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Source: *American Economic Journal: Economic Policy* , August 2019, Vol. 11, No. 3 (August 2019), pp. 261-291

Published by: American Economic Association

Stable URL: https://www.jstor.org/stable/10.2307/26754073

American Economic Journal: Economic Policy 2019, 11(3): 261–291
https://doi.org/10.1257/pol.20170478

How Do Tax Incentives Affect Investment and Productivity? Firm-Level Evidence from China†

By Yongzheng Liu and Jie Mao*

China initiated a major reform for capital taxation in 2004. Completed in 2009, it introduced permanent tax incentives for firms' investment in fixed assets. We explore a unique firm-level dataset from years 2005–2012 and utilize a quasi-experimental design to test the impacts of the reform on firms' investment and productivity. We find that, on average, the reform raised investment and productivity of the treated firms relative to the control firms by 38.4 percent and 8.9 percent, respectively. We also show that the positive effects tend to be strengthened for firms with financial constraints. (*JEL* D24, D25, G31, H25, O25, P31, P35)

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 nationwide Value-Added Tax (VAT) reform to transform its formerly production-based VAT system into a consumption-based one by allowing deduction of firms' purchases of fixed assets from the VAT bases. This tax incentives reform was initially implemented on a local pilot basis in certain industries in three northeastern provinces of China in 2004. It was then expanded to the said industries in a few other provinces in 2007 and 2008 before its nationwide adoption for all industries at the beginning of 2009.

This reform could have large and heterogeneous effects. Under the new VAT system, the purchase of fixed assets can be deducted from final product sales while calculating a firm's VAT liability. In a way, this makes the reform similar allowing for an immediate write-off for capital expenditure as depreciation allowance in the

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†Go to https://doi.org/10.1257/pol.20170478 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

calculation of corporate income tax liability.[1] This substantially lowers the user cost of capital, generating direct tax incentives for firms to invest. Meanwhile, by reducing tax liability of the firms, the VAT reform brings additional cash flows to them, which may indirectly affect investment behaviors of the firms facing different levels of financial constraints. In particular, for firms that find it difficult or too expensive to obtain external financing, the increment in cash flows and the reduction in the user cost of capital lessen financial constraints. Potentially, this can lead to increases in investment (e.g., Cummins, Hassett, and Hubbard 1996; Hubbard 1998; Hassett and Hubbard 2002; Hsieh and Parker 2007; Guariglia, Liu, and Song 2011; Chen and Guariglia 2013; and Gorodnichenko and Schnitzer 2013). By definition, the return to the marginal investment of a constrained firm is at least the after-tax real interest rate. The VAT reform allows precisely this potentially higher productive investment, since it pulls up internal funds and therefore increases investment of the constrained firms. By contrast, unconstrained firms are always able to fund investment through other means and thus the reform may have less or no impact on unconstrained firms' investment.

In this paper, we examine the incentive effects of the VAT reform on firms' investment and productivity, how it varies across firms with different degrees of financial constraints, and the potential mechanisms of the impacts. By using a unique and largely unexplored dataset containing over 120,000 Chinese firms (annually) and taking advantage of the variation of the tax incentives treatment across firms and over time, we exploit a quasi-experimental setting brought by the reform using a difference-in-difference (DID) approach, where the treatment 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.

Our econometric results suggest that investment and productivity of the firms increased substantially. On average, the reform raised investment and productivity of the treated firms relative to the control firms by 38.4 percent and 8.9 percent, respectively. In addition, we find that the reform led to higher increases in investment and productivity for eligible firms more likely to face financial constraints. Finally, we show that the productivity effect of the reform can be partially explained by its positive impacts on firms' productivity-enhancing investment, cash flows, market size for capital goods, 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' 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 four years before the implementation of the VAT reform to three years and above 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) significantly increases right after the reform got started.

[1] Unlike cutting tax rates that has rarely been used in the United States, temporary accelerated depreciation (or bonus depreciation) has been most frequently used as a tool of investment incentives to stimulate the US economy. In this context, the Chinese VAT reform can be treated as an equivalent action to allow for a permanent and immediate write-off for capital expenditure as depreciation allowance. See Section I and Table A1 in the online Appendix of this paper for a detailed description of the reform.

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' activities; the effect of the reform tends to be larger for firms in industries with a 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 relatively 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, the majority are based on developed countries, the main message being that tax incentives can stimulate investment but may not be cost effective (e.g., Cummins, Hassett, and Hubbard 1996; Devereux and Griffith 1998; Desai and Goolsbee 2004; Edgerton 2010; Mertens and Ravn 2012; Yagan 2015; and Maffini, Xing, and Devereux 2016). Studies addressing investment tax incentives in developing countries are scarce, and the evidence on their effectiveness is even more limited.[2]

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, and 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, Xing, and Devereux 2016; Zwick and Mahon 2017). Our analysis 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 within industries and regions (see Section I for more details).

Third, we provide direct evidence for the effect of tax incentives on firm productivity and its potential mechanisms of impact. Firm productivity is at least as vital as investment in determining aggregate economic growth, and even if tax incentives do stimulate investment, this does not automatically lead to 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, Stotsky, and Ley 2002 for a survey), the net effect of tax incentives on firm productivity 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

[2] Shah (1995) examines the effect of tax incentives using several different approaches for a range of developing countries. Although the study holds a favorable view of certain targeted tax preferences, it concludes that tax incentives significantly erode tax-revenue bases without stimulating much investment. In a more recent study, Klemm and Van Parys (2012) uses 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. However, there is 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.

investment and R&D expenditures (e.g., Auerbach 2002, Hasset and Hubbard 2002, and Gemmell et al. 2018).[3]

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., Hassett and Hubbard 2002, Hsieh and Parker 2007, and Gorodnichenko and Schnitzer 2013), and thus increments in their internal cash flows may help solve this problem. Our analysis adds to this literature and reconfirms the main findings of Zwick and Mahon (2017) by revealing that financially constrained firms are more responsive to the tax incentives, partially because of increases in after-tax cash flows and relaxation of financial constraints faced by the firms.

The most closely related work to our study is the paper by Zhang, Chen, and He (2018), 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, instead of using only data on large firms as they did, we use a large and unexplored dataset that includes a large proportion of small and young firms. This improves the representativeness of our overall sample, and it 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. Given their data limitations, Zhang, Chen, and He (2018) analyzes only the initial stage of the reform in 2004, which involved some pilot industries in the three northeastern provinces of China. 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. Lastly, we shed some light on the possible mechanisms of the reform's impacts.

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

I. 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

[3] However, there is a related literature on the sensitivity of productivity to the corporate income tax rate. In particular, Arnold et al. (2011) studies the direct effect of corporation taxation on firm-productivity growth in 12 European OECD countries over the years 1996 to 2004. Gemmell et al. (2018) builds 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.

1994, the year the current fiscal system was established. An important feature of the system is that it defines two types of VAT taxpayers. The first type is the so-called "*general VAT taxpayers*," for whom total sales must exceed a certain amount.[4] For the general VAT taxpayers, the tax base is the usual value-added—the difference between total value of sales and 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 any cost of material inputs. That is, the tax regime does not allow for any deductions of cost of material inputs while calculating a small-scale VAT taxpayer's VAT liability. Additionally, these two types of VAT taxpayers are characterized by different tax rate settings. The standard VAT rate for the general VAT taxpayers is 17 percent, together with a reduced tax rate of 13 percent for some special products such as agricultural products. As a comparison, the standard tax rate for small-scale VAT taxpayers is either 6 percent or 4 percent, depending on the sector involved.[5]

Since its establishment across the nation in 1994, VAT has become the most important source of revenue for the Chinese government. As depicted in Figure 1, the VAT revenue was ¥230.83 billion in 1994, accounting for 45 percent of total tax revenue. It increased steadily thereafter, reaching ¥3,110.95 billion in 2015 that was 25 percent of total tax revenue. Notably, this figure is still 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 final product while calculating a firm's VAT liability, China's VAT system was initially designed as a *production-based* one. Thus, purchases of capital goods were not allowed to be deducted from the VAT bases, resulting in the double taxation on capital goods—the first is as final products of their producers, and the second is as intermediate inputs for their users. 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. That is, the continuing decline of the share of government revenue in GDP, and the continuing increase in the inflation rate. It was hoped that establishing a production-based VAT system would help preserve government tax revenue and restrain firms' investment in a then overheating economy (Zhang, Chen, and He 2018). 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 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

[4] More specifically, to be recognized as a general VAT taxpayer, the annual turnover must exceed: (i) ¥1 million (¥500,000 after 2008) if the taxpayer is engaged in production/manufacturing activities, (ii) ¥1.8 million (¥0.8 million after 2008) if the taxpayer is engaged in wholesaling and retailing activities, or (iii) ¥5 million if the taxpayer is engaged in taxable service activities.

[5] In general, the prescribed time limit for calculating and paying VAT is one month. Additionally, based on the amount of VAT liability of the taxpayers, there are six other types of the prescribed time limit for paying VAT, i.e., one day, three days, five days, ten days, fifteen days, and one-quarter of which the prescribed time limit of one-quarter only applies to the small-scale taxpayer (see Article 23 of the *Provisional Regulations of the People's Republic of China on Value-Added Tax*).

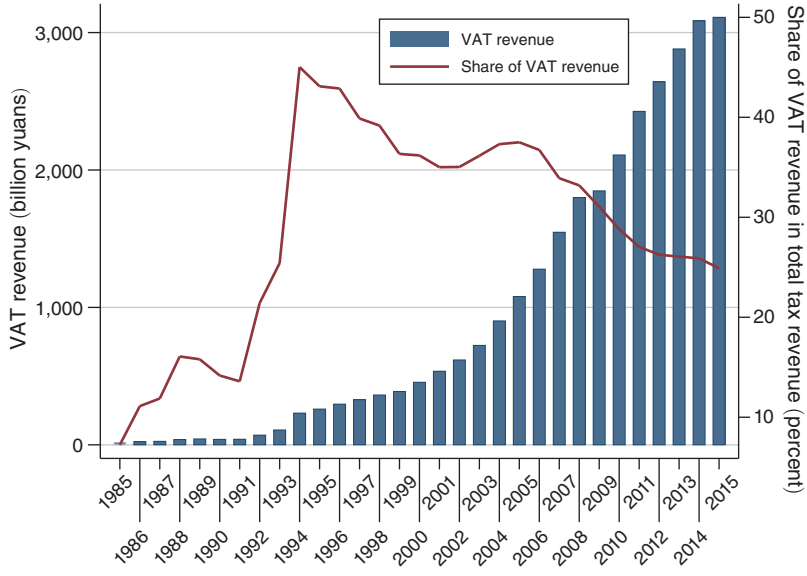

FIGURE 1. SIZE AND SHARE OF THE VAT REVENUE IN CHINA

Source: The National Bureau of Statistics of China

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 until 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 was hoped that the reform would 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 northeastern provinces in July 2004 (i.e., the first stage of the reform). For these pilot industries, 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. By its very nature, this would substantially lower the user cost of fixed investment on equipment by 17 percent. 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 in July 2007, was expanded to another 26 cities of 6 middle provinces, in which general VAT taxpayers in 8 industries (not completely the same as those in the first stage of the reform) were included.

In July 2008, the third stage of the reform was initiated to cover general VAT taxpayers in 8 industries in another 5 cities of Inner Mongolia, and nearly all manufacturing industries in 51 counties suffering from the aftermath of the *Wenchuan* earthquake in 2008.

Finally, in the face of external shocks from the 2008 global 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 in January 2009, covering all general VAT taxpayers in all industries in the country. For detailed information about the coverages of industries and regions in each stage of the reform, see Table A1 in the online Appendix.

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 DID 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.

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 is that building services suppliers are not considered as VAT taxpayers before 2016, and so investment in buildings do not generate VAT invoices that are necessary for claiming 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. Therefore, these two types of ineligible investment provide good sources for conducting placebo tests to our main results (see Section IIID for further discussion).

II. 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 of China (SAT-MOF) based on the stratified random sampling method. The purposes of the dataset are for the Chinese authority to better control for tax base information and evaluate the impacts of tax policies. This leads to a 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.[6]

The NTSD was initiated in 1985; however, for consistency with regard to the data sampling methods and data availability, we only analyze the data from 2005 to 2012.[7] Each year the SAT-MOF uses all actual taxpayers (excluding self-employed entrepreneurs and new start-up firms) as the population of the surveyed sample, and based on it, produces a stratified sample of surveyed taxpayers (firms). Stratification occurs by total sales, industry, and types of taxpayers.[8] 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.

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 (e.g., total assets, investment, 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 (Brandt et al. 2017). Lastly, China has maintained the credit-invoice or invoice-based method in calculating VAT revenues. This means that sales transactions of the firms are taxed with the customer informed of the VAT on the transaction, and firms may receive a credit for VAT paid on input materials, services, and investment on fixed assets (after the VAT reform). Thus, every transaction on the purchases of fixed assets will be recorded by a VAT invoice, which makes it practically easy to audit and punish a firm that lies about its investment amount. Additionally, to avoid tax fraud through counterfeiting VAT invoices or invoicing false transactions, the SAT of China has developed a nationwide network system (the so-called "Golden Tax Project") to monitor information on the VAT invoices

[6]The NTSD provides a wide range of information on firms' activities, including basic firm characteristics, detailed and disaggregated tax information for 17 (out of 18) tax instruments, and numerous operational and financial variables listed in the main accounting statements of these firms.

[7]The collection and data sampling methods of the NTSD experienced significant changes after 2005. In particular, in 2005, a special electronic survey system was established, enabling surveyed firms to fill in and submit their answers to the questionnaire online. At the same time, the electronic system has several built-in functions to automatically check the accuracy and the completeness of the survey before the approval for data submission to the system. Before 2005, the surveyed firms were asked to fill in the answers in their local computers. Later on, all the information for different firms was compiled by local tax officials manually, which frequently led to unintentional mistakes in the data-input process. Thus, the electronic system greatly improved the quality of the data after 2005. Additionally, thanks to the successful launch of the electronic system, in 2005, the SAT-MOF started to apply a uniform-stratified random sampling method (see description in the text) to all firms in the nation. Before 2005, it was under certain discretion of local tax agents in different regions for the selection of the surveyed firms, which potentially undermined the representativeness of the sample.

[8]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 of the service.

since 1994. Under this system, VAT invoices are created automatically through an anti-counterfeiting invoicing subsystem, and they are investigated by the inspection subsystem in the next stage of business for eligibility of tax credit.

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 percent 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 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 ¥5 million (about US$650,000) or more. Third, the dataset covers a full range of sectors (including agriculture, mining, manufacturing, building, and service sectors) in the economy, 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 enables us to explore the effects of the VAT reform over different stages of its implementation.

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. In particular, we convert the new industry classification codes after 2011 to that the year before. 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 1 percentiles of the distribution of variables included in the regressions to avoid misreporting. Finally, imposing these restrictions gives us a sample of 582,615 observations 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 online Appendix provides detailed definitions of the variables, and summary statistics for the manufacturing sample are given in Table 1.

III. 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

Table 1—Summary Statistics: Manufacturing Firms

Variables	Observations	Mean	SD	Min	Max
ln(Investment)	249,803	4.348	3.924	0	18.364
Investment/lagged capital, percent	243,411	5.638	13.313	0	87.600
ln(Net investment)	232,791	4.144	3.730	0	18.467
Net investment/lagged capital, percent	228,236	4.884	9.605	0	64.298
ln(Investment in buildings)	250,076	1.248	2.931	0	17.970
ln(Investment in imported capital goods)	250,135	0.108	0.949	0	14.928
ln(TFP, OP method)	280,352	2.397	1.535	−1.426	7.558
ln(TFP, LP method)	280,352	3.662	1.539	−0.528	9.471
ln(TFP, adjusted OP method)	280,352	−2.941	3.231	−12.444	9.769
ln(TFP, adjusted LP method)	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/lagged capital, percent	240,374	7.594	13.168	−31.679	116.600
R&D expenditures/lagged capital, percent	208,055	0.361	1.340	0	8.223
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, percent	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 terms. For definitions of these variables, see Table A2 in the online Appendix.

taxpayers in certain industries in selected regions. However, they were not available to either, other general VAT taxpayers belonging to ineligible industries and regions or to all small-scale VAT taxpayers regardless of their belonging industries and regions. In the last stage of the reform, which extends the largest expansion of the reform to cover all general VAT taxpayers in all industries, identification relies solely on the variation across firm types, 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 DID regression of the form

$$y_{it} = \alpha + \beta VAT_{it} + \gamma \mathbf{X}_{it} + \mu_i + \psi_t + \varepsilon_{it}, \tag{1}$$

where y_{it} is the investment or productivity of firm i in year t; VAT_{it} is a dummy variable indicating the implementation of the VAT reform for firm i in year t. That is, VAT_{it} equals zero for the years before the VAT reform was introduced for a firm and one for the first year and all the subsequent years of the VAT reform,[9] $\mathbf{X}_{it}$ is a set of time-varying controls at the firm level, μ_i are firm fixed effects, and ψ_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 for types of VAT taxpayers to isolate the possible

[9] We use the detailed information regarding the VAT reform that we described in Section I and Table A1 in the online Appendix to identify the treatment status for each firm in each year while creating this variable. Since the general prescribed time limit for paying VAT is one month and different stages of the VAT reform were implemented on either July 1 or January 1 of the year, we define the implementation year of the reform as the initial treatment year for the firms.

confounding fact that the VAT reform may be enacted in response to these trends. In all regressions, the standard errors are clustered by firm.[10]

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 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, LaLonde, and Sullivan 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,

$$y_{it} = \alpha + \beta_k \sum_{k=-4}^{\geq 3} D_{t_{i0}}^{k} + \gamma \mathbf{X}_{it} + \mu_i + \psi_t + \varepsilon_{it}, \tag{2}$$

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 a firm became eligible to the VAT incentives; $D_{t_{i0}}^{k}$ is a series of dummies indicating whether $t - t_{i0} = k$, with $k = -4, -3, -2, 0, 1, 2, \geq 3$.[11] The omitted time category is $k = -1$, so that the estimated effects β_k are relative to the period 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 with an opportunity to check whether there is any evidence of preexisting 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 dynamics 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 (see, for example, Hsieh and Parker 2007; Poncet, Steingress, and Vandenbussche 2010; Arnold et al. 2011; Liu and Lu 2015; and Zhang, Chen, and He 2018). 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.[12] This is mainly due to

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

[11] 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.

[12] 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).

the reason that the investment rate method requires information on the scaling variable (i.e., lagged real capital stock), which would reduce our sample frame, especially for small firms.[13] In addition, measuring the investment rate involves the construction of a series of real capital stock for the firms, which is challenging and vulnerable to measurement error. Specifically, 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 assets (or 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. Thus, the direct use of these nominal values runs the risk of introducing systematic biases related to a firm's age (Brandt, Van Biesebroeck, and Zhang 2012).

To obtain the real values of capital stock, we follow Brandt, Van Biesebroeck, and Zhang (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 (see online Appendix B for the detailed procedure for calculating real capital stock). The potential measurement error in the calculation of real capital stock just transforms into measurement error in the investment rate. Finally, it is noted that data on the gross investment in fixed assets are reported directly by the NTSD, and we deflate them by the province-level fixed assets investment price index to obtain real values.

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

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

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.[14] 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 and based on firm-level data. That is,

$$TFP_{it} = \ln Y_{it}^{j} - \ln \hat{Y}_{it}^{j}. \tag{4}$$

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

[13] 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 the scaling variable; for another, requiring two consecutive years of data to obtain lagged values further reduces our sample.

[14] 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 online Appendix B. Material input deflators are calculated using the output deflators and information from the 2002 National Input-Output (IO) table.

future profitability. Since productivity is unobserved, it enters the error term, which introduces a positive correlation between the error term and labor input, and hence rendering the OLS estimates inconsistent and biased. 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) proposes 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 in the sense that there are substantial adjustment costs for investment, resulting in a large number of "zero" investment. Thus, investment may not be a strong proxy for productivity shocks. Levinsohn and Petrin (2003) takes 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 (see Section IIIE for more details).

C. *Main Results*

Table 2 presents the results of our baseline specification (1), alternatively using firms' investment (panels A and B) and productivity (panel C) as the dependent variables. Each column of the table represents the estimate from a separate regression, with only the coefficient of the VAT reform (and its clustered standard error) reported.

We start off the estimation by controlling for firm-specific fixed effects and year-specific fixed effects in column 1 of panel A, where the logarithm of investment is used as the dependent variable. It turns out that the VAT reform is positively and statistically significantly associated with firm investment, which is consistent with 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 positive and statistically significant at the 1 percent level. Column 3 includes quadratic time trends and their interaction terms separately with two-digit industry dummies and types of the VAT taxpayer dummy, 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 incentives effect of the reform are quite robust across these alternative specifications. Quantitatively, Column 3 indicates a semi-elasticity of investment with respect to the VAT reform of 0.384, implying that relative to the control firms, the reform led to a 38.4 log points increase in investment of the treated firms.

We prefer the logarithm measure of investment for the reasons explained in Section IIIB. However, many empirical studies in the literature have used investment rate as the measure of firm investment (e.g., Desai and Goolsbee 2004; Edgerton 2010; and

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

	(1)	(2)	(3)
Panel A. LHS variable is ln(Investment)			
VAT reform	0.350	0.333	0.384
	(0.113)	(0.111)	(0.114)
Observations	249,803	247,957	249,803
R^2	0.019	0.036	0.022
Panel B. LHS variable is investment/lagged capital			
VAT reform	0.970	1.013	1.309
	(0.503)	(0.503)	(0.504)
Observations	243,411	242,707	243,411
R^2	0.014	0.029	0.017
Panel C. LHS variable is ln(TFP, OP method)			
VAT reform	0.095	0.058	0.089
	(0.030)	(0.025)	(0.029)
Observations	280,352	277,309	280,352
R^2	0.022	0.235	0.185
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	No	Yes	No
Industry trends	No	No	Yes
VAT taxpayer trends	No	No	Yes

Notes: In panels A, B, and C, the dependent variables are the logarithm of investment, investment/lagged capital, 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 and their interaction terms separately with two-digit industry dummies and types of the VAT taxpayer dummy to the specification. FE stands for "fixed effects." Standard errors are clustered at the firm level for all regressions.

Maffini, Xing, and Devereux 2016). To provide a comparison with this literature, we substitute investment scaled by lagged capital for the logarithm of investment and re-estimate specification (1). The results are presented in panel B of Table 2. Like in our log investment model, in all three specifications, the firms exhibit positive and significant investment sensitivities to the VAT reform. The estimated coefficient of VAT reform in the preferred specification in column 3 is 1.309. This implies that after the reform, investment rate of the treated firms increased on average by 1.309 percentage points more than that of the control firms. Being evaluated at the average investment rate of treated firms prior to the reform (i.e., 5.118 percent), this translates into an elasticity of investment with respect to the user cost of capital around -1.5.[15] This is a sizable effect of the policy, which is higher than the consensus

[15] Note that the tax component of the user cost of capital is defined as $(1 - t \times z)/(1 - t)$, where t is the tax rate (i.e., 17 percent, which is the standard VAT tax rate in China), and z is the present discounted value of one-dollar depreciation deduction of capital investment. The VAT reform, by allowing deduction of the purchase of fixed assets from tax liability, can be treated as the equivalent to an immediate write-off of plant and equipment expenditures at the time of purchase as depreciation. Thus, assuming other parameters constant, the reform changed z from 0 to 1, and the tax component of the user cost of capital from $1/(1 - 0.17) = 1/0.83$ to $(1 - 0.17)/(1 - 0.17) = 1$. Based on a mean investment rate of treated firms prior to the reform (i.e., 5.118 percent), the elasticity of investment with respect to the user cost of capital can be calculated as $(1.309/5.118)/((1 - 1/0.83)/(1/0.83)) = -1.5$.

range of −0.5 to −1 concluded by Hassett and Hubbard's (2002) review for early studies. Yet, it falls within the typical values found in some most recent literature around −1.6 (e.g., Zwick and Mahon 2017; and Zhang, Chen, and He 2018).

In panel C of Table 2, we replace the dependent variable with the logarithm of firms' TFP. This helps 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 firm productivity. Taking column 3 as a benchmark, the estimated coefficient of the VAT reform is 0.089, implying that compared with the control firms, the reform brought an 8.9 log points increase in productivity, which is approximately a productivity gain of 8.9 percent for the treated firms.

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 pretreatment period.[16] For instance, if 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 column 3 of Table 2 with a series of year-wise dummies for a window of several periods around the VAT reform event. Figure 2 plots the estimated coefficients on these dummies and shows the 95 percent level confidence intervals, while the corresponding regression results can be found in Table A4 of the online Appendix. These are interpreted as changes in the treated firms' investment and productivity, relative to the control firms, as compared to the period prior to 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 trends in unobservables.

As a step further, we explore whether certain stages of the reform, by year of reform implementation, are driving the overall average results. Figure 3 depicts the dynamics of firms' responses to each stage of the VAT reform, alternatively using logarithm of investment and logarithm of productivity as the dependent variable. In general, it appears as though firms becoming eligible for the tax incentives in all three different stages contribute significantly to the overall average estimates (note that our dataset starts in 2005, so the number of years leading up to different stages of the VAT reform varies across panels in Figure 3). The very close estimates for 2009 reform in panels C and F of Figure 3, compared to the overall average estimates in panels A and C of Figure 2, likely derive from that being the year in which the greatest number of firms across the nation became eligible for the tax incentives treatment. The larger point estimates for the 2008 reform in panel B of Figure 3 for

[16] By depicting the evolution of logarithmic investment and productivity for the treated and control firms, Figure C1 in the online Appendix provides descriptive evidence on the validity of this assumption. See online Appendix C for a more detailed discussion.

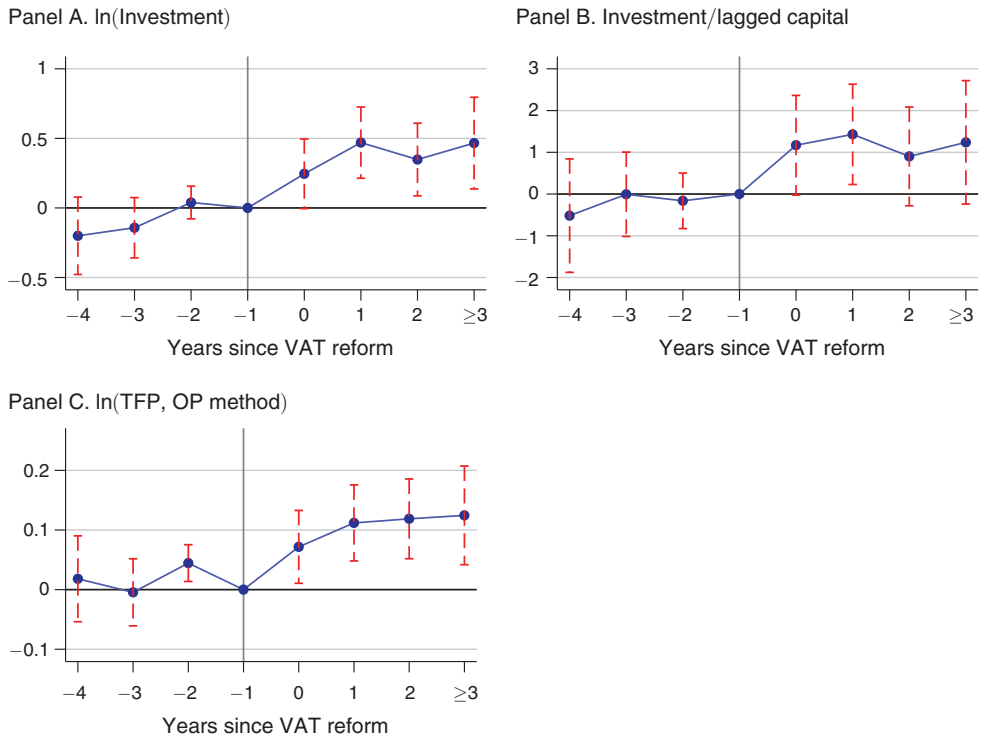

FIGURE 2. EVENT STUDY ESTIMATES: IMPACTS OF THE VAT REFORM ON FIRMS' INVESTMENT AND PRODUCTIVITY

Notes: This figure plots event study estimates and corresponding 95 percent confidence intervals from regressions of specification (2). Dependent variables are the logarithm of investment (panel A), investment rate (panel B), and productivity (panel C). The horizontal axis measures the number of years since the VAT reform. The right end point on the graph is binned so that ≥3 is a bin for years +3 to +8. The coefficient for the year before the VAT reform is normalized to zero. All estimated coefficients can be interpreted as changes in investment (productivity) compared to the year before the VAT reform (indicated by the vertical line) conditional on firm fixed effects, year fixed effects, and quadratic time trends and their interaction terms separately with two-digit industry dummies and types of the VAT taxpayer dummy. Standard errors are clustered at the firm level.

Source: Authors' calculations

investment specification may simply reflect the institutional fact that this stage of the reform mainly involved manufacturing firms in the 51 counties suffering from the aftermath of the *Wenchuan* earthquake. Hence, these larger point estimates may simultaneously reflect firms' need to rebuild equipment in the post-disaster period. By contrast, as shown in panel E of Figure 3 for productivity specification, owing to the destruction of the earthquake on firm productivity, the positive point estimates of the 2008 reform do not appear to be statistically significant. Finally, in every panel of Figure 3, the increase in investment and productivity emerges only after the reform was in place.

D. *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.

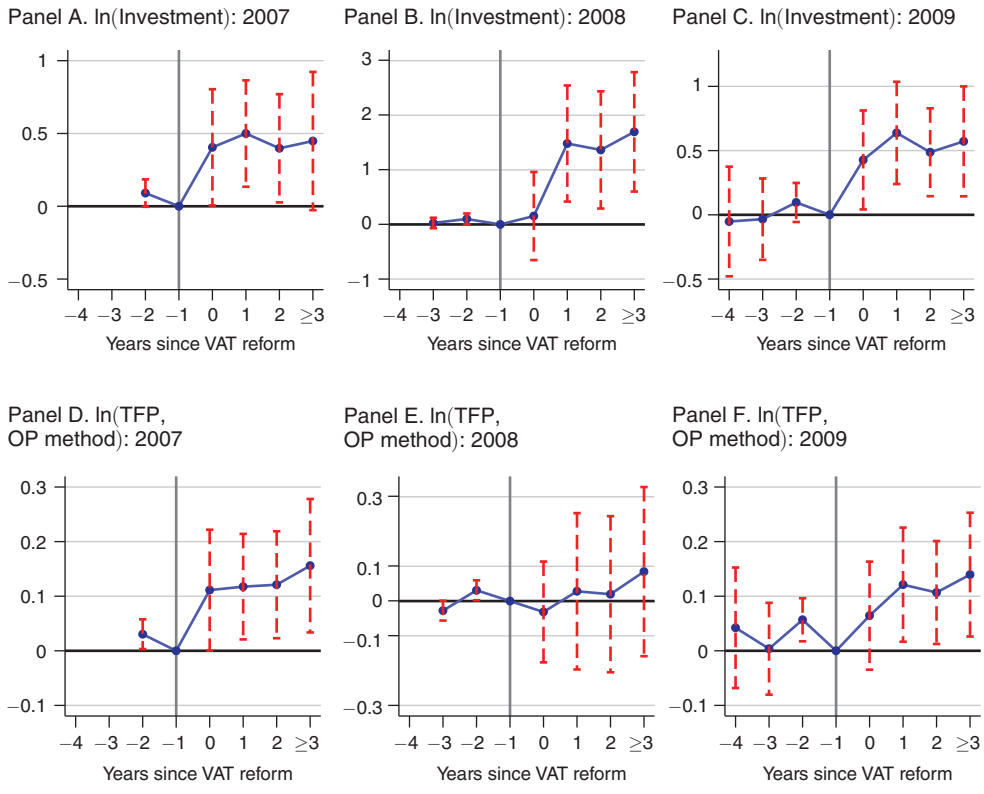

FIGURE 3. EVENT STUDY ESTIMATES: IMPACTS OF DIFFERENT STAGES OF THE VAT REFORM ON FIRMS' INVESTMENT AND PRODUCTIVITY

Notes: Panels A–C (D–F) present the dynamics of firms' investment (productivity) before and after the 2007, 2008, and 2009 VAT reform, respectively. In particular, this figure plots event study estimates and corresponding 95 percent confidence intervals from regressions of specification (2). Dependent variables are the logarithm of investment (panels A–C) and the logarithm of productivity (panels D–F). The horizontal axis measures the number of years since the VAT reform (note that our dataset starts in 2005, so the number of years leading up to different stages of the VAT reform varies across panels). The right end point on the graph is binned so that ≥3 is a bin for years +3 to +8. The coefficient for the year before the VAT reform is normalized to zero. All estimated coefficients can be interpreted as changes in investment (productivity) compared to the year before the VAT reform (indicated by the vertical line) conditional on firm fixed effects, year fixed effects, and quadratic time trends and their interaction terms separately with two-digit industry dummies and types of the VAT taxpayer dummy. Standard errors are clustered at the firm level.

Source: Authors' calculations

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 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 important, the majority of firms in the service sector are recognized as business tax taxpayers rather than VAT taxpayers, 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 to conduct 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. More specifically, in columns 3 and 6, we exclude from the service sector those industries (i.e., wholesale, retail, and press and publishing industries) that are more likely to be affected by the VAT reform. These three excluded industries are mixed with both VAT taxpayers and business tax taxpayers and so may still be subject to the VAT incentives. We find the coefficients of the VAT reform to be negative and generally statistically insignificant in columns 1–3 for the investment specification.[17] In addition, while the signs of the VAT reform vary across different sector samples, they are all statistically insignificant in columns 4–6 for 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 less on fixed assets may be treated as a placebo group when compared to their counterparts. Thus, we calculate an index of fixed assets dependency (i.e., fixed assets/total outputs) for two-digit industries based on national-level data from the China Statistical Yearbook for 2003 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, given the concern that serial correlation can bias standard errors in the baseline DID analysis and hence resulting in an over-rejection of the null hypothesis of no effect (Bertrand, Duflo, and Mullainathan 2004), we follow the literature to implement a nonparametric permutation test by randomly assigning (falsifying) the treatment status of the VAT reform to firms (see, for example, Chetty, Looney, and Kroft 2009 and Mastrobuoni and Pinotti 2015). In particular, since the reform was introduced at different times in different industries and regions, and given the

[17] These negative coefficients are reasonably large in magnitude, which may imply a corresponding crowding effect of the VAT reform on non-manufacturing industries. However, it is also noted that the sizable estimated coefficient for the agriculture sectors might be less accurate due to the very small size of the sample for this sector. A full investigation of all these points is beyond the scope of this paper.

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

	ln(Investment in buildings)			ln(Investment in imported capital goods)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Ineligible investment						
VAT reform	0.074	0.070	0.113	−0.061	−0.062	−0.058
	(0.138)	(0.138)	(0.142)	(0.064)	(0.064)	(0.066)
Controls	No	Yes	No	No	Yes	No
Industry trends	No	No	Yes	No	No	Yes
VAT taxpayer trends	No	No	Yes	No	No	Yes
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
	ln(Investment)			ln(TFP, OP method)		
	Agriculture	Service	Sub-service	Agriculture	Service	Sub-service
Panel B. Placebo sectors						
VAT reform	−0.866	−0.258	−0.321	0.510	0.027	−0.082
	(1.718)	(0.152)	(0.316)	(0.609)	(0.046)	(0.089)
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes
VAT taxpayer trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,397	224,560	90,749	3,171	259,073	96,634
R^2	0.070	0.030	0.046	0.058	0.143	0.263
	ln(Investment)			ln(TFP, OP method)		
	Low	Medium	High	Low	Medium	High
Panel C. Fixed-asset dependency						
VAT reform	0.301	0.325	0.541	0.080	0.073	0.130
	(0.253)	(0.161)	(0.255)	(0.068)	(0.039)	(0.058)
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes
VAT taxpayer trends	Yes	Yes	Yes	Yes	Yes	Yes
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

Notes: The dependent variable is indicated on the top of each column. In panel A, ln(Investment in buildings) and ln(Investment in imported capital goods) represent the logarithm of investment in the (factory) building and purchases of imported capital goods, respectively. In panel B, "agriculture," "service," and "sub-service" represent firms in the agriculture sector, service sector, and service sector excluding wholesale, retail, and press and publishing industries, respectively. In panel C, we split the sample based on an indicator of the fixed-asset dependency of the industries. "Low," "medium," and "high" represent firms belonging to those industries that have relatively low, medium, and high dependency on fixed assets for production, respectively. 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 interacted with two-digit industry dummies, and VAT taxpayer trends indicate quadratic time trends interacted with types of the VAT taxpayer dummy. Standard errors are clustered at the firm level for all regressions.

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.[18] 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 (i.e., column 3 of Table 2), using the false VAT reform variable, and store the estimates. We repeat this exercise 500

[18] 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.

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 apparent impact on firms' performance. Additionally, if the actual VAT reform did have a significantly positive effect on firms' performance, we would expect the actual estimated coefficient to be in the upper tail of the distribution for the estimated placebo effects and so an unusually high level of firms' investment and productivity indicating a rejection of the null hypothesis of no effect. Meanwhile, by making no parametric assumptions about the error structure, this test does not suffer from the over-rejection bias of the *t*-test (Chetty, Looney, and Kroft 2009).

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 online Appendix Figure A1. As expected, the distributions of the estimated coefficients on the placebo VAT reform variable are clearly centered around zero with a small standard deviation, suggesting that there is no apparent effect with the randomly constructed VAT reform variable. More important, the benchmark estimates in panels A and C of Table 2, indicated by vertical red lines at the corresponding values of 0.384 and 0.089 in online Appendix Figure A1, are located outside the upper tail of placebo coefficients estimated in our simulation exercise. This confirms that the VAT reform led to an unusual increase of firms' investment and productivity.

E. *Other Robustness Checks*

In order to further test for the robustness of the basic results, we conduct sensitivity analysis along three dimensions. First, since investment is directly reported in the NTSD, 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 the difference in the real capital stocks between year t and year $t-1$. The results in columns 1 and 2 of Table 4 confirm 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 (De Loecker 2011, Van Beveren 2012). 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.[19] We alternatively denote

[19] 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). 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

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

	ln(Net investment) (1)	Net investment/ lagged capital (2)	ln(TFP, LP method) (3)	ln(TFP, adjusted OP method) (4)	ln(TFP, adjusted LP method) (5)
Panel A. Alternative measures of investment and productivity					
VAT reform	0.922	0.781	0.096	0.066	0.078
	(0.169)	(0.436)	(0.029)	(0.029)	(0.029)
Observations	232,791	228,236	280,352	280,352	280,352
R^2	0.219	0.017	0.135	0.753	0.519

	ln(Investment)			ln(TFP, OP method)		
	Subsample 1 (6)	Subsample 2 (7)	Subsample 3 (8)	Subsample 1 (9)	Subsample 2 (10)	Subsample 3 (11)
Panel B. Alternative samples						
VAT reform	0.386	0.371	0.338	0.092	0.082	0.067
	(0.117)	(0.115)	(0.124)	(0.030)	(0.029)	(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

	ln(Investment)			ln(TFP, OP method)		
	(12)	(13)	(14)	(15)	(16)	(17)
Panel C. Alternative fixed effects						
VAT reform	0.383	0.376	0.372	0.091	0.099	0.100
	(0.115)	(0.192)	(0.193)	(0.029)	(0.043)	(0.043)
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-year FE	No	Yes	Yes	No	Yes	Yes
Province-industry FE	No	No	Yes	No	No	Yes
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

Notes: The dependent variable is indicated on the top of each column. Additionally, ln(Net investment) is derived as the logarithm of the difference in the real capital stocks between year t and year $t-1$; net investment/lagged capital is defined as the ratio of net investment to lagged capital stocks; ln(TFP, LP method) is the logarithm of the standard LP method of TFP, while ln(TFP, adjusted OP method) and ln(TFP, adjusted LP method) represent the logarithm of the adjusted OP and LP methods of TFP (see Section IIIE), 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 percent of their total sales for export), respectively. FE stands for "fixed effects." Regressions in panels A and B include firm fixed effects, year fixed effects, and quadratic time trends and their interaction terms separately with two-digit industry dummies and types of the VAT taxpayer dummy. Regressions in panel C include firm fixed effects and quadratic time trends and their interaction terms with types of the VAT taxpayer dummy. Standard errors are clustered at the firm level for all regressions.

these measures of firm productivity as ln(TFP, adjusted OP method) and ln(TFP, adjusted LP method). 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 (denoted as "subsample 1"). The rationale here is that disasters can directly affect subsequent

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.

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 (denoted as "subsample 2"). 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 aspect is to restrict our sample to non-exporting firms (denoted as "subsample 3"). Since the nationwide 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.[20] Panel B of Table 4 suggests that across all reduced samples, alternatively considering firms' investment and productivity, 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 different levels of fixed effects and by alternatively considering standard errors at different cluster levels. Particularly, in panel C of Table 4, we alternatively estimate models by continuously adding with (two-digit) industry-by-year, province-by-year, and province-by-(two-digit) industry fixed effects. Instead, Table A3 in the online Appendix provides the baseline estimates in Table 2 with standard errors clustering at province, industry, and both province and industry, respectively. With all these alternative specifications, we obtain results that are similar to the baseline results.

IV. Effect Heterogeneity: The Role of Financial Constraints

In this section, we explore the extent to which the impact of VAT reform may vary across firms with different degrees of accessibility to external finance. 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 (e.g., Chaney, Sraer, and Thesmar 2012; and Zwick and Mahon 2017), while the last one is well rooted in the Chinese institutional context (e.g., Poncet, Steingress, and Vandenbussche 2010).

[20] Non-exporting firms are defined as those firms with less than 10 percent 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 percent and 20 percent, respectively). The results are largely unchanged.

Table 5—Firms' Response to the VAT Reform: Effect Heterogeneity

	ln(Investment)		ln(TFP, OP method)	
	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Panel A. Dividend payment				
VAT reform	0.596	0.213	0.178	0.017
	(0.177)	(0.151)	(0.045)	(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
	Small	Large	Small	Large
Panel B. Firm size				
VAT reform	0.827	0.289	0.179	0.047
	(0.312)	(0.166)	(0.080)	(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
	Non-SOEs	SOEs	Non-SOEs	SOEs
Panel C. Firm ownership				
VAT reform	0.464	−0.183	0.134	−0.099
	(0.129)	(0.360)	(0.032)	(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 of 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 and their interaction terms separately with two-digit industry dummies and types of the VAT taxpayer dummy. Standard errors are clustered at the firm level for all regressions.

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 distribution to investors, which are the base of actual dividend payment of the firms. Thus, we 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 reform on investment for firms with no available profits for distribution to investors (and so a 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 a relatively low level of financial constraints). The equality test also shows that the two estimates are statistically and significantly different with a p-value of 0.100.

Meanwhile, the difference of the average treatment effect on firm productivity is even more 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.[21]

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.[22] 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 the one-tailed test.

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 (e.g., Poncet, Steingress, and Vandenbussche 2010). Following this insight, we classify our sample into two groups, namely SOEs and non-SOEs, and re-estimate the model.[23] 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 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 reduction of the user cost of capital and the increment in cash flows brought about by the VAT reform potentially benefit the initially constrained firms to a greater extent.

[21] It is noted that for firms with positive available profits for distribution to investors, it may not necessarily 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.

[22] Given the fact that small firms are having a lot of zero investment, our log investment model may overestimate the sensitivity of firms' response to the tax incentives when investment moved from zero to a positive number. To relieve this concern and also serve as a robustness check, Table A5 in the online Appendix reports the corresponding results for small firms versus large firms by using investment rate as the dependent variable. The results are quite consistent with a statistically significant coefficient of 4.222 for the small firms and a statistically insignificant coefficient of 0.863 for the large firms.

[23] 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.

V. Mechanisms of the Impact

A. *The Impacts of the VAT Reform on R&D Expenditures and Cash Flows*

Theoretically, the impact of the VAT reform is more directly connected to firms' investment decisions due to the tax incentives treatment on fixed assets investment. However, what are the mechanisms by which the VAT reform leads to improved productivity of the firms? The direct mechanism may be that the increased investment brought about by the VAT reform replaced outdated capital, leading to the productivity-enhancing effect. Although our dataset does not allow us to explicitly identify the productivity nature of the increased investment, we do shed light on this mechanism by looking at the reform's impact on R&D expenditures of the firms. Under the Chinese Accounting Standard, R&D expenditures are categorized as a type of "administrative expense," and a large part of it includes the salaries and wages of R&D personnel in addition to the expenses on construction, use, maintenance, and depreciation of R&D-related fixed assets. Thus, R&D expenditures of the Chinese firms are at least partially complementary to the investment on R&D-related fixed assets, which is one of the sources of productivity growth.

To explore this point, we re-estimate specifications (1) and (2), where the ratio of R&D expenditures to lagged capital is treated as an outcome variable. The estimated coefficients for specification (1) are presented in columns 1–3 of Table A6 in the online Appendix, showing that firms responded to the VAT reform by increasing their R&D expenditures. Meanwhile, the dynamic estimation results for specification (2) are displayed in panel A of Figure 4, where it shows that the increase in R&D expenditures does not occur before the implementation of the VAT reform. The positive effect of the policy changes on R&D expenditures is realized during and after the reform year. Given the fact that R&D expenditures are complementary to the investment on R&D-related fixed assets, these results provide tentative evidence that the increased investment in the post-reform period is productivity-enhancing.

Alternatively, it is also possible that the VAT reform may indirectly affect firms' productivity through its impact on firms' internal funds accumulation. Thanks to the reduction of tax liability, the VAT reform may bring additional cash flows to firms, which enables them to finance productivity-enhancing activities (Chen and Guariglia 2013; Krishnan, Nandy, and Puri 2014). To capture this point, we follow the previously analytical framework to estimate the impact of the VAT reform on after-tax cash flows (measured as the sum of net profit after tax and depreciation scaled by lagged capital). Once again, we note that the estimated coefficients of the VAT reform are positive and statistically significant in columns 4–6 of Table A6 in the online Appendix, and cash flows increased only after the implementation of the reform in panel B of Figure 4.

B. *The Impacts of the VAT Reform on Equipment Market Demand*

Productivity could also increase for other reasons. Among them, market size is often deemed as a prerequisite for firms to adopt productivity-enhancing technologies (Impullitti, Kneller, and McGowan 2017). This is because the increases in

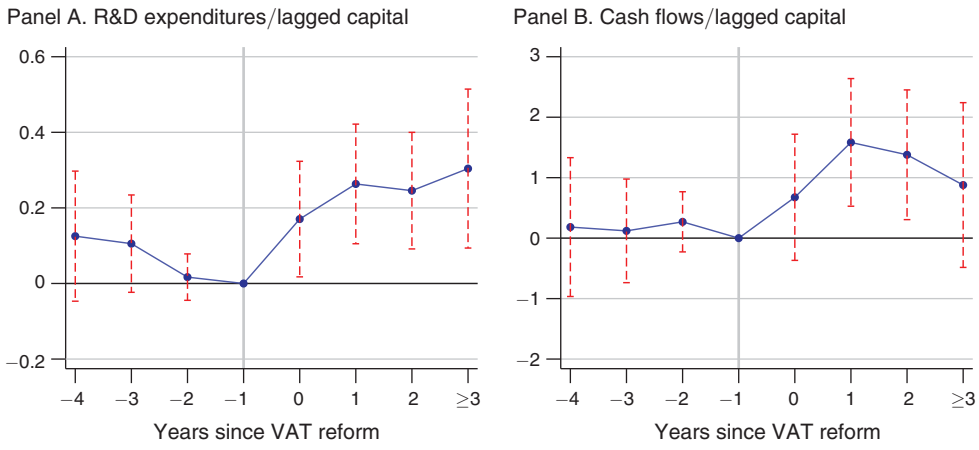

FIGURE 4. EVENT STUDY ESTIMATES: IMPACTS OF THE VAT REFORM ON FIRMS' R&D EXPENDITURES AND CASH FLOWS

Notes: This figure plots event study estimates and corresponding 95 percent confidence intervals from regressions of specification (2). Dependent variables are the ratio of R&D expenditures to lagged capital (panel A) and the ratio of cash flows to lagged capital (panel B). The horizontal axis measures the number of years since the VAT reform. The right end point on the graph is binned so that ≥3 is a bin for years +3 to +8. The coefficient for the year before the VAT reform is normalized to zero. All estimated coefficients can be interpreted as changes in investment (productivity) compared to the year before the VAT reform (indicated by the vertical line) conditional on firm fixed effects, year fixed effects, and quadratic time trends and their interaction terms separately with two-digit industry dummies and types of the VAT taxpayer dummy. Standard errors are clustered at the firm level.

Source: Authors' calculations

actual or potential market size bring more profits for firms to pay the adoption cost of new technologies. Acemoglu and Linn (2004) formalizes these ideas by showing how market size may shape the direction of innovation both theoretically and empirically. Along the same line, recent studies such as Syverson (2004); Dhingra (2013); and Impullitti, Kneller, and McGowan (2017) find consistent evidence of exogenous increases in market size lead to firms adopting innovation and technology.

In our current context, the VAT reform could potentially increase the market demand for machinery equipment goods primarily induced by the tax incentives treatment on fixed assets for all industries. Following this observation, we hypothesize that the VAT reform created the exogenous increase in market size for equipment goods and hence raised the revenue and productivity of firms in the equipment industries to a greater extent than those in other industries. To confirm this prediction and identify market size as another source of productivity growth of firms in the post-reform period, we reapply specification (1) separately to firms in equipment industries and those in a comparison group of industries—consumer goods industries.[24] Table 6 reports the corresponding results, alternatively using logarithm of

[24] "Equipment goods industries" refers to those industries that produce equipment goods, including ordinary machinery manufacturing, special equipment manufacturing, transport equipment manufacturing, electric machines and apparatuses manufacturing, communications equipment, computer, and other electronic equipment manufacturing. "Consumer goods industries" represents those industries that produce goods for final consumption, including agricultural and sideline foods processing; food production; beverage production; textile industry; clothes, shoes, and hat manufacture; and leather, furs, down, and related products.

Table 6—The Impacts of VAT Reform on Market Demand: Equipment Goods versus Consumer Goods

	ln(Total sales)		ln(TFP, OP method)	
	Equipment	Consumer	Equipment	Consumer
	(1)	(2)	(3)	(4)
VAT reform	0.086	−0.057	0.089	0.068
	(0.038)	(0.066)	(0.042)	(0.075)
Equality test	$p = 0.045$		$p = 0.669$	
Observations	86,515	44,655	86,515	44,266
R^2	0.128	0.077	0.148	0.316

Notes: The dependent variable for columns 1–2 is the logarithm of total sales, and the dependent variable for columns 3–4 is the logarithm of the OP method of TFP. "Equipment" represents those industries that produce equipment goods, including ordinary machinery manufacturing, special equipment manufacturing, transport equipment manufacturing, electric machines and apparatuses manufacturing, communications equipment, and computer and other electronic equipment manufacturing. "Consumer" represents those industries that produce goods for final consumption, including agricultural and sideline foods processing; food production; beverage production; textile industry; clothes, shoes, and hat manufacture; and leather, furs, down, and related products. All regressions include firm fixed effects, year fixed effects, and quadratic time trends and their interaction terms separately with two-digit industry dummies and types of the VAT taxpayer dummy. Standard errors are clustered at the firm level for all regressions.

total sales and logarithm of OP method of productivity as the dependent variable. As predicted, columns 1–2 reveal a strong cross-industrial heterogeneity in revenue gains from the VAT reform. In particular, the estimated coefficient for VAT reform is positive and statistically significant for firms in equipment industries (i.e., 0.086). However, it is negative and statistically insignificant for firms in consumer goods industries (i.e., −0.057). The difference between these two coefficients is also statistically significant at the 5 percent level. Similarly, in columns 3–4, we find that the estimate is only statistically significant for firms in equipment industries rather than firms in consumer goods industries. This productivity effect of the reform appears to be quantitatively larger for firms in the former than in the latter industries (i.e., 0.089 versus 0.068), though the equality test does not show a statistical difference between the two. Overall, tentatively, we show that the demand shock for equipment in the post-reform period generates an increase in firms' revenue and stimulates productivity growth.[25]

C. *The Pro-competitive Effect of the VAT Reform*

We have shown previously that the effects of the VAT reform are likely to be asymmetric across firms, in a way favoring those originally in a disadvantageous position (e.g., credit-constrained firms). This 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

[25] By using the logarithm of investment as the dependent variable, we also re-estimate specification (1) separately to firms in equipment and consumer goods industries. We find that while the estimate of the VAT reform is positive and statistically significant for firms in consumer goods industries, it is insignificantly positive and quantitatively neglectable for firms in equipment industries. This potentially implies that the detected investment effect of the reform is indeed driven by the reduced user cost of capital and not much by the increased demand of equipment goods in the market. These results are not reported but are available upon request.

resource misallocation and increasing aggregate productivity (e.g., Edmond, Midrigan, and Yi Xu 2015 and Lu and Yu 2015). 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 the 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 share in total revenue.[26] The intuition behind the calculation is that the output elasticity of a 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 and can obtain the output elasticity of intermediate materials from the estimation of the production function as stated in specification (3) in Section IIIB.

Finally, based on the firm-level markup, we calculate the mean and coefficient of the variation of the markup for all two-digit manufacturing industries in each year and compare the changes of these two indices before and after the largest expansion of the VAT reform in 2009. Specifically, Figure 5 presents the average values of the mean and coefficient of the variation of the industry-level markup before 2009 (i.e., 2005–2008) and percentage changes in these values after 2009, while their original values are reported in Table A7 of the online Appendix. As shown, both indices experienced significant reduction in the post-2009 period, potentially indicating that the 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.

VI. 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 from years 2005–2012 to test whether the VAT reform has successfully stimulated firms'

[26] As pointed out by Brandt et al. (2017), 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 explicit assumptions on demand or behavioral assumptions on competition.

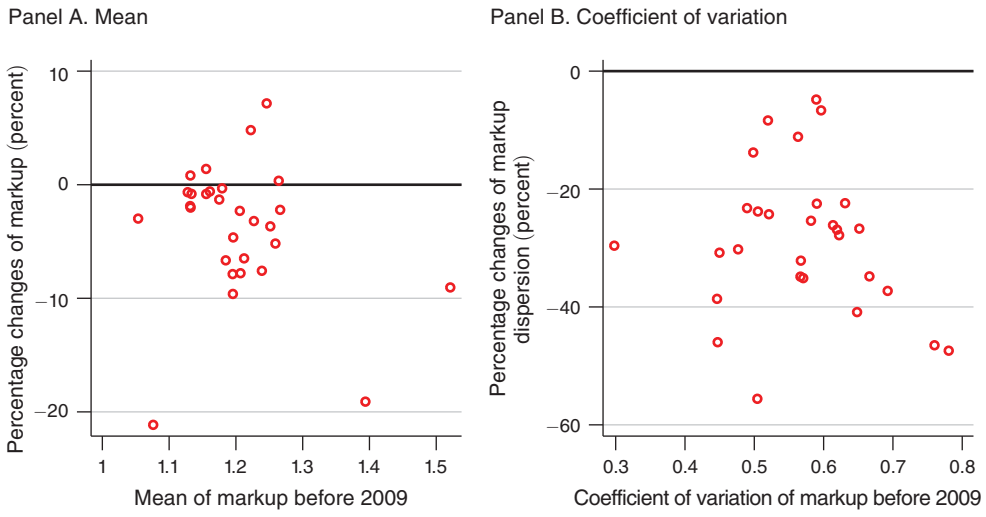

FIGURE 5. CHANGES OF MARKUP DISPERSION WITHIN TWO-DIGIT INDUSTRIES BEFORE AND AFTER 2009

Notes: This figure plots the changes of the mean and coefficient of the variation of the 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 the 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 the markup for each two-digit manufacturing industry after 2009 (i.e., 2009–2012). Panel B presents the corresponding values for the coefficient of the variation of the markup at the two-digit industry level.

Source: Authors' calculations

investment and promoted their productivity, examines the degree to which the presence of financial constraints influences the net policy impacts of the VAT reform, and explores the potential mechanisms of the reform's impacts. Taking advantage of the rich variation in the tax incentives treatment brought about by the VAT reform across firms and over time, we exploit this quasi-experimental setting using a DID approach. Our econometric results suggest that the VAT reform caused an immediate increase in firms' investment and productivity in the post-reform period. Quantitatively, the reform raised investment and productivity of the treated firms relative to the control firms by 38.4 percent and 8.9 percent, respectively. We further validate that eligible firms that are likely to face financial 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 placebo tests and sensitivity 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 financial 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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