Contents lists available at ScienceDirect



Economics of Education Review

journal homepage: www.elsevier.com/locate/econedurev

Intergovernmental transfer under heterogeneous accountabilities: The effects of the 2006 Chinese Education Finance Reform ‡



Yanqing Ding^a, Fengming Lu^{b,c}, Xiaoyang Ye^{*,d}

^a Institute of Economics of Education, Graduate School of Education, Peking University

^b Paul and Marcia Wythes Center on Contemporary China and Department of Politics, Princeton University

^c Department of Political and Social Change, Coral Bell School of Asia Pacific Affairs, Australian National University

^d Woodrow Wilson School of Public and International Affairs, Princeton University. Robertson Hall, Princeton, NJ 08544-1013 United States

ABSTRACT

ARTICLE INFO

While intergovernmental transfers are widely used in improving local education, how local governments in nondemocracies allocate fiscal transfers, given they are not electorally accountable, remains unclear. We study the impacts of the 2006 Chinese Education Finance Reform, one of the world's largest education transfer grants, on public school spending. By comparing 1600 Chinese counties that were treated differently in timing and matching ratios, we show natural experimental evidence on how heterogeneous top-down and bottom-up accountabilities affect the allocation of transfer grants. On average, intergovernmental transfers did not increase the total spending levels of local public schools. The causal mechanism is that the transfers crowded out preexisting local public education investments in extra-budgetary accounts that were not scrutinized and audited by upper-level governments. Heterogeneity analyses further demonstrate that the policy only improved public school spending in counties where public employees had greater means of holding local governments accountable.

Keywords: School finance Intergovernmental transfer Accountability Local government incentives

JEL classification: H52 H75 I22 I28

Low fiscal capacity and unequal endowments across localities often curtail quality and equality of public services, in both developed and developing countries. Intergovernmental transfers from the central government to fiscally constrained local governments, therefore, have been widely recommended as a solution to the inadequate provision of public goods (Card & Payne, 2002; Dixit & Londregan, 1998; Fernández & Rogerson, 1998; Murray, Evans, & Schwab, 1998; Musgrave, 1959; Oates, 1972; 1999). Empirical evidence on the effects of intergovernmental transfers on local education, primarily focusing on federal democracies, however, remains mixed and inconclusive (Campos & Hellman, 2005; Chyi & Zhou, 2014; Gordon, 2004; Hanushek, 2003; Hoxby, 2001; Hyman, 2017; Jackson, Johnson, & Persico, 2016; Lafortune, Rothstein, & Schanzenbach, 2018; Lindert, Skoufias, & Shapiro, 2006; Litschig & Morrison, 2013; Reinikka & Svensson, 2004). Studies of intergovernmental transfers in non-democracies are even more scarce.¹ Local governments in non-democracies are not electorally accountable to voters but appointed by their superiors. Although they are often informally accountable to local elites, it is difficult to observe levels of top-down and bottom-up accountabilities. Therefore, how heterogeneous accountabilities affect effectiveness of transfer grants remains unclear. Another challenge is the paucity of exogenous shocks and high-quality education finance data. Public policy decisions by nature, intergovernmental transfers are inevitably endogenous to unobserved political factors, which create biases in non-experimental analyses.

This paper estimates the causal effects of intergovernmental

^{*} We thank Xiaoyu Chen, Susan Dynarski, Max Gallop, Qiang Guo, Wei Ha, Brian Jacob, Herbert Kitschelt, Mingxing Liu, Xiaobo Lü, Brian McCall, Mariana Medina, Raegen Miller, Ana de la O, Rena Salayeva, Lori Taylor, Dashu Wang, Erik Wang, Rong Wang, Simon Weschle, Erik Wibbels, Feng Yang, Xufeng Zhu, and seminar participants at the 2015 MPSA Annual Conference, the 2015 AERA Annual Conference, the 2017 China Education Finance Conference, the 2018 AEFP Annual Conference, Duke University, Peking University, Tsinghua University, and University of Michigan for helpful comments and suggestions. The education finance data were provided by the Ministry of Education of China through the Institute of Economics of Education at Peking University. We are immensely grateful to Bingyu Sun, Yinduo Wu, and Jin Yang for excellent research assistance. Financial support from the National Natural Science Foundation of China (16JJD880001) is gratefully acknowledged. The opinions, findings, and conclusions expressed in this paper are those of the authors. All errors and omissions are our own.

* Corresponding author.

E-mail addresses: yqding@gse.pku.edu.cn (Y. Ding), fl8@princeton.edu (F. Lu), xiaoyang.ye@princeton.edu, yxy@umich.edu (X. Ye).

¹ For notable examples, see Meng (2013) and Lü (2015).

https://doi.org/10.1016/j.econedurev.2020.101985

Received 19 December 2018; Received in revised form 17 February 2020; Accepted 18 March 2020 Available online 22 May 2020

0272-7757/ © 2020 Elsevier Ltd. All rights reserved.

transfers on local public school spending, the major sector of public service managed by county-level governments in China, and investigates how heterogeneous accountabilities affect the policy treatment effects. We use a difference-in-differences (DID) research design that exploits the exogenous variations between central and western counties in the 2006 Chinese Education Finance Reform. One of the largest of its kind in the world, the reform has affected tens of millions of Chinese low-income students, providing a rare opportunity to identify average and heterogeneous treatment effects of intergovernmental transfers. Before 2006, Chinese county-level governments financed rural public schools in villages and towns on their own.² Since the 2006 reform, however, the Chinese central government has provided billions of dollars of earmarked fiscal transfers per year to county-level governments. The transfers have been earmarked to cover operational spending of public primary (grades 1-6) and middle schools (7-9) in central and western counties.³ Beijing has set a homogeneous per-pupil minimum operational spending benchmark and updated it every other year for both central and western counties.⁴ But there has been a staggered exogenous variation of funding ratios and timing between the two regions. First, the central government has funded 80% of rural and township schools' minimum operational spending benchmarks in western counties and 60% of those in central counties.⁵ Second, western counties began receiving transfers in 2006, while central countries started in 2007. As we use central counties as a control group for western counties over time, the policy variation in the preferred specification (an additional 33.3%) is comparable to or larger than those in earlier research of intergovernmental transfers (Litschig & Morrison, 2013). Still, to further discern long-term effects of the reform, we employ alternative identification strategies by using urban schools in eastern counties and urban schools in western and central counties, which mostly did not receive central grants after the reform, as counterfactuals.

The key identification assumption underlying our DID design is *not* random assignment of central versus western counties, but that rural and township school expenditures in central and western counties would have shared common trends in the absence of the reform. The central government initiated the reform unexpectedly and the Ministry of Education (MoE) exogenously determined the phase-in variation between the two regions. The two regions are similar in economic development and fiscal capacity. Moreover, our analysis shows that key outcomes, such as budgetary, extra-budgetary, and total operational spending per pupil in both rural and township public schools trended similarly for the treatment and control groups before 2006 in both preferred and alternative strategies.

An idiosyncratic feature of local public finance in China allows us to test whether intergovernmental transfers and accountability to upperlevel governments led to crowd-in or crowding out effects, that is, the extent to which local governments matched central grants and the extent to which central grants substituted for pre-existing local education spending. Since the 1994 Tax Sharing Scheme (TSS) Reform, apart from a budgetary account, Chinese local governments have also used a separate extra-budgetary account to finance local public services. Land transfer revenue and other governmental fees fund extra-budgetary expenditures, which local government need not to report to the superiors (Hawkins, 2000; Li, Park, & Wang, 2007; Zhao, 2009). But since the 2006 reform, intergovernmental transfers for operational education spending have been distributed into counties' budgetary accounts, which are heavily monitored by the central government and used to evaluate performance. We are among the first to utilize both budgetary and extra-budgetary fiscal data and examine whether the reform caused real crowd-in or crowding out effects.⁶ As our data also include school expenditure types that the central grants did not directly cover, such as teacher salaries and capital expenditures, we can observe whether the transfers were moved to other types of public education investments.

This paper uses a unique set of confidential county-level itemized education finance data from 1998 to 2011. Collected by the MoE, this administrative dataset contains itemized education finance data and statistics of school operation across all Chinese county-level governments. The main sample in this paper is a balanced panel consisting of more than 1500 central and western county-level governments (excluding municipal districts) in fourteen years.⁷ We couple the education finance data with annual county-level socioeconomic data from the National Bureau of Statistics (NBS).

Our DID estimates reveal crowding out effects. The heavily monitored budgetary school operational expenditures, which the superiors use to evaluate local governments' performance, increased after the reform by 130-190 yuan (USD 20-28) per student. But decreases in extra-budgetary spending nearly canceled this increase out, leaving the total operational spending increase statistically insignificant. The alternative identification strategies, using urban schools in eastern counties and those in central and western counties which have never received central grants as control groups, yield similar results. The transfers were not channeled to other education spending categories: spendings on building maintenance, construction, acquisition, and teachers wages all declined.8 This finding is robust in both rural and township schools and applies to alternative specifications, different control groups, different sample frames and timeframes, in both nominal and constant prices, different degrees of winsorization and measures of dependent variables, and various methods of sample weighting. Falsification tests show that the findings are not caused by other confounding policies such as the elimination of tuitions. No evidence suggests that local governments moved the grants to mitigate educational inequality, improve other social services, or invest in infrastructure. As the transfers were not moved to any of the above categories, it is most likely that local governments used transfers to cover public payroll.

To further explore the underlying mechanisms of the null result, we estimate six triple-difference (DDD) regressions with six prominent county-level pre-reform characteristics: shares of public employment, shares of rural population, economic development, fiscal capacity, student-teacher ratio, and minority autonomous county. Studies of local education provision in non-democracies suggest that local public employees, instead of nominal beneficiaries, can hold local political leaders accountable when implementing education policies (Lü, 2011). We choose the third and fourth traits because the education finance literature, largely focusing on school finance reforms in the United States, diverges on whether increases in education spending benefit low-income students (Candelaria & Shores, 2019; Jackson et al., 2016; Lafortune et al., 2018; Lutz, 2010) or wealthier families (Cascio, Gordon, & Reber, 2013; Cascio & Reber, 2013; Hyman, 2017). Pre-

 $^{^{2}}$ For more institutional details about county-level governments, see 1.2.

³ Their approximate counterparts in the United States are elementary schools and junior high schools. According to accounting rules of Chinese public schools, operational spending does not include personnel spending such as teachers' salaries. For detailed definitions of spending categories, see 2.2.

⁴ For detailed definitions of central and western counties, see 1.4.

⁵ For instance, in 2009 and 2010, the minimum per-pupil operational spending benchmark for each primary school student was 500 Chinese Yuan (USD 73) per year. Therefore, a western county received 400 yuan for each enrolled primary school student that year, and a central county received only 300 Yuan.

 $^{^{6}}$ Lü (2011, 2015) has access to the extra-budgetary data but addresses other questions.

⁷ Around 700 eastern county-level governments, whose jurisdictions are more economically developed, are not part of this reform. Nearly 600 municipal districts of western and central provinces (also at the county level) only manage and finance urban schools.

⁸ The first three items are reported in subsection A.4.

reform student-teacher ratios capture pre-reform qualities of local public education, and minority autonomous counties may receive additional fiscal transfers through channels that are not observed in our data.

After dividing the sample by share of public employees and share of rural residents in the population, we find a pair of opposite relationships in total operational spending of rural primary schools. Counties with relatively more local public employees before the reform invested more in rural primary schools after the reform, as they did not reduce existing investments but matched central grants. The heterogeneous treatment effect across counties with different relative sizes of public employment is comparable in size to the treatment effects among richer, fiscally more abundant counties, and counties with better prereform public education qualities. Counties with a larger rural population before the reform, however, spent less on public education after the reform. For township primary schools, only counties with relatively more local public employees spent more on total operational expenditures.

This paper contributes to the literature in three ways. The null result directly speaks to a vast literature on intergovernmental transfers. Advocates have long argued that intergovernmental transfers improve efficiency and quality of local social service provision (Campos & Hellman, 2005; Candelaria & Shores, 2019; Fernández & Rogerson, 1999; Guryan, 2001; Lafortune et al., 2018; Litschig & Morrison, 2013) and encourage local governments to match public spending (de Janvry, Finan, & Sadoulet, 2012; Litschig & Morrison, 2013). By distributing fiscal transfers, the federal or central government may achieve policy objectives that local governments lack incentives to implement (Rodden, 2006; Treisman, 2007). Empirical evidence on the effects of intergovernmental transfers, however, is mixed. Central or federal grants may draw resources away from non-funded programs (Baicker & Gordon, 2006), fail to improve qualities of public service (Köthenbürger, 2011; Lü, 2015; Sharma & Cárdenas, 2008), or barely induce small substantive effects (Cascio & Reber, 2013). Transfers might induce corruption (Brollo, Nannicini, Perotti, & Tabellini, 2013; Nikolova & Marinov, 2017; Reinikka & Svensson, 2004), fail to lessen tax burden (Dahlberg, Mörk, Rattsø, & Ågren, 2008), and exacerbate inequality across subnational divisions (Courant & Loeb, 1997). By examining policy outcomes of the 2006 Chinese Education Finance Reform - one of the world's largest intergovernmental transfers programs for education finance - we document that additional intergovernmental transfers caused increases in budgetary accounts, which the central government closely monitored. But the transfers also crowded out existing spending in less monitored accounts and spending types, canceling out the impact on increasing overall education spending, and failed to improve other school finance outcomes. Our findings resonate the literature on Title I in the United States, which shows that federal grants were crowded out because of weak regulations and accountability, suggesting that authoritarian local governments behave much like democratically elected ones (Cascio & Reber, 2013; Gordon, 2004).

Moreover, since we document that per-student spending gains in public education were conditional on the size of the public employee workforce, our findings complement a growing literature on accountability, political participation of citizens, and provision of public goods. There is an ongoing debate on whether public goods provision improves through accountability and citizens' organizational capability (Aghion, Jaravel, Persson, & Rouzet, 2019; Banerjee, Deaton, & Duflo, 2004; Banerjee & Duflo, 2006; Bardhan, 2002; Bardhan & Mookherjee, 2006; Bursztyn, 2016; Hanushek, 2006; Hinnerich & Pettersson-Lidbom, 2014; Lafortune et al., 2018; Martínez-Bravo, Mukherjee, & Stegmann, 2017; Mulligan, Gil, & Sala-i-Martín, 2004; Olken, 2010; Stasavage, 2005). Previous studies mostly focus on democracies where citizens participate in politics via observable channels such as elections and political activism. However, the research has largely ignored how the political leverage of different societal groups matter in an authoritarian context, where political processes are opaque.⁹ As county governments only diverted additional transfers to the highly monitored budgetary accounts but kept the total spending level unchanged, we reveal that in an authoritarian context, local governments' top-down accountability to the central government is highly dependent on the central government's monitoring capabilities and thus plays an extremely limited role in public goods provision. Therefore, our findings support the argument that upper-level governments' limited monitoring capabilities curtail local governments' incentives in public goods provision (Björkman & Svensson, 2009; Fan, Kanbur, & Zhang, 2011; Xu, 2011). Similarly, ordinary residents cannot hold local governments accountable for public goods provision because they lack means of sanctioning. Our result accords with the "informal accountability" argument that local public employees are the primary pressure group of public goods provision because, as residents, they are aware of education quality, and as public employees, they can pose credible threats to local political leaders' careers, and thus successfully lobby for favorable policies (Lü, 2011). The heterogeneous responses of county governments thus suggest that besides formal enfranchisement (Cascio & Washington, 2014; Husted & Kenny, 1997; Lott & Kenny, 1999; Miller, 2008), informal accountability of local political elites also motivates public service spending. Our findings also resonate a growing literature on the relationship between information, local accountability, and public goods provision, which argues that bottom-up accountability improves public goods provision or even outperforms centralized, top-down accountability if local beneficiaries have sufficient information and the ability to reward and punish politicians (Andrabi, Das, & Khwaja, 2017; Björkman & Svensson, 2009; Björkman Nyqvist, de Walque, & Svensson, 2017; Duflo, Dupas, & Kremer, 2015; Pradhan et al., 2014; Serra, 2012).

Finally, this paper is the first robust empirical analysis of the 2006 Chinese Education Finance Reform, one of the largest education fiscal transfers programs in the world. Little empirical work has been done on local governments' responses to the reform.¹⁰ As past research has ignored extra-budgetary expenditures, we are the first to access and cleanse the complete raw data we examine, estimate whether the reform changes local governments' behavior in education investment, and explore underlying mechanisms of the heterogeneous treatment effects with a national sample. We also help explain intriguing findings from a few works on the relationship between the reform and citizens' behavior and political attitudes. Lü (2014) reports that the reform increased rural parents' trust in the central governments but not the local ones. The increasing trust in the central government was driven by policy awareness rather than benefits. Moreover, although the reform was designed to make public education more affordable to rural households (Wang, 2008), parents continued to pay significant amounts to educate their children (Shi, 2012). Our robust null results help us understand

⁹ The only exception is the literature on whether small-scale elections (Luo, Zhang, Huang, & Rozelle, 2010; Martínez-Bravo, Padró i Miquel, Qian, & Yao, 2017; Zhang, Fan, Zhang, & Huang, 2004) and lineage groups (Tsai, 2007; Xu & Yao, 2015) motivate elected village committees, which are neither a formal part of the Chinese bureaucracy nor a level of formal government, to provide a limited range of public goods.

¹⁰ A few papers in Chinese attempt to estimate the effects of the reform with problematic research designs, such as simple regressions that generate biased estimates. Several papers in English, using quasi-experimental designs such as DID, Regression Discontinuity (RD), and matching with small and non-random samples, generally present non-causal and inconclusive findings that could not discern policy effects (Chyi & Zhou, 2014; Ding, 2012; Liu, Murphy, Tao, & An, 2009). A recent paper exploring the long-term effect of free tuitions on individuals' educational attainment inappropriately omits the main component of this reform (central grants) and pools respondents in eastern provinces, where county-level governments did not receive central grants but only transfers from provincial governments based on unclear formulas, together with those in central and western provinces (Xiao, Li, & Zhao, 2017).

these irregularities by documenting that earmarked transfers did not improve funding for public education in most localities.

The remainder of this paper is organized as follows. Section 1 describes public finance and political accountabilities of Chinese countylevel governments, the 2006 Education Finance Reform, and how central and western counties differ in receiving central transfers during the reform. Section 2 introduces our unique county-level education finance data. Section 3 estimates the policy effects of the reform and provides robustness checks. Section 4 reveals how the policy effects vary with levels of bottom-up accountabilities. Section 5 details how heterogeneous accountabilities are linked to our findings. Section 6 concludes.

1. Institutional background and the reform

1.1. Budgetary and extra-budgetary fiscal accounts

A consolidated fiscal budget of Chinese local governments consists of two separate accounts: a budgetary account and an extra-budgetary one. While their usages are fungible, they differ dramatically in funding sources, the upper-level governments' degrees of monitoring, and requirements of accountability. Funded by formal tax revenue and intergovernmental transfers, budgetary accounts are subject to revenue sharing with the central government and thus tightly monitored by upper-level governments. Local governments are obliged to report budgetary spending timely to both upper-level governments and local rubber-stamp legislatures as an indicator of their performance. However, upper-level governments rarely monitor extra-budgetary accounts, which are funded by surcharges, administrative fees, and land transfer revenue (Hawkins, 2000; Li et al., 2007; Zhao, 2009). The 1994 TSS reform made the taxes that once funded local government payable to the central government, making Chinese local governments increasingly dependent on extra-budgetary revenue, which they use to cover daily operation and social services (Eckaus, 2003; Jin, Qian, & Weingast, 2005; Lin, 2000). Because county-level governments do not need to share extra-budgetary revenue with upper-level governments, they are not obliged to disclose extra-budgetary spending to either upper-level governments or local rubber-stamp legislatures. The central government's inspection of sub-national governments' extra-budgetary spending is rare, intermittent, and deferred.¹¹ For our primary data source, county-level governments reported the raw extra-budgetary spending data to the MoE two to three years after the fact. Given that the average tenure of county political leaders who determine local budgets is about three years, they cannot be held accountable with this schedule of reporting.

1.2. Accountability of county-level governments

Below provincial and prefectural governments, China's more than 2800 county-level governments are practically the country's third level of administrative hierarchy.¹² Except for a small number of elite urban schools and hospitals that prefecture-level governments administer, county-level governments are responsible for managing and financing

all public social services in their jurisdictions. As public health care facilities have relied on the sale of services in private markets rather than public allocations from local governments to cover their expenses since the 1980s (Blumenthal & Hsiao, 2005), public education expenses make up the lion's share of county-level governments' public service spending. However, county-level governments have been on tight budgets since the 1994 TSS Reform and the Tax-for-Fee Reform in the late-1990s. Furthermore, county-level political leaders are not electorally accountable to residents, who may demand higher public education expenditures. Thus, county-level governments lack electoral incentives to make public education spending a priority in county-level governments' consolidated budgets.

Still, two channels of accountability may incentivize county-level governments to spend on public education. Upper-level governments, and ultimately upper-level organization of the Chinese Communist Party (CCP), appoints party secretaries and county governors, the major political leaders of county-level governments in China (Maskin, Qian, & Xu, 2000; Xu, 2011). In this formal, top-down accountability, the superiors' performance evaluations of county-level political leaders include implementation of public education programs and ultimately contribute to the leaders' career advancements (Landry, 2008; Tsui & Wang, 2004). Nevertheless, the fact that upper-level governments rarely audit extra-budgetary accounts means they have imperfect information and limited monitoring capabilities.

Studies of county-level governments suggest that local public employees provide informal, bottom-up accountability within county-level governments (Lü, 2011). County-level political leaders in China are usually transferred from other localities, but local public employees are recruited and promoted in their native counties. Thus they are immediate beneficiaries of the improved provision of local public education because they send their children to local public schools. But unlike other residents, they have the means to hold county-level political leaders accountable through several informal channels. First, local public employees can punish county-level political leaders for failing to fund the schools by exerting lower levels of efforts or even defying the leaders' orders. Successful policy-making and implementation in Chinese counties (as elsewhere) require cooperation and consensus-making between county-level political leaders and local public employees, so such punishment might jeopardize the leaders' performance and political careers (Liu, Zhang, Qian, & Zhang, 2013; Lü, 2011). Moreover, local public employees can voice their discontent explicitly in meetings, interviews, and internal questionnaires or opinion polls conducted by upper-level organization departments of the CCP, which determine the leaders' careers. Carrying considerable weights in the CCP's evaluation of officials, such complaints could undermine leaders' chances of promotion or even trigger investigations (Edin, 2003; Lü, 2011).

1.3. The 2006 Chinese Eeducation Ffinance Rreform

Before the 2006 reform, the central government had no fiscal responsibility for pre-tertiary public schools. Before 2001, villages and townships, respectively, solely financed rural and township public schools in China.¹³ At that time, the Rural Tax-for-Fee Reform the major funding sources of rural public schools, extra-budgetary fees charged to students parents in rural areas. To address the shortage of school funds, the Chinese government ordered county-level governments to take over the funding and management of rural schools. This measure did not improve school finance in central and western counties, as these counties lacked the abundant extra-budgetary accounts on which eastern counties had come to rely after the 1994 TSS reform. Without sufficient extra-budgetary revenue, many county-level governments in western and central China could only fund daily school operations at a subsistence level (Liu et al., 2009; Tsang & Ding, 2005).

¹¹ Originally a legacy of two waves of economic decentralization (i.e., the Great Leap Forward between 1958 and 1959, and the Cultural Revolution between 1966 and 1976) and Mao Zedong's hostility against central planning in the Maoist era (1949–1978), extra-budgetary accounts have been institutionalized since the economic reform of 1978. For detailed discussions of the history and the central government's rare, irregular, and non-binding inspections of local governments' extra budgetary accounts, see Zhang (1999) and Jin et al. (2005).

 $^{^{12}}$ With an average population of about 500,000, county-level administrative units could either be a municipal/urban district, which is a part of a contiguous urban area, or a county in various forms, which covers towns, townships, and rural areas.

¹³ All rural and township public schools are pre-tertiary.

In a retrospective review, an official report documented that before the 2006 reform, in many poor counties, the principle of fiscal management was to make sure that "teachers receive base pays and schools operate at minimum expenses." To keep up the wages with inflation, many local governments reduced school operational spending (The State Council of China, 2011).

To address these problems, the Chinese government launched a reform known as "New Security Mechanism for Financing Rural Compulsory Education" in 2006.¹⁴ This paper focuses on the intergovernmental fiscal transfers program of the reform.¹⁵ Since 2006, the central government has set per-pupil minimum operational education spending benchmarks for primary and middle schools across Chinese counties. The minimum benchmarks are subject to a biannual increase.¹⁶ Based on the minimum benchmarks and the number of enrolled students, the central government has distributed earmarked transfers to county-level governments' budgetary accounts with different funding ratios. The MoE and the MoF also set up a special national audit group and published monthly auditing reports at the county level in the first few years. However, only budgetary school operational spending was audited.¹⁷

The central government announced the policy in the last week of 2005 and started the implementation in western counties in early 2006, but in central counties in 2007.¹⁸ The window-dressing legislation processes did not impede policy implementation.¹⁹ Because of the short notice, official documents and news reports show that this top-down reform was an unanticipated policy change.²⁰

According to official figures, as one the world's largest fiscal transfers programs in education finance, the reform costs the central government billions of dollars each year.²¹ County-level governments

¹⁵ The other component, known as the "Two Exemptions and One Subsidy (TEOS)," has eliminated tuitions and fees for rural students enrolled in compulsory education (grades 1–9) and provided subsidies for boarding students from low-income families. The TEOS was first introduced in state-designated poor counties in 2001 and applied to all counties in 2006. Because this policy started earlier than our observational period and does not introduce any treatment variation across county-level governments and the subsidies have been delivered directly to students, we do not explore it here. Still, our falsification tests in 3.3 show that potential confounding policies did not affect the results.

are responsible for matching the central grants and distributing funds to rural and township schools in their jurisdictions.

1.4. Western vs. central counties

In the transfer program of the 2006 Chinese Education Finance Reform, rural and township schools in western, central, and eastern counties were treated differently.²² Generally more economically developed, eastern counties have had higher minimum operational perpupil spending benchmarks and negotiated their ratios (smaller than 60%) of central transfers with the central government on a case-by-case basis. Central and western counties, however, share many similarities. They are generally less economically developed than eastern counties, and their governments are more fiscally constrained as a result. The per-pupil minimum operational spending benchmarks in primary and middle schools are identical among these counties. The differential treatment of western and central counties lies in funding levels and starting years. First, in western counties, the central government fund 80% of the per-pupil minimum spending benchmarks, while the central government has only paid for 60% of those in central counties. As a result, western counties have received 33.3% more funds than central counties. The variation is comparable to or larger than those in earlier research (Litschig & Morrison, 2013). Western counties also started to receive the earmarked central grants in 2006, one year earlier than central counties did.

Moreover, based on a close examination of official documents, we have corrected three categories of special treatment cases that have been classified improperly by previous studies. First, all 24 counties of three minority autonomous prefectures in the central region have received the same "western" treatment of 80% central grants since 2006.²³ Second, 243 counties in the central region were selected by the State Council in 2007, solely based on their levels of economic development, to benefit from industrial policies of the "Develop the West" program to boost their socioeconomic development. As a result, these 243 central counties have received 80% funding levels since 2007. Finally, although Hainan is classified as an eastern province, only counties in two major prefecture-level cities (Haikou and Sanya) negotiated their own funding ratios as eastern counties did. The remaining counties are eligible to receive larger ratios of central grants from this program. Eight of them (mostly minority autonomous counties of ethnic Li or Miao people) have been classified as western counties that receive 80% of required operational spending from central grants since 2006, while the rest of them have received 60% of school operational spending from the central government since 2007 just as central counties.

We have correctly recoded the treatment status of receiving central transfers for all counties, and refer to all counties that have received 80% of school operational spending from the central government are referred to as "western counties" (that is, in the treatment group) hereafter. Fig. 1 plots the over 1600 central and western counties that are in the analytical sample. The western or central administrative units excluded from the analytical sample either belong to Chongqing, a province-level municipality in western China,²⁴ or are municipal districts which the reform did not affect. Summary statistics and empirical analyses confirm that central and western counties were similar before

¹⁴ In recent years, the reform has been expanded to public urban and suburban schools that municipal district governments manage. We limit our main analyses and discussions to rural and township schools and county-level governments that are not municipal districts. For simplicity, we use the term "counties" to refer to them throughout the paper. Chinese rural education finance reforms in the past decades proceeded in a very similar way to the recent waves of education finance reforms in the United States, which also centralized school finance (see Hyman, 2017; Jackson et al., 2016; Lafortune et al., 2018).

¹⁶ For exact amounts, see Table A.1 and Table A.2.

¹⁷ See http://www.moe.gov.cn/srcsite/A05/s7052/200610/t20061020_ 78333.html.

¹⁸ Circular of the State Council No. 43 [2005]: State Council Notice on Comprehensively Deepening the Reform of Security Mechanisms of Financing Rural Compulsory Education, December 24, 2005.

¹⁹ The 2006 revision of the Compulsory Education Law of the People's Republic of China codifies the central government's responsibility for financing compulsory education for the first time. China's rubber stamp legislature, the Standing Committee of the National People's Congress, passed the revision and President Hu Jintao promulgated it on June 29, 2006. This superfluous revision officially "came into effect" on September 1, 2006, eight months after the introduction of the reform.

²⁰ Kept from the public and subnational governments, the preparation and drafting process only took eight months, as the MoE secretly started the reform planning and research in March 2005. See Transcript of the Ministry of Education Press Conference, February 27, 2007 (http://www.scio.gov.cn/xwfbh/xwfbh/33978/34777/xgfbh34782/Document/1483151/

^{1483151.}htm).

²¹ Earmarked transfers for school operational spending have steadily

⁽footnote continued)

increased from RMB 2.4 billion (\$301 million) in 2006 to RMB 47.17 billion (\$7.3 billion) in 2011. The figure reached RMB 88 billion (\$14.3 billion) in 2014, surpassing the funding amount of Title I in the same year (\$14 billion). 22 See subsection A.2 for the definitions of eastern, central, and western

counties.

²³ See subsection A.3.

²⁴ Counties in province-level municipalities are prefecture-level administrative units. As they are not equivalents to county-level governments, we exclude them from the sample.



Fig. 1. Geographical distribution of central and western counties.

the reform. As Figs. 2 and 3 show, per-pupil operational spending levels in central and western counties' have trended similarly before the 2006 reform.

2. Data

2.1. Data sources and analytical sample

Our main analytical sample includes about 1600 central and western counties in China. Based on three sources, we construct a unique dataset of education finance data, socioeconomic variables, and public employment across all western and central counties from years 1998–2011. The education finance data come from the "China Education Finance Statistical Yearbooks (County-level)" (CEFSY), a series of confidential documents the MoE compiled. The CEFSY contain both K-12 and higher education data of every county-level government (both municipal districts and counties) in China, including itemized education revenue and expenditure, as well as statistics on schools, teachers, students, and facilities. Because the MoE only publishes province-level education finance data to the public, we are among the first researchers to use the county-level confidential data.

We supplement the education finance data with county-level annual economic, fiscal, and demographic data from the "China Statistical Yearbooks (County-level)" (CSY), which the NBS compiled. To examine origins of the heterogeneous treatment effect, we also collected public employment data of county-level governments from the "Compendium of Financial Statistics of Prefectures, Cities, and Counties" (CFSPCC), a series of internal documents that the Department of Budget and the Department of State Treasury of the Ministry of Finance (MoF) jointly compiled.

The final dataset contains information on an unbalanced sample of 1612 central and western counties.²⁵ We use the balance panel for baseline results and repeat all analyses with an unbalanced panel which includes about 60 more counties. We also repeat the analyses with different percentiles of winsorization.

2.2. Variables

As the intergovernmental transfers in the reform are earmarked for school operational spending, we use per-pupil budgetary, extrabudgetary, and total operational spending as our main dependent variables. Following official accounting rules of the MoE and the MoF, we define and construct operational spending as school expenditures that cover educational activities and management, teacher training, laboratory experiments, internships, cultural and sports activities, utilities, heating, travel, postage, telecommunication, purchases of equipment and books, and maintenance of buildings, equipment, and facilities. Operational spending does not include personnel compensation, capital construction, and debt repayment.²⁶ According to the accounting rules, however, all construction expenses not exceeding RMB 50,000 yuan (about USD 6500-7400) were counted as building maintenance expenses rather than capital construction investment. Therefore, we construct broad measures of budgetary, extra-budgetary, and total operational spending which include building maintenance expenses, as well as narrow measures which do not.

School total expenditures include three major categories: personnel, operational, and construction. Personnel expenditure includes both formal and informal teachers' salaries.²⁷ School total revenues consist of fiscal budgetary funds, other fiscal funds, additional local fees, tuition, school business and commercial revenues, capital revenues, and other revenues. School total expenditures may not necessarily equal total revenues in any given year. While counties draw on both budgetary and extra-budgetary accounts for expenditures, they each have one consolidated account for school revenues. School fiscal budgetary revenues should generally match budgetary school expenditures.

The central government disburses transfers to county-level governments at the beginning of year t based on numbers of enrolled students by the end of year t - 2, because the numbers of students in year t - 2 are the most up-to-date available statistics.²⁰ Following this rule, we also construct our main per-student spending variables based on counts of the enrolled student in year t - 2. We also find very similar results when repeating our analyses based on numbers of students in years t - 1 or year t.

To address remaining omitted variable bias concerns, we also control for county-level time-varying covariates that would affect school spending, including number of schools from the CEFSY, population, share of rural population, number of townships, GDP per capita, ratio of local fiscal revenue to expenditure, and ratio of fiscal revenue to total GDP from the CSY. Information on social services (e.g., numbers of hospital beds, numbers of welfare home beds, and numbers of landline telephone users) is also from the CSY.

To measure relative sizes of local public employees, we construct multiple measures of shares of local public employees based on both the CFSPCC and CEFSY data. We impute shares of all public employees on the county public payroll and shares of all non-educational public employees (excluding teachers who were on the public payroll) in the county population in years 1998–2005. Earlier research shows that relative sizes of Chinese county-level governments' public employment have aligned with their unchanged public employee quota since the 1993 administrative reform (Lü, 2011). Our data confirm that relative sizes of public employment across different years were highly correlated: correlation coefficients between each pair of measures all exceed 0.88. The baseline heterogeneity analyses employ percentages of public employees on the county public payroll in 2005, the year before the 2006 reform, as the main measure of local public employment.

²⁵ As the NBS assigns a new and unique ADC to counties that change administrative status (most commonly, when a county becomes a county-level city), we manually implement the merge of these observations based on their names. We also construct a dataset of over 300 eastern counties in the same manner for robustness checks.

²⁶ The MoE and the MoF changed the accounting scale of these itemized expenditures several times. Therefore, we could not perform very detailed analyses on single items. For details, see the Circular of the Ministry of Finance and the Ministry of Education No.5 [2006]: Notice on Circulating Temporary Administrative Rules for Operational Spending of Rural and Township Pretertiary Schools.

²⁷ While formal teachers are formally employed by county-level governments, substitute teachers (*daike* or *minban jiaoshi*) are not formal public employees and earn much lower wages. The term of informal teachers is often misleadingly translated as substitute teachers, supply teachers, or community teachers.



Fig. 2. Effects on per-pupil budgetary operational spending in rural primary schools. *Notes:* This figure shows estimates from an event study regression by comparing the yearly difference in per-pupil budgetary operational spending between counties that would have received 100% transfer grant and counties that would have received 0% transfer grant (weighted from the 80% vs. 60% difference). The difference in the year before the reform (2006 or 2007) is normalized as zero. Sample includes data from years 2002–2011. All other regression controls are the same as in Column 6 of Table 2.

2.3. Summary statistics

Our main annual by-school-type data at the county level include aggregated information about 223,216 rural primary schools, 2.7 million teachers, and 49 million students across over 1600 counties by 2005. Table 1 reports sample means and standard deviations of the 819 western counties that received the 80% funding from 2006 and 468 central counties that received 60% funding from 2007. The remaining 275 central counties that began receiving 80% funding in 2007 have very similar sample distributions.

In 2005, one year before the reform, western and central counties had nearly identical per-pupil school operational spending, teacher salary expenditures, and thus school total spending. Tuition was about 20 yuan higher in central counties, but the difference was not statistically significant. In 2006, when western counties started to receive the central government's transfer grant, their per-pupil budgetary school operational spending more than doubled. The difference in mean between western and central counties was large (143 yuan) and statistically significant at 0.001 level. However, given the crowding-out of extra-budget operational spending, the difference in total school operational spending is still small and insignificant. Total school spending or teacher salary expenditures did not change significantly. Because the reform also waived tuition for all rural students in western counties, the average tuition revenue of treated counties was 0 in 2006.

Table 1 also reports sample means for other school statistics and covariates. Western counties generally had fewer schools, teachers, fewer residents, and lower GDP per capita. The Rural School Consolidation Policy rapidly reduced the number of schools while the One-Child policy steadily decreased numbers of students across both types of counties. But none of the between-group differences changed dramatically over time.

3. Effects of the transfers on education spending

3.1. Effects on operational expenditures

Fig. 2 presents the event study estimates of the gaps between central and western counties' rural primary schools regarding budgetary perpupil operational spending in the balanced panel, net of a set of full controls that regression model 1 will use. All time series are centered around the year before a county started to experience the reform (2005 for western counties and 2006 for central counties) as year 0. We find

that counties that experienced the 2006 Chinese Education Finance Reform had a sizable and statistically significant increase in the closely monitored per-pupil budgetary school operational spending intermediately in the first year of reform. This increase in the budgetary spending remained similar in magnitude five years after the reform.

Fig. 3 (a), however, visualizes a pronounced crowding out effect. Decreases in the extra-budgetary accounts, one main funding source for local public education before the 2006 reform, offset the increases in the budgetary accounts. As a result, as Fig. 3(c) depicts, the time series of total perpupil operational spending trended similarly in western counties and in central counties after the 2006 reform. This result suggests that the additional intergovernmental transfers that the reform caused had little or no effect on total school operational spending. Fig. 3(b) and (d) reveal an identical pattern when using logged forms of dependent variables.

Table 2 formalizes the visual evidence by presenting estimates of the following DID regression that uses the same definitions and controls underlying the figures:

$$Expenditure_{ijt} = \alpha Policy_{ijt} \times Post_{ijt} + X_{ijt}\beta + T_j\gamma + \delta_{ij} + \lambda_t + \varepsilon_{ijt}$$
(1)

where *Expenditure*_{*ijt*} denotes budgetary, extra-budgetary, or total operational per-pupil education spending for county *i* in province *j* in year *t*. *Post*_{*ijt*} denotes an indicator for year *t* being 2006 or later for western counties and year *t* being 2007 or later for central counties (including the 243 central counties that have had the western county status since 2007). *Policy*_{*ijt*} denotes the scaled treatment, that is, the ratio of central grants in operational per-pupil minimum spending benchmark for county *i* in province *j*. Specifically, *Policy*_{*ijt*} equals 0.8 for western county status since 2007) and 0.6 for central counties. *X*_{*ijt*} denotes a possibly empty vector of county-level controls. *T*_{*j*} denotes province specific year trends. δ_{ij} and λ_t are county fixed effects and year fixed effects, respectively.

We examine two time periods (2002–2006 and 2002–2011) so that α , our key DID estimator, captures different substantive meanings.²⁸

²⁸ Similar short-term and long-term estimates validate that the long-term policy effects are appropriately identified, though there are not "pure" counterfactuals that received zero treatment in the main empirical strategy. We limit our main sample to post-2002 periods to eliminate bias the 2001 Rural Tax-for-Fee Reform may introduce. Robustness checks using other time periods show consistent estimates.



Fig. 3. Effects of on per-pupil operational spending in rural primary schools. *Notes*: This figure shows estimates from an event study regression with different outcome measures. All the model details are the same as in Fig. 2.

From 2002 to 2006, α captures the difference between counties that received intergovernmental transfers (at 80% of the minimum benchmark) and those that received none. From 2002 to 2011, α captures the different ratios of intergovernmental transfers (at 80% of the minimum benchmark vs. none in 2006 and at 80% vs. 60% from 2007 onward).

Columns 3 of Table 2 reports estimates from our preferred specification in model 1. The top and bottom 1st percentiles of the outcome are winsorized. We also report estimates from the basic two-way fixed effects DID model (columns 1) and DID model only with time-varying controls (columns 2). Since the MoE exogenously launched the reform, we expect minimal omitted variable bias. Consistent estimates from columns 1 to 3 support the hypothesis that intergovernmental transfers only cause spending increases in accounts that the central government monitors well and use to evaluate policy implementation, at the expenses of crowding out effects in ill-monitored ones.²⁹

Consistent with the graphic results in Fig. 2, Panel A shows that the transfer grant from the central government in 2006 was translated into a large increase (100% of the mean spending level in central counties in 2005) in budgetary per-pupil operational spending of rural public primary schools in western counties. Figure A.4 presents the yearly trends of the shares of both western and central counties meeting the national

minimum benchmark of per-pupil budgetary school operational spending, and the trends are consistent with counties' compliance with the policy regarding budgetary operational spending. In 2005, only 42% of western counties spent more than 150 yuan (the minimum benchmark from 2005 to 2008) in budgetary operational expenditures. This number increased to 84% in 2006 and 91% in 2007. This reveals that local governments spent the transfer funds properly when as the central government closely monitored the account.

Panel B reports that when controlling for all controls and trends used in the graphs, intergovernmental transfer in the reform had an insignificant and small positive effect on actual total operational perpupil spending in rural public primary schools. The effect was 36.90 yuan with a standard error of 31.66 yuan, relative to a pre-reform control group mean of 331 yuan and standard deviation of 214 yuan. As the actual per-pupil transfer to western counties in 2006 was 120 yuan (80% of the minimum benchmark), the grant only caused a 29.52 yuan increase in 2006, with a standard error of 25.33 yuan.

Panel C shows that a crowding out effect of over 100 yuan in the illmonitored extra-budgetary accounts largely canceled out the increase in budgetary spending. Therefore we observe a null effect on total school operational spending. This is consistent with the hypothesis that the weak monitoring capabilities of the central government curtail the top-down accountability between the central government and local governments.

Column 6 of Table 2 reports similar findings when we examine a balanced panel between 2002 and 2011. The increase in intergovernmental transfers, which averaged around 258 yuan per year between 2006 and 2011, caused a 191.47-yuan increase (with a standard error of 48.21 yuan) in budgetary spending. The increase in budgetary

²⁹ Figure A.2 plots the event study estimates from different model specifications. While the point estimate and statistical significance in the longer term differs, all these models show very similar trends. If we use the DID model without province-year trends as the main specification, all the results and conclusions will remain qualitatively the same. However, as Figure A.2 suggests, controlling for province-year trends depicts parallel common trends between western and central counties.

Table 1 Summary statistics.

| | 2005 | | | 2006 | | |
|---|----------------|------------------|------------------------|----------------|------------------|------------------------|
| | Control (1) | Treatment (2) | <i>p</i> -value (3) | Control (4) | Treatment (5) | <i>p</i> -value (6) |
| Operational spending (budgetary) | 146 | 158 | 0.712 | 201 | 343 | 0.004 |
| | (161) | (221) | | (208) | (260) | |
| Operational spending (budgetary, nominal) | 105 | 113 | 0.712 | 150 | 256 | 0.004 |
| | (115) | (159) | | (155) | (193) | |
| Operational spending (total) | 332 | 321 | 0.790 | 390 | 419 | 0.557 |
| | (241) | (278) | | (280) | (293) | |
| Operational spending (extra-budget) | 186 | 163 | 0.380 | 189 | 76 | 0.000 |
| | (149) | (158) | | (149) | (105) | |
| Total school spending (budgetary) | 1452 | 1407 | 0.811 | 1793 | 1720 | 0.755 |
| | (805) | (817) | | (1052) | (976) | |
| Total school spending (total) | 1746 | 1751 | 0.982 | 2104 | 2000 | 0.687 |
| | (949) | (956) | | (1208) | (1129) | |
| Full-time salaries | 1372 | 1368 | 0.980 | 1668 | 1514 | 0.493 |
| | (748) | (759) | | (965) | (938) | |
| Tuitions | 121 | 98 | 0.333 | 119 | 0 | 0.000 |
| | (86) | (78) | | (97) | | |
| Number of schools | 148 | 115 | 0.119 | 134 | 107 | 0.155 |
| | (92) | (111) | | (89) | (105) | |
| Number of students | 29,397 | 26,695 | 0.610 | 28,009 | 25,336 | 0.606 |
| | (20,740) | (29,072) | | (20,271) | (28,568) | |
| Number of teachers | 1932 | 1407 | 0.017 | 1923 | 1363 | 0.008 |
| | (1152) | (1149) | | (1135) | (1106) | |
| Population (10,000) | 49 | 32 | 0.014 | 50 | 33 | 0.015 |
| | (27) | (27) | | (27) | (28) | |
| % Rural population | 53 | 48 | 0.550 | 52 | 39 | 0.257 |
| 1 1 | (27) | (34) | | (26) | (34) | |
| GDP per capita | 10.035 | 6427 | 0.002 | 11.452 | 7164 | 0.002 |
| 1 1 1 | (5712) | (5605) | | (7012) | (7070) | |
| % Fiscal revenue of GDP | 3 | 4 | 0.132 | 3 | 4 | 0.330 |
| | (1) | (3) | | (2) | (3) | |
| Number of counties | 468 | 819 | | 468 | 819 | |

Notes: This table reports summary statistics of selected outcomes and covariates from the main balance sample (2002–2006). Western counties are in the treatment group. Central counties received the 60% treatment from 2007 are included in the control group. The other 275 central counties with particular treatments have very similar sample means (e.g., with a budgetary operational spending mean of 147 (sd = 119) in 2005). All per-pupil school revenues and expenditures are weighted using numbers of students in year t-2. All monetary measures (except the second variable) are in year 2011 constant price. Covariates are weighted using population in year t. *p*-values are adjusted from robust standard errors clustered at province level, which are reported in parentheses.

spending, however, crowded out 122.24 yuan (with a standard error of 35.88 yuan) in extra-budgetary spending per year. The impact of the intergovernmental transfers on the total spending remains small and insignificant (67.13 yuan with a standard error of 43.17 yuan).

3.2. Other schools as the control group

Skeptical readers may worry that as the reform started in the two groups within a year of each other, there is not a counterfactual group that received zero transfer after 2007 and it is impossible to discern genuine long-term effects. To address this concern, we employ alternative identification strategies by using urban schools in western and central counties, urban schools in eastern counties, and rural schools in eastern counties as control groups.³⁰ Urban schools have also been managed and funded by county-level governments. Unlike rural and township schools, however, only a small number of urban schools in western in this program, and none of the urban schools in eastern counties received any central grants.³¹

We first use urban schools in eastern counties, which never received central grants after the reform, as the counterfactual group. One caveat of this strategy is that, while rural schools across western and central counties are very similar, urban schools in eastern counties may be different from them. Nevertheless, the event study results, shown in the first two panels of Figure A.3, demonstrate that school operational spending trends of rural schools in western and central counties before the reform were similar to those of urban schools in eastern counties. The estimated first-year and long-term policy effects are very close to the main results in Figs. 2 and 3.

Our second strategy is to use urban schools in western and central counties, which are more comparable to rural schools within the same counties than urban schools in eastern counties, as the control group. We have modified the main event study model in two ways: (1) we do not control for time-varying covariates because there are no county-level variations between rural and urban schools in the same county, and (2) we allow the province-year trends to be different between rural and urban schools. The estimates are expected to be smaller than what our preferred strategy and the first alternative strategy yield, because various official or anecdotal sources suggest that a small number of urban schools in western and central counties received intergovernmental transfer in this program. Panels (C) and (D) of Figure A.3 show that the results are qualitatively unchanged. The two groups exhibit

 $^{^{30}}$ We do not use them in the main identification strategy as all these strategies have caveats. However, as we show later, the results are consistent with the main results and the caveats largely do not affect the assumption of parallel trends and the results.

³¹ Rural and township schools in eastern counties received similar transfer grants from provincial governments, but the ratios are unknown. Urban schools

⁽footnote continued)

in eastern counties, however, are not the target of the reform and never received transfers from upper-level governments.

Effect of the additional central grants on budgetary, total, and extra-budgetary school operational spending at the county level.

| Winsorization: | 1-99% winsorizing | | | | | | | |
|---|-------------------|---------|---------|-----------|---------|---------|--|--|
| Time period: | 2002–2006 | | | 2002-2011 | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | | |
| A. Budgetary operational spending (per-p | upil) | | | | | | | |
| Policy × Post | 153.11 | 151.02 | 139.94 | 125.94 | 142.36 | 191.47 | | |
| | (45.52) | (51.67) | (45.93) | (42.22) | (43.50) | (48.21) | | |
| Pre-control mean | | 146 | | | 146 | | | |
| Pre-control SD | | [156] | | | [156] | | | |
| R-squared | 0.63 | 0.63 | 0.66 | 0.67 | 0.67 | 0.70 | | |
| B. Total operational spending (per-pupil) | | | | | | | | |
| Policy \times Post | 51.50 | 48.69 | 36.90 | -10.89 | 9.24 | 67.13 | | |
| | (34.97) | (40.73) | (31.66) | (41.67) | (43.21) | (43.17) | | |
| Pre-control mean | | 331 | | | 330 | | | |
| Pre-control SD | | [214] | | | [213] | | | |
| R-squared | 0.61 | 0.62 | 0.63 | 0.59 | 0.61 | 0.63 | | |
| C. Extra-budgetary operational spending | (per-pupil) | | | | | | | |
| Policy \times Post | -102.12 | -102.70 | -102.75 | -136.05 | -131.92 | -122.24 | | |
| | (29.07) | (29.93) | (33.01) | (35.62) | (39.51) | (35.88) | | |
| Pre-control mean | | 184 | | | 184 | | | |
| Pre-control SD | | [127] | | | [126] | | | |
| R-squared | 0.53 | 0.54 | 0.56 | 0.31 | 0.33 | 0.36 | | |
| Controls | No | Yes | Yes | No | Yes | Yes | | |
| Province-Year Trends | No | No | Yes | No | No | Yes | | |
| Observations | | 7810 | | | 15,510 | | | |
| Clusters (Counties) | | 1562 | | | 1551 | | | |

Notes: This table reports difference-in-differences estimates of central grant in the 2006 Chinese Education Finance Reform on main school per-pupil operational spending outcomes at the county level during two time periods (2002–2006 and 2002–2011). All columns display the coefficient on the interaction term between an indicator for western counties (*Policy*) and an indicator for the years under the reform (*Post*), from a regression of the outcome on this interaction, county and year fixed effects, and possibly additional time-varying controls and province-year trends. Time-varying controls include number of schools, population, share of rural population, number of townships, GDP per capita, ratio of local fiscal revenue and expenditure, ratio of fiscal revenue and GDP. All regressions are weighted by numbers of students in t - 2. These regressions use the broad definition of operational spending, which includes expenditures of school operations, maintenance, construction, and acquisition of buildings. Balanced samples are used that observations are required for all years 2002–2011 or 2002–2006. Coefficients of per-pupil budgetary spending and extra-budgetary spending do not add up to those of per-pupil total operational spending due to winsorization. All prices are constant in 2011 price. Robust standard errors, reported in parentheses, are clustered by province.

similar pre-reform trends, and more importantly, while the reform caused an increase in budgetary spending accounts, there were no policy effects on total operational spending both in the first year of the reform and over the long run.

Finally, we substantiate the robustness of our findings by including rural schools in eastern counties as the control group. Despite the fact that rural schools in eastern counties have received undisclosed ratios of transfers, we specify different hypothetical "intensities" of post-reform treatment for three groups of rural schools: 0% for those in eastern counties, 60% for those in the Central, and 80% for those in the West. Panels (E) and (F) of Figure A.3 render similar results, noting that there might be non-parallel pre-trends in total operational spending. While the reform caused a small increase in budgetary spending, the rise is small and insignificant if we combine budgetary and extra-budgetary expenses.

3.3. "Compliers" as the control group

Another approach to the robustness of our findings is to use counties that were compliant to the minimum spending benchmark prior to 2006 as the control group. As these "compliers" met the benchmark before the reform, they have no incentives to further increase post-reform spending levels even in the closely-monitored budgetary accounts. For "noncompliers" that did not meet the benchmark before the reform, however, they are incentivized to spend more in budgetary accounts and impress upper-level governments. Fig. 4 matches our prediction and shows that the reform only causes positive changes in budgetary spending among counties that did not meet the benchmark in 2005 ("non-compliers"). The budgetary spending level of "compliers" has not changed. Changes of total spending are around zero for both groups of counties.

3.4. Other robustness and falsification tests

We conduct three sets of robustness checks. First, Table 3(a) replicates the baseline regression in column 3 of Table 2 with different sample frames. Columns 1–5 replicate the baseline regression with both balanced panels with different winsorizations and an unbalanced panel. Our purpose is to confirm that the null result is not sensitive to extreme values or 50 omitted counties in the unbalanced panel. To verify that observations in 2002-03 do not drive the null result, column 6 uses 2004-06 observations only. Column 7 includes observations from 2000 to 2006 to verify whether the null result is sensitive to the 2001 Tax-for-Fee reform. In column 8, we drop counties from four minority autonomous regions (Inner Mongolia, Tibet, Ningxia, and Xinjiang) in order to confirm that the null result is robust across counties of other provinces.

The second set of robustness checks, reported in Table 3(b), replicate the baseline regression with various measures of dependent variables. Column 1 uses current nominal prices. Column 2 uses the logged form of dependent variables. Column 3 uses one-year-lagged dependent variables. Column 4 calculates per-pupil spending figures based on numbers of students in current years (year *t*) instead of year t - 2.

The last set (Table 3(c)) replicate column 3 of Table 2 with various weighting strategies. Column 1 uses propensity scores as the weights, which are estimated from fitting the probability of being treated using the 2005 data. Column 2 uses the products of propensity scores and numbers of students as the weights to further control for differences in school sizes. Column 3 uses DiNardo, Fortin, and Lemieux (1996)'s DFL reweighting method, which is similar to conventional matching but more flexibly controls for any time-varying shocks in the numbers of students. Column 4 presents results without weighting.

Regarding per-pupil total operational spending, all columns report

95% CI

95% CI



Fig. 4. Effects on per-pupil operational spending in rural primary schools by pre-reform spending levels. Notes: This figure shows estimates from an event study regression using separate samples based on pre-reform spending levels. "Non-complies" group includes counties that did not meet the minimum spending benchmark in 2005. "Complies" group includes counties that already met the minimum spending benchmark in 2005. All the model details are the same as in Fig. 2.

point estimates and 95% confidence lower bounds that are similar to or smaller than those in Table 2. Regarding per-pupil budgetary and extrabudgetary operational expenditures, all point estimates and standard errors are similar to the baseline findings. The robustness check results are consistent with our baseline findings: the central grants caused a sizable and significant budgetary increase, accompanied by an equally large extrabudgetary decrease.

A final threat to the assumption of our DID identification is that western and central counties might experience differential shocks in 2006. We provide a falsification test by examining whether high school expenditures changed because of the reform. As the reform was not supposed to affect high school funding, high school expenditures are ideal proxies for general education spending trends that are associated with unobserved socioeconomic factors or confounding policies. Panel A and B of Fig. 5 show no relationship between the 2006 reform and budgetary and total high school operational expenditures, which supports our identification strategy.³² Panels C and D show that the crowed-out transfer grants were not channeled to high schools (extra-budgetary operational spending or total spending). In subsection A.4, we disaggregate the schools'

operational expenditures into fungible spending categories, including a narrow definition of operational spending, building maintenance, and building construction and acquisition. We do not find that local governments channeled the transfer to any of these spending categories.

3.5. Effects on other public education outcomes

As we have demonstrated the null result and the crowding out effect, where are the yuan that are statistically unaccounted for? An alternative explanation of the null effects is that local governments channeled the transfers to cover personnel spending or recoup losses of school revenue. To address this concern, Table 4 replicates columns 3 and 6 of Table 2 for key education finance and accounting outcomes, including major categories of schools' personnel and capital expenditures, school revenue, and values of school assets and facilities.³³ Columns 5 and 6 of Table 4(b) demonstrate that because the 2006 reform also waived tuition for all rural or township public primary and middle school students, tuition revenue decreased by around 110 yuan, which is comparable in size to the pre-control mean. Although public primary schools almost completely lost the tuition revenue, intergovernmental transfers did not compensate for the losses. As columns 1 and 2 of Table 4(b) report, total school revenue also decreased considerably because of the reform.

Table 4 (a) suggests that counties made ends meet by cutting

³²We do not provide formal estimates of the policy effects on township/ county primary schools and middle schools (there are no rural or village middle schools), because the local classification of these types of schools in our data is unclear. The unclear classification makes it difficult to determine whether all of the township/county schools were included in the reform. Still, Figure A.5 shows similar event study trends that the reform had positive and significant impacts on budgetary operational spending, but insignificant impacts on total operational spending.

³³ Unlike expenditures, school revenues and assets are pooled into a single account.

Robustness Checks of the Effects of Additional Central Grants on Budgetary, Total, and Extra-budgetary School Operational Spending (per-pupil) at the County Level, 2002-06 (if not otherwise specified).

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|--|--|---|---|---|------------|---------|---------|------------|
| A. Different Winsorizations of | and Samples | | | | | | | |
| Sample: | Balanced | | | | Unbalanced | 2004-06 | 2000-06 | Drop 4 ARs |
| Winsorizing: | All | 5% | 10% | drop 1% | 1% | 1% | 1% | 1% |
| Policy Effect | 139.68 | 140.57 | 139.10 | 141.19 | 139.79 | 156.67 | 153.21 | 153.21 |
| (Budgetary) | (46.64) | (44.03) | (41.02) | (45.21) | (45.95) | (36.33) | (44.17) | (52.44) |
| Policy Effect | 35.39 | 40.15 | 40.60 | 44.45 | 38.78 | 50.15 | 30.31 | 35.39 |
| (Total) | (31.76) | (30.59) | (26.78) | (31.71) | (33.84) | (33.64) | (32.10) | (35.33) |
| Policy Effect | -104.29 | -98.93 | -97.25 | -99.01 | -100.65 | -105.95 | -122.80 | -115.23 |
| (Extra-budgetary) | (32.14) | (31.09) | (28.72) | (32.93) | (33.13) | (39.28) | (29.47) | (34.35) |
| Observations | 7810 | 7810 | 7810 | 7525 | 7964 | 4755 | 10,542 | 6345 |
| Clusters (Counties) | 1562 | 1562 | 1562 | 1505 | 1612 | 1585 | 1506 | 1269 |
| B. Different Measures | | | | | | | | |
| | Current prices | Log of outcome | Lagged outcome | # of students in t | | | | |
| Policy Effect | 105.88 | 0.96 | 136.94 | 170.51 | | | | |
| (Budgetary) | (34.63) | (0.37) | (41.74) | (49.89) | | | | |
| Policy Effect | 28.43 | 0.15 | 25.19 | 79.34 | | | | |
| (Total) | (23.54) | (0.10) | (31.06) | (42.57) | | | | |
| Policy Effect | -77.45 | -1.50 | -111.27 | -101.52 | | | | |
| (Extra-budgetary) | (23.70) | (0.31) | (37.32) | (35.52) | | | | |
| Observations | 7810 | 7810 | 6248 | 7810 | | | | |
| Clusters (Counties) | 1562 | 1562 | 1562 | 1562 | | | | |
| C. Different Weightings | | | | | | | | |
| | P score weighting | P score \times # of students | DFL weighting | Not weighted | | | | |
| Policy Effect | 131.56 | 158.69 | 161.58 | 112.01 | | | | |
| (Budgetary) | (44.52) | (43.02) | (52.14) | (47.61) | | | | |
| Policy Effect | 41.05 | 48.57 | 21.14 | 12.56 | | | | |
| (Total) | (33.50) | (31.08) | (41.58) | (37.90) | | | | |
| Policy Effect | -99.37 | -110.44 | -139.66 | -103.05 | | | | |
| (Extra-budgetary) | (34.36) | (34.74) | (51.52) | (28.67) | | | | |
| Observations | 7775 | 7775 | 7145 | 7810 | | | | |
| Clusters (Counties) | 1555 | 1555 | 1551 | 1562 | | | | |
| (<i>Total</i>) Policy Effect (<i>Extra-budgetary</i>) Observations Clusters (Counties) | (33.50) -99.37 (34.36) 7775 1555 | (31.08) -110.44 (34.74) 7775 1555 | (41.58) -139.66 (51.52) 7145 1551 | (37.90) -103.05 (28.67) 7810 1562 | | | | |

Notes: This table reports difference-in-differences estimates of central grant in the 2006 Chinese Education Finance Reform on main school per-pupil operational spending outcomes at the county level using different samples, measures, and weights. Time-varying controls include number of schools, population, share of rural population, number of townships, GDP per capita, ratio of local fiscal revenue and expenditure, ratio of fiscal revenue and GDP. See notes of Table 2 for more information. Robust standard errors, reported in parentheses, are clustered by province.

teachers' wages. Although column 6 of Table 4(a) reports a small rise of 8.60 yuan (with a standard error of 2.45 yuan) per student per year in informal teachers' salaries between 2006 and 2011, columns 3 and 4 report a dramatic decrease in formal teachers' salaries, which constituted over 99% of public schools' personnel expenditures. The 2006 reform caused a per-pupil decrease of 290 yuan (or 21.2% of the prereform real wage level) in formal teachers' wages.³⁴ The total decline in school expenditures, as reported in column 2 of Table 4(a), was similar in size to the drop in school revenue (see column 2 of Table 4(b)). Because the reform failed to cause an increase in school revenue but motivated a decrease in schools' personnel expenditures, it is unlikely that county-level governments diverted the earmarked intergovernmental transfers to compensate for public schools' losses of revenue or to increase teachers' salaries and benefits. As Table 4(c) illustrates, intergovernmental transfers also caused a decrease in total assets of rural primary schools and failed to have any effect on other asset categories. Because all prices in the models are constant, it suggests that since the reform, (1) there have been few asset purchases, and(2) preexisting assets have depreciated.

Arguably, one potential reason for the reform is to reduce withincountry inequality in educational spending. Fig. 6, however, shows that the reform fails to mitigate inequality in educational spending across counties. While Gini coefficients of budgetary operational spending levels at the province level decreased momentarily as a result of the reform, the effect is no longer observable after a couple of years. Moreover, Gini coefficients of extra-budgetary operational spending levels at the province level slightly increased after the reform. Therefore, the reform only caused a small and momentary decrease in the Gini coefficient of total operational spending.

3.6. Null effects on other social services or investments

Readers may ask whether county-level governments channeled the earmarked intergovernmental transfers for school operational spending to other public service programs. The only other major category of public service that county-level governments manage and fund are public healthcare. But unlike public schools, which county-level governments have had full financial responsibility since 2001, public hospitals and other healthcare facilities are only partially funded by county-level governments. While this fact makes it unlikely that other social services would divert funds, we nonetheless examine whether the earmarked intergovernmental transfers were channeled to public healthcare services or other infrastructure investments that might be funded by county-level governments. Echoing Baicker and Gordon (2006)'s findings about negative spillover effects of earmarked education funds, Table 5 and Fig. 7 show that the earmarked intergovernmental transfers for school operational spending had no effect on numbers of hospital beds, welfare home beds, and landline telephone users. The intergovernmental transfers were not shuffled to other categories of county-funded social services or investments. It is most likely that the transfers were extracted from public social services and used to cover payroll for local public employees, the last remaining

³⁴ Chinese news reports indicate that in order to make ends meet, many county-level governments have frozen teachers' salary increases since the 2006 reform. This piece of anecdotal evidence is consistent with our result, as all prices in the models are constant.







(d) Total (personnel, operational, capital)

Fig. 5. Effects on per-pupil spending in high schools within the same county. Notes: This figure shows estimates from an event study regression using high school spending outcomes as placebo tests. All the model details are the same as in Fig. 2.

government spending category.35

3.7. Where do "Missing" funds end up

So far, we have exhausted all possible items in the dataset and found that the earmarked transfers failed to improve any category of public education or public service. While we do not have high-quality data to examine where the "missing" funds go, several journalistic accounts suggest that county-level governments in China have diverted the funds to administrative uses. A 2019 report by a major nationwide state-run newspaper reveals that underfunded county-level governments have diverted the transfers to administrative expenses. One school reported that it only received as low as 2% of their entitled transfers. For some schools that received some transfers, they may use the transfers that were earmarked for operational spending to pay for other inadequately funded categories, such as payroll, construction, and teacher training.³ In a reader's letter to another nationwide newspaper, a former head of county-level education bureau admitted that the county government he

worked for diverted the earmarked transfers to pay for teachers' health insurance, which the county refused to reimburse. The funds that were supposed to fund teachers' payroll benefits possibly funded other projects such as the government's office buildings.³⁷ These accounts suggest that as extra-budgetary accounts are not closely monitored by upper-level governments, counties in China are incentivized to use earmarked transfers for school operational spending to pay for their administrative expenses.

4. Heterogeneity analyses

The null findings may obscure heterogeneous treatment effects: some county governments were more incentivized to disburse central grants to rural or township public schools than others. The literature on intergovernmental transfers and local education spending mainly focuses on income levels and fiscal capacity of school districts or local governments (Candelaria & Shores, 2019; Cascio et al., 2013; Cascio & Reber, 2013; Hyman, 2017; Jackson et al., 2016; Lafortune et al., 2018; Lutz, 2010). Meanwhile, studies of education provision and governmental accountability in China reveal that greater relative numbers of public employees incentivized county-level governments to invest in local education. Public employees share ordinary citizens' interests in

 $^{^{35}\,\}mathrm{We}$ do not have data on local payroll spending to directly test this explanation, but we have demonstrated that the crowded-out grants from the central government were not used in education or other local services.

³⁶ See "Bofu yanchi, biantong shiyong, yingu jizhan: bufen nongcun xuexiao gongyong jingfei weihe daobuliaowei (Late payments, diverted usages, and unjustified crowd-outs: why some rural schools do not receive operational funding in full)," Guangming Ribao (Guangming Daily), 4 November, 2019. http://www.xinhuanet.com/local/2019-11/04/c_1125188207.htm.

³⁷ See "Naxie nuoyong jingfei de gaoming shouduan ('Smart' tricks of diverting education funding)," Zhongguo Qingnian Bao (China Youth Daily), 17 May, 2013. http://zqb.cyol.com/html/2013-05/17/nw.D110000zgqnb_ 20130517_1-02.htm.

Effect of the additional central grants on other school outcomes.

| Time period: | 2002-06 (1) | 2002-11 (2) | 2002-06 (3) | 2002-11 (4) | 2002-06 (5) | 2002-11 (6) | 2002-06 (7) | 2002-11 (8) | | |
|--------------------------------------|------------------|---------------------|----------------|----------------------------|-----------------|--------------------|----------------|--------------------|--|--|
| A Other Major Categories | of School Expend | litures (per-pupil) | | | | | | | | |
| Dependent Variable: Total (Budgtary) | | | Formal Salary | (Budgtary) | Informal Salary | (Budgtary) | Capital (Bud | Capital (Budgtary) | | |
| Policy Effect | 22.31 | -147.09 | -126.82 | -298.84 | -1.18 | 8.60 | 5.77 | -17.16 | | |
| Toney Ender | (60.95) | (124 92) | (43.84) | (105.36) | (2.55) | (2.45) | (13 13) | (9.92) | | |
| Pre-control mean | 1451 | 1449 | 1275 | 1272 | 8 | 8 | 20 | 20 | | |
| Pre-control SD | [730] | [726] | [617] | [613] | [20] | [20] | [60] | [60] | | |
| Dependent Variable: | Total | [, _•] | Formal Salary | (Total) | Informal Salary | (Total) | Capital (Tota | al) | | |
| Policy Effect | -90.76 | -258.52 | -140.20 | -290.05 | -2.04 | 7.79 | 9.28 | -8.97 | | |
| , | (78.42) | (151.61) | (62.03) | (115.22) | (3.00) | (2.55) | (14.26) | (13.52) | | |
| Pre-control mean | 1742 | 1739 | 1368 | 1366 | 10 | 10 | 31 | 31 | | |
| Pre-control SD | [801] | [797] | [641] | [637] | [21] | [21] | [73] | [73] | | |
| B. Major Categories of Sch | ool Revenue (per | -pupil) | | | | | | L | | |
| Dependent Variable: | To | otal | 1 | Fiscal | Т | Tuitions | | Capital | | |
| Policy Effect | -85.36 | -266.62 | 37.19 | -98.32 | -110.41 | -106.41 | 5.77 | -17.21 | | |
| 2 | (76.72) | (150.74) | (61.98) | (125.73) | (30.08) | (22.85) | (13.13) | (9.94) | | |
| Pre-control mean | 1750 | 1748 | 1384 | 1382 | 120 | 120 | 20 | 20 | | |
| Pre-control SD | [802] | [798] | [721] | [717] | [67] | [67] | [60] | [60] | | |
| Dependent Variable: | Other | r fiscal | Lo | cal Fees | В | Business | | Other | | |
| Policy Effect | -5.03 | -33.01 | 2.39 | -2.97 | -2.04 | -0.37 | -13.17 | -3.66 | | |
| • | (14.10) | (56.17) | (6.30) | (12.83) | (1.92) | (1.89) | (16.58) | (12.28) | | |
| Pre-control mean | 116 | 116 | 35 | 35 | 9 | 9 | 65 | 64 | | |
| Pre-control SD | [175] | [175] | [61] | [60] | [21] | [21] | [94] | [94] | | |
| C. Assets and Facilities | | | | | | | | | | |
| Dependent Variable: | Total | assets | Gross fl | oor area (m ²) | Value | Value of buildings | | | | |
| Policy Effect | -328.97 | -327.93 | -0.54 | -0.37 | -211.84 | -177.20 | | | | |
| • | (142.06) | (153.82) | (0.17) | (0.23) | (73.94) | (122.07) | | | | |
| Pre-control mean | 2830 | 2829 | 5 | 5 | 2201 | 2200 | | | | |
| Pre-control SD | [2511] | [2512] | [2] | [2] | [1223] | [1224] | | | | |
| Dependent Variable: | Value of | equipment | Purchases | s of equipment | Valu | Value of books | | | | |
| Policy Effect | -21.77 | -14.61 | -1.66 | 1.66 | 0.07 | 0.19 | | | | |
| | (9.25) | (15.25) | (3.18) | (2.38) | (0.65) | (0.74) | | | | |
| Pre-control mean | 241 | 241 | 18 | 18 | 4 | 4 | | | | |
| Pre-control SD | [242] | [241] | [23] | [23] | [6] | [6] | | | | |

Notes: This table reports difference-in-differences estimates of central grant in the 2006 Chinese Education Finance Reform on other school finance outcomes at the county level. All asset categories, expect gross floor area (which is at the school level), are per pupil. See notes of Table 2 for more information. Robust standard error, reported in parentheses, are clustered by province.

education investment, but they have informal power to sanction county-level political leaders. Because relative sizes of county-level public employment in China have been sticky since the 1993 administrative reform, which cut public payroll and assigned public employment quota for subnational governments, percentages of public employees served as an institutional incentive for county governments (Lü, 2011).

To estimate heterogeneous treatment effects, we use six DDD models, each with one of the following six county-level pre-reform traits: (1) relative sizes of local public employees (shares of local public employees in population in 2005), (2) relative sizes of rural residents (percentages of rural population in 2005), (3) economic development (GDP per capita in 2005), (4) fiscal capacity (percentages of fiscal revenue relative to GDP in 2005), (5) pre-reform quality of education (student-teacher ratios in 2005), and (6) whether the county i is a minority autonomous county. We add the last trait because minority autonomous counties may be more likely than other counties to receive fiscal transfers from the central government through channels our data do not capture. To avoid strong parametric assumptions such as whether these traits should be linear or in log forms in regressions, we divide counties along these traits into two groups: above-median and below-median, for the first five traits, and according to category, for the last trait. Then we estimate the following DDD regression model 2:

$$\begin{aligned} Expenditure_{ijt} &= \alpha_1 Policy_{ijt} \times Post_{ijt} + \alpha_2 Policy_{ijt} \times Post_{ijt} \times TRAII_{ij,2005} \\ &+ X_{ijt}\beta + T_j\gamma + \delta_{ij} + \lambda_t + \varepsilon_{ijt} \end{aligned}$$

$$(2)$$

where $TRAIT_{ij,2005}$ is the above-median (or dummy-variable for the last trait) indicator of one of the county-level pre-reform traits defined

above in 2005, the last year before the reform. All other variables retain the same definitions we used above. The DDD estimator α_2 is the primary quantity of interest in this series of regressions, as it represents the difference between the treatment effect of the additional intergovernmental transfers the 2006 reform caused to above-median (or minority autonomous) counties and that on below-median (or ordinary) counties. Meanwhile, the DID estimator α_1 represents the effect of the reform on below-median (or ordinary) counties.

Columns 1–8 of Table 6 report the results for per-pupil budgetary and total school operational spending in rural and township primary schools between 2002 and 2011. Columns 3 and 7 report the point estimate of the DDD coefficient and its standard error from a separate regression in which *TRAIT*_{*ij*,2005} is defined as the row heading. Columns 1 and 5 report the point estimate and the standard error of the DID coefficient, which captures the effect of the reform on below-median counties. To better illustrate the treatment effects on above-median (or minority autonomous) counties, we estimate separate regressions using flipped definitions of *TRAIT*_{*ij*,2005} (i.e., *TRAIT*_{*ij*,2005} is a below-median or ordinary county indicator). We report the point estimates and the standard errors of the DID coefficients of these separate regressions, which represent treatment effects on above-median (or minority autonomous) counties, in columns 2 and 6.

In response to increased intergovernmental transfers, counties with larger percentages of public employees allocated more funds to the total operational spending of both rural and township public primary schools. As column 7 of panel A shows, counties with larger percentages of public employees were more likely to increase spending rural public schools after central grants began. The heterogeneous treatment effect of 137.36 yuan (with a standard error of 31.07 yuan) was comparable



(a) Budgetary operational spending



(b) Extra-budget operational spending



(c) Total operational spending

Fig. 6. Effects on Gini coefficients of operational spending at the province level. *Notes*: This figure shows estimates from an event study regression. The outcomes are within province Gini coefficients on operational spending measures. All the model details are the same as in Fig. 2.

 Table 5

 Effect of the additional central grants on other government-funded services.

| Dependent | Log (# of Hospital | | Log (# o | of Welfare | Log (# of Telephone | | |
|---------------------|--------------------|----------------|--------------------|----------------|---------------------|----------------|--|
| Variable: | Beds) | | Home Be | eds) | Users) | | |
| Time period: | 2002- 06 (1) | 2002-11 (2) | 2002- 06 (3) | 2002-11 (4) | 2002- 06 (5) | 2002-11 (6) | |
| Policy Effect | -0.01 | 0.01 | 0.00 | -0.06 | 0.00 | -0.01 | |
| | (0.03) | (0.02) | (0.07) | (0.09) | (0.05) | (0.04) | |
| Pre-control mean | 7 | 7 | 6 | 6 | 11 | 11 | |
| Pre-control SD | [1] | [1] | [1] | [1] | [1] | [1] | |
| Observations | 7810 | 15,510 | 7810 | 15,510 | 7810 | 15,510 | |
| Clusters | 1562 | 1551 | 1562 | 1551 | 1562 | 1551 | |

Notes: This table reports difference-in-differences estimates of central grant in the 2006 Chinese Education Finance Reform on other public services outcomes at the county level. See notes of Table 2 for more information. Robust standard errors, reported in parentheses, are clustered by province.

in size to the heterogeneous treatment effects among more economically developed counties, more fiscally abundant counties, and counties that had lower pre-reform student-teacher ratios, a proxy for education quality. Meanwhile, counties with relatively more rural residents spent even *less* on operational spending of rural primary schools. Total spending levels of minority autonomous counties are largely identical to those of ordinary counties.

The heterogeneous treatment effect among counties with more

public employees is even more salient for township primary schools, which most local public employees' children attend. The intergovernmental transfers motivated the above-median counties to spend 93.73 yuan more (with a standard error of 46.64 yuan) in township primary schools than below-median counties, while the effect was small and statistically insignificant, at 37.41 yuan (with a standard error of 77.07 yuan) for the below-median counties. By contrast, as columns 4 and 5 report, richer counties, fiscally more abundant counties, and counties with lower pre-reform student-teacher ratios did not increase spending for township primary schools after they began receiving central grants.

For counties where public employees had more bargaining power, the bias toward township public schools was even more pronounced if we only examine the short-term effect of intergovernmental transfers in 2006, when only western counties received the transfers. As Table A.9 shows, in response to the central grants in 2006, counties with more public employees spent 165.10 yuan more (with a standard error of 63.43 yuan) in township primary schools. However, those counties did not spend more money in rural primary schools. On the other hand, other pre-reform characteristics did not lead to heterogeneous responses in 2006 across either rural or township primary schools.

Focusing on the role of public employment in explaining the heterogeneous effects of the 2006 reform, we further expand the DDD model in Eq. (2) by estimating the heterogeneous effects on each decile of shares of local public employment. Table 7 and Fig. 8 display an increasing trend in the effect of intergovernmental transfers on budgetary and total per-pupil operational spending by decile of relative sizes of public employment. Table 7 reveals that within-decile estimates are largely monotonically increasing: they become positive and

relative to refe

95% CI

95% CI

Policy effect





F

n

. .

-1

_ F

Log of hospital beds

(c) Numbers of welfare home beds

(d) Numbers of landline telephone users

Policy effect

Fig. 7. Effects on all major county-funded social services. Notes: This figure shows estimates from an event study regression. The outcome measures are major county-funded social services. All the model details are the same as in Fig. 2.

statistically significant at the 5th decile (median).³⁸ As the average sizes of the per-pupil minimum spending benchmark (around 300 yuan in 2011 price), the within-decile estimates are close to or even exceed the size of transfers from the 9th decile onward. Counties in the 10th decile matched 20% and 80% of the transfers on their own in budgetary and total operational expenditures. Teachers' salaries and total spending also increased accordingly by 44.5% and 60% from the pre-control means, although the magnitudes are far smaller than those of budgetary and total operational expenditures (96% and 86%). In other words, we observe flypaper or even crowd-in effects among county-level governments above the 80th percentile, as they matched central grants with money from their own pockets. We also observe similar heterogeneities and trends in formal teachers' salaries and total spending, including operational, personnel, and capital spending, across counties with different sizes of public employees.

5. Discussion

5.1. The null effects and the top-down accountability

A closer look at the findings in 3 reveals a consistent causal chain. The influx of intergovernmental transfers induced increased spending in places that the central government monitored, but crowded out existing investments in less noticeable accounts or spending categories,

causing a null effect on total spending levels. First, the additional central grants for school operational spending stimulated an increase of similar amounts in county-level governments' budgetary accounts, which upper-level governments closely monitor and use to evaluate policy implementation. The increase, however, crowded out existing investments of similar sizes in extra-budgetary accounts, which upperlevel governments do not monitor. As a result, additional intergovernmental transfers did not lead to an increase in total spending levels of operational education expenditures. Second, additional intergovernmental transfers did not cause increases (if not decreases) in other fungible spending categories, which were not direct targets of this transfers program. In 2006, when the program began only in western counties, the 150-yuan intergovernmental transfer per student caused a sizable and significant increase in operational school spending (excluding building maintenance, construction, and acquisition), which was the main target of the reform. However, the increase also led to a decrease in expenditure on building construction and acquisition. Over the long term (2006-2011), the reform did not cause any distinguishable rise in total operational spending, total expenditure of building maintenance, or total expenditure of building construction and acquisition. Finally, the additional central grants, which were earmarked for operational spending, did not improve other education spending. They failed to recoup the losses of school revenue from waived tuition. Rather they crowded out personnel spending, such as formal teachers' salaries.

The central government's limited monitoring capabilities caused the null result. As the central government only had timely access to budgetary spending data and focused on budgetary operational spending, county-level governments were incentivized to only increase spending

³⁸ Figure A.6 shows the event study trends for the bottom and top deciles, which confirms that the heterogeneous effects are solely from the post-reform periods.

Heterogeneous treatment effects on total operational spending (per-pupil), 2002-11.

| Dependent Variable: | Budgetary operational spending | | | | Total operational spending | | | | | |
|-----------------------------|--------------------------------|---------------------|-------------------|--------------------|----------------------------|---------------------|-------------------|--------------------|--|--|
| | Below-median (1) | Above-median (2) | Difference (3) | Pre-control (4) | Below-median (5) | Above-median (6) | Difference (7) | Pre-control (8) | | |
| A. Rural Primary Schools | | | | | | | | | | |
| % Public employment | 162.39 | 257.80 | 95.42 | 126 | 21.39 | 158.74 | 137.36 | 304 | | |
| | (51.47) | (53.58) | (26.85) | [138] | (47.30) | (47.97) | (31.07) | [217] | | |
| % Rural population | 247.89 | 172.20 | -75.69 | 230 | 166.33 | 28.59 | -137.74 | 430 | | |
| | (55.84) | (48.98) | (25.61) | [230] | (46.58) | (46.90) | (33.95) | [304] | | |
| GDP per capita | 176.72 | 248.18 | 72.46 | 138 | 32.20 | 188.34 | 156.31 | 310 | | |
| | (49.85) | (45.78) | (30.17) | [150] | (45.60) | (47.98) | (42.69) | [169] | | |
| % Fiscal Revenue of GDP | 177.64 | 206.53 | 28.89 | 146 | 15.79 | 119.76 | 103.97 | 318 | | |
| | (57.50) | (44.17) | (32.81) | [150] | (44.16) | (51.71) | (34.64) | [201] | | |
| Student-teacher ratio | 259.33 | 168.69 | -90.64 | 188 | 172.75 | 26.73 | -146.01 | 390 | | |
| | (47.66) | (50.53) | (26.18) | [181] | (50.75) | (45.13) | (33.52) | [254] | | |
| Minority county | 189.04 | 210.24 | 21.20 | 144 | 60.10 | 85.21 | 25.11 | 328 | | |
| | (49.36) | (64.82) | (50.77) | [154] | (43.83) | (71.90) | (57.70) | [230] | | |
| B. Township Primary Schools | | | | | | | | | | |
| Public employment | 244.04 | 342.00 | 95.60 | 128 | 37.41 | 138.54 | 93.73 | 414 | | |
| | (83.43) | (76.65) | (36.75) | [178] | (77.07) | (61.35) | (46.64) | [310] | | |
| % Rural population | 324.53 | 258.62 | -67.61 | 271 | 107.76 | 59.37 | -53.11 | 642 | | |
| | (77.38) | (81.36) | (38.66) | [358] | (59.81) | (77.66) | (62.70) | [476] | | |
| GDP per capita | 287.83 | 266.22 | -21.25 | 165 | 52.06 | 122.63 | 62.28 | 440 | | |
| | (80.20) | (75.76) | (42.11) | [213] | (70.99) | (70.33) | (56.92) | [284] | | |
| % Fiscal Revenue of GDP | 326.69 | 246.87 | -81.47 | 160 | 59.46 | 88.87 | 24.37 | 469 | | |
| | (85.83) | (79.96) | (73.87) | [234] | (78.00) | (76.48) | (84.92) | [370] | | |
| Student-teacher ratio | 285.94 | 278.66 | -8.74 | 197 | 82.00 | 72.19 | -14.30 | 545 | | |
| | (67.26) | (86.21) | (43.32) | [269] | (55.67) | (75.96) | (49.30) | [399] | | |
| Minority county | 276.95 | 349.29 | 59.30 | 153 | 72.57 | 131.82 | 11.06 | 468 | | |
| | (80.32) | (94.03) | (111.93) | [230] | (67.84) | (94.55) | (97.57) | [373] | | |

Notes: This table reports difference-in-differences-in-differences estimates of central grant in the 2006 Chinese Education Finance Reform on main school per-pupil operational spending outcomes at the county level. We divide counties in to two groups with above and below median traits in 2005. See notes of Table 2 for more information. Robust standard errors, reported in parentheses, are clustered by province.

of the most monitored spending categories in the most monitored account, at the expense of decreases in less monitored ones. According to extant studies on local governments and public goods provision in nondemocracies, the fundamental cause of the under-provision of public goods is lack of incentives. Researchers argue that the top-down accountability between superiors and lower-level governments, the dominant form of political accountability in non-democracies, provides insufficient incentives for subnational political leaders to invest in public goods provision (Landry, 2008; Reuter & Robertson, 2012). Our findings challenge this argument by showing that local political leaders were incentivized to invest in public goods provision as long as the upper-level governments observe their efforts well. The null result aligns with an alternative explanation: upper-level governments have difficulty motivating local governments to provide public goods because upper-level governments have much weaker monitoring capabilities than local residents (Björkman & Svensson, 2009; Fan et al., 2011; Xu, 2011).

5.2. Heterogeneous effects and the bottom-up accountability

As the top-down accountability between Beijing and county-level governments could not motivate counties to allocate transfers properly, 4 systematically investigates the mechanism behind the flypaper and crowd-in effects in some counties. The heterogeneity analysis examines the bottom-up, informal accountability between county-level governments and local public employees, as well as two dominant explanations on public goods provision, economic development and fiscal capacity. We find that the informal accountability between county-level governments and local public employees is likely to be the main mechanism that drives the former to invest in public education. The bottom-up accountability had a spillover effect that counties with more public employees did not only spent more on township primary schools (which public employees' children usually attend) but also rural schools. Economic development, fiscal capacity, pre-reform qualities of education, and relative sizes of ordinary rural residents, however, do not explain the heterogeneous treatment effects among township primary schools or either type of schools.

Our findings thus suggest that in an authoritarian context, in spite of serving only a limited number of "constituencies," the bottom-up accountability outperforms top-down accountability in improving public goods provision and service at the local level. The central government faces an inherent informational problem: even after making costly investments, it cannot monitor local governments' spending and behavior perfectly. As we observe, county-level governments responded to the top-down accountability by only exerting efforts that the superiors can observe, leaving the total level of public goods provision unchanged. But bottom-up accountability had far greater effect. Local informal "constituencies," such as local public employees, do not suffer from the informational problem as the central government experiences. Their families directly benefit from increased spending on public education. As they have both incentives and means to sanction county political leaders for poor implementation of the 2006 reform, stronger accountability between county-level governments and public employees can motivate county political leaders to invest more on education. Our findings are in line with a bourgeoning literature that finds that bottomup accountability can motivate public goods provision if local beneficiaries can monitor and sanction politicians (Andrabi et al., 2017; Björkman & Svensson, 2009; Björkman Nyqvist et al., 2017; Duflo et al., 2015; Pradhan et al., 2014; Serra, 2012).

5.3. Impacts on schools, teachers, and students

The final question of interest is how the education finance reform would have impacted schools, teachers, and students. We supplement the main analyses in 3 and 4 using data from two surveys of elementary schools and middle schools in typical western, central, and eastern

10

95% C



(a) Budgetary operational spending



800

600

200

constant price in 2011 400

(c) Total spending

(d) Full-time teachers' salaries

Docile

Policy effect relative to bottom decile

(b) Total operational spending

Fig. 8. Effects of intergovernmental transfers by public employment size decile. Notes: This figure shows estimates from the DDD regressions by decile of relative sizes of public employment, corresponding the estimates in Table 7. The estimates are the differences between each decile and the bottom decile (interaction effects). All the spending outcomes are measured as per-pupil.

regions in 2007 and 2010, respectively. The detailed results are reported in Section A.5.

The reform affected school management in several ways. The increased budgetary spending from transfer grants and the associated high monitoring level decreased the flexibility of local school spending. However, given that the reform crowded out extra-budgetary spending, about half of the 158 surveyed schools in one central province and one eastern province reported that the expenditure was not sufficient for school operation after the reform. 10%-38% of schools experienced a decrease in school spending.

We have shown that, on average, the reform had decreased teacher salaries across the country in both the short term and longer term. Data from a survey of 1388 teachers suggests that, particularly in economically disadvantaged areas, many teachers experienced a drop in real incomes. Counties with poor education finance conditions had to allocate their limited revenues across expenditure items. When being required to increase operational spending, cutting teacher salaries might have been the county governments' preferred policy choice.

Panel E of Table 7 suggests that in counties with large increases in school spending, student enrollment also increased. Survey data show that the increased school enrollment might be from three sources: students who previously dropped out came back to school; students who previously attended private schools transferred to public schools; and students who previously attended urban schools transferred back to rural schools. However, the decrease in dropout rate could be caused by a combined effect of both tuition waiver and (possible) increased school spending. Combining school-level dropout rate data and county-level spending data, Table A.6 suggests a negative correlation between

school spending and student dropout. This finding is consistent with the national estimates.

A series of policy programs that aim to improve education in both developed and developing countries have focused on increasing the financial resources to public schools. Researchers and policymakers have long debated over whether school spending can improve student outcomes in general. Recent research in the United States has mostly supported a strong empirical relationship between increased school spending and student outcomes (Hyman, 2017; Jackson et al., 2016; Lafortune et al., 2018). By substantiating the null effects of intergovernmental transfers in non-electoral contexts and under non-perfect monitoring, we contribute to the understanding of school spending, a strong predictor of education provision and student outcomes. Still, the cutting-edge causal literature on education spending is still puzzled by why some funding programs lead to positive impacts while some do not (Jackson, 2018, p.13). Our finding about the large policy impact heterogeneity on school spending reveals the mechanism behind why some areas outperform others in the reform. We provide new insights on conditions that facilitates intergovernmental transfer programs in education finance: bottom-up accountability, even informal, can improve allocation of financial resources in education. The findings particularly complement a series of studies about failures in education finance (Gordon, 2004; Reinikka & Svensson, 2004).

6. Conclusion

The 2006 Chinese Education Finance Reform introduced one of the largest fiscal transfers programs in the history of education finance in

Heterogeneous treatment effects by percentage of public employment decile, 2002-11.

| Relative size decile: | 1st (1) | 2nd (2) | 3rd (3) | 4th (4) | 5th (5) | 6th (6) | 7th (7) | 8th (8) | 9th (9) | 10th (10) |
|---|-------------------|------------|------------|------------|------------|------------|------------|------------|------------|--------------|
| A. Budgetary Operational Spending (per-pupil) | | | | | | | | | | |
| Policy Effect | 137.95 | 31.83 | 13.52 | 21.39 | 58.89 | 90.77 | 69.22 | 124.79 | 174.60 | 357.64 |
| | (60.08) | (30.70) | (37.65) | (46.79) | (35.28) | (43.67) | (49.89) | (46.53) | (66.00) | (130.26) |
| Pre-control mean | 98 | 118 | 118 | 150 | 157 | 144 | 235 | 243 | 256 | 371 |
| Pre-control SD | [120] | [143] | [104] | [148] | [175] | [141] | [201] | [195] | [253] | [301] |
| B. Total Operational Spen | ding (per-pupil) | | | | | | | | | |
| Policy Effect | -24.20 | 80.47 | 43.11 | 47.71 | 83.19 | 130.58 | 132.13 | 189.02 | 267.72 | 542.27 |
| | (44.71) | (39.18) | (31.35) | (46.01) | (33.13) | (37.70) | (50.33) | (47.80) | (82.50) | (153.20) |
| Pre-control mean | 274 | 293 | 293 | 318 | 354 | 343 | 440 | 441 | 499 | 630 |
| Pre-control SD | [140] | [253] | [244] | [182] | [247] | [205] | [256] | [241] | [392] | [416] |
| C. Total Spending (per-pu | pil) | | | | | | | | | |
| Policy Effect | -664.11 | 239.30 | 255.91 | 299.61 | 393.68 | 439.44 | 688.76 | 850.87 | 1281.32 | 1974.54 |
| | (160.91) | (128.44) | (100.82) | (136.98) | (145.49) | (142.14) | (173.41) | (179.97) | (266.18) | (533.80) |
| Pre-control mean | 1398 | 1611 | 1530 | 1588 | 1942 | 1947 | 2204 | 2440 | 2364 | 3304 |
| Pre-control SD | [473] | [946] | [1148] | [649] | [802] | [1015] | [1108] | [869] | [919] | [1313] |
| D. Full-time Teachers' Sal | aries (per-pupil) | | | | | | | | | |
| Policy Effect | -564.07 | 129.25 | 191.61 | 223.98 | 271.71 | 276.68 | 510.02 | 583.95 | 863.57 | 1165.04 |
| | (114.11) | (102.41) | (95.34) | (104.38) | (120.71) | (124.78) | (138.96) | (131.16) | (203.85) | (474.00) |
| Pre-control mean | 1086 | 1285 | 1212 | 1230 | 1514 | 1541 | 1713 | 1951 | 1807 | 2614 |
| Pre-control SD | [396] | [746] | [900] | [537] | [553] | [809] | [863] | [722] | [705] | [1057] |
| E. # of Students in Public | Schools | | | | | | | | | |
| Policy Effect | 0.07 | -0.02 | 0.01 | 0.02 | -0.01 | 0.04 | 0.01 | 0.02 | 0.05 | 0.05 |
| | (0.03) | (0.02) | (0.02) | (0.01) | (0.02) | (0.03) | (0.02) | (0.03) | (0.03) | (0.03) |
| Pre-control mean | 11 | 11 | 11 | 11 | 10 | 10 | 10 | 10 | 10 | 9 |
| Pre-control SD | [0] | [0] | [0] | [1] | [0] | [0] | [1] | [0] | [1] | [1] |
| Observations | 1588 | 1594 | 1602 | 1610 | 1616 | 1597 | 1607 | 1600 | 1608 | 1541 |
| Clusters (Counties) | 161 | 161 | 162 | 161 | 162 | 161 | 162 | 161 | 162 | 161 |

Notes: This table reports difference-in-differences estimates of central grant in the 2006 Chinese Education Finance Reform on main school per-pupil operational spending outcomes at the county level. We divide counties in to 10 decile groups based on traits in 2005. Column (1) reports the DID (main) effect on the 1st decile counties, columns (2)-(10) reports the DDD effects relative to the 1st decile. See notes of Table 2 for more information. Robust standard errors, reported in parentheses, are clustered by province.

the world. The reform was intended to increase funding on rural and township public schools' operational expenditures through intergovernmental transfers. This paper used a confidential and unique sample of itemized education finance data from around 1600 Chinese county-level governments that received intergovernmental transfers at varying levels and different beginning years, in order to test whether intergovernmental transfers from the central government caused increases in rural and township public schools' operational spending. We find that the intergovernmental transfers caused sizable increases in per-pupil budgetary operational spending of rural or township primary schools, which were closely monitored by the central government as an indicator of policy implementation. But the transfers also crowded out pre-existing funds of similar sizes in the ill-monitored extra-budgetary accounts. As county governments' accountability to the central government is curtailed by the latter's limited monitoring capabilities, the reform did not cause any significant increase in per-pupil operational spending of rural or township primary schools. By examining other outcomes, we also find that county-level governments did not channel the intergovernmental transfers to other fungible spending categories, recoup the losses of school revenue caused by waived tuition, fund school teachers' salary bills, or improve other county-funded public social services. The null result suggests that most counties used the transfers to cover public payroll. However, the reform caused heterogeneous responses among county-level governments. We find that counties where public employees can hold local political leaders accountable increased their overall spending on rural and township primary schools.

Our findings complement the literature on intergovernmental transfers by documenting the null effects of transfers. More importantly, our results imply that the failure was mainly due to upper-level governments' limited monitoring capabilities. Our results of crowding out effects are consistent with similar findings from the research of Title I (Cascio & Reber, 2013; Gordon, 2004) and Medicaid

(Baicker & Staiger, 2005), suggesting that authoritarian local governments may behave much like democratically elected ones that are under weak accountability and regulations. Meanwhile, the heterogeneity in policy effects reveals that in spite of being skewed and imperfect, the informal, bottom-up accountability between county political leaders and local public employees could incentivize county-level governments to allocate central grants optimally and improve qualities of public education. This insight suggests that as long as local beneficiaries have access to information and the ability to sanction politicians, they are more capable of monitoring and motivating public goods provision than authorities at higher levels.

CRediT authorship contribution statement

Yanqing Ding: Funding acquisition, Data curation, Writing - original draft. Fengming Lu: Conceptualization, Methodology, Data curation, Writing - original draft. Xiaoyang Ye: Conceptualization, Methodology, Data curation, Writing - original draft.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.econedurev.2020.101985.

References

- Aghion, P., Jaravel, X., Persson, T., & Rouzet, D. (2019). Education and military rivalry. Journal of the European Economic Association, 17(2), 376–412.
- Andrabi, T., Das, J., & Khwaja, A. I. (2017). Report cards: The impact of providing school and child test scores on educational markets. *American Economic Review*, 107(6), 1535–1563.
- Baicker, K., & Gordon, N. (2006). The effect of state education finance reform on total local resources. *Journal of Public Economics*, 90(8-9), 1519–1535.
- Baicker, K., & Staiger, D. (2005). Fiscal shenanigans, targeted federal health care funds, and patient mortality. *Quarterly Journal of Economics*, 120(1), 345–386.

- Banerjee, A., Deaton, A., & Duflo, E. (2004). Wealth, health, and health service delivery in rural Rajasthan. *American Economic Review*, *94*(2), 326–330.
- Banerjee, A., & Duflo, E. (2006). Addressing absence. Journal of Economic Perspectives, 20(1), 117–132.
- Bardhan, P. (2002). Decentralization of governance and development. Journal of Economic Perspectives, 16(4), 185–205.
- Bardhan, P., & Mookherjee, D. (2006). Pro-poor targeting and accountability of local governments in West Bengal. *Journal of Development Economics*, 79(2), 303–327.
- Björkman, M., & Svensson, J. (2009). Power to the people: Evidence from a randomized field experiment on community-based monitoring in Uganda. *Quarterly Journal of Economics*, 124(2), 735–769.
- Björkman Nyqvist, M., de Walque, D., & Svensson, J. (2017). Experimental evidence on the long-run impact of community-based monitoring. *American Economic Journal: Applied Economics*, 9(1), 33–69.
- Blumenthal, D., & Hsiao, W. (2005). Privatization and its discontents the evolving Chinese health care system. New England Journal of Medicine, 353(11), 1165–1170.
- Brollo, F., Nannicini, T., Perotti, R., & Tabellini, G. (2013). The political resource curse. American Economic Review, 103(5), 1759–1796.
- Bursztyn, L. (2016). Poverty and the political economy of public education spending: Evidence from Brazil. *Journal of the European Economic Association*, 14(5), 1101–1128.
- Campos, J. E., & Hellman, J. S. (2005). Governance gone local: Does decentralization improve accountability? In W. Bank (Ed.). *East Asia decentralizes: Making local government work* (pp. 237–252). World Bank Publications.
- Candelaria, C. A., & Shores, K. A. (2019). Court-ordered finance reforms in the adequacy era: Heterogeneous causal effects and sensitivity. *Education Finance and Policy*, 14(1), 31–60.
- Card, D., & Payne, A. A. (2002). School finance reform, the distribution of school spending, and the distribution of student test scores. *Journal of Public Economics*, 83(1), 49–82.
- Cascio, E. U., Gordon, N., & Reber, S. (2013). Local responses to federal grants: Evidence from the introduction of title I in the south. *American Economic Journal: Economic Policy*, 5(3), 126–159.
- Cascio, E. U., & Reber, S. (2013). The poverty gap in school spending following the introduction of title I. American Economic Review, 103(3), 423–427.
- Cascio, E. U., & Washington, E. (2014). Valuing the vote: The redistribution of voting rights and state funds following the voting rights act of 1965. *Quarterly Journal of Economics*, 129(1), 379–433.
- Chyi, H., & Zhou, B. (2014). The effects of tuition reforms on school enrollment in rural China. Economics of Education Review, 38, 104–123.
- Courant, P. N., & Loeb, S. (1997). Centralization of school finance in Michigan. Journal of Policy Analysis and Management, 16(1), 114–136.
- Dahlberg, M., Mörk, E., Rattsø, J., & Ågren, H. (2008). Using a discontinuous grant rule to identify the effect of grants on local taxes and spending. *Journal of Public Economics*, 92(12), 2320–2335.
- DiNardo, J., Fortin, N. M., & Lemieux, T. (1996). Labour market institutions and the distribution of wages, 1973-1992: A semiparametric approach. *Econometrica*, 64(5), 1001–1044.
- Ding, Y. (2012). The problems with access to compulsory education in China and the effects of the policy of direct subsidies to students: An empirical study based on a small sample. *Chinese Education & Society*, *45*(1), 13–21.
- Dixit, A., & Londregan, J. (1998). Fiscal federalism and redistributive politics. Journal of Public Economics, 68(2), 153–180.
- Duflo, E., Dupas, P., & Kremer, M. (2015). School governance, teacher incentives, and pupil-teacher ratios: Experimental evidence from Kenyan primary schools. *Journal of Public Economics*, 123, 92–110.
- Eckaus, R. S. (2003). Some consequences of fiscal reliance on extrabudgetary revenues in China. *China Economic Review*, 14(1), 72–88.
- Edin, M. (2003). State capacity and local agent control in China: CCP cadre management from a township perspective. *China Quarterly*, 173, 35–52.
- Fan, S., Kanbur, R., & Zhang, X. (2011). China's regional disparities: Experience and policy. *Review of Development Finance*, 1(1), 47–56.
- Fernández, R., & Rogerson, R. (1998). Public education and income distribution: A dynamic quantitative evaluation of education-finance reform. *American Economic Review*, 88(4), 813–833.
- Fernández, R., & Rogerson, R. (1999). Education finance reform and investment in human capital: Lessons from California. *Journal of Public Economics*, 74(3), 327–350.
- Gordon, N. (2004). Do federal grants boost school spending? Evidence from title I. Journal of Public Economics, 88(9-10), 1771–1792.
- Guryan, J. (2001). Does money matter? Regression-discontinuity estimates from education finance reform in Massachusetts. NBER Working Paper No. 8269.
- Hanushek, E. A. (2003). The failure of input-based schooling policies. *Economic Journal*, 113(485), F64–F98.
- Hanushek, E. A. (2006). School resources. In E. A. Hanushek, & F. Welch (Vol. Eds.), Handbook of the economics of education. 2Elsevier.
- Hawkins, J. N. (2000). Centralization, decentralization, recentralization: Educational reform in China. Journal of Educational Administration, 38(5), 442–454.
- Hinnerich, B. T., & Pettersson-Lidbom, P. (2014). Democracy, redistribution, and political participation: Evidence from Sweden 1919-1938. *Econometrica*, 82(3), 961–993. Hoxby, C. M. (2001). All school finance equalizations are not created equal. *Quarterly*
- *Journal of Economics*, 116(4), 1189–1231. Husted, T. A., & Kenny, L. W. (1997). The effect of the expansion of the voting franchise
- on the size of government. *Journal of Political Economy*, 105(1), 54–82. Hyman, J. (2017). Does money matter in the long run? Effects of school spending on
- educational attainment. *American Economic Journal: Economic Policy*, *9*(4), 256–280. Jackson, C. K. (2018). Does school spending matter? The new literature on an old

question. NBER Working Paper No. 25368.

- Jackson, C. K., Johnson, R. C., & Persico, C. (2016). The effects of school spending on educational and economic outcomes: Evidence from school finance reforms. *Quarterly Journal of Economics*, 131(1), 157–218.
- de Janvry, A., Finan, F., & Sadoulet, E. (2012). Local electoral incentives and decentralized program performance. *Review of Economics and Statistics*, 94(3), 672–685.
- Jin, H., Qian, Y., & Weingast, B. R. (2005). Regional decentralization and fiscal incentives: Federalism, Chinese style. *Journal of Public Economics*, 89(9-10), 1719–1742.
- Köthenbürger, M. (2011). How do local governments decide on public policy in fiscal federalism? Tax vs. expenditure optimization. *Journal of Public Economics*, 95(11-12), 1516–1522.
- Lafortune, J., Rothstein, J., & Schanzenbach, D. W. (2018). School finance reform and the distribution of student achievement. *American Economic Journal: Applied Economics*, 10(2), 1–26.
- Landry, P. F. (2008). Decentralized authoritarianism in China. New York: Cambridge University Press.
- Li, W., Park, A., & Wang, S. (2007). School equity in rural China. In E. Hannum, & A. Park (Eds.). Education and reform in China. New York: Routledge.
- Lin, S. (2000). The decline of China's budgetary revenue: Reasons and consequences. Contemporary Economic Policy, 18(4), 477–490.
- Lindert, K., Skoufias, E., & Shapiro, J. (2006). Redistributing income to the poor and the rich: Public transfers in latin America and the Caribbean. Social safety nets primer series no. 605. Washington, D.C.: World Bank.
- Litschig, S., & Morrison, K. M. (2013). The impact of intergovernmental transfers on education outcomes and poverty reduction. *American Economic Journal: Applied Economics*, 5(4), 206–240.
- Liu, M., Murphy, R., Tao, R., & An, X. (2009). Education management and performance after rural education finance reform: Evidence from western China. *International Journal of Educational Development*, 29(5), 463–473.
- Liu, M., Zhang, D., Qian, T., & Zhang, Q. (2013). Informal power structure and economic policy of local government in China. Sociological Studies, 28(5), 26–52.
- Lott, J. R. Jr., & Kenny, L. W. (1999). Did women's suffrage change the size and scope of government. *Journal of Political Economy*, 107(6), 1163–1198.
- Lü, X. (2011). The political causes and consequences of inequality of opportunity. Yale University Ph.D. thesis.
- Lü, X. (2014). Social policy and regime legitimacy: The effects of education reform in China. American Political Science Review, 108(2), 423–437.
- Lü, X. (2015). Intergovernmental transfers and local education provision Evaluating China's 8-7 national plan for poverty reduction. China Economic Review, 33, 200–211.
- Luo, R., Zhang, L., Huang, J., & Rozelle, S. (2010). Village elections, public goods investments and pork barrel politics, Chinese-style. *Journal of Development Studies*, 46(4), 662–684.
- Lutz, B. (2010). Taxation with representation: Intergovernmental grants in a plebiscite democracy. *Review of Economics and Statistics*, 92(2), 316–332.
- Martínez-Bravo, M., Padró i Miquel, G., Qian, N., & Yao, Y. (2017). The rise and fall of local elections in China: Theory and empirical evidence on the autocrat's trade-off. NBER Working Paper No. 24032.
- Martínez-Bravo, M., Mukherjee, P., & Stegmann, A. (2017). The non-democratic roots of elite capture: Evidence from soeharto mayors in Indonesia. *Econometrica*, 85(6), 1991–2010
- Maskin, E., Qian, Y., & Xu, C. (2000). Incentives, information, and organizational form. *Review of Economic Studies*, 67(2), 359–378.
- Meng, L. (2013). Evaluating China's poverty alleviation program: A regression discontinuity approach. Journal of Public Economics, 101, 1–11.
- Miller, G. (2008). Women's suffrage, political responsiveness, and child survival in American history. Quarterly Journal of Economics, 123(3), 1287–1327.
- Mulligan, C. B., Gil, R., & Sala-i-Martín, X. (2004). Do democracies have different public policies than nondemocracies? *Journal of Economic Perspectives*, 18(1), 51–74.
- Murray, S. E., Evans, W. N., & Schwab, R. M. (1998). Education-finance reform and the distribution of education resources. *American Economic Review*, 88(4), 789–812.
- Musgrave, R. A. (1959). The theory of public finance: A study in political economy. New York: McGraw-Hill.
- Nikolova, E., & Marinov, N. (2017). Do public fund windfalls increase corruption? Evidence from a natural disaster. *Comparative Political Studies*, 50(11), 1455–1488.
- Oates, W. E. (1972). Fiscal Federalism. New York: Harcourt, Brace, Jovanovich.
- Oates, W. E. (1999). An essay on fiscal federalism. Journal of Economic Literature, 37(3), 1120–1149.
- Olken, B. A. (2010). Direct democracy and local public goods: Evidence from a field experiment in Indonesia. *American Political Science Review, 104*(2), 243–267.
- Pradhan, M., Suryadarma, D., Beatty, A., Wong, M., Gaduh, A., Alisjahbana, A., & Prama Artha, R. (2014). Improving educational quality through enhancing community participation: Results from a randomized field experiment in Indonesia. *American Economic Journal: Applied Economics*, 6(2), 105–126.
- Reinikka, R., & Svensson, J. (2004). Local capture: Evidence from a central government transfer program in Uganda. Quarterly Journal of Economics, 119(2), 679–705.
- Reuter, O. J., & Robertson, G. B. (2012). Subnational appointments in authoritarian regimes: Evidence from russian gubernatorial appointments. *Journal of Politics*, 74(4), 1023–1037.
- Rodden, J. A. (2006). *Hamilton's paradox: The promise and peril of fiscal federalism.* New York: Cambridge University Press.
- Serra, D. (2012). Combining top-down and bottom-up accountability: Evidence from a bribery experiment. Journal of Law, Economics, & Organization, 28(3), 569–587.
- Sharma, A., & Cárdenas, O. J. (2008). Education spending and fiscal reform in Mexico. Journal of International and Global Economic Studies, 1(2), 112–127.
- Shi, X. (2012). Does an intra-household flypaper effect exist? Evidence from the

educational fee reduction reform in rural China. Journal of Development Economics, 99(2), 459–473.

Stasavage, D. (2005). Democracy and education spending in Africa. American Journal of Political Science, 49(2), 343–358.

- The State Council of China (2011). A research report on rural comprehensive reform: 2006-2009. Beijing: China Financial & Economic Publishing House.
- Treisman, D. (2007). The architecture of government: Rethinking political decentralization. New York: Cambridge University Press.
- Tsai, L. L. (2007). Solidary groups, informal accountability, and local public goods provision in rural China. American Political Science Review, 101(2), 355–372.

Tsang, M. C., & Ding, Y. (2005). Resource utilization and disparities in compulsory education in China. *China Review*, 5(1), 1–31.

Tsui, K.-y., & Wang, Y. (2004). Between separate stoves and a single menu: Fiscal decentralization in China. *China Quarterly*, 177, 71–90.

Wang, R. (2008). Reform of the rural compulsory education assured funding mechanism:

Policy design perspective. Chinese Education and Society, 41(1), 9-16.

- Xiao, Y., Li, L., & Zhao, L. (2017). Education on the cheap: The long-run effects of a free compulsory education reform in rural China. *Journal of Comparative Economics*, 45(3), 544–562.
- Xu, C. (2011). The fundamental institutions of China's reforms and development. Journal of Economic Literature, 49(4), 1076–1151.
- Xu, Y., & Yao, Y. (2015). Informal institutions, collective action, and public investment in rural China. American Political Science Review, 109(2), 371–391.
- Zhang, L.-Y. (1999). Chinese central-provincial fiscal relationships, budgetary decline and the impact of the 1994 fiscal reform: An evaluation. *China Quarterly*, 157, 115–141.
 Zhang, X., Fan, S., Zhang, L., & Huang, J. (2004). Local governance and public goods
- provision in rural China. *Journal of Public Economics*, 88(12), 2857–2871. Zhao, L. (2009). Between local community and central state: Financing basic education in
- Zhao, L. (2009). Between local community and central state: Financing basic education in China. International Journal of Educational Development, 29(4), 366–373.