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THREE ESSAYS IN DEVELOPMENT ECONOMICS AND POLITICAL ECONOMY

by

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ABSTRACT

This dissertation consists of three chapters studying topics in political economy and development. In the first chapter, I study how firms' participation in development programs influences credit allocation by state banks, focusing on China's Targeted Poverty Alleviation (TPA) scheme. To identify the causal effect of firms' participation, I develop an IV strategy based on the share of a firm's senior managers born in poor counties. I find that TPA participation improves firms' credit access and reduces borrowing costs. Firms use new loans to finance production, leading to higher sales and profits. I argue that these results are best explained by favor exchange between governments and firms, in which firms help governments achieve political goals and earn rents in return.

In the second chapter (joint with Siddharth George), we study how being admitted via affirmative action affects minority students in university. Our identification strategy exploits a unique feature of Chinese college admissions, where minority students receive bonus points. We estimate an RD design, comparing minority students who scored just above the cutoff (and were admitted on merit) against minority students who scored just below the cutoff (and were admitted to the same college via

affirmative action). We find that students admitted via affirmative action have significantly lower self-image, weaker social relationships and perform worse academically. Our results suggest that affirmative action triggers impostor feelings among minority students, which may hurt their performance in elite environments. Placebo tests of students who receive merit-based bonus points support this interpretation.

In the third chapter (joint with Junxue Jia, Yongzheng Liu, and Jorge Martinez-Vazquez, *European Journal of Political Economy* 2021) we use a nationwide city-level panel dataset for China for the years 1999-2009 and examine the effects of vertical fiscal imbalances (VFI) on local fiscal discipline and explicitly explore the institutional conditions under which these effects may take place. We find that higher VFI levels induce fiscal indiscipline by reducing tax effort of local governments. We show that local governments respond to the presence of the VFI by lowering their tax effort on local taxes, but do not do so for shared taxes. In addition, we show that the (in)disciplining effect of the VFI is not present for extra-budgetary revenues, which reflects the institutional fact that extra-budgetary revenues are not considered for the determination of central fiscal transfers to local governments, thus creating no incentive for local governments to respond in this area.

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List of Abbreviations

2SLS	.....	Two-Stage Least Squares
ASIF	.....	Annual Survey of Industrial Firms
CET	.....	College English Test
CNRDS	.....	Chinese Research Data Service
CPC	.....	Communist Party of China
CSDS	.....	College Student Development Survey
DID	.....	Difference in Differences
FDI	.....	Foreign Direct Investment
FE	.....	Fixed Effects
GDP	.....	Gross Domestic Product
IV	.....	Instrumental Variables
NPC	.....	National Poor County
OLS	.....	Ordinary Least Squares
RD	.....	Regression Discontinuity
R&D	.....	Research and Development
RMB	.....	Ren Min Bi (Chinese Yuan)
ROA	.....	Return on Assets
TFR	.....	Tax-for-Fee Reform
SAP	.....	School-Age Population
SES	.....	Socioeconomic Status
SOE	.....	State-Owned Enterprise
STEM	.....	Science, Technology, Engineering, and Mathematics
TPA	.....	Targeted Poverty Alleviation
TSS	.....	Tax-Sharing System
VAT	.....	Value-Added Tax
VFI	.....	Vertical Fiscal Imbalance
US	.....	United States

Chapter 1

Finding Credit at the Bottom of the Pyramid: Chinese State Lending and Corporate Investment in Impoverished Areas

1.1 Introduction

Government officials often seek to influence firms' behavior in ways that serve their political or personal interests. Firms may contribute to public goods provision or target their investments in districts of prominent politicians. Firms, naturally, hope to obtain favors in return. This dynamic of favor-exchange is prominent in both democratic and authoritarian countries (Campos and Giovannoni, 2007). Much of the discussion has focused on the *quid pro quo* that provides direct benefit to politicians and leads to policies that favor businesses rather than the public (Snyder Jr, 1990; Grossman and Helpman, 1996; Mian et al., 2010; Fisman and Wang, 2015; Bertrand et al., 2021). Yet such favor-exchange has the potential to improve social welfare under certain circumstances. For example, governments have an incentive to cater to the interests of the median voter in providing public goods, and to enlist firms' assistance in doing so. Subsequently, firms may be repaid with loans or subsidies that may cause little or no social harm (see, in particular, Amsden's (1992) discussion of the case of Korea). Alternatively, if the public policy employed by the government to induce private effort on providing public goods selects more capable firms and reduces

the resource constraints, this favor-exchange relation may create more economic value for both parties.[1]

In this paper, I present evidence on one such potentially beneficial (from a social welfare perspective) favor-exchange and the existence of positive selection in the context of China's targeted poverty alleviation program ("TPA" hereafter). Implemented in 2013, TPA's goal is to reduce extreme poverty by targeting every poor household and creating custom-made projects adapted to their needs. As part of President Xi Jinping's broader objective to eradicate extreme poverty by the end of 2020, TPA became one of Chinese local governments' top policy priorities. Since the promotion of local leaders was implicitly linked to the achievements of their anti-poverty efforts, local governments urged enterprises, both state-owned and private, to take part in the program. Massive firm involvement in TPA started in 2016. According to a report by the All-China Federation of Industry and Commerce, over 109,000 private firms have participated in TPA, and as of 2020, they had invested about 107 billion RMB (approximately 16.6 billion U.S. dollars) in its anti-poverty programs.

In contrast to conventional philanthropic expenditures (e.g., charitable donations), private participation in the TPA program directly caters to the interests of local government officials. This presents an intriguing possibility: in China, where anti-corruption measures have been strictly enforced since 2013, helping local officials achieve their TPA goals can provide firms with a way to seek potential returns from governments, particularly when a direct transfer of benefits (e.g., bribery) is not viable (Cheng, 2018). In other words, China's TPA can potentially foster a more positive form of favor exchange, wherein firms help local officials achieve political goals instead of merely serving their personal interests. Thus, the participation of firms in TPA potentially constitutes a successful case of a regime that deploys favor

[1]For example, Fisman et al. (2017) find that cultural proximity between lenders and borrowers in India improves the quantity and quality of credit and reduces default by mitigating information frictions in lending.

exchange strategically to realize a private provision of public goods that enhances welfare.

The goal of this paper is to evaluate firm participation in the TPA program and explore the potential reciprocal relationship, achieved through the TPA program, between governments and firms. Specifically, I ask if corporate investments in TPA can help firms gain preferential treatment in state credit allocation, over which the local governments in China exert strong control, and I examine what the consequences of and rationales for the private provision of public goods. To this end, I assemble a data set drawn from the annual reports of publicly listed firms in China from 2010 to 2020. In addition to containing annual data on firms' balance sheets, production, and borrowing, the data include rich information about firm decisions to make corporate investments in the TPA program, the first year that firms participated in the program, the amount of money they invested in the TPA, and the number of poor people they helped in the program. Such detailed panel data makes it possible to construct the key treatment variable in my analysis and allows for an examination of loan outcomes before and after TPA participation.

Nevertheless, empirically examining the consequences of preferential treatment and distinguishing among the various explanations underlying such behaviors is inherently challenging. In particular, selection effects could confound any improvement in outcomes precipitated by reciprocal interactions between governments and firms. That is, firm TPA participation status may be endogenously self-selected based on firm attributes. For example, highly productive firms may have more financial resources to undertake investments in social programs. Alternatively, if highly productive firms are more (rather than less) financially constrained,[2] their TPA participation willingness is ambiguous, depending on whether the potential returns from the TPA

[2]It has long been argued that highly productive firms are generally credit-constrained in China because Chinese financial markets are underdeveloped, leading to distorted financial allocations (Song et al., 2011).

program are higher than the costs. Thus, a naïve regression of loan access on TPA participation could combine both selection and treatment effects. In addition, a simple OLS estimate could be subject to omitted variable bias. For example, credit access and firm participation decisions could simultaneously be affected by an unobserved relationship between firms and governments, such as the preexisting social ties between firm leaders and government officials, which would bias the impact of TPA participation on credit access.

To identify the average treatment effects that TPA participation has on credit access, conditional on firms being self-selected into the program, I exploit the staggering of firm participation in TPA and develop an instrumental variables strategy. Specifically, I use the share of senior managers born in poor counties as a plausibly exogenous instrument for firms' TPA participation status.

The rationale for this approach is based on the following two observations. First, firm decision-makers may show hometown favoritism when making decisions about resource allocation and project investment — i.e., they may be more willing to participate in anti-poverty programs in their hometowns. Second, in addition to those favoring home areas, firm managers born in poor counties may be willing to help other poor areas because of their shared experience with poverty. I further provide evidence that the instrument plausibly satisfies the exclusion restriction conditional on the inclusion of a rich set of firm-level controls and fixed effects; specifically, I show that the instrument is not significantly correlated with firm loan outcomes before 2016 when firms started to participate in TPA. But a statistically significant reduced-form relationship between the instrument and loans appears after 2016, suggesting that the instrument affects loan outcomes through its impact on TPA participation. I further show that the instrument is unlikely to reflect firm productivity and rent-seeking behaviors that otherwise affect borrowing. (See Section 1.3.2 for a more detailed

discussion.)

In my first set of results, I find that after participating in the TPA program, firms received significantly more state credit. In my preferred specification, my estimates imply that firms that participation in TPA leads to a 27 percent increase in credit access (relative to non-participating firms). Given that non-participants on average were loaned 1.6 billion RMB annually, this estimate is quantitatively and economically large. My subsequent analysis of program participation intensity shows that the spending of 1 RMB on the TPA program induced 78.5 RMB in new loans. I complement my main firm-year analyses with a more detailed loan-level analysis of interest rates and loan maturity. The loan-level findings reveal that the newly received loans had lower interest rates while maturity stayed unchanged. These results survive the inclusion of bank-province and bank-industry fixed effects.

In subsample analysis I show that the benefits of TPA participation are more salient for private firms than for state-owned enterprises (SOEs), which is likely because only private entities need to cater to governments' interests to obtain state lending (see, e.g., Zhou et al., 2017). This finding fits in particular with my interpretation that TPA-loan relation results from favor exchange between governments and firms: by participating in the anti-poverty program, firms seek to politically influence the allocation of state credit. This interpretation fits with abundant anecdotal accounts of firm anti-poverty project memos, wherein it is clearly stated that the aim of the anti-poverty project is to enhance collaboration with the local government and gain policy and/or resource support from it.

Several alternative explanations for my main results are nonetheless plausible. For example, to the extent that TPA participation is financially feasible only for more-productive firms, my findings could reflect firms' exploitation of the TPA program to signal their creditworthiness to banks. It might also be the case that governments

finance firms' anti-poverty activities through bank loans. I argue, however, that these alternative explanations are unlikely, given the institutional details. Furthermore, I find no substantive evidence in favor of these explanations — in particular, I show that the positive TPA-loan relation only appears for loans from state-owned banks and is greater in more impoverished regions (see Section 1.5 for more details). While I cannot rule out these possibilities completely, the weight of the evidence is more in line with my favored interpretation of TPA as favor-exchange relationship between firms and governments.

I go on to examine the link between TPA participation and firm profitability. I observe a notable increase in return-on-assets (ROA) and market value after a firm participates in the TPA program. The positive impact of TPA participation on firm profitability can be explained by the improved access to, and lower cost of, state credit for financially constrained firms. That is, initially constrained firms use the expanded credit to finance production instead of substituting for other costly borrowing — there is a significant increase in sales for TPA participants. However, it is worth noting that improved access to state credit is only one of the potential benefits that firms may obtain through this favor-exchange relationship with governments. While it is beyond the scope of this paper to pin down all mechanisms through which TPA affects firm performance, I explore the extent to which my data are consistent with the credit constraint channel, and quantify the aggregate benefits of the TPA participation to firms. The estimates show that firms that participated in TPA gain relatively large increases in evaluation of firm prospects in the marke —- every 1% increase in the TPA spending will lead to about 0.036% increase in firm value.

To draw a broader implication for credit-allocation efficiency, I begin by documenting positive selection into the TPA program. That is, highly productive firms are more inclined to take up the anti-poverty responsibilities. Together with the pos-

itive treatment effect of TPA on access to external credits, conditional on selection, these results suggest the intriguing possibility of an allocation-efficiency gain among listed firms, as credits are granted to firms with more valuable growth opportunities. Following Hsieh and Klenow (2009), I provide complementary evidence on the potential efficiency gain by examining the effect of TPA participation on industry-level dispersion of revenue productivity.[3] I find that industries with a higher proportion of firms participating in TPA experience a decline in the dispersion of revenue productivity, implying that the TPA program not only increases welfare for borrowers, but also, to some extent, generates allocation-efficiency gain among publicly listed firms in the capital market.

This paper contributes most directly to the literature on the consequences of reciprocal favor-exchange between firms and governments. Existing literature focuses in large part on the darker side of this phenomenon —- diverting resource allocation, distorting information, undermining productivity, and bubbling vulnerable profits, which ultimately leads to a welfare loss (see, for example, Fisman, 2001; Khwaja and Mian, 2005; Bertrand et al., 2020; Hao et al., 2020). This paper focuses instead on a more positive side of political influence; that is, I provide evidence of a novel positive effect of reciprocal favor-exchange between corporations and governments, which are realized through potentially welfare-improving investment. My IV approach also offers a methodological innovation that addresses the concerns of De Figueiredo and Richter (2014) that prior findings might be confounded due to endogeneity concerns.

My work also adds to our understanding of the potential channels that firms used to seek political influence. To a much greater extent, firms' influence-seeking takes

[3]Hsieh and Klenow (2009) suggest that, in a model of monopolistic competition, revenue productivity (the product of physical productivity and a firm's output price, denoted TFPR) should be equated across firms in the absence of allocative distortions. The greater the dispersion of revenue productivity, the worse the extent of misallocation, and the larger the loss of aggregate total factor productivity will be.

the form of bribes or campaign contributions (Grossman and Helpman, 1996; Milyo et al., 2000; Mian et al., 2010), charitable giving (Bertrand et al., 2020), strategically sharing expertise (Austen-Smith and Wright, 1994), or even threatening to expose voters to negative information about politicians (Grossman and Helpman, 1999). Using anti-poverty efforts as an example, I show that, particularly in countries with weak institutions, special interests are also likely to exert political influence if they conduct activities that are both pro-social and foster the promotion of government officials.

Finally, my research contributes to a broad literature on the provision of public goods. To overcome the market failure in providing public goods, conventional wisdom relies on either Pigouvian (centralized) provision or Coasean (decentralized) provision. Departing from this dichotomy, the interplay between the two provisions has received increasing attention (Costello and Kotchen, 2020). I show that in the presence of favor-exchange, firms are self-selected to provide public goods. This new channel sheds light on the importance of joint-provision of public goods induced by government policy instrument choices, especially when governments are also constrained in providing public goods and services.

The rest of the paper is organized as follows. Section 1.2 introduces the institutional background of the Targeted Poverty Alleviation program and data, including discussions of the role of firms in the TPA program and potential channels through which participating in TPA is thought to affect firm loan outcomes. Section 1.3 discusses the empirical strategy and its validity. Section 1.4 presents evidence on the link between TPA participation and loan outcomes, while Section 1.5 discusses the interpretation of the main finding and explores the plausibility of alternative explanations. Section 1.6 examines how TPA participation affects firm performance, followed by a simple structural model, on which I draw to quantify the gains of firms,

in terms of firm value. Section 1.7 provides a tentative implication of the study on credit-allocation efficiency. Finally, Section 1.8 concludes the paper.

1.2 Background and Data

1.2.1 Targeted Poverty Alleviation in China

China was once the developing country with the largest rural poor population in the world. The number of people living in extreme poverty in China was nearly half of total number of poor people all over the world in 1980s.[4] Being aware of the potential social instability induced by regional poverty, China has implemented large-scale anti-poverty programs since 1986.[5] Before the 2010s, the anti-poverty campaign focused on alleviating the suffering of the impoverished through regional development (i.e., setting up national poor counties and villages). The central government directly allocated special poverty-alleviation fiscal transfers to those poor areas.

The targeting priority switched from the regional-level to the individual-level when Chinese President Xi Jinping suggested the concept of the targeted poverty alleviation (TPA) in 2013; this requires local governments to target every poor household and individual with projects aimed poverty reduction. As part of Xi's broader objective of eradicating extreme poverty by the end of 2020, TPA became one of Chinese governments' top policy priorities and was later included in the National Five-Year plan of the Chinese government for the first time in 2015. In addition, to supervise and evaluate the poverty-reduction performance of local governments, a state council leading group office for poverty alleviation was founded.[6] From then on, poverty alleviation was not only an economic, but also a political goal for local governments. Furthermore, the promotion of local leaders was implicitly linked to the achievements of their

[4]World Bank, http://data.worldbank.org/

[5]Appendix A.1 shows a detailed description of anti-poverty evolution in China since 1986.

[6]To enforce the leadership of the Leading Group and enhance anti-poverty efficiency, special offices of poverty alleviation were further established in both provincial and county levels.

anti-poverty efforts (Zhou et al., 2018). However, Chinese local governments are relatively time-constrained. By the end of 2015, there were still around 56 million people living in extreme poverty;[7] among total 2847 counties in China, 832 are National Poor Counties.[8] Thus, local governments have urged enterprises, both state-owned and private, to take part in the program.

1.2.2 Firm Participation in TPA

Massive firm involvement in TPA started in 2016. Unlike governments that directly grant cash transfers to poor households, firms customize the anti-poverty program by employing various aid projects that are specific to different poor households, including vocational education and training, micro-finance, entrepreneurship training, and so on. For example, Midea Group, a private Chinese electrical appliance manufacturer, provided vocational training, educational scholarships, and internship opportunities for students from poor villages in Guizhou Province. At the same time, Midea Group offered to eligible students the opportunity that they can choose to stay and work for the company after their training. According to a report by the All-China Federation of Industry and Commerce, there have been over 109,000 private firms that had participated in the TPA program and invested about 107 billion RMB (approximately 16.6 billion U.S. dollars) in the anti-poverty programs as of 2020. These firms have created about 200,000 jobs and provided vocational education and training to more than 1.2 million poor people. Based on a rough calculation, firm participation in the TPA program helped over 15.6 million people living in extreme poverty.[9]

While mitigating poverty and other destabilizing shocks seems to be a political

[7]The criteria of people living in extreme poverty varies across time. In 2015, the poverty threshold was 2,300 RMB (approximately 350 U.S. dollars) in terms of annual income per person.

[8]National Poor County is a designation for counties of which the GDP per capita is below 2,700 RMB (approximately 420 U.S. dollars).

[9]https://news.cgtn.com/news/2020-09-13/How-do-companies-help-alleviate-poverty-in-China–TKAtKzLUJ2/index.html

obligation for state-owned enterprises in China (Wen, 2020), the resource dependence model suggests that the explicit goal of participating in TPA for privates firms was to absorb and exploit the market resources that benefit the firms in the long run (Pfeffer and Salancik, 2003; Morgan and Tumlinson, 2019).[10] In China, local governments have considerable power in the allocation of resources. The scope of government resources includes, but is not limited to, tax deduction and exemption, fiscal subsidies, preferable bureaucratic regulations, preferential treatment in government procurement allocation, and so on. In particular, state bank lending is subject to a great deal of government intervention — governments can indirectly control the loan amount and who receives the loan, and directly determine interest rates and length of the loan term (Cull and Xu, 2003; García-Herrero et al., 2009).

From the firms' perspective, helping local officials achieve their TPA goals provides a way of seeking potential returns from governments, especially when direct transfer of benefits (e.g., bribery) became a less viable option due to China's strict enforcement of anti-corruption measures since 2013 (Cheng, 2018). A firm described the objective of participation in the TPA program in the project memo as follows:

> *"TPA investments would help the company* ***enhance collaboration with the county government and gain policy support in conducting business****. ... The company takes the advantages of the resources of the poverty alleviation to improve the industrial chain, and further expand the company's main business. ... We expect a significantly positive effect from participating in TPA on company performance. ..."*

Given this background, China's TPA has the potential to foster a more positive

[10] In *The Social Responsibility of Business is to Increase Profits* (1970), Milton Friedman express a similar idea that socially responsible actions by firms reflect that the firm is maximizing profits in a relatively sophisticated way. The shareholders do not have reason to delegate these socially beneficial responsibilities to managers.

form of favor-exchange between governments and firms, in which local officials benefit from the help of firms to achieve their political goals in-stead of mere personal interests.

Besides building an informal state-firm relationship, corporate investments in the TPA program may help firms establish a good reputation in the market and gain social recognition, which improves the public's attitudes and perceptions of them. In addition, participating in TPA can work as a signaling device that reduces information frictions in the market. A great deal of literature has argued that corporate philanthropy can convey positive information about high-quality products, strong and effective internal corporate governance, sufficient internal funds, and the creditworthiness of the firm (Klein and Leffler, 1981; Lys et al., 2015). Under this argument, firms' investments in the TPA program increases firm competitiveness in both factor and goods markets.

1.2.3 Data

I exploit information from multiple sources to construct my dataset. First, I use the Chinese Research Data Service (CNRDS) platform, which is a widely used database concerning Chinese publicly listed firms, to obtain the firm-year-level loan data for each firm.[11] CNRDS also provides rich information on listed firms' financial statements, including a summary of the annual balance sheet, firm production, profit and loss information, firm investment activities, and so on. In particular, CNRDS records information about firm decisions regarding corporate investments in the TPA program, such as the first year that firms participated in the program, the amount of money they invested in the TPA, and the number of poor people they helped in the program, which makes it possible to construct the key treatment variable in my

[11]It is also worth noting that the financial industry is excluded from the sample for sake of the research question of this paper.

analysis. In addition, CNRDS provides a sub-database that contains the detailed information of all senior managers and independent directors of a firm.

Second, I complement my firm-level loan data with a more detailed loan-level dataset, taken from the Wind database. The Wind database not only specifies the amount of each loan received by publicly listed firms, but also describes the interest rate and the maturity for each loan.

The information about the background of senior managers and independent directors is mainly obtained from a sub-database of CNRDS, named *Firm Figure Characteristics Database*. However, a few key variables, in particular, the birth city/county of senior managers, contain many missing values in this database, which may have a detrimental effect on the power of my instrumental variable. Thus, I manually complemented missing information on the background of each senior managers using a range of sources. First, I merge the background information of senior managers included in both CNRDS and Wind database based on their names, positions, and tenure. Then, I hand-collected resumes of firm leaders from Google and Baidu based on their past work experiences. For those managers that do not share any information online but still holding a position in the company, I sent emails, made calls, and conducted onsite visits to the company, when it was applicable, to collect basic information on managers.

My final unbalanced panel data include 13,479 firm-year observations. Figure 1·1 shows the number of firms that participated in the TPA program for the first time over the years in my data, which also presents the source of identifying variation at the extensive margin in my analysis. In 2016 when Chinese governments urged the private sector to participate in the anti-poverty campaign, approximately 500 listed firms participated in TPA. While the number of new participants decreased over time, there have been more than 1,300 listed firms that have invested in anti-poverty

activities as of December 2019, which accounts for around 40% of total publicly listed firms.

To get an overview of the pattern in my data, I plot the average new loans approved every year by firm TPA participation status and industries in Figure 1·2. A consistent pattern can be observed in all industries that firms that invest into the TPA program received significantly higher loans than never-participating firms.

To further understand the potential intentions of firm poverty-alleviation decisions, I present the difference in new loans approved between three subsample groups of firms based on whether they lost political connection as of 2012 and whether they participated in the TPA program. Figure 1·3(a) and 1·3(b) plot the mean difference in new loans approved between "treatment" and "control" groups across years based on my classification. Specifically, in Figure 1·3(a), the "treatment" group consists of firms that lost political connection (measured by whether government and party officials resigned from their positions in the firm) in 2012 and participated in the TPA program in 2016, while the "treatment" group in Figure 1·3(b) comprises politically unconnected firms (never hired a government or party official) that participated in the TPA program in 2016. The "control" group in both figures represents unconnected firms that never participated in the TPA program during the sample period. The markers and capped spikes represent the difference in new loans between "treatment" and "control" groups and corresponding 95% confidence intervals.

I highlight two patterns in these plots, as they provide suggestive evidence on the firm credit-seeking incentives underlying the TPA program. First, I find that participation in the TPA program is immediately followed by a statistically significant increase in total new loans approved for those firms compared to non-TPA firms. Second, firms used to hire politicians enjoy preferential treatment in credit allocation, which is normally regarded as the benefit of political connection for firms (Khwaja

and Mian, 2005). However, this privilege disappeared after the government or party official resigned from the position in the firm. Taken together, the patterns suggest that participation in the TPA may serve as a bridge of favor-exchange relationship between firms and governments.

I present summary statistics for all variables used in the analysis in Table 1.1.

1.3 Empirical Strategy

1.3.1 Baseline Specification

To identify effects of participation in TPA on credit allocation, I exploit the staggering of firm participation in TPA since 2016 and estimate a standard two-way fixed effects difference-in-differences model. Thus, I compare the loan outcomes of firms before and after they participated in the TPA program, with those of firms that had not yet participated during my sample period.

The baseline specification can be formalized as follows:

$$y_{it} = \alpha + \beta TPA_{i,t-1} + \gamma \mathbf{X}_{i,t-1} + u_i + \theta_t + \tau_p t + \lambda_j t + \epsilon_{it} \tag{1.1}$$

where y_{it} is a series of loan outcomes of firm i in year t; TPA_{it} is a firm-year dummy variable indicating participation in the TPA program for firm i in year t. The timing of participation in the TPA program varies across firms. In our data, no firms opt out of the TPA program after they participated the program for the first time. That is, TPA_{it} equals zero for years before the firm participated in the TPA program for the first time and one for the year of participation and all the subsequent years. $\mathbf{X}_{it}$ represents a set of control variables, including: $\log(Asset\ per\ Employee)$, $R\&D\ Investment$, $\log(Sales)$, and ROA. I include the firm fixed effects to control for self-selection of firms into the TPA program and for any firm-specific unobservables that are constant over time. θ_t captures the common shocks to all firms. In my

preferred specification, I also control for the industry and province time trends and isolate the possible confounding fact that the firm TPA participation decision might be made in response to these trends. ϵ_{it} is the idiosyncratic error term. Standard errors are clustered at firm level.

1.3.2 Instrumental Variable Estimation Strategy

Endogeneity

A potential concern is that the endogeneity of TPA participation may bias the simple OLS estimates in my baseline specification (1.1). This issue may arise because of self-selection, reverse causality and omitted variable bias. In particular, firm TPA participation status may be endogenously self-selected based on firm type. For example, highly productive firms are more financially able to undertake additional investment in social responsibilities using free cash flows. Alternatively, financially constrained firms also have incentives to participate in TPA if their perceived returns from the TPA program are higher than the costs. Reverse causality may occur because firms that received more loans are more likely be financially able to participate in the poverty alleviation program because financially constrained firms may not have enough funds to finance the social responsibilities. Even though I control for many firm-level characteristics and a full set of firm fixed effects, the firm decision to participate in the TPA program could potentially be correlated with many other unobservables that affect the loans received. For example, it is possible that both credit lending and the firm participation decision are simultaneously affected by the unobserved informal relationship between firms and governments, which biases the impact of TPA participation on credit lending.

Instrumental Variable

To mitigate the endogeneity concern and identify the causal impacts of TPA participation on loan outcomes of firms, I exploit an instrumental variable estimation method. My strategy is to search for instruments with some exogenous variations that may explain the firm's decision to participate in the TPA program, but do not influence the allocation of loans from financial institutions directly. The hometown background of senior managers of a firm falls in this range. Specifically, I use the share of senior managers born in National Poor Counties (NPC) as an instrument to the TPA participation of a firm. There are several reasons why this variable can be a good instrument in my context. First, firm decision-makers may show hometown favoritism when making decisions about resource allocation and project investment — i.e., they may be more willing to participate in anti-poverty programs in their hometowns.[12] If the senior managers of a firm were born in one of the National Poor Counties, it is reasonable to expect that they would value the welfare of people in their hometown and invest more in home areas than other places. In fact, there have been well-publicized examples of entrepreneurs volunteering to participate in anti-poverty programs in their hometowns.[13] Thus, the share of senior managers who were born in National Poor Counties, to some extent, reflects the preference of a firm towards participating in the TPA program. Second, in addition to those favoring home areas, firm managers born in poor counties may be willing to help other poor areas because of their shared experience with poverty. Hence, the variation of the share of senior managers born in National Poor Counties represents a plausibly exogenous shock to the preference of a firm toward participating in the TPA program, varying across

[12]The role of hometown favoritism in shaping the behaviors of people and allocation of resources has been well documented in the literature, especially in the context of China (see, e.g.. Fisman et al., 2018; Chu et al., 2021).

[13]https://news.cgtn.com/news/2020-09-13/How-do-companies-help-alleviate-poverty-in-China–TKAtKzLUJ2/index.html

firms and years. However, this share should not directly affect the loan outcomes in the credit market of the firm or vice versa.

Validity of the IV

The IV estimates are unbiased only if the instrument is valid. From a technical perspective, a valid instrument needs to meet both the relevance condition and the exclusion restriction. Here, I provide more formal evidence that these two criteria are likely satisfied with the instrument.

First-Stage Results Table 1.2 presents the first-stage estimation results in the IV estimations along with different sets of fixed effects and time trends. I find that the instrument (i.e., the share of senior managers who were born in National Poor Counties) is positively and significantly correlated with the TPA participation regardless of whether province × year or industry × year fixed effects, or province and industry time trends are controlled for. The coefficients do not change much across different specifications, indicating a strong positive relationship between the share of NPC managers and the TPA participation. Under-identification is also strongly rejected with the LM statistic ranging from 489.074 to 511.332 depending on different specifications. Meanwhile, for all first-stage specifications, the robust Kleibergen-Paap Wald F-statistics are well above 10, suggesting that our IV estimates are not prone to the weak instrument concern. confirming my argument for the relevance of using the share of NPC managers as an effective predictor for firm participation in the TPA program.

Exclusion Restriction The exclusion restriction requires that the instrument should not directly affect loan outcomes except through its impact on TPA participation. While the exclusion restriction condition is difficult to check directly, I implement a

placebo test that may provide indirect evidence on the exclusion restriction condition.

Specifically, I split the sample into two separate time periods: 2010-2015 and 2016-2020. Then I study the reduced-form relationships between total new loans approved and the instrument for firms within these two sample periods. My conjecture is that before the TPA program was introduced to firms (2010-2015), there should be no reduced-form relationship between the instrument and loans, even for firms that eventually participated in the TPA program. In other words, the mechanism through which the instrument affects loans only works when firms participated in the TPA program. Table 1.3 reports the corresponding results of this placebo test. In Column (1), the estimated coefficient of the instrument is statistically insignificant for pre-20216 sample. However, as shown in Column (2), I find a significantly positive relationship between the instrument and loans approved after the TPA program was introduced to firms. This pattern is further illustrated in Table A.2 in the Appendix. The table reports the dynamic effects of the instrument on firm total new loans over the sample period. Before the TPA program was introduced to firms, the instrument does not have a substantially significant effect on firm loans, while the coefficients become statistically significant right after firms' participation in TPA. This provides relatively more convincing evidence that the instrument affects the preference of participating in the TPA program, and then loans received by firms.

While this placebo test provides initial evidence for the exclusion restriction condition of the instrument, a potential concern with using this instrument is that the share of managers born in poor counties might be correlated with other firm characteristics that affect firm borrowing. In particular, the instrument may be itself a reflection of firm productivity, which may affect credit lending through its impact on the firm's performance. I argue that this concern is lessened, at least partially, by full sets of firm, province-year, and industry-year fixed effects as well as many firm-year

level controls, such as total assets, sales, ROA, and so on, relating to the firm performance included in the estimations. Furthermore, I investigate the reduced-form relationship between the share of senior managers born in poor counties and firm productivity. Firm productivity is calculated using the methods developed by Olley and Pakes (1996) and Levinsohn and Petrin (2003). I find no evidence of potential correlation between the instrument and firm productivity (see Appendix Table A.3).

Another source of potential concern about the exclusion restriction is that the instrument captures other form of rent-seeking and corruption of firms. Specifically, if local governments are more corrupt in poor areas, firms managers born in National Poor Counties may be also better informed of such rent-seeking opportunities through corruption. While this concern might be significantly mitigated by China's intensive anti-corruption campaign during my sample period, I nevertheless present evidence against this view by looking at whether the instrument is correlated with firm business entertainment expenses, which is often a proxy for the level of firm rent-seeking behaviors and corruption (Cai et al., 2011). As shown in Table A.4 in the Appendix, firms with more senior managers from poor counties display no significantly higher amounts of business entertainment expenses.

I also show that controlling for firm productivity and business entertainment spending in the baseline specification does not undermine the power of my instrument. Hence, my instrument is less likely to affect loan allocation via firm productivity or other rent-seeking behaviors, conditioned on a rich set of firm-level controls and fixed effects.

Although these tests provide tentative evidence supporting the validity of the instrument, I acknowledge that the exclusion restriction which requires perfect exogeneity of the instrument is unlikely to hold exactly. I emphasize that my data do not allow me to definitively rule out all channels through which that the instrument

may affect credit allocation other than TPA participation. To provide the robustness of our IV estimation results, I relax the assumption of perfect exogeneity of the instrument and conduct inference that is consistent with the instrument being only plausibly exogenous using the Union of Confidence Interval Approach described in Conley et al. (2012).

1.4 Firm Participation in TPA and Lending

In this section, I investigate the potential linkage between firm TPA participation and loan outcomes by answering two questions. First, which firms participated in the TPA program? A discussion of the potential self-selection mechanisms underlying the firm participation decision makes it possible to better understand the motives of firm anti-poverty activities and potential benefits to both firms and the credit market. The second step is to identify how many benefits, in terms of new loans, firms would receive through participation in TPA if there exist self-selection and other confounders. To make plausibly causal claims, I apply the instrumented difference-in-differences methodology and examine impacts of firm TPA participation, at both extensive and intensive margins, on firm loan outcomes. After demonstrating a credible firm-level TPA-loan relationship, I complement my main findings with a loan-level analysis and show that participating firms also enjoy lower cost of credits, which cannot be explained by changes in term length of the loan. I close with a discussion of robustness and a heterogeneity analysis.

1.4.1 Selection into Participation

I start off by examining firm decisions to participate in the TPA program. I first assess whether firm participation status is endogenously self-selected based on productivity. Theoretically, in a market without financial frictions, highly productive firms have more financialy resources to undertake additional investments in innova-

tion and corporate social responsibilities (Gorodnichenko and Schnitzer, 2013; Servaes and Tamayo, 2013). However, in the context of China where Chinese highly productive firms are generally credit-constrained (Hsieh and Klenow, 2009; Liu and Mao, 2019), firm participation motives are ambiguous. For example, in the absence of sufficient internal and external funds, the firm TPA participation incentive may high or low depending on the perceived returns on investments in impoverished areas, because these costly investments may degrade the financial situation and impede production.

To understand the extent to which the firm TPA participation decision depends on different productivity levels, I regress firms' participation status for 2016 on average productivity level before 2016. Columns (1) and (2) in Table 1.4 report the results using the average logarithm of firm TFP as the predictor, calculated using the methods developed by Olley and Pakes (1996) and Levinsohn and Petrin (2003), respectively. The statistically significant estimates suggest that there is positive selection that highly productive firms are more inclined to participate in TPA.

Inspired by the pattern observed in Figure 1·3 that the loss of political connections is associated with a loss of privilege in credit resources, I then examine whether firms that lost political connections before participating in the TPA program tend to have a higher participation rate.[14] Intuitively, if helping governments providing public goods or services is perceived to be a feasible tool for firms to rebuild informal associations with governments and gain political resources, firms that lost political connections are more likely to invest in poverty-alleviation programs. A statistically significant and positive estimate in Column (3) of Table 1.4 is in line with the above

[14]Hiring a government or party official as the independent director on the board is typically regarded as the standard way to build political connection between the government and firms. In 2012, around 10% of Chinese publicly listed firms have at least one government officer as its independent director. As a part of President Xi's anti-corruption campaign, in late 2012, the central government of China stipulated that government and party officials who are currently in office or not in retirement are banned from holding concurrent posts in companies. Thus, losing political connections in this context denotes whether firms have government or party officials resigned from their positions in the companies.

hypothesis. However, we should be somewhat circumspect about adopting this selection interpretation, because a government official's resigning from the firm does not necessarily imply that the firm "lost" political connections. It can still be interpreted as a reflection of inherent connection to governments. But this result does suggest that dynamics of firm-government interaction is crucial when firms make a decision to invest in TPA.

I next turn to the differences in participation decisions across a firm's industry. In Column (4) of Table 1.4, I regress the firm TPA participation decision on a full set of industry indicators, including mining, real estate, agriculture, manufacturing, retailing, and public affairs. The service industry is omitted as the baseline industry. The estimates of coefficient on mining and real estate are positive and statistically significant at the 1% and 5% level, respectively, indicating that participation is more prevalent in mining real estate industries. To get a sense of the extent to which the selection may be attributed to industries, I turn to Figure 1·4, which plots the TPA participation rate across industries as of December 2019. Black bars represent the share of firms that had participated in the TPA program. Similar to the findings in Table 1.4, mining and real estate industries exhibit particularly high TPA participation rates, with a share of 62% and 47%. These observations can be explained by findings from other studies, such as Fisman and Wang (2015) and Chen and Kung (2019) who show that firms in mining and real estate industries are likely to rely on the informal connections with governments to gain political resources.[15]

[15]Fisman and Wang (2015) show that the political ties allow mining firms to circumvent government regulations that are typically costly to firms. Chen and Kung (2019) find that firms with connection to China's supreme elites enjoy preferential discounts in the land market because governments in China have been made the sole statutory owner and supplier of land.

1.4.2 Effects of TPA Participation on Loan Outcomes

Graphical Evidence Before turning to the baseline DID results, I first provide a graphical description of the potential linkage between the loan allocation and firm participation in the TPA program. Specifically, I use the firm TPA participation status to construct an event study plot around the year of participation in the TPA program using the following specification:

$$y_{it} = \alpha + \sum_{k=-6}^{+3} \beta_k D_{t_{i0}}^k + \gamma \mathbf{X}_{it} + u_i + \theta_t + \tau_p t + \lambda_j t + \epsilon_{it}. \tag{1.2}$$

where $D_{t_{i0}}^k$ is a series of "event-time" dummies that equal one when the participation in the TPA is k years away for a firm. In particular, t_{i0} denotes the year when a firm participated in the TPA program; $D_{t_{i0}}^k$ is a series of dummies indicating whether $t - t_{i0} = k$, with $k = -6, -5, -4, -3, -2, 0, 1, 2, 3$. The omitted time category is $k = 0$, so that the estimated effects β_k are relative to the first year that firms participated in TPA.

Figure 1·5 plots the estimates of β_k and corresponding 95% confidence intervals from the specification (1.2).[16] The vertical axis of the plot measures the average logarithm of total new loans approved for a group of firms in a year. The horizontal axis measures time in years since the participation in the TPA program. The loan allocation pattern in the figure shows that before participation in the TPA program, there does not seem to be a significant difference trend in loans between firms that participated and those that did not. Instead, I observe a notable increase in average loans approved for firms after they participated in the TPA program. The pattern observed in Figure 1·5 provides strongly suggestive evidence on the relationship of TPA participation on loan allocation.

[16] The corresponding estimates of the dynamic estimation of specification (1.2) are presented in Table A.5 in the Appendix.

Baseline Results The OLS estimation results for specification (1.1) with different sets of control variables are presented in Table 1.5. In all estimations, the dependent variable is the logarithm of total new loans. I first examine the effect of the TPA participation on loan allocation by only including firm and year fixed effects to control for unobserved heterogeneity and year shocks. As shown in Column (1), the coefficient of TPA participation is positive and statistically significant at the 1% level, supporting my hypothesis that the participation in TPA program would influence the credit allocation to firms. I then add a full set of province $\times$ year and industry $\times$ year fixed effects in Column (2) and Column (3), which capture, for example, the ad hoc province- and industry-specific policies that may explain the difference in loan outcomes between TPA and non-TPA firms. The coefficients and level of the significance of the TPA participation remain consistent with the results in Column (1). Finally, in Column (4), I include province- and industry-specific linear time trends to control for the differences in time trends between the treatment and control groups. Again, I find a positive impact of the TPA participation on total new loans.

IV Estimation Results As I discussed in the previous section, while a full set of firm fixed effects could control for self-selection of firms into the TPA program and for any fixed firm-specific unobservables that are constant over time, the baseline OLS results may still be subject to reverse causality and omitted variable bias. For example, it is possible that credit lending and firm participation decisions are simultaneously driven by the unobserved selection of firms that varies over time (e.g., informal relationships between firms and government). Thus, I resort to instrumental variable estimations.

The IV estimation results are presented in Table 1.6. After being instrumented, TPA participation still has a positive and statistically significant impact on total new loans. Quantitatively, the estimated coefficient of the TPA participation in my

preferred specification (Column (4)) is 0.267, indicating a semi-elasticity of total new loans with respect to the TPA participation of 0.267, implying that relative to the non-TPA firms, participation in the TPA program led to a 26.7% increase in total new loans of the firms that participated.

1.4.3 Effects of TPA Participation Intensity

I next exploit the variations in TPA participation intensity across firms and years to investigate the intensive margin of TPA participation. The TPA participation intensity is measured by the amount of money that firms spent on the TPA program and the number of poor people that the firm helped in the program. Following Callaway et al. (2021), I rewrite my baseline specification (1.1) by replacing the TPA participation dummy variable with the intensity measures and re-estimate the instrumental variable estimations.[17] Table 1.7 presents the coefficients of the intensity of the TPA participation on total new loans. I find that participation intensity has a positive and statistically significant impact on total new loans in all specifications, regardless of the measures of the TPA participation intensity, which is consistent with the extensive margin of the impact of the TPA participation. In my preferred IV specification in Column (4) and (8) of Table 1.7, the estimated coefficients for TPA spending and TPA population are 0.069 and 0.068, respectively. These are economically significant magnitudes: a 1% increase in the amount of money spent on the TPA program increases the amount of new loans approved for the firm by 0.069%. At the mean values, this is equivalent to every 1 RMB spending on the TPA program can help firms gain 78.5 RMB in new loans.

[17] For space concerns, all the corresponding OLS estimation results are presented in the Appendix.

1.4.4 Loan-Level Evidence

I further conduct a set of analyses at the loan level, which complements my firm-level findings in two ways. First, loan-level data allow me to assess the impact of participation in TPA program on richer credit outcomes, including interest rates and loan maturity. It may give greater confidence in my interpretation if I find consistent results across the firm- and loan-level analyses. Second, measurement error may simply arise due to the use of firm-level data that are more vulnerable to measurement errors during their compiling process. Hence, I rely on the loan-level dataset from CSMAR to test the robustness of my main findings.

I repeat my baseline IV estimation strategy using loan-level outcomes. The estimation results are presented in Table 1.8. The dependent in Columns (1)-(3) is the logarithm of loans approved, while Columns (4)-(6) and Columns (7)-(9) report the estimated coefficients using interest rate and loan maturity in months as dependent variables, respectively. In Column (1), I show results including only firm and year fixed effects. I observe a positive effect on the scale of new credit, with a coefficient of 0.131 on TPA participation. I then add progressively more controls in the next set of columns, including province $\times$ year and industry $\times$ year fixed effects (Columns (2), (5), (8)), and full sets of bank $\times$ province and bank $\times$ industry fixed effects (Columns (3), (6), (9)). I find robust evidence that the impact of firm participation in the TPA program on the amount of loans approved is positive and statistically significant, which is in line with my firm-level findings. What is even more intriguing is that the impact of firm participation in the TPA program on interest rates is negative and statistically significant at the 1% level. The magnitudes of coefficients in Columns (4)-(6) are largely unchanged regardless of various sets of fixed effects. The estimated coefficient of the specification with a richer controls in Column (6) implies that the annualized interest rate decreases by 0.057 percentage points for TPA par-

ticipants. Nevertheless, I do not find a statistically significant effect on loan maturity (in months). This is an indication that firms that participated in the TPA program enjoyed lower cost of bank credits, which is not driven by the change in term length of the loan.

1.4.5 Analysis of Subsamples

In my main IV estimation results, I present the average treatment of investing in the TPA program over all types of firms. To investigate the possible heterogeneous effects of TPA participation on credit lending, I re-estimate my model using various subsamples, differentiating firms by firm ownership, size, and age. To save space, in all estimations I only report results from the specifications equivalent to those in columns (4) of Table 1.6 and using IV estimation. These results are reported in Table 1.9.

First, I show that the impact of TPA participation varies across firms' ownership. I classify my sample into two groups, namely SOEs and private (or non-SOEs), and re-estimate the IV estimation.[18] Column (1) and Column (2) of Table 1.9 show that while the estimated coefficient of the TPA participation is positive and statistically significant for private firms, its sign for the SOEs is statistically insignificant. These results are consistent with the idea that that private firms have more incentives to take the advantage of the TPA program to gain favors in credit allocation, as SOEs are wholly or partially owned by governments that usually enjoy preferential treatment in state bank lending (Cull and Xu, 2003; Lu et al., 2005).

I also explore the extent to which the impact of TPA participation may vary across firms with different levels of demand for external finance. The firm's size is usually deemed as typical sorting criteria to sort between financially "constrained"

[18]Firm ownership is classified by firms' registered type (*qiye dengji zhuce leixing*). For my classifications, SOEs include state-owned enterprises and collective-owned firms, while the remaining firms are regarded as private firms.

and "unconstrained" firms. This is because small firms are more likely to experience a lack of external funds as well as severe informational frictions in financial markets, and thus, would rely on personal or commercial relations to obtain informal finance (Beck et al., 2008). Following this insight, I use the mean value of the number of employees for each firm to split my sample into two groups: large firms (above the 50th percentile of the average number of employees distribution) and small firms (below the 50th percentile of the average number of employees distribution). The results in Column (3) and Column (4) of Table 1.9 show that the estimated coefficient of the TPA participation is statistically more significant for small firms than for large ones. Quantitatively, the difference of the average treatment effect on the size of loans approved is even more striking: small firms yield an estimate of 1.386 which is almost double as large as the average effects on large firm. This finding may have several implications. For example, it may suggest that smaller and younger firms have fewer other avenues for political influence than older and larger firms. Or those who face severe credit constraints have a particularly stronger preference for credit resources as the return to investing in impoverished areas, while financially unconstrained firms may seek other types of political resources.[19]

In a similar vein, Columns (5) and (6) of Table 1.9 show that the effects of TPA participation is more salient for young firms than for old firms.[20] This result can be explained by the idea that young firms have shorter history and have had fewer opportunities to accumulate internal funds, and hence face a higher level of financial constraints (Poncet et al., 2010; Chaney et al., 2012; Gorodnichenko and Schnitzer, 2013).

[19] Alternatively, the finding can also be explained in a way that favoritism in credit allocation is achieved immediately after firms' participation in TPA for financially constrained firms because they have higher demand for external funding.

[20] I split the sample into deciles based on the mean age of a firm during the sample period, and compare the bottom five ("young" firms) deciles to the top five ("old" firms) deciles.

1.5 Interpretation

The empirical results presented in Section 1.4 provide robust causal evidence that participating in the TPA program increases firms' access to credit and reduces the cost of borrowing. A very likely interpretation of this TPA-loan relation is a favor-exchange between governments and firms — firms cater to governments' political interests by investing in impoverished areas and obtain favors in allocation of state credit in return. This interpretation is largely supported by abundant anecdotal accounts of firm anti-poverty project memos in which it is clearly stated that the aim of anti-poverty projects is to enhance collaboration with local governments and gain policy support from them. Nevertheless, there may be some alternative interpretations of this pattern. In this section, I attempt to delve into the two most prominent alternative channels that may give rise to my current findings and to evaluate the extent to which they are consistent with patterns in the data.

1.5.1 Signaling

One alternative explanation of my main findings is signaling. That is, firms may take advantage of the TPA program to signal their creditworthiness if participation in TPA was only financially feasible (or less costly) for high creditworthy firms. In this case, firms with high repayment ability (or low risk) would self-select into participating in the TPA to signal their type, and banks would use TPA participation to screen out low-type firms and lend more to those participating in TPA (which they perceive as high-types). As a result, TPA participation may serve as a signaling device that reduces information frictions and improves credit access and allocation efficiency without any favor-exchange between firms and governments. In other words, financial institutions' incentive to lend efficiently explains the current findings, rather than governments' incentive.

While a signaling explanation is possible in theory, it is more likely to be relevant in settings in which there is a lack of effective tools for banks to assess creditworthiness. This condition is, however, not likely satisfied in my context. In particular, since all firms in my sample are publicly listed, they are required to report their operating situation and financial statements to the public truthfully and on a regular basis. Banks thus plausibly have better tools for identifying creditworthy firms. To further probe the plausibility of this explanation, I provide two sets of indirect tests that evaluate the extent to which signaling can explain my findings.

State Banks versus Other Financial Institutions First, I conduct a placebo analysis looking at the effects of TPA participation on loans from government banks and those from other financial institutions: for example, foreign banks and financial intermediaries. If signaling were responsible for the positive relationship between TPA participation and loan outcomes, we should expect similar reactions from state-owned banks and other financial institutions to firm TPA participation (as the information structures are the same for government and for non-government institutions). Conversely, if the incremental lending was pushed out by the government in exchange for the firms' TPA services, we would expect the effects to come exclusively from state banks.

Table 1.10 reports the IV results with the logarithm of loans from state-owned banks (government banks) and the logarithm of loans from other financial institutions as the dependent variable. As shown in Columns (1) to (4) of Table 1.10, regardless of various sets of fixed effects, the coefficients of the TPA participation are consistently positive in all four specifications, supporting my hypothesis that governments respond to firm participation in the TPA program by granting more bank loans. However, I do not find a similar pattern for loans from non-government institutions in Columns (5)-(8) of Table 1.10. The differential reactions between government and non-government

institutions (in particular, the non-effect from non-government institutions) suggest that the government likely played a role in directing the loans to TPA firms.[21]

Regional Poverty Level I perform another test of the signaling explanation based on the observations that whether the treatment effects of TPA participation would vary across provinces of different National Poor County shares. The provinces with the highest poor county shares typically correspond to poor areas and the lowest shares to relatively rich ones. Given local governments' goal of eradicating extreme poverty by the end of 2020 as mentioned in Section 1.2, governments in poor areas face a heavier poverty-alleviation task; therefore they have stronger incentives to urge local companies to participate in the TPA program.

Following this logic, I place firms in quartiles based on the share of National Poor Counties in a province where the firm is located and estimate effects of TPA participation on the amount of total new loans using the IV estimation strategy for each quartile group. Table 1.11 shows the heterogeneity of the estimated treatment effects across areas of different poor county shares. I find that the effect of TPA participation is strongest in places where local governments face a heavy poverty-alleviation burden. If signaling is the only mechanism that explains the positive effects of TPA participation on firm access to credit, to reconcile with the findings in Table 1.11, we need to rely on an assumption that allows the signaling intensity of TPA participation to vary across poor-county share quartiles. For example, in poor areas firms invest more in anti-poverty activities, which indicates a stronger signal for sufficient internal funds and creditworthiness to banks. However, I do

[21] One alternative explanation of the non-effect from non-government financial institutions could be that, firms rarely borrow from non-state institutions in the first place, because the credit market in China is dominated by state-owned banks (Cull and Xu, 2003). However, in my sample, approximately 50% of the firms borrow from both types of financial institutions. Thus, it seems non-state banks that I study actually participate a great deal in China's credit market, but they just do not consider TPA participation as a factor that weighs into their credit decision-making (while state banks do).

not find significant differences in firm TPA investments across areas with different poverty levels (see Table 1.11), which suggests that the heterogeneity of the estimated treatment effects is likely explained by variations in government incentives rather than firms' signaling intensity across poor and rich areas.

Taken together, while I do not provide definitive evidence to fully rule out the possibility that signaling might also be playing a role, my full set of findings — in particular, the information disclosure requirement for publicly listed firms and the heterogeneous effects across bank ownerships and areas of different poverty levels — are more easily reconciled with an interpretation based on a favor-exchange relationship between firms and governments in which TPA participation serves as a channel to seek influence on state credit allocation.

1.5.2 Government Financing

Another channel that may explain my findings is that governments may finance firms' anti-poverty activities through bank loans. That is, the loans are used specifically for the purpose of anti-poverty activities, and therefore do not constitute the government's reciprocal favor to the firms. I believe that this explanation is relatively unlikely for three reasons. First, it is unlikely that governments would finance social activities of firms in private sector via bank loans directly, because in the Chinese fiscal system, the financing behaviors of governments towards firms are usually achieved institutionally through direct government transfers/subsidies rather than bank loans.

Second, my empirical estimate of the effect of TPA spending on bank loans approved (see Column (4) of Table 1.7) suggests that every 1 RMB investment in the TPA program would help firms receive 78.5 RMB new loans, which is much higher in magnitude than firm spending on anti-poverty activities. The striking ratio of benefit to cost makes it hard to believe that the loans are just compensation for the cost of participating in the TPA program.

In addition, I examine the specific set of policies that the government has adopted to incentivize firms. I have listed all official policies regarding the benefits to firms that participated in the TPA program in the Appendix. Most provinces provide additional fiscal subsidies or tax discounts compensate firms' spending in the TPA program. It is rare for the government to adopt preferential policies regarding lending money from state-owned banks to firms that participated in the TPA program, with Anhui and Sichuan being the two only exceptions. To formally address the concern of this alternative explanation, I exclude all firms in Anhui and Sichuan provinces from the sample and re-estimated specification (1.1). The results are largely unchanged (see Table 1.12), confirming that my interpretation is not vulnerable to this alternative explanation.

1.6 The Impact of the TPA on Firm Performance

The preceding section showed a robust link between TPA participation and new credit. It is natural to ask how these new credits obtained through poverty alleviation would benefit firms and what the value of TPA is to a firm. In this section, I try to provide further evidence on how doing governments a favor affects firm performance, and I quantify the value of participation in TPA to firms.

I begin with a graphical depiction of firm profitability, measured by the return on assets (ROA), around the year of TPA participation. Figure 1·6(a) plots the point estimates from specification of the form of equation (1.2) using ROA as the outcome variable. I normalize the coefficient for the year before TPA participation to zero. The graph shows that firms participating in TPA exhibit no tendency toward higher profitability prior to the involvement in TPA, whereas a notable increase in ROA can be observed after the firm participated in TPA.

As an alternative measure of firm profitability to investigate the impact of TPA

participation on firm performance, I also examine the market reaction to firms' participation in the TPA program. In Figure 1·6(b), I present the dynamics of firm market value before and after TPA participation. A symmetrical pattern can be observed — firm anti-poverty activities are immediately followed by a statistically significant increase in firm market value, despite some information noise from the market.

I then present the impacts of TPA participation on firm profitability, at both the extensive and intensive margins, in Table 1.13, estimated using the IV strategy, similar to the specification of my main results. The first three columns consider the return on assets. Column (1) in Table 1.13 shows the impact of extensive margin of TPA participation on firm profits, conditional on firm-year characteristics. The coefficient on TPA participation is 1.454, which is statistically significant at the 5% level, suggesting that the decision to participate in TPA increases ROA by 1.454 percentage points on average. Columns (2) and (3) present the intensive margin of TPA effects. I find that a 1% increase in firm TPA spending (TPA population) helps ROA increase by 0.0037 (0.0043) percentage points (about 0.15%), which is significant at the 5% level. The remaining three columns examine the effects of firm TPA participation on the logarithm of firm market value. The coefficients on participation status and intensity are positive and statistically significant at the 1% level. On average, a 1% increase in TPA spending leads to a 0.036% increase in firm value which echoes that pattern observed in Figure 1·6(b) — firms that participated in TPA gain relatively large increases in evaluation of firm prospects in the market.

The positive impact of TPA participation on firm profitability can be explained by the improved access to state credit. Intuitively, if a firm is credit constrained, enhancing the access to, as well as reducing the cost of, bank credit is likely to lead to more outlay, output, and profits because the credit will be used to finance production instead of being a substitute for other costly borrowing (Banerjee and Duflo, 2014).

To further investigate how TPA participation affects firm performance and to shed light on the existence of credit constraints for firms participated in TPA, I now turn to firm production activities to assess whether the sales are expanded by participation in TPA.

In Columns (1) to (3) of Table 1.14, I start by looking at the impact of TPA participation on the logarithm of firm total sales using the IV estimation strategy. In line with my prediction, the positive and statistically significant estimates of firm anti-poverty activities suggest that firms may finance production using the new credit obtained through participating in TPA. Interestingly, I do not find any statistically significant effects on the logarithm of cost regardless of how I define firm involvement in TPA program (Columns (4) to (6)). This finding is also consistent with the notion that the cost of state credit is lower for firms that participated in TPA, which may offset the increased cost of physical input because of increased production.

Nevertheless, improved access to state credit is only one of the potential benefits that firms obtained, underlying this favor-exchange relationship with governments. For example, apart from greater access to credits, governments can help participating firms circumvent bureaucratic constraints and get round regulations, which are usually costly to firms (Johnson et al., 1998; Fisman and Wang, 2015), gain preferential treatment in public procurement allocation (Titl and Geys, 2019), or reduce the firm entry barrier in specific areas (Claessens et al., 2008; Dreher and Gassebner, 2013; Baltrunaite, 2020), etc. All these possible mechanisms can exert a positive impact on firm profits. However, due to problems with data availability, it is hard to decisively determine all of the mechanisms through which TPA participation affects firm performance. Despite this, I explore the extent to which my data are consistent with the increased-credit channel.

Taken together, the results I presented in this section suggest that corporate

investment in impoverished areas does benefit firms, in terms of higher sales, profits, and market value. While we should be circumspect in attributing higher profitability to improved access to state credit, these patterns are broadly consistent with the idea that increased credit is used to expand production rather than as a substitute for other borrowing. From the firm's perspective, increased access to credit and a lower cost of borrowing unambiguously increase the welfare of the firm.

1.7 Discussion on Welfare Implications

My analyses in the preceding sections have shown that firms are self-selected into anti-poverty activities. In particular, highly productive firms are more inclined to participate in the TPA program. Conditional on selection, the IV estimates show a positive treatment effect of TPA participation on access to external credits. Taken together, while it is very hard to fully evaluate the welfare implications of my findings, these results suggest an intriguing possibility of an allocation efficiency gain among listed firms, as credits are granted, at the extreme, to firms with more valuable growth opportunities (i.e., highly productive firms but are financially constrained or in temporary financial distress).

To get a sense of the extent of potential efficiency gain at the macro level, I follow a seminal paper by Hsieh and Klenow (2009) to construct the measure of capital misallocation. In a model of monopolistic competition, revenue productivity (the product of physical productivity and a firm's output price, denoted TFPR) should be equated across firms in the absence of allocative distortions. The greater the dispersion of revenue productivity, the worse the extent of misallocation, and the larger the loss of aggregate total factor productivity will be. Following their method,

I calculate firms' TFPR given by the equation as follows:

$$TFPR_{it} = \frac{P_{it}Y_{it}}{K_{it}^{1-\alpha}(\omega_{it}L_{it})^{\alpha}} \tag{1.3}$$

where $P_{it}Y_{it}$ is the revenue of the firm, K_{it} is the capital input and $\omega_{it}L_{it}$ is the labor input, and α is the labor share.[22] Then, I use the standard deviation of TFPR within an industry to proxy for the degree of misallocation of that industry.

To build a link between TPA participation and capital misallocation at the industry level, I compute the average share of firms that participated in the TPA program in each industry. Then I use this share as a continuous treatment variable and perform a standard continuous difference-in-differences approach using the following specification:

$$TFPR\ Dispersion_{jt} = \alpha + \beta TPA\ Share_{j,t-1} + \gamma \mathbf{X}_{j,t-1} + \lambda_j + \theta_t + \mu_{jt} \tag{1.4}$$

where $TFPR\ Dispersion_{jt}$ is the dispersion measure for industry j in year t; TPA Share$_{jt}$ represents the share of firms that participated in the TPA program for industry j. To be consistent with my baseline specification (1.1), I also control for the industry-mean values of log(*Asset per Employee*), *R&D Investment*, log(*Sales*), and *ROA*. Industry and year fixed effects are included. Robust standard errors are clustered by industry.

Estimation results are presented in Table 1.15. The coefficients of the share of TPA firms are consistently negative and statistically significant in both columns, regardless of whether controlling for the industry-level covariates, which indicates that after the implementation of the TPA program, the industry-level capital misallocation drops

[22]Capital input is measured by total amount of investment in fixed assets. Labor input is measured by the firm's total wage bill rather than by employment because wages per worker could adjust for differences in working hours per worker and workers' skills among firms. Labor share can vary across firms, which is approximated by the share of total wage payments in nominal output of the industry under the assumption of a simple Cobb-Douglas production function.

significantly for industries with more firms participating in the TPA program. This result is consistent with my firm-level findings that credit allocation through poverty alleviation can mitigate capital misallocation.

One should be cautious in interpreting the results regarding industry-level capital misallocation. Since I only use listed firms in my analysis, the welfare implications generated from the above tests are limited to a subsample of the economy. If the loans, originally granted to more efficient private sectors (non-listed firms), are now directed to participating firms, my results will not capture this welfare loss. While it is impossible to fully evaluate the welfare implications, my findings still provide some suggestive evidence that the shifted allocation scheme favoring firms participating in the TPA program not only increases welfare for borrowers, but also, to some extent, generates allocation efficiency gain among publicly listed firms in the capital market.

While it is beyond the scope of this paper to fully evaluate the overall welfare implications of my findings, my study has a number of implications for understanding the consequences of the reciprocal relationship between firms and governments and highlights a number of potential welfare gains within the TPA program. First, in contrast to existing literature that focuses on the darker side of this phenomenon, this is one of the first studies to highlight a more positive side of political influence; that is, the TPA program fosters a positive form of reciprocal relationship between governments and firms, in which firms assist the government to achieve certain policy objectives in social relief and welfare-improving programs instead of merely serving politicians' personal interests. And firms are repaid with loans or subsidies that may have little or no social harm. Second, firms indeed benefit from investing in the TPA program, in terms of higher sales, profits, and market value. While we should be circumspect in attributing higher profitability to the improved access to state credit, these patterns are broadly consistent with the idea that increased credit is used to

expand production rather than being a substitute for other borrowing. From the firm's perspective, increased access to credit and a lower cost of borrowing unambiguously increase the welfare of the firm. Finally, the credit-allocation scheme favoring firms participating in the TPA program not only increases welfare for borrowers, but also, to some extent, generates allocation efficiency gain among publicly listed firms in the capital market in the way that highly productive firms are self-selected into the TPA program, and obtain higher access to credits.

1.8 Conclusion

In this paper, I studied whether corporate investments in impoverished areas help firms gain preferential treatment in state credit allocation, and what the consequences of, and rationales for, the impacts of private efforts in public goods provision are, in the context of China's targeted poverty alleviation program. Taking advantage of panel data for Chinese publicly listed firms from 2010 to 2020, I employ an instrumented difference-in-differences strategy based on firms' staggered involvement in the TPA program, and exploiting the plausibly exogenous variation in the share of firms' senior managers born in poor counties as an instrument for TPA participation. I find robust evidence that TPA participation improves firms' credit access and reduces the cost of borrowing. Further, I find that the impact of helping governments provide public goods may be economically substantial to firms — there are large increases in sales and profits in response to TPA participation. Firms that participated in TPA also gain relatively large increases in evaluation of firm prospects in the marke: a 1% increase in TPA spending leads to a 0.03% increase in firm value.

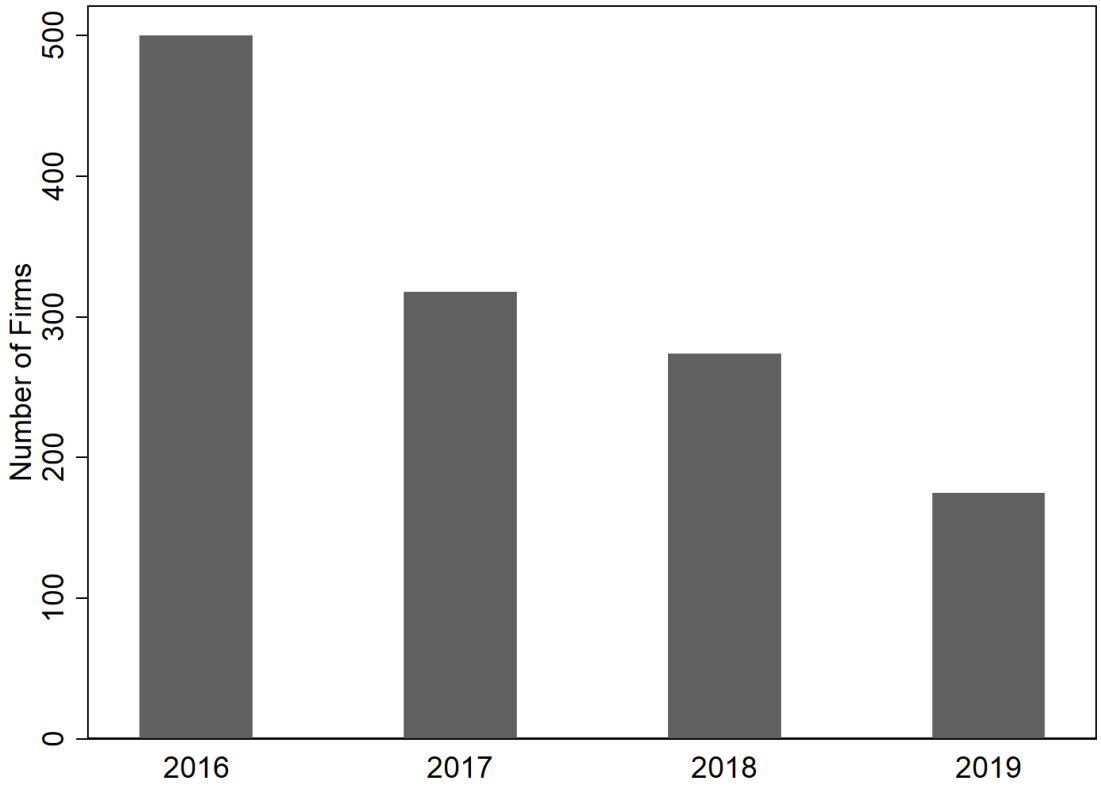
Overall, given the institutional details and abundant anecdotal evidence on politics in China, this finding is more easily reconciled with an interpretation based on a favor-exchange relationship between firms and governments, in which local officials benefit

from the help of firms to achieve their political goals (i.e., poverty alleviation) instead of mere personal interests. And firms obtain political influence in resource allocation in return.

From a policy perspective, although the preceding analysis is conducted in the context of China, it is reasonable to believe that the results have broader policy implications for other countries, particularly those with weak institutions so that governments are constrained in providing public goods and services. I emphasize the importance of joint-provision of public goods from both public and private sectors. When incentives for local governments are to serve the interests of citizens, I propose a potential public instrument that can be employed by the government to induce private effort in assisting governments, which has little or no social harm. If the policy tool helps governments select socially responsible firms which are more efficient in production and more responsive to citizens' concerns, this favor-exchange relation can create more economic value for both parties.

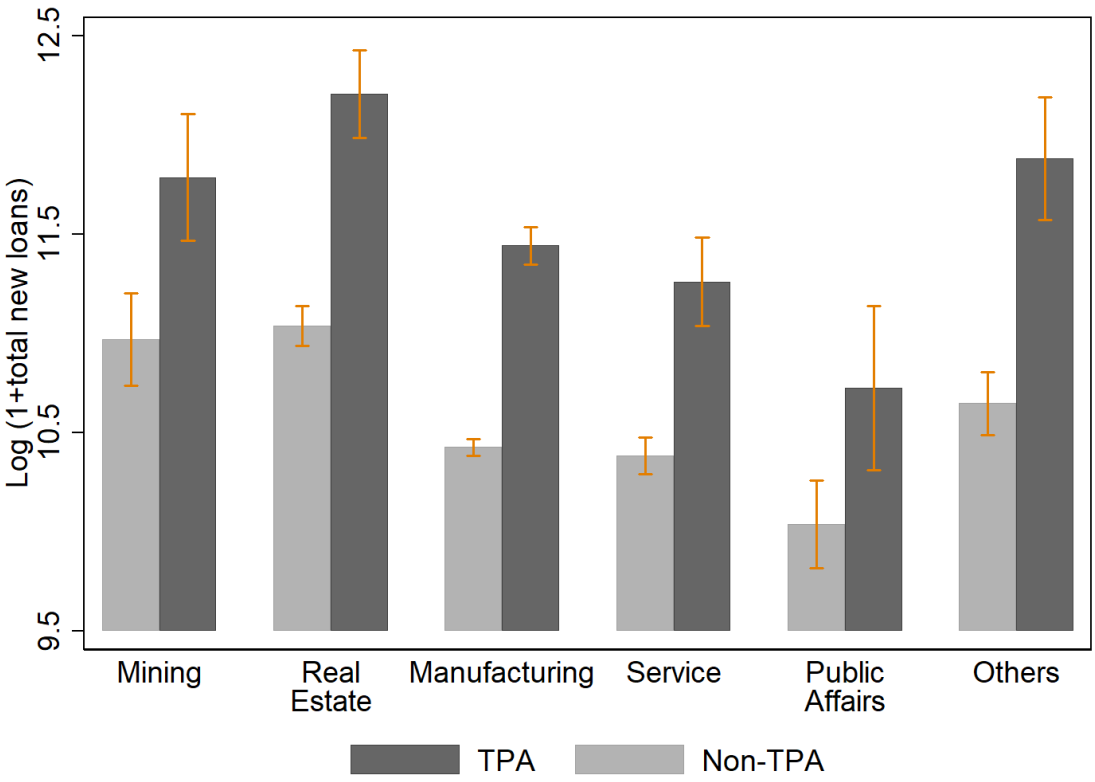
1.9 Figures and Tables

Figure 1·1: Number of First-Time Participating Firms across Years



Notes: The figure plots the margins of variation used in the empirical analysis. Each bar represents the number of firms that participated in the Targeted Poverty Alleviation (TPA) program for the first time in a given year (x-axis).

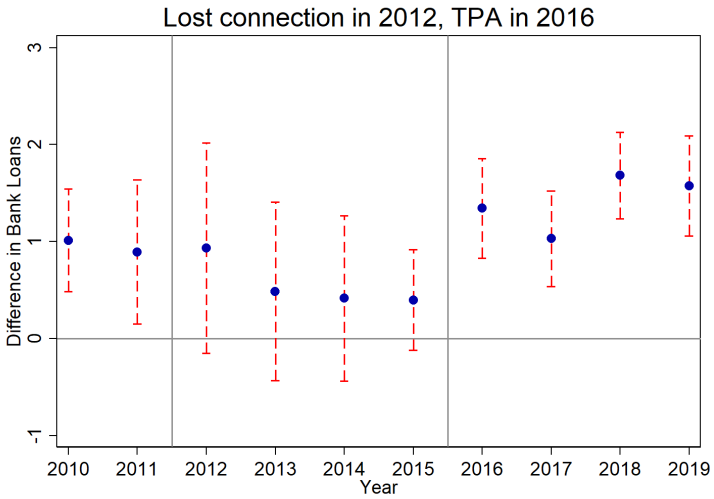
Figure 1·2: Average New Loans Approved for TPA and Non-TPA Firms, by Industry



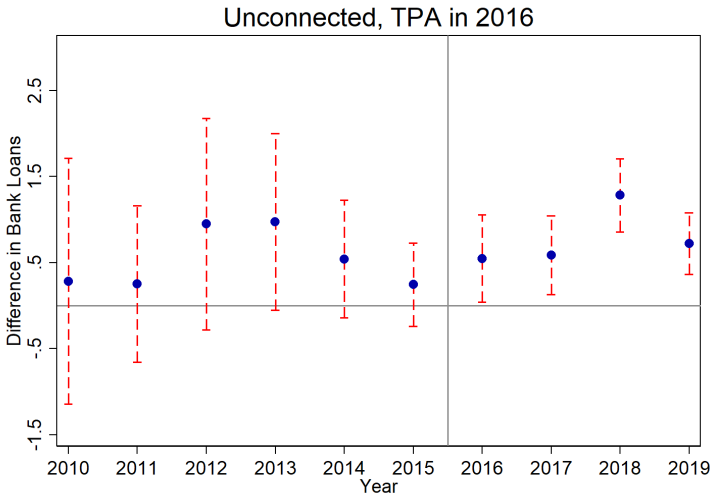
Notes: The figure plots the average new loans approved every year for firms that had participated in TPA and those that have never participated in each industry. Black bars represent the share of firms that had participated TPA, while grey bars show those that never participated.

Figure 1·3: Political Connection, TPA Participation, and Loans

(a) Comparison A

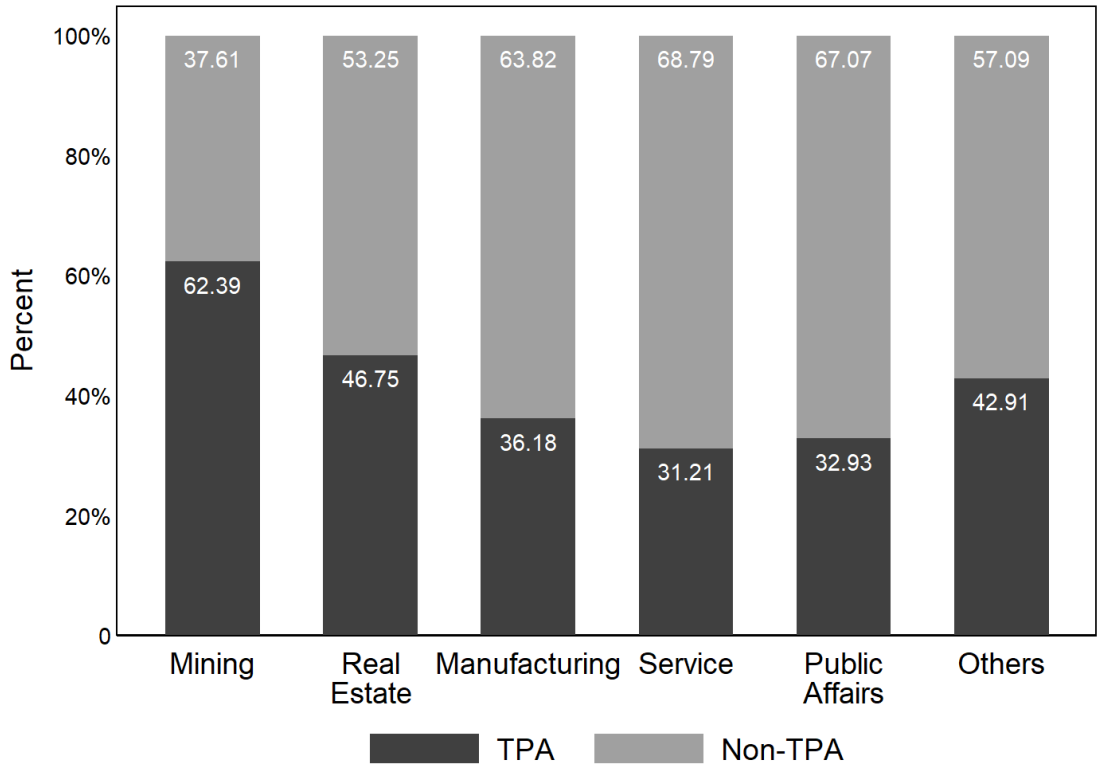


(b) Comparison B



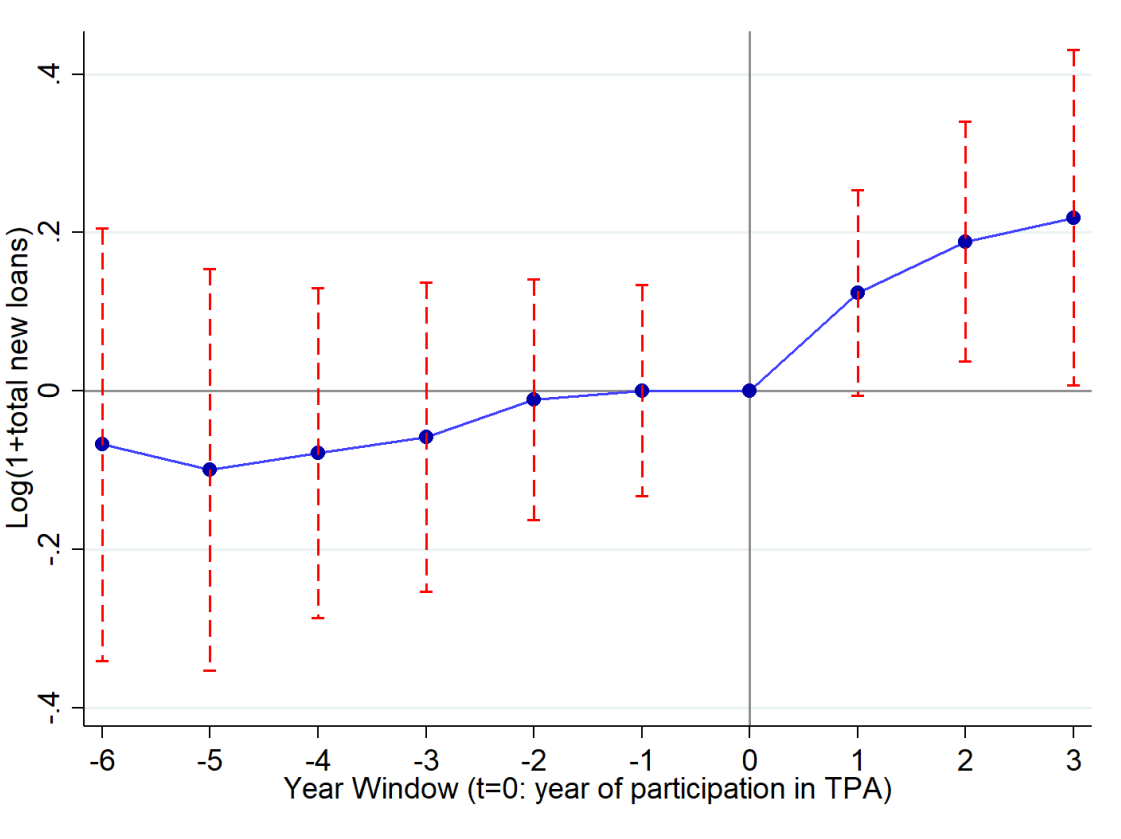
Notes: The figures plot the mean difference in new loans approved between "treatment" and "control" groups across years. In Figure (a), the "treatment" group consists of firms that lost political connection (measured by whether government and party officials resigned from their positions in the firm) in 2012 and participated in the TPA program in 2016, while the "treatment" group in Figure (b) comprises politically unconnected firms (never hired a government or party official) but participated in the TPA program in 2016. The "control" group in both figures represents unconnected firms that never participated in the TPA program during the sample period. The markers and capped spikes represent the difference in new loans between "treatment" and "control" groups and corresponding 95% confidence intervals.

Figure 1·4: TPA Participation Rate, by Industry



Notes: The figure plots the firm TPA participation rate of each industry as of December 2019. Black bars represent the share of firms that had participated in the TPA program, while grey bars show those that never participated.

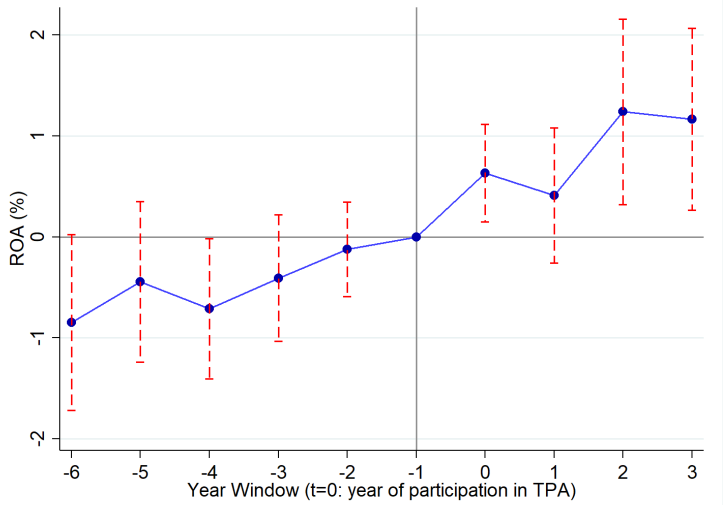
Figure 1·5: New Loans Approved Before and After Participation in TPA



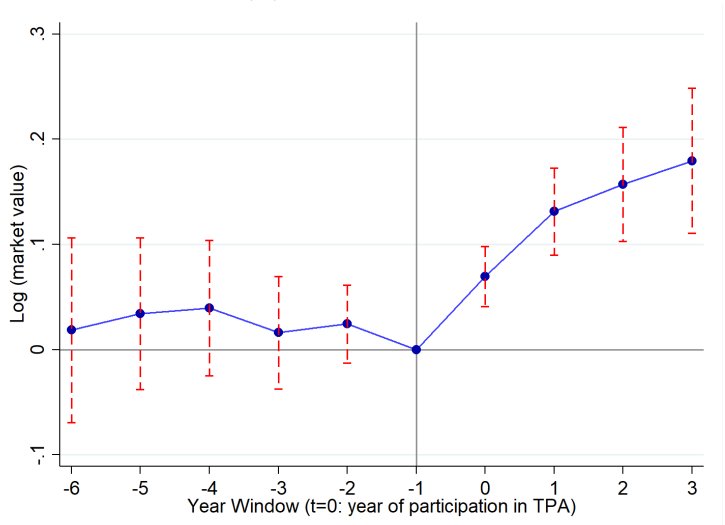
Notes: The figure plots estimates of β_k and corresponding 95% confidence intervals from the specification (1.2). The dependent variable is the logarithm of total new loans approved in a year. The horizontal axis measures time in years since participation in the TPA program. The coefficient for the year of participation in TPA ($t = 0$) is normalized to zero. All estimated coefficients can be interpreted as changes in new loans compared to the first year that firms participated in TPA (indicated by the vertical line) conditional on firm fixed effects, year fixed effects, and industry and province time trends. The regression controls for log(*Asset per Employee*), *R&D Investment*, log(*Sales*), and *ROA*. Standard errors are clustered at the firm level.

Figure 1·6: Firm Profit and Market Value Before and After Participation in TPA

(a) Return on Assets



(b) Market Value



Notes: The figures plot estimates of β_k and corresponding 95% confidence intervals from regression (1.2). Dependent variables are ROA(%) in Figure (a) and the logarithm of firm market value in Figure (b). The horizontal axis measures time in years since participation in the TPA program. The coefficient for the year before TPA participation ($t = -1$) is normalized to zero. All estimated coefficients can be interpreted as changes in firm profits (market value) compared to the year before TPA participation (indicated by the vertical line) conditional on firm fixed effects, year fixed effects, and industry and province time trends. Both regressions control for log(*Asset per Employee*), *R&D Investment*, log(*Sales*), and *ROA*. Standard errors are clustered at the firm level.

Table 1.1: Summary Statistics

	Mean	S.D.	Min.	Max.	Obs.
Firm-Level:					
log (1+Total New Loans)	10.853	1.743	0.000	18.855	13,479
log (1+Loans from State-Owned Banks)	9.421	3.762	0.000	18.855	13,479
log (1+Loans from Other Institutions)	3.824	5.161	0.000	16.977	13,479
TPA Participation	0.171	0.376	0.000	1.000	13,458
log (1+TPA Spending)	0.624	1.736	0.000	14.364	13,105
log (1+TPA Population)	0.471	1.607	0.000	13.017	12,752
ROA (%)	3.028	6.976	-97.925	52.616	13,143
log (Asset per Employee)	14.575	0.998	11.762	19.910	13,150
log (Sales)	21.398	1.346	15.552	28.495	13,150
R&D Investment	3.160	4.328	0.000	58.249	10,728
IV: Share of NPC Managers (%)	0.713	1.828	0.000	31.250	13,151
Loan-Level:					
log (1+Loans)	9.327	1.405	0.095	19.008	69,512
Interest Rate (%)	5.118	0.820	4.350	7.830	30,633
Loan Maturity (months)	18.440	20.241	0.96	300	30,620

Notes: The table shows the summary statistics of variables used in the empirical analysis. All monetary values are in real terms, deflated by national-level consumer prices in 2009. *TPA Participation* is a firm-year indicator variable that equals 1 if the firm participates in the Targeted Poverty Alleviation program; *TPA Spending* is the amount of money that a firm spends on the TPA program in a year; *TPA Population* is the number of poor people who benefit from a given firm's specific anti-poverty policies in a year; *R&D Investment* is the ratio of total R&D expenditures to total assets; *Share of NPC Managers* is the share of senior managers who were born from National Poor Counties (NPC) in total senior managers.

Table 1.2: IV First-Stage Results

	TPA Participation			
	(1)	(2)	(3)	(4)
Share of NPC Managers	0.118***	0.115***	0.116***	0.118***
	(0.012)	(0.012)	(0.012)	(0.012)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Province × Year FE		Y		
Industry × Year FE			Y	
Province Trend				Y
Industry Trend				Y
IV F-stat	97.834	100.150	95.858	98.413
Underidentification LM Statistic	511.332	489.074	507.587	508.295
LM statistic P-value	0.000	0.000	0.000	0.000
No. of Observations	10,083	10,080	10,066	10,080
Adjusted R^2	0.586	0.608	0.595	0.586

Notes: The table reports the first-stage results of IV estimations. The dependent variable in all specifications is *TPA Participation*, which is a firm-year indicator variable that equals 1 if the firm participates in the TPA program. *Share of NPC Managers* is the share of senior managers who were born in National Poor Counties (NPC) out of total senior managers. All regressions control for log(*Asset per Employee*), *R&D Investment*, log(*Sales*), and *ROA*. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.
*** denotes the significance at the 1% level.

Table 1.3: IV Validity Test

Sample	log (1+Total New Loans)	
	Before 2016	After 2016
	(1)	(2)
Share of NPC Managers	-0.044	0.038**
	(0.037)	(0.016)
Controls	Y	Y
Firm FE	Y	Y
Year FE	Y	Y
Province Trend	Y	Y
Industry Trend	Y	Y
No. of Observations	3,809	6,909
Adjusted R^2	0.550	0.666

Notes: The table reports results of IV validity tests using the baseline specification (1.1). The dependent variable in both specifications is $\log(1 + Total\ New\ Loans)$. *Share of NPC Managers* is the share of senior managers born in National Poor Counties (NPC) among total senior managers. I split the sample into two separate time period: 2010-2015 and 2016-2020. Column (1) presents pre-2016 results, while Column(2) report post-2016 results. All regressions control for $\log(Asset\ per\ Employee)$, $R\&D\ Investment$, $\log(Sales)$, and ROA. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.
** denotes the significance at the 5% level.

Table 1.4: Firm Self-Selection into the TPA Program

	TPA Participation$_{2016}$			
	(1)	(2)	(3)	(4)
log (TFP), OP approach	0.072***			
	(0.015)			
log (TFP), LP approach		0.132***		
		(0.038)		
Lost Connection			0.040**	
			(0.016)	
Mining				0.207***
				(0.062)
Real Estate				0.106**
				(0.042)
Agriculture				0.159*
				(0.080)
Manufacturing				0.018
				(0.019)
Retailing				0.038
				(0.030)
Public Affairs				0.101*
				(0.056)
Province FE	Y	Y	Y	Y
Industry FE	Y	Y	Y	
Mean of Dependent Variable	0.164	0.164	0.164	0.164
No. of Observations	2,691	2,691	2,691	2,691
Adjusted R^2	0.097	0.088	0.086	0.067

Notes: The table shows relationship between firm characteristics and participation in the TPA program. The dependent variable, $TPA\ Participation_{2016}$, in all columns is an indicator variable that equals 1 if the firm had participated in the TPA program as of the end of 2016, and 0 otherwise. $\log(TFP)$, *OP approach* and $\log(TFP)$, *LP approach* denote the average logarithm of firm TFP before participating in TPA, calculated using the methods developed by Olley and Pakes (1996) and Levinsohn and Petrin (2003), respectively. *Lost Connection* indicates that firms lost political connections (measured by whether firms had government or party officials who resigned from their positions in the firm) before participating in the TPA program. The baseline industry in Column (4) is the service industry. FE stands for "fixed effects." Robust standard errors clustered at provincial level are reported in parentheses.
*, **, and *** denote the significance at the 10%, 5%, and 1% level, respectively.

Table 1.5: OLS Results: Effects of TPA on Total New Loans

	log (1+Total New Loans)			
	(1)	(2)	(3)	(4)
TPA Participation	0.161***	0.166***	0.173***	0.160***
	(0.056)	(0.060)	(0.056)	(0.057)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Province × Year FE		Y		
Industry × Year FE			Y	
Province Trend				Y
Industry Trend				Y
Mean of Dependent Variable	10.853	10.853	10.853	10.853
No. of Observations	10,720	10,718	10,720	10,718
Adjusted R^2	0.615	0.616	0.620	0.616

Notes: The table reports the OLS results of effects of the TPA participation on total new loans. The dependent variable in all specifications is $\log(1 + Total\ New\ Loans)$. *TPA Participation* is a firm-year indicator variable that equals 1 if the firm participates in the Targeted Poverty Alleviation program. All regressions control for $\log(Asset\ per\ Employee)$, $R\&D\ Investment$, $\log(Sales)$, and ROA. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.

*** denotes the significance at the 1% level.

Table 1.6: IV Second-Stage Results: Effects of TPA on Total New Loans

	log (1+Total New Loans)			
	(1)	(2)	(3)	(4)
TPA Participation	0.279**	0.240*	0.312**	0.267**
	(0.130)	(0.136)	(0.133)	(0.134)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Province × Year FE		Y		
Industry × Year FE			Y	
Province Trend				Y
Industry Trend				Y
Mean of Dependent Variable	10.853	10.853	10.853	10.853
IV F-stat	97.834	100.150	95.858	98.413
Underidentification LM Statistic	511.332	489.074	507.587	508.295
LM statistic P-value	0.000	0.000	0.000	0.000
No. of Observations	10,083	10,080	10,066	10,080
Adjusted R^2	0.030	-0.001	0.016	0.019

Notes: The table reports the IV second-stage results of effects of the TPA participation on total new loans. The dependent variable in all specifications is $\log(1+Total\ New\ Loans)$. *TPA Participation* is a firm-year indicator variable that equals 1 if the firm participates in the Targeted Poverty Alleviation program. All regressions control for $\log(Asset\ per\ Employee)$, *R&D Investment*, $\log(Sales)$, and *ROA*. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.

* and ** denote the significance at the 10% and 5% level, respectively.

Table 1.7: IV Results: Effects of TPA Intensity on New Loans

	log (1+Total New Loans)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log (1+TPA Spending)	0.072**	0.060*	0.081**	0.069*				
	(0.034)	(0.036)	(0.035)	(0.035)				
log (1+TPA Population)					0.071*	0.060	0.081*	0.068*
					(0.040)	(0.042)	(0.041)	(0.041)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Province × Year FE		Y				Y		
Industry × Year FE			Y				Y	
Province Trend				Y				Y
Industry Trend				Y				Y
Mean of Dependent Variable	10.853	10.853	10.853	10.853	10.853	10.853	10.853	10.853
IV F-stat	84.790	85.527	83.219	84.507	67.869	66.522	65.467	69.380
Underidentification LM Statistic	317.924	303.718	321.276	312.751	188.219	184.966	192.805	188.984
LM statistic P-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
No. of Observations	10,042	10,039	10,025	10,039	9,639	9,636	9,624	9,636
Adjusted R^2	0.030	-0.001	0.015	0.019	0.028	-0.003	0.013	0.017

Notes: The table reports the IV results of effects of the TPA intensity on total new loans. The dependent variable in all specifications is $\log(1 + Total\ New\ Loans)$. *TPA Spending* is the amount of money that a firm spends on the TPA program in a year; *TPA Population* is the number of poor people who benefit from a given firm's specific anti-poverty policies in a year. All regressions control for $\log(Asset\ per\ Employee)$, $R\&D\ Investment$, $\log(Sales)$, and ROA. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.

* and ** denote the significance at the 10% and 5% level, respectively.

Table 1.8: Effects of TPA on Loan Outcomes: Loan-level Evidence

	log (1+Loans)			Interest Rate			Loan Maturity		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
TPA Participation	0.131**	0.206***	0.181***	-0.053***	-0.058***	-0.057***	0.134	-0.204	-1.504
	(0.061)	(0.067)	(0.068)	(0.019)	(0.020)	(0.021)	(1.276)	(1.311)	(1.189)
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Province × Year FE		Y	Y		Y	Y		Y	Y
Industry × Year FE		Y	Y		Y	Y		Y	Y
Bank × Province FE			Y			Y			Y
Bank × Industry FE			Y			Y			Y
Mean of Dependent Variable	9.327	9.327	9.327	5.118	5.118	5.118	18.440	18.440	18.440
IV F-stat	382.553	504.570	464.916	143.325	288.254	230.652	143.334	288.432	230.653
Underidentification LM Statistic	849.717	997.975	955.323	456.672	751.711	679.571	456.684	751.779	679.640
LM statistic P-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
No. of Observations	41,624	40,726	39,779	19,166	18,757	18,053	19,154	18,747	18,043
Adjusted R^2	-0.039	-0.052	-0.113	-0.063	-0.086	-0.194	-0.064	-0.088	-0.197

Notes: The table reports the IV results of effects of the TPA participation on loan outcomes using loan-level data. All regressions control for log(*Asset per Employee*), *R&D Investment*, log(*Sales*), and *ROA*. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.

** and *** denote the significance at the 5% and 1% level, respectively.

Table 1.9: Subsample Analysis

	Ownership		Firm Size		Firm Age	
	Private	SOE	Large	Small	Old	Young
	(1)	(2)	(3)	(4)	(5)	(6)
TPA Participation	0.881**	0.981	0.689	1.386***	0.891	0.921*
	(0.400)	(0.840)	(0.612)	(0.497)	(0.659)	(0.492)
Controls	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Province Trend	Y	Y	Y	Y	Y	Y
Industry Trend	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable	9.741	10.037	10.107	9.465	9.875	9.826
IV F-stat	79.094	45.285	54.300	75.621	50.966	67.942
Underidentification LM Statistic	275.071	211.027	323.358	189.583	280.152	226.928
LM statistic P-value	0.000	0.000	0.000	0.000	0.000	0.000
No. of Observations	6,162	3,858	5,560	4,519	5,229	4,851
Adjusted R^2	-0.006	-0.037	-0.018	-0.020	-0.015	-0.022

Notes: The table reports the IV results of effects of the TPA participation on total new loans. The dependent variable in all specifications is $\log(1 + Total\ New\ Loans)$. *TPA Participation* is a firm-year indicator variable that equals 1 if the firm participates in the Targeted Poverty Alleviation program. *Private* refers to private firms; *SOE* refers to state-owned enterprises; Large refers to firms whose average number of employees is above the 50th percentile of the distribution of all the sample firms, while *Small* refers to those below the 50th percentile; *Old* and *Young* in the last two columns represent firms whose average age is above and below the 50th percentile of the distribution of all the sample firms, respectively. All regressions control for $\log(Asset\ per\ Employee)$, *R&D Investment*, $\log(Sales)$, and *ROA*. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.

*, **, and *** denote the significance at the 10%, 5%, 1% level, respectively.

Table 1.10: IV Results: State-Owned Banks v.s. Other Financial Institutions

	log (1+Loans from State-Owned Banks)				log (1+Loans from Other Institutions)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TPA Participation	0.882**	0.861**	0.972**	0.887**	-0.013	-0.138	-0.072	-0.009
	(0.404)	(0.428)	(0.440)	(0.424)	(0.407)	(0.435)	(0.434)	(0.417)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Province × Year FE		Y				Y		
Industry × Year FE			Y				Y	
Province Trend				Y				Y
Industry Trend				Y				Y
Mean of Dependent Variable	9.421	9.421	9.421	9.421	3.824	3.824	3.824	3.824
IV F-stat	97.834	100.150	95.858	98.413	97.834	100.150	95.858	98.413
Underidentification LM statistic	511.332	489.074	507.587	508.295	511.332	489.074	507.587	508.295
LM statistic P-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
No. of Observations	10,083	10,080	10,066	10,080	10,083	10,080	10,066	10,080
Adjusted R^2	0.030	-0.001	0.016	0.019	0.006	-0.025	-0.008	-0.004

Notes: The table reports the IV results of effects of the TPA participation on loans across different financial institutions. The dependent variable in Columns (1) to (4) is $\log(1+Loans\ from\ State-Owned\ Banks)$, while in Columns (5) to (9) is $\log(1+Loans\ from\ Other\ Institutions)$. *TPA Participation* is a firm-year indicator variable that equals 1 if the firm participates in the Targeted Poverty Alleviation program. All regressions control for $\log(Asset\ per\ Employee)$, $R\&D\ Investment$, $\log(Sales)$, and ROA. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.

** denotes the significance at the 5% level.

Table 1.11: IV Results: Effects on Total New Loans across Poverty Level

	log (1+Total New Loans)			
	Quartile 1 (Poorest)	Quartile 2	Quartile 3	Quartile 4 (Least Poor)
	(1)	(2)	(3)	(4)
TPA Participation	1.248***	0.657**	0.143	-0.689
	(0.404)	(0.296)	(0.402)	(0.571)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Province Trend	Y	Y	Y	Y
Industry Trend	Y	Y	Y	Y
Mean of Dependent Variable	10.464	10.761	10.765	10.617
Mean of log (1+TPA Spending)	1.145	0.948	1.002	0.658
IV F-stat	16.142	30.598	17.922	14.816
Underidentification LM statistic	44.738	65.688	54.298	37.495
LM statistic P-value	0.000	0.000	0.000	0.000
No. of Observations	1,005	908	899	760
Adjusted R^2	-0.037	-0.015	-0.016	-0.066

Notes: The table reports the IV second-stage results of effects of the TPA participation on total new loans across regions (provinces) with different poverty levels. I put firms into four quartiles based on the share of National Poor Counties (NPC) in total counties of the province where the firm is located. Quartile 1 represents firms located in provinces that are in top 25% of NPC share distribution. In a similar vein, Quartiles 2, 3, and 4 represent firms located in provinces that are in 50% to 75%, 25% to 50%, and lowest 25% of the NPC share distribution, respectively. The dependent variable in all specifications is $\log(1 + Total\ New\ Loans)$. *TPA Participation* is a firm-year indicator variable that equals 1 if the firm participates in the Targeted Poverty Alleviation program. All regressions control for $\log(Asset\ per\ Employee)$, *R&D Investment*, $\log(Sales)$, and *ROA*. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.

** and *** denote the significance at the 5% and 1% level, respectively.

Table 1.12: IV Results: Subsample without Preferential Borrowing Policies

	log (1+Total New Loans)			
	(1)	(2)	(3)	(4)
TPA Participation	0.358***	0.317**	0.395***	0.351**
	(0.135)	(0.143)	(0.137)	(0.139)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Province × Year FE		Y		
Industry × Year FE			Y	
Province Trend				Y
Industry Trend				Y
Mean of Dependent Variable	10.853	10.853	10.853	10.853
IV F-stat	86.954	88.856	85.591	87.742
Underidentification LM statistic	451.569	433.444	456.435	452.122
LM statistic P-value	0.000	0.000	0.000	0.000
No. of Observations	9,363	9,360	9,346	9,360
Adjusted R^2	0.026	-0.005	0.010	0.016

Notes: The table reports the IV results of effects of the TPA participation on total new loans using the sample which excluded all firms in Sichuan and Anhui provinces where preferential policies exist regarding firm borrowing. The dependent variable in all specifications is $\log(1 + Total\ New\ Loans)$. *TPA Participation* is a firm-year indicator variable that equals 1 if the firm participates in the Targeted Poverty Alleviation program. All regressions control for $\log(Asset\ per\ Employee)$, $R\&D\ Investment$, $\log(Sales)$, and ROA. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.

** and *** denote the significance at the 5% and 1% level, respectively.

Table 1.13: IV Results: Effects of TPA on Firm Profits

	ROA (%)			log (Market Value)		
	(1)	(2)	(3)	(4)	(5)	(6)
TPA Participation	1.454**			0.141***		
	(0.606)			(0.045)		
log (1+TPA Spending)		0.372**			0.036***	
		(0.160)			(0.012)	
log (1+TPA Population)			0.428**			0.041***
			(0.181)			(0.014)
Controls	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Province Trend	Y	Y	Y	Y	Y	Y
Industry Trend	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable	2.356	2.356	2.356	22.416	22.416	22.416
IV F-stat	98.338	84.424	69.363	98.413	84.507	69.380
Underidentification LM statistic	507.668	312.182	188.949	508.295	312.751	188.984
LM statistic P-value	0.000	0.000	0.000	0.000	0.000	0.000
No. of Observations	10,071	10,030	9,628	10,080	10,039	9,636
Adjusted R^2	-0.006	-0.009	-0.009	0.092	0.093	0.090

Notes: The table reports the IV results of effects of the TPA participation on firm profits. *TPA Participation* is a firm-year indicator variable that equals 1 if the firm participates in the Targeted Poverty Alleviation program. *TPA Spending* is the amount of money that a firm spends on the TPA program in a year; *TPA Population* is the number of poor people who benefit from a given firm's specific anti-poverty policies in a year. All regressions control for log(*Asset per Employee*), *R&D Investment*, log(*Sales*), and *ROA*. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.
** and *** denote the significance at the 5% and 1% level, respectively.

Table 1.14: IV Results: Effects of TPA on Sales and Costs

	log (Sales)			log (Costs)		
	(1)	(2)	(3)	(4)	(5)	(6)
TPA Participation	0.069*			0.034		
	(0.038)			(0.040)		
log (1+TPA Spending)		0.019*			0.009	
		(0.010)			(0.011)	
log (1+TPA Population)			0.025**			0.014
			(0.012)			(0.012)
Controls	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Province Trend	Y	Y	Y	Y	Y	Y
Industry Trend	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable	21.448	21.448	21.448	21.100	21.100	21.100
IV F-stat	95.857	83.219	65.468	98.402	84.497	69.375
Underidentification LM statistic	507.585	321.275	192.816	508.097	312.556	188.865
LM statistic P-value	0.000	0.000	0.000	0.000	0.000	0.000
No. of Observations	10,064	10,023	9,622	10,078	10,037	9,634
Adjusted R^2	0.420	0.420	0.415	0.412	0.412	0.407

Notes: The table reports the IV results of effects of the TPA participation on firm sales and costs. *TPA Participation* is a firm-year indicator variable that equals 1 if the firm participates in the TPA program. *TPA Spending* is the amount of money that a firm spends on the TPA program in a year; *TPA Population* is the number of poor people who benefit from a given firm's specific anti-poverty policies in a year. All regressions control for log(*Asset per Employee*), *R&D Investment*, log(*Sales*), and *ROA*. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.
* and ** denote the significance at the 10% and 5% level, respectively.

Table 1.15: TPA and Capital Misallocation

	TFPR Dispersion	
	(1)	(2)
Share of TPA Firms	-0.583*	-0.663**
	(0.306)	(0.300)
Controls		Y
Industry FE	Y	Y
Year FE	Y	Y
Mean of Dependent Variable	0.623	0.623
No. of Observations	166	166
Adjusted R^2	0.660	0.680

Notes: The table reports the OLS results of effects of the TPA participation on within-industry capital misallocation using the specification (1.4). The dependent variable in both columns is the capital misallocation measured by TFPR dispersion within a given industry defined in Hsieh and Klenow (2009). *Share of TPA Firms* represents the share of firms that participated in the TPA program in a industry. Both regressions control for log(*Asset per Employee*), *R&D Investment*, log(*Sales*), and *ROA*. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.
* and ** denote the significance at the 10% and 5% level, respectively.

Chapter 2

Am I An Impostor? The Psychological and Educational Impacts of Being Admitted by Affirmative Action

2.1 Introduction

Affirmative action is a policy tool that is commonly used to rectify historical inequalities and improve the representation of disadvantaged minority groups. Despite being adopted by a wide range of countries and organizations — ranging from Indian village governments to Malaysian firms and South African sports teams — its use remains highly controversial in many settings (Bagde et al., 2016) (e.g., Bagde et al., 2016; Lee, 2016; Miller, 2017; Khanna, 2020). In the US, college admissions are at the center of the debate around affirmative action. Many elite universities employ affirmative action to boost the socioeconomic diversity of their incoming class and improve the educational outcomes of minority students.

Yet affirmative action has a theoretically uncertain impact on minority student outcomes. Proponents of affirmative action argue that it provides minority students access to educational opportunities at elite institutions that would not be available otherwise (e.g., Bagde et al., 2016; Cassan, 2019).[1] Affirmative action might also have positive incentive effects: by bringing previously unattainable goals within reach, it

[1] Long (2004) estimates that accepted minority students in elite colleges would be reduced by 27 percent if preferential admissions for minority students in the United States were eliminated. Howell (2010) fins advantages in admission for Black and Hispanic applicants among the most selective colleges.

may encourage younger minority students to invest more in their human capital — while possibly also weakening incentives for high-ability minority students who might now get in more easily. On the other hand, affirmative action may actually worsen the educational outcomes of minority students because of "mismatch" — the idea that disadvantaged minority students may be unprepared for the rigorous curriculum at elite institutions and may do worse as a result (Fryer and Loury, 2005; Arcidiacono et al., 2011; Arcidiacono and Lovenheim, 2016). Underlying this may be feelings of self-doubt — indeed, poor self-image and impostor feelings may be either the cause — or effect — of minority students' poor academic performance in elite environments (Leslie et al., 2014). Despite the large literature on affirmative action in education, very little work has studied the psychological effects of affirmative action.

In this paper, we investigate the psychological effects of being admitted through affirmative action and document how it affects academic achievement and career aspirations. It is empirically challenging to study how being admitted via affirmative action affects minority students, for two reasons. First, the measurement challenge: in many systems (e.g., the US college admissions system) we often do not know which minority applicants were admitted via affirmative action and who would have been admitted on merit nonetheless. This information is invisible to both the researcher and the admitted student. Second, there is a significant identification challenge: minority students admitted through affirmative action are likely quite different from students admitted on merit along many dimensions. They may be of lower ability and from different schools or social backgrounds. Naive OLS regressions might misattribute the effect of these factors to affirmative action. The ideal experiment to examine this question would inform randomly selected minority students that they were admitted because of affirmative action, and study how their self-image, educational performance, and future plans change.

Chinese college admissions provide a unique setting to approximate this ideal experiment. Some students get bonus points — typically ethnic minorities and students with special skills (e.g., musical talent or winners of academic competitions like math Olympiads). Students know both how many bonus points they receive and each college's cutoff, so they know if they qualified on merit or through bonus points. But importantly, this is private information to the students — teachers cannot find out how many bonus points a student receives and it is seldom discussed by peers. Our data on Chinese college admissions and bonus points comes from a survey of two cohorts of students in 15 selective colleges in Beijing.

We begin by presenting some descriptive facts about affirmative action in the Chinese college admissions context. Overall, 7.5 percent of students receive some ethnicity-based bonus points. On average, minority students in our sample are at the 45th percentile of the college entrance exam score distribution. Affirmative action takes them to the 50th percentile. In the group studied, 41.7 percent enter via affirmative action, while 58.3 percent enter on merit. The fraction of minority students who enter via affirmative action varies from 10 percent in some colleges to 70 percent in other colleges and is higher in more elite colleges.

Second, we identify the causal effect of qualifying via affirmative action on minority students using an RD design. Focusing on minority students, we compare students who scored just above the cutoff (i.e., who qualified on merit) against students who scored just below the cutoff (i.e., who qualified via affirmative action). All students attend the same college and track, so our RD identifies the causal effect of how a student qualified for a given college rather than which college a student attends. Moreover, since bonus points are private information, we should interpret the treatment effect of scoring just below the cutoff as the effect of knowing that you qualified through affirmative action. As a placebo, we estimate the same RD specifi-

cation with students who receive bonus points for merit-based reasons (like winning an academic competition). We present three main empirical findings.

First, we show that being admitted via affirmative action worsens self-image. Students admitted via affirmative action have significantly worse self-assessment of academic ability. We construct an index of self-image assessment and find that minority students who qualify via affirmative action have approximately 0.8 sd worse self-image. Students admitted via affirmative action have weaker social connections with their peers and report greater depression and more emotional problems. These patterns are not seen with the placebo group which receives merit-based bonus points and are robust to a battery of RD specification tests including different controls, polynomials, bandwidths, kernel measures, placebo cutoffs, and measures of self-image. Turning to heterogeneity, we show that being admitted via affirmative action has a particularly negative impact on the self-assessment of students from rural areas, more disadvantaged socioeconomic backgrounds, and who attend more elite colleges. This suggests that affirmative action may have a particularly negative psychological impact on students with less prior exposure to elite settings, who may be most at risk from developing impostor feelings.

Second, we show that students admitted via affirmative action perform worse academically. They score lower on a standardized English test, are more likely to skip class, and are less likely to earn academic rewards. Quantitatively, the magnitudes of these results are large: students score 0.6 sd lower on the English test, are 0.3 sd more likely to skip class, and 0.8 sd less likely to receive academic rewards.

Third, we show that being admitted via affirmative action affects students' career plans. Treated students are 0.2 sd more likely not to have decided their future career at the time of the survey, were 0.6 sd more likely to state an interest in stable public sector careers, and 0.5 sd less likely to express an interest in entrepreneurship as a

career. They are also less likely to be Communist Party members. These findings are suggestive that the mode of admission — affirmative action or merit — can have longer-term impacts on minority students' economic outcomes.

Why does being admitted via affirmative action change the behavior of minority students? We argue that peer and teacher responses are unlikely to explain this, since minorities in China are often hard to identify from their names (Uyghurs and Tibetans are well-known exceptions) and bonus points are private information and seldom discussed. Instead, we argue that the negative effect of affirmative action on performance is driven by perceptions of low self-competence. Results from the placebo test suggests that qualifying on merit provides a coarse signal about one's own ability, which students factor into their human capital investment decisions. However, this requires some bounded rationality, limited attention, or rule-of-thumb behavior on minority students' part, since they know their position relative to the cutoff and marginal students know how close they were to get in on merit.

We believe this paper contributes to the literature on affirmative action in several ways. First, we document how many minority students are admitted to elite colleges via affirmative action and how many would have been admitted on merit even if affirmative action were not in place. This is an important descriptive fact which we are able to produce due to China's unique system of affirmative action in college admissions, where minority students are awarded bonus points. It would not be possible to produce an equivalent number in the US, even though it is the case that some minority students would be admitted even if affirmative action were not in place (Yagan, 2016).

Second, we are one of the first papers that credibly estimate the psychological consequences of affirmative action. We show that being admitted via affirmative action can trigger impostor feelings amongst high-achieving minority students. These

impostor feelings can affect academic achievement and career plans and therefore have longer-run consequences. We believe our paper constitutes among the first well-identified field (i.e., not lab) evidence of the impostor phenomenon.

Third, we complement the literature on affirmative action that explores why minority students – beneficiaries of affirmative action – struggle in elite institutions. The mismatch hypothesis holds that students admitted via affirmative action may be unprepared for the academic rigors of an elite college (e.g., Bagde et al., 2016; Arcidiacono and Lovenheim, 2016; Barrow et al., 2020). In our context, this cannot be the mechanism, since students admitted via affirmative action very narrowly missed the cutoff and are thus just as academically well-prepared as minority students who qualified on merit. Instead, our results suggest another reason why minority students might perform poorly in elite environments: that being admitted via affirmative action triggers impostor feelings that worsen self-image and ultimately performance.

The remainder of this paper is organized as follows. Section 2.2 briefly introduces affirmative action in the college admission system in China. Section 2.3 describes the data and measurements of key outcomes. Section 2.4 discusses the empirical strategy and its validity. Section 2.5 presents the main empirical results, and Section 2.6 concludes.

2.2 Institutional Background

The National College Entrance Examination (also known as the *Gaokao*) is one of the most important institutions in China, and it has served as the only academic criterion for college admission since 1952. Chinese colleges and universities rely on exam scores to evaluate and admit students. This admission system seems to be a relatively reliable method for distributing educational opportunities equally; nevertheless, it is also recognized that allocating educational resources solely based on the exam score

is not perfectly equitable, particularly in a country with such a large population and a great variety of ethnic groups. Thus, China has implemented affirmative action policies in college admission to mitigate the inequality in higher education access. Below we discuss affirmative action in the Chinese college admission system in more detail.

2.2.1 College Admission in China

China's exam-based college admission system primarily consists of examination, application submission, and admissions (Wu and Zhong, 2014). Figure 2·1 shows the timing of different events in the Chinese college admission in 2006 as an example. Every year, college applicants take the National College Entrance Examination in early June.[2] The exam has two independent tracks - science and humanities (or liberal arts).[3] Each track has its own exam papers with a full score of 750 points for both tracks. Specifically, both tracks include Chinese, English, and Mathematics, each of which has a full score of 150 points, in the exam. In addition to these three subjects, students in the science track need to take a comprehensive test in physics, chemistry, and biology, while students in humanities take the test in politics, history, and geography. To better match the high school curricula in different provinces and ensure equity in education resource allocation, the exam papers are designed, administrated, and graded by the education authority of each province independently.[4] Around two weeks after the exam, the provincial education authority releases the exam score and the ranking within the province for every student. Students then start their college applications by submitting a rank-ordered list of 4-6 colleges and at most 6 majors

[2]The exam dates are basically the same for all provinces throughout China every year. Nevertheless, the exam can be postponed under extremely special circumstances. For example, due to the risk of COVID-19, the exam in 2020 was postponed to early July.

[3]Students choose a track at the end of the first year of high school.

[4]Note that although a few provinces employ the exam papers provided by the Ministry of Education for some subjects, those provinces still have full discretion in grading the exam.

in each college based on their own preferences.

Colleges begin admissions for each province right after the period of application. Before 2008, the admission process followed the "Boston Mechanism" under which colleges admit students who list that college as their first choice unless the college's capacity is not full in the first round of admission, which is extremely rare.[5] But unlike the admissions in the United States where colleges have full discretion in determining the size of a cohort and whom they would like to admit, China's college admissions exhibit a centralized quota attribute. That is, admission quotas for each college are specific to both province and track; they are also predetermined before the exam by the Ministry of Education of China.[6] Colleges are not allowed to reject any students assigned to them by the matching algorithm as long as there are still available seats within the provincial quotas. Students then compete with others within the same province/track for better schools, and can only be admitted by one college. Their priorities in the admission process are solely determined by total exam scores. In turn, the cutoff of a certain college is the lowest score among all admitted students, which is publicly released shortly after the admission is completed. Thus, for a given cohort, the cutoffs of a college are province-track specific.

2.2.2 Affirmative Action for Ethnic Minorities in the Admission

While admitting students solely based on exam scores seems most equitable for selecting the talent, the mode of exam-based unified admission system still raises enormous

[5]Being aware of the high risk of inequality in college admissions, since 2008, the Chinese government has reformed the admission algorithm from the Boston to Deferred Acceptance mechanism in which the admission process will not stop until all the preferences have been considered. In 2008, more than 20 provinces adopted the Boston mechanism. Nonetheless, as of 2016, almost all provinces have implemented the Deferred Acceptance algorithm (Bo et al., 2019).

[6]For example, in 2008, Peking University planned to admit 39 science students and 25 students from humanities in Jiangsu Province, while the admission quota for Shanghai is 22 and 18, respectively. In principle, the allocation of college quotas across provinces is based on the quality of the colleges, population, and socioeconomic development of the province, with the aim of balancing the regional development.

concerns about inequality in higher education access for a country with such a large population and a great variety of ethnic groups.[7] On the one hand, given the regional differences in educational endowment, students from some minority communities and underdeveloped areas have fewer educational resources and a poorer quality of education. Adopting the same cutoff for all students may lead to inequality in access to college education among rich and poor areas within a province. On the other hand, the high-stakes nature of the "once-a-year" exam lowers parents' perceived return to education, affecting family decision-making on students' human capital investment, especially for socially disadvantaged groups. Being aware of the disparities in higher education access and potential social instability induced by the admission system, the Chinese government introduced affirmative action policies to narrow the gaps between Han and minorities by granting bonus exam points to ethnic minority students.[8]

Each province has its own affirmative action policies applied to ethnic minority students. In most provinces, the bonus points are granted based on students' ethnicity.[9] For example, Hui students in Ningxia Hui Autonomous Region are given 20 bonus points in college admissions, while other minorities receive 10 points. There are also some provinces and autonomous regions that only grant bonus points to specific ethnic groups. Inner Mongolia Autonomous Region stipulated that only Daur, Ewenki, Mongolian, and Oroqen students are eligible to receive 10 bonus points, while other minorities merely have priority in admission over Han students with the same exam scores. It is also very common that ethnic minority students from specific compact minority communities (minority autonomous counties) may enjoy preferential

[7] According to the sixth wave of the national census, the total population of China in 2010 is 1.34 billion. Ethnic minorities comprise approximately 9 percent of the total population.

[8] This is equivalent to lowering the cutoff scores of regular admissions for ethnic minority students.

[9] A student can be recognized as an ethnic minority if one or both of their parents are ethnic minorities. In a mixed ethnic family, parents are free to choose whichever of their two ethnicities for their children. The social norm in China is to follow the father's ethnicity. However, there is an increasing trend in ethnic minority identification, particularly in a minority-Han family, that parents choose minority status for their children for the sake of preferential policies (Jia and Persson, 2021).

treatment over those from non-compact areas. Hebei Province gives 10 extra points to minority students residing in ethnic autonomous counties. However, those who live in non-minority communities only receive 5 points (Wang, 2007).[10] Thus, the affirmative action policies for minority students are approximately idiosyncratic at the county level. Bonus points received by the same ethnic minority vary both within and across provinces.

The introduction of affirmative action policy in China's score-based college admission system provides us with a promising institutional context to study the effect of affirmative action quantitatively, which is usually challenging in the U.S. contest. In the U.S., universities will comprehensively evaluate an applicant's academic performance, extracurricular activities, and personal background. Sometimes students are interviewed as well, and these interviews may affect admissions decisions. Accordingly, the admissions threshold of each college is difficult to quantify. At the same time, we often do not know which minority applicants were admitted via affirmative action and who would have been admitted on merit nonetheless. This information is invisible to both the researcher and the admitted student. Although students can infer whether they have benefited from affirmative action by comparing their SAT scores and high school academic performance with their peers, it is still hard to quantify other indicators such as extracurricular activities. Notably, Chinese students know exactly how many bonus points they will have in admissions before taking the exam. Most minority students learn that they enjoy preferential treatment during college admissions from their parents or official documents during high school.[11] After the admission results and college cutoffs are announced, students will also know if they

[10]Please see Wang (2007) for a comprehensive description of affirmative action policies in each province for ethnic minorities during China's college admissions.

[11]Thus, the direct comparison of minority and Han students to obtain the potential effects of affirmative action might be misleading because the incentive to study hard could be systematically lower for minorities than that of Han students.

have qualified on merit or through bonus points. However, affirmative action beneficiaries seldomly discuss how they are admitted in college with the concern of negative stereotypes and losing the respect of peers, which is important in Chinese culture. Indeed, all information during the admission process is private to the students — teachers in colleges cannot find out how students are admitted and how many bonus points a student receives. This bonus point nature of affirmative action allows me to obtain an RD estimate of the impact of these policies. Admittedly, teachers can identify a minority student from names (e.g., Uyghurs and Tibetans). We provide comparisons between minorities qualified on affirmative action and those qualified on merits, which precludes the concern that the differential treatment from teachers confounds the interpretation of our results.

Figure 2·2(a) plots the distribution of bonus points received by minority students in our sample. Each bar represents the percentage of students who received corresponding bonus points. As shown, bonus points granted to ethnic minority students mostly fall in the 5- to 20-point range within 20 points, with nearly half of the students receiving 10 points. There are also some rare cases in which students are given more than 30 points. For instance, in Xinjiang, 50 points are granted if students belong to specific ethnic groups and from certain minority communities. While the bonus points make up a very small proportion of total exam scores ($10/750 \approx 1.3\%$), the impacts on college attainment for minority students are substantially significant. In 2010, minority students scored 18 points, on average, lower than Han students in the exam, but college acceptance rates for minority students are higher. Indeed, the effects are more salient in first-tier or elite college admissions. 16.8 percent of ethnic minority students were admitted by the first-tier Chinese colleges, which is about 5 percentage points higher than for Han students (Wen, 2014). Despite the fact that this discrepancy is widely reported, little is known about the short- and

long-run effects of affirmative action policies on beneficiaries both economically and psychologically. More importantly, the literature to date has not provided any clear evidence on why and how affirmative action shapes students' beliefs and behaviors. Taking the advantage of a novel RD setting in Chinese college admission, our paper fills this gap by studying the impact of affirmative action on students' self-image, academic performance, and decision-making behaviors.

2.2.3 Other Preferential Policies in Chinese College Admissions

Aside from ethnic minorities, many other groups enjoy preferential treatment in the Chinese college admissions process. These groups, which include children of martyrs[12], overseas Chinese, military personnel, and residents of Hongkong, Macao, and Taiwan, receive between 10 and 20 points for their special status. Nonetheless, these groups comprise a much smaller numbers of beneficiaries than ethnic minority students, amounting to about 0.22 percent of our sample.

In contrast to previously discussed policies, which are available only to particular groups based on ethnicity or family background, a parallel merit-based bonus policy is implemented with the aim of encouraging the development of students' specialties (Administrative Provisions on General Institutions of Higher Education, 1987). Students who have special talents in sports, music, science, or math (e.g., national athletes or students who have won prizes in the high school science/math Olympiad) are eligible to receive bonus exam points. Similarly, the number of merit-based bonus points is determined before the exam, varying slightly across provinces. Figure 2·2(b) shows the distribution of merit-based bonus points received by students. As shown in the figure, most students receive 10 or 20 bonus points for their exceptional talents, which are in the same range as the number of bonus points of affirmative action for minorities. But unlike ethnicity-based bonus points, which are solely determined by

[12]Martyr is an official designation for people who have sacrificed their lives for patriotic reasons.

the ethnicity of the student, merit-based bonus points are obtained through personal effort; they often serve as an acknowledgment of students' talents and signal higher ability to both college and students themselves. Indeed, students who benefit from merit-based points usually receive respect and admiration from peers in high school. In turn, these students are relatively more confident in themselves. Given the distinct feature of ethnicity- and merit-based bonus policies, the analysis of the merit-based group provides a placebo test to further validate our main hypothesis and help us understand why and how affirmative action discourages minority students.[13]

2.3 Data and Variables

2.3.1 Data

We use data from the 2009 *College Student Development Survey* (CSDS), an individual-level survey of a representative sample of all full-time undergraduate students enrolled in 54 public universities in Beijing that investigates socioeconomic problems in Chinese higher education, such as social stratification, student academic activities, and career trajectories. To ensure the representativeness of the sample, the CSDS employs a stratified and probability-proportional-to-size random sampling method. Specifically, the CSDS first randomly selects 15 universities from all 54 public universities in Beijing, stratified by college affiliation and tiers.[14] Within each selected college, it

[13]We do not study those who were admitted through independent recruitment programs, as colleges have full autonomy in independent recruitment, similar to the U.S. admissions system, and the program involves many subjective criteria such as interviews and recommendations in the review process. The relatively subjective criteria leave room for under-the-table deals and corruption. A notorious example of this came to light when the Chief Director of Admissions for a top Chinese University was arrested and prosecuted for corruption by fabricating and falsifying students' information in the independent recruitment admissions process (https://www.chinadaily.com.cn/china/2014-05/30/content_17554126.htm). Thus, it is difficult to tell why and how students are admitted in these cases.

[14]There are six strata in the sampling design. The six strata are (1) Peking University, (2) Renmin University, (3) Tsinghua University, (4) "Project-211" universities affiliated with the Ministry of Education, (5) non-"Project-211" universities affiliated with the Ministry of Education, and (6) universities affiliated with the Beijing municipal government. "Project-211" is a designation for 100 universities selected by the Chinese Government, that are generally considered to be top-quality

then randomly draws 15 to 25 majors and 20 students from each selected major on the database of registration records for all students matriculated in 2006 and 2008 (i.e., freshman and junior students at the time of the survey). To protect the privacy of respondents through bundling of potentially sensitive questions, the questionnaires are completed by students individually and anonymously. And at the beginning of the questionnaire, respondents are told that the survey will only be used for research purposes and their personal information will be kept confidential. Ultimately, the survey successfully surveyed 4771 students, including 2174 freshmen and 2283 juniors who reported complete information on the year when they matriculated, with a high response rate of 93.5 percent (Wu, 2017).

The CSDC data contain rich information on students' pre- and within-college life and cover a broad range of topics regarding Chinese college students themselves. Besides the basic demographic information, such as gender, age, and ethnicity, the survey included detailed information on each student's college admission, major, academic activities, aspirations, job preferences, and social values and behavior in college. Importantly, the survey collected data on each student's assessment of self-image (e.g., their perceived ability to complete different tasks and assessment of personal qualities) and several psychological measures (e.g., anxiety, depression, life attitudes, etc.) which can help us understand the factors that drive students to behave differently and how they affect students both economically and psychologically. In addition, the questionnaire also included questions about family background, such as parent's education level, family income, self-reported family socioeconomic status, and family's Communist Party of China membership status, etc.

It is worth stressing that a challenge to study the impact of affirmative action is the lack of data measuring the extent of affirmative action quantitatively and

institutions. Since the enrollment scale of top universities is relatively small even after the rapid expansion of higher education in China, the survey stratified top universities separately in the sampling design to ensure enough sample size for those universities.

consistently across different colleges. For this project, we are able to leverage the fact that the CSDC constructed a unique data set that illustrates the interplay between affirmative action policy and students' college activities. The survey collected detailed information on the college admissions process of each student, including how the student was admitted by the college, what the total score of the college entrance exam is, and whether the student enjoyed affirmative action or other preferential treatment during the admissions process. Importantly, if students are eligible to enjoy lenience in admissions, we also know what the preferential policies are and how many bonus points students received during the admissions process. Overall, around 7.5 percent of students are eligible to receive some ethnicity-based bonus points in the survey, while 7.2 percent enjoy merit-based bonus points. As we have discussed in Section 2, Figure 2·2 plots the distribution of bonus points received by students in our sample. On average, minority students in our sample are at the 45th percentile of the college entrance exam score distribution. Affirmative action takes them to the 50th percentile.

In addition to the survey data, we collected administrative records of province-track specific admissions cutoffs for the 15 universities in the survey in 2006 and 2008, allowing us to identify who entered via affirmative action, and who qualified on merit. Given the focus of our paper, our sample encompasses 334 minority students who benefited from affirmative action.[15] Of the students who enjoyed ethnicity-based bonus points (hereafter "ethnicity-based group"), 41.7 percent entered via affirmative action, while 58.3 percent qualified on merit. The fraction of minority students who entered via affirmative action varies from 10 percent in some colleges to 70 percent in other colleges. We find that this fraction is higher in more elite colleges. As shown in Table 2.1, approximately 30 percent of minority students were admitted through

[15]In our sample, 323 students received merit-based bonus points. We rely on this subsample to conduct the placebo analysis.

affirmative action in non-"Project-211" universities. This fraction increases to around 50 percent for top-3 elite colleges on average.

2.3.2 Key Outcomes

Self-image Assessment We construct a self-image index using responses from survey questions in the CSDC asking each student to assess the extent to which they agree with five statements reflecting higher self-efficacy. Those statements include "I am satisfied with my ability", "I feel as competent as others", "I can complete the most difficult academic tasks", "I am positive about myself", and "I feel I have good qualities". The responses to statements range from a scale of 1 ("Strongly Disagree") to a scale of 5 ("Strongly Agree").[16] We then constructed the summary index of self-image assessment by calculating a z-score for each component statement and taking the average z-score across components, in order to address the concern about multiple hypothesis testing and reduce the likelihood of inappropriately limiting reported effects to those that are significant.[17] To check the robustness of our results, we also consider an alternative measure of self-image assessment, where we computed a self-image percentile rank for each student in the sample by taking the first principal component of those five statements about self-efficacy.

Social Connection We measure a summary social connection index in the same way as the construction of the self-image assessment index described above. Specifically, we calculate a z-score for each response variable which is based on the answers to questions asking students about the closeness to three groups: other students, professors, and department staff, with five responses ranging from "not close at all"

[16] All responses include "Strongly Disagree=1", "Disagree=2", "Undecided=3", "Agree=4", and "Strongly Agree=5".

[17] To ease the interpretation of the results, we standardized the each response variable such that they all have a mean zero and standard deviation of one.

(or 1) to "very close" (or 5). The social connection index is the average z-score over these three components.

Academic Performance Although the direct measure of academic performance, i.e. GPA, is not available in the data, the CSDC still collected rich information on students' college academic activities, which can help us infer the academic performance of a student explicitly. We first used the score of the standardized College English Test (CET-4) as a proxy of academic performance. The CET-4 is a national English test that examines the English proficiency of Chinese college students from all majors. Since the CET-4 is a national standardized test and is graded consistently throughout China, scores of the test are comparable both within and across colleges. To large extent, the test score captures a student's performance in college. Nevertheless, there is a concern that the CET-4 test score reflects the inherent ability in English of a student, rather than the college performance. To mitigate this concern, we further investigate a student's likelihood of skipping classes and of receiving academic awards during college, which are closely related to academic performance and thus meaningful indicators of college academic performance.

Career Plans To shed light on the longer-term impacts of affirmative action on minority students, we first define a dummy variable which equals 1 if the student has future plans after graduation (e.g., attend a graduate school in China, study abroad, and work, etc.) at the time of the survey, and 0 otherwise. We further construct three indicator variables for jobs, dividing them into three categories: public sector, private sector, and entrepreneurial jobs, to study how affirmative action affects students' preferences in career planning.

Summary statistics for all variables are presented in Table 2.2.

2.4 Empirical Strategy

2.4.1 Econometric Specification

To estimate the causal effects of affirmative action on minority students, we employ a regression discontinuity (RD) design that compares students who scored just below the admission cutoff (i.e., who qualified via affirmative action) against students who scored just above the cutoff (i.e., who qualified on merit). All students attend the same college and track, so our RD estimate identifies how a minority student qualified for a given college rather than which college a student attends. The baseline RD specification is as follows:

$$\begin{aligned} Y_{ipcyt} =& \alpha + \beta I(Score_i < Cutoff_{pcyt}) + \theta_1 f(Score\ Margin_i) \\ &+ \theta_2 f(Score\ Margin_i) \times I(Score_i < Cutoff_{pcyt}) + \gamma_y + \lambda_{pct} + \epsilon_{ipcyt} \end{aligned} \tag{2.1}$$

where Y_{ipcyt} denotes the outcome variable of interest for student i from college c in province p, cohort y and track t. Indicator function $I(Score_i < Cutoff_{pcyt})$ equals one if the student's exam score is below the admissions cutoff of the college (s)he attends, and zero otherwise. As mentioned in Section 2.1, for a given cohort, the cutoffs of a college are province-track specific. Since the sample consists of students who are eligible to benefit from affirmative action, the dummy variable also captures students who qualified via affirmative action (actual beneficiaries). $Score\ Margin_i$ denotes the distance between the exam score of a student and the cutoff scores for the college that the student attends. γ_y and λ_{pct} refer to cohort and province-college-track fixed effects. All standard errors from parametric methods are two-way clustered at the college and province levels.

To preclude the concern that our estimation results might be vulnerable to the misspecification of the function form of the running variable, as discussed in Lee and Lemieux (2010), we employ both the non-parametric local linear approach and

the parametric method for estimating Equation (1). Our baseline local linear approach specifies a triangular kernel in the estimation. We later discuss alternative specification using rectangular and Epanechnikov kernels in the robustness check. In parametric estimations, we allow the function for running variable to differ across the cutoff scores by including the interaction between $f(Score\ Margin_i)$ and the indicator function $I(Score_i < Cutoff_{pcyt})$. Following Lee and Lemieux (2010), we show that our estimation results are robust to both linear and quadratic terms of the distance to cutoff scores. To improve the precision and reduce the sampling variability in the RD estimates, we control for gender, age, family income, and parent's education as a robustness check.[18]

We employ the method developed by Calonico et al. (2020) to compute the optimal bandwidths, which fall in the range between 16 and 24 points across different outcomes in our analysis. Since the bonus points granted to ethnic minority students mostly fall in the 5- to 20-point range, we limit our analytic sample to those who fall within 20 points of the admission cutoff scores. As robustness checks, we also report estimates from specifications with a set of alternative bandwidths.

2.4.2 Validity of RD Specification

The validity of an RD design relies on the assumption of local random assignment around the discontinuity threshold. For this project, the assumption primarily requires that there should not exist a non-random sorting either below or above cutoffs. Although students do exert control over the exam score through studying efforts, we argue that it is unlikely for a student to manipulate whether (s)he is below or above the admissions cutoffs or precisely sort around the cutoffs. As discussed in Section 2.1, the admissions cutoff scores of a college for a given province and track are the

[18]The random nature of the RD design makes it so that control variables need not be included to obtain consistent estimates, so we do not include any additional controls in our baseline specification.

lowest scores among all admitted students *ex post*. These cutoffs vary across colleges and years. Even though students may have a general expectation of the tier of colleges they will be able to reach, no one knows precisely what college (s)he will attend and what the cutoffs are *ex ante*. Thus, it is impossible for students to manipulate their placement. Figure 2·3 depicts the distribution of exams scores in terms of the distance in points to the cutoff scores. The curve represents the kernel density estimate of the distribution, showing a smooth density around the cutoffs. We also perform a formal McCrary test to examine whether there is a discontinuity in the density at the cutoffs and obtain a p-value of 0.7475, which fails to reject the null hypothesis that the density is smooth around the cutoffs, suggesting that there is no evidence of manipulation or bunching around the cutoffs.[19]

Second, the local randomization assumption also requires that students just below and above the cutoff scores are similar in their pre-college characteristics, which guarantees that pre-treatment characteristics cannot influence the treatment assignment, and that the control group (i.e., who qualified on merit) is an appropriate counterfactual of the treatment group (i.e., who qualified via affirmative action). Thus, we conduct balance tests on students' individual characteristics and family backgrounds. Figure 2·4(a) – 2·4(h) depict the smoothness of gender, age, family income, parental levels of education, parental status of being a CPC member, and living in rural/city areas before the college, across the admissions cutoffs. In addition to the graphical evidence, we further formally estimate regressions of Equation (1) using these characteristics as dependent variables. To be consistent with the baseline estimation, we use the parametric estimation method with quadratic polynomial under 20-point bandwidth. Table 2.3 reports the corresponding estimates for balance tests. The statistically insignificant results in Columns (1) and (2) indicate that there is no discontinuity in student demographic characteristics. Additionally, Columns (3) to (8),

[19]See McCrary (2008) for details on this procedure for inference.

showing the balance for students' family background, further ensure that there are no substantial disparities in students characteristics across the cutoff scores. Taken together, these findings significantly increase our confidence on the validity of the RD design in our setting.

2.5 Empirical Results

2.5.1 Affirmative Action and Self-Image

Main Results

We first present results showing how being admitted via affirmative action affects minority students' self-image. Figure 2·5 shows a clear discontinuity at the cutoff, indicating that minority students admitted on merit have significantly higher self-image than minority students admitted to the same college and track with the help of ethnicity-based bonus points. Columns 1-3 of Table 2.4 demonstrate that the effect sizes are large: being admitted via affirmative action reduces self-image by approximately 0.8 s.d., and the statistical significance is not affected by the parametric fit of the polynomial. Next, we present results from our placebo test, which compares students admitted on merit (i.e., who met the entrance exam cutoff) against students who were admitted with the help of academic-based bonus points (such as winning a science competition or math Olympiad). Columns 4-6 of Table 2.4 show that there is no difference between these two groups of students, suggesting that the treatment effect we pick up in columns 1-3 stems not just from failing to qualify on merit, but rather from qualifying on the basis of ethnicity-based bonus points. This strengthens our claim that ethnicity-based affirmative action is causing its beneficiaries – minority students who are admitted to elite colleges through it – to have worse self-image.

Robustness Checks

In this section, we conduct a series of robustness checks on our baseline RD results.

We first check whether our findings might be driven by the choice of the bandwidth. Figure 2·6 plots the point estimates of the RD regression based on Equation (1) along with the 90 percent confidence interval using a set of alternative bandwidths ranging from 10 points to 30 points. Results in Panel (a) and Panel (d) are obtained from non-parametric local linear regressions, while others are from parametric regressions with either linear or quadratic polynomials. As shown in the figures, the RD estimates are stable under different bandwidths. Affirmative action has a negative and statistically significant effect on self-image for minority students, while we do not find a significant impact for the placebo group in which students benefit from merit-based bonus points.

Second, following Imbens and Lemieux (2008), we also test for discontinuity at placebo cutoff scores. The identifying assumption requires that there should be no jumps; the presence of jumps would indicate a misspecification of the RD estimation. To be consistent with our baseline specification and avoid including samples around the cutoff where it is known to have a discontinuity, we estimate the RD regression with 20-point bandwidth at points that are 20-point below and above the cutoff scores. Specifically, we run estimations for students whose exam scores are within [-40,0] and [0,40] ranges around the true cutoff scores. Thereby, two placebo cutoffs are -20 and +20 in terms of the distance to the true cutoff scores, respectively. Table 2.5 shows RD estimates at placebo cutoffs for the self-image index. While the point estimate from the local linear approach is marginally significant at the -20 placebo cutoff, none of the parametric specifications support the jumps at placebo cutoffs.

Third, we repeat the baseline non-parametric local linear estimation using Epanechnikov and Rectangular kernel functions, respectively, as robustness checks. The corre-

sponding estimates, presented in Table 2.6, are largely similar to our baseline results, both in magnitude and statistical significance.

We also calculate a self-image percentile rank for each student in the sample by taking the first principal component of those five statements about self-efficacy as the outcome variable in order to ensure that our findings are not prone to specific measurement of the assessment of self-image. The results are reported in Table 2.7. As shown, the results are largely consistent with our baseline ones, lessening the concern over the measure of the self-image assessment index.

Finally, we show results controlling for student characteristics and family background, including gender, age, family income, parental education level, and parental CPC status (Table 2.8), and weighted results by sampling probability (Table 2.9). As expected, the RD estimates in all specifications are very close to the baseline estimations for minority students and placebo groups in terms of both magnitudes and significance level.

Heterogeneity

We explore heterogeneity along four different dimensions, examining in each case how the effect of being admitted via ethnicity-based bonus points on self-image varies with a particular characteristic. Results are presented in Table 2.10. Column 1 indicates that being admitted via affirmative action has a particularly deleterious impact on minority students from rural areas. Columns 2 shows that being admitted via affirmative action worsens the self-image of low-SES minority students. Column 3 shows that attending an elite college exacerbates the impostor feelings that are triggered by being admitted via affirmative action. Finally, column 4 shows that individuals who are younger siblings – a group who may be especially prone to impostor feelings – also respond more negatively to being admitted via affirmative action.

2.5.2 Effects on Academic Performance

Table 2.11 presents our baseline results on academic performance. In Panel A, columns 1-3 indicate that being admitted via affirmative action lowers minority students' performance on a standardized English test. Again, the RD estimates are large, indicating that treatment worsens performance by approximately 0.5 sd. Columns 4-6 and 7-9 present evidence that both left-tail and right-tail academic outcomes are affected by affirmative action. In columns 4-6, we show that minority students admitted via affirmative action are approximately 23pp more likely to skip class (a left-tail outcome), almost a 0.5 sd increase. In columns 7-9, we show that minority students admitted via affirmative action are less likely to win an academic award – again this effect is almost 0.5 sd. Figure 2·7 presents the RD graph analogs of these results, showing clear discontinuity at the cutoff in each case.

Panel B shows the results from our placebo specifications. In all cases, we see that students admitted via academic or merit-based bonus points do not suffer any academic costs. This is further evidence that the reason behind the bonus points affects how students perceive them and therefore themselves and their abilities.

2.5.3 Effects on Career Aspiration

We then examine whether being admitted via affirmative action triggers impostor feelings that affect short-run self-image and academic performance but eventually do not matter. Table 2.12 presents evidence against this optimistic interpretation. We show that being admitted via affirmative action affects students' career plans.

In columns 1-3, we show that minority students admitted via affirmative action are less certain about their career plans. Columns 4-6 indicate that they are more likely to state an interest in a public sector career and columns 10-12 indicate that they are less likely to indicate an interest in entrepreneurship. The survey was conducted

during a period of rapid economic growth in China, when many technology start-ups grew rapidly and creating immense wealth and status for their founders. During the same period, an anti-corruption campaign reduced returns to public sector jobs and diminished the status of these jobs. We therefore interpret these results as suggestive evidence that students admitted via affirmative action are less confident about their future, prefer the stability of the public sector and do not believe they have the ability to succeed at a career (entrepreneurship) where all the returns come from being in the right tail. The results of Table 2.14 Panel B – which show that students admitted via ethnicity-based bonus points are less likely to be party members – indicates that the preference for government careers is not rooted in a fondness for government service but rather for the stability of the career track.

In Panel B, we present results from our placebo test, which again show that there are essentially no effects of being admitted via academic/merit-based bonus points on career choices.

Though we are unable to follow students for long enough to observe long-run outcomes, these results on career plans are suggestive evidence that affirmative action may affect choices made in university that have lasting consequences.

2.5.4 Other Outcomes

As further evidence that being admitted via affirmative action changes individuals' future plans, we show in Table 2.13 how it affects marriage and family plans. Columns 1-3 show that minority students admitted via affirmative action are less likely to indicate that they believe they will get married; columns 4-6 show that they are also less likely to indicate that they aspire to have children. Once again, panel B of Table demonstrates that the placebo comparison indicates no effect of being admitted via merit-based bonus points.

Table 2.14 Panel A, Columns 1-3 shows that students admitted via ethnicity-

based bonus points have weaker social relationships at university. Panel B, Columns 1-3 shows that they are less likely to be CPC members, in spite of their greater preference for public sector careers. This might be a consequence of their worse self-image, weaker social relationships and poorer academic performance and generally lower aspirations. Given the long-run value of being a CPC member in China in many careers, both public and private, this is an example of a university choice or outcome that may have longer-run impacts.

Columns 4-6 of both panels indicate again no effect on either outcome of being admitted via merit-based bonus points.

2.5.5 Discussion

Why does being admitted via affirmative action change the behavior of minority students? We argue that peer and teacher responses are unlikely to explain this, since minorities in China are generally hard to identify from their names (Uyghurs and Tibetans are well-known exceptions) and bonus points are private information, seldom discussed because discussion leads to fear of negative stereotyping and loss of peer respect (Jia and Persson, 2021).

Our findings might be also explained by the disparities in majors between students who qualified via affirmative action and those who qualified on merit. In particular, if students enter through affirmative action, they are less likely to be enrolled in their preferred majors as the places allocated for those majors have been taken by other students with higher scores. Students with a major that is not of their choosing may tend to perform worse academically and have strong feelings of self-doubt. If this is true, we should expect students who qualified on merit-based bonus points (placebo group) to show similar patterns to minority students. However, the effects on the placebo group are statistically insignificant, which precludes the explanation that our findings are driven by students being in the "discomfort zone".

Similarly, another concern is that students admitted via affirmative action might be more likely to be assigned to relatively "weaker" majors, where the quality of education and the academic caliber of faculty and peers are both lower. Such students may perform worse than those above the cutoff scores. To address this possibility, we conduct balance tests on majors for minority students who score just below and above cutoff scores. Following Jia and Li (2021), we pool students into three major categories: Econ-Finance-Law, STEM, and Humanities, and run our baseline specification with each major indicator as the dependent variable using both parametric and non-parametric estimation methods. As shown in Table 2.15, qualifying via affirmative action is not statistically correlated with specific majors, suggesting that there is no evidence of sorting across majors around the cutoff scores.

Instead, we argue that the negative effect of affirmative action on performance is driven by perceptions of low self-competence. Results from the placebo test suggests that qualifying on merit provides a coarse signal about one's own ability, which students factor into their human capital investment decisions. However, this requires some bounded rationality, limited attention, or rule-of-thumb behavior on minority students' part, since they know their position relative to the cutoff and marginal students know how close they were to getting in on merit.

2.6 Conclusion

In this paper, we studied how being admitted via affirmative action affects minority students. We examine a unique context, Chinese college admissions, that offers suitable empirical variation to test this hypothesis. Because all minority students receive bonus points and we know the entrance exam cutoff of each college, we can estimate whether a minority student was admitted to a particular college on merit or with the help of bonus points. We document that nearly 40 percent of minority students

who attend Chinese colleges in our sample were admitted with the aid of affirmative action.

We estimate a RD design, comparing minority students who were admitted on merit against other minority students who were admitted to the same college and track but via affirmative action. We show that being admitted via affirmative action worsens self-image and academic performance in the short-run. Moreover, being admitted via affirmative action also influences choices – such as career and family plans and the decision to join the CPC – that affect an individual's prospects in the long-run.

Our interpretation is that being admitted via affirmative action triggers impostor feelings amongst minority students. Consistent with this interpretation, students who are admitted via merit- or academic-based bonus points do not seem to suffer the same negative consequences. In addition, affirmative action is particularly likely to trigger these impostor feelings for low SES students and students from rural areas as well as those admitted to elite colleges.

Our paper sheds light on some of the psychological impacts of affirmative action, which have received relatively little attention in the economics literature to date. The mechanism we document – that affirmative action triggers impostor feelings amongst admitted minority students – might help explain some stylized facts in the literature, such as shedding light on the "mismatch hypothesis". Our results might also constitute arguably the first evidence of the impostor phenomenon in a natural or field setting.

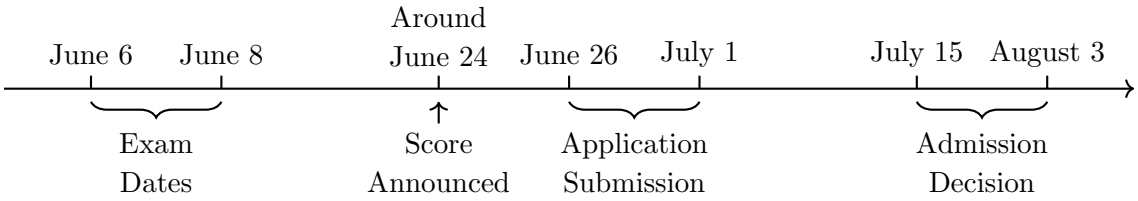
These findings notwithstanding, affirmative action may still be an attractive policy for several reasons. First and foremost, it gives minority students access to high-quality educational opportunities that will likely not otherwise be available (Miller, 2017). These may offer significant benefits to minority students even after accounting

for the negative impacts of impostor feelings on educational performance. Second, many American universities justify affirmative action not only on equity grounds, but also in terms of promoting socioeconomic and cultural diversity in its community. This argument is also likely unaffected by the presence of impostor feelings amongst minority students.

We hope our results will help policymakers understand how affirmative action could be made more effective at redressing inequalities and raising the life chances of talented minority students around the world.

2.7 Figures and Tables

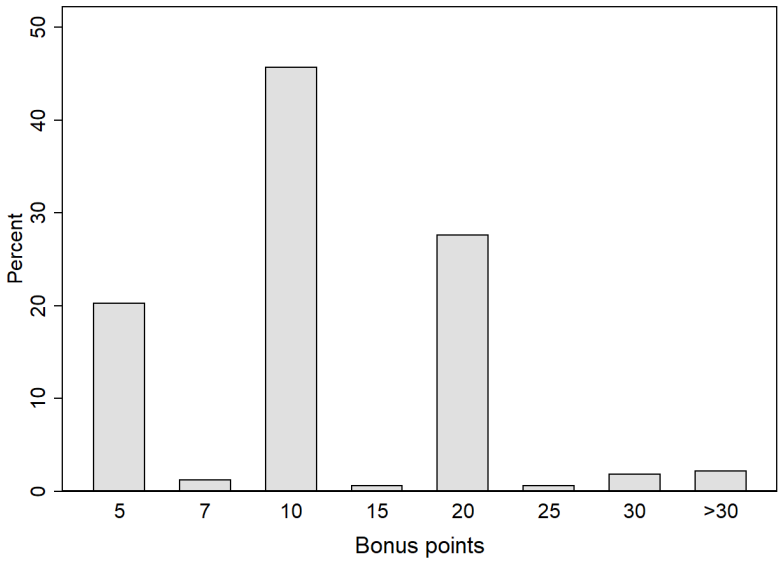
Figure 2·1: Timeline of Chinese College Admissions in 2006



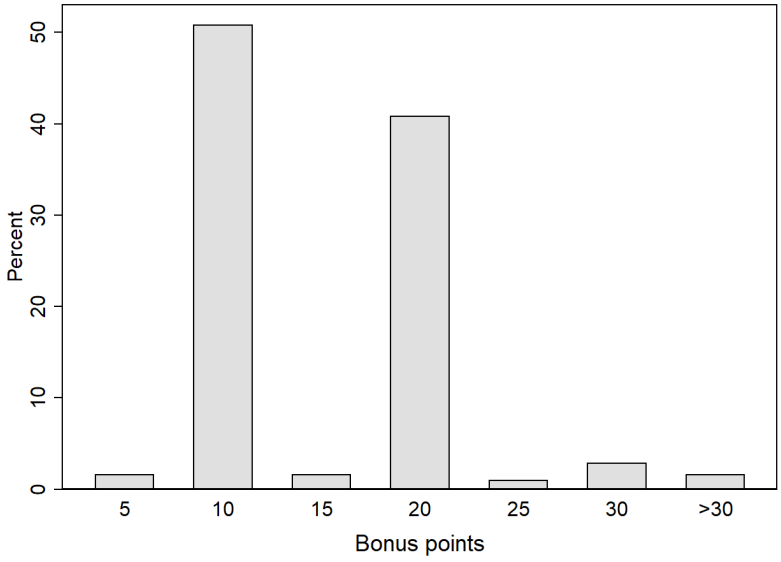
Notes: This figure shows the timing of different events in the Chinese college admissions in 2006, including the exam, college application and admission. The exam dates are the same for all provinces across China, while announcement, submission, and decision dates may vary a few days by province and by year.

Figure 2·2: Distribution of Bonus Points

(a) Ethnicity-based

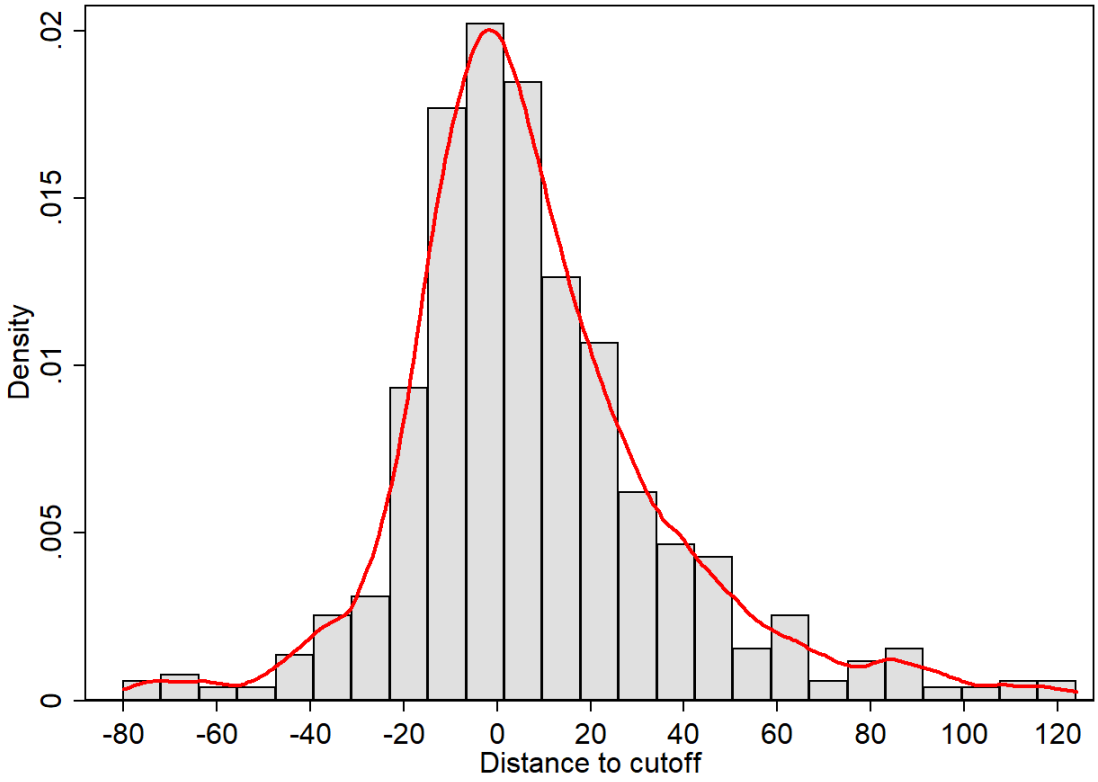


(b) Merit-based



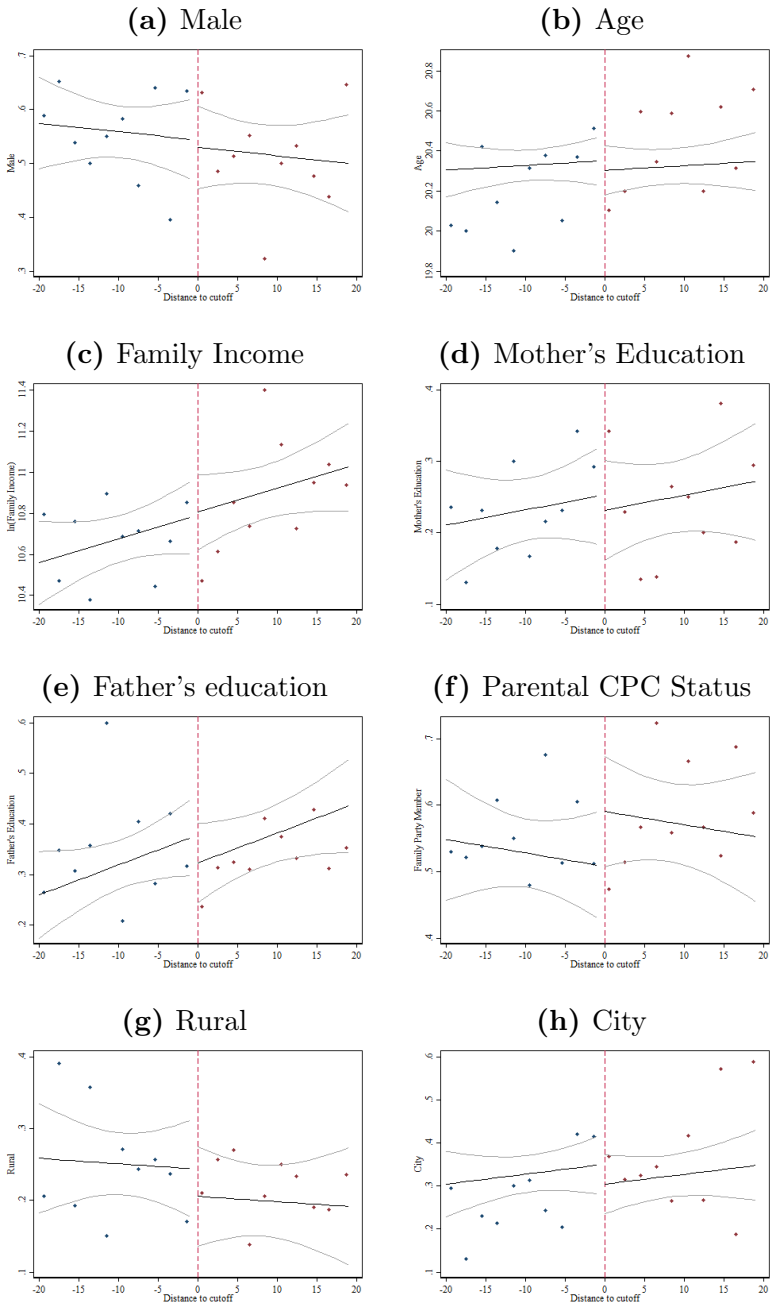
Notes:The figure depicts the distribution of ethnicity-based and merit-based bonus points Each bar represents the percentage of students who received each of the bonus points.

Figure 2·3: Student Distribution



Notes: This figure depicts the distribution of exams scores in terms of the distance in points to the cutoff scores. Each bar represents the probability density of students within the bin. The red curve shows the kernel density estimate of the distribution, suggesting a smooth density around the cutoff.

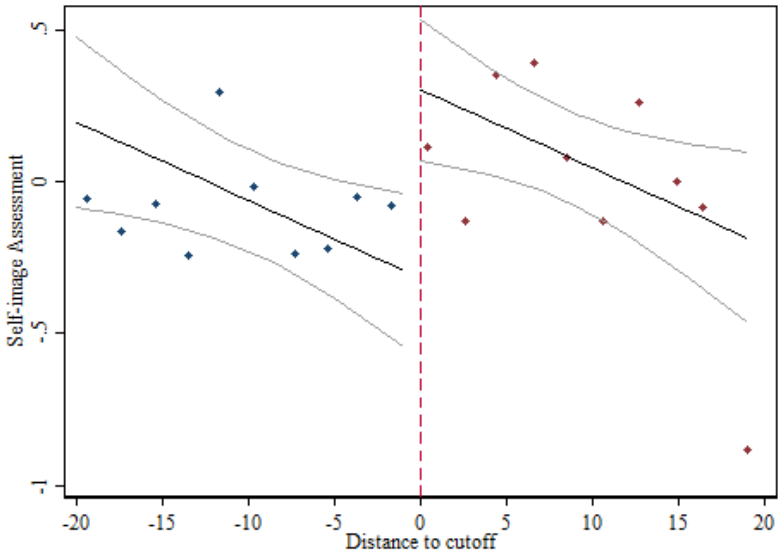
Figure 2·4: Balance Tests of Student Individual Characteristics



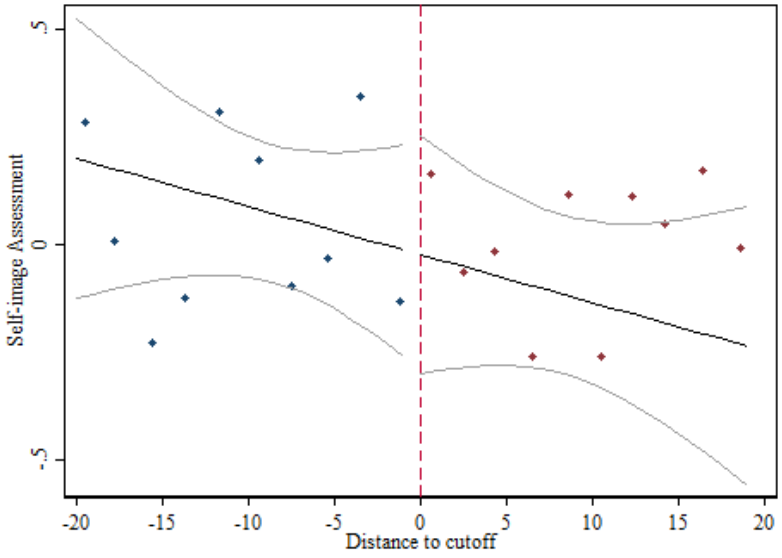
Notes: The figures plot the results of balance tests of student individual characteristics. Each dot represents the student's characteristics averaged over 2-point bins. Black lines fit parametric linear regressions within 20-point bandwidth on both sides of the cutoff. Grey curves denote 90 percent confidence interval. Cohort fixed effects and a full set of college-province-track fixed effects are controlled for in all these figures.

Figure 2·5: Self-Image for Ethnicity-based and Merit-based Students

(a) Ethnicity-based

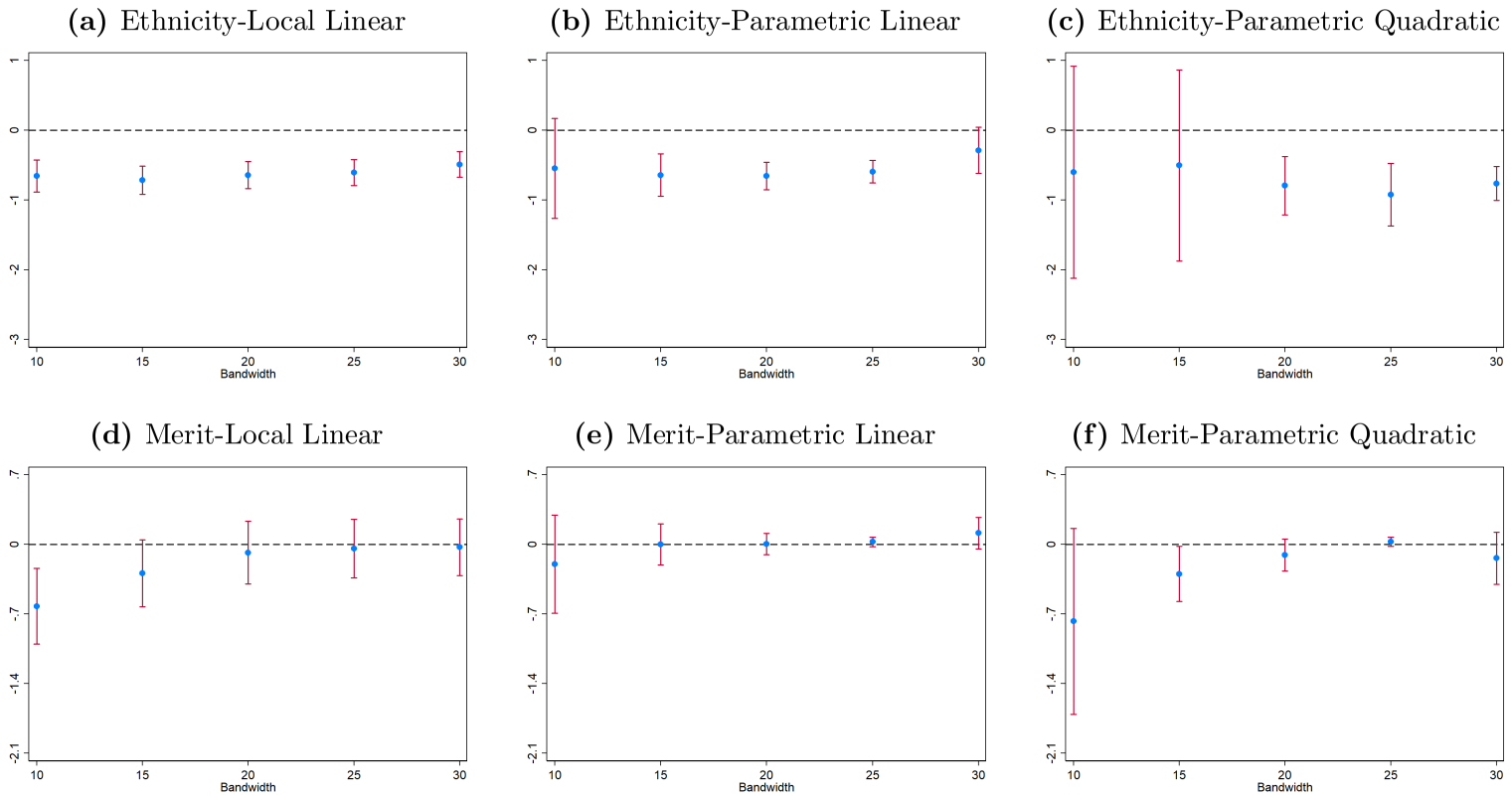


(b) Merit-based



Notes: The figures plot the student's self-image assessment against distance to the cutoff scores. Each dot represents the self-image index averaged over 2-point bins. Black lines fit parametric linear regressions within 20-point bandwidth on both sides of the cutoff. Grey curves denote 90 percent confidence interval. Cohort fixed effects and a full set of college-province-track fixed effects are controlled for in both figures.

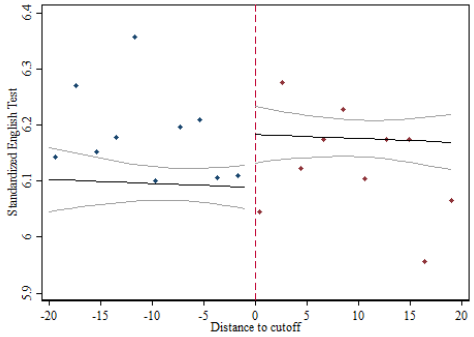
Figure 2·6: Robustness: Alternative Bandwidth



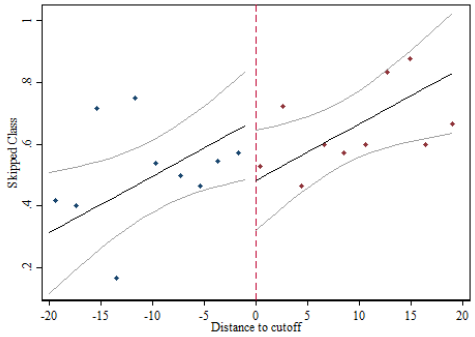
Notes: The figures plot RD estimates of effects of scoring below the cutoff on students' self-image assessment using alternative bandwidths along with the 90 percent confidence interval, ranging from 10-point to 30-point bandwidths. The dependent variable is *Self-Image Index* constructed by calculating a z-score for each component (self-image related questions) and taking the average z-score across components. The results in Panel (a) and Panel (d) are obtained from the local linear non-parametric estimation method, while other results are from the parametric method with either linear interaction or quadratic interactions. Panel (a), (b), (c) report results for ethnicity-based group, while last three are for merit-based group. All estimations include cohort fixed effects and a full set of college-province-track fixed effects. Standard errors for all parametric specifications are two-way clustered at the college and province levels.

Figure 2·7: Academic Performance for Ethnicity-based Students

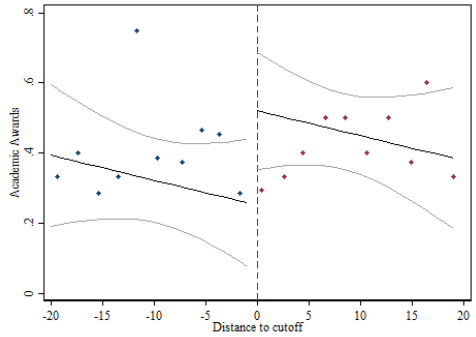
(a) Standardized English Test Score



(b) Skipped Class

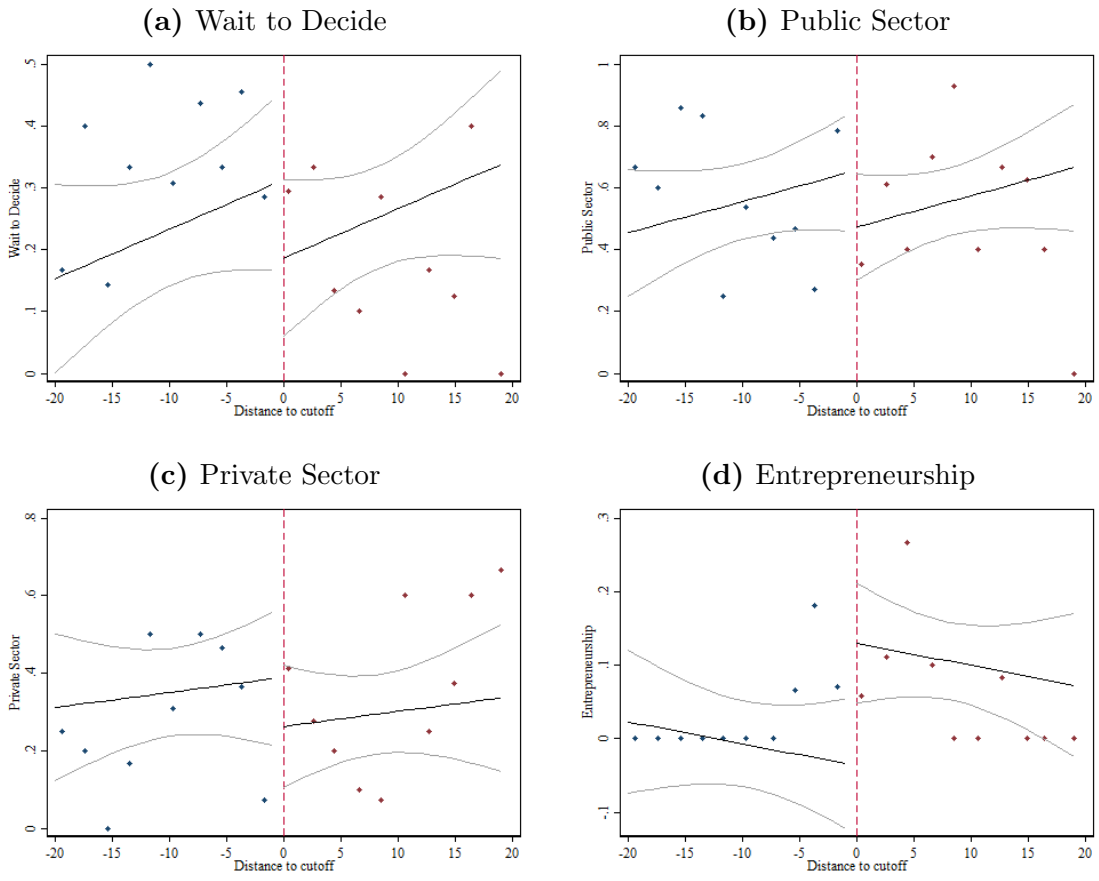


(c) Academic Awards



Notes: The figures plot the student's academic performance against distance to the cutoff scores for ethnicity-based group. Each dot represents the academic performance outcomes averaged over 2-point bins. Black lines fit parametric linear regressions within 20-point bandwidth on both sides of the cutoff. Grey curves denote 90 percent confidence interval. Cohort fixed effects and a full set of college-province-track fixed effects are controlled for in all these figures.

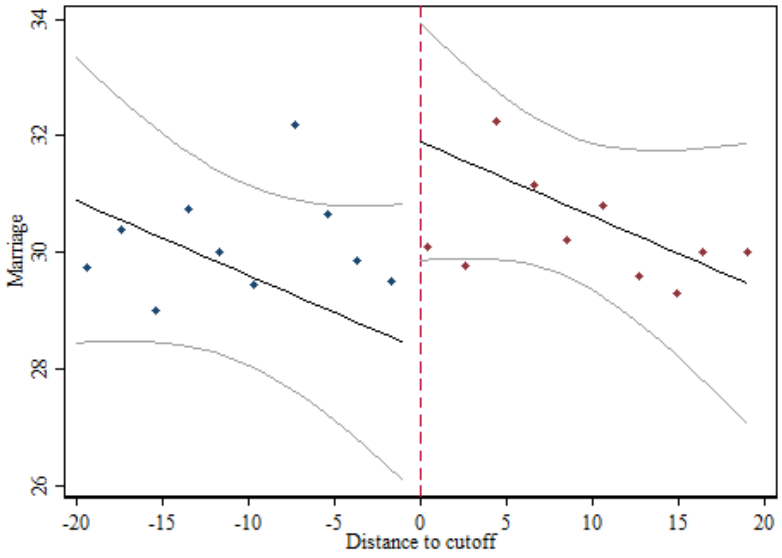
Figure 2·8: Career Aspiration for Ethnicity-based Students



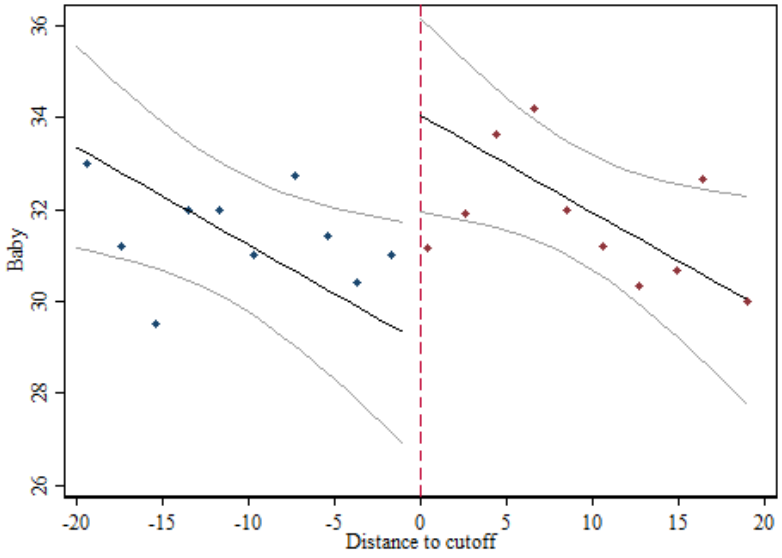
Notes: The figures plot the student's career plan against distance to the cutoff scores for ethnicity-based group. Each dot represents the aspirations for career outcomes averaged over 2-point bins. Black lines fit parametric linear regressions within 20-point bandwidth on both sides of the cutoff. Grey curves denote 90 percent confidence interval. Cohort fixed effects and a full set of college-province-track fixed effects are controlled for in all these figures.

Figure 2·9: Family Plan for Ethnicity-based Students

(a) Marriage



(b) Baby



Notes: The figures plot the student's family plan against distance to the cutoff scores for ethnicity-based group. Each dot represents the aspirations for family outcomes averaged over 2-point bins. Black lines fit parametric linear regressions within 20-point bandwidth on both sides of the cutoff. Grey curves denote 90 percent confidence interval. Cohort fixed effects and a full set of college-province-track fixed effects are controlled for in both figures.

Table 2.1: Number and Share of Students Admitted through Affirmative Action

	Number of Students Admitted through Affirmative Action	Total Number of Students in Sample	Percent
Top-3 Elite Colleges	36	74	48.6
Other "Project-211" Colleges	76	174	43.7
Non-"Project-211" Colleges	24	78	30.7
Total	136	326	41.7

Notes: The table shows the number and share of students who were admitted through affirmative action in our sample. "Project-211" is a designation for 100 colleges selected by the Chinese government, that are generally considered to be top-quality institutions. Top-3 elite colleges refer to the top 3 best colleges in our sample.

Table 2.2: Summary Statistics

	Ethnicity-based			Merit-based		
	Mean	S.D.	Obs.	Mean	S.D.	Obs.
(a) Outcomes:						
Self-Image Index	0.018	0.765	334	-0.007	0.803	322
Standardized English Test	6.179	0.148	157	6.257	0.206	164
Skipped Class	0.557	0.497	334	0.526	0.500	323
Academic Awards	0.401	0.491	334	0.498	0.501	323
Major: Econ-Finance-Law	0.302	0.460	331	0.262	0.440	317
Major: STEM	0.450	0.498	331	0.574	0.495	317
Major: Humanities	0.248	0.432	331	0.164	0.371	317
Wait to Decide	0.240	0.427	334	0.251	0.434	323
Public Sector	0.509	0.501	334	0.536	0.500	323
Private Sector	0.338	0.474	334	0.303	0.460	323
Entrepreneurship	0.063	0.243	334	0.034	0.182	323
Marriage	30.927	4.000	232	30.976	3.484	211
Baby	32.141	3.699	184	32.000	3.118	175
(b) Individual Characteristics:						
Male	0.485	0.501	334	0.542	0.499	323
Age	20.605	1.410	334	20.356	1.218	323
ln(Family Income)	10.492	1.128	332	10.749	1.148	316
Father's Education	0.204	0.403	334	0.353	0.479	323
Mother's Education	0.162	0.369	334	0.260	0.439	323
Parental CPC Status	0.488	0.501	334	0.560	0.497	323
Rural	0.290	0.455	334	0.229	0.421	323
Low Class	0.416	0.494	334	0.347	0.477	323
Elite College	0.763	0.426	334	0.799	0.402	323
Younger	0.305	0.461	334	0.155	0.362	323

Notes: The ethnicity-based and merit-based groups contain information for students who gained ethnicity-based bonus points and merit-based bonus points, respectively. *Self-Image Index* is constructed by calculating a z-score for each component (self-image assessment questions) and taking the average z-score across components. *Standardized English Test* is the logarithm of the score of the College English Test (CET-4). *Skipped Class* and *Academic Awards* are dummy variables which equal one if the student has been truant from class at least once or has received academic awards. *Wait to Decide*, *Public*, *Private*, and *Entrepreneurship* are dichotomous variables about students' career plans. A value of 1 for *Wait to Decide* means that the student has not yet decided what she wants to do upon graduation. Public, private and entrepreneurship are dummies which equal 1 if the student plans to work in public sectors, private sector, and starts own business, respectively. *Marriage* and *Baby* variable measures student's life planning, which represent the latest age of the student's plan to get married and to have a child, respectively. Variables in panel (b) are students' individual characteristics. *Father's Education* and *Mother's Education* are dichotomous variables which equal one if the student's father or mother have a college degree or above. *Parental CPC Status* represents the Communist Party of China (CPC) membership of the student's parents. *Rural* and *Low Class* represent the family background, taking value of 1 if the student is from rural area, and the student self-reports that their family socioeconomic status (SES) is low, respectively. *Elite College* equals one if the college is in "Project-211" (Top 100 universities). *Younger* indicates whether the student has older siblings or not.

Table 2.3: Balance Test

	Male	Age	ln(Family Income)	Father's Education	Mother's Education	Family Party Member	Rural	City
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Below Cutoff	0.058	-0.632	-0.039	-0.158	-0.212	-0.212	-0.129	0.012
	(0.110)	(0.537)	(0.369)	(0.109)	(0.209)	(0.167)	(0.137)	(0.175)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	215	215	215	215	215	215	215	215
R-squared	0.701	0.872	0.641	0.697	0.619	0.594	0.628	0.693

Notes: This table reports balance tests using the parametric method with quadratic interactions. The regressions in all columns use the ethnicity-based sample within the 20-point bandwidth. All regressions include cohort fixed effects and a full set of college-province-track fixed effects. Standard errors are two-way clustered at the college and province levels.

Table 2.4: Effects of Scoring Below the Cutoff on Self-Image

Polynomial Approach	Ethnicity-based			Merit-based		
	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic
	(1)	(2)	(3)	(4)	(5)	(6)
Below Cutoff	-0.642***	-0.654***	-0.792***	-0.082	0.004	-0.107
	(0.098)	(0.101)	(0.213)	(0.161)	(0.055)	(0.082)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	215	215	215	210	210	210
R-squared		0.684	0.696		0.393	0.395

Notes: This table shows the RD results of effects of scoring below the cutoff on students' self-image assessment. The dependent variable is *Self-Image Index* constructed by calculating a z-score for each component (ability perception related questions) and taking the average z-score across components. The results in Column (1) and Column (4) are obtained from the local linear non-parametric estimation method, while other results are from the parametric method with either linear interaction or quadratic interactions. Column (1)-(3) and Column (4)-(6) report the results for ethnicity-based and merit-based groups within the 20-point bandwidth, respectively. All regressions include cohort fixed effects and a full set of college-province-track fixed effects. Standard errors for all parametric specifications are two-way clustered at the college and province levels.
*** denotes significance at the 1% level.

Table 2.5: Robustness: Placebo Cutoffs

Cutoffs	-20			0 (baseline)			+20		
Polynomial Approach	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Below Cutoff	-0.262*	-0.647	0.163	-0.642***	-0.654***	-0.792***	-0.026	-0.115	-0.286
	(0.149)	(1.265)	(0.871)	(0.098)	(0.101)	(0.213)	(0.145)	(0.260)	(0.563)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	131	131	131	215	215	215	156	156	156
R-squared		0.762	0.764		0.684	0.696		0.697	0.707

Notes: This table shows the RD results of effects of scoring below the cutoff on students' self-image assessment using different placebo cutoffs. The dependent variable is *Self-Image Index* constructed by calculating a z-score for each component (self-image related questions) and taking the average z-score across components. The results in Columns (1), (4), and (7) are obtained from the local linear non-parametric estimation method, while other results are from the parametric method with either linear interaction or quadratic interactions. All columns report results for ethnicity-based group within 20-point bandwidth and include cohort fixed effects and a full set of college-province-track fixed effects. Standard errors for all parametric specifications are two-way clustered at the college and province levels.
* and *** denote significance at the 10% and 1% level, respectively.

Table 2.6: Robustness: Alternative Kernel Functions

	Ethnicity-based		Merit-based	
	(1)	(2)	(3)	(4)
Below Cutoff	-0.644***	-0.654***	-0.017	0.004
	(0.102)	(0.099)	(0.162)	(0.157)
Cohort FE	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes
# Observations	215	215	210	210
Kernel Function	Epanechnikov	Rectangular	Epanechnikov	Rectangular

Notes: This table shows the non-parametric RD estimates of effects of scoring below the cutoff on students' self-image assessment with alternative kernel functions. The dependent variable is *Self-Image Index* constructed by calculating a z-score for each component (self-image related questions) and taking the average z-score across components. Column (1)-(2) and Column (3)-(4) report the results for ethnicity-based and merit-based groups within the 20-point bandwidth, respectively. All regressions include cohort fixed effects and a full set of college-province-track fixed effects.
*** denotes significance at the 1% level.

Table 2.7: Robustness: Alternative Measure of Self-Image Index

Polynomial Approach	Ethnicity-based			Merit-based		
	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic
	(1)	(2)	(3)	(4)	(5)	(6)
Below Cutoff	- 0.230***	-0.221***	-0.282***	0.036	0.048	0.009
	(0.037)	(0.061)	(0.086)	(0.058)	(0.042)	(0.021)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	214	214	214	210	210	210
R-squared		0.674	0.690		0.416	0.420

Notes: This table shows the RD results of effects of scoring below the cutoff on an alternative self-image measure. The dependent variable is a student's percentile rank in terms of self-image assessment which is constructed based on the principal component of the underlying components (self-image assessment related questions). The results in Column (1) and Column (4) are obtained from the local linear non-parametric estimation method, while other results are from the parametric method with either linear interaction or quadratic interactions. Column (1)-(3) and Column (4)-(6) report the results for ethnicity-based and merit-based groups within the 20-point bandwidth, respectively. All regressions include cohort fixed effects and a full set of college-province-track fixed effects. Standard errors for all parametric specifications are two-way clustered at the college and province levels.
*** denotes significance at the 1% level.

Table 2.8: Robustness: Additional Controls

Polynomial Approach	Ethnicity-based			Merit-based		
	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic
	(1)	(2)	(3)	(4)	(5)	(6)
Below Cutoff	-0.550***	-0.540***	-0.734**	0.136	-0.076	-0.227
	(0.095)	(0.131)	(0.283)	(0.147)	(0.154)	(0.154)
Individual-level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	215	215	215	210	210	210
R-squared		0.707	0.718		0.453	0.455

Notes: This table shows the RD results of effects of scoring below the cutoff on students' self-image assessment controlling for students' idiosyncratic characteristics. The dependent variable is *Self-Image Index* constructed by calculating a z-score for each component (self-image related questions) and taking the average z-score across components. The individual-level controls include gender, age, family income, father's and mother's education level, and parental CPC status. The results in Column (1) and Column (4) are obtained from the local linear non-parametric estimation method, while other results are from the parametric method with either linear interaction or quadratic interactions. Column (1)-(3) and Column (4)-(6) report the results for ethnicity-based and merit-based groups within the 20-point bandwidth, respectively. All regressions include cohort fixed effects and a full set of college-province-track fixed effects. Standard errors for all parametric specifications are two-way clustered at the college and province levels.
** and *** denote the significance at the 5% and 1% level, respectively.

Table 2.9: Robustness: Weighted Regressions

Polynomial Approach	Ethnicity-based			Merit-based		
	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic
	(1)	(2)	(3)	(4)	(5)	(6)
Below Cutoff	-0.653***	-0.653***	-0.796**	0.045	-0.071	0.358*
	(0.110)	(0.098)	(0.329)	(0.138)	(0.066)	(0.174)
Weighted	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	215	215	215	210	210	210
R-squared		0.691	0.701		0.421	0.436

Notes: This table shows the RD results of effects of scoring below the cutoff on students' self-image assessment weighted by sampling probability to reflect the population. The dependent variable is *Self-Image Index* constructed by calculating a z-score for each component (self-image related questions) and taking the average z-score across components. The results in Column (1) and Column (4) are obtained from the local linear non-parametric estimation method, while other results are from the parametric method with either linear interaction or quadratic interactions. Column (1)-(3) and Column (4)-(6) report the results for ethnicity-based and merit-based groups within the 20-point bandwidth, respectively. All regressions include cohort fixed effects and a full set of college-province-track fixed effects. Standard errors for all parametric specifications are two-way clustered at the college and province levels.
** and *** denote significance at the 5% and 1% level, respectively.

Table 2.10: Heterogeneity Analysis

Dependent Variable	Self-Image Index			
	(1)	(2)	(3)	(4)
Below Cutoff × *Noncity*	-0.498***			
	(0.110)			
Below Cutoff × *Low Class*		-0.286**		
		(0.115)		
Below Cutoff × *Elite College*			-0.446**	
			(0.192)	
Below Cutoff × *Younger*				-0.465**
				(0.168)
Individual-level Controls	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes
No. of Observations	324	324	324	324
R-squared	0.594	0.585	0.587	0.594

Notes: This table shows the difference-in-differences (DID) results of heterogeneous effects of scoring below the cutoff on students' self-image assessment. The dependent variable is *Self-Image Index* constructed by calculating a z-score for each component (self-image related questions) and taking the average z-score across components. *Noncity* and *Low Class* represent the family background, taking value of 1 if the student is from non-city area (county or other rural area), and the student self-reports that their family socioeconomic status is low, respectively. *Elite College* equals one if the college is in Project-211 (Top 100 universities). *Younger* indicates whether the student has older siblings or not. The individual-level controls include gender, age, family income, father's and mother's education level, and parental CPC status. All columns report results using full sample of ethnicity-based group and include cohort fixed effects and a full set of college-province-track fixed effects. Standard errors for all parametric specifications are two-way clustered at the college and province levels.
** and *** denote significance at the 5% and 1% level, respectively.

Table 2.11: Effects of Scoring Below the Cutoff on Academic Performance

Dependent Variables	Standardized English Test			Skipped Class			Academic Awards		
Polynomial Approach	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Ethnicity-based									
Below Cutoff	-0.125***	-0.096***	-0.219***	0.234***	0.150***	0.342***	-0.390***	-0.342***	-0.488***
	(0.011)	(0.005)	(0.056)	(0.069)	(0.037)	(0.079)	(0.071)	(0.058)	(0.153)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of Dep.Var	6.193	6.193	6.193	0.579	0.579	0.579	0.405	0.405	0.405
# Observations	109	109	109	215	215	215	215	215	215
R-squared		0.934	0.943		0.627	0.632		0.600	0.609
Panel B: Merit-based									
Below Cutoff	0.015	0.029	0.018	0.105	-0.033	-0.091	0.100	0.133	0.081
	(0.016)	(0.036)	(0.049)	(0.082)	(0.115)	(0.129)	(0.091)	(0.134)	(0.168)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of Dep.Var	6.252	6.252	6.252	0.575	0.575	0.575	0.536	0.536	0.536
# Observations	118	118	118	211	211	211	211	211	211
R-squared		0.936	0.937		0.462	0.478		0.500	0.501

Notes: This table shows the RD results of effects of scoring below the cutoff on students' academic performance. *Standardized English Test* is the logarithm of the score of the College English Test (CET-4). *Skipped Class* and *Academic Awards* are dummy variables which equal one if the student has been truant from class at least once or has received academic awards. The results in Columns (1), (4), and (7) are obtained from the local linear non-parametric estimation method, while other results are from the parametric method with either linear interaction or quadratic interactions. Panel A and Panel B report results for ethnicity-based and merit-based groups within 20-point bandwidth, respectively. Control mean refers to the mean of dependent variable for the sample above the cutoff. All columns include cohort fixed effects and a full set of college-province-track fixed effects. Standard errors for all parametric specifications are two-way clustered at the college and province levels.
*** denotes significance at the 1% level.

Table 2.12: Effects of Scoring Below the Cutoff on Career Aspiration

Dependent Variables	Wait to Decide			Public Sector			Private Sector			Entrepreneurship		
Polynomial Approach	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Ethnicity-based												
Below Cutoff	0.100*	0.129**	0.125***	0.321***	0.125	0.511***	-0.013	0.182	-0.138	-0.122***	-0.150***	-0.127*
	(0.055)	(0.058)	(0.022)	(0.079)	(0.135)	(0.117)	(0.079)	(0.140)	(0.129)	(0.037)	(0.030)	(0.070)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	215	215	215	215	215	215	215	215	215	215	215	215
R-squared		0.720	0.725		0.576	0.608		0.588	0.603		0.583	0.586
Panel B: Merit-based												
Below Cutoff	0.162	0.173	0.166	-0.197*	-0.043	-0.347	0.073	0.011	0.228	0.071**	0.049	0.043
	(0.102)	(0.140)	(0.193)	(0.104)	(0.179)	(0.243)	(0.092)	(0.216)	(0.242)	(0.035)	(0.072)	(0.095)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	211	211	211	211	211	211	211	211	211	211	211	211
R-squared		0.531	0.533		0.488	0.519		0.475	0.522		0.484	0.500

Notes: This table shows the RD results of effects of scoring below the cutoff on students' aspirations for career. *Wait to Decide*, *Public Sector*, *Private Sector*, and *Entrepreneurship* are dichotomous variables about students' career plans. A value of 1 for *Wait to Decide* means that the student has not yet decided what she wants to do upon graduation. Public, private and entrepreneurship are dummies which equal 1 if the student plans to work in public sectors, private sector, and starts own business, respectively. The results in Columns (1), (4), (7), and (10) are obtained from the local linear non-parametric estimation method, while other results are from the parametric method with either linear interaction or quadratic interactions. Panel A and Panel B report results for ethnicity-based and merit-based groups within 20-point bandwidth, respectively. All columns include cohort fixed effects and a full set of college-province-track fixed effects. Standard errors for all parametric specifications are two-way clustered at the college and province levels.
*, **, and *** denote the significance at the 10%, 5%, and 1% level, respectively.

Table 2.13: Effects of Scoring Below the Cutoff on Family Plans

Dependent Variables	Marriage			Baby		
Polynomial Approach	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Ethnicity-based						
Below Cutoff	-4.220***	-3.399***	-6.469***	-5.025***	-5.002**	-6.057*
	(0.848)	(0.556)	(1.010)	(0.667)	(1.785)	(2.877)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of Dep.Var	30.881	30.881	30.881	32.114	32.114	32.114
# Observations	151	151	151	120	120	120
R-squared		0.414	0.432		0.585	0.586
Panel B: Merit-based						
Below Cutoff	0.770	1.202	-0.963	-1.061	-0.188	-1.762
	(0.624)	(0.705)	(1.479)	(0.739)	(1.266)	(1.820)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of Dep.Var	30.858	30.858	30.858	32.147	32.147	32.147
# Observations	136	136	136	116	116	116
R-squared		0.574	0.605		0.580	0.588

Notes: This table shows the RD results of effects of scoring below the cutoff on students' family plan. *Marriage* and *Baby* variable measures student's life planning, which represent the latest age of the student's plan to get married and to have a child, respectively. The results in Columns (1) and Column (4) are obtained from the local linear non-parametric estimation method, while other results are from the parametric method with either linear interaction or quadratic interactions. Panel A and Panel B report results for ethnicity-based and merit-based groups within 20-point bandwidth, respectively. Control mean refers to the mean of dependent variable for the sample above the cutoff. All columns include cohort fixed effects and a full set of college-province-track fixed effects. Standard errors for all parametric specifications are two-way clustered at the college and province levels.
*, **, and *** denote the significance at the 10%, 5%, and 1% level, respectively.

Table 2.14: Effects of Scoring Below the Cutoff on Other Outcomes

Polynomial Approach	Ethnicity-based			Merit-based		
	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dependent Variable: Social Relationship Index						
Below Cutoff	-0.937***	-0.806***	-0.964**	-0.156	-0.073	-0.243
	(0.098)	(0.173)	(0.381)	(0.168)	(1.785)	(2.877)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	215	215	215	211	211	211
R-squared		0.618	0.620		0.437	0.440
Panel B: Dependent Variable: Party Member						
Below Cutoff	-0.336***	-0.221***	-0.550***	0.106	0.160	0.141
	(0.059)	(0.028)	(0.074)	(0.115)	(0.127)	(0.217)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	215	215	215	211	211	211
R-squared		0.624	0.641		0.468	0.468

Notes: This table shows the RD results of effects of scoring below the cutoff on students' social relationship and CPC party membership. The dependent variable in Panel A is the social relationship index constructed by calculating a z-score for each component (closeness to students, professors, and department staffs) and taking the average z-score across components. In Panel B, the dependent variable is party membership dummy variable which equals one if the student is a CPC party member or has applied for the membership. The results in Columns (1) and Column (4) are obtained from the local linear non-parametric estimation method, while other results are from the parametric method with either linear interaction or quadratic interactions. All columns include cohort fixed effects and a full set of college-province-track fixed effects. Standard errors for all parametric specifications are two-way clustered at the college and province levels.

** and *** denote the significance at the 5% and 1% level.

Table 2.15: Effects of Scoring Below the Cutoff on Majors

Dependent Variables	Major: Econ-Finance-Law			Major: STEM			Major: Humanities		
Polynomial Approach	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic	Local Linear	Parametric Linear	Parametric Quadratic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Ethnicity-based									
Below Cutoff	-0.054	-0.144	0.090	-0.046	0.013	-0.182	0.100*	0.131	0.092
	(0.071)	(0.122)	(0.109)	(0.061)	(0.034)	(0.171)	(0.055)	(0.119)	(0.103)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	212	212	212	212	212	212	212	212	212
R-squared		0.621	0.632		0.740	0.744		0.739	0.742
Panel B: Merit-based									
Below Cutoff	0.039	-0.045	0.106	-0.096	-0.053	-0.203	0.057	0.098	0.097
	(0.068)	(0.040)	(0.112)	(0.066)	(0.038)	(0.144)	(0.050)	(0.071)	(0.108)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
College × Province × Track FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	206	206	206	206	206	206	206	206	206
R-squared		0.616	0.625		0.785	0.790		0.758	0.762

Notes: This table shows the RD results of effects of scoring below the cutoff on students' majors. The results in Columns (1), (4), and (7) are obtained from the local linear non-parametric estimation method, while other results are from the parametric method with either linear interaction or quadratic interactions. Panel A and Panel B report results for ethnicity-based and merit-based groups within 20-point bandwidth, respectively. All columns include cohort fixed effects and a full set of college-province-track fixed effects. Standard errors for all parametric specifications are two-way clustered at the college and province levels.

Chapter 3

Vertical Fiscal Imbalance and Local Fiscal Indiscipline: Empirical Evidence from China[1]

3.1 Introduction

The past decades have witnessed wide implementation of fiscal decentralization reforms in a vast number of developed and developing countries. The primary theoretical justification for this movement is that fiscal decentralization is an effective tool for increasing the economic efficiency of public expenditures in a variety of ways, including increased competition among subnational governments in delivering public services (see, for example, Tiebout, 1956; Oates, 1972; Brennan et al., 1980; Qian and Roland, 1998). Nevertheless, fiscal decentralization may also distort the incentives of subnational governments if it is not well designed. A general concern in this regard is the incomplete and asymmetric nature of many decentralization reforms. Usually there is a mismatch between the generous devolution of expenditure responsibilities to local governments and the scant assignment of devolved revenue sources to them. This process, recognized as "partial fiscal decentralization" in the language of some scholars such as Brueckner (2009), Borge et al. (2014), Baskaran et al. (2016), and Salinas and Solé-Ollé (2018), implies that while many important spending responsibilities are decentralized to local governments, the major sources of revenues are

[1]This chapter is previously published by *European Journal of Political Economy* and reproduced here with the permission of the copyright holder (Jia et al., 2021).

retained and controlled by the central government, giving rise to what is known in the literature as the "vertical fiscal imbalance" (hereafter: VFI). This phenomenon becomes a source of distortionary incentives, threatening to undo the efficiency gains conventionally associated with fiscal decentralization (Rodden et al., 2003; Bouton et al., 2008; Eyraud and Lusinyan, 2013).

While some level of VFI can be vital for the central government to achieve certain political and economic objectives (Dahlby, 1996; Boadway and Keen, 1996), the main criticism leveled against it in the (partial) fiscal decentralization literature is that high VFI levels and thus its corollary, the heavy reliance on fiscal transfers, may lead to local fiscal indiscipline in the form of overspending or lowering local tax effort (see, e.g., Stein, 1999; Velasco, 2000; Borge and Rattsø, 2002; Rodden et al., 2003; Eyraud and Lusinyan, 2013; Ben-Bassat et al., 2016; Köppl-Turyna and Pitlik, 2018). Two arguments pinpoint this concern. First, in the presence of VFI, while practically all benefits of public expenditures are obtained at the local level, the expenditures are at least partly financed from a common pool of national resources, thus the perceived local costs of public services are lower than the actual costs. This, in turn, generates an overspending pressure demanding more central government funds and/or a disincentive for taxing local sources since the price of this financing source is relatively higher than that of grants (i.e., the common pool problem). Second, high dependence on intergovernmental transfers or borrowing, as a consequence of VFI, "softens" the budget constraint of subnational governments. This may induce bailout expectations by the central government, and, beyond over-borrowing, may also strengthen strategic behaviors for both overspending and lowering tax effort.

The existing empirical literature examining the relationship between VFI and local fiscal indiscipline has focused largely on the issue of overspending. In particular, those studies test the hypothesis that a high VFI level undermines fiscal discipline by

motivating local governments to expand their expenditures (see, for example, Ehdaie et al., 1994; Stein, 1999; Jin and Zou, 2002; Rodden, 2003; Jia et al., 2014).[2] A few studies also analyze the link between VFI and the overall fiscal balance at the national level. For instance, using cross-country panel datasets, Rodden (2002), Neyapti (2010), Eyraud and Lusinyan (2013), Aldasoro and Seiferling (2014), Asatryan et al. (2015), and Lago-Peñas et al. (2020) find supporting evidence that higher VFI levels are detrimental to governments' fiscal performance and increase fiscal deficits (or government debt). In addition, De Mello Jr (2000) argues that the presence of VFI is likely to result in a deficit bias in decentralized policymaking, especially in the developing countries, where the central government exercises less stringent control on subnational finances.

Despite these relevant works, little effort has been explicitly devoted to identifying the impact of VFI on the other important side of local fiscal indiscipline—local government taxing behavior.[3] Moreover, the literature to date has not provided any clear evidence on the sort of working mechanisms for the impact of VFI on local taxing effort.

Our paper fills this gap by studying the impact of VFI on local government taxing behaviors in the Chinese context and makes three main contributions to the current

[2]A recent exception is Dietrichson and Ellegård (2015) who find that conditional bailouts may enhance fiscal discipline. However, they also point out that the condition being attached to the grants might be the key to dampen the soft-budget constraint because it makes politicians realize that even if central government bailouts are available, they come at a cost.

[3]Many recent studies attribute China's robust economic growth to the strong fiscal and political incentives local governments face to promote local economic development, fiercely competing to attract capital (Xu, 2011). Although they cannot control tax rates, local governments still have lowered their tax collection effort and/or offered a large amount of illegal tax incentives (Liu and Martinez-Vazquez, 2014). Also motivated by central bailout expectations, most local governments have operated with a soft budget constraint (e.g., Qian and Roland, 1998), and recently have become highly indebted via illegal borrowing (with the central government' tacit approval even though the Budget Law had prevented local governments from such borrowing till 2014). Recent estimates placed (illegal) local debt surpassing 30% of GDP by the end of 2014 (Liu, 2010; Gao et al., 2021). Central government measures in late 2014 aimed at cleaning illegal tax incentives and auditing the overall size of illegal local debt, but still some of these actions included the bailout of some part of the (illegal) local debts.

literature. First, we are among the first to conduct an empirical examination on this important issue. China offers a good case to test this theoretical argument, as the Chinese provincial governments have been granted substantial discretion in determining their own fiscal decentralization policy towards sub-provincial governments within their jurisdictions. This gives rise to a high level of variation in the level of VFI across sub-provincial treatments. Meanwhile, China has maintained a highly centralized tax legislation system with the central government setting uniform statutory tax rates for all local governments, Therefore, the large variation among effective tax rates across localities is likely due to variations in local governments' tax collection efforts. For these reasons, a panel study of city-level VFI in China presents the advantage of avoiding the potential unobserved heterogeneity that may exist in the relevant cross-country studies on the impact of VFI on local fiscal indiscipline. Additionally, given the unique feature of China's fiscal decentralization architecture, which assigns tax administration of local revenues to two separate tax bureaus (i.e. local tax bureau for local taxes and state tax bureau for shared taxes), we hypothesize that the only feasible way for local governments to make a strategic response to the presence of high VFI is to change their tax collection behaviors regarding local taxes. These latter are under the direct administration of local governments, as opposed to the local portion of the shared taxes, which is under the direct supervision of the central government. Thus, the analysis on the collections for shared taxes provides a placebo test to further validate our main research hypothesis.

Second, we provide explicit evidence on the working mechanism for the impact of VFI. In theory, this mechanism roots in the fact that the fiscal actions taken by local governments can directly affect the resources they receive from the central government. Particularly, under the VFI structure a decrease in local tax collection effort, and hence a lower level of actual tax revenues collected, will result in an increase in

fiscal transfers from the central government. We shed light on this mechanism by analyzing the taxing behaviors of local governments on the so-called "extra-budgetary" (off-budgetary) revenue, which is quite a sizable revenue for local governments but that itself exists completely outside the formal fiscal budgetary system in China. This means that this part of revenue is not considered formally as part of the local fiscal capacity, and therefore it is not considered as part of the local tax bases for the determination of central fiscal transfers to local governments. Thus, our conjecture is that in the case of extra-budgetary revenues, local governments have no incentive to respond to the presence of the VFI because less extra-budgetary revenues will not affect the size of the transfers received from upper level governments. This, in turn, provides an opportunity to test the hypothesized mechanism.

Third, and last, we address the endogeneity issue of the VFI in the estimations by employing an instrumental variable approach. From a technical perspective, a crucial yet unsolved issue with the existing empirical literature on the impact of VFI is that most contributions fail to address the endogeneity of VFI or do so in an unsatisfactory manner. In this paper, we exploit the change in the predicted school-age population as a potential instrument for VFI. The rationale is that the change in the predicted school-age population represents an exogenous shock, varying across cities and over time, to local public (education) expenditures, and thus VFI of the cities. However, this shock should not be directly affected by the taxing behaviors of local governments or vice versa.

Using a nationwide city-level panel dataset for the years 1999-2009 and applying both fixed effects and instrumental estimation strategies, we find strong support for our conjectures. Specifically, we find the following: (i) Higher VFI levels reduce the ratio of total own revenues to gross domestic product (GDP),[4] supporting the main

[4]Total own revenues for local governments are composed of total tax revenues and non-tax revenues. Total tax revenues are revenues from local taxes and shared taxes elaborated in subsection

argument that higher VFI levels may induce fiscal indiscipline caused by reductions in the tax effort exercised by local governments. (ii) While higher VFI levels significantly reduce the ratio of local taxes (and its main components, including urban infrastructure taxes and agriculture taxes) to GDP, its impact on the ratio of shared taxes (and its main components, including value-added tax–VAT and income taxes) to GDP is statistically insignificant. These latter placebo results provide further support to our main theoretical argument, and highlight the potential role of central oversight in addressing local fiscal indiscipline (De Mello Jr, 2000; Baskaran et al., 2016). (iii) Higher VFI levels have no impact on the ratio of extra-budgetary revenues to GDP, backing our interpretation of how the functioning mechanism for the impact of VFI may work. Finally, our results are shown to be robust across alternative specifications and measures of key variables.

The rest of the paper is organized as follows. Section 3.2 briefly introduces the fiscal institutions in China. Section 3.3 summarizes the theoretical arguments on the potential impact of VFI on local government taxing behaviors and derives the baseline hypotheses for the empirical tests. Section 3.4 discusses the empirical methodology, measurements of key variables, and the dataset. Section 3.5 presents the main empirical results, and Section 3.6 concludes.

3.2 Institutional Background

An important feature of China's vertical structure of government is the strong hierarchical link between the different levels of government. Currently, there are five hierarchical levels of government in China (Liu et al., 2015). Starting with the highest, these levels are the center, provinces, prefecture-level cities, counties, and townships. During China's transition from a highly planned to a market economy since 1978,

3.2.1.

intergovernmental fiscal relations between the central and local governments have experienced several major reforms. However, these reforms have largely concentrated on the revenue side of the budget, and generally have not been coordinated with an explicit strategy for the reform of expenditure assignments. As such, much of the revenue assignment authority has been re-centralized at the central level, including that the central government sets uniform statutory tax rates across all local governments, while leaving the expenditure responsibilities largely decentralized at different levels of local government. Below we discuss the current tax assignments and their implication for VFI in more detail.

3.2.1 The 1994 Tax-Sharing System Reform and the VFI

The total fiscal resources of local governments at each level consists of three components: own revenues, transfers from the upper-level government, and extra-budgetary revenues. In 1994, the Chinese government implemented the tax-sharing system (TSS) reform, classifying all taxes into three categories: central taxes, local taxes, and shared taxes between the central and local governments. Local taxes (mainly composed of the business tax, urban infrastructure tax, and agriculture tax) are entirely retained by local governments, while revenues from the shared taxes are shared proportionally between the central government and local governments. Specifically, the TSS reform defined the VAT being shared at a ratio of 75% (central) and 25% (local), and personal and corporate income taxes being shared at a ratio of 60% (central) and 40% (local). The total own revenues of local governments are thus composed of local taxes, the local portion of the shared taxes, and total non-tax revenues.[5] Table B.1 in Appendix B.3 presents the total own revenue structure of the prefecture-city governments for the period 1999-2009. On average, VAT, business tax, and corporate

[5]Total non-tax revenues under the budgetary system include administrative fees, penalty and confiscatory revenue, stated-owned assets operating revenue, and other miscellaneous non-tax revenues.

income tax together accounted for approximately 50% of total own revenues, whereas the agriculture tax and urban infrastructure tax only accounted for 12% of total own revenues.

In order to prevent the potential losses of central revenues that had plagued the system before, the 1994 TSS reform also established separate state and local tax bureaus at the provincial, prefectural, county, and township levels. The state tax bureaus, on behalf of the central governments, were put in charge of collecting central taxes,[6] and the majority of the shared taxes; while the local tax bureaus, on behalf of local governments, were made mainly responsible for the collection of all local taxes and the remaining small portion of the shared taxes. Moreover, the state tax bureaus adopted a vertical management system in the way that the immediate-upper level state tax bureaus directly manage their subordinate lower-level state tax bureaus in terms of organization, staffing, finance, appointment, and removal. That is, the province-level state tax bureaus directly manage their respective city-level state tax bureaus, and the latter directly manage the county-level state tax bureaus. In a sense, the state tax bureau is placed as an independent agency parallel to the same level of local government where it is located. For this main reason, local governments tend to have little practical influence on the collection effort of shared taxes.

In contrast, the local tax bureau, administering local taxes, is essentially an operational department part of the local government, with the latter in charge of the financing, appointment, and removal.[7] Even though local governments have little authority to change the statutory rates of local taxes (which, otherwise, is set up by

[6]Central taxes include the tariff and tonnage tax, the consumption tax and VAT levied by the customs department, vehicle purchase tax, business tax, income tax, and urban infrastructure tax collected from rail transportation, state post, state-owned commercial banks, and head office of insurance companies.

[7]The upper-level local tax offices may simultaneously provide guidance for the interpretation on some of the state tax policies, but it does not virtually intervene the functioning of the lower-level local tax offices.

the central government), they have a considerable number of measures that can be used to manipulate their tax enforcement, so to change the de facto local taxes being collected from the residents and firms. These measures may include strategically placing firms in the "development zones" to enjoy preferable tax treatment, offering reduced rates for local property tax and urban infrastructure tax, negotiating revenue loss contracts with enterprises to actually hide profit from taxation, and lowering tax collection effort directly (Liu and Martinez-Vazquez, 2014).[8] In a similar vein, Ziying and Binbin (2013) provide supporting evidence that local governments in China collude with firms and act as a "helping" hand for firms to evade local taxes but not shared taxes. A typical way to do so is to let local tax bureaus to keep a blind eye on firms' using fake invoices for reducing tax liability.

Additionally, the 1994 TSS reform only explicitly stipulates the tax sharing rules between the central government and provincial governments, leaving space for the provincial governments to specify their own sharing rules for their retained revenues at the sub-provincial level. In practice, the business tax, the resource tax, and the retained shared taxes (including 25% of total VAT and 40% of total income tax) are usually shared in a ratio of 50:50 (provincial: sub-provincial) or other ad hoc negotiation ratios across provinces, while sub-provincial governments are generally allowed to retain 100% of the urban infrastructure tax and the agriculture tax.

By setting the VAT and income taxes as shared taxes, and assigning the central government with the largest share of these major tax instruments, the 1994 TSS reform successfully recentralized revenues to the central level and met the twin reform objectives of raising the central government's revenues and strengthening the control

[8]One example is the setup of Foxconn's factory (Apple's manufacturing partner) in Zhengzhou in 2012. As reported by the *New York Times*, the city government exempted Foxconn from all local taxes on the construction of the huge manufacturing complex and accommodations, provided a discount of 5 percent in utility costs, and lowered social insurance and other payments by up to 100 million dollars a year.

of the central government over the fiscal system. Immediately after the TSS reform, the share of total fiscal revenues assigned to the central government increased from 22.0% in 1993 to 55.7% in 1994 and has remained at around 52.0% since then (Jia et al., 2014). Nevertheless, as pointed out above, expenditure assignments at the different levels of government today are largely what they were decades ago, which assigned sub-national governments (especially city and county governments) with, what may be considered by international norms, excessive expenditure responsibilities. Consequently, the TSS reform started a period of severe VFI for the Chinese local governments, especially for prefecture-level city governments. Figure 3·1 illustrates the general tendency of total own revenues, total own expenditures, and VFI at the city level for the sample period of 1999-2009 (for more detailed discussion of the measure of VFI, see subsection 3.4.2). As shown, the average revenue size of city governments in the sample, measured by the ratio of total own revenues to GDP, is relatively stable at around 5%, experiencing only a slight increase after 2006. By contrast, the average expenditure size of city governments—the ratio of total own expenditures to GDP—has increased continuously, from 9.4% in 1999 to 19.8% in 2009. The gap in revenue and expenditure assignments has contributed to a continuous increase in VFI over the years, and it has been mainly financed by fiscal transfers from the central government.

3.2.2 Fiscal Transfers in China

The rules-based intergovernmental fiscal transfer system in China was initially established in the 1994 TSS reform with the objective of accommodating the increasing gaps in fiscal capacity and expenditure needs across local jurisdictions. Specifically, all transfers from the central government can be broadly categorized into three groups: tax rebates, conditional or earmarked (ad hoc) transfers, and equalization transfers. The tax rebate was a compromised outcome to politically smooth out the implemen-

tation of the TSS reform, with the objective of guaranteeing the vested interests of the richer provincial governments prior to the reform. In essence, the objective was to return to the provinces the amounts of VAT, consumption taxes, and income taxes that otherwise would have gone to those provinces under the system existing prior to the reform. More specifically, the tax rebates on VAT and consumption taxes considered 1993 as the base year and, beginning in 1994, all provinces were guaranteed to receive at a minimum the VAT and consumption tax revenues they had retained in 1993. Earmarked transfers typically involve the central government response to high-priority emergencies or are generally associated with particular programmatic objectives; some examples include fiscal stimulus packages, bail-outs of local government social protection programs, or the "compulsory education transfer" introduced in support of the rural compulsory education program (Liu et al., 2014).

The equalization transfers involve a number of formula-based transfers, all of which have in common the general objective of reducing horizontal fiscal disparities. Over time this component of transfers with equalization objectives has grown steadily to be the most important one. The amount of equalization transfer received by a locality is calculated on the basis of the gap between standard revenues and standard expenditures of the locality, with coefficients that adjust for the size of the gap. Standard revenue is estimated by using the tax bases and standard tax rates, and standard expenditure is calculated by using a myriad of norms for many expenditure categories, including spending on administrative services, public safety, education, city maintenance, social assistance, and heating (see Zhang et al. (2003) and Liu et al. (2014) for more detailed discussion).

Finally, all the amounts of transfers received by each local government are calculated and determined *directly* by the central authority, even though the provincial governments are responsible for the management and the allocation of the funds.

That is, under the hierarchical system, intergovernmental relationships are typically defined and implemented between the government at one level and its immediate upper level government; hence the determined amounts of transfers (to each locality) from the central government will first go to the provincial accounts before reaching the locality (Li, 2010). Practically, although the intermediate governments (e.g., city governments) may have the incentives to retain some of the transfers that otherwise should be directed to the lower level of governments (e.g., county governments), the actual amount of transfers received by the locality should be within a certain range of the one calculated by the central authority.

3.2.3 Extra-Budgetary Revenue as a Source of Local Revenue

Despite the existence of the newly-introduced fiscal transfers system in China, the recentralization of revenue assignments after the 1994 reform and the weakness of revenue bases led local governments to search for more sources of revenue outside the budget system, mainly in the form of extra-budgetary (off-budgetary) revenues (see, for example, Liu and Martinez-Vazquez, 2014; Han and Kung, 2015).

Extra-budgetary revenues include a variety of non-tax items — mostly fees and funds authorized by various departments of local governments, and sometimes "illegal" fees. Unlike own revenues, which are subject to the Chinese budgetary law and the TSS reform regulations, extra-budgetary revenues are income sources over which local governments have exclusive claims; often they are not even required to report them to the central government. In some cases, they are simply the result of the abuse of local governments' power. A good case in point is the land conveyance fees – a revenue stream that local governments obtain from selling the land rights of formerly arable land to a third party for a different (e.g., urban) usage. In addition, local governments have complete authority over deciding which fees and funds should be allocated to the extra-budgetary revenues (Montinola et al., 1995). As the

local governments are given exclusive authority over the disposal of extra-budgetary revenues, they have become increasingly dependent on these revenues for achieving their policy goals. Figure 3·2 provides an overview of the quantitative importance of extra-budgetary revenues at the prefecture-city level in China. As shown, extra-budgetary revenues of city governments increased from less than 0.23% of GDP (i.e., 17.03 billion RMB) in 1999 to about 2.91% of GDP (i.e., 957.69 billion RMB) in 2009.

3.3 Theoretical Review and Basic Hypotheses

The theoretical literature on fiscal federalism often emphasizes that partial fiscal decentralization coupled with large VFI may undermine the fiscal discipline of local governments. In particular, two theoretical constructs, the common pool problem and soft budget constraint, explain how the presence of large VFI may affect local government fiscal behaviors. The common pool problem originates from the well-known theory of "the tragedy of the commons" (Hardin, 1968). Weingast et al. (1981) extend it to the fiscal framework and reach an important implication, namely governments having access to common pool resources would fail to internalize the full cost of public expenditures that benefit their own jurisdictions. Fiscal transfers from the central government are usually deemed a typical common pool resource in a partially decentralized fiscal system, where the central government collects the majority of total revenues and finances local expenditures using transfers (Stein, 1999; Brueckner, 2009; Asatryan et al., 2015). In this case, the perceived costs of public services at the local level become lower than the actual costs. This also creates a wedge in the perceived marginal costs of raising funds from own revenue sources being larger versus the perceived marginal cost of funds from inter-governmental transfers. Thus, local governments face the incentive to expand their public expenditures and/or lower their costly tax effort on own source revenues, shifting part of their cost of expenditures to

other jurisdictions through the intergovernmental fiscal transfers system (e.g., Stein, 1999; Velasco, 2000; Rodden et al., 2003). The larger the VFI, the more severe the common pool problem becomes.

The literature on soft budget constraint also provides consistent implications for the disciplining effects of VFI. It argues that the existence of large VFI (and hence high transfer dependence) may soften local governments' budget constraints because it will create expectations of potential bailouts for local governments. Specifically, given local governments' limited ability in raising revenues in the event of a financial crisis, local voters and creditors may find it difficult to fault local governments and feel compelled to believe that it would be the central government's responsibility to solve the crisis. Such expectations will deteriorate local fiscal discipline and aggravate the common pool problem among local governments (Von Hagen and Eichengreen, 1996; Qian and Roland, 1998; Pisauro, 2003; Rodden et al., 2003). This problem is perhaps exacerbated in the Chinese context because of the role of the central government in the political system. Historically, the Chinese Communist Party and the State's primary task has been to preserve local social political order and stability, which essentially prevents the central government from making a credible commitment not to bail out those local governments in a financial crisis. Thus, the central government is bound to pay a price to ensure that local governments maintain political order and social stability, and this price manifests itself in the form of a growing number of transfers, some of which occur at a massive scale, consequently leading to local fiscal indiscipline (Guo, 2008).

Given that our primary focus is on the revenue indisciplining effect of VFI, we encapsulate the prediction of the theoretical framework into the following testable Hypothesis 1:[9]

[9]In Appendix B.1, we build a simple static model in the tradition of Weingast et al. (1981) to further characterize how a greater VFI may imply a greater level of local fiscal indiscipline.

Hypothesis 1. *Higher VFI levels undermine local fiscal discipline by discouraging local tax effort.*

Even though this classic view provides valuable insights into the indisciplining effect of VFI, the essential premise for this prediction to take place in reality is that local governments should enjoy (at least) certain degree of tax autonomy so they can actually influence their tax collections in response to VFI; clearly, otherwise, it would not be possible to observe any effect. The division of local taxes and shared taxes in the Chinese context, as elaborated previously in subsection 3.2.1, provides us an opportunity to validate this premise, and at the same time serves as a placebo test to confirm Hypothesis 1. That is, while the local portion of shared taxes consist of important source of local revenues, these revenues are administrated and collected by the central authority (i.e., state tax bureau); hence the only feasible way for local governments to lower local revenue collection (in response to VFI) will be to change their tax collection effort on local taxes (but not so for the shared taxes). This prediction is summarized in testable Hypothesis 2:

Hypothesis 2. *Higher VFI levels undermine local fiscal discipline by discouraging local tax collections but do not do so for shared taxes.*

Empirically, what seems to be missing in the previous literature is clear evidence on the working mechanism of the impact of VFI on tax effort. This mechanism assumes that some changes in local fiscal behaviors can directly affect the central policy toward local governments and therefore indirectly influence how local governments actually behave. In the spirit of both theoretical constructs of the common pool and the soft budget constraint, local governments may lower their costly tax enforcement either because they anticipate an offsetting compensation in the form of central transfers or simply some form of a bailout from the central government. Thus, in institutional situations where the link between local fiscal behaviors and

central policies is disconnected, then we should also expect a break in the functioning mechanism powered by VFI.

In the Chinese context, we have seen, extra-budgetary revenues are not only under the full discretion of local governments, but they are also outside the fiscal budgetary system. This means that extra-budgetary revenues are not considered by the central government when determining the amounts of transfers granted to local governments. That is, there is no direct linkage between local performance regarding extra-budgetary revenues and the central government policy. Hence, a straightforward logic follows that local governments should have no incentive to change their collecting (taxing) behaviors for extra-budgetary revenues regardless of their VFI levels. This unique fiscal regime, thus, enables us to indirectly check the functioning mechanism of VFI. A confirmation of our main question of interest would predict an insignificant effect of VFI on the collection of extra-budgetary revenues. We summarize the same in Hypothesis 3.

Hypothesis 3. *Higher VFI levels should have no impact on local collection effort of extra-budgetary revenues.*

3.4 Empirical Strategy and Data

3.4.1 Econometric Specification

In this section, we discuss the empirical specification with the objective of testing Hypotheses 1-3. Specifically, to assess the impacts of VFI on local revenue indiscipline, we estimate a standard two-way fixed effects model of the form

$$I_{ipt} = \alpha + \beta VFI_{ipt} + \mathbf{X}_{ipt}\gamma + \eta_i + u_{pt} + \epsilon_{ipt} \tag{3.1}$$

where i represents a prefecture-level city, p represents the home province of city i, and t denotes the year. The dependent variable I_{ipt} is a series of indicators capturing local

fiscal discipline on the side of taxing behaviors; VFI_{ipt} is the measurement of vertical fiscal imbalance of a city; $\mathbf{X}_{ipt}$ represents a set of control variables, including: real GDP per capita, the shares of the secondary and tertiary sectors in GDP, population density, total retail sales of consumer goods as a percentage of GDP, government size, cultivation land per capita, FDI as a percentage of GDP, the length of highways, total numbers of passengers, number of cities in a province, number of counties in a city, a dummy variable for the rural tax-for-fee reform (TFR), and a series of characteristics of the city party secretary. To capture the potential heterogeneous time patterns of the outcome variables across the provinces, we control for a full set of province-year fixed effects (u_{pt}). Furthermore, the model includes city dummies (η_i) to control for unobserved heterogeneity that are constant over time across cities; ϵ_{ipt} is an idiosyncratic error term. To account for potential correlation across cities, we cluster standard errors at the city level.

3.4.2 Measurement of Key Variables

Local Fiscal Discipline

To test Hypothesis 1, we follow the conventional empirical literature to measure local tax collection effort, which serves as our dependent variable (I_{ipt}), as the ratio of total own revenues to GDP of a city. Other factors being equal, cities with larger values of these ratios should stand for higher levels of revenue/tax collection effort, and thus, stronger fiscal discipline (e.g., Bird et al., 2008; Clist and Morrissey, 2011; Besley and Persson, 2014; Crivelli and Gupta, 2014).[10] This measure is particularly relevant for the Chinese context, as tax legislation in China is highly centralized with the central

[10]Tax revenue as a percentage of GDP has been widely used in the literature as a measure of tax effort. Bird et al. (2008) note that in many countries, especially Latin America, taxes as a share of GDP ("tax effort") has experienced little change over the decades despite major tax reforms because of discretionary ad hoc political measures. Besley and Persson (2014) offer a similar insight with low tax ratios in developing countries largely determined by weak institutions, fragmented polities, and the lack of transparency due to weak news media.

government setting uniform statutory tax rates across all local jurisdictions. Hence, conditional on other characteristics of the cities, a variation in the ratio of the total own revenues to GDP should reflect the differences in tax enforcement and collection effort across cities. To check the robustness of our results, we also consider several alternative measures of tax effort in the subsequent analysis.

Following the same logic, we employ the ratio of local taxes to GDP and the ratio of shared taxes to GDP as dependent variables to test Hypothesis 2 and further confirm our main results from a placebo test angle. As robustness checks, we also utilize as dependent variables the ratios of individual components for both local taxes (including business taxes, urban infrastructure taxes, and agriculture taxes) and shared taxes (including the VAT and income taxes) to GDP to re-conduct the analysis.

Finally, for shedding light on the working mechanism of VFI's impact as summarized in Hypothesis 3, we employ the ratios of both the overall and the specific components of extra-budgetary revenues to GDP as the dependent variables.

Vertical Fiscal Imbalance

Essentially, VFI represents the degree of mismatch between local governments' expenditure responsibilities and their own source revenues, which, in theory, should capture the statutory level of imbalance between the two. Given this conceptual nature, it is in reality inherently difficult to measure it accurately. To solve this problem, the existing literature has mainly used the actual values of spending and revenue as a substitute of their corresponding statutory levels when measuring VFI. In particular, the share of a locality's actual own spending not financed through actual own revenues (i.e., the difference between a locality's actual own spending and actual own revenues) to its actual own spending is usually employed as a proxy of the VFI (e.g., Eyraud and Lusinyan, 2013; Jia et al., 2014). The direct use of this method, nevertheless, is less desirable in our context because actual own revenue may be a result of local

governments' response to the statutory VFI. Therefore, we refine this method a bit in the way that instead of using actual own revenue as a proxy of statutory revenue in calculating VFI, we take as its proxy the difference between actual expenditures and transfers.

Three types of considerations buttress this alternative approach to measuring VFI. First, since subnational borrowing was not legally allowed in China, the difference between statutory expenditure and revenues are supposed to be fully filled by intergovernmental fiscal transfers. Second, actual fiscal transfers received by local governments tend to reflect their own statutory level. This is because the intergovernmental fiscal transfer system in China is largely rules-based with the objective of accommodating the gaps in fiscal capacity and expenditure needs across local jurisdictions.[11] Moreover, all the amounts of transfers received by each local governments are calculated and determined directly by the central authority. The formula-based and non-discretionary features of intergovernmental transfers implies that the actual figures on transfers, based on fiscal capacity and expenditure need, should be close to the level of transfers intended in the statues, that is legally and economically. Third, even though it might be a little bit more of a stretch to claim that actual spending is definitely the statutory expenditure, actual figures do reflect the *de jure* expenditure responsibilities in China's context. Given the highly centralized political system in China, local governments at all levels must propose their annual budget plans of all projects based on their local expenditure needs to the corresponding Local and National People's Congress at the beginning of each fiscal year. Despite the slight deviations from the planned budget, government actual expenditures in most localities

[11]For example, the equalization transfers, which take up the most important part, involve a number of formula-based transfers. All of which have in common the general objective of reducing horizontal fiscal disparities. The strictly formula-based feature of equalization transfers enables us to conduct a robustness check using the ratio of equalization transfers to total expenditures as a further more statutory partial measure of VFI.

in China still fall within a reasonable range of the approved budgets. Additionally, it is also quite common that the central government sets the targets and standards for local governments expenditures in China.[12] In all, we believe that, especially in the case of China, actual spending could be an acceptable measure of the statutory expenditures, reflecting the *de jure* expenditure responsibilities, and hence, our measure of VFI should be close to its statutory level.

As shown in Figure 3·1, city governments in China evolved a severe mismatch between expenditure responsibilities and their revenue capacities from 1999 to 2009, with an average VFI of 49.03% during this period (see Table 3.1).[13] Table B.2 in Appendix B.3 provides a detailed description and sources of all the variables, while their summary statistics are reported in Table 3.1.

3.4.3 Endogeneity

A potential concern is that the endogeneity of VFI may bias the estimates in our baseline regressions. This issue may arise because of reverse causality, omitted variable bias, and measurement errors. In our particular context, the endogeneity may be mainly rooting in the potential measurement errors of VFI, which, as discussed in subsection 3.4.2, might be captured by different dimensions, and which, by nature is difficult to be measured accurately with limited data information.

To circumvent the endogeneity issue and identify the causal impact of VFI on local fiscal indiscipline, we use an instrumental variable estimation. Specifically, we use as an instrument (IV_{ipt}) *the change in predicted school-age population* (0-14 years old) between two consecutive years. The predicted school-age population ($PSAP_{ipt}$) is

[12] China is not a unique case that central government controls and oversights the local government spending. The practice of setting regulations for subnational expenditures is prevalent in many other countries. For instance, the central government closely manages the subnational implementation of various sectoral policies in Germany, Norway, Italy, and Portugal (Joumard and Kongsrud, 2003).

[13] Under China's current fiscal transfer system, cities may submit remittances and receive grants at the same time. There are a few negative values of VFI in the dataset, which indicates the cases where cities' remittances to the upper-level government are larger than the grants they receive.

calculated by using the actual school-age population in 1999 as the base and assuming time-variant growth rates over years, that is

$$PSAP_{ipt} = SAP_{ip,1999} \times \prod_{j=2000}^{t} (1 + \widehat{Popgr_{ipj}}), t = 2000, 2001, ..., 2009 \tag{3.2}$$

$$IV_{ipt} = PSAP_{ipt} - PSAP_{ip,t-1}, t = 1999, 2000, ..., 2009 \tag{3.3}$$

where $SAP_{ip,1999}$ is the actual school-age population for city i in province p in year 1999, and $\widehat{Popgr_{ipj}}$ is the simulated natural population growth rate for city i in province p in year j.[14] To ensure the exogeneity of the population growth rate for city i in the calculation of the instrument, we do not use the actual natural population growth rate for city i, but instead we construct it (i.e., $\widehat{Popgr_{ipj}}$) as the weighted average of the actual population growth rate for all other cities (except city i itself) in the same province.[15]

The validity of the instrument can be justified as follows. Our strategy is to search for instruments with some exogenous variations that may explain the constructed VFI, which mainly involves local expenditures and transfers, but not local revenue collection. School-age population falls in this range. As we previously noted, China has maintained a rather decentralized system in terms of expenditure assignments, with local governments providing the majority of essential public services, especially (compulsory) elementary and middle school education. In particular, the share of education expenditure in total local government expenditure is around 20% on average in our sample for the period between 1999 and 2009 at the prefecture-city

[14]The predicted school-age population in 1998 is calculated as $SAP_{ip,1999} \times \frac{1}{1+\widehat{Popgr_{ip,1999}}}$. Note that annual data on actual population age structure are not available at the sub-provincial level. The most comprehensive and authoritative data on population age structure can be sourced from the population census conducted by the central government every five years since 1990.

[15]The weight is total population of the city. The argument here is that the population growth rate in city i is correlated with the population growth rate in other cities in the same province. We have also tried using the actual growth rate of population for the calculation of the instrument and our estimation results are largely unchanged.

level. Additionally, the 1995 "Education Law of the People's Republic of China" explicitly stipulates that the growth rate of education expenditure should exceed the growth rate of government revenues, and per student education expenditure should increase annually (Article 55). These institutional facts suggest a high correlation between local public education expenditure needs and VFI. Furthermore, there exists an education subsidy program belonging to the formula-based equalization transfers category, and school-age population is exactly one of the key determining factors in the formula of that subsidy program. This further suggests that school-age population is a major predictor for the demand and increase in basic education and hence local expenditures, but not directly for local revenue collection. This justifies using school-age population as an arguably good candidate for instrumenting the VFI. In subsection 3.5.1, we provide additional discussion and perform additional formal tests to check the validity of the selected instrument.

3.4.4 Data

The panel dataset we use for the quantitative analysis covers 325 prefecture-level cities in China for the years 1999-2009. We exclude the four province-level municipality cities, Beijing, Tianjin, Shanghai, and Chongqing, as the legal status of these municipalities is non-comparable to other regular cities. Cities in Tibet are also excluded from the sample because of data unavailability. Since data on some of the key variables such as personal income taxes and corporate income taxes are only available from 1999, we select 1999 as the starting period in our analysis; the end year of the panel dataset, 2009, is the last year that disaggregated fiscal data at the sub-provincial level were released.

Data on the key variables are collected from the *Prefecture, City, and County Public Finance Statistics (Quanguo Dishixian Caizheng Tongji Ziliao, 2000-2010)*. This is the most detailed and disaggregated dataset on subnational public finances and some

basic economic and socio-economic variables (such as GDP, shares of secondary and tertiary sectors in GDP, and fiscally supported population, etc.). Data on the natural population growth rate, FDI, the length of highways, and total number of passengers, are collected from various issues of the *China Statistical Yearbook for Regional Economy* (National Bureau of Statistics, 2000-2010). Information on population of school age (i.e., 0-14 years old) in 1999 is obtained from the *Provincial Tabulation on the 2000 Population Census* (National Bureau of Statistics, 2001) for each province. The city party secretaries' information is obtained from their curriculum vitae, which are publicly available online.

3.5 Empirical Results

3.5.1 The Impact of VFI on Local Fiscal Discipline

Descriptive Evidence

Before we present the results from the formal estimations, Figure 3·3 depicts the scatter plot for the relationship between VFI and our measure of local fiscal discipline (i.e., the ratio of total own revenues to GDP) across cities over the sample period. As it clearly shows, there exists a strong negative correlation between the two, providing tentative evidence on the discouraging effect of VFI on local fiscal discipline.

OLS and IV Results

The OLS estimation results for specification (1) with different sets of control variables are presented in Columns (1)-(2) of Table 3.2. In all estimations, the dependent variable is the ratio of total own revenues to GDP.[16] We include city fixed effects and the full set of province-year dummies to control for unobserved heterogeneity and

[16]Note that total own revenues include shared taxes, and shared taxes' collection is out of the direct control of local governments. Here we use this dependent variable as the starting point of the analysis and later decompose them into local taxes, shared taxes, and non-tax revenues for further analysis.

specific province-time fixed effects.

To begin with, we examine the effect of VFI on local fiscal discipline without adding any other explanatory variables to the model. As shown in Column (1), the coefficient of VFI is negative and statistically significant at the 1% level, supporting Hypothesis 1 that the presence of VFI reduces local revenue collection efforts. We then add a rich set of economic explanatory variables and city leader's characteristics in Column (2). Again, we find a negative impact of VFI on local fiscal discipline on the side of revenue collection. Quantitatively, the estimated coefficient of VFI in Column (2) is -0.070, indicating that a one-percentage point increase in VFI is associated with a reduction of the share of total own revenues in GDP by 0.07 percentage points. At the mean values (see Table 3.1), this translates into an equivalent reduction of 1.35% (i.e., 0.07/5.19) in the share of total own revenues to GDP.

The IV estimation results are presented in Columns (3)-(4) of Table 3.2. After being instrumented, VFI still has a negative and statistically significant impact on local fiscal discipline on taxing behavior, which further supports Hypothesis 1. In our preferred IV specifications of Column (4), the coefficient of VFI is -0.103. Hence, a one-percentage point increase in VFI tends to result in a decrease of 0.103 percentage points in the share of total own revenues to GDP.

In Table B.3 of Appendix B.3, we decompose total own revenues into its two main components—total tax revenues and total non-tax revenues—and take their ratios with respect to GDP as alternative dependent variables to re-estimate specification (1). Again, we find a significantly negative impact of VFI on the two ratios, with the estimated coefficients are small in magnitude compared to the results in column (4) of Table 3.2. This is consistent with the fact that total own revenues is a broader concept than total tax revenues and non-tax revenues, thus implying a larger scope for strategic responses

Validity of the Instrument

The IV estimates are unbiased only if the instrument is valid. A potential concern with using this instrument variable is that change in the predicted values of school-age population might be correlated with other economic variables affecting local revenues. In particular, the group of school-age population may be itself a reflection of the age composition of the city population, which may affect city revenues through its impact on the labor market and consumption pattern in the city. We argue that this concern is lessened, at least partially, by the many control variables relating to the labor market and consumption pattern of the city included in the estimations. Furthermore, Figures B·1 and B·2 in the Appendix show that the predicted change of school age population is actually not correlated with the change of employment in any industry and the total retail sales of consumer goods. Hence, our instrument is less likely to affect local revenues via labor market performance and consumption pattern of the city.

From a technical perspective, a valid instrument needs to meet both the relevance and exogenous conditions criteria. Here, we provide some more formal evidence that these two criteria are satisfied with the selected instrument. To start with, Columns (1)-(2) in Table 3.3 report the first-stage estimation results in the IV estimations. We find that the instrument (i.e., the change in predicted school-age population) is positively and significantly correlated with VFI, confirming our argument for the relevance of using school-age population as an effective predictor for local expenditures, and thus, the VFI. The under-identification is also strongly rejected with the LM statistic ranging from 8.853 to 11.400 depending on different specifications (see Table 3.2). Meanwhile, for both the IV specifications, the F-statistics are well above 10, suggesting that our IV estimates are not prone to the weak instrument concern. To further preclude the concern of weak instrument, we also report 95%

(heteroskedasticity-adjusted) Anderson-Rubin confidence intervals in Table 3.2, which are robust against weak instrument problems and heteroskedasticity in the second stage estimation (Chernozhukov and Hansen, 2008; Mikusheva and Poi, 2006). This interval also comfortably excludes a zero effect.

The exogenous condition requires that the instrument should not directly affect local government's taxing behaviors except through its impact on VFI. This condition is difficult to check directly. Nevertheless, we implement an indirect test by adding the instrument as an additional explanatory variable to the baseline specification (1). We anticipate an insignificant result of it in this augmented specification; otherwise, it would indicate that the instrument does have other channels to influence local taxing behaviors after controlling for its impact on VFI. Columns (3)-(4) in Table 3.3 report the results for the augmented specification. The estimated coefficients of the instrument are consistently insignificant across different specifications. Taken all together, this finding significantly increases our confidence on the validity of the instrument.

Finally, we employ the "plausible exogenous" estimation developed by Conley et al. (2012) to provide a further robustness check to our IV estimation results. This method allows for consistent inference for 2SLS estimation in situations where the instrument being only plausibly exogenous. The basic idea is that it allows the instrument to enter linearly into the second-stage regression with a coefficient (i.e., δ), and then consistently estimates the true effect of the endogenous variable when δ takes on different values near 0 but not exactly 0 (plausibly exogenous). Following Conley et al. (2012), Figure 3·4 plots the 95% confidence intervals for the estimated β for different values of δ over the support condition that $\delta \in [-0.5, 0.5]$.[17] We

[17]We employ the Union of Confidence Interval Approach described in Conley et al. (2012). The specification here is $I_{ipt} = \alpha + \beta VFI_{ipt} + IV_{ipt}\delta + \mathbf{X}_{ipt}\gamma + \eta_i + u_{pt} + \epsilon_{ipt}$, where the change in the predicted values of school-age population (IV) is allowed to enter linearly into the second-stage regression with a coefficient δ. By assigning different values to δ over a small range of possible values

find that in our framework, when $\delta > 0$, the confidence intervals for the estimates of β are actually further away from zero relative to the IV estimate of β. In other words, the negative estimate of δ reported in Column (4) of Table 3.3 (-0.246) is not below the value of δ necessary to lose confidence in the finding of a negative impact of VFI on local fiscal discipline. Therefore, even with a plausibly or approximately exogenous instrument, we are still able to confirm a negative effect of VFI on local fiscal discipline.

3.5.2 Placebo Analysis

Having obtained strong empirical support for Hypothesis 1, we now proceed to conduct placebo analysis by testing Hypothesis 2. As discussed in Section 3.3, our conjecture is that the collection of local taxes and shared taxes should be influenced differently by VFI, since they are administrated by different tax authorities. While the collection of local taxes is at the discretion of local governments, and thus should be vulnerable to changes in incentives faced by local governments, the collection of shared taxes is under the direct and strict control of the central government, and thus it might not be sensitive to the presence of VFI at the local level (hence serving as a placebo case). To shed light on this, we consider these two different sources of revenues as the dependent variable, respectively.

Tables 3.4 and 3.5 report the IV results with the ratio of local taxes (along with its main individual tax categories including business taxes, urban infrastructure taxes, and agriculture taxes) to GDP, and the ratio of shared taxes (along with its main individual tax categories, including the VAT and corporate income taxes) to GDP as the dependent variables, respectively. As shown in Table 3.4, regardless of the

near zero, we are able to examine how the true confidence interval of β in the 2SLS estimation is influenced when IV is plausibly exogenous. Note that when δ is zero, the inference under this strategy is identical to ordinary 2SLS estimation. See Conley et al. (2012) for a more detailed discussion of the method.

use of aggregated local taxes or individual local taxes as the dependent variable, the coefficients of VFI are consistently negative in all four specifications, supporting the first part of Hypothesis 2 that city governments respond to VFI by reducing their taxing effort on local taxes. However, the negative effect of VFI is not statistically significant when the ratio of business taxes to GDP is used as the dependent variable. This is not a surprise though. As we have mentioned in subsection 3.2.1, even though the business tax is a type of local tax collected by local tax bureaus, it is commonly shared between provincial governments and sub-provincial governments (including cities, counties, and townships) at certain ratios varying across provinces.[18] Hence, local collection of business tax tends to be still under strict scrutiny by the upper level authorities (i.e., the provincial governments) because they also share in the tax. To give an example of how the scrutiny may work, provincial governments frequently set up annual tax planning targets for their subordinated governments regarding shared taxes, leaving little room for city governments to manipulate their taxing behavior in the case of the business tax. Consequently, even though local tax bureaus collect the business tax, city governments actually have limited autonomy on this particular tax. The insignificant effect of VFI on business tax tends to suggest the hierarchical supervision or administrative subordination plays some role for revenue discipline.

Quantitatively, the VFI appears to have the largest impact on the collection of agricultural taxes. This result may be because the agriculture sector represents the hardest to tax sector in the economy, and given the rising opposition among farmers to many diverse local charges in rural China in the period examined, local governments have had more leeway to change their taxing behaviors for the agriculture sector in the presence of VFI.

[18] As a matter of fact, some of the business tax revenues are shared between local governments and the central governments. For instance, 37.5% of business tax from the financial and insurance sectors belongs to the central government, while the rest is shared among different levels of sub-provincial governments.

Table 3.5 provides strong evidence for the second part of Hypothesis 2, which works as an important placebo test. As predicted, the VFI has no significant impact on the collection of either aggregate shared tax or its main components (the VAT and income taxes). Moreover, the results do not show either any discernible pattern for the effect of VFI on different types of shared taxes, where we observe a negative estimate for the VAT and a positive estimate for income taxes.

While, in general, the findings for the shared taxes are in line with our theoretical prediction, we need to be cautious when interpreting the results for the corporate income tax. The power to collect corporate income tax has only been shifted from local tax bureaus to state tax bureaus in 2002, a result of the income tax reform in that year. In particular, the reform stated that firms established since 2002 are assigned to state tax bureaus for corporate income tax collection, while tax collections for firms established before 2002 remain the task of local tax bureaus. This implies that our estimations based on aggregated data could not capture this confounding factor. To further investigate this point, we rely on firm-level data from the Annual Survey of Industrial Firms (ASIF) conducted by the National Bureau of Statistics of China[19] for the period (i.e., 1999-2007) to identify whether the firms were established before or after 2002. Then we calculate effective corporate tax rates for these two groups of firms (under supervision of local tax bureaus and state tax bureaus, respectively) at the firm-year level. Finally, we perform separate estimations by using the aggregated effective corporate tax rate at the city-year level for firms established before and after 2002 as dependent variables,[20] respectively, and report the results in Columns (4) and (5) of Table 3.5. The coefficient of VFI in Column (4) is quantitatively much larger

[19]This dataset has been widely used in the literature (for example, Hsieh and Klenow, 2009; Guariglia et al., 2011; Brandt et al., 2012; Feenstra et al., 2014).

[20]The effective corporate tax rate for the firms is defined as the ratio of income tax payable to earnings before interest and tax. We calculate the weighted average of effective corporate income tax rate for firms established before and after 2002 in a city. The weight is a firm's added value.

than that in Column (5) (although they are statistically insignificant), and the former also has much smaller standard error (statistically significant at the margin) than the latter. These results support our main argument that local governments respond to the presence of VFI only when they have enough taxing power discretion.

In sum, our analysis validates Hypothesis 2 and constructs a valid placebo test for our main theoretical argument on the effect of VFI on fiscal discipline. The placebo results on the shared taxes also highlight the importance of central oversight in addressing the distortive effect of VFI under a partial decentralization setting — a finding that is consistent with the previous work such as Baskaran et al. (2016).

3.5.3 The Mechanism of Impact

In this subsection, we continue to explore the empirical evidence on the working mechanism for the impact of VFI by examining extra-budgetary revenues. Recall that this part of local revenues is an incentive-incompatible element as it does not enter the budgetary system and therefore is not considered as part of the local revenue capacity when calculating central transfers to local governments. In other words, transfers from the central government will not compensate for the lower collections of extra-budgetary revenues. Therefore, local governments will have less motivation to change their collection effort for these revenues in face of VFI. Therefore, an insignificant impact of VFI on local governments' revenue collection effort for extra-budgetary revenues will support Hypothesis 3 and shed further light on the working mechanism highlighted by the theory.

Table 3.6 reports the estimation results for the effect of VFI on local collection of extra-budgetary revenues: Column (1) with the ratio of total extra-budgetary revenues to GDP as the dependent variable, while Columns (2)-(4) with the ratios of the three specific components of extra-budgetary revenues (i.e., industry and transportation sector funds, agriculture sector funds, and land conveyance fees) to GDP

as the dependent variables, respectively. Although the estimated coefficients of VFI are negative, they are highly insignificant in all specifications, supporting Hypothesis 3. These results also reinforce our main argument and underline the functioning mechanism for the impact of VFI.

3.5.4 Robustness Checks

Alternative Measure of VFI

We first check whether our findings might be driven by the measurements of VFI. As highlighted in subsection 3.4.2, the equalization transfer is the part of transfers that is strictly formula-based. Since the formula involves the comparison of expenditure needs and fiscal capacity of each locality, the equalization transers itself may capture a certain degree of the statutory level of VFI. Thus, we repeat the empirical tests for Hypotheses 1-3 using the ratio of equalization transfers to actual total expenditures as the new measure of VFI and report the results in Panel A of Table B.4. Largely consistent with our baseline results, we find that the equalization transfer dependency has a negative and statistically significant effect on the ratio of total own revenues to GDP and the ratio of local taxes to GDP. And it has no significant impact on the ratio of shared taxes to GDP and the ratio of extra-budgetary revenues to GDP.

Alternative Measures of Local Fiscal Indiscipline

Because of its very nature, using the tax-to-GDP ratio for measuring local fiscal indiscipline may be subject to certain measurement error. In particular, our baseline results might be confounded by the potential effect of VFI on GDP (the denominator) rather than actual tax revenues collected (the numerator). To address this concern, and as a robustness check, we use directly the actual revenues collected (in logarithm terms) as the dependent variables. The results presented in Panel A of Table B.5 in Appendix B.3 are quite robust and largely unchanged.

Similarly, another potential concern can be raised regarding the use of realized tax revenue as a measure of local tax effort. In particular, realized revenue could be a function of both the local tax effort and underlying tax base. An alternative measure, more free of those concerns, would be the difference between the realized revenues and the potential revenues. Thus, we follow the regression approach initially pioneered by several IMF studies (e.g., Lotz and Morss, 1967; Bahl, 1971; Chelliah et al., 1975) to obtain an alternative measure of local tax effort. Specifically, the approach first regresses the actual tax revenue to GDP ratio on a set of explanatory variable proxies for tax bases and "tax handles". Those estimates are then used to obtain the potential (projected) local revenues. The index of local tax effort is calculated as the ratio of actual tax revenues to the predicted potential tax revenues. Panel B of Table B.4 repeats the estimations for specification (3.1) using this new tax effort index as the dependent variable.[21] Again, the estimation results are largely consistent with our baseline ones, lessening the concern over the measures of the dependent variable.

Lastly, measurement error may simply arise due to the use of macro-level data that are more vulnerable to measurement errors during their compiling process. Hence, we rely on firm-level data from the ASIF to re-calculate the key dependent variables. In particular, we first calculate total effective tax rates, effective local tax rates, and effective shared tax rates for each firm in each year,[22] and then aggregate them to

[21]More specifically, in the first-step, we regress the ratio of tax revenues to GDP on a set of explanatory variables including GDP per capita, the share of agricultural sector in GDP, and the share of secondary sectors in GDP, with robust standard error clustered at city level. In the second-step regression, we use bootstrap to correct standard errors because of the potential correlation between imputed tax revenues generated by the first-stage analysis.

[22]The ASIF provides a wide range of information on tax payments of the firms, including corporate income tax, VAT, business tax and surcharges for main operations, and other local taxes for management (a combination of stamp tax, urban real estate tax, land usage tax, and vehicle and vessel usage tax). We take the summation of all these taxes as a measure of total tax burden of the firms, the summation of business tax and surcharges for main operations and other local taxes for management as a measure of total local taxes paid by the firms, and the summation of corporate income tax and VAT as a measure of total shared taxes paid by the firms. All the effective values are calculated using firms' value-added as a reference (i.e., the denominator).

the city level by taking the weighted average of the firms in a city (the weight is firm value-added). The estimation results, using these new dependent variables, are reported in Panel B of Table B.5 in Appendix B.3. We find robust evidence from the micro-level data—the effect of VFI on total effective tax rate and effective local tax rate is still negative and statistically significant, and not so for the effective shared tax rate.

Alternative Measure of the Instrument

We re-calculate the instrument (i.e., the change in the predicted school-age population) using historical data on school-age population in 1989 (instead of 1999) as the base in order to ensure that we avoid the endogeneity concern. The results are reported in Panel C of Table B.5 in Appendix B.3. As shown, the results are quite comparable to the corresponding baseline ones, confirming our notion that local governments respond to the presence of the VFI by reducing local revenue collection, particularly by lowering the collection of local taxes under the direct control of local governments.

Other Potential Confounding Factors

We also consider some other confounding factors that may bias our estimations. First, given that local officials primarily care about their own political promotion, there may be a concern that in the presence of VFI, local officials may face stronger incentives to increase (rather than reduce) their tax effort in order to collect sufficient revenues for development. While this concern should have been lessened by controlling many political factors in our baseline estimations, we further address it by restricting our sample to those cities whose leaders (party secretaries) are under the age of 53—In china, this age is usually regarded as a threshold for the final chance of promotion for city party secretaries (Yu et al., 2016). If political promotion incentives have

not been well controlled for in our baseline estimations, we would expect that in the estimations for this subsample, the negative effects of VFI should be reduced. The results in Panel A of Table B.6 in Appendix B.3 indicate that the estimates for VFI in all specifications are very close to the baseline estimations in terms of both magnitudes and significance level. Hence, this concern does not bias our main results.

Second, throughout our sample period (i.e., 1999-2009), there were a few tax reforms being implemented, and their effects may be not fully captured by the city and province-year fixed effects. In particular, in 2004 the Chinese authorities implemented the so-called VAT transformation reform, allowing for the deduction of firms' purchases of fixed assets from the VAT bases in certain industries in three North-eastern provinces (i.e., Liaoning, Jilin, and Heilongjiang). This reform may have significantly reduced tax burdens of the firms in these regions, and thus bias our results. To address this issue, we exclude all prefecture-cities in those three North-eastern provinces from the sample and re-estimate the equation (1). The results are largely unchanged (see Panel B of Table B.6 in Appendix B.3).

Analogously, in 2006 the new tax legislation increased the personal income tax threshold from 800RMB to 1600RMB and in the same year the agriculture tax was abolished. Both of these reforms may also reduce the tax ratio. To preclude this concern, we restrict our sample to only include observations prior to 2006 (i.e., 1999-2005) and re-estimate our specification. As shown in Panel C of Table B.6 in Appendix B.3, our results are not affected by these reforms in any significant way.

Lastly, we consider some more control variables to further rule out the possibility of omitted variables. These include the ratio of fixed assets to GDP, the number of above-scale firms in the manufacturing sector,[23] and the share of working population in total population. The ratio of fixed assets to GDP is included to control for factors

[23]Above-scale firms are those firms with sales over 5 million RMB (approximately 0.7 million U.S. dollars). Given data availability, we do not have information on the total number of all firms in the city.

such as expansion in real estate's market that may affect GDP (the denominator of our dependent variable) to a larger extent than it may affect tax revenues (the numerator of our dependent variable). By contrast, the number of above-scale firms and the share of working population in the total population are included on the grounds that the increase in the number of firms and the enlargement in working population may spontaneously increase tax revenues but not necessarily tax enforcement of the cities. As shown in Panel D of Table B.6 in Appendix B.3, even with all these additional controls the estimated effects of the VFI on local government taxing behaviors are strongly consistent with our baseline IV estimation results.

3.6 Conclusion

Since the TSS reform in 1994, the Chinese fiscal system has been characterized by the marked presence of VFI at the local level, giving rise to the twin common pool and soft budget constraint problems with the potential of significantly distorting the fiscal discipline of local governments. Using a large panel dataset at the city level in China from 1999 to 2009, this paper is among the first to examine the effects of VFI on local fiscal discipline in the context of tax behavior responses of local governments, and it explicitly explores the working mechanism for the impact of VFI on tax effort.

We find that VFI significantly reduces the ratio of total own revenues to GDP, supporting the prevailing argument that higher levels of VFI may induce fiscal indiscipline by reducing tax effort of local governments. We take advantage of the unique Chinese fiscal institutional framework that assigns the taxing power for local taxes and shared taxes to two separate authorities (i.e., the local tax bureau and the state tax bureau, respectively). This allows us to further explore the heterogeneous effect of VFI on the revenue collection of local taxes and shared taxes. Our results reveal that local governments respond to the presence of VFI by lowering their tax effort on

local taxes, for which they have certain tax autonomy. However, higher levels of VFI have no significant impact on the collection of shared taxes, which are under the direct administration of the central government. We further show that the effect of VFI on fiscal discipline does not exist in the context of extra-budgetary revenues, which provides additional evidence on the working mechanism of the effect of VFI on tax effort. Hence, local fiscal institutional arrangements are important for understanding how the effect of VFI on fiscal discipline works. Our results are robust across different specifications, correcting for the endogeneity of VFI, and for alternative measures of VFI.

From a policy perspective, the evidence from China is very relevant to other decentralized countries around the world because it comes from a case where there is limited local tax autonomy, so that if the effects of VFI are strong there, it is likely that they would be even more present in countries where local governments have more tax discretion. For instance, there is considerable cross country evidence on the low tax yields of traditional local taxes, such as the property tax, even where local governments have discretion to set rates—as is the case in many decentralized countries. This has been a puzzle in many developing countries, but also in some developed countries. Low tax effort has been blamed on transfer dependence, soft budget constraints, and other factors. Thus, our findings fit into the explanations for this observed phenomenon. In addition, the advantage of doing the study for China is that it allows a much clearer identification of the role of VFI on local tax effort because China has separate identified local taxes as well as local tax administrations, together with shared taxes that are mostly collected by the state tax administration. From the perspective of policy implication, the results on the shared taxes highlight the potential role of central oversight in addressing the distortive effect of VFI in partial decentralization settings. In addition, if the continued expansion of VFI at the local

level and the resulting distorted taxing behaviors of local governments are deemed undesirable by the Chinese central authority, then there will be a need to rebalance the expenditure assignments and revenue assignments to local governments. In theory, either assigning more revenue sources to local governments or recentralizing some of the existing expenditure responsibilities to the central government should be measures in the right direction. In practice, since many social expenditure responsibilities in public welfare, including unemployment compensation and pensions, have long been wrongly assigned to subnational governments, especially at the lowest level (Liu et al., 2015), there is plenty of scope for China's central government to reassign and centralize those expenditure responsibilities. As the findings of this paper attest, this type of policy—or providing local governments with greater tax autonomy—would contribute significantly towards raising the fiscal discipline of local governments in China.

3.7 Figures and Tables

Figure 3·1: Average Values of Total Own Revenues, Expenditures, and VFI of City Governments

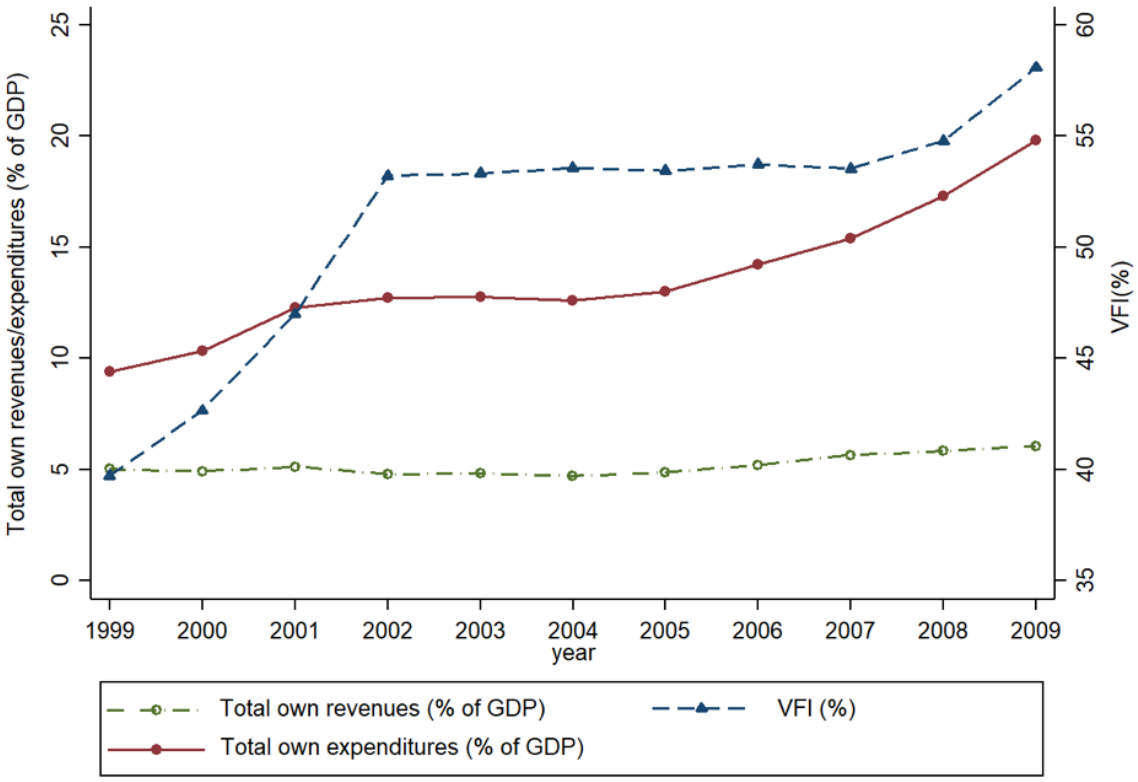


Figure 3·2: The Size of Extra-Budgetary Revenues of City Governments

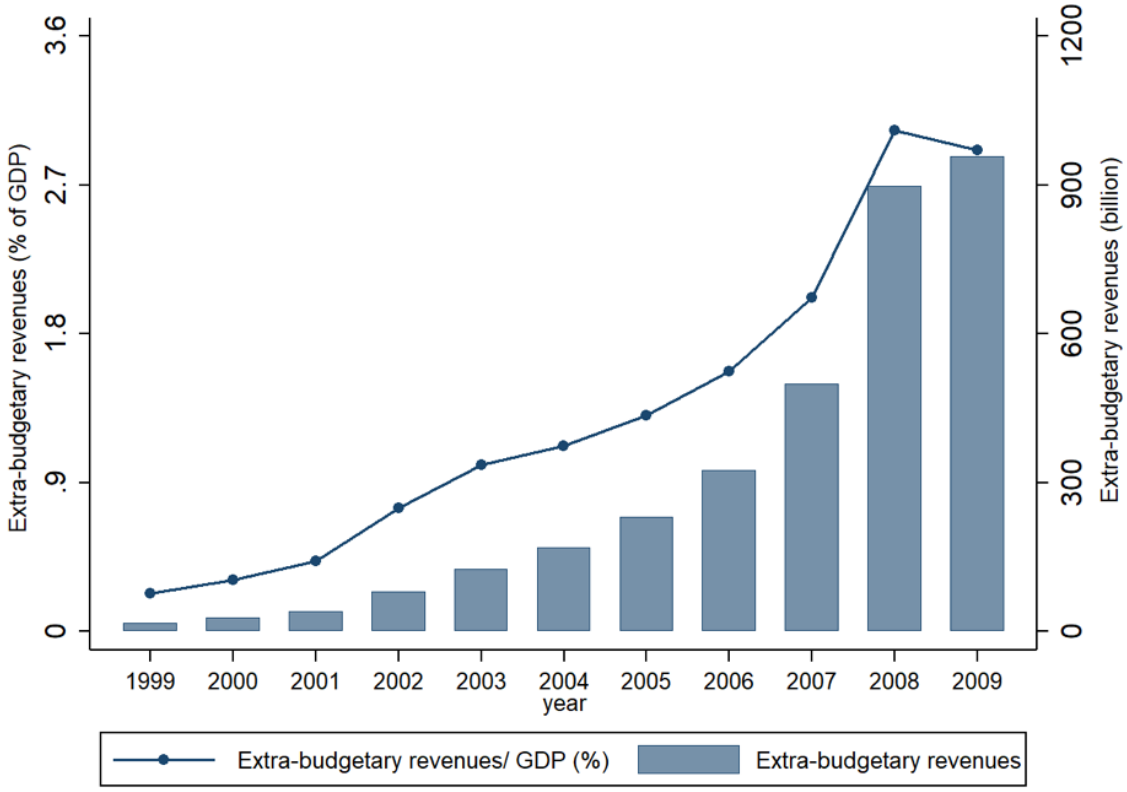


Figure 3·3: The Correlation Between VFI and the Ratio of Total own Revenues to GDP

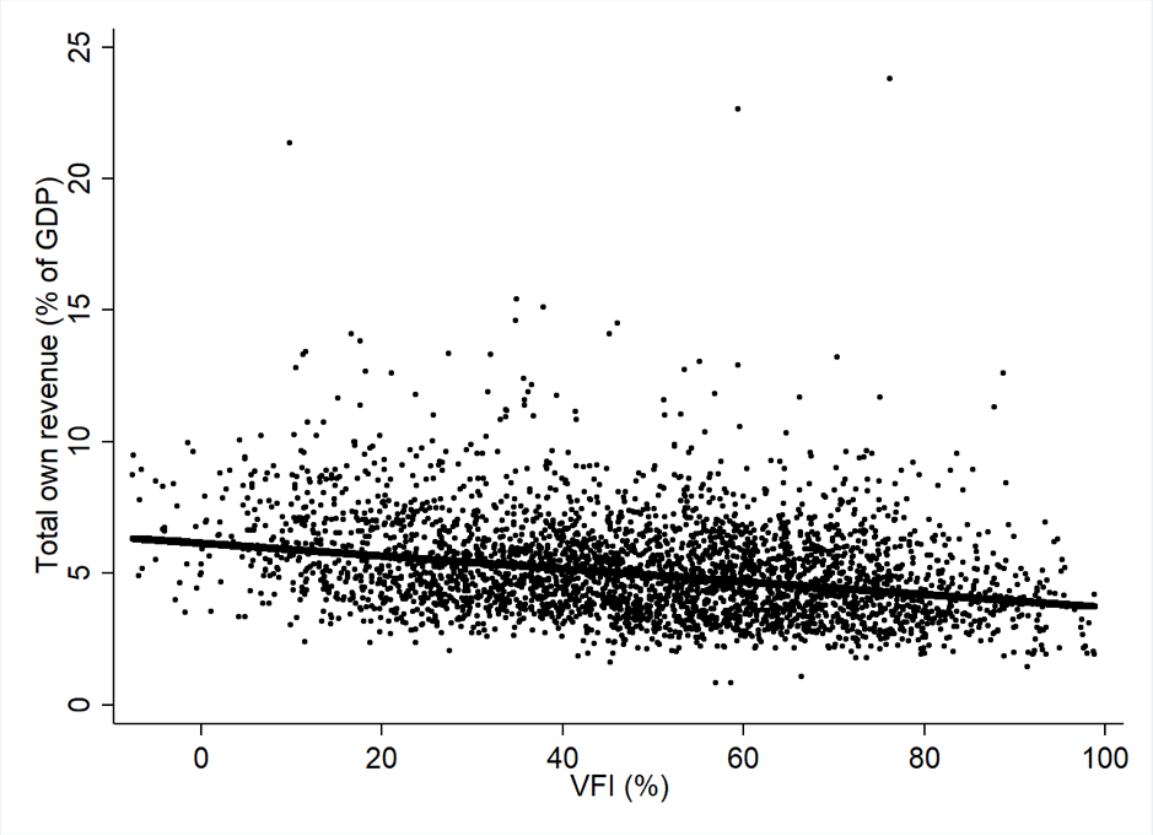


Figure 3·4: 95% confidence interval estimates for different values of δ

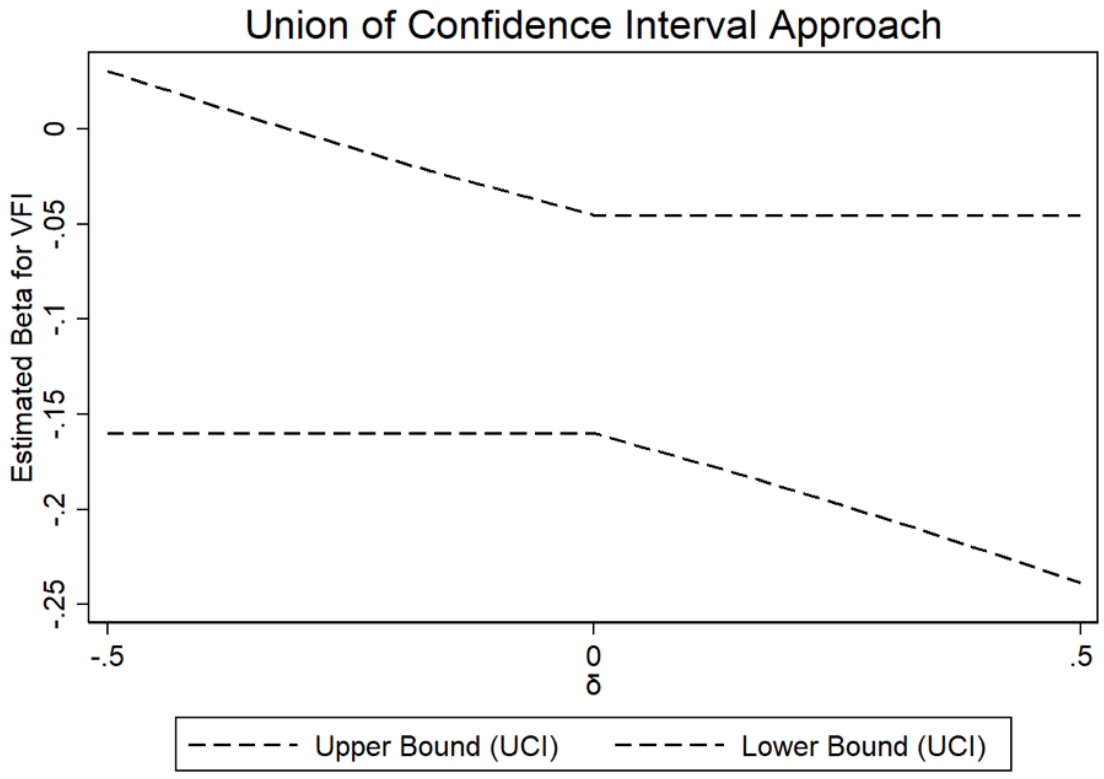


Table 3.1: Summary Statistics

Variables	**Obs.**	**Mean**	**Std. Dev.**	**Min.**	**Max.**
Total own revenues (% of GDP)	3,494	5.19	1.93	0.84	23.8
Local tax revenues (% of GDP)	2,782	1.92	0.82	0.35	7.52
Business tax revenue (% of GDP)	3,477	1.15	0.67	0.15	6.65
Agriculture tax revenue (% of GDP)	2,797	0.51	0.38	0.01	2.93
Urban infrastructure tax revenue (% of GDP)	2,823	0.3	0.2	0.05	2.39
Shared tax revenues (% of GDP)	3,140	1.53	0.84	0.11	6.65
VAT revenue (% of GDP)	3,477	0.89	0.54	0.07	5.93
Corporate income tax revenue (% of GDP)	3,142	0.41	0.37	0.01	3.48
Extra-budgetary revenues (% of GDP)	3,308	1.27	1.37	0.04	6.82
Industry and transportation sector fund (% of GDP)	1,822	0.03	0.05	0	0.4
Agriculture sector fund (% of GDP)	1,893	0.03	0.04	0	0.25
Land conveyance fee (% of GDP)	2,185	0.37	0.42	0.01	2.03
VFI (%)	3,500	49.03	21.4	-7.59	98.8
Log (real GDP per capita)	3,493	9.19	0.79	7.07	12.6
Log (population density)	3,507	5.34	1.36	-0.44	8.45
Share of secondary industry, as % of GDP	3,507	44.73	12.7	8.15	90.97
Share of tertiary sector, as % of GDP	3,467	35.6	7.69	8.5	85.34
Log (cultivation land per capita)	3,321	6.5	0.79	2.76	8.98
FDI/GDP (%)	2,870	2.45	3.56	0	51.41
Log (highway)	3,528	8.66	0.78	5.36	10.62
Log (passengers)	2,979	8.37	0.88	4.81	11.89
Number of cities	3,575	13.96	3.89	2	21
Number of counties	3,559	5.73	3.68	0	22
Total retail sales of consumer goods, as % of GDP	2,951	37.1	28.2	2.86	98.71
Rural Tax-for-Fee reform	3,452	0.73	0.44	0	1
Fiscally supported population, as % of total population	3,497	3.38	1.15	0.55	12.01
Log (age of the city party secretary)	3,371	3.93	0.09	1.61	4.25
Minority dummy for the city party secretary	3,383	0.92	0.27	0	1
Education of the city party secretary	3,318	4.66	0.82	1	7
Tenure of the city party secretary (years)	3,530	4.14	1.73	1	10
Natural growth rate of population (‰)	3,283	5.75	3.43	-3.35	20
School-age (0-14 years) population in 1999 (10,000 persons)	3,542	83.11	53.28	2.82	277.28

Table 3.2: The Fiscal Indiscipline Effect of the VFI: OLS and IV Estimations

	OLS		IV	
	(1)	(2)	(3)	(4)
VFI	-0.058***	-0.070***	-0.130**	-0.103***
	(0.006)	(0.007)	(0.053)	(0.029)
Anderson-Rubin confidence interval			[-0.292, -0.032]	[-0.180, -0.032]
Economic controls	No	Yes	No	Yes
Political controls	No	Yes	No	Yes
City fixed effects	Yes	Yes	Yes	Yes
Province × Year fixed effects	Yes	Yes	Yes	Yes
IV first stage F-stat			15.69	11.34
Pr > F-stat			0.000	0.001
Underidentification LM statistic			11.400	8.853
LM statistic P-value			0.001	0.003
Adjusted R-squared	0.414	0.477	-0.295	-0.096
Observations	3,047	2,368	3,043	2,366
Number of cities	292	266	288	264

Notes: The dependent variable is the ratio of total own revenues to GDP. Economic control variables include *Log (real GDP per capita)*, *Log (population density)*, *Share of secondary sector*, *Share of tertiary sector*, *Fiscally supported population*, *Log (cultivation land per capita)*, *FDI/GDP*, *Log (highway)*, *Log(passengers)*, *total retail sales of consumer goods (as % of GDP)*, *Rural Tax-for-Fee reform*, *Number of cities*, and *Number of counties*. Political control variables include *Log (age of the city party secretary)*, *Square of Log (age of the city party secretary)*, *Minority dummy for the city party secretary*, *Education of the city party secretary*, and *Tenure of the city party secretary*. All regressions include city fixed effects and a full set of province-year fixed effects. Robust standard errors clustered at prefecture level are reported in parentheses.
** and *** denote the significance at the 5% and 1% level, respectively.

Table 3.3: IV Validity Tests

	IV first-stage results		IV exclusion condition	
	(1)	(2)	(3)	(4)
Change in the predicted school-age population (IV)	9.797***	7.520***	-0.716	-0.246
	(2.473)	(2.233)	(0.531)	(0.256)
VFI			-0.057***	-0.070***
			(0.006)	(0.007)
Economic controls	No	Yes	No	Yes
Political controls	No	Yes	No	Yes
City fixed effects	Yes	Yes	Yes	Yes
Province × Year fixed effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.742	0.775	0.415	0.477
Observations	3,043	2,366	3,047	2,368
Number of cities	288	264	292	266

Notes: The dependent variables in Columns (1)-(2) and (3)-(4) are VFI and the ratio to total own revenues to GDP, respectively. Economic control variables include *Log (real GDP per capita)*, *Log (population density)*, *Share of secondary sector*, *Share of tertiary sector*, *Fiscally supported population*, *Log (cultivation land per capita)*, *FDI/GDP*, *Log (highway)*, *Log(passengers)*, *total retail sales of consumer goods (as % of GDP)*, *Rural Tax-for-Fee reform*, *Number of cities*, and *Number of counties*. Political control variables include *Log (age of the city party secretary)*, *Square of Log (age of the city party secretary)*, *Minority dummy for the city party secretary*, *Education of the city party secretary*, and *Tenure of the city party secretary*. All regressions include city fixed effects and a full set of province-year fixed effects. Robust standard errors clustered at prefecture level are reported in parentheses.
*** denotes the significance at the 1% level.

Table 3.4: IV Estimations: the Effect of the VFI on Local Taxes

Dependent variable	Local tax revenues (% of GDP)	Business tax revenue (% of GDP)	Urban infrastructure tax revenue (% of GDP)	Agriculture tax revenue (% of GDP)
	(1)	(2)	(3)	(4)
VFI	-0.048***	-0.001	-0.008**	-0.046**
	'(0.015)	(0.011)	(0.004)	(0.011)
IV first stage F-stat	9.825	11.34	11.06	9.475
Underidentification LM statistic	7.966	8.853	8.764	7.707
LM statistic P-value	0.005	0.003	0.003	0.006
Adjusted R-squared	-0.333	-0.08	-0.323	-1.042
Observations	1,935	2,365	1,950	1,942
Number of cities	263	264	263	263

Notes: Given data availability, the year coverage for the business tax is 1999-2009, and the year coverage for the urban infrastructure tax and the agriculture tax is 1999-2007. All regressions include economic and political control variables, city fixed effects and a full set of province-year fixed effects. Economic and political controls are the same as that in Table 3.2. Robust standard errors clustered at prefecture level are reported in parentheses.
** and *** denote the significance at the 5% and 1% level, respectively.

Table 3.5: IV Estimations: the Effect of the VFI on Shared Taxes

Dependent variable	Shared tax	VAT	Corporate tax	Effective corporate income tax rate (%)	
				Firms established before 2002	Firms established after 2002
	(1)	(2)	(3)	(4)	(5)
VFI	-0.004	-0.001	0.003	-0.542	-0.049
	(0.013)	(0.011)	(0.008)	(0.347)	(0.352)
IV first stage F-stat	13.610	11.340	14.130	10.810	13.630
Underidentification LM statistic	10.140	8.853	10.480	8.520	10.330
LM statistic P-value	0.001	0.003	0.001	0.004	0.001
Adjusted R-squared	-0.133	-0.143	-0.311	-0.508	-0.405
Observations	2,199	2,365	2,200	1,921	1,375
Number of cities	264	264	264	263	259

Notes: Given data availability, the year coverage for the VAT and corporate income tax is 1999-2009 and 2000-2009, respectively. The dependent variables in Columns (4)-(5) are the weighted effective corporate income tax rate at prefecture-city level for firms that were established before and after 2002, respectively. All regressions include economic and political control variables, city fixed effects and a full set of province-year fixed effects. Economic and political controls are the same as that in Table 3.2. Robust standard errors clustered at prefecture level are reported in parentheses.

Table 3.6: IV Estimations: the Effect of the VFI on Extra-Budgetary Revenues

Dependent variable	Extra-budgetary revenues (% of GDP)	Industry and transportation sector fund (% of GDP)	Agriculture sector fund (% of GDP)	Land conveyance fee (% of GDP)
	(1)	(2)	(3)	(4)
VFI	-0.048	0.002	0.001	-0.020
	(0.031)	(0.003)	(0.002)	(0.019)
IV first stage F-stat	20.450	5.680	8.430	19.770
Underidentification LM statistic	12.850	4.811	6.799	10.860
LM statistic P-value	0.000	0.028	0.009	0.001
Adjusted R-squared	-0.202	-0.545	-0.391	-0.441
Observations	2,288	1,341	1,383	1,577
Number of cities	264	243	240	260

Notes: All regressions include economic and political control variables, city fixed effects and a full set of province-year fixed effects. Economic and political controls are the same as that in Table 3.2. Robust standard errors clustered at prefecture level are reported in parentheses.
** and *** denote the significance at the 5% and 1% level, respectively.

Appendix A

Appendix of Chapter 1

A.1 Anti-Poverty Evolution in China

The reform and open-up policy (launched in 1978), though viewed as the engine of "China's miracle," opened a Pandora's box of wealth stratification among over one billion people, due to its strategy of (geographically) uneven progress of economic growth. Since then, China's poverty has been closely linked to geography, wherein the disparity in poverty incidence was huge between opulent coastal and remote inland areas. Being aware of the potential social instability induced by regional poverty, the Chinese government launched its state-led anti-poverty campaign in 1986. To oversee this campaign, the State Council (or Chinese central government) established the Leading Group for Economic Development in Poor Areas (Leading Group hereafter), which was a specialized inter-ministerial anti-poverty institution that consisted of all the ministers whose duties were associated with poverty alleviation.[1] As illustrated in Figure A.1 in the Appendix, from 1986 to 2020, according to the change of the Leading Group's preferred targets, China's anti-poverty campaign could be divided into two phases.

Phase I: Regional Level In response to emerging regional poverty, the aim of phase I is to alleviate the impoverished through regional development. Considering that the poor were concentrated in 18 large connected areas in 1986, the Leading

[1]To enforce the leadership of the Leading Group and enhance anti-poverty efficiency, special offices of poverty alleviation were further established in both provincial and county levels.

Group first targeted counties and enacted the National Poor Counties policy (NPC policy hereafter), which is the largest regional targeted anti-poverty program in the developing world. To achieve its goals, the NPC policy introduced three major interventions to the designated counties: (i) credit assistance – subsidized loans disbursed by Agricultural Banks of China; (ii) block transfer payment – budgetary grants transmitted by the Ministry of Finance; and (iii) job creation – the Food-for-Work project provided by the State Planning Commission. The NPC policy was first launched in 1986, and 328 counties were involved in the first wave (1986-1993).[2] However, it was then criticized for its heavy political compromises in the selection procedure of the designated counties (Park et al., 2002). To address this issue, the Leading Group started the second wave (1994-2000), also known as the 8-7 plan,[3] adopting a more rigid selection standard. After that, the NPC program continued with a renewed list, yet the favored anti-poverty target has gradually switched to the more micro-level units – villages.[4]

At the end of the 8-7 plan, given the fact that the impoverished resided more sporadically, the linkage of poverty coverage by the NPC program became considerable (Ravallion and Chen, 2005).[5] To enhance targeting effectiveness, the Leading Group enacted the world's largest village-targeted anti-poverty program in 2001 – a flagship poverty alleviation program (Park and Wang, 2010). This program identified 148,000 National Poor Villages, covering 80% of the total poverty population. Each designated village can decide whether and how to design and implement its

[2]258 counties were initially designated as NPCs in 1986, whose rural net income per capita should be below 150 yuan in 1985; and 70 pastoral and semi-pastoral counties were further included in 1988.

[3]The 8-7 Plan means to lift the majority of the remaining 80 million rural poor within 7 years.

[4]Please respectively refer to Park et al. (2002), Meng (2013), and Qin and Chong (2018) for detailed descriptions of selection procedures and implementation of each wave.

[5]In the second wave (1994-2000), in order to target more people in poverty, the Leading Group extended the volume of NPC list to 592 counties. However, according to official statistics, the 148,000 National Poor Villages were located in 1,861 counties, indicating the low targeting effectiveness of the NPC program.

own anti-poverty plan through a democratic process with the full participation of its village members. In principle, the plan should be development-oriented, aiming at improving local infrastructural conditions and/or social services (Gao et al., 2001). The former three major interventions in the NPC program pledged to additionally finance this program. By 2009, 108,4000 designated villages had participated in the program, among which 38,4000 were located in China's most impoverished areas – revolutionary based areas (*Laoqu*), minority autonomous areas (*Shaoqu*), and inland frontier areas (*Bianqu*).

Phase II: Individual Level The targeting priority switched to individual-level in the 2010s. Despite that the success of phase I was astonishing – the poverty population sharply decreased from 125 million in 1985 to 17 million in 2010, the validity of regional development deteriorated over time. The rationale of phase I is that the poverty population is highly concentrated, thus regional development can alleviate the poor through the "trickle-down effect." However, on the one hand, regional development (even targeting down to the village level) cannot capture the remaining poor due to their more sporadic location; on the other hand, the prior efforts were claimed to be pro-rich (Park and Wang, 2010). In recognition of this, in 2013, President Xi announced a renewed target to individuals (or households). In Xi's refined anti-poverty theory, he highlighted three aspects: (i) identification – each impoverished individual should be identified and registered in the national anti-poverty system; (ii) poverty alleviation – local governments are required to provide effective anti-poverty programs to the registered individuals based on their causes of poverty and personal characteristics; and (iii) management – to avoid elite capture and enhance the effectiveness, the program implementation and fund disbursement should be unanimously managed by the system.

In addition to Xi's design, to meet the commitment (by the central government)

that eradicating extreme poverty by the end of 2020, the Leading Group encouraged corporate involvement as a supplement to state-led efforts. Among all corporates, the listed firms were the pillar. On September 8, 2016, the China Securities Regulatory Commission issued a document titled *Opinions of the China Securities Regulatory Commission on Giving Play to the Role of the Capital Market in Serving the National Poverty Alleviation Strategy* (*Opinions* hereafter). This document clarified the role of listed firms in China's national poverty-alleviation strategy. First, all firms should disclose their social responsibility fulfillment on anti-poverty activities in their annual reports. Before 2016, poverty alleviation was a *de facto* political assignment for state-owned firms[6] and not formally considered as a type of social responsibility for private firms. Immediately following this document, the Shanghai Stock Exchange and the Shenzhen Stock Exchange enacted unified formats of information disclosure, including annual achievements (e.g., anti-poverty sites, monetary and physical investment, and the number of beneficiaries), and follow-up plans. Second, mergers and acquisitions and restructuring of enterprises in poverty-stricken areas are encouraged. The examination and approval process is required to be accelerated. Third, priorities should be given to the employment of university graduates from poverty-stricken areas and registered poor.

Apparently, the *Opinions* is a general guidance at the central level. To further induce firms conducting anti-poverty investment, some provincial governments then provided detailed supports. As depicted in Figure A·1 in the Appendix, there were eight provinces that formulated encouraging policies between 2016 and 2020. Among those provinces: (i) Guizhou province and Shanxi province reiterated the importance of the Opinions without additional rewards; (ii) Gansu province, Hunan province, and Guangxi autonomous region provided fiscal subsidies to firms based on the number

[6]Compulsory anti-poverty tasks were first assigned to state-owned firms in 1994 as a part of the 8-7 plan

registered poor they hired; (iii) Shandong province provided subsidies to agricultural firms involved in anti-poverty; (iv) in addition to employment subsidies and agricultural subsidies, Anhui province and Sichuan province offered financial credit and tax discount to qualified firms.

A.2 Additional Figures

Figure A·1: Provinces with Local Encouraging Policies for Firms Involvement in TPA

Notes: Provinces that enacted local supporting policies to encourage firm involvement in TPA are shown in dark grey. Despite the fact that Inner Mongolia did not announce policies at the provincial level, prefectural policies were enacted by its three cities, which are shown in light grey.

A.3 Additional Tables

Table A.1: China's Anti-Poverty Evolution since the Reform and Opening-Up

China's anti-poverty evolution	*Phase I: Regional Level*
	County targeted (1986-2000)
	Village targeted (2001-2011)
	Phase II: Individual Level
	Targeted Poverty Alleviation program (2013-2020)
	Firm involvement (2016-2020)

Notes: The table illustrates the anti-poverty evolution of China since the reform and open-up (1978). In phase I, the priority was to narrow the regional imbalance; and in phase II the targets are precisely designated to individuals. The duration of each (sub)stage is in parenthesis.

Table A.2: Dynamic Effects of the IV on Total New Loans

	log (1+Total New Loans)
IV × Year=2010	-0.019
	(0.040)
IV × Year=2011	-0.083*
	(0.042)
IV × Year=2012	-0.043
	(0.033)
IV × Year=2013	0.054
	(0.034)
IV × Year=2014	0.019
	(0.025)
IV × Year=2015	0.024
	(0.020)
IV × Year=2016	0.045**
	(0.021)
IV × Year=2017	0.053***
	(0.018)
IV × Year=2018	0.044**
	(0.022)
IV × Year=2019	0.051**
	(0.022)
Firm FE	Y
Year FE	Y
Province Trend	Y
Industry Trend	Y
Mean of Dependent Variable	10.853
No. of Observations	10,136
Adjusted R^2	0.610

Notes: This table reports the dynamic effects of the IV (share pf NPC managers) on total new loans over the sample period. Dependent variable is the logarithm of total new loans approved in a year. Standard errors are clustered at the firm level.
*, **, and *** denote the significance at the 10%, 5%, and 1% level, respectively.

Table A.3: IV and Firm Productivity

	log (TFP), OP approach				log (TFP), LP approach			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of NPC Managers	-0.092	-0.172	-0.187	-0.131	-0.070	-0.064	-0.044	-0.016
	(0.347)	(0.314)	(0.347)	(0.323)	(0.121)	(0.121)	(0.115)	(0.115)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Province × Year FE		Y				Y		
Industry × Year FE			Y				Y	
Province Trend				Y				Y
Industry Trend				Y				Y
No. of Observations	10,718	10,715	10,718	10,715	10,714	10,711	10,714	10,711
Adjusted R^2	0.906	0.906	0.911	0.909	0.775	0.776	0.784	0.781

Notes: The table reports OLS results of firm productivity on the instrument. The dependent variable in columns (1)-(4) is the logarithm of firm TFP calculated using the method developed by Olley and Pakes (1996), while in columns (5)-(8), I use the Levinsohn and Petrin's (2003) method to estimate firm TFP. *Share of NPC Managers* is the share of senior managers who were born from National Poor Counties (NPC) in total senior managers. All regressions control for log(*Asset per Employee*), *R&D Investment*, log(*Sales*), and *ROA*. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.

Table A.4: IV and Business Entertainment Expenses

	log (Entertainment Expenses)				Entertainment Expenses/Revenue (%)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of NPC Managers	-4.406	-5.226	-3.756	-5.369	-1.805	-2.089	-1.453	-1.734
	(4.680)	(4.335)	(4.651)	(4.555)	(1.315)	(1.468)	(1.146)	(1.318)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Province × Year FE		Y				Y		
Industry × Year FE			Y				Y	
Province Trend				Y				Y
Industry Trend				Y				Y
No. of Observations	10,713	10,710	10,713	10,710	10,713	10,710	10,713	10,710
Adjusted R^2	0.815	0.819	0.816	0.815	0.002	-0.018	0.003	-0.007

Notes: The table reports OLS results of firm business entertainment expenses on the instrument. The dependent variable in columns (1)-(4) is the logarithm of total amount of firm business entertainment, while in columns (5)-(8), the dependent variable is the ratio of entertainment expenses to total revenue. *Share of NPC Managers* is the share of senior managers who were born from National Poor Counties (NPC) in total senior managers. All regressions control for log(*Asset per Employee*), *R&D Investment*, log(*Sales*), and *ROA*. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.

Table A.5: Dynamic Effects of TPA on Total New Loans

	log (1+Total New Loans)
TPA Participation (-6)	-0.067
	(0.139)
TPA Participation (-5)	-0.099
	(0.129)
TPA Participation (-4)	-0.078
	(0.106)
TPA Participation (-3)	-0.058
	(0.100)
TPA Participation (-2)	-0.010
	(0.077)
TPA Participation (-1)	0.001
	(0.068)
TPA Participation (+1)	0.124*
	(0.066)
TPA Participation (+2)	0.189**
	(0.077)
TPA Participation (+3)	0.219**
	(0.108)
Firm FE	Y
Year FE	Y
Province Trend	Y
Industry Trend	Y
Mean of Dependent Variable	10.853
No. of Observations	10,293
Adjusted R^2	0.607

Notes: This table reports the results from the regression (1.2). Dependent variable is the logarithm of total new loans approved in a year. The estimate for the year before the TPA participation ($t = 0$) is normalized to zero. All estimates can be interpreted as changes in new loans compared to the first year that firms participated in TPA, conditional on firm fixed effects, year fixed effects, and industry and province time trends. The regression controls for log(*Asset per Employee*), *R&D Investment*, log(*Sales*), and *ROA*. Standard errors are clustered at the firm level. * and ** denote the significant at the 10% and 5% level, respectively.

Table A.6: OLS Results: Effects of TPA Intensity on New Loans

	log (1+Total New Loans)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log (1+TPA Spending)	0.037***	0.040***	0.039***	0.037***				
	(0.011)	(0.012)	(0.011)	(0.011)				
log (1+TPA Population)					0.033***	0.038***	0.034***	0.035***
					(0.011)	(0.012)	(0.011)	(0.012)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Province × Year FE		Y				Y		
Industry × Year FE			Y				Y	
Province Trend				Y				Y
Industry Trend				Y				Y
Mean of Dependent Variable	10.853	10.853	10.853	10.853	10.853	10.853	10.853	10.853
No. of Observations	10,682	10,680	10,682	10,680	10,272	10,270	10,272	10,270
Adjusted R^2	0.614	0.615	0.619	0.616	0.615	0.616	0.620	0.616

Notes: The table reports the OLS results of effects of the TPA intensity on total new loans. The dependent variable in all specifications is $\log(1 + Total\ New\ Loans)$. *TPA Spending* is the amount of money that a firm spends on the TPA program in a year; *TPA Population* is the number of poor people who benefit from a given firm's specific anti-poverty policies in a year. All regressions control for $\log(Asset\ per\ Employee)$, $R\&D\ Investment$, $\log(Sales)$, and ROA. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.

*** denotes the significance at 1% level.

Table A.7: Effects of TPA on Loan Outcomes: Loan-level Evidence

	log (1+Loans)			Interest Rate			Loan Maturity		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
TPA Participation	0.070**	0.092***	0.080**	-0.026**	-0.052***	-0.062***	0.201	-1.552*	-2.017**
	(0.031)	(0.032)	(0.032)	(0.012)	(0.013)	(0.014)	(0.759)	(0.849)	(0.818)
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Province × Year FE		Y	Y		Y	Y		Y	Y
Industry × Year FE		Y	Y		Y	Y		Y	Y
Bank × Province FE			Y			Y			Y
Bank × Industry FE			Y			Y			Y
Mean of Dependent Variable	9.327	9.327	9.327	5.118	5.118	5.118	18.440	18.440	18.440
No. of Observations	41,624	40,726	39,779	19,166	18,757	18,053	19,154	18,747	18,043
Adjusted R^2	0.428	0.437	0.491	0.902	0.907	0.912	0.311	0.357	0.473

Notes: The table reports the OLS results of effects of the TPA participation on loan outcomes using loan-level data. All regressions control for log(*Asset per Employee*), *R&D Investment*, log(*Sales*), and *ROA*. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.

*, **, and *** denote the significance at the 10%, 5%, and 1% level, respectively.

Table A.8: OLS Results: State-Owned Banks v.s. Other Financial Institutions

	log (1+Loans from State-Owned Banks)				log (1+Loans from Other Institutions)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TPA Participation	0.250	0.201	0.295*	0.181	0.198	0.167	0.246	0.303
	(0.168)	(0.172)	(0.170)	(0.169)	(0.204)	(0.213)	(0.207)	(0.207)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Province × Year FE		Y				Y		
Industry × Year FE			Y				Y	
Province Trend				Y				Y
Industry Trend				Y				Y
Mean of Dependent Variable	9.421	9.421	9.421	9.421	3.824	3.824	3.824	3.824
No. of Observations	10,720	10,718	10,720	10,718	10,720	10,718	10,720	10,718
Adjusted R^2	0.391	0.401	0.392	0.398	0.474	0.477	0.480	0.479

Notes: The table reports the OLS results of effects of the TPA participation on loans across different financial institutions. The dependent variable in Column (1)-(4) is $\log(1+Loans\ from\ State-Owned\ Banks)$, while in Column (5)-(9) is $\log(1+Loans\ from\ Other\ Institutions)$. *TPA Participation* is a firm-year indicator variable which equals one if the firm participates in the Targeted Poverty Alleviation program. All regressions control for $\log(Asset\ per\ Employee)$, *R&D Investment*, $\log(Sales)$, and *ROA*. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.

* denotes the significance at the 10% level.

Table A.9: OLS Results: Subsample without Preferential Borrowing Policies

	log (1+Total New Loans)			
	(1)	(2)	(3)	(4)
TPA Participation	0.171***	0.178***	0.187***	0.174***
	(0.059)	(0.063)	(0.058)	(0.060)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Province × Year FE		Y		
Industry × Year FE			Y	
Province Trend				Y
Industry Trend				Y
Mean of Dependent Variable	10.853	10.853	10.853	10.853
No. of Observations	9,975	9,973	9,975	9,973
Adjusted R^2	0.612	0.613	0.617	0.614

Notes: The table reports the OLS results of effects of the TPA participation on total new loans using the sample which excluded all firms in Sichuan and Anhui provinces where exists preferential policies regarding firm borrowing. The dependent variable in all specifications is $\log(1+Total\ New\ Loans)$. *TPA Participation* is a firm-year indicator variable which equals one if the firm participates in the Targeted Poverty Alleviation program. All regressions control for $\log(Asset\ per\ Employee)$, $R\&D\ Investment$, $\log(Sales)$, and ROA. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.
*** denotes the significance at the 1% level.

Table A.10: OLS Results: Effects of TPA on Firm Profit

	ROA (%)			log (Market Value)		
	(1)	(2)	(3)	(4)	(5)	(6)
TPA Participation	0.633**			0.081***		
	(0.305)			(0.022)		
log (1+TPA Spending)		0.071			0.021***	
		(0.054)			(0.005)	
log (1+TPA Population)			0.084			0.020***
			(0.057)			(0.005)
Controls	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Province Trend	Y	Y	Y	Y	Y	Y
Industry Trend	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable	2.356	2.356	2.356	22.416	22.416	22.416
No. of Observations	10,071	10,030	9,628	10,080	10,039	9,636
Adjusted R^2	0.291	0.290	0.289	0.842	0.842	0.841

Notes: The table reports the OLS results of effects of the TPA participation on firm profits. *TPA Participation* is a firm-year indicator variable which equals one if the firm participates in the Targeted Poverty Alleviation program. *TPA Spending* is the amount of money that a firm spends on the TPA program in a year; *TPA Population* is the number of poor people who benefit from a given firm's specific anti-poverty policies in a year. All regressions control for log(*Asset per Employee*), *R&D Investment*, log(*Sales*), and *ROA*. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.
** and *** denote the significance at the 5% and 1% level, respectively.

Table A.11: OLS Results: Effects of TPA on Sales and Costs

	log (Sales)			log (Costs)		
	(1)	(2)	(3)	(4)	(5)	(6)
TPA Participation	0.010			-0.013		
	(0.016)			(0.017)		
log (1+TPA Spending)		0.004			0.001	
		(0.003)			(0.003)	
log (1+TPA Population)			0.006*			0.003
			(0.003)			(0.003)
Controls	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Province Trend	Y	Y	Y	Y	Y	Y
Industry Trend	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable	21.448	21.448	21.448	21.100	21.100	21.100
No. of Observations	10,064	10,023	9,622	10,078	10,037	9,634
Adjusted R^2	0.937	0.937	0.936	0.940	0.940	0.939

Notes: The table reports the OLS results of effects of the TPA participation on firm sales and costs. *TPA Participation* is a firm-year indicator variable which equals one if the firm participates in the TPA program. *TPA Spending* is the amount of money that a firm spends on the TPA program in a year; *TPA Population* is the number of poor people who benefit from a given firm's specific anti-poverty policies in a year. All regressions control for log(*Asset per Employee*), *R&D Investment*, log(*Sales*), and *ROA*. FE stands for "fixed effects." Robust standard errors clustered at firm level are reported in parentheses.
* denotes the significance at the 10% level.

Appendix B

Appendix of Chapter 3

B.1 A Simple Model to Characterize Hypothesis 1

Suppose a local government's expenditure (E) is financed by the fiscal transfers (TR) and local own revenues (R), so that $E = TR + R$. The benefit of local government expenditure is $U(E)$ with $U(0) = 0$, $U^{'} > 0$, and $U^{''} < 0$. The actual cost of expenditure is $C(TR + R)$ with $C(0) = 0$, $C^{'} > 0$, and $C^{''} > 0$. The perceived cost of financing expenditure is lower than the actual cost in a way that local governments only take into account a share α (which is usually their share of population) of the burden of fiscal transfers, while considering the entire burden of their own tax revenues. Thus, the object function and maximization problem for local government is

$$\max \quad U(E) - C(\alpha TR + R) \tag{B.1}$$

Thus, the perceived marginal cost of raising funds from intergovernmental transfers is $\alpha C^{'}(\alpha TR + R)$, which is lower than the marginal cost of raising funds from own tax revenues, that is $C^{'}(\alpha TR + R)$. The divergence between real and perceived cost becomes wider when VFI or fiscal transfer is high, as

$$\frac{\partial[C^{'}(\alpha TR + R) - \alpha C^{'}(\alpha TR + R)]}{\partial TR} = (1 - \alpha)\alpha C^{''}(\alpha TR + R) > 0 \tag{B.2}$$

Since $E = TR + R$, we solve the maximization problem above and get the first-

order condition:

$$U^{'}(TR+R) = C^{'}(\alpha TR+R) \tag{B.3}$$

Then we define the reaction function of the local government to VFI or fiscal transfers as $R = R(TR)$. From the first order condition, we obtain that

$$\frac{\partial R}{\partial TR} = \frac{U^{''}(TR+R) - \alpha C^{''}(\alpha TR+R)}{C^{''}(\alpha TR+R) - U^{''}(TR+R)} < 0 \tag{B.4}$$

Together with equations B.2 and B.4, we can get our Hypothesis 1 that partial fiscal decentralization coupled with higher VFI levels undermine local fiscal discipline by discouraging local tax effort.

B.2 Additional Figures

Figure B·1: Predicted Change of School-Age Population (SAP) and Change of Employment

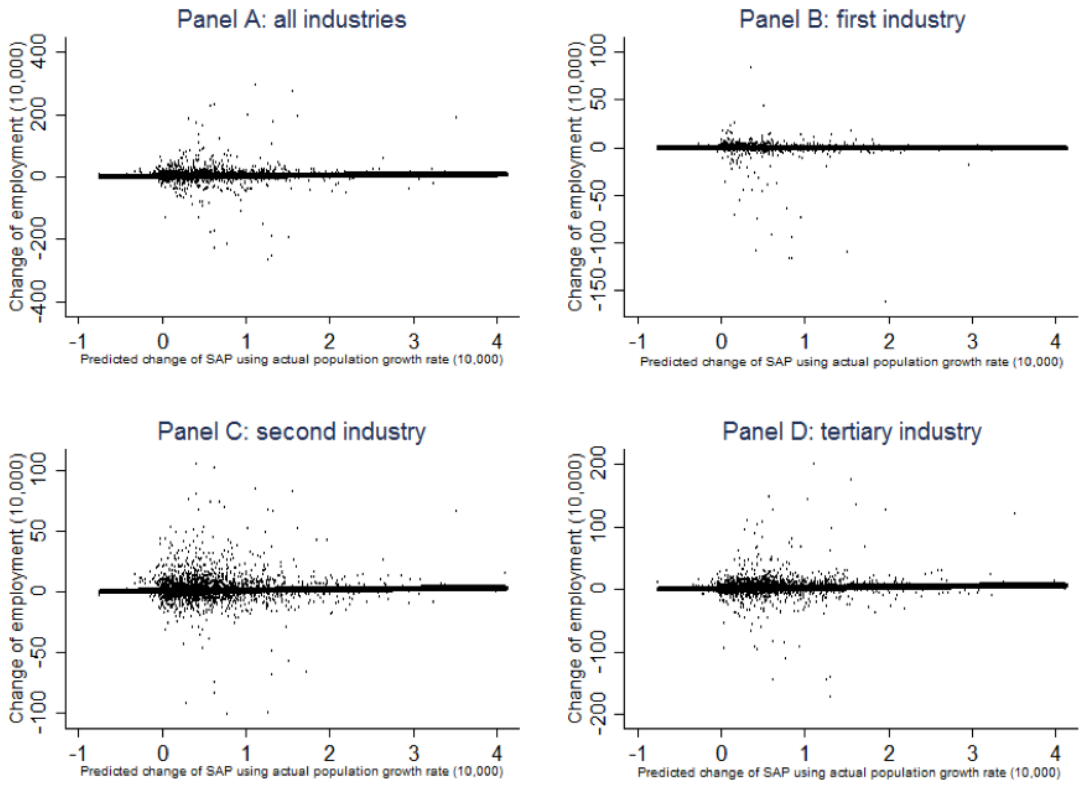
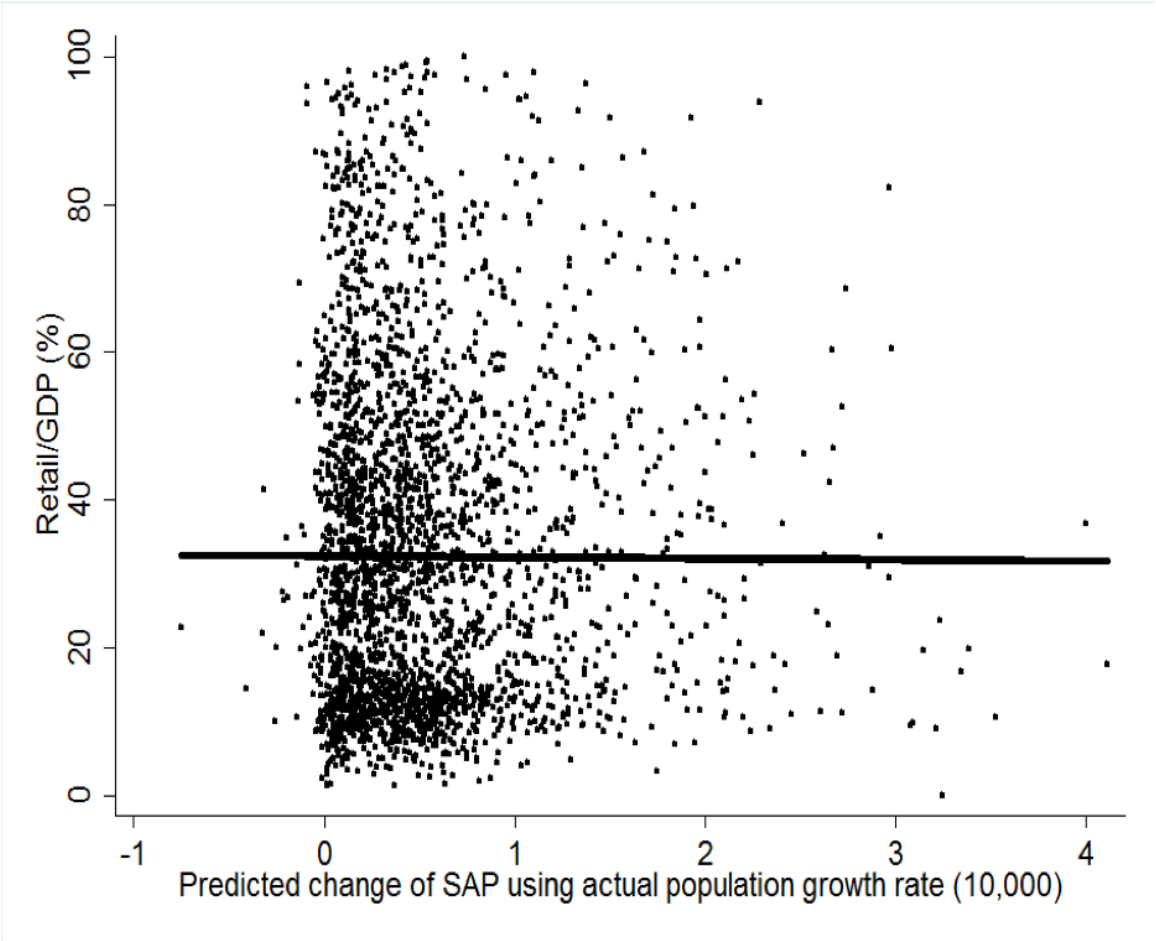


Figure B·2: Predicted Change of School-Age Population (SAP) and Total Retail Sales of Consumer Goods



B.3 Additional Tables

Table B.1: Average Fiscal Revenue Structure of Prefecture-City Governments, 1999-2009 (Million RMB)

Year	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009
Total own revenues	1215.61	1349.70	1615.93	1686.31	1966.24	2333.49	2947.51	3646.32	4763.19	5870.70	6718.42
Value-added Tax	213.86	243.08	284.70	327.10	382.35	345.57	556.52	674.30	835.39	983.88	1032.53
Corporate income tax	-	201.87	316.25	164.65	149.44	206.16	266.97	341.13	491.05	603.23	614.18
Business tax	258.92	288.55	327.40	409.53	491.42	620.98	726.31	892.15	1137.46	1331.61	1650.38
Urban infrastructure tax	94.72	89.22	97.50	120.00	140.23	171.54	203.61	242.44	297.87	-	-
Agriculture tax	119.37	125.77	129.94	190.61	226.99	218.91	214.18	-	-	-	-
Non-tax revenues	341.66	335.30	344.82	389.21	474.07	618.79	687.27	840.74	983.30	1273.15	1447.30

Table B.2: Variable Description and Data Source

Variables	Definition	Source
Total own revenues (% of GDP)	Ratio of total own revenues to GDP	Prefecture, City, and County Public Finance Statistics
Local tax revenues (% of GDP)	Ratio of local tax revenues to GDP	Prefecture, City, and County Public Finance Statistics
Business tax revenue (% of GDP)	Ratio of business tax revenue to GDP	Prefecture, City, and County Public Finance Statistics
Agriculture tax revenue (% of GDP)	Ratio of agriculture tax revenue to GDP	Prefecture, City, and County Public Finance Statistics
Urban infrastructure tax revenue (% of GDP)	Ratio of urban infrastructure tax revenue to GDP	Prefecture, City, and County Public Finance Statistics
Shared tax revenues (% of GDP)	Ratio of shared tax revenues to GDP	Prefecture, City, and County Public Finance Statistics
VAT revenue (% of GDP)	Ratio of value-added tax revenue to GDP	Prefecture, City, and County Public Finance Statistics
Corporate income tax revenue (% of GDP)	Ratio of corporate income tax revenue to GDP	Prefecture, City, and County Public Finance Statistics
Extra-budgetary revenues (% of GDP)	Ratio of total extra-budgetary revenues to GDP	Prefecture, City, and County Public Finance Statistics
Industry and transportation sector fund (% of GDP)	Ratio of industry and transportation sector fund to GDP	Prefecture, City, and County Public Finance Statistics
Education sector fund (% of GDP)	Ratio of education sector fund to GDP	Prefecture, City, and County Public Finance Statistics
Agriculture sector fund (% of GDP)	Ratio of agriculture sector fund to GDP	Prefecture, City, and County Public Finance Statistics
Land conveyance fee (% of GDP)	Ratio of land conveyance fee GDP	Prefecture, City, and County Public Finance Statistics
Log (real GDP per capita)	Log of real GDP per capita	China Statistical Yearbook for Regional Economy
Log (population density)	Total population divided by the area (log)	China Statistical Yearbook for Regional Economy
Share of secondary sector	Ratio of value-added of secondary sector to GDP	Prefecture, City, and County Public Finance Statistics
Share of tertiary sector	Ratio of value-added of tertiary sector to GDP	Prefecture, City, and County Public Finance Statistics
Log (cultivation land per capita)	Log of cultivation land per capita	China Statistical Yearbook for Regional Economy
FDI/GDP (%)	Ratio of foreign direct investment to GDP	China Statistical Yearbook for Regional Economy
Log (highway)	Log of the length of highways	China Statistical Yearbook for Regional Economy
Log (passengers)	Log of the total number of annual passengers	China Statistical Yearbook for Regional Economy
Number of cities	The number of cities in a province	China Statistical Yearbook
Number of counties	The number of counties in a city	China Statistical Yearbook
Total retail sales of consumer goods, as % of GDP	The ratio of total retail sales of consumer goods to GDP	China Statistical Yearbook for Regional Economy
Rural Tax-for-Fee reform	=1 if the city has implemented the rural Tax-for-Fee reform	Official documents from provincial governments
Fiscally supported population	Ratio of fiscally supported population to total population	Prefecture, City, and County Public Finance Statistics
Log(age of the city party secretary)	Log of the age of the party secretary of the city	Public information from the prefecture-city government's bulletins
Minority dummy for the city party secretary	=1 if the party secretary of the city is from ethnic minorities	Public information from the prefecture-city government's bulletins
Education of the city party secretary	The level of the education of the party secretary of the city. 1:technical secondary school; 2: high school; 3: junior college; 4: bachelor degree; 5: master degree; 6: doctorate; 7: post-doc	Public information from the prefecture-city government's bulletins
Natural growth rate of population	Natural population growth rate (‰)	China Statistical Yearbook for Regional Economy
School-age population	School-age (0-14 years) population in 1999 (10,000 persons)	Provincial Tabulation on the 2000 Population Census

Table B.3: Effect of the VFI on Total Tax and Non-Tax Revenues

Dependent variable	Total tax revenue		Total non-tax revenues	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
VFI	-0.044***	-0.073**	-0.026***	-0.042**
	(0.005)	(0.029)	(0.007)	(0.021)
IV first stage F-stat		11.430		11.040
Underidentification LM statistic		8.902		8.609
LM statistic P-value		0.003		0.003
Adjusted R-squared	0.517	-0.169	0.146	-0.240
Observations	2,361	2,359	2,357	2,355
Number of cities	266	264	266	264

Notes: All regressions include economic and political control variables, city fixed effects and a full set of province-year fixed effects. Economic and political controls are the same as that in Table 3.2. Robust standard errors clustered at prefecture level are reported in parentheses.
** and *** denote the significance at the 5% and 1% level, respectively.

Table B.4: Robustness Checks: Alternative Measures of VFI and Fiscal Indiscipline

Dependent variable	Total own revenues	Local tax revenues	Shared tax revenues	Extra-budgetary revenues
	(1)	(2)	(3)	(4)
Panel A: Alternative measure of the VFI				
ET/Exp	-0.128***	-0.076***	-0.007	-0.053
	(0.039)	(0.027)	(0.016)	(0.040)
IV first stage F-stat	11.440	7.202	11.350	15.730
Underidentification LM statistic	9.387	6.601	9.078	11.180
LM statistic P-value	0.002	0.010	0.003	0.001
Adjusted R-squared	-0.560	-1.779	-0.172	-0.210
Observations	2,365	1,927	2,219	2,291
Number of cities	265	265	265	265
Panel B: Estimated tax efforts by the regression approach				
VFI	-0.021***	-0.029**	-0.005	-0.012
	(0.006)	(0.013)	(0.008)	(0.019)
IV first stage F-stat	23.220	15.920	25.790	37.920
Underidentification LM statistic	23.790	15.380	26.250	35.320
LM statistic P-value	0.000	0.000	0.000	0.000
Adjusted R-squared	-0.086	-0.435	-0.001	-0.171
Observations	2,368	1,937	2,201	2,290
Number of cities	264	263	264	264

Notes: Instrumental variable estimations are employed in the estimations. In Panel A, the ratio of equalization transfers to local government own spending (ET/Exp) is employed as an alternative measured of VFI. The dependent variables are ratio of different revenues to GDP, respectively. In Panel B, all measures of local tax effort are obtained by using the regression approach. All regressions include economic and political control variables, city fixed effects and a full set of province-year fixed effects. Economic and political controls are the same as that in Table 3.2. Robust standard errors clustered at prefecture level are reported in parentheses.

** and *** denote the significance at the 5% and 1% level, respectively.

Table B.5: Robustness Checks: Alternative Measures of Dependent Variables and the IV

Panel A: Log of (actual revenues) as the dependent variables

Dependent variable	Log(total own revenues)	Log(local tax revenues)	Log(shared tax revenues)	Log(extra-budgetary revenues)
	(1)	(2)	(3)	(4)
VFI	-0.019***	-0.029***	-0.000	-0.010
	(0.005)	(0.008)	(0.011)	(0.020)
IV first stage F-stat	11.340	9.825	13.610	20.450
Underidentification LM statistic	8.853	7.966	10.140	12.850
LM statistic P-value	0.003	0.005	0.001	0.000
Adjusted R-squared	0.386	0.02	-0.062	-0.095
Observations	2,366	1,935	2,199	2,288
Number of cities	264	263	264	264

Panel B: Effective Tax Rates as the Dependent Variables

Dependent variable	Total own revenues (%/GDP)	Local tax revenues (% of GDP)	Shared tax revenues (% of GDP)
	(5)	(6)	(7)
VFI	-0.352*	-0.104**	-0.248
	(0.181)	(0.049)	(0.151)
IV first stage F-stat	10.760	10.760	10.760
Underidentification LM statistic	8.516	8.516	8.516
LM statistic P-value	0.003	0.003	0.003
Adjusted R-squared	-0.827	-0.883	-0.641
Observations	1,941	1,941	1,941
Number of cities	263	263	263

Panel C: Alternative Measure of the Instrument

Dependent variable	Total own revenues (%/GDP)	Local tax revenues (% of GDP)	Shared tax revenues (% of GDP)	Extra-budgetary revenues (% of GDP)
	(8)	(9)	(10)	(11)
VFI	-0.118***	-0.050***	0.004	-0.038
	(0.031)	(0.015)	(0.012)	(0.030)
IV first stage F-stat	12.880	10.740	14.210	19.390
Underidentification LM statistic	10.080	8.993	10.610	13.100
LM statistic P-value	0.002	0.003	0.001	0.000
Adjusted R-squared	-0.144	-0.386	-0.229	-0.162
Observations	2,276	1,860	2,115	2,198
Number of cities	254	253	254	254

Notes: Instrumental variable estimations are employed in the estimations. All regressions include economic and political control variables, city fixed effects and a full set of province-year fixed effects. Economic and political controls are the same as that in Table 3.2. Robust standard errors clustered at prefecture level are reported in parentheses.

*, **, and *** denote the significance at the10%, 5%, and 1% level, respectively.

Table B.6: Robustness Checks: Other Potential Confounders

Dependent variable	Total own revenues (%/GDP)	Local tax revenues (% of GDP)	Shared tax revenues (% of GDP)	Extra-budgetary revenues (% of GDP)
	(1)	(2)	(3)	(4)
Panel A: Subsample with city leader under 53 years old				
VFI	-0.114***	-0.063***	-0.008	-0.031
	(0.027)	(0.016)	(0.013)	(0.031)
IV first stage F-stat	11.220	8.000	13.520	27.660
Adjusted R-squared	-0.130	-0.601	-0.100	-0.168
Observations	1,683	1,368	1,573	1,626
Number of cities	189	188	189	189
Panel B: VAT reform in 2004				
VFI	-0.103***	-0.048***	-0.01	-0.044
	(0.030)	(0.016)	(0.013)	(0.031)
IV first stage F-stat	11.510	9.820	14.600	21.640
Adjusted R-squared	-0.085	-0.227	-0.081	-0.167
Observations	2,087	1,694	1,940	2,011
Number of cities	230	229	230	230
Panel C: Income tax and agriculture tax reforms in 2006				
VFI	-0.125***	-0.074***	-0.002	-0.028
	(0.041)	(0.022)	(0.017)	(0.034)
IV first stage F-stat	9.304	9.132	15.270	14.450
Adjusted R-squared	-0.282	-1.175	-0.235	-0.073
Observations	1,711	1,696	1,545	1,672
Number of cities	262	262	262	262
Panel D: Additional controls				
VFI	-0.100***	-0.042***	-0.001	-0.044
	(0.026)	(0.014)	(0.012)	(0.030)
IV first stage F-stat	11.300	9.447	14.590	21.010
Adjusted R-squared	-0.092	-0.149	-0.184	-0.174
Observations	2,237	1,829	2,087	2,167
Number of cities	261	260	261	261

Notes: Instrumental variable estimations are employed in the estimations. In Panel A, we restrict our sample to those cities whose leaders (party secretaries) are under the age of 53. In Panel B, we exclude all prefecture-cities in those three Northeastern provinces that were affected by the 2004 VAT reform from the sample. In Panel C, we restrict our sample to only include observations prior to 2006 (i.e.,1999-2005). In Panel D, additional controls include fixed assets investment (as % of GDP), the share of working population in total population, and log (number of firms in the manufacturing sector). All regressions include economic and political control variables, city fixed effects and a full set of province-year fixed effects. Economic and political controls are the same as that in Table 3.2. Robust standard errors clustered at prefecture level are reported in parentheses.

*** denotes the significance at 1% level.

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