

**How do Tax Incentives Affect Investment  
and Productivity?  
Firm-Level Evidence from China**

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# How do Tax Incentives Affect Investment and Productivity? Firm-Level Evidence from China<sup>\*</sup>

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## Abstract

China initiated a critical value-added tax reform in 2004. Completed in 2009, it introduced permanent tax credit for firms' investment in fixed assets. We use a quasi-experimental design and a unique firm-level dataset covering all sizes of firms across a broad range of sectors and regions between 2005 and 2012, to test whether the reform promoted firms' investment and productivity. We estimate that on average, the reform raised investment and productivity of the treated firms relative to the control firms by 8.8 percent and 3.7 percent, respectively. We also show that the positive effects tend to be strengthened for firms with financial constraints.

**Keywords:** Tax incentives; investment; productivity; financial constraints; China

**JEL Classifications:** D22, D24, G31, G32, H25, H32

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<sup>\*</sup>This paper was previously circulated under the title "Tax Incentives, Financial Constraints, and Firm Performance: Evidence from China". We would like to thank Julie B. Cullen, Jicheng Huang, Ruixue Jia, Ben Li, Yu Liu, Jorge Martinez-Vazquez, Matthew Notowidigdo and seminar and conference participants at Zhejiang University, Xiamen University, Central University of Finance and Economics, the 2017 CCER Summer Institute in Yantai, and the 2017 IIPF Annual Congress in Tokyo for helpful comments. This research was supported by the National Natural Science Foundation of China (No.71773128; No.71533006; No.71573038) and funds for building world-class universities (disciplines) of Renmin University of China (No.17GJC001). Please address all correspondence to: Yongzheng Liu, School of Finance, Renmin University of China, Beijing, 100872 China (phone +86 10-82500501; fax +1 10-82509260).

## I. Introduction

The use of tax incentives is widespread and constantly evolving in many countries. It is used to stimulate investment and economic growth, especially when the economy is in a downturn or recession. As part of the policy responses to the global financial crisis in 2008, the Chinese authority launched the nation-wide Value-Added Tax (VAT) reform to transform its formerly production-based VAT system into a consumption-based one by exempting firms' purchases of fixed assets from the tax base. This tax incentive reform was initially implemented on a local pilot basis in certain industries in three North-eastern provinces of China in 2004. It was then expanded to the said industries in a few other provinces in 2007 and 2008 before its nation-wide adoption for all industries at the beginning of 2009.<sup>1</sup>

This reform could have large and heterogeneous effects. Under the new VAT system, the purchase of fixed assets can be deducted from the sales of the final product while calculating a firm's VAT liability. This substantially lowers the user cost of capital, generating direct tax incentives for firms to invest.<sup>2</sup> Meanwhile, by reducing the tax liability of the firms, the VAT reform brings additional cash flows to them, which may indirectly affect the investment behaviours of firms facing different levels of credit constraints. In particular, in the case of firms that find it difficult or too expensive to obtain external finance, the increment in cash flows lessens financial constraints and potentially this can lead to increases in investment (e.g., Cummins et al., 1996; Hubbard, 1998; Hassett and Hubbard, 2002; Hsieh and Parker, 2007; Guariglia et al., 2011; Chen and Guariglia, 2013; Gorodnichenko and Schnitzer, 2013). By definition, the return to the marginal investment of a constrained firm is, at least weakly, greater than the after-tax real interest rate. The VAT reform allows precisely this potentially higher productive investment, since it pulls up internal funds and therefore potentially increases the investment of constrained firms. In contrast, unconstrained firms are always able to fund investment through other means; thus, the increment in internal funds resulting from the reform may have less or no impact on the unconstrained firms' investment.

In this paper, we examine the incentive effects of the VAT reform on firms' investment and productivity and how it varies across firms with different degrees of financial constraints. By using a unique and largely unexplored panel dataset of 120,000 Chinese firms from the National Tax Survey Database<sup>3</sup> and taking advantage of the vast variation of the tax incentives treatment across firms over time, we exploit a quasi-experimental setting brought by the reform using a difference-in-differences approach, where the treatment

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<sup>1</sup>See section II and Table A1 in the Appendix of this paper for a detailed description of the reform.

<sup>2</sup>It may also induce firms to participate in more entrepreneurial risk-taking activities (e.g., Cullen and Gordon, 2007).

<sup>3</sup>To the best of our knowledge, we are among the first to explore this valuable dataset. See section III for more detailed description of the dataset and its advantages over other major firm-level datasets in China.

group is composed of firms that are eligible for claiming the VAT incentive benefits in a particular year, and the ineligible firms in the same year constitute the control group.<sup>4</sup>

Our econometric results unambiguously suggest that the investment and productivity of the firms were positively affected by the VAT reform. Relative to the control group, the reform raised investment and productivity of the treated firms by 38 percentage points and 9 percentage points, respectively. At the mean values, these findings translate into an 8.8% increase in investment and a 3.7% increase in productivity. In addition, we find that the reform led to higher increases in investment and productivity for eligible firms more likely to face financial constraints. To assess this finding, we present evidence from several measures of financial constraints faced by the firms. Finally, we show that the detected effects of the reform can be explained, at least partially, by its positive impacts on firms' cash flows, R&D expenditures, and market competition within industries.

The key challenge to our identification strategy is the possibility that the implementation of the VAT reform may be correlated with unobserved determinants of firms' performance on investment and productivity. We address this concern in three ways. First, we use an event study design to regress firms' investment (productivity) on a set of dummies going from three years before the implementation of the VAT reform to three years after. The results show that there is no increase in firms' investment (productivity) before the year in which the reform took place, while firms' investment (productivity) sharply increases right after the reform got started. Second, we conduct a series of placebo tests and find that the VAT reform has no significant impacts on ineligible investment and non-manufacturing firms' investment and productivity;<sup>5</sup> the effect of the reform tends to be higher for firms in industries with higher level of dependency on fixed assets for production; and randomly assigning (falsifying) the VAT treatment status to the firms does not predict firms' investment and productivity. Finally, the estimates of the reform are stable across alternative specifications, sample frames, and measures of investment and productivity.

Our study contributes to the literature in several significant aspects. First, we complement the literature by providing explicit evidence on the effectiveness of investment tax incentives from the world's largest developing country. While many early studies examine the effect of tax policy on investment,<sup>6</sup> the majority are based on developed countries, the main message being that tax incentives can stimulate investment but may not be cost effective (see, for example, Cummins et al., 1996; Devereux and Griffith, 1998; Desai and Goolsbee, 2004; Edgerton, 2010; Mertens and Ravn, 2012; Yagan, 2015; Maffini et

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<sup>4</sup>Eligibility of the VAT incentives varies a bit over different stages of the reform; see section II.B for a detailed description of the VAT reform.

<sup>5</sup>For non-manufacturing firms (i.e., firms in the agriculture and service sectors), investment in fixed assets is not likely to be the key and binding constraint for firm development; thus, we hypothesize that the VAT reform will have less impact on these firms.

<sup>6</sup>See Klemm (2009) and Zee et al. (2002) for in-depth reviews.

al., 2016). Studies addressing investment tax incentives in developing countries are very scarce, and the evidence on their effectiveness is even more limited.<sup>7</sup>

Second, most of the previous studies on investment tax incentives identify the policy impacts by exploring variations in the tax treatment across industries that differ significantly in terms of capital structure, depreciation schedules, life spans, and so on (e.g., Desai and Goolsbee, 2004; House and Shapiro, 2008; Edgerton, 2010; Zwick and Mahon, 2017). This setting thus gives rise to the concern that industry-specific shocks may coincide with the offer of tax incentives policies, leading to endogeneity problems (Maffini et al., 2016; Zwick and Mahon, 2017). Our analysis largely avoids this potential pitfall by taking advantage of the variation in the timing of the VAT reform within industries across different regions, as well as the variation of tax incentives treatment across different firm types (i.e., general VAT taxpayers versus small-scale VAT taxpayers) within industries and regions.<sup>8</sup>

Third, we provide direct evidence on the effect of tax incentives on firm productivity. Firm productivity is at least as vital as investment in determining aggregated economic growth, and even if tax incentives do stimulate investment, this does not automatically gauge a corresponding increase in firm productivity. Indeed, if tax incentives cause fiscal problems such as forgone revenue, which worsens other elements of the investment climate (see Zee et al. (2002) for a survey), the net effect of tax incentives on firm productivity, and thereby growth, might be negative rather than positive. Nevertheless, the current literature specifically addressing the productivity effect of tax incentives is rather limited, and it has typically only been examined as indirect consequences of changes in firms' capital investment and R&D (e.g., Auerbach, 2002; Hasset and Hubbard, 2002; Gemmell et al., 2013).<sup>9</sup>

Fourth, we explore the role of financial constraints in the nexus between tax incentives and firm performance. In recent years, a number of studies, particularly those based on stylized facts from developed countries, have argued that financial constraints impede firms' performance (e.g., Cummins et al., 1996; Hubbard, 1998; Hasset and Hubbard, 2002; Hsieh and Parker, 2007; Gorodnichenko and Schnitzer, 2013), and thus, increments

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<sup>7</sup>Shah (1995) examines the effect of tax incentives using several different approaches for a range of developing countries. Although the study holds a favourable view of certain targeted tax preferences, it concludes that, in general, tax incentives significantly erode tax revenue bases without stimulating much investment. In a more recent study, Klemm and Van Parys (2012) use a panel of African, Caribbean, and Latin American countries to explore the effects of tax incentives on foreign direct investment (FDI) and total investment. They find that tax incentives, particularly tax holidays, promote FDI flows. There is, however, no robust evidence that tax incentives have a positive impact on gross fixed capital formation or economic growth, suggesting that the induced increment in FDI may crowd out other investment.

<sup>8</sup>See section II for a detailed description of the VAT reform.

<sup>9</sup>There is, however, a related literature on the sensitivity of productivity to the corporate income tax rate. In particular, Arnold et al. (2011) study the direct effect of corporation taxation on firm productivity growth in 12 European OECD countries over the years 1996 to 2004. Gemmell et al. (2013) build on previous work to further explore how corporation taxation affects firm productivity convergence by reducing the after-tax returns to productivity-enhancing investment for small firms.

in their internal cash flows may help solve this problem. Our analysis adds to this literature by highlighting that independent of the impact of tax incentives on the user cost of capital, tax incentives affect firm performance through increasing net-of-tax cash flows and relaxing financial constraints faced by the firms.<sup>10</sup>

The most closely related work to our study is the paper by Chen et al. (2015), who also examine the effect of the same VAT reform on firms' investment. However, in addition to our dual analysis on both investment and productivity, our paper differs from theirs in several key aspects. First, we use a large and unexplored dataset that includes a large proportion of small and young firms.<sup>11</sup> This improves the representativeness of our overall sample and is particularly relevant for studying the issue of financial constraints. Second, we estimate the effect of the VAT reform using data covering all stages of its implementation. However, given their data limitations, Chen et al. (2015) analyze only the initial stage of the reform in 2004, which involved some pilot industries in the three North-eastern provinces of China.<sup>12</sup> Thus, their estimates may not represent the real impacts of the reform on the whole nation. Third, our dataset contains much richer information (including ineligible investment and a full range of industries), which enables us to conduct valuable placebo tests for checking the validity of our empirical strategy. Fourth, we explore the heterogeneous effect of the reform by highlighting the role of financial constraints. Lastly, we shed some light on the possible mechanisms of the impacts of the reform.

The rest of the paper is organized as follows. Section II briefly introduces the institutional background of the VAT in China and provides details of the evolution of the examined VAT reform. Section III describes the data. Section IV presents the empirical method, measures of key variables, main results, placebo tests, and robustness checks. Section V explores the role of financial constraints. Section VI discusses potential mechanisms, and Section VII concludes.

## II. Institutional Background

### A. *The Value-Added Tax in China*

China's VAT was introduced on an experimental basis in the early 1980s in selected industries of some provinces, and it was adopted nationwide for all industries in 1994, the year the current tax sharing system was established. An important feature of the system

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<sup>10</sup>In this regard, our work also provides empirical support to Keuschnig and Ribi's (2013) theoretical prediction that taxes, by eroding cash flows and pledgeable income, tighten financing constraints and reduce investment independent of their effects on the user cost of capital.

<sup>11</sup>Chen et al. (2015) use data from the Annual Survey of Industrial Firms (ASIF) conducted by the National Bureau of Statistics of China, which only contains data on large manufacturing and mining firms with annual sales of five million Yuan (about 650,000 US Dollars) or more.

<sup>12</sup>Chen et al. (2015) use data from the ASIF dataset, which only contains data up to 2007.

is that it defines two categories of VAT taxpayers. The first type is the so-called “*general VAT taxpayers*” having an annual turnover of a certain amount.<sup>13</sup> For the general VAT taxpayers, the tax base is the usual value-added, that is, the difference between the total value of sales and the cost of purchased material inputs. The other type is what we call the “*small-scale VAT taxpayers*”, for whom total sales are generally small in scale. The tax base of the small-scale VAT taxpayers is simply the total value of sales without deducting the cost of material inputs. That is, the tax regime does not allow for any deductions of the cost of material inputs while calculating a small-scale VAT taxpayer’s VAT liability. Additionally, these two types of VAT taxpayers are characterised by different tax rate settings. The standard VAT rate for the general VAT taxpayers is 17%, together with a reduced tax rate of 13% for some special products such as agricultural products.<sup>14</sup> As a comparison, the standard tax rate for small-scale VAT taxpayers is either 6% or 4%, depending on the sector involved.

Since its establishment across the nation in 1994, VAT has become the most important source of revenue for the Chinese government. As depicted in Appendix Figure A1, the revenue from VAT was 230.83 billion Yuan in 1994,<sup>15</sup> accounting for 45% of total tax revenue, and it increased steadily thereafter, reaching 3,110.95 billion Yuan in 2015, accounting for 25% of total tax revenue. Notably, this figure is still much higher than for the other taxes collected, such as corporate income tax.

Nevertheless, unlike many countries’ practice of establishing a *consumption-based* VAT system in which all purchases of capital goods from other firms are deductible from the sales of the final product while calculating a firm’s VAT liability, China’s VAT system was initially designed as a *production-based* tax. Thus, purchases of capital goods were not allowed to be deducted from the VAT bases, resulting in the double taxation on capital goods.<sup>16</sup> The selection of this type of VAT system was a result of the central government’s action aimed at dealing with two primary concerns for the Chinese economy in the early 1990s—the continuing decline of the share of government revenue in GDP, and the continuing increase in the inflation rate. It was, thus, hoped that establishing a production-based VAT system will help preserve government tax revenue and restrain firms’ investment in a then overheating economy (Chen et al., 2015). After a decade of its implementation and other structural reforms to the tax system since 1994, government revenue increased dramatically, strengthening the controls of the central government over

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<sup>13</sup>More specifically, to be recognized as a general VAT taxpayer, the annual turnover must exceed (a) 1 million Yuan (0.5 million Yuan after 2008) if the taxpayer is engaged in production/manufacturing activities, (b) 1.8 million Yuan (0.8 million Yuan after 2008) if the taxpayer is engaged in wholesaling and retailing activities, or (c) 5 million Yuan if the taxpayer is engaged in taxable service activities.

<sup>14</sup>Meanwhile, for encouraging export activities, exporting firms were given VAT refunds at rates ranging from 9% to 17%.

<sup>15</sup>Because of data availability, we only consider domestic VAT here.

<sup>16</sup>The first is as final products of their producers, and the second is as intermediate inputs for their users.

the fiscal system. However, the system started to receive an increasing number of critiques in that the production-based VAT system was creating large investment disincentives, causing firms to operate worn equipment and obsolete technology, hence allowing low productivity to persist. This finally led to the first substantial reform on VAT, starting in 2004 and lasting till 2009.

### B. The Policy Reform

The main objective of the reform was to transform the existing *production-based* VAT system to a *consumption-based* one by allowing deduction of the costs for purchasing capital goods when calculating the VAT bases. By doing so, it is hoped that the reform will reduce the user costs of capital and stimulate investment on fixed assets that should be beneficial for accelerating technology progress and improving firm productivity.

The reform was initially implemented on a local pilot basis for general VAT taxpayers in eight manufacturing industries in three North-eastern provinces in July 2004 (i.e., the first stage of the reform).<sup>17</sup> For these pilot industries,<sup>18</sup> all costs related to the purchase, installation, and transportation of fixed investment in equipment were allowed to be claimed as credits against the total value of sales while calculating a firm's VAT liability, which, by its very nature, would substantially lower the cost of fixed investment on equipment by 13% to 17%.<sup>19</sup> In the meantime, to prevent a sharp decline in government revenue, the claim of a VAT credit was restricted to no more than the increment in a firm's VAT liability in the same year, while the remaining credits were allowed to be carried forward to future years.

The second stage of the reform, starting July 2007, was expanded to another 26 cities of six middle provinces,<sup>20</sup> in which general VAT taxpayers in eight industries (not completely the same as those in the first stage of the reform) were included.<sup>21</sup>

In July 2008, the third stage of the reform was initiated to cover general VAT taxpayers in eight industries in another five cities of Inner Mongolia,<sup>22</sup> and nearly all manufacturing industries in 51 counties suffering from the aftermath of the *Wenchuan* earthquake in

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<sup>17</sup>The three North-eastern provinces are Liaoning, Jilin, and Heilongjiang. For details, see Table A1 in the Appendix.

<sup>18</sup>These eight piloted industries include: (1) machine and equipment manufacturing, (2) petroleum, chemical, and pharmaceutical manufacturing, (3) ferrous and non-ferrous metallurgy, (4) agricultural product processing, (5) shipbuilding, (6) automobile manufacturing, (7) selected military products, and (8) selected hi-tech products.

<sup>19</sup>The labor costs associated with transportation and installation are also deductible, but this is only limited to those services that are directly provided by the firms selling the capital goods.

<sup>20</sup>These provinces are Shanxi, Anhui, Jiangxi, Henan, Hubei, and Hunan.

<sup>21</sup>These industries are: (1) machine and equipment manufacturing, (2) petroleum, chemical, and pharmaceutical manufacturing, (3) ferrous and non-ferrous metallurgy, (4) automobile manufacturing, (5) agricultural product processing, (6) electric power, (7) mining, and (8) hi-tech production.

<sup>22</sup>These cities are Hulunbuir, Xingan, Tongliao, Chifeng, and Xilingele. The eight reformed industries are the same as those in the first stage of the reform.

2008.<sup>23</sup>

Finally, in the face of the external shocks from the 2008 financial crisis, the Chinese central government decided to fully extend the reform to general VAT taxpayers in all industries across the nation. As a consequence, the reform entered its last stage starting January 2009, covering all industries in the country. For further details about the reform, see Table A1 in the Appendix of this paper.

It is notable that the VAT reform *only* refers to the general VAT taxpayers, who face higher tax rates but simultaneously have the privilege to deduct the costs of purchased material inputs while calculating the tax bases. By contrast, since the tax regime does not allow for any deductions for the small-scale VAT taxpayers, they are not affected in any aspect throughout all stages of the reform. Therefore, in the subsequent difference-in-differences analytical framework, the treatment group for the years 2005 to 2008 consists of general VAT taxpayers in a specific industry–region–year, while the control group is composed of those general VAT taxpayers not included in the reform in a specific industry–region–year and all small-scale VAT taxpayers across the nation. For the period after January 2009, the treatment group includes all general VAT taxpayers across the nation, while the control group simply comprises all the small-scale VAT taxpayers.<sup>24</sup>

A last point worth highlighting here is that while the reform was applied to general investment in capital goods, two specific types of investment were excluded. That is, investment in (factory) buildings and purchases of imported capital goods were not allowed to be claimed as credits against a firm’s VAT liability. The reason for this treatment on investment in buildings was that building services suppliers are not considered as VAT taxpayers in China<sup>25</sup> and so investment in buildings do not generate the VAT invoices that are necessary for claiming the tax deductions. For a different reason, imported capital goods are exempted from paying VAT on imports in China and so no further tax incentives can be applied to these capital goods. These two types of ineligible investment, thus, provide good sources for conducting placebo tests to our main results.<sup>26</sup>

### III. Data

We use firm-level data originating from the National Tax Survey Database (NTSD), which is a unique, comprehensive, and largely unexplored database. The data are jointly collected by the State Administration of Taxation of China and the Ministry of Finance

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<sup>23</sup>The 51 counties affected by the earthquake are located in the provinces of Sichuan, Gansu, and Shaanxi. Industries subject to specific regulations/national policies, such as coke processing and electrolytic aluminium production, were exempt.

<sup>24</sup>As highlighted in the Introduction, one of the novel features of our paper is that we focus on all the stages of the reform; this topic has not been previously examined in such a manner.

<sup>25</sup>It was only recently in May 2016 that the building industry was transformed from paying business taxes to VAT nationally. Unlike VAT, business tax is not deductible against onward sales to other businesses.

<sup>26</sup>These placebo tests can be found in subsection IV.E.

of China (SAT-MOF) based on stratified random sampling and using a special electronic system. The purposes of the dataset are for the Chinese authority to better control for tax base information and evaluate the impacts of tax policies, which, by its very nature, leads to broad coverage for all sectors across all regions in the nation and ensures the accuracy of the essential information on background, taxation, financing, and performance of the firms.<sup>27</sup>

The NTSD was initiated in 1985; however, for consistency with regard to the data sampling methods and the variables covered, we only analyze the data from 2005 to 2012.<sup>28</sup> Each year the SAT-MOF exports all taxpayers' information from the tax return system, and based on it, produces a stratified sample of surveyed taxpayers (firms).<sup>29</sup> Stratification occurs by total sales, industry, and types of taxpayers.<sup>30</sup> After that, the surveyed firms are asked to log in to a specially designed electronic system within a period of time to complete the survey online. Meanwhile, local tax agencies in charge of the firms are required to ensure the completion and quality of the survey.<sup>31, 32</sup>

In this study, approximately 120,000 surveyed firms (annually) from a broad array of industries and regions across the whole nation are included in the sample that we used, accounting for about 10% of total outputs and tax revenues of China. The dataset is valuable and unique for its several features. First, since the dataset is jointly collected by the

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<sup>27</sup>The NTSD provides a wide range of information on firms' activities, including basic firm characteristics, very detailed and disaggregated tax information for 17 (out of 18) tax instruments, and hundreds of financial variables listed in the main accounting statements of these firms.

<sup>28</sup>A consistent data sampling method was first used in 2005, and we can access the data from the database till 2012.

<sup>29</sup>Practically, the NTSD uses all actual taxpayers in the current year as the population of the surveyed sample, with only self-employed entrepreneurs and new start-up firms in the current surveyed year excluded.

<sup>30</sup>Depending on the industries where the firms operate and the natures of their businesses, firms are broadly categorized as VAT taxpayers or business tax taxpayers in China. In general, companies providing services in China are subject to business tax, which is a sales tax on the price on the service.

<sup>31</sup>Although the NTSD appears as a survey dataset, it is actually a mandatory task for the firms to complete the survey. This is because, by its very nature, the data collection is officially enforced by local tax agencies in charge, which in turn are given the right by laws to collect information relating to taxes and other financial conditions (see Articles 20, 25 and 60 of *Law of the People's Republic of China on the Administration of Tax Collection* (2015 Amendment)). Equally important, the SAT-MOF uses enforcement of the survey as one of the indicators for evaluating the annual performance of local tax agencies, thus strengthening the incentives for local tax agencies to enforce the implementation of the survey.

<sup>32</sup>Relatively speaking, the NTSD is less vulnerable to misreporting issues for several reasons. First, the electronic system has several internal (built-in) functions that automatically check whether the information filled in by the firms is consistent across some relevant variables and whether the required information has been completely filled in by the firms. Second, before the submission of the survey forms to the final NTSD, local tax agencies usually perform additional checks by comparing the survey data filled in by the firms with the relevant data in their tax return files (*nashui shenbao biao*), which substantially increases the risk of the firms for misreporting information, especially for taxes and tax-related information. Third, the variables we used (total assets, material inputs, employment, and others) are mostly basic information from the Chart of Accounts of the firms, which are least likely to suffer from measurement errors (e.g., Brandt et al., forthcoming). Finally, as we highlight in the last paragraph of this section, we restrict our sample in some other aspects to further reduce the potential misreporting issues.

SAT-MOF with the objectives of better controlling tax base information and evaluating the impacts of tax policies, the accuracy of essential information on taxation, financing, and firms' performance is much enhanced. Second, the dataset has a large sample size and contains a significant number of small and young firms that are particularly likely to be hit by financial constraints, and are thus especially relevant for the study of how the presence of financial constraints may affect the net impacts of the VAT reform. This feature of the dataset also makes it superior to other major firm-level datasets in China such as the Annual Survey of Industrial Firms (ASIF) conducted by the National Bureau of Statistics of China, which only contains data on large firms with annual sales of five million Yuan (about 650,000 US Dollars) or more.<sup>33</sup> Third, the dataset covers a full range of sectors (including agriculture, mining, manufacturing, building, and service sectors) in the economy,<sup>34</sup> again making it more representative than other major datasets such as the ASIF that contain only information on manufacturing and mining firms. Lastly, the dataset spans a long time period (up to 2012), which enable us to explore the effects of the VAT reform over the different stages of its implementation.<sup>35</sup>

Our working sample is further cleaned and derived by imposing the following restrictions. First, to make the industry codes comparable across the entire period, we harmonize the industry classification codes before and after 2011, the year in which the modified classification system was introduced.<sup>36</sup> Second, we drop as outliers those firms with zero employees, negative total assets, negative fixed assets, and negative outputs. Third, we winsorize the upper and lower one percentiles of the distribution of variables included in the regressions to avoid misreporting. Finally, imposing these restrictions gives us a sample of 582,615 firms for empirical analysis, of which 280,741 are manufacturing firms and 301,874 are non-manufacturing firms. For the benchmark analysis in this paper, we focus only on manufacturing firms, as the VAT reform is mainly targeted at this sector. We rely on the sample of non-manufacturing firms for the placebo tests. Table A2 in the Appendix provides detailed definitions of the variables, and the summary statistics for the manufacturing sample are given in Table 1.

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<sup>33</sup>Researchers who have used the ASIF dataset include Hsieh and Klenow (2009), Guariglia et al. (2011), Brandt et al. (2012), Brandt et al. (forthcoming), and many others.

<sup>34</sup>During 2005-2012, there are totally 913 four-digit industries in China, only seven of which are not covered by the NTSD. These seven four-digit industries are 0340 (animal hunting), 9032 (archives), 9422 (foreign affairs), 9431 (courts), 9432 (procuratorates), 9520 (political parties), and 9612 (Women's Federation).

<sup>35</sup>Again, this is superior to the ASIF dataset, which only contains consistent data up to 2007.

<sup>36</sup>We convert the new industry classification codes after 2011 to that the year before.

## IV. The Effect of VAT Reform on Firm Investment and Productivity

### A. Empirical Method

We have highlighted in the institutional background that our identification varies a bit over different stages of the reform. In the first three stages of the reform, identification builds upon the fact that the VAT incentives were eligible to general VAT taxpayers in certain industries in selected regions, while they were not available to either, other general VAT taxpayers belonging to ineligible industries and regions or to all small-scale VAT taxpayers.<sup>37</sup> However, in the last stage of the reform, which extends the largest expansion of the reform, identification relies solely on the variation across firm types (but not any regions or industries), with general VAT taxpayers in all industries being eligible to the incentives, but not the small-scale VAT taxpayers. To capture the key of the identification throughout, we conduct a difference-in-differences (DID) estimation or, more generally, a fixed-effects model of the form,

$$y_{it} = \alpha + \beta VAT_{it} + \gamma \mathbf{X}_{it} + \mu_i + \psi_t + \varepsilon_{it} \quad (1)$$

where  $y_{it}$  is the investment or productivity of firm  $i$  in year  $t$ ;<sup>38</sup>  $VAT_{it}$  is a dummy variable indicating the implementation of the VAT reform for firm  $i$  in year  $t$ . That is,  $VAT_{it}$  equals 0 for the years before the VAT reform was introduced for a firm, and 1 for the first year and all the subsequent years of the VAT reform;<sup>39</sup>  $\mathbf{X}_{it}$  is a set of time-varying controls at the firm level;  $\mu_i$  are firm fixed effects and  $\psi_t$  are year fixed effects. By adding firm fixed effects, we control for the mean differences in investment and productivity across firms; the year dummies capture common shocks to activities across all firms. Since industries and types of VAT taxpayers are important identifiers of eligibility for the VAT incentives, we also control for time trends for two-digit industries and types of VAT taxpayers to isolate the possible confounding fact that the VAT reform may be enacted in response to these trends. In all regressions the standard errors are clustered by firm.<sup>40</sup>

Our identifying assumption is that in the absence of the VAT reform the control firms would have similar trends to the treated firms. While we cannot directly test this assumption, we can check for whether the time trends in the control and treated firms

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<sup>37</sup>These small-scale VAT taxpayers include those firms belonging to the same industry-region as the treated firms.

<sup>38</sup>See a detailed discussion on the measurements of firms' investment and productivity in the next subsection.

<sup>39</sup>We use the detailed information regarding the VAT reform that we described in section II and Appendix Table A1 to identify the treatment status for each firm in each year while creating this dummy variable.

<sup>40</sup>We present results clustered by firm, which is consistent with vast existing literature (e.g., Desai and Goolsbee, 2004; Edgerton, 2010; Yagan, 2015; Zwick and Mahon, 2017). Nevertheless, we have also explored alternative level of clustering, including province level, industry level, and two-way clustering at the province and the industry level. Our results are largely robust to all these alternative choices and the corresponding results are reported in Panel A, Table A3 of the Appendix.

were the same in the pre-reform period. If this is indeed the case, then it is likely that they would have been the same in the post-reform period if the treated firms were not eligible for the VAT incentives. To explore the validity of this assumption, we extend the DID analysis to an event-time specification (Jacobson et al., 1993). In practice, we re-estimate specification (1), but in place of  $VAT_{it}$ , we include a series of year-wise dummies for the years leading up to a firm’s eligibility of the VAT incentives, the year of eligibility, and the years after eligibility. That is, we consider the following specification,

$$y_{it} = \alpha + \beta_k \sum_{k \geq -3}^{3+} D_{t_{i0}}^k + \gamma \mathbf{X}_{it} + \mu_i + \psi_t + \varepsilon_{it} \quad (2)$$

where  $D_{t_{i0}}^k$  is a series of “event-time” dummies that equal one when the VAT reform is  $k$  years away in a firm. In particular,  $t_{i0}$  denotes the year when firm became eligible to the VAT incentives;  $D_{t_{i0}}^k$  is a series of dummies indicating whether  $t - t_{i0} = k$ , with  $k = -3, -2, -1, 0, 1, 2, 3+$ .<sup>41</sup> The omitted time category is  $k \leq -4$ , so that the estimated effects  $\beta_k$  are relative to the period of four years and more prior to the start of the reform. By focusing on the lead variables (i.e., the year dummies leading up to the reform), this specification provides us an opportunity to check whether there is any evidence of pre-existing trends, which in turn serves as an effective way to verify whether our main results suffer from selection bias. In addition, the lags of the reform (i.e., the year dummies after the reform) identify heterogeneity in the reform’s effect.

## B. Variables

*Investment.* In the literature, two methods have usually been employed to measure firms’ investment: the logarithm of real gross investment in fixed assets and the ratio of real gross investment in fixed assets to lagged real capital stock.<sup>42</sup> While we report results from both measures, we follow Zwick and Mahon (2017) to rely on the former method as the primary measure of firms’ investment.<sup>43</sup> This is mainly due to the reason that the ratio method requires information on the scaling variable (i.e., lagged real capital stock), which would reduce our sample frame, especially for small firms.<sup>44</sup> In addition, measuring the investment ratio involves the construction of a series of real capital stock for the firms, which is challenging and vulnerable to measurement error. Specifically,

<sup>41</sup>Note that the covered period for our analysis is 2005-2012, and the year of the VAT reform ranges from 2004 to 2009, depending on the stages of the reform. Event-time dummies that are more than three years after the reform are grouped and denoted as 3+.

<sup>42</sup>See, for example, Hsieh and Parker (2007), Poncet et al. (2010), Arnold et al. (2011), Chen et al. (2015), and Liu and Lu (2015).

<sup>43</sup>Results from using investment scaled by lagged capital stock as the dependent variable are reported in subsection IV.F as robustness checks.

<sup>44</sup>Here, we share the same problem as that of Zwick and Mahon (2017): for one, small firms are not always required to report balance sheet information including scaling variable; for another, requiring two consecutive years of data to obtain lagged values further reduces our sample.

firms' original accounting statements only report information on the value of their fixed assets at the original purchase prices. In other words, the gross fixed/total assets stock in our dataset is simply the sum of the values of all fixed assets still in use measured at their respective original purchase prices. The direct use of these nominal values, thus, runs the risk of introducing systematic biases related to a firm's age (Brandt et al., 2012).

To obtain the real values of capital stocks, we follow Brandt et al.'s (2012) approach to reconstruct the entire series of fixed investment and depreciation using the perpetual inventory method and make assumptions about investment growth rate and depreciation rate.<sup>45</sup> The potential measurement error in the calculation of real capital stock just transforms into measurement error in the investment ratio.

Finally, it is noted that data on the gross investment in fixed assets are reported directly by the NTSD database and we deflate them by the province-level fixed assets investment price index to obtain real values.<sup>46</sup>

*Total Factor Productivity.* In what follows, we discuss the measurement of firm productivity (i.e., TFP) based on the estimation approach. For illustration purposes, let us consider a standard Cobb–Douglas production function in the form of,

$$\ln Y_{it}^j = \alpha + \beta \ln K_{it}^j + \gamma \ln L_{it}^j + \delta \ln M_{it}^j + \varepsilon_{it} \quad (3)$$

where  $Y_{it}^j$ ,  $K_{it}^j$ ,  $L_{it}^j$ , and  $M_{it}^j$  refer to firm  $i$ 's output, capital stock, labor, and materials in industry  $j$  in year  $t$ , respectively.<sup>47</sup> Traditionally, the simplest benchmark of TFP is the Solow residual from the ordinary least squares (OLS) regression for specification (3), which is usually run industry-by-industry<sup>48</sup> and based on firm-level data. That is,

$$TFP_{it} = \ln Y_{it}^j - \ln \hat{Y}_{it}^j \quad (4)$$

However, the OLS estimates of specification (3) are likely to be inconsistent and biased, largely due to the possible existence of simultaneity and selection biases. The simultaneity bias may arise because of the correlation between unobserved productivity and firms' input decisions. For instance, firms with higher productivity tend to hire more workers because of higher current and anticipated future profitability; since productivity is unobserved, it enters the error term, introducing a positive correlation between the error term and labor input, and rendering the OLS estimates inconsistent and biased.<sup>49</sup>

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<sup>45</sup>The detailed procedure for the calculation of real capital stock is explained in Appendix B.

<sup>46</sup>Given the existence of zero investment for some firms, our primary measure of investment takes the form of  $\log(1+\text{real investment})$ . This approach has been used in many of the previous literature (see, for example, Liu and Lu (2015)).

<sup>47</sup>All the values are in real terms. Specifically, the nominal values of total output are deflated using industry-specific output price indices from the National Bureau of Statistics of China. Data on real capital stock are explained in Appendix B. Material input deflators are calculated using the output deflators and information from the 2002 National Input–Output (IO) table.

<sup>48</sup>This allows each industry to have a different production function.

<sup>49</sup>Fixed effect estimators can only partially solve the problem since they can only address the bias due

In addition, firms with low productivity are more likely to collapse and exit from the market, leading to a selection bias in the sample.

To address these biases, Olley and Pakes (1996) propose a semi-parametric approach, in which they add investment as a proxy for unobserved TFP to the specification (denoted as ‘the OP approach’). However, later evidence from the firm-level datasets suggests that investment is a ‘lumpy’ variable,<sup>50</sup> and thus, it may not be a strong proxy for productivity shocks. Levinsohn and Petrin (2003) take a similar approach, but instead of using investment, they employ intermediate inputs (such as raw materials, electricity, or fuels) as a proxy for the unobserved productivity shocks, thus limiting the lumpy investment problem (denoted as ‘the LP approach’). Nevertheless, the LP approach controls only for simultaneity bias but not selection bias. In light of the relative advantages of both approaches, we employ the OP approach as the primary measure of firms’ TFP in this paper, and use the LP approach as an alternative measure for robustness checks.<sup>51</sup>

### C. Descriptive Evidence

The validity of our identification strategy and main results are visually presented in Figure 1, which depicts the evolution of logarithmic investment and TFP for the treated and control firms. In Figure 1(a), the red solid lines represent weighted average values of the logarithm of investment and TFP for firms that have experienced the VAT incentives treatment over years, while the blue dashed lines denote the corresponding weighted average values for firms ineligible for the tax incentives in the same years.<sup>52</sup> In the years before (and including) 2008, a year before the start of the nation-wide implementation of the VAT reform, the investment and TFP of the treated firms increased slightly at rates similar to those of the control firms. However, after 2008, the investment and TFP of the treated firms experienced a moderate increase, while the control firms saw a decrease in

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to the correlation between unobserved time invariant productivity and firms’ input decisions.

<sup>50</sup>That is, there are substantial adjustment costs for investment, resulting in a large number of “zero” investment.

<sup>51</sup>In subsection IV.F, we employ two more alternative measures of TFP to check the robustness of our results. Nevertheless, it has to be acknowledged that all these different measures of TFP are estimates of the real productivity of the firms and so measurement errors may still exist for each of them. In the current context, since our strategy is to identify the relative change of productivity between the treated firms and the control firms for the periods before and after the VAT reform, it is believed that as long as the sources of measurement errors of TFP are not related to the implementation of the VAT reform, the DID estimation framework would be able to deal with (at least partially) the systematic measurement errors between the two groups of firms.

<sup>52</sup>Note that in the calculation of Figure 1, to ensure comparison of the distribution of firms over years, we non-parametrically reweight the distribution of firms over years by ten size bins based on total assets crossed with ten size bins based on total sales. Specifically, as a reference base, we set the bins based on the distribution of total assets and total sales and compute bin counts and total counts separately for each group of (two-digit) industry-province for 2005 (i.e., the first year of our covered period). For all other years, we set weights at bin-level equal to the base-year fraction of firms in a bin divided by the corresponding year fraction of firms in a bin. This reweighting procedure was initially proposed by DiNardo et al. (1996) and was recently employed by Zwick and Mahon (2017).

investment and TFP. Although these patterns in Figure 1(a), to some extent, shed light on the common trend experienced by the two groups of firms in the pre-treatment period, and reflect the treatment effects of the VAT reform, they should be interpreted with caution. This is because the red solid lines do not contain any pre-treatment information for the treated firms, though a significant difference exists for the treated firms before and after 2008.<sup>53</sup> Similarly, the gaps between the treated and control firms after 2008 may not result from the pure treatment effects of the VAT reform; in particular, the blue dashed lines in the years before 2008 contain both the general VAT taxpayers that are not yet eligible for tax incentives treatment and the small-scale VAT taxpayers. However, in the years after 2008, these lines only contain the small-scale VAT taxpayers, and thus, the gaps after 2008 are also confounded with the original systematic difference between the general VAT taxpayers and small-scale taxpayers.

To further illustrate the validity of the common trend condition of the two groups of firms and the treatment effects of the VAT reform, Figure 1(b) shows the time trends of the logarithm of investment and TFP of the firms that became eligible for the tax incentives only after 2008 (denoted by the red solid lines) and those that were ineligible for the tax incentives during the whole sample period (represented by the blue dashed lines). The treatment group (the red solid lines) and control group (the blue dashed lines) show similar trends before the nation-wide adoption of the VAT reform in 2009. However, they diverge a bit after 2008, when the investment and TFP of the treated firms increased faster than the corresponding values for the control firms. As we shall show, this timing is commensurate with the timing of the nation-wide implementation of the VAT reform.

#### *D. Main Results*

Table 2 presents the results of our baseline specification (1), alternatively using firms' investment (i.e., Panel A) and productivity (i.e., Panel B) as the dependent variables. Each column of the table represents estimate from a separate regression, with only the coefficient of the VAT reform (and its clustered standard error) is reported. We start off the estimation by controlling for firm-specific fixed effects and year-specific fixed effects in Column (1) of Panel A. It turns out that the VAT reform is positively and statistically significantly associated with firm investment, which is consistent with the previous finding on the effectiveness of investment tax incentives. Column (2) takes a step further to add fourth order polynomials in total assets, total sales, profit margin, and firm age as additional control variables. The coefficient of the VAT reform is persistently pos-

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<sup>53</sup>In the years before and including 2008, the treated firms only include a small fraction of firms in selected industries located in certain areas that had initiated the VAT reform. These are listed in Table A1 in the Appendix of the paper. After 2008, the treated firms include all general VAT taxpayers in the nation.

itive and statistically significant at the 1% level. Column (3) includes quadratic time trends interacted separately with two-digit industry dummies and types of VAT taxpayer dummy,<sup>54</sup> addressing the possible confounding factors that may simultaneously affect the implementation of the VAT reform and the outcome variables. As shown, our results on the investment incentive effect of the reform are quite robust across these alternative specifications. Quantitatively, Column (3) indicates that after removing mean firm investment and common year effects, investment grew by approximately 38.4 percentage points more in firms implementing the VAT reform than in non-implementing firms. At the mean, this translates into an 8.8% increase in firm investment.<sup>55</sup>

In Panel B of Table 2, we replace the dependent variable with the logarithm of firms' TFP, to verify whether the policy impacts of the VAT reform hold when another important aspect of firm performance is considered. Our results, regardless of the use of alternative specifications, show that the estimated coefficient of the VAT reform is positive and statistically significant, suggesting an effective role of the VAT incentives in promoting productivity growth of the firms. Taking Column (3) as a benchmark, the estimated coefficient of the VAT reform is 0.089, implying that the productivity of the treated firms was 8.9 percentage points higher than that of the control firms. At the mean, this translates into a 3.7% increase in firm productivity.<sup>56</sup>

The validity of our main results in Table 2 relies on the assumption that there were no differential trends for the treated and control firms in the pre-treatment period. For instance, if the firm investment and/or productivity were already growing for general VAT taxpayers before the VAT reform, then our estimates in Table 2 could be biased upward. To address this concern we conduct an event study by estimating specification (2), where we replace the VAT reform dummy in Table 2 with a series of year-wise dummies for a window of three periods around the VAT reform event. Figures 2 and 3 plot the estimated coefficients on these dummies and show the 95% level confidence intervals.<sup>57</sup> These are interpreted as changes in the treated firms' investment and productivity, relative to the control firms, as compared to the period until four years before the implementation of the VAT reform. The point estimates show that there was little to no pre-trend for either firm investment or productivity before the VAT reform and that the significant increase in firm investment and productivity started only after the VAT incentives became available. This result increases our confidence in the validity of our identification strategy, as it would be difficult to explain the discontinuous increase of investment and productivity in the year immediately following the implementation of the VAT reform as a result of

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<sup>54</sup>We have also explored alternative specifications with industry-year fixed effects, province-year fixed effects, and province-industry fixed effects. Our results are largely robust to all these alternative specifications and the corresponding results are reported in Panel B, Table A3 of the Appendix.

<sup>55</sup>That is,  $0.384/4.348=0.088$ , where 4.348 is the sample mean of  $\ln(\text{Investment})$  in Table 1.

<sup>56</sup>That is,  $0.089/2.397=0.037$ , where 2.397 is the sample mean of  $\ln(\text{TFP\_OP})$  in Table 1.

<sup>57</sup>The regression results for Figures 2 and 3 can be found in Table A4 in the Appendix.

trends in unobservables.

### *E. Placebo Tests*

Over and above the previous evidence on the common time trend assumption, we conduct a series of placebo tests to further check the validity of our empirical design. The first is based on the types of investment. As we previously noted in the institutional background, investment in (factory) buildings and purchases of imported equipment (or other imported fixed assets) are not eligible for claiming deductions throughout all the stages of the VAT reform. Thus, we perform analogous regressions to the ones we performed in Panel A of Table 2, but instead of using eligible investment as the dependent variable, we respectively use these two types of ineligible investment as the dependent variables. The results are displaced in Panel A of Table 3. As can be seen in this table, the coefficient of the VAT reform is indistinguishable from zero.

The second placebo test exploits sector attributes. Given that the VAT incentives were applied exclusively to the purchases of fixed assets, the policy impact of the reform can be local to the manufacturing firms and does not necessarily apply to non-manufacturing firms, especially firms in agriculture and service sectors, for which investment in fixed assets is not the key and binding constraint for firm development. More importantly, the majority of firms in service sector are not recognized as VAT taxpayers,<sup>58</sup> and thus they are not eligible for the VAT incentives throughout the whole period of our sample. We therefore use samples from these two sectors as alternative placebo tests. Columns (1)-(2) and Columns (4)-(5) in Panel B of Table 3 present the results using full samples of agriculture and service sectors in our dataset, whereas Columns (3) and (6) further restrict the service sample to a subset of it that are even more immune to the VAT reform.<sup>59</sup> We find the coefficient of the VAT reform to be negative and generally statistically insignificant in the investment specification.<sup>60</sup> In addition, while the signs of the VAT reform vary across different sector samples, they are all statistically insignificant in the productivity specification.

Along the same lines as the previous test, the third placebo test exploits industry attributes. Our conjecture is that for those industries relying more heavily on fixed assets for their production, the VAT reform would have stimulated their investment to a greater extent than it did for other industries. In other words, those industries relying

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<sup>58</sup>Instead, these firms are recognized as business tax taxpayers. Recall that companies providing services in China are generally subject to business tax, which is not deductible against onward sales to other businesses.

<sup>59</sup>More specifically, we exclude from the service sector those industries that are more likely to be affected by the VAT reform. These include wholesale, retail, and press and publishing industries. These three industries are mixed with both VAT taxpayers and business tax taxpayers and so, to some extent, may be subject to the VAT incentives.

<sup>60</sup>This evidence is consistent with Djankov et al. (2010), who suggest that higher effective corporate income taxes are associated with lower investment in manufacturing but not in services.

less on fixed assets may be treated as a placebo group when compared to its counterpart. Thus, we calculate an index of fixed assets dependency (i.e., fixed assets/total outputs) for two-digit industries based on national-level data for year 2003,<sup>61</sup> and use this index to split our sample into deciles. Panel C of Table 3 reports and compares the corresponding results for the bottom three to the middle and the top three deciles. Being consistent with our conjecture, we note that although the signs of the VAT reform are positive in both Columns (1) and (4) for the bottom deciles sample, they are statistically insignificant. As a comparison, the estimates of the VAT reform for the middle and top deciles are significantly positive, and the top deciles sample appears to have the largest policy effect in magnitude.

Finally, in our last placebo test we randomly assign (falsify) the treatment status of the VAT reform to firms (see, for example, Chetty et al. (2009) and Mastrobuoni and Pinotti (2015)). Since the reform was introduced at different times in different industries and regions, and given the imbalance structure of our panel dataset, the numbers of true treated firms vary across years during the sample period. To preserve this fact, within each year, the same number of firms is randomly designated as the treatment group.<sup>62</sup> In this way, we construct a “false” VAT reform variable based on the random selection of treated firms. We then re-estimate our benchmark model, Column (3) of Table 2, using the false VAT reform variable, and store the estimates. We repeat this exercise 500 times in order to increase the identification power of the test. Given the random process involved in generating the data, the false VAT reform variable should have no significant impact on firms’ performance; otherwise, it would indicate a misspecification of the DID estimation in our baseline estimates. The empirical density of the estimated coefficients of the false VAT reform variable from the 500 simulation tests along with the benchmark estimates of the actual VAT reform variable from Column (3) of Table 2 are presented in Figure 4. As expected, the distributions of the estimated coefficients on the placebo VAT reform variable are clearly centred around zero with a small standard deviation, suggesting that there is no significant effect with the randomly constructed VAT reform variable. Meanwhile, the benchmark estimates, indicated by vertical red lines at the corresponding values of 0.384 and 0.089 in Figure 4, are located outside the entire range of coefficients estimated in our simulation exercise. In sum, the simulated results suggest that the positive and significant effect of the VAT reform on firms’ performance is not likely to be driven by unobserved factors.

Taken together, the results in Table 3 and Figures 2-4 validate our identification strategy from different aspects, which increase our confidence that our findings are not spurious.

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<sup>61</sup>The data source is China Statistical Yearbook for 2004.

<sup>62</sup>For instance, 3,416 (out of 45,965) firms are selected at random and assigned the status of having adopted the VAT incentives in 2005, and 3,975 (out of 47,580) firms are randomly selected as treated firms in 2006. This random selection continues until 2012.

### F. Other Robustness Checks

In order to further test for the robustness of the basic results, we conduct sensitivity analysis along three dimensions. First, we employ alternative measures of investment and productivity. Column (1) of Table 4 reports the result by using the investment ratio (i.e., the ratio of investment to lagged capital stocks) as the dependent variable. As shown, our results do not alter much from this change. Additionally, since investment is directly reported in the NTSD dataset, there is a concern that our results may merely reflect a reporting response. We follow Zwick and Mahon's (2017) method to address this issue by replacing our measure of investment with net investment, which is derived as logarithm of the difference in the real capital stock between year  $t$  and year  $t-1$ . The result in Column (2) of Table 4 confirms a positive and indeed an increased investment response of the firms in the post-VAT reform period. On the alternative measures of firm productivity, Column (3) shows the result from using the standard LP method of TFP as the dependent variable; while Columns (4)-(5) take a step further to modify the standard OP and LP methods to address the potential omitted price bias in calculating TFP. It is noted that in the absence of information on firm-specific price deflators, which is a typical issue in many other studies, we have used industry-level price deflators to deflate values of inputs and outputs and then used them as proxies for their quantities in our production function estimates. However, if firm-level price variation is correlated with input choice, it will bias our input coefficients (Van Beveren, 2012; De Loecker, 2011). Thus, we follow De Loecker (2011) to introduce industry output (proxying for unobserved firm-level prices) as an additional regressor in the context of OP and LP estimators of TFP and solve for firm-level prices bias.<sup>63</sup> We alternatively denote these measures of firm productivity as  $\ln(TFP\_OP2)$  and  $\ln(TFP\_LP2)$ . Similarly, the results indicated in Columns (3)-(5) of Table 4 do not challenge our main finding on the productivity-improvement effect of the reform, either in terms of estimated magnitude or statistical significance level.

In the second dimension of the robustness check, we restrict our full sample in three separate aspects. The first aspect is to exclude firms in the 51 counties affected by the *Wenchuan* earthquake throughout the whole periods in our dataset. The rationale here is that disasters can directly affect subsequent investment as infrastructure and equipment need to be rebuilt, which may contaminate the stimulating effect of the reform. The second aspect is to exclude firms that changed their types between general VAT taxpayers and small-scale VAT taxpayers in the covered periods. By doing so, we enhance comparability among firms by ensuring a more stable composition of firms and eliminate the possible confounding impacts from the strategic response of the firms. The third

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<sup>63</sup>This modified method was initially suggested by Klette and Griliches (1996). Readers interested in the details of this estimator are referred to De Loecker (2011) and Van Beveren (2012).

aspect is to restrict our sample to non-exporting firms. Since the nation-wide adoption of the VAT reform in 2009 was the Chinese central authority’s partial response to the 2008 global financial crisis, one may be concerned that the estimated treatment effect of the VAT reform may not be attributed only to the VAT incentives. That is, if the global financial crisis were to hit exporting firms more strongly, our aforementioned estimates of the effect of the VAT reform could be contaminated. To alleviate this concern, we exclusively focus on non-exporting firms.<sup>64</sup> Panel B of Table 4 suggests that across all reduced samples, alternatively considering firms’ investment and TFP, the estimated coefficient of the VAT reform is persistently positive and statistically significant, implying that our findings are not significantly affected by the aforementioned factors.

Lastly, given that the policy variation is partially at both regional and industrial levels in the early stages of the reform, there may be concerns with the possible influences of regional and industry-level shocks. We respond to this by alternatively considering standard errors at different cluster levels and by alternatively considering different levels of fixed effects. Particularly, Panel A of Table A3 in the Appendix provides the baseline estimates in Table 2 with standard errors clustering at province, industry, and both province and industry, respectively. Instead, in Panel B of the same table we alternatively estimate models by continuously adding with province-by-year, (two-digit) industry-by-year, and province-by-(two-digit) industry fixed effects. With all these alternative specifications, we obtain results that are similar to the baseline results.

## V. Effect Heterogeneity: The Role of Financial Constraints

In this section we consider the heterogeneity characterizing firms’ accessibility to external finance, to explore the extent to which the impact of VAT reform may vary across firms with different degrees of financial constraints. We proceed by dividing our sample across three indicators of ex ante financial constraints faced by the firms and performing separate estimations for each set of firms. These three measures of financial constraints include dividend payment, firm size, and firm ownership. The first two are usually deemed as typical sorting criteria to sort between “constrained” and “unconstrained” firms in the investment literature (see, for example, Chaney et al., 2012; Zwick and Mahon, 2017), while the last one is well rooted in the Chinese institutional context (see, for example, Poncet et al., 2010).

Table 5 summarizes the main results concerning the influence of financial constraints on the net impacts of the VAT reform. To begin with, we focus on dividend payment of the firms. While our dataset does not provide information on the actual dividend payment of the firms, it contains annual information on the available profits for distri-

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<sup>64</sup>Non-exporting firms are defined as those firms with less than 10% of their total sales for export. We also try alternative definitions of non-exporting firms by imposing smaller and larger values of exporting dependency (i.e., 5% and 20%, respectively). The results are largely unchanged.

bution to investors, which are the base of actual dividend payment of the firms. We, thus, use the mean value of this variable in the pre-reform period for each firm to split our sample into two groups: firms with no (zero and/or negative) available profits for distribution to investors and firms with positive available profits for distribution to investors. As indicated in Panel A of Table 5, the average treatment effect of the VAT reform on investment for firms with no available profits for distribution to investors (and so relatively high level of financial constraints) is almost triple as large as that for firms with positive available profits for distribution to investors (and so relatively low level of financial constraints) and statistically and significantly different with a p-value of 0.100. Meanwhile, the difference of the average treatment effect on firm productivity is even striking, with a statistically significant coefficient of 0.178 for firms in the former group and a negligible and statistically insignificant coefficient of 0.017 for firms in the latter group.<sup>65</sup>

In Panel B of Table 5, we split the sample into deciles based on the mean value of firms' assets in the pre-reform period and compare the bottom three to the top three deciles. The results indicate that small firms are significantly more responsive to the VAT incentives. Quantitatively, small (i.e., "constrained") firms yield an estimate of 0.827 as compared to 0.289 for large (i.e., "unconstrained") firms in the investment specification. In line with the previous finding, the stimulus effect of the reform on firm productivity is much more salient for small firms than that for their counterparts, with a statistically significant coefficient of 0.179 for the former and a statistically insignificant coefficient of 0.047 for the latter. Although the equality test between the two coefficients in both specifications is only statistically significant at the margin, they are all significant at the 10 percent level under one-tailed test.<sup>66</sup>

Owing to historical reasons and special government policies in China, it has long been argued that state-owned enterprises (SOEs) generally have much better access to bank credits than other types of enterprises.<sup>67</sup> Following this insight, we classify our sample into two groups, namely SOEs and non-SOEs, and re-estimate the model.<sup>68</sup> We expect that the SOEs should be less sensitive in responding to the VAT reform than the non-SOEs firms, as the SOEs are generally considered as less-constrained firms. Panel C

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<sup>65</sup>It is noted that for firms with positive available profits for distribution to investors, it may not necessary imply a positive dividend payment. If this is the case, then our defined group of "unconstrained" firms may indeed include some firms actually facing credit constraints, and so our estimated difference in Panel A of Table 5 may be underestimated.

<sup>66</sup>As also pointed out by Zwick and Mahon (2017), firm size itself is not a sufficient proxy for firms' other characteristics in accounting for the heterogeneity effect.

<sup>67</sup>For instance, Poncet et al. (2010) provide explicit evidence that the geographical and sectoral presence of SOEs aggravates financial constraints for private firms, and at the individual firm level, private firms are found to depend to a greater extent on internally generated funds for their investment, and appear to be more strongly credit constrained than the SOEs.

<sup>68</sup>Firm ownership is classified by firms' registered type (*qiye dengji zhuce leixing*), which is provided directly by the NTSD. For our classifications, SOEs include state-owned enterprises and collective-owned firms; while non-SOEs include foreign, Hong Kong, Macao or Taiwan (HMT), and private firms.

of Table 5 confirms this expectation by revealing that while the estimated coefficient of the VAT reform is positive and statistically significant for non-SOEs firms, its sign for the SOEs is negative and statistically insignificant in both investment and productivity specifications.

In sum, we provide consistent evidence for the heterogeneous effect of the VAT reform. A possible explanation to that may be what we pointed out previously that the increment in cash flows brought about by the VAT reform lessens financial constraints and potentially benefits the initially constrained firms to a greater extent.

## VI. Mechanisms of the Impact

### A. *The Impact of the VAT Reform on Cash Flows*

What are the mechanisms by which the VAT reform leads to improved investment and productivity of the firms?<sup>69</sup> The direct mechanism may simply be the reduction of user costs of capital due to the tax incentives treatment, which lends itself to improvements in investment directly. Alternatively, it is also possible that the VAT reform may indirectly affect firms' performance via its impact on firms' internal funds accumulation. While our aforementioned analysis on the role of credit constraints has already shed some light on it, in this subsection we provide more clear evidence that the VAT reform indeed raised cash flows of the firms.

To explore more fully the impact of the VAT reform on cash flows, we estimate an event study framework similar to specification (2), where cash flows (i.e., the ratio of cash flows to lagged total assets) are treated as an intermediate outcome variable.<sup>70</sup> The estimated coefficients are displayed in Figure 5 together with their 95 percent confidence intervals.<sup>71</sup> It shows that the increase in cash flows does not occur before the implementation of the VAT reform. None of the coefficients for the years preceding the reform are significantly different from zero. The positive effect of the policy changes on cash flows is realized on and after the year of reform, supporting our argument for the increased cash flows as a possible mechanism of the impact.

### B. *The Impact of the VAT Reform on R&D Expenditures*

Our discussion on the theoretical impacts of the VAT reform is more directly connected to firms' investment decisions, and relatively speaking, it is somehow less straightforward to link it to firms' productivity improvement. Certainly, if the increased investment brought

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<sup>69</sup>It has to be noted that we do not attempt to uncover exact working mechanisms of the impact in this paper, but instead shed some light on some possible theoretical arguments.

<sup>70</sup>It is noted that we also report the results for the net impact of the VAT reform on cash flows in Table A5 of the Appendix. As shown, the VAT reform has a consistent positive and statistically significant coefficient across different specifications.

<sup>71</sup>The regression table for this result can be found in Table A4 in the Appendix.

about by the VAT reform is productivity-enhancing, it may automatically imply a corresponding increase in firms' productivity. Beyond this point, we explicitly explore another possibility, namely that the VAT reform induces an increase in R&D expenditures of the firms, which in turn is a traditional source of productivity growth. This mechanism can be justified on two grounds. For one, the VAT reform may indeed bring more investment in R&D-oriented fixed assets, and for another, the aforementioned increase in cash flows resulting from the reform enables the firms to spend more on R&D-related expenditures (including expenditures on human capital). While we do not intend to detangle the exact working mechanisms, we follow the analytical framework in the previous subsection to provide evidence on the impact of the reform on R&D expenditures.

The results are plotted in Figure 6, where R&D expenditures (i.e., logarithm of R&D expenditures per employee) are used as an alternative intermediate outcome variable.<sup>72</sup> We, once again, note that the estimates for the years preceding the reform are statistically insignificant, and the increase in R&D expenditures becomes more salient only after the reform was in place. This confirms R&D expenditures as a possible channel for the VAT reform to influence firms' productivity.

### *C. Further Discussion*

We have established that the VAT reform raised firms' investment in fixed assets (and also R&D expenditures) and that the effects are likely to be asymmetric across firms, in a way favoring those originally in a disadvantageous position (e.g., credit constrained firms). If this is the case, then it may, in turn, imply a pro-competitive role of the reform in intensifying market competition. By exposing firms to greater competition, the policy reform may reduce markup dispersion, thereby reducing resource misallocation and increasing aggregate productivity.<sup>73</sup> Although a thorough investigation of this point is beyond the scope of the current paper, we attempt to shed some light on it below by showing that the VAT reform may have reduced markup dispersion within industries in the post-reform period. This is an important step in investigating the pro-competitive role of the policy.

To proceed, we follow the methodology developed by De Loecker and Warzynski (2012) to calculate markup of the firms. More specifically, the markup is calculated as the ratio of the output elasticity for a variable input to its corresponding expenditure shares in total revenue.<sup>74</sup> The intuition behind the calculation is that the output elasticity of a

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<sup>72</sup>The regression result for this figure can be found in Table A4 in the Appendix. In Table A5 of the Appendix, we also report the results for the net impact of the VAT reform on R&D expenditures.

<sup>73</sup>It has long been identified in the literature that market competition helps to reduce markup dispersion, thus reducing resource misallocation and increasing aggregate productivity (see, for example, Edmond et al., 2015; Lu and Yu, 2015).

<sup>74</sup>As pointed out by Brandt et al. (forthcoming), an important advantage of this methodology is that it provides a measure of firm performance that varies with a firm's market power without having to make

variable input should equal its corresponding expenditure share in total revenue when the market is perfectly competitive (and so price equals marginal cost). In a world with imperfect competition, markup is the wedge between expenditure share of the input in total revenue and the output elasticity of this input. In the calculation, we rely on materials and intermediate inputs as our variable inputs, as firms can adjust more flexibly with these inputs than others such as capital or labor. The materials and intermediate inputs are also less likely to suffer from measurement error. With the data at hand, we are readily able to calculate the share of expenditure on intermediate materials to total revenue (i.e., total sales) and can obtain the output elasticity of intermediate materials from the estimation of the production function as stated in specification (3) in subsection IV.B.<sup>75</sup>

Finally, based on firm-level markup, we calculate the mean and coefficient of variation of markup for all two-digit manufacturing industries in each year and compare the changes of these two indexes before and after the largest expansion of the VAT reform in 2009. Specifically, Figure 7 reports the average values of the mean and coefficient of variation of industry level markup before 2009 (i.e., 2005-2008) and percentage changes in these values after 2009.<sup>76</sup> As shown, both indexes experienced significant reduction in the post-2009 period, potentially indicating that markup and its dispersion within industries are significantly reduced in this period. Although this evidence itself is not sufficient to establish a causal relationship between the VAT reform and market competition, it does serve as tentative evidence of the pro-competitive role of the VAT reform and hence suggests an alternative possible source of the reform’s impact on firm productivity growth.

## VII. Conclusions

The VAT reform initiated in 2004 and completed in 2009 was among the most significant reforms in China’s tax system. This reform has had large and heterogeneous effects on firms’ investment and productivity, thus influencing the Chinese economy to a considerable extent. This paper uses a large and unique firm-level dataset, covering all sizes of firms in a very broad range of sectors across regions for the years 2005 to 2012, to test whether the VAT reform has successfully stimulated firms’ investment and promoted their productivity. Equally important, the paper also examines the degree to which the presence of financial constraints influences the net policy impacts of the VAT reform. Taking advantage of the vast variation in the tax incentives treatment brought about by the VAT reform across firms over time, we exploit this quasi-experimental setting using

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explicit assumptions on demand or behavioral assumptions on competition.

<sup>75</sup>To be consistent with the productivity specification, we rely on the OP approach as the baseline for the estimation of the production function.

<sup>76</sup>Appendix A6 contains the original values of the mean and coefficient of variation of the markup at two-digit industry level before and after 2009.

a DID approach. Our econometric results confirm the salience and relevance of the VAT reform by revealing an immediate increase in firms' investment and productivity in the post-reform period. Quantitatively, the reform raised the investment and productivity of the treated firms by 38.4 percentage points and 8.9 percentage points, translating into an 8.8% and a 3.7% increase in investment and productivity, respectively. We further validate that eligible firms that are likely to face credit constraints experience higher increases in investment and productivity following the implementation of the VAT reform. These results are shown to be quite robust across a number of sensitivity tests and placebo analyses.

Fundamentally, our results add to the literature by providing strong evidence from the world's largest developing country on the effectiveness of using tax incentives to promote firms' investment and productivity. The results are important as this has long been a key public policy goal in many countries, especially when the economy is in a downturn. In particular, we highlight that the positive roles of tax incentives tend to be intensified for firms with financial constraints. Given that small firms make a major contribution in many economies and the constraints on investment activity due to impediments in accessing external capital markets are especially stronger for smaller firms, our results deliver an important message: if it is not feasible to spur the development of a country's financial sector to loosen credit constraints of firms, especially small ones, it is highly prudent for that country to employ tax incentives to stimulate economic growth. This insight will be particularly critical for emerging economies, where new and small firms have to be encouraged to develop economically. Simultaneously, access to finance is identified as the most binding constraint for firm growth in these countries.

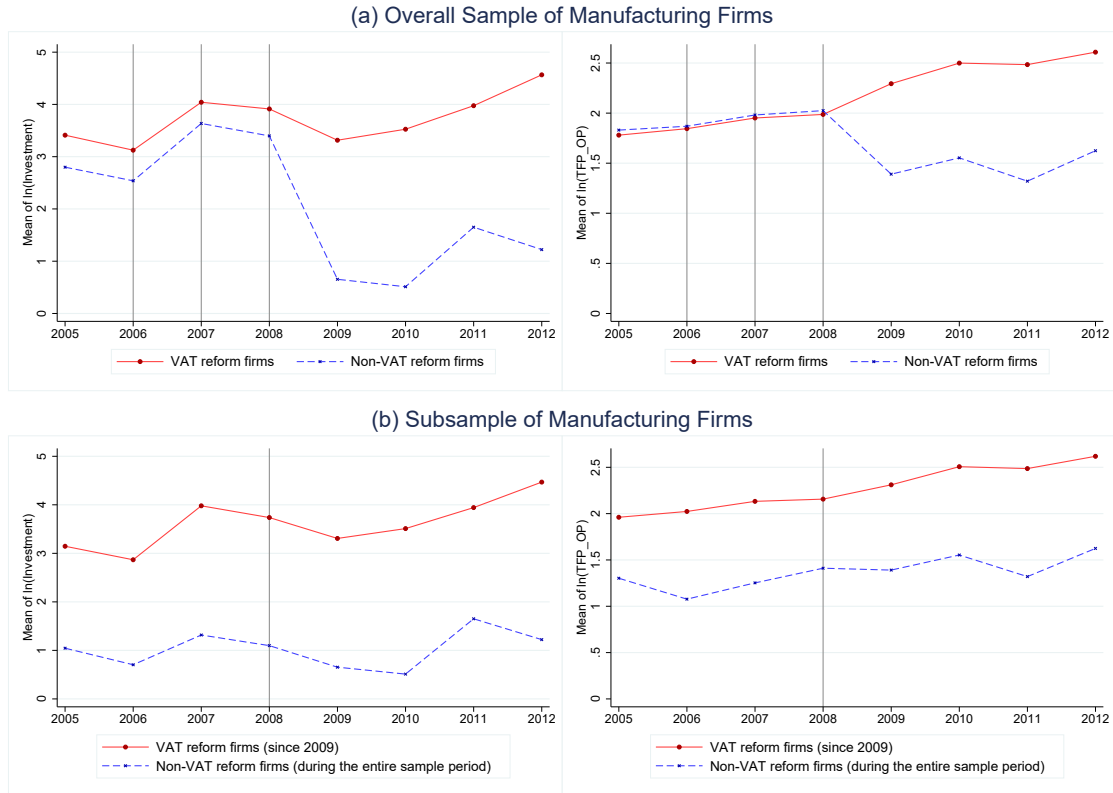
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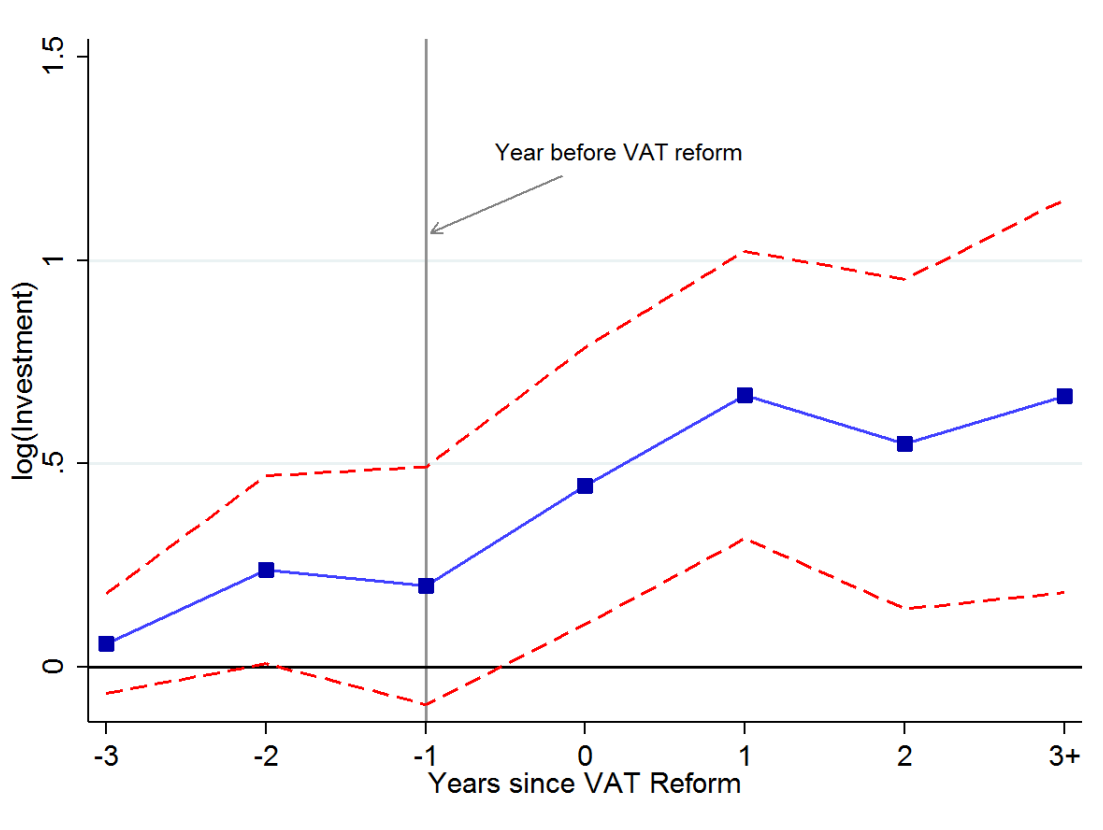
**Figure 1: Trend Comparison of Firms' Investment and Productivity**



*Notes:* Figure 1(a) illustrates the time trends of weighted average values of the logarithm of investment and TFP for the VAT-reform firms (i.e., firms that are eligible to the VAT incentives in every corresponding year) and the non-VAT-reform firms (i.e., firms that are not eligible to the VAT incentives in the same years). Vertical lines on years 2006, 2007, and 2008 represent one year before the treatment years for different stages of the VAT reform in the overall manufacturing sample. Figure 1(b) shows the time trends of weighted average values of logarithm of investment and TFP for the firms that became eligible for the tax incentives only after 2008 and those that were ineligible for the tax incentives during the whole sample period. Vertical line on year 2008 represents the year before the final stage of the reform. To ensure comparison of the distribution of firms over years, we non-parametrically reweight the distribution of firms over years by ten size bins based on total assets crossed with ten size bins based on total sales. Specifically, as a reference base, we set the bins based on the distribution of total assets and total sales and compute bin counts and total counts separately for each group of (two-digit) industry-province for 2005 (i.e., the first year of our covered period). For all other years, we set weights at bin-level equal to the base-year fraction of firms in a bin divided by the corresponding year fraction of firms in a bin. This reweighting procedure was initially proposed by DiNardo et al. (1996) and was recently employed by Zwick and Mahon (2017).

*Source:* Authors' calculations.

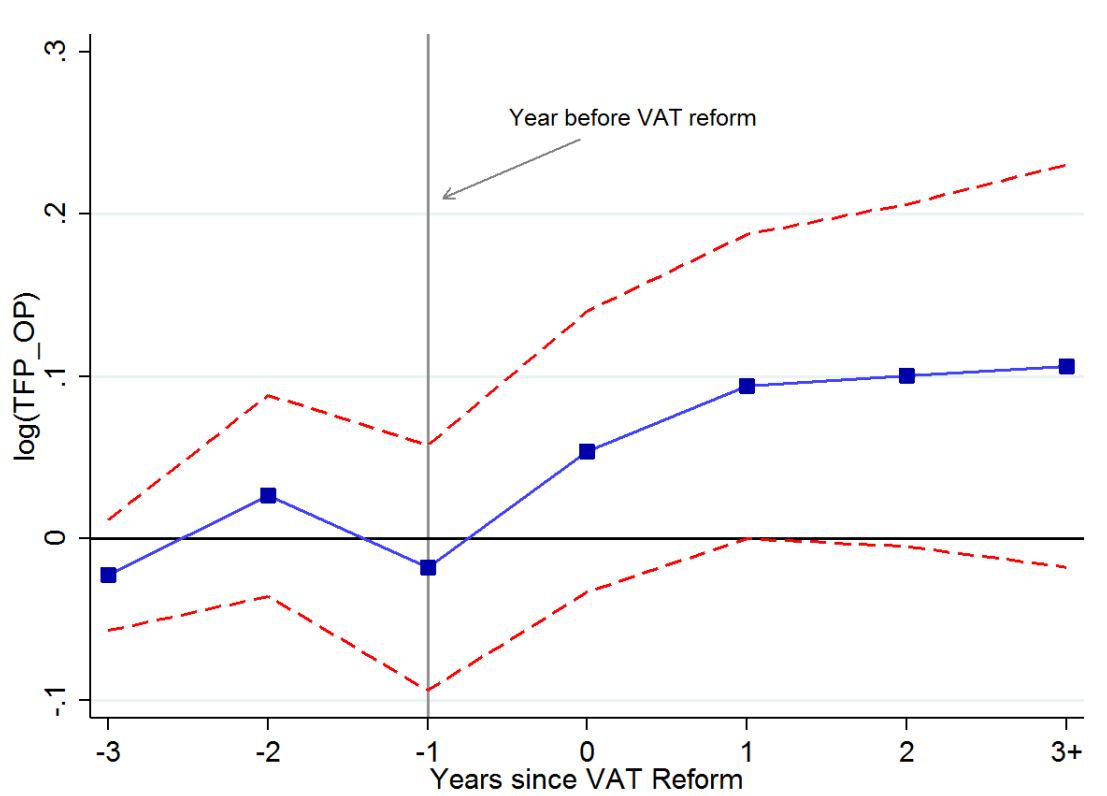
Figure 2: Dynamics of Firms' Investment Before and After the VAT Reform



*Notes:* This figure plots the estimated coefficients and 95 percent confidence interval from a regression of logarithm of investment on a set of dummies from  $t-3$  to  $t+3$ , where  $t=0$  is the year of the VAT reform. The horizontal axis measures the number of years since the VAT reform. The solid line indicates changes in investment compared to the period until four years before the VAT reform conditional on firm fixed effects, year fixed effects, and quadratic time trends for two-digit industries and types of VAT taxpayers. The dashed lines indicate the 95% confidence intervals where standard errors are clustered at the firm level.

*Source:* Authors' calculations.

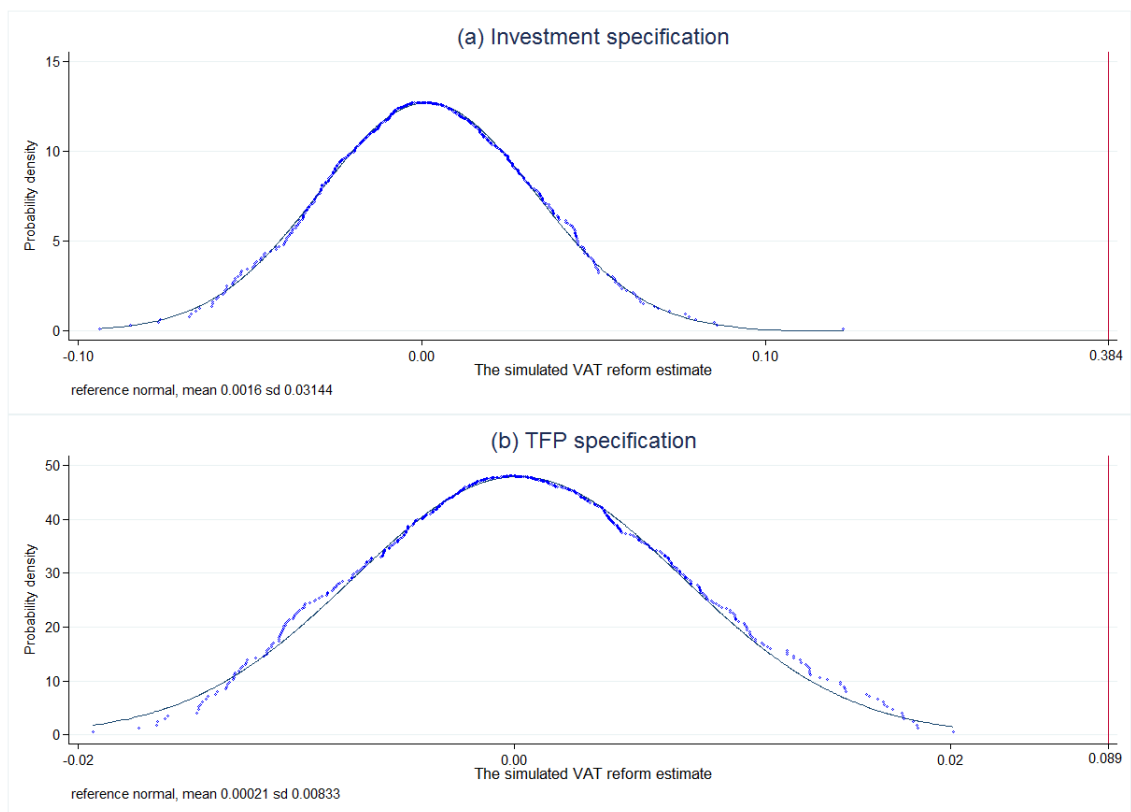
**Figure 3: Dynamics of Firms' Productivity Before and After the VAT Reform**



*Notes:* This figure plots the estimated coefficients and 95 percent confidence interval from a regression of logarithm of productivity on a set of dummies from  $t-3$  to  $t+3$ , where  $t=0$  is the year of the VAT reform. The horizontal axis measures the number of years since the VAT reform. The solid line indicate changes in productivity compared to the period until four years before the VAT reform conditional on firm fixed effects, year fixed effects, and quadratic time trends for two-digit industries and types of VAT taxpayers. The dashed lines indicate the 95% confidence intervals where standard errors are clustered at the firm level.

*Source:* Authors' calculations.

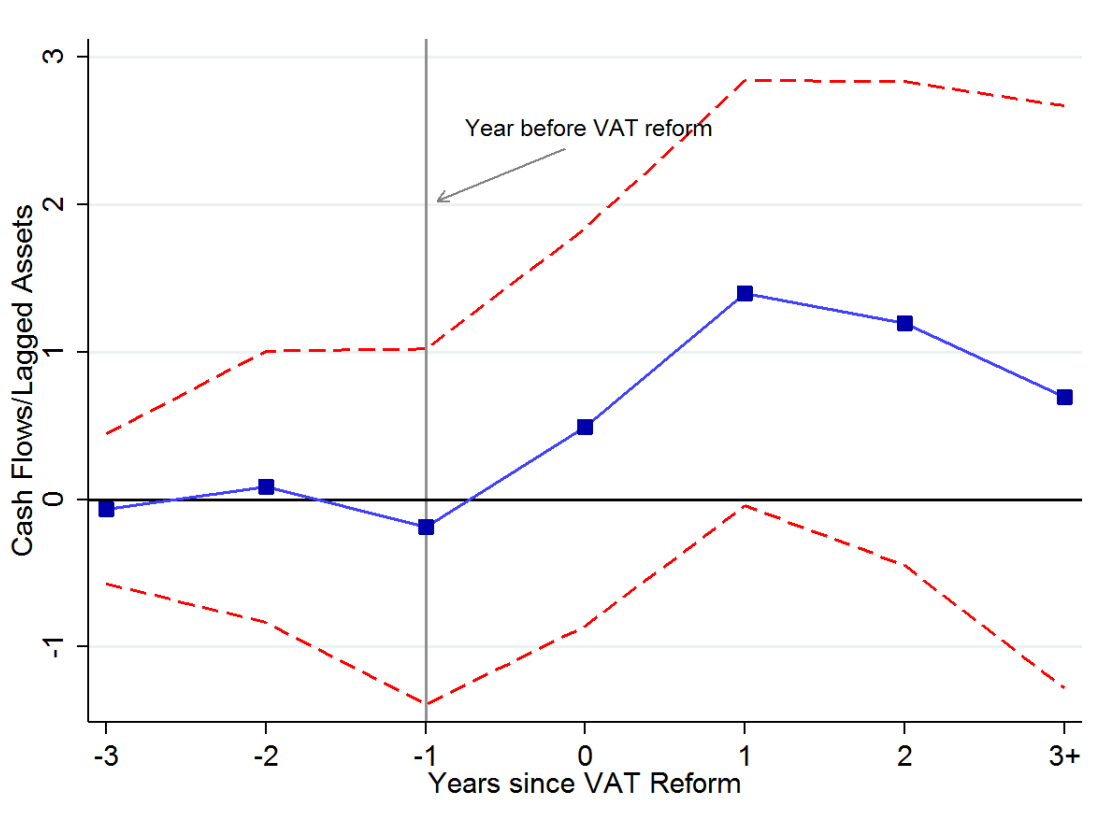
**Figure 4: The Distribution of Estimates for the “False” VAT Reform Variable**



*Notes:* The figure plots the density of the estimated coefficients of the false VAT reform variable from the 500 simulation tests using the specification in Column (3) of Table 2. The vertical red lines present the treatment effect estimates reported in Column (3) of Table 2.

*Source:* Authors' calculations.

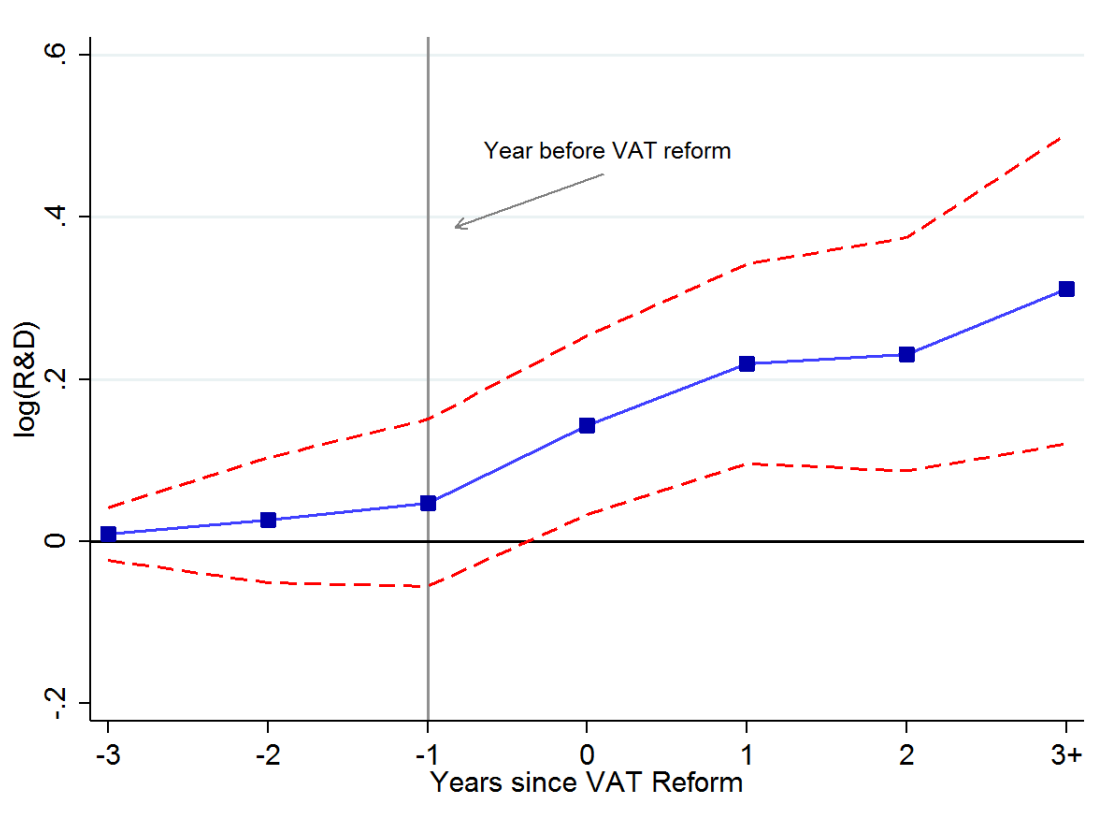
Figure 5: Dynamics of Firms' Cash Flows Before and After the VAT Reform



*Notes:* This figure plots the estimated coefficients and 95 percent confidence interval from a regression of cash flows (i.e., the ratio of cash flows to lagged assets) on a set of dummies from  $t-3$  to  $t+3$ , where  $t=0$  is the year of the VAT reform. The horizontal axis measures the number of years since the VAT reform. The solid line indicate changes in cash flows compared to the period until four years before the VAT reform conditional on firm fixed effects, year fixed effects, and quadratic time trends for two-digit industries and types of VAT taxpayers. The dashed lines indicate the 95% confidence intervals where standard errors are clustered at the firm level.

*Source:* Authors' calculations.

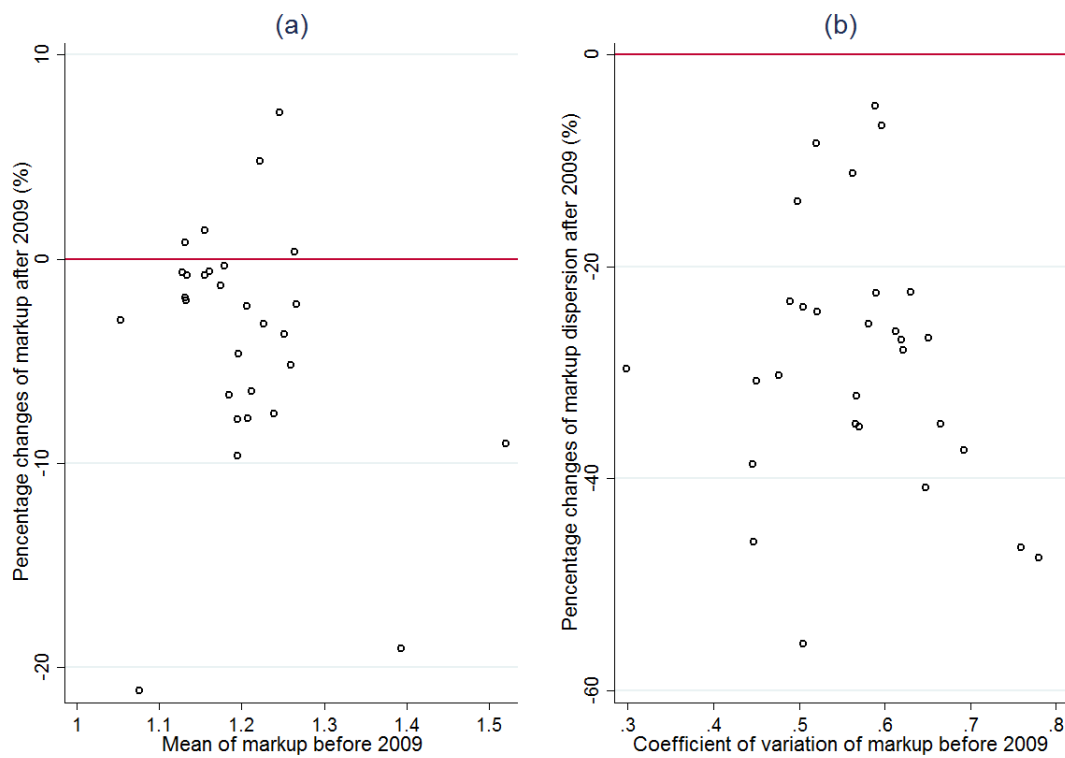
**Figure 6: Dynamics of Firms' R&D Expenditures Before and After the VAT Reform**



*Notes:* This figure plots the estimated coefficients and 95 percent confidence interval from a regression of logarithm of R&D expenditures per employee on a set of dummies from  $t-3$  to  $t+3$ , where  $t=0$  is the year of the VAT reform. The horizontal axis measures the number of years since the VAT reform. The solid line indicate changes in R&D expenditures compared to the period until four years before the VAT reform conditional on firm fixed effects, year fixed effects, and quadratic time trends for two-digit industries and types of VAT taxpayers. The dashed lines indicate the 95% confidence intervals where standard errors are clustered at the firm level.

*Source:* Authors' calculations.

**Figure 7: Changes of Markup Dispersion Within Two-digit Industries Before and After 2009**



*Notes:* This figure plots the changes of the mean and coefficient of variation of markup for all two-digit manufacturing industries before and after the largest expansion of the VAT reform in 2009. Each circle represents a two-digit manufacturing industry. In Panel (a), the horizontal axis represents the average value of the mean of markup for each two-digit manufacturing industry before 2009 (i.e., 2005-2008), and the vertical axis represents the percentage changes in the average values of the mean of markup for each two-digit manufacturing industry after 2009 (i.e., 2009-2012). Panel (b) presents the corresponding values for the coefficient of variation of the markup at two-digit industry level.

*Source:* Authors' calculations.

**Table 1: Summary Statistics: Manufacturing Firms**

Variables	Obs.	Mean	Std Dev	Min	Max
ln(Investment)	249,803	4.348	3.924	0	18.364
Investment/lagged capital	243,411	0.056	0.133	0	0.876
ln(Net investment)	232,791	4.144	3.730	0	18.467
ln(Investment_building)	250,076	1.248	2.931	0	17.970
ln(Investment_import)	250,135	0.108	0.949	0	14.928
ln(TFP_OP)	280,352	2.397	1.535	-1.426	7.558
ln(TFP_LP)	280,352	3.662	1.539	-0.528	9.471
ln(TFP_OP2)	280,352	-2.941	3.231	-12.444	9.769
ln(TFP_LP2)	280,352	0.849	2.247	-5.089	14.936
VAT reform	280,741	0.506	0.500	0	1
Markup	280,352	1.184	0.639	0.653	6.869
Cash flows, %	240,374	7.594	13.168	-31.679	116.600
ln(R&D per employee)	213,863	0.266	0.798	0	3.769
ln(Total assets)	279,349	9.520	2.594	4.463	16.115
ln(Total sales)	280,741	9.554	2.709	3.954	15.531
Profit margin, %	279,112	1.173	11.034	-56.694	54.059
Age	280,695	9.115	6.349	1	111

*Notes:* All monetary values are in real term. For definitions of these variables, see Table A2 in the Appendix.

**Table 2: Firms' Responses to the VAT Reform: Baseline Results**

	(1)	(2)	(3)
<i>Panel A. LHS variable is <math>\ln(\text{Investment})</math></i>			
VAT reform	0.350*** (0.113)	0.333*** (0.111)	0.384*** (0.114)
Observations	249,803	247,957	249,803
$R^2$	0.019	0.036	0.022
<i>Panel B. LHS variable is <math>\ln(\text{TFP\_OP})</math></i>			
VAT reform	0.095*** (0.030)	0.058** (0.025)	0.089*** (0.029)
Observations	280,352	277,309	280,352
$R^2$	0.022	0.235	0.185
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Controls	No	Yes	No
Industry trends	No	No	Yes
VAT taxpayer trends	No	No	Yes

*Notes:* In Panel A and Panel B, the dependent variables are logarithm of investment and OP method of TFP, respectively. Column (1) starts off the estimation by controlling for firm fixed effects and year fixed effects. Column (2) augments the specification with quartics in assets, sales, profit margin, and firm age as additional explanatory variables. Column (3) adds quadratic time trends for two-digit industries and types of VAT taxpayers to the specification. Standard errors are clustered at firm level for all regressions.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

**Table 3: Firms' Responses to the VAT Reform: Placebo Tests**

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Ineligible investment</i>						
	ln(Investment_building)			ln(Investment_import)		
VAT reform	0.074 (0.138)	0.070 (0.138)	0.113 (0.142)	-0.061 (0.064)	-0.062 (0.064)	-0.058 (0.066)
Observations	250,076	248,227	250,076	250,135	248,287	250,135
$R^2$	0.005	0.009	0.005	0.004	0.005	0.010
Controls	No	Yes	No	No	Yes	No
Industry trends	No	No	Yes	No	No	Yes
VAT taxpayer trends	No	No	Yes	No	No	Yes
<i>Panel B. Placebo sectors</i>						
	ln(Investment)			ln(TFP_OP)		
	Agriculture	Service	Sub-service	Agriculture	Service	Sub-service
VAT reform	-0.866 (1.718)	-0.258* (0.152)	-0.235 (0.326)	0.510 (0.609)	0.027 (0.046)	-0.117 (0.091)
Observations	3,397	224,560	83,649	3,171	259,073	88,374
$R^2$	0.070	0.030	0.044	0.058	0.143	0.262
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes
VAT taxpayer trends	Yes	Yes	Yes	Yes	Yes	Yes
<i>Panel C. Fixed asset dependency</i>						
	ln(Investment)			ln(TFP_OP)		
	Low	Medium	High	Low	Medium	High
VAT reform	0.301 (0.253)	0.325** (0.161)	0.541** (0.255)	0.080 (0.068)	0.073* (0.039)	0.130** (0.058)
Observations	80,566	95,803	73,434	90,844	107,348	82,160
$R^2$	0.018	0.024	0.026	0.143	0.107	0.058
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes
VAT taxpayer trends	Yes	Yes	Yes	Yes	Yes	Yes

*Notes:* The dependent variable is indicated on the top of each column, where ln(Investment\_building) and ln(Investment\_import) represent logarithm of investment in (factory) building and purchases of imported capital goods, respectively. All other dependent variables are the same as those in Table 2. All regressions include firm fixed effects and year fixed effects. Controls indicate quartics in assets, sales, profit margin, and firm age. Industry trends indicate quadratic time trends for two-digit industries; and VAT taxpayer trends indicate quadratic time trends for types of VAT taxpayers. Standard errors are clustered at firm level for all regressions.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

**Table 4: Firms' Responses to the VAT Reform: Robustness Checks**

<i>Panel A. Alternative measures of investment and productivity</i>						
	Investment/ lagged capital	ln(Net investment)	ln(TFP_LP)	ln(TFP_OP2)	ln(TFP_LP2)	
	(1)	(2)	(3)	(4)	(5)	
VAT reform	0.013*** (0.005)	0.922*** (0.169)	0.096*** (0.029)	0.066** (0.029)	0.078*** (0.029)	
Observations	243,411	232,791	280,352	280,352	280,352	
$R^2$	0.017	0.219	0.135	0.753	0.519	
<i>Panel B. Alternative samples</i>						
	ln(Investment)			ln(TFP_OP)		
	Subsample	Subsample	Subsample	Subsample	Subsample	Subsample
	1	2	3	1	2	3
	(6)	(7)	(8)	(9)	(10)	(11)
VAT reform	0.386*** (0.117)	0.371*** (0.115)	0.338*** (0.124)	0.092*** (0.030)	0.082*** (0.029)	0.067** (0.031)
Observations	248,128	246,930	218,089	278,468	277,126	246,714
$R^2$	0.023	0.022	0.023	0.185	0.186	0.170

*Notes:* The dependent variable is indicated on the top of each column. Investment/lagged capital is defined as the ratio of investment to lagged capital stocks; ln(Net investment) is derived as logarithm of the difference in the real capital stock between year  $t$  and year  $t-1$ . ln(TFP\_LP) is the standard LP method of TFP, while ln(TFP\_OP2) and ln(TFP\_LP2) represent adjusted OP and LP methods of TFP (see subsection IV.F), respectively. Subsamples 1, 2 and 3 represent exclusions of firms in the 51 counties affected by the *Wenchuan* earthquake, firms that changed their types between general VAT taxpayers and small-scale VAT taxpayers in the covered periods, and exporting firms (i.e., firms with more than 10% of their total sales for export), respectively. All regressions include firm fixed effects, year fixed effects, and quadratic time trends for two-digit industries and types of VAT taxpayers. Standard errors are clustered at firm level for all regressions.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

**Table 5: Firms' Responses to the VAT Reform: Effect Heterogeneity**

	ln(Investment)		ln(TFP_OP)	
	(1)	(2)	(3)	(4)
<i>Panel A. Dividend payment</i>				
	No	Yes	No	Yes
VAT reform	0.596*** (0.177)	0.213 (0.151)	0.178*** (0.045)	0.017 (0.037)
Equality test	$P=0.100$		$P=0.005$	
Observations	147,991	101,812	153,612	126,740
$R^2$	0.028	0.024	0.162	0.227
<i>Panel B. Firm size</i>				
	Small	Large	Small	Large
VAT reform	0.827*** (0.312)	0.289* (0.166)	0.179** (0.080)	0.047 (0.042)
Equality test	$P=0.128$		$P=0.143$	
Observations	70,671	75,725	86,180	79,239
$R^2$	0.016	0.033	0.130	0.211
<i>Panel C. Firm ownership</i>				
	Non-SOEs	SOEs	Non-SOEs	SOEs
VAT reform	0.464*** (0.129)	-0.183 (0.360)	0.134*** (0.032)	-0.099 (0.084)
Equality test	$P=0.090$		$P=0.009$	
Observations	216,962	25,010	244,205	27,629
$R^2$	0.023	0.032	0.192	0.134

*Notes:* We split the sample based on different indicators of financial constraints faced by the firms. In Panel A, dividend payment is based on the information that whether firms have available (positive) profits for distribution to investors in the pre-reform period. In Panel B, firm size is defined as the average value of firms' assets in the pre-reform period; the bottom three deciles are categorized as "small" firms, while the top three deciles are grouped as "large" firms. In Panel C, firm ownership is provided directly by the NTSD. SOEs include state-owned enterprises and collective-owned enterprises; Non-SOEs include foreign, Hong Kong, Macao or Taiwan (HMT), and private firms. All regressions include firm fixed effects, year fixed effects, and quadratic time trends for two-digit industries and types of VAT taxpayers. Standard errors are clustered at firm level for all regressions.

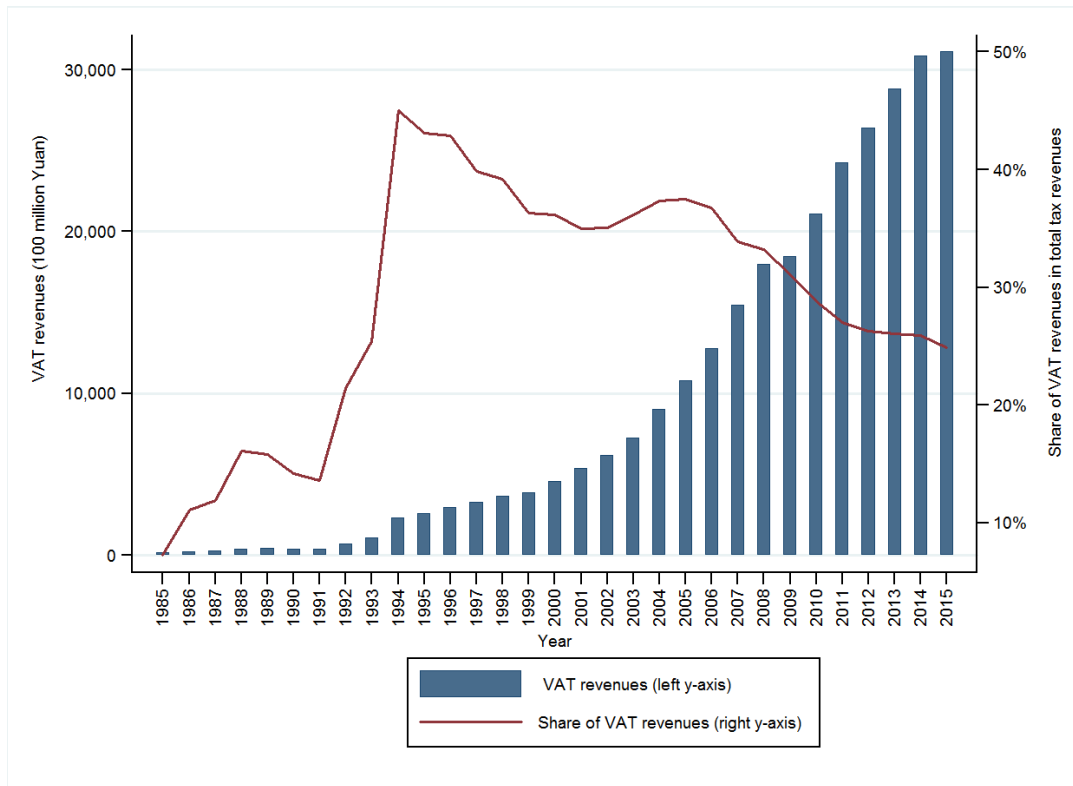
\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

## Appendix A: Supplementary Figures and Tables

Figure A1: Size and Share of the VAT Revenues in China



Source: The National Bureau of Statistics of China.

**Table A1. Evolution of the VAT Reform**

Stage of the Reform (Starting Time)	Regions Covered	Industries Covered (Industry Classification Codes)
1 (July 2004)	The three North-eastern provinces: Liaoning (including Dalian city), Jilin and Heilongjiang.	Machine and equipment manufacturing (35, 36, 39, 40, 41, 42); Petroleum, chemical, and pharmaceutical manufacturing (25, 26, 27, 28, 29, 30); Ferrous and non-ferrous metallurgy (32, 33); Agricultural product processing (13, 14, 15, 17, 18, 19, 20, 21, 22); Shipbuilding (375); Automobile manufacturing (371, 372, 376, 379); Selected military and hi-tech products (a list of 249 firms, 62 of which are in our sample).
2 (July 2007)	26 cities of the six middle provinces: 4 (Taiyuan, Datong, Yangquan and Changzhi) in Shanxi province, 5 (Hefei, Maanshan, Bengbu, Wuhu and Huainan) in Anhui province, 4 (Nanchang, Pingxiang, Jingdezhen and Jiujiang) in Jiangxi province, 5 (Zhengzhou, Luoyang, Jiaozuo, Pingdingshan and Kaifeng) in Henan province, 4 (Wuhan, Huangshi, Xiangfan and Shiyan) in Hubei province, and 4 (Changsha, Zhuzhou, Xiangtan and Hengyang) in Hunan province.	Machine and equipment manufacturing (35, 36, 39, 40, 41, 42); Petroleum, chemical, and pharmaceutical manufacturing (25, 26, 27, 28, 29, 30); Ferrous and non-ferrous metallurgy (32, 33); Automobile manufacturing (371, 372, 376, 379); Agricultural product processing (13, 14, 15, 17, 18, 19, 20, 21, 22); Electric power (441, 442); Mining (6, 8, 9, 10, 11); Hi-tech (253, 2665, 271, 272, 274, 276, 368, 3761, 3762, 3769, 401, 402, 403, 4041, 4042, 4043, 405, 406, 407, 409, 411, 412, 4141, 4154, 4155, 419, 6211, 6212).
3 (July 2008)	(1) 5 cities of Inner Mongolia: Hulunbuir, Xingan, Tongliao, Chifeng and Xilingele. (2) 51 counties suffering from Wenchuan earthquake: 39 (Wenchuan, Beichuan, Mianzhu, Shifang, Qingchuan, Mao, An, Dujiangyan, Pingwu, Pengzhou, Li, Jiangyou, Lizhou district of Guangyuan city, Chaotian district of Guangyuan city, Yuanba district of Guangyuan city, Wangcang, Zitong, Youxiandistrict of Mianyang city, Fucheng district of Mianyang city, Jingyang district of Deyang city, Xiaojin, Luojiang, Heishui, Chongzhou, Jiange, Santai, Langzhong, Yanting, Songpan, Cangxi, Lushan, Zhongjiang, Dayi, Baoxing, Nanjiang, Guanghan, Hanyuan, Shimian, Jiuzhaigou) in Sichuan province, 8 (Wen, Wudu district of Longnan city, Kang, Cheng, Hui, Xihe, Liangdang, Zhouqu) in Gansu province, and 4 (Ningqiang, Lueyang, Mian, Chencang district of Baoji city) in Shaanxi province.	(1) 5 cities of Inner Mongolia: Machine and equipment manufacturing (35, 36, 39, 40, 41, 42); Petroleum, chemical, and pharmaceutical manufacturing (25, 26, 27, 28, 29, 30); Ferrous and non-ferrous metallurgy (32, 33); Agricultural product processing (13, 14, 15, 17, 18, 19, 20, 21, 22); Shipbuilding (375); Automobile manufacturing (371, 372, 376, 379); Military (2664, 3751, 4141); Hi-tech (253, 2665, 271, 272, 274, 276, 368, 3761, 3762, 3769, 401, 402, 403, 4041, 4042, 4043, 405, 406, 407, 409, 411, 412, 4141, 4154, 4155, 419, 6211, 6212). (2) 51 counties suffering from Wenchuan earthquake: All the manufacturing sector (6-46) except coke processing (2520) and electrolytic aluminum producing (3316).
4 (January 2009)	Nationwide	All the remaining general VAT taxpayers

*Source:* Authors' compilation from relevant official documents, including *File of the Ministry of Finance of China and the State Administration of Taxation of China No. 156 in 2004*, *File of the Ministry of Finance of China and the State Administration of Taxation of China No. 227 in 2004*, *File of the Ministry of Finance of China and the State Administration of Taxation of China No. 28 in 2005*, *File of the Ministry of Finance of China and the State Administration of Taxation of China No. 75 in 2007*, *File of the Ministry of Finance of China and the State Administration of Taxation of China No. 94 in 2008*, *File of the Ministry of Finance of China and the State Administration of Taxation of China No. 108 in 2008*, and *File of the Ministry of Finance of China and the State Administration of Taxation of China No. 170 in 2008*.

**Table A2: Variable Definitions**

Variable	Definition
$\ln(\text{Investment})$	logarithm of (1+real investment)
Investment/lagged capital	real investment/lagged real capital stock
$\ln(\text{Net investment})$	logarithm of (the difference in the real capital stock between two consecutive years)
$\ln(\text{Investment\_building})$	logarithm of (1+real investment in buildings)
$\ln(\text{Investment\_import})$	logarithm of (1+real value of purchases of imported capital goods)
$\ln(\text{TFP\_OP})$	logarithm of TFP, calculated by the OP method
$\ln(\text{TFP\_LP})$	logarithm of TFP, calculated by the LP method
$\ln(\text{TFP\_OP2})$	logarithm of TFP, calculated by the adjusted OP method (see section IV.F)
$\ln(\text{TFP\_LP2})$	logarithm of TFP, calculated by the adjusted LP method (see section IV.F)
VAT reform	dummy for the VAT reform (1 if the firm is eligible to the VAT reform incentives; 0 otherwise)
Markup	the ratio of the output elasticity for intermediate input to its corresponding expenditure shares in total revenue
Cash flows	(net profit + current year depreciation)/lagged assets, %
$\ln(\text{R\&D per employee})$	logarithm of (1+real R&D expenditures/number of employees)
$\ln(\text{Total assets})$	logarithm of total assets (total assets are in real value)
$\ln(\text{Total sales})$	logarithm of total sales (total sales are in real value)
Profit margin	net profit/total sales, %
Age	firm's age

**Table A3. Firms' Responses to the VAT Reform: Additional Robustness Checks**

	ln(Investment)			ln(TFP_OP)		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Alternative standard error</i>						
VAT reform	0.384*** (0.139)	0.384*** (0.110)	0.384*** (0.136)	0.089** (0.041)	0.089*** (0.027)	0.089** (0.037)
Observations	249,803	249,803	147,902	280,348	280,352	170,879
$R^2$	0.022	0.022	0.022	0.185	0.185	0.185
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes
VAT taxpayer trends	Yes	Yes	Yes	Yes	Yes	Yes
Cluster(province)	Yes	No	No	Yes	No	No
Cluster(industry)	No	Yes	No	No	Yes	No
Two-way clusters	No	No	Yes	No	No	Yes
<i>Panel B. Alternative fixed effects</i>						
VAT reform	0.383*** (0.115)	0.376** (0.192)	0.372* (0.193)	0.091*** (0.029)	0.099** (0.043)	0.100** (0.043)
Observations	249,803	249,803	249,803	280,352	280,348	280,348
$R^2$	0.026	0.041	0.050	0.190	0.195	0.206
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
VAT taxpayer trends	Yes	Yes	Yes	Yes	Yes	Yes
Province-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	No	Yes	Yes	No	Yes	Yes
Province-industry fixed effects	No	No	Yes	No	No	Yes

*Notes:* In Columns (1)-(3) and Columns (4)-(6), the dependent variables are logarithm of investment and OP method of TFP, respectively. In Panel A, we cluster errors at different levels. Two-way clusters represent clustering at both the province and the industry levels. In Panel B, we control different levels of fixed effects, and standard errors clustered at the firm level are in parentheses.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

**Table A4: Dynamics of the Policy Impacts Before and After the VAT Reform**

	ln(Investment)	ln(TFP_OP)	Cash flows	ln(R&D per employee)
	(1)	(2)	(3)	(4)
3 year before	0.058 (0.060)	-0.023 (0.017)	-0.062 (0.247)	0.009 (0.016)
2 year before	0.239** (0.112)	0.026 (0.030)	0.087 (0.447)	0.026 (0.038)
1 year before	0.200 (0.142)	-0.018 (0.037)	-0.183 (0.586)	0.048 (0.050)
Year of VAT reform	0.445*** (0.165)	0.054 (0.042)	0.493 (0.655)	0.143*** (0.054)
1 year after	0.670*** (0.172)	0.094** (0.046)	1.400** (0.700)	0.219*** (0.060)
2 year after	0.548*** (0.197)	0.101** (0.051)	1.196 (0.797)	0.231*** (0.070)
3+ year after	0.666*** (0.235)	0.106* (0.060)	0.695 (0.958)	0.311*** (0.093)
Observations	249803	280352	240374	213863
$R^2$	0.023	0.185	0.019	0.035
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes
VAT taxpayer trends	Yes	Yes	Yes	Yes

*Notes:* This table reports the estimation results for event studies using specification (2). The dependent variable for each estimation is indicated on the top of each column. These results are graphically presented in Figures 2, 3, 5, and 6, respectively. Industry trends indicate quadratic time trends for two-digit industries; and VAT taxpayer trends indicate quadratic time trends for types of VAT taxpayers. Standard errors clustered at the firm level are in parentheses.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

**Table A5: Net Impacts of the VAT Reform on Cash Flows and R&D Expenditures**

	Cash flows			ln(R&D per employee)		
	(1)	(2)	(3)	(4)	(5)	(6)
VAT reform	1.469*** (0.455)	0.698* (0.372)	1.336*** (0.459)	0.105*** (0.037)	0.104*** (0.036)	0.117*** (0.037)
Observations	240374	240234	240374	213863	212274	213863
$R^2$	0.013	0.330	0.018	0.029	0.036	0.034
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	No	Yes	No
Industry trends	No	No	Yes	No	No	Yes
VAT taxpayer trends	No	No	Yes	No	No	Yes

*Notes:* The dependent variable for Columns (1)-(3) is cash flows (the ratio of cash flows to lagged assets), and the dependent variable for Columns (4)-(6) is logarithm of R&D expenditures per employee. Controls indicate quartics in assets, sales, profit margin, and firm age. Industry trends indicate quadratic time trends for two-digit industries; and VAT taxpayer trends indicate quadratic time trends for types of VAT taxpayers. Standard errors are clustered at firm level for all regressions.

\*\*\*Significant at the 1 percent level.

\*Significant at the 10 percent level.

**Table A6: Changes of Industry Markup Before and After 2009**

Industry Code	Mean			Coefficient of Variation		
	05-08	09-12	Change (%)	05-08	09-12	Change (%)
13	1.20	1.08	-9.62	0.69	0.43	-37.28
14	1.26	1.19	-5.18	0.67	0.43	-34.82
15	1.52	1.38	-9.04	0.76	0.41	-46.50
17	1.13	1.11	-2.01	0.52	0.39	-24.28
18	1.25	1.21	-3.67	0.63	0.49	-22.40
19	1.18	1.18	-0.32	0.62	0.45	-27.83
20	1.18	1.11	-6.66	0.65	0.38	-40.87
21	1.21	1.18	-2.30	0.52	0.48	-8.36
22	1.16	1.17	1.39	0.45	0.31	-30.80
23	1.23	1.19	-3.20	0.59	0.56	-4.82
24	1.20	1.10	-7.87	0.65	0.48	-26.72
25	1.08	0.85	-21.14	0.50	0.22	-55.58
26	1.16	1.15	-0.59	0.57	0.38	-32.15
27	1.25	1.34	7.16	0.61	0.45	-26.13
28	1.13	1.14	0.81	0.30	0.21	-29.61
29	1.13	1.12	-0.81	0.50	0.43	-13.80
30	1.16	1.15	-0.82	0.49	0.38	-23.24
31	1.21	1.11	-7.79	0.62	0.45	-26.89
32	1.13	1.11	-1.88	0.45	0.27	-38.63
33	1.05	1.02	-2.98	0.45	0.24	-45.98
34	1.21	1.13	-6.49	0.57	0.37	-34.86
35	1.18	1.16	-1.30	0.51	0.38	-23.81
36	1.20	1.14	-4.64	0.57	0.37	-35.12
37	1.27	1.24	-2.21	0.59	0.46	-22.48
39	1.13	1.12	-0.66	0.48	0.33	-30.23
40	1.26	1.27	0.35	0.58	0.43	-25.38
41	1.22	1.28	4.81	0.56	0.50	-11.14
42	1.24	1.15	-7.58	0.60	0.56	-6.66
43	1.39	1.13	-19.10	0.78	0.41	-47.42

*Notes:* Based on firm-level markup, this table calculates the mean and coefficient of variation of markup for all two-digit manufacturing industries in each year, and takes their respective average values for the years before and after 2009. Industry names and codes are specified as follows: Agricultural & Sideline Foods Processing (13), Food Production (14), Beverage Production (15), Textile Industry (17), Clothes, Shoes & Hat Manufacture (18), Leather, Furs, Down & Related Products (19), Timber Processing, Bamboo, Cane, Palm Fiber & Straw Products (20), Furniture Manufacturing (21), Papermaking & Paper Products (22), Printing & Record Medium Reproduction (23), Cultural, Educational & Sports Articles Production (24), Petroleum Processing, Coking & Nuclear Fuel Processing (25), Raw Chemical Material & Chemical Products (26), Medical & Pharmaceutical Products (27), Chemical Fiber (28), Rubber Products (29), Plastic Products (30), Non-metal Mineral Products (31), Smelting & Pressing of Ferrous Metals (32), Smelting & Pressing of Non-ferrous Metals (33), Metal Products (34), Ordinary Machinery Manufacturing (35), Special Equipment Manufacturing (36), Transport Equipment Manufacturing (37), Electric Machines & Apparatuses Manufacturing (39), Communications Equipment, Computer & Other Electronic Equipment Manufacturing (40), Instruments, Meters, Cultural & Office Machinery Manufacture (41), Craftwork & Other Manufactures (42), Waste Resources and Old Material Recycling & Processing (43).

## Appendix B: Calculating the Real Capital Stock

In their original accounting statements, firms only report the value of their fixed capital stock at original purchase prices, which means that these book values are simply the sum of the nominal values of their fixed capital stock still in use, measured in their respective purchasing years. Thus, the direct use of these nominal values runs the risk of introducing systematic biases related to a firm's age (Brandt et al., 2012).

Below, we follow Brandt et al.'s (2012) method to construct real values of firms' fixed capital stock that are comparable across firms and over time. The procedure begins with estimating the real value of the fixed capital stock for the first year in which the firm appears in our dataset. For the firms established in or after 2005, the first year of our panel, the real value of the fixed capital stock in this start-up year is obtained by deflating the book value in the same year by the industry-level fixed assets investment price deflator. The problem becomes complicated for those firms established before 2005 in our dataset, as we do not have information on a firm's past investments and initial stock of capital. We address this problem by making assumptions about the investment growth and depreciation rates of the firms. In particular, we use information from the China Statistical Yearbooks to construct estimates of the average rate of growth of total investment in fixed assets. We do this at the sector level by provinces, between 1996 and each year from 2005 to 2012.<sup>77</sup> We then use the obtained average growth rates of investment in fixed assets at the province–sector level as the average investment growth rates of the firms in the period between the year of establishment and the first year they appear in the dataset.

Thus, the nominal capital stock of a firm in 2005 or whichever year the firm initially appeared in the dataset is equal to the product of the firm's initial nominal capital stock and  $(1 + r_{ps})^n$ , where  $n$  is the number of years since the firm was established,<sup>78</sup> and  $r$  is the estimated rate of growth in fixed assets investment in province  $p$  in sector  $s$ . Using this equation, we then calculate the nominal capital stock in each year up to 2005 or the first year the firm appeared in the dataset. Annual investment is directly reported by the NTSD. The real capital stock for 2005 or whichever year the firm initially appeared in the dataset is calculated by the perpetual inventory method, assuming a depreciation rate of 9% and deflating annual investment using the industry-level fixed assets investment price deflator. For the years after 2005 or the first year the firm appeared in the dataset, we use the observed change in the firm's nominal capital stock at original purchase prices between years as the estimate of nominal fixed investment. The same rate of depreciation and investment deflator are applied to roll the calculation of firms' real capital stock estimates forward.

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<sup>77</sup>Data on total investment in fixed assets by sectors and provinces are available from 1996 onwards.

<sup>78</sup>We consider 1978 as the start-up year for firms established before 1978. The assumption here is that the nominal capital stock before 1978, the year the open-door policy was adopted in China, has no significant impact on the firms' real capital stock in 2005 and later.