China Economic Review 33 (2015) 200–211

Contents lists available at ScienceDirect

China Economic Review

Intergovernmental transfers and local education provision — Evaluating China's *8-7 National Plan for Poverty Reduction*☆

Xiaobo LÜ*

Department of Government, Campus mail code: A1800, University of Texas, Austin, TX 78712, United States

ARTICLE INFO

Article history:
Received 23 April 2014
Received in revised form 3 February 2015
Accepted 3 February 2015
Available online 12 February 2015

Keywords:
Intergovernmental transfers
Poverty relief
Education spending
China

ABSTRACT

Intergovernmental transfers are an important source of local public goods and services provision in many developing countries. Yet the empirical evidence on their effectiveness remains inconclusive partly because transfers are endogenous to political influence. This paper investigates the impact of a mix of intergovernmental transfers from a large-scale poverty relief program on local education spending in China between 1994 and 2000. Using a fuzzy regression discontinuity design, I first show no systematic evidence that counties benefiting from the program enhanced local education spending during the period of program implementation. I further show that the program has neither short-term nor long-term impacts on illiteracy reduction for the targeted counties.

1. Introduction

Decentralization has been advocated as an important institutional design to enhance economic development in many developing countries (Bardhan, 2002; World Bank, 2000, 2004). Due to weak local fiscal capacity and regional variation in economic conditions, the provision of public goods and services, such as education and healthcare, is often unequal across different localities. Intergovernmental transfers from the national government (i.e., transfers of fiscal resource from the higher-level government to local governments), then become an important policy tool for reducing regional disparities in local public goods and services provision (e.g., Card & Payne, 2002; Dixit & Londregan, 1998; Khemani, 2007a). Further, transfers are an important feature of fiscal federalism because national governments often use them as the means to achieve policy objectives that are not prioritized by local governments (e.g., Rodden, 2006; Treisman, 2007).

Yet scholars remain skeptical about the effectiveness of intergovernmental transfers to achieve these objectives because of the distortion and inefficiency of transfers. The first concern is local elite capture and corruption resulting from weak local accountability (Bardhan, 2002; Bardhan & Mookherjee, 2006). Even if local accountability is strong, there remains a second concern that local governments have incentives to use transfers for achieving their own objectives rather than the transfers' originally intended purposes. For example, incumbents in local governments may use transfers to advance their political aims. Thus, the transfers either

☆ I greatly appreciate Ken Scheve, Pierre Landry, Frances Rosenbluth, Thad Dunning, Lianjiang Li, Jean Hung, Shukai Zhao, Yang Bonny Lin, Yi Kang, Brian Fried, Allison Sovey, Atheendar Venkataramani, Valerie Frey, Alex Debs, Eric Weese, Nikolay Marinov, Ana De La O, the editor and three anonymous reviewers at the *China Economics Review*, as well as participants at the Comparative Politics Workshop and Leitner Political Economy Workshop at Yale and the BOCD Workshop at the Chinese University of Hong Kong for helpful comments and suggestions at various stages of the research project. I am also very grateful to Albert Park and Sangui Wang for providing the poverty county data, and the BOCD project at USC (CUHK) for the county education spending data. I received financial support from the Council of East Asian Studies and the MacMillan Center (Mun Yew Chung Fellowship) at Yale University, and the NSF Dissertation Improvement Grant (SES-0819263), for which I am deeply grateful. All errors remain my own.

* Tel.: +1 512 232 7257.
E-mail address: xiaobolu@austin.utexas.edu.

http://dx.doi.org/10.1016/j.chieco.2015.02.001
1043-951X/

become fungible for spending in unintended areas or offer partial tax reliefs for local residents as a means to win votes (Feyzioglu, Swaroop, & Zhu, 1998; van de Walle & Mu, 2007).

The empirical evidence on the effectiveness of intergovernmental transfers on local public goods provision remains mixed. For instance, some studies suggest that central government's fiscal transfers could be effective in improving local education provision. Litschig and Morrison (2013), using a regression discontinuity design, find that transfers from Brazil's central government increased local government spending on education, increased school enrollments and reduced the poverty rate. On the other hand, studies have shown that transfers for local education provision are often subject to corruption and leakages. For instance, Reinikka and Svensson (2004) show that only 13% of the intergovernmental education grants reached local schools in Uganda between 1991 and 1995. Ferraz, Finan, and Moreira (2012) argue that corruption is the main factor undermining education grant transfers to local governments in Brazil.

The mixed evidence on the effectiveness of intergovernmental transfers is in part due to the fact that such transfers are usually not randomly assigned. In the absence of random assignment, researchers may be unable to identify unbiased estimates of transfers by merely controlling for observable factors that appear to be correlated with the transfers and the provision of local goods. Specifically, intergovernmental transfers are endogenous to many observed and unobserved political factors. The endogeneity problem consequently creates challenges to identify the causal effects of intergovernmental transfers on local public goods and services provision. For example, politicians may prefer to allocate funding to more affluent places in order to target rich constituents, which tend to have better education outcomes. Or they might find political value in directing resources to less affluent places to cultivate certain voters' loyalties. After all, political parties at the national level have a great interest in extending their influence at the local level, whether rewarding the core supporters or appealing to swing voters. Hence, political factors have a profound impact on the distribution of intergovernmental transfers (e.g., Cox & McCubbins, 1986; Lindbeck & Weibull, 1987). Researchers have unveiled consistent evidence for the political influence of intergovernmental transfers in many countries, such as Argentina (Gervasoni, 2010), Brazil (Brollo & Nannicini, 2012), India (Khemani, 2007b), Spain (Solé-Ollé & Sorribas-Navarro, 2008), and the United States (Grossman, 1994; Levitt & Snyder, 1995), just to name a few.

One could argue that the political influence on intergovernmental transfers is equally salient in nondemocratic regimes, largely because rulers are interested in distributing public spending as means to maintain regime stability. Whether the politically motivated public spending is welfare-enhancing in nondemocratic regimes remains an open question, because existing empirical evidence of the effectiveness of transfers is largely based on studies of democratic regimes. To address this question, I focus on the effects of transfers on one particular type of local public service (education) in a nondemocratic regime (China). Specifically, I make use of a dataset that contains all counties in China between 1994 and 2000, and employ a fuzzy regression discontinuity design as the identification strategy. The key to this identification strategy rests on the design of a large-scale poverty relief program (*The 8-7 National Plan for Poverty Reduction* or hereafter *8-7 Plan*) initiated by the Chinese government in 1994, which transferred fiscal resources to counties designated as a "National Poverty County" for economic development. The *8-7 Plan* used 1992 rural income per capita to determine a threshold below which a county would be designated a "National Poverty County." Between 1994 and 2000, a county with this status received larger intergovernmental transfers from upper-level governments. Because this designation process was imperfect, a fuzzy regression discontinuity design is useful to evaluate the causal effects of the *8-7 Plan* on education provision by targeted counties.

This paper shares the same empirical strategy used by Meng (2013) to evaluate the impact of the *8-7 Plan* in China. Although Meng (2013) finds that the *8-7 Plan* significantly improved rural income, I do not find any long-term or short-term systematic evidence to suggest that targeted counties generally had greater spending on education than the non-targeted counties; my findings contrast with the positive picture painted in the government report on the *8-7 Plan*'s outcomes.[1] Furthermore, I evaluate the short-term or long-term impacts of the program on illiteracy reduction, and I do not find any evidence that the targeted counties achieved better short-term or long-term outcomes in this aspect. My main results remain robust with different model specifications and alternative measures of the dependent variable.

The findings in this paper cast doubt on the perception held by many researchers and policymakers that intergovernmental transfer from the central and provincial governments is the panacea to address the problem of under-provision of local public goods and services in China. In particular, many scholars have long called for greater intergovernmental transfers from the central and/or provincial governments to local governments in order to enhance education spending in China (Tsang, 2002; World Bank, 2002; Yuan, 2005). Their reasoning is that the 1994 fiscal reform, the Tax Sharing Scheme (TSS hereafter), has re-centralized local governments' fiscal resources while de-centralizing spending on public goods and services, thereby limiting the available fiscal resources for local public goods and services provision. While these scholars may be correct in asserting that TSS has limited local governments' ability to adequately fund education systems, my research questions the effectiveness of intergovernmental transfers as a method for addressing those inadequacies, particularly in these targeted poor counties. Without creating mechanisms to align local governments' incentives in public goods provision, any increase in intergovernmental transfers, whether general transfers or targeted transfers, could remain ineffective because local governments have incentives to prioritize spending in other areas.

Proceeding from here, Section 2 discusses the *8-7 Plan* that led to more transfers to some counties in China, and my theoretical expectations of the effects of the transfers. Section 3 details the empirical strategy to evaluate the causal effects of intergovernmental transfers on local education spending, and Section 4 discusses the main results. In Section 5, I provide further evidence about the short-term and long-term impacts of the program on illiteracy reduction. I conclude the paper by discussing its implications in Section 6.

[1] See more details in The National Bureau of Statistics, Division of Rural Social and Economic Survey (Guojia Tongji ju Nongchun Shehui Jingji Diaocha Si), ed. 2006. *Report on Rural Poverty Monitoring in China — 2005 (Zhongguo Nongchun Pingkun Jiance Baogao — 2005)*. Beijing: China Statistics Press.

2. The *8-7 National Plan for Poverty Reduction* and local education spending

The Chinese central government launched the *8-7 National Plan for Poverty Reduction* in 1994 in the hope of lifting 80 million citizens above the poverty line. The plan designated 592 counties as National Poverty Counties and used various targeted transfer programs to alleviate poverty. The plan was implemented between 1994 and 2000, and was replaced by the *Rural Poverty Relief and Development Plan* that has directly targeted poor villages since 2001. This is one of the largest poverty relief programs in the world, and previous studies have shown that the *8-7 Plan* had some success in improving income and consumption for residents in the targeted regions (Meng, 2013; Park, Wang, & Wu, 2002; Wang, Li, & Ren, 2004).

In particular, the 1994 *8-7 Plan* aimed to correct the mis-targeting of an earlier poverty relief effort by the central government in 1986 (Park et al., 2002; World Bank, 2000). To identify poor counties based on objective criteria, the Chinese central government used the 1992 rural income per capita of the county as the main criterion for the "National Poverty County" designation in 1994. The *8-7 Plan* stipulated that any county with rural income per capita below RMB 400 in 1992 was to be designated as a "National Poverty County." Owing to various political pressures, the central government also decided that any county that was designated as a "National Poverty County" prior to 1994 but whose rural income per capita was below RMB 700 could keep the designation status. As a result, 592 counties across 27 provinces were designated as National Poverty Counties, a status that did not change until 2001.[2]

One might argue that the primary objective of the transfers from the *8-7 Plan* was for rural development instead of local education spending. However, the designation of "National Poverty County" generated various fiscal benefits to local governments' education spending. First, although the funding allocated to education and health was a relatively small share (around 3%) of the total poverty relief fund, it still amounted to approximately RMB 3 billion over the course of the plan. Second and more importantly, the central government also launched the *Compulsory Education in National Poor Regions Project* in 1995, which aimed to improve infrastructure and teacher quality for schools at the compulsory education level, specifically in National Poverty Counties. The Chinese government invested approximately RMB 12.4 billion in this project between 1995 and 2000 (Zhang, 2007). If we divide the total funding from the *8-7 Plan* and the *Compulsory Education in National Poor Regions Project* for these 592 counties in a span of six years (1995–2000), the education related transfers provide on average RMB 4.3 million per year to each designated national poverty county, amounting to approximately 12% of total education spending in these designated poverty counties.

In addition, provincial governments provided financial assistance to these designated poverty counties to enhance their education spending. A survey of 12 selected provinces[3] where the *Compulsory Education in National Poor Regions Project* was implemented suggests that the total investment amounted to RMB 5.2 billion in these provinces (Ministry of Education, 1997). Further, approximately 70% to 80% of the funding from the *Project* went to building of new schools, renovating existing school buildings, and purchasing educational equipment.

Although there are complicating factors, the education funds allocated to local governments through the *8-7 Plan* may be considered intergovernmental transfers, thus offering researchers a unique opportunity to evaluate such transfers' effectiveness on local governments' education spending. In support of this characterization, I offer several observations. First, the discussion above suggests that "National Poverty Counties" received significantly greater fiscal transfers from various government channels that aimed to enhance local governments' education spending. This observation is also consistent with patterns of fiscal transfers in my data discussed below. Second, because at least part of the intergovernmental transfers these counties received resulted from the *8-7 Plan*, the income criterion for the plan's poverty county designation allows us to address the endogeneity issue embedded in the allocation process through the fuzzy regression discontinuity identification strategy. Third, although some might argue that the *8-7 Plan* was far more complicated than intergovernmental transfer because it involved major government investment in infrastructure (e.g., road construction, electricity and water projects, as well as introduction of high-yield crops and agricultural technology), intergovernmental transfer was a major component of this program and few studies evaluate its effectiveness.

In fact, it remains theoretically ambiguous whether these transfers through the *8-7 Plan* actually induced greater total local education spending by the government. One could argue that the designation of "National Poverty County" should have enhanced local education spending in these poor counties primarily for two reasons. First, the specific fiscal transfers to improve the education infrastructure would increase the education-related consumption by local government. Second, there is an income effect, as the budgetary grant program enhanced the fiscal resources of local governments, thus allowing them to consume more of various types of public goods and services such as education. One could even contend that the poverty relief program had a broader impact on local education provision, because an improvement in local household income could enhance children's school attendance and mass demand for education.

Nonetheless, others remain skeptical whether intergovernmental transfers are an effective way to enhance local education spending in non-democratic regimes. First, local governments may have different objectives in government spending. Social spending, such as education spending, was not prioritized by most local governments during the 1990s in China (Jia, Guo, & Zhang, 2014). Second, intergovernmental transfers may not necessarily enhance local public spending because the costs of monitoring and auditing are high. Taken together, one could argue that intergovernmental transfers have no effect on local education spending because funds could be misused or fungible by local governments.

[2] Although this regional targeting strategy is far from perfect, it was a significant improvement in targeting the poor compared to the previous regional targeting method (Park et al., 2002).

[3] These 12 provinces are Hebei, Shanxin, Heilongjiang, Anhui, Fujian, Jiangxi, Henan, Hubei, Hunan, Hainan, Sichuan, and Shaanxi.

3. Empirical strategy

This section begins with a description of the data used in this paper. I then discuss how I use a fuzzy regression discontinuity design (which relies on the *8-7 Plan*'s eligibility rule for national poverty counties) to identify causal effects.

3.1. Data

This paper focuses on the county as the main unit of analysis for two reasons. First, the *8-7 Plan* directly targeted county-level governments for fiscal transfers. The county governments were responsible for managing and allocating these funds. Second, county governments in China have been playing a major role in financing and providing local public goods and services. A report by the World Bank (2002) points out that Chinese sub-national governments are responsible for financing nearly 70% of public spending, and that among all the levels of sub-national governments, county governments are responsible for financing 55% of total public spending. In regard to education spending, county government expenditures accounted for more than 90% of total pre-tertiary education spending. Most existing studies of Chinese sub-national social spending are able to use only province-level or prefecture-level data. However, since counties are the primary funders of local pre-tertiary education, county-level data is appropriate.

The local education spending data is from the 1994 to 2000 editions of the *China Education Finance Statistical Yearbooks* (Zhongguo jiaoyu jingfei tongji nianjian) published by the Ministry of Education.[4] This dataset offers detailed information on all the counties and county-level cities in China, which account for 80% of the county-level jurisdictions.[5] More importantly, the yearbooks offer a complete picture of local education spending, including both budgetary and extra-budgetary education spending, while most existing studies use only data on budgetary education spending.[6] I supplement the education data with county-level economic and demographic data from the *National Prefecture and County Finance Statistics Compendium* (Quanguo Di Shi Xian Caizheng Tongji Ziliao), published between 1994 and 2000 by the Ministry of Finance.[7] To control for local conditions prior to the 1994 National Poverty County designation, I include indicators for previous National Poverty County status (designated in 1986) and minority county status,[8] as well as the population size and the illiteracy rate for the population aged 15 years and above reported in the 1990 Chinese census. To evaluate the education outcomes of these counties, I calculated the changes in county-level illiteracy rate between 1990 and 2000, and between 1990 and 2010, using rates obtained from the 1990, 2000, and 2010 Chinese census. The descriptive statistics for the full sample of these key variables are reported in Table A1.

The exact amounts of specific education transfers received from these programs by targeted counties are not publicly available from the sources above. It is worth noting that the intergovernmental transfers in China can be broadly classified into three categories (Huang & Chen, 2012): tax rebates, general-purpose transfers and specific-purpose transfers. The tax rebate transfer is an important component of the TSS reform in 1994 that allows central and local governments to share the revenues of taxes, such as Value Added Taxes (VAT), consumption taxes, and income taxes. The general-purpose transfers include equalization transfers and pre-tax sharing system grants. The specific-purpose transfers consist of a number of conditional transfers from the central government to local government for specific policies and programs. Education-related transfers to National Poverty Counties generally fall under the specific-purpose transfers. Unfortunately, the *National Prefecture and County Finance Statistics Compendium* does not give detailed information on the education-related transfers under this category for the period under investigation (1994–2000). However, we can deduce the amount of transfers received by these targeted counties by using the total specific-purpose transfers received by counties as a proxy. Table 1 illustrates the descriptive statistics of total specific-purpose transfers per capita received by counties for the full sample as well as those near the cutoff points. As shown, designated counties have received significantly greater transfers than non-designated counties. The mean differences are statistically significant not only for the national sample, but between the different bandwidths of 1992 County Rural Income per capita that were used during the designation process.

A natural starting point is to use the specific-purpose transfers as the independent variable to evaluate their effects on local education spending. As shown in Table A2 in the Appendix, the estimates are positive and statistically significant in fixed effect models, even in models with narrow bandwidth around the cutoff point. However, this approach is likely to yield biased estimates, primarily because transfers are endogenous to unobserved political influence during the process of funding allocation. To overcome this problem, the next section describes the use of fuzzy regression discontinuity design to obtain the unbiased estimates of transfers from the *8-7 Plan*.

[4] These data were provided by the Barometer of Chinese Development (BOCD) project at the Universities Service Centre for China Studies, Chinese University of Hong Kong. I have conducted several consistency checks of the data to eliminate scanning and data input errors.

[5] Unfortunately, this set of statistical yearbook does not have data for urban districts in China. However, none of the urban districts are qualified to be considered as "National Poverty County," thus omitting these jurisdictions is not a concern for the empirical analysis.

[6] The difference between budgetary spending and extra-budgetary spending is that the source of the latter spending comes from the additional fees collected by local governments, which are often not reported in the publicly available official government budgets.

[7] The data were also construed by the Barometer on China's Development (BOCD) project at the Universities Service Centre for China Studies, Chinese University of Hong Kong.

[8] Ideally, one would include an indicator of "Revolutionary base" county status. "Revolutionary base" counties received preferential treatment during the 1994 National Poverty County designation. However, the list of "Revolutionary base" counties was not available to the author. Alternatively, I include the status of "designated National Poverty County in 1986," which includes many "Revolutionary base" counties.

Table 1
Descriptive statistics: specific-purpose transfers per capita.
Sources: National Prefecture and County Finance Statistics Compendium (Quanguo Di Shi Xian Caizheng Tongji Ziliao), Vol. 1994–2000; author's calculation.

		NP94 = 1	NP94 = 0	Between group difference
All year	National sample	84.51	61.97	22.55
		(87.32)	(85.56)	(1.60)
	±RMB 100	81.01	58.35	22.67
		(93.41)	(67.81)	(2.60)
	±RMB 200	82.65	55.72	26.93
		(87.62)	(62.59)	(1.84)
1994	National sample	42.38	46.81	4.43
		(47.79)	(42.81)	(2.15)
	±RMB 100	45.21	35.85	9.37
		(43.55)	(37.36)	(3.44)
	±RMB 200	46.22	36.06	10.16
		(43.79)	(39.80)	(2.65)
1995	National sample	48.18	42.72	5.46
		(42.21)	(51.19)	(2.18)
	±RMB 100	47.95	35.56	12.38
		(43.78)	(61.08)	(3.49)
	±RMB 200	47.86	35.32	12.53
		(43.45)	(37.94)	(2.59)
1996	National sample	69.57	50.70	19.62
		(71.88)	(55.53)	(3.37)
	± RMB 100	111.37	70.93	18.87
		(133.53)	(79.74)	(5.40)
	±RMB 200	71.31	46.79	24.51
		(68.50)	(47.51)	(3.72)
1997	National sample	93.99	59.80	34.18
		(107.32)	(88.26)	(5.01)
	±RMB 100	90.82	59.04	31.77
		(125.08)	(67.04)	(8.26)
	±RMB 200	92.21	52.99	39.22
		(110.81)	(56.44)	(5.61)
1998	National sample	98.73	67.16	31.57
		(99.72)	(82.21)	(4.67)
	±RMB 100	92.76	63.07	29.69
		(106.56)	(66.43)	(7.42)
	±RMB 200	95.84	60.58	35.26
		(98.87)	(61.23)	(5.27)
1999	National sample	123.47	92.17	31.30
		(91.38)	(120.49)	(4.91)
	±RMB 100	117.24	86.83	30.41
		(95.55)	(80.21)	(7.62)
	±RMB 200	119.59	83.70	35.89
		(89.21)	(73.28)	(5.25)
2000	National sample	109.51	76.77	32.74
		(100.90)	(103.05)	(5.00)
	±RMB 100	105.72	80.27	25.45
		(108.82)	(95.18)	(8.92)
	±RMB 200	107.94	76.96	30.99
		(103.46)	(89.73)	(6.27)

Notes: NP94 is an indicator variable that is equal to one if the county was designated as a National Poverty County in 1994. For each panel, National Sample includes all the counties in the dataset; ±RMB 100 includes only counties whose 1992 rural income per capita were RMB 100 above or below the cutoff point (RMB 400); ±RMB 200 includes only counties whose 1992 rural income per capita were RMB 200 above or below the cutoff point (RMB 400). Standard deviations are reported in parentheses for the first two columns, and standard errors are reported in parentheses in the third column. The t-tests for between group differences are mean tests with unequal variances.

3.2. Identification strategy

To identify the causal effect of intergovernmental transfers made through the *8-7 Plan* on local education provision, I employ a fuzzy regression discontinuity (FRD) design based on the National Poverty County designation in 1994. The FRD design provides a quasi-experimental setting to estimate the causal effect of a treatment, if the treatment is determined completely or partly by falling on either side of a fixed threshold of a continuous forcing variable *X*. Hahn, Todd, and Van der Klaauw (2001) make important theoretical and conceptual contributions to develop fuzzy RD design. This method has been used to estimate the effects of the PROGRESA program (Battistin & Rettore, 2008) and the effects of financial aid offers on college enrollment (van der Klaauw, 2002). It has also been used to test whether financial transfers to schools enhance student performance in New York public schools (van der Klaauw, 2008). In the present case, the treatment is the designation of National Poverty County in 1994, and the continuous forcing variable is the 1992 rural income per capita used by the central government during the designation process.

The use of a regression discontinuity design relies on the key assumption that the designation of treatment is *random* within a certain bandwidth of the cutoff point. This assumption could be violated in reality. The main concern is manipulation of the forcing variable to gain selection (i.e., rural income per capita) by the county governments because of the potential fiscal benefits from the *8-7 Plan*. As pointed out in Park et al. (2002), political considerations could play a role during the designation process carried out by the upper levels of government. The *8-7 plan* further complicates the RD design because it effectively features two cutoff points for the designation. That is, there is a cutoff point of RMB 400 for counties that were not designated prior to 1994, and RMB 700 for those that were. For the analyses below, I consider only the RMB 400 cutoff point for the FRD, because observations around the RMB 700 cutoff are more likely to be subject to unobserved political factors. As shown in Fig. A1 in the online appendix, there is a discontinuity in the probability of being designated between counties whose 1992 rural income per capita fell below the RMB 400 cutoff, and those counties where 1992 income was above it.

To ensure the validity of the FRD approach for data analysis, I employ several strategies to investigate whether the key assumptions of FRD are violated. First, I investigate whether the National Poverty County designation process was manipulated by county governments. Note that in the 1994 designation process the central government used the 1992 rural income per capita statistics that it already held. It would have been extremely difficult for counties to revise these numbers in an attempt to qualify for the designation. More formally, I use a density function test[9] of the forcing variable (i.e., rural income per capita) developed by McCrary (2008) to evaluate any potential manipulation during the designation process. As shown in Fig. A2 in the online appendix, the density function of the forcing variable, county rural income per capita, does not have a significant discontinuity at the RMB 400 cutoff point (especially at the left hand side of the cutoff point) as it would if there had been a manipulation of the designation. This formal test suggests the Log discontinuity estimate, $\hat{\theta}$, is statistically insignificant,[10] indicating that there is no evidence of a discontinuity in the distribution of county rural income per capita around the RMB 400 cutoff point.

Second, I conduct a series of tests of FRD validity by ensuring no discontinuity in the pre-treatment covariates at the cutoff point (Lee & Lemieux, 2010). That is, I investigate whether counties are significantly different prior to the National Poverty County designation in 1994. Fig. A3 in the online appendix shows no discontinuities of county characteristics on both sides of the cutoff point when investigating county 1993 GNP per capita and 1993 Government Total Spending Per Capita, two key factors that affect a county's education spending. Similarly, I do not find discontinuity in county illiteracy rate in 1990. The results of these pre-treatment balance tests are similar to Meng (2013), who uses the same fuzzy RD approach to analyze the impact of the *8-7 Plan* on rural income in China. He found that counties are similar in other county attributes around the cutoff points.[11]

Given that the 1992 county rural income per capita is not fully deterministic for National Poverty County designation, I use the fuzzy regression discontinuity design instead of the sharp regression discontinuity design to analyze the data. We can conceptualize the fuzzy RD approach in an instrumental variable (IV) framework. The causal effect can be interpreted as a local average treatment effect (Lee & Lemieux, 2010). The effect of the treatment in the fuzzy RD design can be estimated using the following baseline 2SLS specification (Hahn et al., 2001; Lee & Lemieux, 2010) to analyze the panel dataset:

$$NP94_{ij} = \alpha_0 + \alpha_1 T_{ij} + g\left(X_{ij} - c\right) + \rho Z_{ij} + year_t + P_j + \mu_{ij} \tag{1}$$

$$Y_{ijt} = \beta_0 + \tau NP94_{ij} + f\left(X_{ij} - c\right) + \omega Z_{ij} + year_t + P_j + \varepsilon_{ijt} \tag{2}$$

where $T = 1[X \le c]$. Y_{ijt} is the outcome variable indicating education spending for county i in province j at year t, X is the 1992 county rural income per capita, c is the RMB 400 cutoff point, and *NP94* is the 1994 designation status (1 if National Poverty County, 0 otherwise). Because the starting points of local education spending vary across counties, I use log transformation of local education spending as the outcome variable of interest, Y_{ijt}, which allows us to interpret τ as the percent change in spending due to the National Poverty County designation. $f(\cdot)$ and $g(\cdot)$ are functional forms that indicate the order of polynomial regression to account for different slopes on two sides of the cutoff points. In the analysis, I report the estimation results of the second and the third order polynomial regressions, respectively.

In this specification, I include covariates Z_{ij}[12] in the second stage for two reasons. First, there may be potential factors (Z_{ij}) that may compromise the validity of the fuzzy RD. For example, minority counties received preferential treatment during this process, even though some of them may not qualify based on the rural income per capita criterion. If the fuzzy RD design were valid, the inclusion of these covariates would not affect the consistency of the estimator for τ (Lee & Lemieux, 2010). Second, I could obtain more statistical power when I control for Z_{ij}. Because I use a panel dataset, I estimate the clustered standard error at the county level to account for within-county correlation of the errors over time in all model specifications. I also include provincial dummies P_j to account for the time-invariant unobserved factors and year dummies $year_t$ to account for common shocks.

[9] The test may not be informative if there are incentives for some counties to opt in and for others to opt out of the designations. In this case, counties have little reason to refuse more transfers from upper-tier government as a result of the designation.

[10] The estimate for $\hat{\theta}$ is 0.040 with a standard error of 0.110.

[11] Meng (2013) conducted the tests on the minority county indicator, revolutionary base indicator, fraction with high school or more, and fraction with college or more in 1990.

[12] These local conditions are the indicators of minority county, previous designation status, logged total government spending per capita in 1993, logged GNP per capita in 1993, logged 1990 population, and the 1990 illiteracy rate for population aged 15 and above.

4. Empirical results

Before I present the FRD estimation results, I offer some evidence through graphing county education spending over the 1992 county rural income per capita in every year between 1994 and 2000. The continuous solid line represents the predicted values from a second order polynomial in the running variable separately for observations above and below the threshold,[13] and the continuous dashed line represents the 95% confidence interval of the predicted values. Fig. A4 in the online appendix shows little evidence of a discontinuity in total county education spending per capita around the RMB 400 cutoff point in the distribution of rural income per capita across all years. The gaps of the predicted values on both sides of the cutoff point are small and not statistically significant at the 5% level. These findings do not change when using different sizes of non-overlapping bins, or using budgetary education spending as the dependent variable.

I then formally estimate the model using the FRD approach. Columns 1–2 in Table 2 provide the result for the full sample.[14] The estimates of *NP94* are both negative, and statistically insignificant from zero when *Log Education Spending Per Capita* is the dependent variable (Panel 1). Substantively, while the estimates seem to suggest that total education spending per capita in designated counties were 6.4% (standard error: 7.5) to 11.5% (standard error: 14.2) less than in counties that were not designated, the lack of statistical significance in the estimates suggests that this finding is inconclusive. Columns 3–6 estimate the model within a small bandwidth around the county rural income per capita cutoff point of RMB 400 (±RMB 200 and ±RMB 100 respectively), and the estimates of *NP94* are largely in line with previous models based on the full sample because they are not statistically insignificant. It is worth noting that the estimates are relatively stable in both second and third polynomial regressions, despite different bandwidths.

Next, I turn to *Log Budgetary Education Spending Per Capita* between 1994 and 2000 as the dependent variable. Since the transfers are channeled through the existing fiscal institutions, one may expect that these transfers should, at the least, have a positive effect on budgetary education spending. Yet, the results in Panel 2 of Table 2 are consistent with the results in Panel 1. As shown, estimates of designation are not statistically significant in any model. When I estimate the model with observations in a narrow bandwidth, even the signs of the estimates are negative. Overall, this finding suggests no evidence of crowding-out effects related to intergovernmental transfers. That is, the transfers did not decrease extra-budgetary education spending because of an increase in budgetary education spending.

Note that the NP94 is time invariant because the designation of National Poverty County did not change between 1994 and 2000. Alternatively, I employ the fuzzy RD to analyze yearly data. As shown in Tables A4 and A5 in the online appendix, the same pattern persists when I analyze the data for each year instead of using pooled cross-section data. I also follow the strategy used in Meng (2013) by using the change in the outcome variables between 1994 and 2000 as the alternative dependent variables, and the estimates for NP94 remain statistically insignificant across all model specifications.[15]

In sum, the empirical evidence suggests that, despite the intergovernmental transfers that resulted in greater fiscal resources to local governments for education provision, the counties targeted by the *8-7 Plan* did not systematically spend more on education than the non-targeted counties. It is worth noting that there are some inherent disadvantages when using fuzzy RD as the identification strategy. Since the fuzzy RD analysis only identifies a local average treatment effect (LATE) near the cutoff point, it implies limited external validity in the full sample because this is not an average treatment effect (ATE). In other words, the estimates from the fuzzy RD are only applicable to "complier" counties near the cutoff. However, the failure to identify even a local average treatment effect (LATE) suggests that intergovernmental transfers do not affect local governments' social spending decisions even in counties that were specifically targeted by the Chinese central government.

The null-results above could be due to measurement errors in the dependent variable or mis-specifications in the empirical model. I conduct several robustness checks to address these concerns. The results from the robustness checks are consistent with my main findings, that intergovernmental transfers do not have any systematic positive effects on enhancing local education spending in China, regardless of the measures of the dependent variable or the choice of sample size.

In my evaluation of local education spending, I consider two alternative dependent variables. First, the size of the local population could be subject to measurement errors given the increase in internal migration in China. Measurement errors in dependent variables (per capita spending) lead to a greater variance in the estimates, reducing the likelihood that the estimates will be statistically significant. Therefore, per student spending on education is probably a better measure than per capita spending. The *China Education Finance Statistical Yearbooks* contain data on budgetary spending per student at the primary school level and at the junior high school level respectively. Although these measures indicate only budgetary expenditures on education and not the total education spending figures, I use them as alternative dependent variables and re-analyze the data based on the same specifications as in Table 2. These alternative dependent variables also allow me to disentangle the "treatment effects" in different levels of the education system. Panel 1 in Table 3 suggests little systematic evidence yet again to support the claim that transfers through the *8-7 Plan* enhanced local education spending at the primary school level in China. There is, however, some evidence for a positive effect on budgetary spending per student at the junior high school level in some model specifications, as shown in Panel 2 in Table 3. One interpretation is that since most junior high schools are located in townships closer to the county seat, rather than villages; a positive effect suggests a potential elite capture of the transfers for junior high school spending, but not an increase of the overall education spending.

[13] The results remain consistent if I use a third order polynomial in the running variable. I chose to present the results by using a second order polynomial in order to make the results comparable to Meng (2013), which used the same identification strategy by exploring the discontinuity in the designation of National Poverty Counties.

[14] See Table A3 in the online appendix for the first stage results.

[15] See Table A6 in the online appendix for more details.

Table 2
Education spending per capita (1994–2000).
Sources: China Education Finance Statistical Yearbooks (Zhongguo jiaoyu jingfei tongji nianjian), Vol. 1994–2000; National Prefecture and County Finance Statistics Compendium (Quanguo Di Shi Xian Caizheng Tongji Ziliao), Vol. 1994–2000.

	Full sample		±RMB 200		±RMB 100	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: logged total education spending per capita						
NP94	−0.064	−0.115	−0.021	0.078	0.132	−0.211
	(0.075)	(0.142)	(0.148)	(0.265)	(0.222)	(0.288)
County-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Provincial dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial order	2	3	2	3	2	3
Observations	11,006	11,006	5921	5921	3471	3471
Panel 2: logged budgetary education spending per capita						
NP94	−0.002	−0.066	−0.030	−0.137	0.012	−0.450
	(0.074)	(0.140)	(0.147)	(0.273)	(0.214)	(0.365)
County-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Provincial dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial order	2	3	2	3	2	3
Observations	11,053	11,053	5938	5938	3431	3431

Note: NP94 is an indicator variable that is equal to one if the county was designated as a National Poverty County in 1994. For each panel, National Sample includes all the counties in the dataset; ±RMB 100 includes only counties whose 1992 rural income per capita were RMB 100 above or below the cutoff point (RMB 400); ±RMB 200 includes only counties whose 1992 rural income per capita were RMB 200 above or below the cutoff point (RMB 400). County-level controls include minority county indicator, previous designated National Poverty County status, 1993 logged GNP per capita, 1993 logged government revenue per capita, as well as logged population and the illiteracy rate for population aged 15 and above calculated from the 1990 Population Census. Clustered standard errors at the county level are reported in parenthesis.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Second, intergovernmental transfers, particularly those for education through the *Compulsory Education in National Poor Regions Project*, only began to increase significantly in 1996, thus the null-effect observed in prior analysis could be masked by the small magnitudes of transfers prior to 1996. Further, it is worth noting that missing data for county-level education spending could pose a potential problem in the estimation results reported in Tables 2 and 3. Overall, nearly one-third of the counties have missing data

Table 3
Alternative education spending measures.
Sources: China Education Finance Statistical Yearbooks (Zhongguo jiaoyu jingfei tongji nianjian), Vol. 1994–2000; National Prefecture and County Finance Statistics Compendium (Quanguo Di Shi Xian Caizheng Tongji Ziliao), Vol. 1994–2000.

	Full sample		±RMB 200		±RMB 100	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: logged budgetary education spending per student (primary school)						
NP94	−0.057	−0.146	−0.095	−0.620	−0.298	−0.677
	(0.097)	(0.193)	(0.196)	(0.486)	(0.311)	(0.496)
County-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Provincial dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial order	2	3	2	3	2	3
Observations	11,066	11,066	5944	5944	3424	3424
Panel 2: logged budgetary education spending per student (junior high school)						
NP94	0.374***	0.529**	0.464**	0.234	0.323	−0.036
	(0.109)	(0.223)	(0.232)	(0.358)	(0.312)	(0.354)
County-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Provincial dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial order	2	3	2	3	2	3
Observations	11,049	11,049	5931	5931	3415	3415

Note: NP94 is an indicator variable that is equal to one if the county was designated as a National Poverty County in 1994. For each panel, National Sample includes all the counties in the dataset; ±RMB 100 includes only counties whose 1992 rural income per capita were RMB 100 above or below the cutoff point (RMB 400); ±RMB 200 includes only counties whose 1992 rural income per capita were RMB 200 above or below the cutoff point (RMB 400). County-level controls include minority county indicator, previous designated National Poverty County status, 1993 logged GNP per capita, 1993 logged government revenue per capita, as well as logged population and the illiteracy rate for population aged 15 and above calculated from the 1990 Population Census. Clustered standard errors at the county level are reported in parenthesis.
*** $p < 0.01$.
** $p < 0.5$.
* $p < 0.1$.

Table 4
Education spending per capita (1996–2000).
Sources: China Education Finance Statistical Yearbooks (Zhongguo jiaoyu jingfei tongji nianjian), Vol. 1994–2000; National Prefecture and County Finance Statistics Compendium (Quanguo Di Shi Xian Caizheng Tongji Ziliao), Vol. 1994–2000.

	Full sample		± RMB 200		± RMB 100	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: logged total education spending per capita						
NP94	−0.127	−0.169	−0.049	0.037	0.086	−0.258
	(0.083)	(0.160)	(0.163)	(0.283)	(0.234)	(0.294)
County-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Provincial dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial order	2	3	2	3	2	3
Observations	8133	8133	4339	4339	2467	2467
Panel 2: logged budgetary education spending per capita						
NP94	−0.044	−0.102	−0.046	−0.183	−0.037	−0.520
	(0.080)	(0.155)	(0.161)	(0.301)	(0.229)	(0.386)
County-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Provincial dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial order	2	3	2	3	2	3
Observations	8180	8180	4356	4356	2481	2481

Note: NP94 is an indicator variable that is equal to one if the county was designated as a National Poverty County in 1994. For each panel, National Sample includes all the counties in the dataset; ± RMB 100 includes only counties whose 1992 rural income per capita were RMB 100 above or below the cutoff point (RMB 400); ± RMB 200 includes only counties whose 1992 rural income per capita were RMB 200 above or below the cutoff point (RMB 400). County-level controls include minority county indicator, previous designated National Poverty County status, 1993 logged GNP per capita, 1993 logged government revenue per capita, as well as logged population and the illiteracy rate for population aged 15 and above calculated from the 1990 Population Census. Clustered standard errors at the county level are reported in parenthesis.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

on education spending in 1994 in the *China Education Finance Statistical Yearbooks*. To assess the potential estimation bias due to transfer scale and data coverage, I re-analyzed the data by only including the observations between 1996 and 2000. Table 4 reports the results, and the estimates of *NP94* remain statistically insignificant from zero in most model specifications.

5. Short-term and long-term impacts on illiteracy

Although I have found that the transfers resulting from the *8-7 Plan* have no impact on local education spending, this poverty relief program could still have a positive impact on local education provision different from the mechanism of intergovernmental transfers. In particular, an improvement in household income could lead to greater school attendance because parents do not need the child to work to supply additional income. If this mechanism were at work, we should observe an improvement in education outcomes for the counties that benefited from the program. To evaluate this alternative mechanism, I use the same FRD specification as above to investigate the short-term and long-term impacts of the *8-7 Plan* on the change in illiteracy rate for targeted counties. To evaluate the short-term impact, I calculated the changes in illiteracy rate between 1990 and 2000, the year when the *8-7 Plan* ended, from the 1990 and 2000 Chinese census. To evaluate the long-term impact, I calculated the changes in illiteracy rate between 1990 and 2010, ten years after the program ended, from the 1990 and 2010 Chinese census.[16]

Panel 1 in Table 5 shows some evidence that the *8-7 Plan* has reduced illiteracy rates more significantly in the designated counties than the non-designated counties in 2000 in one model specification, but the results are not very robust in other specifications. For example, Column 1 suggests that designated counties have reduced the illiteracy rate by 6.08 percentage points (standard error: 2.57) more than the non-designated counties, and the estimate is statistically significant. Column 2, which has a third polynomial order in the model specification, suggests that designated counties have reduced the illiteracy rate by only 1.5 percentage points (standard error: 5.12) more than the non-designated counties, and the estimate is not statistically significant. Estimates in models with smaller bandwidth are not statistically significant, despite the correct signs in most model specifications (Columns 3–4).

The analysis of short-term impact may not reveal the true effect of the program because the illiteracy rate only accounts for the population aged 15 and above in 2000, and many of these individuals were not compulsory-education aged at the time of the *8-7 Plan*. I then investigate the difference in illiteracy rate changes ten years after the end of the program (i.e., 2010), and it paints a drastically different picture. The estimates become positive, and statistically significant in some model specifications, on the changes in illiteracy rate (Panel 2 in Table 5). For example, Column 2 suggests an increase of 12.8 percentage points (standard error: 7.33) in

[16] One objection to this analysis is that the samples used in calculating the illiteracy rates for any given county are different in 1990, 2000, and 2010, largely due to internal migration. Specifically, more educated individuals tend to move out of the rural areas. However, the fuzzy RD design addresses this concern because the counties close to the threshold should have similar migration patterns. Empirically, I evaluate the change in population aged between 20 and 49, the groups of individuals who are more likely to migrate out of rural areas to seek employment, and do not find any evidence that control and treatment counties have different migration patterns. See Tables A7 and A8 in the online appendix for more details.

Table 5
Short-term and long-term impact on illiteracy.
Sources: China Education Finance Statistical Yearbooks (Zhongguo jiaoyu jingfei tongji nianjian), Vol. 1994–2000; National Prefecture and County Finance Statistics Compendium (Quanguo Di Shi Xian Caizheng Tongji Ziliao), Vol. 1994–2000; China Population Census (1990, 2000, 2010).

	Full sample		±RMB 200		±RMB 100	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: change in illiteracy rate 1990–2000 (age 15 +)						
NP94	−6.080**	−1.501	2.400	−0.126	−1.057	−7.737
	(2.567)	(5.116)	(6.244)	(10.717)	(9.591)	(10.433)
County-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Provincial dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial order	2	3	2	3	2	3
Observations	1767	1767	930	930	528	528
Panel 2: change in illiteracy rate 1990–2010 (age 15 +)						
NP94	−3.988	12.833*	17.961*	24.541	27.180	11.613
	(2.957)	(7.325)	(9.968)	(20.900)	(19.885)	(14.403)
County-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Provincial dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial order	2	3	2	3	2	3
Observations	1755	1755	929	929	528	528

Note: NP94 is an indicator variable that is equal to one if the county was designated as a National Poverty County in 1994. For each panel, National Sample includes all the counties in the dataset; ± RMB 100 includes only counties whose 1992 rural income per capita were RMB 100 above or below the cutoff point (RMB 400); ± RMB 200 includes only counties whose 1992 rural income per capita were RMB 200 above or below the cutoff point (RMB 400). County-level controls include minority county indicator, previous designated National Poverty County status, 1993 logged GNP per capita, 1993 logged government revenue per capita, as well as logged population and the illiteracy rate for population aged 15 and above calculated from the 1990 Population Census. Clustered standard errors at the county level are reported in parenthesis.
*** $p < 0.01$
** $p < 0.05$.
* $p < 0.1$.

illiteracy rate in the targeted counties, compared to the non-targeted counties. The estimates for models with a narrow bandwidth are all positive, but not statistically significant because of large standard errors. Substantively, these results suggest not only that the transfers did not reduce illiteracy in the short run, but also that they potentially created negative outcomes in the long run.

Does the program have heterogeneous effect on different populations? When I break down the data by gender, I find that the reduction of the illiteracy rate between 1990 and 2000 is approximately equal in magnitude for both male and female populations, as shown in Panels 1 and 2 in Table 6. The analysis of long-term impact (i.e., between 1990 and 2010), however, suggests that the rising of the illiteracy rate mostly comes from the female population (Panels 3 and 4). Although one potential reason for the rising illiteracy rate is due to the mergers and consolidation of primary schools in rural areas during the 2000s, the calculation of the illiteracy rate only accounts for the population aged 15 and above, who are too old to be affected the primary school merges and consolidation policy in the early 2000s.

6. Conclusion

Identifying effective ways to provide public goods and services is a central topic in studies of the political economy of development and decentralization. In particular, public goods and services such as education promote the accumulation of human capital, which could be a long-term solution to poverty and underdevelopment. Intergovernmental transfers have been considered an important resource for provision of public goods and services, but evaluating their effectiveness faces the challenge of potential estimation bias from endogenous transfers. This paper evaluates the effects of intergovernmental transfers made through a poverty relief program on local education provision in China. I use a dataset on county governments' education spending, and employ a fuzzy regression discontinuity design as the identification strategy. I show that counties benefiting from intergovernmental transfers through the *8-7 National Poverty Reduction Plan* did not perform better in education spending between 1994 and 2000 than their non-benefiting counterparts. Furthermore, I find that the program has neither short-term nor long-term impacts on illiteracy reduction in the targeted counties. If anything, there is some evidence that the illiteracy rates have increased, particularly for the female population, in the targeted counties ten years after the end of the program.

The results here may give policymakers reason to pause before rushing to use the intergovernmental transfers to address local provision of public goods and services. The Chinese central government has revamped its efforts since 2001, and has boosted intergovernmental transfers to fund them. The findings of this paper cast doubt on the effectiveness of the transfers in achieving their stated goals, if local accountability remains weak or even absent. Further, disgruntled citizens may shift the blame for under-provision of public goods and services from local governments to the central government, as the transfers may build perceptions and expectations regarding the central government's responsibilities. Given these issues, policymakers may find that increasing intergovernmental transfers could be a cure that is worse than the disease, especially when reforms to enhance local political accountability remain lacking.

Table 6
Short-term and long-term impact on illiteracy (by gender).
Sources: China Education Finance Statistical Yearbooks (Zhongguo jiaoyu jingfei tongji nianjian), Vol. 1994–2000; National Prefecture and County Finance Statistics Compendium (Quanguo Di Shi Xian Caizheng Tongji Ziliao), Vol. 1994–2000; China Population Census (1990, 2000, 2010).

	Full sample		±RMB 200		±RMB 100	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: change in illiteracy rate 1990–2000 (male, age 15 +)						
NP94	−5.833***	−1.536	1.969	0.314	1.861	−0.507
	(2.187)	(4.267)	(5.160)	(8.786)	(8.031)	(7.735)
County-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Provincial dummy	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial order	2	3	2	3	2	3
Observations	1767	1767	930	930	528	528
Panel 2: change in illiteracy rate 1990–2000 (female, age 15 +)						
NP94	−6.175*	−0.880	3.608	−0.298	−3.810	−14.550
	(3.311)	(6.743)	(8.391)	(14.322)	(12.963)	(15.109)
County-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Provincial dummy	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial order	2	3	2	3	2	3
Observations	1767	1767	930	930	528	528
Panel 3: change in illiteracy rate 1990–2010 (male, age 15 +)						
NP94	−3.203	8.952	13.318	19.833	23.560	13.516
	(2.651)	(6.216)	(8.435)	(17.728)	(17.580)	(13.686)
County-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Provincial dummy	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial order	2	3	2	3	2	3
Observations	1755	1755	929	929	528	528
Panel 4: change in illiteracy rate 1990–2010 (female, age 15 +)						
NP94	−4.651	17.647*	23.421*	29.571	31.331	10.234
	(3.557)	(9.060)	(12.336)	(25.169)	(23.308)	(16.522)
County-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Provincial dummy	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial order	2	3	2	3	2	3
Observations	1755	1755	929	929	528	528

Note: NP94 is an indicator variable that is equal to one if the county was designated as a National Poverty County in 1994. For each panel, National Sample includes all the counties in the dataset; ±RMB 100 includes only counties whose 1992 rural income per capita were RMB 100 above or below the cutoff point (RMB 400); ±RMB 200 includes only counties whose 1992 rural income per capita were RMB 200 above or below the cutoff point (RMB 400). County-level controls include minority county indicator, previous designated National Poverty County status, 1993 logged GNP per capita, 1993 logged government revenue per capita, as well as logged population and the illiteracy rate for population aged 15 and above calculated from the 1990 Population Census. Clustered standard errors at the county level are reported in parenthesis.
*** $p < 0.01$.
** $p < 0.05$
* $p < 0.1$.

This paper makes use of the designation criterion of the *8-7 National Plan for Poverty Reduction* program to partly address the endogeneity issue of the allocation of intergovernmental transfer. However, the *8-7 Plan* is not a pure intergovernmental transfer program, which might generate some concerns by some researchers about the results of this paper. As a consequence, scholars may want to explore an alternative empirical strategy to identify unbiased estimates of the effect of intergovernmental transfers on local governments' provision of public goods and services in the future.

Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.chieco.2015.02.001.

References

Bardhan, P. (2002). Decentralization of governance and development. *Journal of Economic Perspectives*, *16*(4), 185–205.
Bardhan, P., & Mookherjee, D. (2006). Decentralisation and accountability in infrastructure delivery in developing countries. *The Economic Journal*, *116*(508), 101–127.
Battistin, E., & Rettore, E. (2008). Ineligibles and eligible non-participants as a double comparison group in regression-discontinuity designs. *Journal of Econometrics*, *142*(2), 715–730.
Brollo, F., & Nannicini, T. (2012). Tying your enemy's hands in close races: The politics of federal transfers in Brazil. *American Political Science Review*, *106*(4), 742–761.
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.
Chinese Education Finance Statistical Yearbooks (1994-2000). *Zhongguo Jiaoyu Jingfei Tongji Nianjian.* Beijing: China Statistics Press.
Cox, G.W., & McCubbins, M.D. (1986). Electoral politics as a redistributive game. *The Journal of Politics*, *48*(02), 370–389.
Dixit, A., & Londregan, J. (1998). Fiscal federalism and redistributive politics. *Journal of Public Economics*, *68*(2), 153–180.

Ferraz, C., Finan, F., & Moreira, D.B. (2012). Corrupting learning: Evidence from missing federal education funds in Brazil. *Journal of Public Economics*, *96*(9–10), 712–726.
Feyzioglu, T., Swaroop, V., & Zhu, M. (1998). A panel data analysis of the fungibility of foreign aid. *The World Bank Economic Review*, *12*(1), 29–58.
Gervasoni, C. (2010). A rentier theory of subnational regimes: Fiscal federalism, democracy, and authoritarianism in the Argentine provinces. *World Politics*, *62*(02), 302–340.
Grossman, P.J. (1994). A political theory of intergovernmental grants. *Public Choice*, *78*(3), 295–303.
Hahn, J., Todd, P., & Van der Klaauw, W. (2001). Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica*, *69*(1), 201–209.
Huang, B., & Chen, K. (2012). Are intergovernmental transfers in China equalizing? *China Economic Review*, *23*(3), 534–551.
Jia, J., Guo, Q., & Zhang, J. (2014). Fiscal decentralization and local expenditure policy in China. *China Economic Review*, *28*, 107–122.
Khemani, S. (2007a). The political economy of equalization transfers. In J. Martinez-Vazquez, & R. Searle (Eds.), *Fiscal equalization: Challenges in the design of intergovernmental transfers*. New York: Springer.
Khemani, S. (2007b). Does delegation of fiscal policy to an independent agency make a difference? Evidence from intergovernmental transfers in India. *Journal of Development Economics*, *82*(2), 464–484.
Lee, D.S., & Lemieux, T. (2010). Regression discontinuity designs in economics. *Journal of Economic Literature*, *48*(2), 281–355.
Levitt, S.D., & Snyder, J.M., Jr. (1995). Political parties and the distribution of federal outlays. *American Journal of Political Science*, *39*(4), 958–980.
Lindbeck, A., & Weibull, J.W. (1987). Balanced-budget redistribution as the outcome of political competition. *Public Choice*, *52*(3), 273–297.
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.
McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, *142*(2), 698–714.
Meng, L. (2013). Evaluating China's poverty alleviation program: A regression discontinuity approach. *Journal of Public Economics*, *101*, 1–11.
Ministry of Education (1997). *Handbook of Compulsory Education Project in National Poverty Region.* Beijing: Higher Education Publishing.
National Bureau of Statistics, Division of Rural Social and Economic Survey (2006). In Guojia Tongji ju Nongchun Shehui Jingji Diaocha Si (Ed.), *Report on rural poverty monitoring in China — 2005 (Zhongguo Nongchun Pingkun Jiance Baogao — 2005)*. Beijing: China Statistics Press.
Prefecture, National, & Finance Statistics Compendium, County (1994-2000). *Quanguo Di Shi Xian Caizheng Tongji Ziliao.* Beijing: China Financial & Economic Publishing House.
Park, A., Wang, S., & Wu, G. (2002). Regional poverty targeting in China. *Journal of Public Economics*, *86*(1), 123–153.
Reinikka, R., & Svensson, J. (2004). Local capture: Evidence from a central government transfer program in Uganda. *The Quarterly Journal of Economics*, *119*(2), 679–705.
Rodden, J. (2006). *Hamilton's paradox: The promise and peril of fiscal federalism.* New York: Cambridge University Press.
Solé-Ollé, A., & Sorribas-Navarro, P. (2008). The effects of partisan alignment on the allocation of intergovernmental transfers. Differences-in-differences estimates for Spain. *Journal of Public Economics*, *92*(12), 2302–2319.
Treisman, D. (2007). *The architecture of government: rethinking political decentralization.* New York: Cambridge University Press.
Tsang, M.C. (2002). Intergovernmental grants and the financing of compulsory education in China. *Harvard China Review*, *5*, 15–20.
van de Walle, D., & Mu, R. (2007). Fungibility and the flypaper effect of project aid: Micro-evidence for Vietnam. *Journal of Development Economics*, *84*(2), 667–685.
van der Klaauw, W. (2002). Breaking the link between poverty and low student achievement: An evaluation of Title I. *International Economic Review*, *43*.
van der Klaauw, W. (2008). Breaking the link between poverty and low student achievement: An evaluation of Title I. *Journal of Econometrics*, *142*(2), 731–756.
Wang, S., Li, Z., & Ren, Y. (2004). The 8-7 National Poverty Reduction Program in China — The national strategy and its impact. *A case study from reducing poverty, sustaining growth —What works, what doesn't and why*. Beijing: Institute of Agricultural Economics, Chinese Academy of Agricultural Sciences.
World Bank (2000). *Entering the twenty first century.* Oxford and New York: Oxford University Press.
World Bank (2002). *China: National development and sub-national finance: A review of provincial expenditure.* Washington, DC: World Bank.
World Bank (2004). *World development report 2004: Making services work for poor people.* Washington D.C.: World Bank, Oxford University Press.
Yuan, Z. (Ed.). (2005). *Shuoxiao Chaju: Zhongguo Jiaoyu Zhengche de Zhongda Mingti (Narrowing the gap: A key issue in China's educational policies)*. Beijing: Renmin Jiaoyu Chuban She (People's Education Press).
Zhang, L. (Ed.). (2007). *The course of poverty reduction in China (1949–2005)*. Beijing: China Financial and Economic Publishing House.