role of political power in shaping the tax benefits derived by FTZ firms.

[Table 5 approximately here]

6.2. The financial expense channel

One core objective of FTZs is to cultivate a more open financial market environment, in which capital mobilization and currency exchange costs are greatly reduced (Ministry of Commerce, 2013). To achieve this goal, the FTZs are equipped with a range of incentives and operational measures, including attracting banks with foreign exchange qualifications to establish branches within the zones, relaxing foreign exchange quotas, and streamlining the approval process for financial transactions (Shanghai FTZ Administration, 2015). Policy designers envision that by adopting these measures, which resemble those of liberal financial markets, companies investing in FTZs can substantially lower financing costs and currency exchange risks, thereby attracting investment and expanding foreign trade. The decrease in financial expenses can effectively decrease firms' operational costs, thereby potentially enhancing their performance and competitiveness (Harris and Raviv, 1991). However, firms with greater political influence may gain privileged access to financing channels and secure even more favorable financial expense rates (FTRs) within FTZs (Khwaja and Mian, 2005).

Using a triple-difference method, we investigate the impact of FTZs on firms' FERs and explore impact variations based on ownership structures and political connections. The estimation models are analogous to those in section 6.1, with FER as the dependent variable.

The results presented in columns 3 and 4 of Table 5 show that, resulting from the FTZ implementation, FTZ-benefiting firms experience an average 0.5 to 2.5 percentage reduction in FER compared to non-FTZ beneficiaries (Calculate from Tables 1 and 5). More specifically, firms with state ownership experience an additional 2.3 percentage point decrease in FER than those without state ownership (column 3), and firms having political connections observe a larger decrease of 1.7 percentage points in FER compared to those without any political connections (column 4). These reductions translate to 31.2% to 44.2% of average financial tax rate among firms in our sample These results indicate that the FER serves as an underlying mechanism, and that political power shapes the distribution of FER reduction among firms benefiting from the FTZs.

Firms receiving political power may enjoy lower FERs because government connections can facilitate their access to preferential credit, favorable financing terms, and alleviate credit constraints (Khwaja and Mian, 2005; Li et al., 2008). This aligns with theoretical perspectives emphasizing political power as a valuable factor for firm growth and development (Faccio, 2006; Boubakri et al., 2012).

7. Conclusion and Discussion

This study provides robust evidence of the positive impact of China's Free Trade Zones (FTZs) on firm-level economic outcomes, particularly in enhancing return on assets, total factor productivity, and exports. However, these benefits are not distributed equally. Our findings reveal that firms with state ownership or with political connections capture a disproportionate share of the gains, highlighting the influential role of political power in shaping benefit distribution from market liberalization. The skewed allocation suggests that politically powerful firms are able to exploit legal and regulatory loopholes within the FTZ framework, securing preferential financial and tax advantages that amplify their competitive edge. Consequently, the FTZs' original objective of fostering an independent, market-driven environment is compromised, illustrating the complex interaction between market forces and political influence.

The significance of these findings extends beyond the immediate context of China's FTZs. Free Trade Zones are established to stimulate broad-based economic growth by providing an environment where firms can compete on equal footing, regardless of ownership structure or political ties. However, when firms with privileged access to political power exploit regulatory gaps, the entire premise of fair competition is undermined. This results in a distorted competitive landscape, where politically unconnected firms struggle to benefit from the intended advantages of the FTZs, ultimately stifling innovation and market efficiency. This not only impairs the policy's capacity to drive economic development but also risks eroding public trust in its effectiveness, raising concerns about whether FTZs are truly advancing their stated goals.

To address these distortions, it is essential to establish a clearer and more robust regulatory framework that limits opportunities for firms that receive political power to leverage their influence for preferential treatment. Policymakers should prioritize consistent enforcement of regulations to ensure that firms cannot exploit loopholes or manipulate the system for financial gain. By creating a more transparent and standardized legal structure, one that guarantees equal access to the benefits of FTZs regardless of political or ownership status, we can foster a more competitive and innovative business environment. This approach not only better aligns with the original goals of the FTZs but also holds broader implications for emerging markets, where political power often skews the outcomes of market reforms. A fair, well-regulated system is critical to ensuring that market liberalization initiatives genuinely promote inclusive growth and economic resilience across diverse economies.

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Figures and Tables

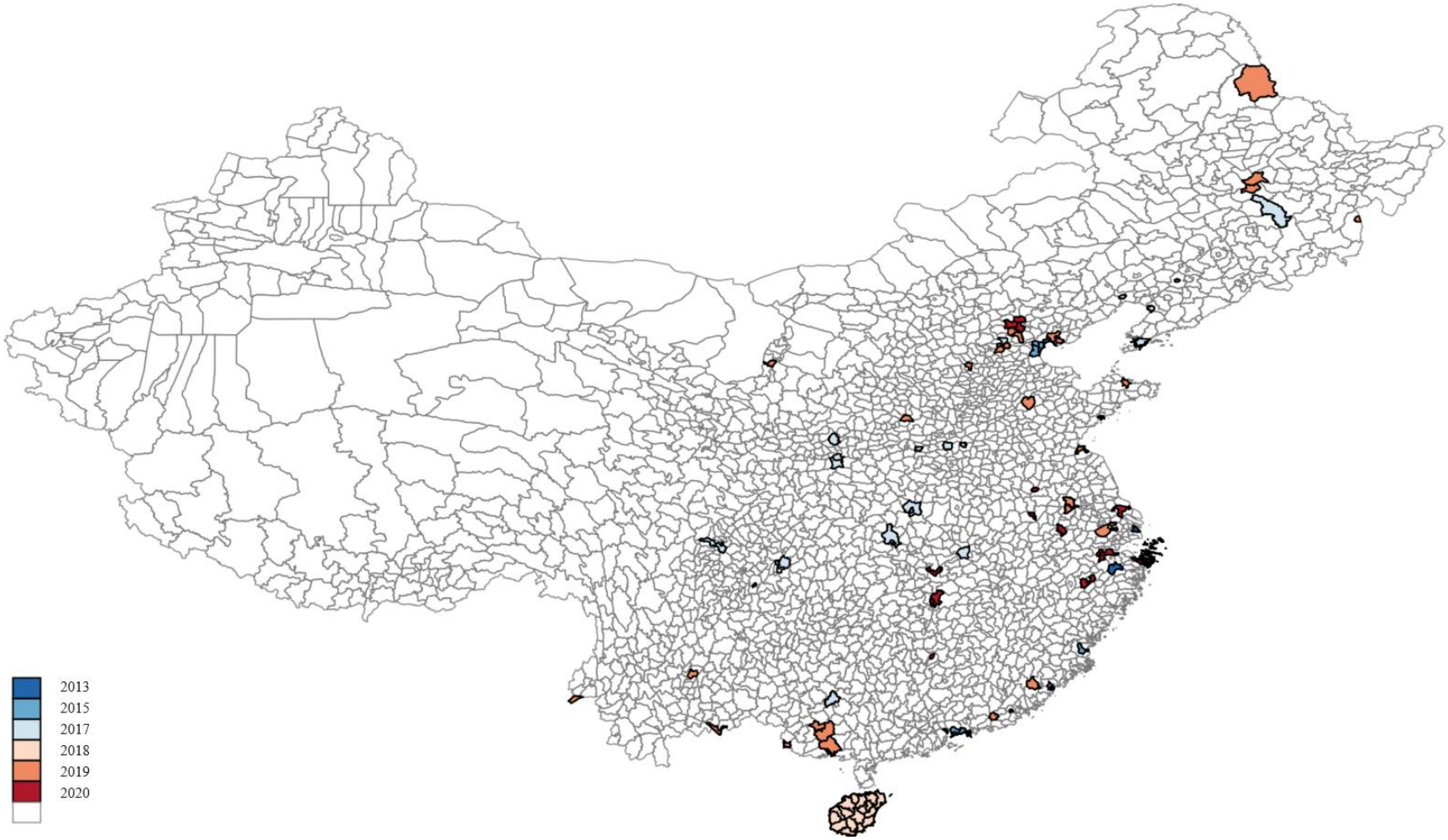
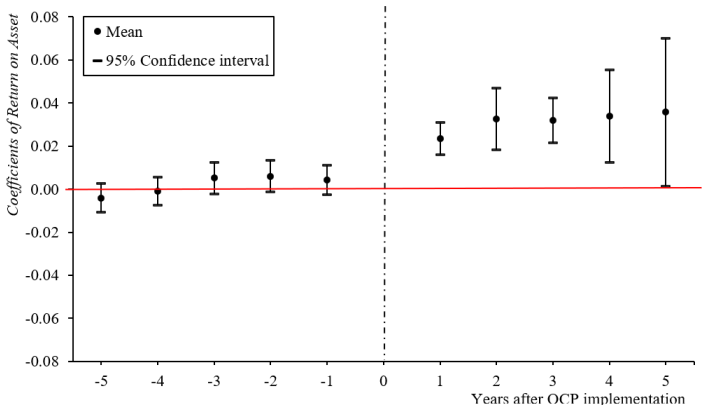
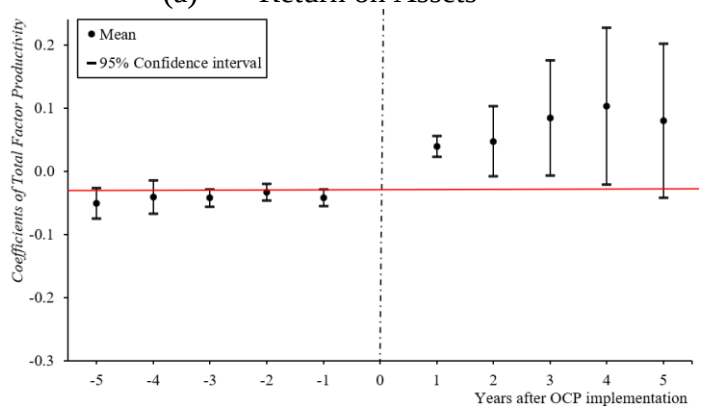


Figure 1. The Establishment of FTZs over China (2013–2020)

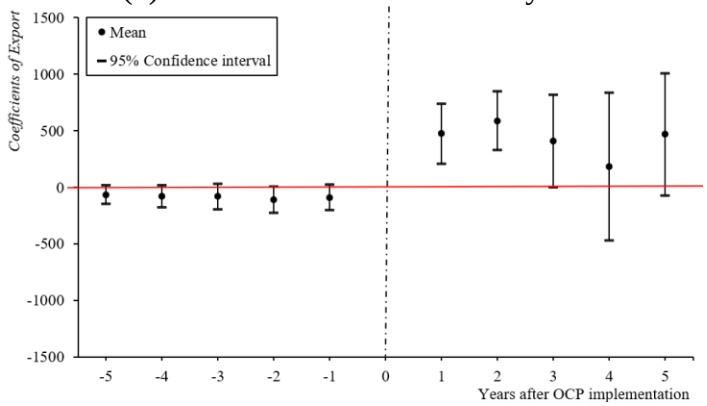
Note: The map depicts the locations of China's free trade zones established between 2013 and 2020, spanning 21 provincial-level administrative regions. The color scheme denotes the time order of their establishment. Regions marked in dark blue indicate earlier establishment, while those shaded closer to dark red represents later development stage.



(a) Return on Assets



(b) Total Factor Productivity



(c) Exports

Figure 2. Event Study Estimates of the FTZs

Note: The figure shows the DID coefficients and their corresponding 95% confidence intervals of equation 5, controlling for time, industry, and firm fixed effects. On the Y-axis are the outcome variables: (a) return on assets, (b) total factor productivity, or (c) exports. Years before and after the implementation of the FTZ policy are marked on the X-axis, of which the zero label indicates FTZ initiating year.

Table 1. Summary Statistics (Balance Test)

Variables	Treatment group			Control group (Never treated matching firms)			Difference	t-value
	Obs.	Mean	S.d.	Obs.	Mean	S.d.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variables								
Return on Asset	53,498	0.314	1.041	63,777	0.264	0.876	0.051	0.053
Total Factor Productivity (OP method)	48,653	8.313	0.965	59,282	7.874	0.831	0.439	0.491
Total Factor Productivity (LP method)	44,504	8.167	1.757	55,401	7.909	1.556	0.259	0.157
Export	46,589	1,013.632	5,293.176	55,772	238.616	2110.073	775.016	0.199
Effective Tax Rate	53,498	0.128	0.253	63,777	0.132	0.248	-0.004	-0.017
Financial Expense Rate	53,498	0.059	0.214	63,777	0.050	0.185	0.009	0.044
Independent Variables								
State-ownership	53,498	0.186	0.389	63,777	0.228	0.420	-0.042	-0.104
Political Connection (Dummy)	7,124	0.609	0.488	6,545	0.630	0.483	-0.020	-0.042
Political Connection Level	7,124	1.973	1.698	6,545	2.033	1.703	-0.061	-0.036
Others								
Age	53,460	11.212	8.391	63,737	12.317	10.267	-1.106	-0.117
Size	50,528	18.431	2.207	61,182	18.050	2.059	0.381	0.179
Revenue	52,906	1,380.432	12,035.710	63,114	992.973	8303.924	387.460	0.038
Financial leverage	53,498	1.036	2.200	63,777	0.930	1.895	0.106	0.052
Investment	50,506	337.995	3,163.029	61,164	252.724	2526.479	85.271	0.030

Notes: We compile our data set by combining the financial statement records from the Shanghai Stock Exchange and Shenzhen Stock Exchange spanning from 1998 to 2022, and the data from Chinese industrial enterprises database (1998–2015), conducted by the National Bureau of Statistics. The sample consists of 117,275 observations from 9,370 firms (4,685 in treatment group and 4,685 in matched control group). This data set is an unbalanced panel data, with information missing for some time points.

Table 2. DID Results of the FTZs

	Matching setting			Staggered setting		
	ROA	TFP	Export	ROA	TFP	Export
	(1)	(2)	(3)	(4)	(5)	(6)
FTZ	0.015***	0.037***	546.763***	0.016***	0.107***	332.668***
	(0.003)	(0.008)	(125.036)	(0.004)	(0.008)	(86.978)
Observations	111,632	107,919	97,776	50,490	48,642	43,945
R-squared	0.0553	0.355	0.156	0.0181	0.419	0.188

Note: Each column reports the coefficients of the impact of FTZ policy on firm outcomes (equation 1) using the matching technique (columns 1–3) or the sample consisting of FTZ-benefiting firms with staggered FTZ treatment (Column 4–6). The dependent variables are Return on Assets (ROA) (columns 1 and 4), Total Factor Productivity (TFP) (columns 2 and 5), and exports (columns 3 and 6). All regressions are controlled for time, firm, industry, and province fixed effects, as well as the interaction between industry and time periods, assets, years since establishment, and its quadratic term. Standard errors in parentheses are clustered at the firm level. * denotes $p < .10$; ** denotes $p < .05$; and *** denotes $p < .01$.

Table 3. DDD Results of the FTZs Interacting with State Ownership

	Matching setting			Staggered setting		
	ROA	TFP	Export	TFP	ROA	Export
	(1)	(2)	(3)	(4)	(5)	(6)
State ownership X	0.031***	0.132***	2,333.518***	0.028***	0.181***	2,200.361***
FTZ	(0.007)	(0.034)	(646.154)	(0.007)	(0.034)	(628.821)
FTZ	0.010***	0.015**	48.426	0.011***	0.079***	-77.592
	(0.004)	(0.007)	(66.104)	(0.004)	(0.008)	(68.640)
Observations	111,632	107,919	97,776	50,490	48,642	43,945
R-squared	0.0558	0.357	0.167	0.0188	0.425	0.195

Notes: Each column reports the coefficients of the heterogeneity of FTZ impact on firm outcomes regarding state ownership (equation 3) using the matching technique (columns 1–3) or the sample consisting of FTZ-benefiting firms with staggered FTZ treatment (Columns 4–6). The dependent variables are Return on Assets (ROA) (columns 1 and 4), Total Factor Productivity (TFP) (columns 2 and 5), and exports (columns 3 and 6). All regressions are controlled for time, firm, industry, and province fixed effects, as well as the interaction between industry and time periods, assets, years since establishment, and its quadratic term. Standard errors in parentheses are clustered at the firm level. * denotes $p < .10$; ** denotes $p < .05$; and *** denotes $p < .01$.

Table 4. DDD Results of the FTZs Interacting with Political Connections

	Matching setting			Staggered setting		
	ROA	TFP	Export	ROA	TFP	Export
	(1)	(2)	(3)	(4)	(5)	(6)
Political connection dummy X FTZ	0.017**	0.193**	1,602.489	0.019**	0.209**	1,845.621
	(0.008)	(0.098)	(1,493.631)	(0.008)	(0.096)	(1,537.705)
FTZ	0.060***	0.262***	261.652	0.037***	0.024	-992.808
	(0.006)	(0.074)	(1,118.708)	(0.008)	(0.071)	(753.714)
Observations	12,419	9,634	12,415	6,532	4,991	6,532
R-squared	0.0441	0.454	0.194	0.0531	0.448	0.164

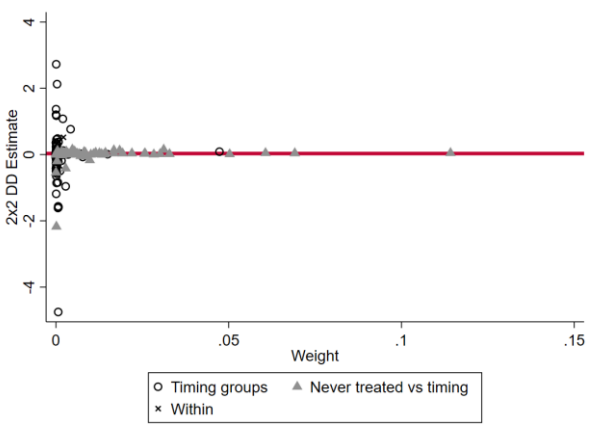
Notes: Each column reports the coefficients of the heterogeneity of FTZ impact on firm outcomes regarding political connections (equation 4) using the matching technique (columns 1–3) or the sample consisting of FTZ-benefiting firms with staggered FTZ treatment (Columns 4–6). The dependent variables are Return on Assets (ROA) (columns 1 and 4), Total Factor Productivity (TFP) (columns 2 and 5), and exports (columns 3 and 6). All regressions are controlled for time, firm, industry, and province fixed effects, as well as the interaction between industry and time periods, assets, years since establishment, and its quadratic term. Standard errors in parentheses are clustered at the firm level. * denotes $p < .10$; ** denotes $p < .05$; and *** denotes $p < .01$.

Table 5. Results of Using the Effective Tax Rate (ETR) and financial expense rate (FER) as Outcomes, Channel

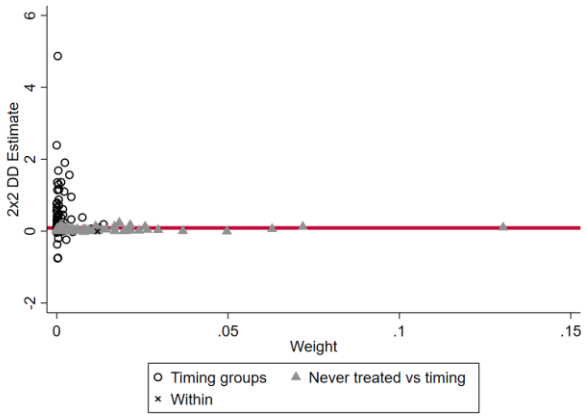
	ETR	ETR	FER	FER
	(1)	(2)	(3)	(4)
State ownership X FTZ	-0.058***		-0.023***	
	(0.010)		(0.003)	
Political connections X FTZ		-0.033*		-0.017**
		(0.018)		(0.007)
FTZ	-0.011***	-0.022	-0.001	-0.015***
	(0.004)	(0.015)	(0.001)	(0.006)
Observations	111,632	12,419	111,632	12,419
R-squared	0.0716	0.125	0.0966	0.315

Note: Each column reports the coefficients of FTZ impact on Effective Tax Rate (ETR, column 1) and Financial Expense Rates (FER, column 3) and its heterogeneity concerning political connections (column 2) and political connections (column 4), using the matching technique. All regressions are controlled for time, firm, industry, and province fixed effects, as well as the interaction between industry and time periods, assets, years since establishment, and its quadratic term. Standard errors in parentheses are clustered at the firm level. * denotes $p < .10$; ** denotes $p < .05$; and *** denotes $p < .01$.

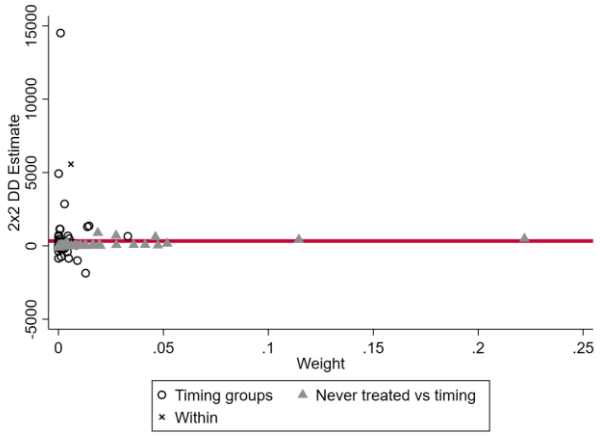
Online Appendix



(b) Return on Assets



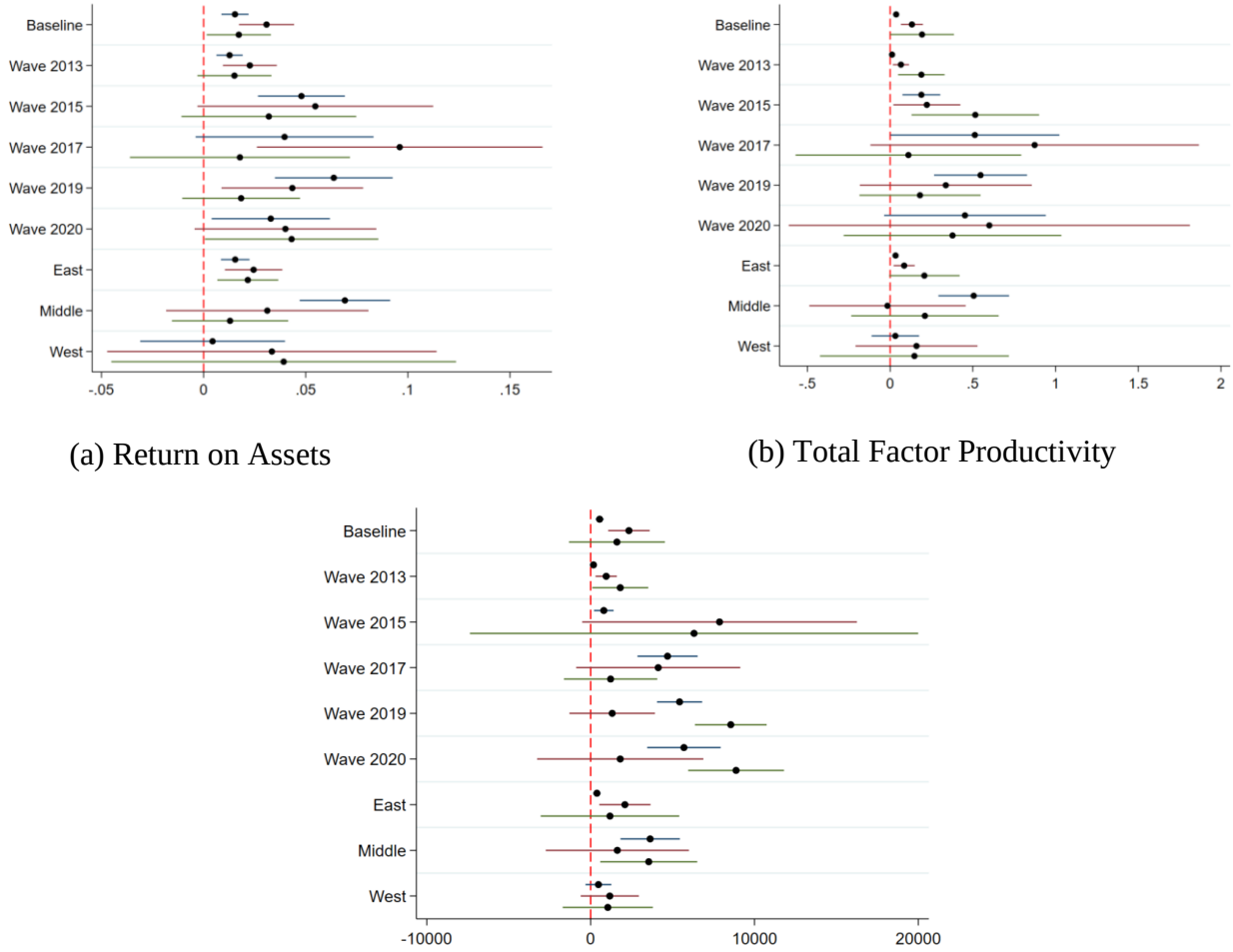
(a) Total Factor Productivity



(c) Export

Figure A1. Difference-in-differences Decomposition Robustness Check

Notes: This table displays the results of the Goodman-Bacon Decomposition. Each point represents a 2×2 DID estimate, with the x-axis representing the weights and y-axis the estimated coefficients. The red horizontal line represents the weighted estimate. Figures a, b, and c respectively show the decomposition results for Return on Assets (ROA), Total Factor Productivity (TFP), and Exports. Each estimate is divided into multiple different groups: comparisons between timing groups (circles), comparisons between the never treated group and the treated group (triangles), and comparisons where the treatment variable is the same but other covariates are different (X shapes). All regressions are controlled for time, firm, industry, and province fixed effects, as well as the interaction between industry and time periods, assets, years since establishment, and its quadratic term.



(a) Return on Assets

(b) Total Factor Productivity

(c) Exports

Figure A2. Summary of Time and Geographic Heterogeneity

Notes: This figure shows DID/DDD coefficients (represented by dots) and their 95% confidence intervals (represented by lines) from robustness estimations. Each set of labels on the vertical axis indicates a set of robustness check results for the outcome variables: return on assets, total factor productivity, or exports. Within each set, the three horizontal lines from top to bottom represent the outcomes of the matching DID regression of the impact of FTZ policy, DDD regression of the impact of FTZ policy interacting with state ownership, and DDD regression of the impact of FTZ policy interacting with political connections, respectively. These labels correspond to the robustness checks for the heterogeneity across policy waves and geographical locations outlined in section 5.5.4. All regressions are controlled for time, firm, industry, and province fixed effects, as well as the interaction between industry and time periods, assets, years since establishment, and its quadratic term.

Table A1. The Establishment of FTZs over China (2013–2020)

Date	Name	Region
1st batch: Sep. 2013	Shanghai FTZ (1st wave)	Shanghai Waigaoqiao Bonded area, Shanghai Waigaoqiao Bonded Logistics Park, Yangshan Bonded Port area and Shanghai Pudong Airport Comprehensive Bonded area
2nd batch: Apr. 2015	Tianjin FTZ	Tianjin Port area, Tianjin Airport area, Binhai New area
	Fujian FTZ	Pingtan area, Xiamen area, Fuzhou area
	Guangdong FTZ	Nansha New area of Guangzhou, Qianhai Shekou area of Shenzhen, Hengqin New area of Zhuhai
	Shanghai FTZ (2nd wave)	Lujiazui Financial area, Jinqiao Development area, Zhanghai High-tech Park
3rd batch: Apr. 2017	Liaoning FTZ	Dalian area, Shenyang area, Yingkou area
	Zhejiang FTZ (1st wave)	Zhoushan outlying island area, Zhoushan Island north area, Zhoushan Island south area
	Hubei FTZ	Wuhan area, Xiangyang area, Yichang area
	Henan FTZ	Zhengzhou area, Kaifeng area, Luoyang area
	Chongqing FTZ	Liangjiang area, Xiyong area, Guoyuan Port area.
	Sichuan FTZ	Chengdu Tianfu New area, Chengdu Qingbaijiang Railway Port area, southern Sichuan Lingang area
	Shaanxi FTZ	Central area, Xi ‘an International Port area, Yangling Demonstration area
4th batch: Oct. 2018	Hainan FTZ	The whole island of Hainan
5th batch: Jul. 2019	Shanghai FTZ (3rd wave)	Lingang New City area
6th batch: Aug. 2019	Shandong FTZ	Jinan area, Qingdao area, Yantai area
	Jiangsu FTZ	Nanjing area, Suzhou area, Lianyungang area
	Guangxi FTZ	Nanning area, Qinzhou Port area, Chongzuo area
	Heibei FTZ	Xiongan area, Zhengding area, Caofeidian area, Daxing Airport area
	Yunnan FTZ	Kunming area, Honghe area, Dehong area
	Heilongjiang FTZ	Harbin area, Heihe area, Suifenhe area
7th batch: Sep. 2020	Beijing FTZ	Science and technology innovation area, international business service area, high-end industry area
	Hunan FTZ	Changsha area, Yueyang area, Chenzhou area
	Anhui FTZ	Hefei area, Wuhu area, Bengbu area
	Zhejiang FTZ (2nd wave)	Ningbo area, Hangzhou area, Jinyi area

Note: Table A1 presents the establishment dates of China’s free trade zones, along with the corresponding provinces (or municipalities directly under the central government), prefecture-level cities (or municipalities directly under the central government) hosting the pilot free trade zones, and the specific areas where these zones are situated, as per relevant documents from the State Council of China across various years.

Table A2. P-Values for Correlations of FTZ Establishment with Firm Performance-Related Variables

Variables	Previous 1 year (1)	Previous 2 years (2)	Previous 3 years (3)	Previous 4 years (4)	Previous 5 years (5)	Previous 6 years (6)	Previous 7 years (7)
ROA	0.144	0.702	0.383	0.987	0.723	0.140	0.895
TFP, LP method	0.500	0.514	0.919	0.786	0.422	0.304	0.486
TFP, OP method	0.640	0.473	0.998	0.995	0.487	0.245	0.523
Exports	0.162	0.379	0.409	0.283	0.461	0.625	0.144
Effect Tax Rate	0.473	0.442	0.704	0.603	0.990	0.702	0.619
Financial Expense Rate	0.936	0.547	0.695	0.795	0.684	0.778	0.940
Investment	0.786	0.280	0.479	0.403	0.965	0.491	0.269
Revenue	0.181	0.668	0.897	0.788	0.313	0.937	0.453
Size	0.458	0.385	0.695	0.504	0.566	0.796	0.836
Financial Leverage	0.418	0.892	0.655	0.470	0.667	0.630	0.224

Note: We regress whether the FTZs are implemented on the firm-level variables one to seven years prior to local FTZ implementation, respectively. Standard errors are clustered at the firm level. Each cell in the table reports the corresponding p-value of the key efficient estimate.

Table A3. The Dynamic Impact of the FTZs

Quarters after implementation	ROA			TFP			Exports		
	Mean	95% Confidential interval		Mean	95% Confidential interval		Mean	95% Confidential interval	
-5	-0.0041	-0.0108	0.0026	-0.0204	-0.0444	0.0036	-64.3475	-148.7110	20.0160
-4	-0.0009	-0.0075	0.0057	-0.0107	-0.0371	0.0157	-78.7086	-175.6510	18.2338
-3	0.0052	-0.0022	0.0125	-0.0122	-0.0261	0.0017	-80.9429	-193.1087	31.2229
-2	0.0060	-0.0013	0.0134	-0.0030	-0.0161	0.0100	-109.1681	-227.5883	9.2521
-1	0.0041	-0.0027	0.0109	-0.0119	-0.0247	0.0010	-88.0988	-202.6073	26.4096
0									
1	0.0234	0.0159	0.0309	0.0695	0.0530	0.0861	474.3648	208.7180	740.0116
2	0.0326	0.0183	0.0469	0.0775	0.0226	0.1325	589.2177	331.1730	847.2624
3	0.0320	0.0215	0.0425	0.1144	0.0233	0.2056	409.1661	-0.9567	819.2888
4	0.0339	0.0124	0.0554	0.1328	0.0087	0.2570	182.9942	-469.3806	835.3689
5	0.0358	0.0014	0.0701	0.1101	-0.0116	0.2319	470.0126	-69.6093	1009.6340

Notes: This table corresponds to Figure 2. It shows the mean and the 95% confidence intervals of the separate yearly effect of the FTZs on ROA, TFP, or Exports.

Table A4. Goodman-Bacon Decomposition

Source of variation	ROA	Weight	TFP	Weight	Export	Weight
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment timing	0.002	0.127	0.350	0.106	344.825	0.155
Never treated versus treated	0.033	0.871	0.058	0.883	292.373	0.844
Within covariates	0.524	0.002	-0.004	0.012	5578.557	0.006
Overall estimate	0.030		0.088		332.901	

Note: This table presents the results of the Goodman-Bacon Decomposition, corresponding to the graphical results in Figure A1. Columns 1, 3, and 5 show the weighted coefficients of different comparison groups for outcomes of ROA, TFP, and exports, respectively. There are three group comparison categories: comparisons between timing groups, comparisons between never treated and treated groups, and comparisons where treatment variables are the same while other covariates differ. Columns 2, 4, and 6 show the corresponding weight of each group. Here, we employ the matching setting, where we decompose the sample into various possible weighted 2×2 DID estimates. We firstly enumerate all possible 2×2 DID combinations within the entire sample, separately run regressions, and then perform group weighting. All regressions are controlled for time, firm, industry, and province fixed effects, as well as the interaction between industry and time periods, assets, years since establishment, and its quadratic term. Standard errors in parentheses are clustered at the firm level.

Table A5. Permutation Placebo Test

	ROA	TFP	Export
	(1)	(2)	(3)
Panel A			
FTZ, Baseline	0.015***	0.037***	546.763***
	(0.003)	(0.008)	(125.036)
FTZ, 95th percentile placebo	0.003	0.021	78.49
	(0.040)	(0.041)	(107.36)
FTZ, 5th percentile placebo	-0.005	-0.013	-146.05
	(0.057)	(0.035)	(128.72)
Panel B			
State Ownership X FTZ, Baseline	0.031***	0.132***	2,333.518***
	(0.007)	(0.034)	(646.154)
State Ownership X FTZ, 95th percentile placebo	0.011	0.054	84.19
	(0.090)	(0.072)	(477.46)
State Ownership X FTZ, 5th percentile placebo	-0.007	-0.006	-122.23
	(0.091)	(0.075)	(551.37)
Panel C			
Political connections X FTZ, Baseline	0.017**	0.193**	1,602.489
	(0.008)	(0.098)	(1,493.631)
Political connections X FTZ, 95th percentile placebo	0.011	0.093	276.20
	(0.039)	(0.161)	(1182.95)
Political connections X FTZ, 5th percentile placebo	-0.005	-0.081	-180.57
	(0.045)	(0.072)	(1124.89)

Note: This table reports the 95th and 5th percentile permutation coefficients, along with the baseline estimates. We randomly shuffle the treatment variable to rerun the baseline regressions with 1000 draws. Panels A, B, and C display the estimate of effects of the FTZs, its interaction with state ownership, and its interaction with political connections, respectively. These permutation regression settings are identical to the baseline regression settings. Standard errors in parentheses are clustered at the firm level. * denotes $p < .10$; ** denotes $p < .05$; and *** denotes $p < .01$.

Table A6. Alternative Contemporary Policy Interpretations: Value-added Tax Reform and the One Belt One Road Initiative

	ROA	TFP	Export
	(1)	(2)	(3)
Panel A			
FTZ	0.017***	0.052***	293.208***
	(0.003)	(0.007)	(97.192)
Value-added Tax Reform	-0.009*	0.007	1,954.368***
	(0.005)	(0.037)	(223.711)
One Belt One Road Initiative	-0.004	-0.093***	422.700
	(0.004)	(0.022)	(364.433)
Panel B			
State-ownership X FTZ	0.037***	0.178***	1,705.351***
	(0.007)	(0.032)	(618.463)
Value-added Tax Reform	-0.016***	-0.042	1,633.571***
	(0.005)	(0.036)	(210.348)
One Belt One Road Initiative	-0.003	-0.092***	468.433
	(0.004)	(0.022)	(374.916)
Panel C			
Political connections X FTZ	0.016**	0.184*	1,629.139
	(0.008)	(0.097)	(1,479.922)
Value-added Tax Reform	-0.008**	0.142***	-765.353**
	(0.004)	(0.038)	(383.648)
One Belt One Road Initiative	-0.026***	-0.187***	180.594
	(0.006)	(0.056)	(519.925)

Notes: This table shows the results of robustness check in 5.5.2, where the Value-Added Tax Reform and the One Belt One Road Initiative, contemporary policies with the FTZ, are added. Panels A, B, and C sequentially display the effects of the FTZs, their interaction with state ownership, and with political connections. The outcome variables are: return on assets (ROA, Columns 1), total factor productivity (TFP, Columns 2), and exports (Column 3). All regressions are controlled for time, company, residential province fixed effects, and interactions between industries and time periods, assets, years since establishment and its quadratic term. Standard errors in parentheses are clustered at the firm level. * denotes $p < .10$; ** denotes $p < .05$; and *** denotes $p < .01$.

Table A7. Alter measurement: Standardized Political Connections level

	Matching setting			Staggered setting		
	ROA	TFP	Export	ROA	TFP	Export
	(1)	(2)	(3)	(4)	(5)	(6)
Political Connections level X FTZ	0.006*	0.088*	795.981	0.008**	0.096**	885.774
	(0.004)	(0.047)	(689.939)	(0.004)	(0.045)	(715.277)
FTZ	0.071***	0.380***	1,250.267**	0.049***	0.151***	146.429
	(0.005)	(0.044)	(489.072)	(0.007)	(0.042)	(411.827)
Observations	12,419	9,634	12,415	6,532	4,991	6,532
R-squared	0.0438	0.453	0.194	0.0528	0.447	0.163

Notes: Each column reports the coefficients from the Matching (Columns 1-3) or staggered setting DDD regression (Column 4-6) of the impact of FTZ policy interacting with standardized political connections level on firm outcomes. The dependent variables are Return on Asset (Columns 1 and 4), Total Factor Productivity (TFP, Columns 2 and 5), and export (Column 3 and 6). All regressions are controlled for time, company, residential province fixed effects, and interactions between industries and time periods, assets, years since establishment and its quadratic term. Standard errors in parentheses are clustered at the firm level. * denotes $p < .10$; ** denotes $p < .05$; and *** denotes $p < .01$.

Table A8. Alternative TFP Measurement (Using the OP Method)

	TFP-OP (1)	TFP-OP (2)	TFP-OP (3)	TFP-OP (4)	TFP-OP (5)	TFP-OP (6)
FTZ	0.213***	0.156***	0.185***	0.134***	0.239***	0.177***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.046)	(0.049)
State Ownership X FTZ			0.144***	0.123***		
			(0.040)	(0.039)		
Political connections X FTZ					0.153***	0.162***
					(0.058)	(0.059)
Observations	99,889	44,493	99,889	44,493	9,634	4,991
R-squared	0.501	0.648	0.502	0.649	0.652	0.664

Notes: Each column reports the coefficient estimates from the DID/DDD regression analyzing the effect of the FTZs on TFP using the OP method (Column 1-2), its interaction with State ownership (Column 3-4), or its interaction with political connections (Column 5-6). All regressions are controlled for time, firm, industry, and province fixed effects, as well as the interaction between industry and time periods, assets, years since establishment, and its quadratic term. Standard errors in parentheses are clustered at the firm level. * denotes $p < .10$; ** denotes $p < .05$; and *** denotes $p < .01$.