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Policy Failure and Educational Attainment in Indonesia

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By the late 1980s, child completion of primary education was near universal in Indonesia. The country has since turned its attention to increasing access to secondary school. We examine the causal impact of two classic education policies on secondary school participation in Indonesia: compulsory schooling and spending mandates. We find that the country's 1994 nine-year compulsory schooling initiative had no impact on child educational attainment. We also determine that Indonesia's 2002 constitutionally-imposed education spending mandate has been ineffective in influencing secondary school enrolments. Both policies suffer from weak enforcement. Improved enforcement would be beneficial in the case of compulsory schooling. However, the major risk in the case of spending mandates is that government begins to enforce them more rigorously, as they are applied to additional sectors, thereby constraining the efficient delivery of education and other local public services.

Keywords: education policy, compulsory schooling, spending mandates, regression discontinuity, dynamic panel data models, Indonesia

JEL codes: I25, I28, H75, H77

Policy failure and educational attainment in Indonesia

1. Introduction

By the late 1980s, child completion of primary school was near universal in Indonesia. The country has since turned its attention to increasing participation in secondary school. At present only about one-third of Indonesians complete secondary education. It is well recognized in Indonesia that increasing education participation rates is vital for the country to improve its human capital and sustain its economic development (OECD, 2015).[1]

This paper examines the effectiveness of two major government education policy initiatives in encouraging higher participation rates in secondary education in Indonesia: compulsory schooling and education sector spending mandates. We use regression discontinuity (RD) methods to exploit the timing of the 1994 nine-year compulsory education policy and establish its causal effect on junior secondary school completion rates. We model endogenous local government education spending by employing dynamic panel data (DPD) methods to explore the causal impact of the 2002 constitutionally-imposed education expenditure mandate on junior and senior secondary school enrolments. Our objective is to tease out commonalities and differences in approach and impact across the two reforms to obtain a better understanding of the key features of these policies and assess the likely effectiveness of new and similar initiatives going forward.

[1] Expanding secondary school participation rates is not the only education objective in Indonesia, of course; others include increasing schooling efficiency and equity of access at the secondary level and improving the quality and relevance of education, in general. (OECD, 2015). This paper focuses on the secondary school participation agenda.

Compulsory education policies are very popular across nations of the world. The vast bulk of countries have obligatory schooling arrangements of one kind or another. As of this writing, based on World Bank Education Statistics data, we find only five countries, for which information is available, that do not have compulsory schooling laws or regulations: Nepal, Oman, Papua New Guinea, Solomon Islands, and Vanuatu.

Despite the ubiquity of compulsory schooling, comparatively few assessments have been carried out that focus directly on its educational attainment effects. While the studies are limited in number, the results are quite varied. In an early study, Lang and Kropp (1986) find that compulsory schooling in the US increases enrolments, but only of students not directly affected by the policy, a result that is consistent with the educational sorting hypothesis. Cabus and De Witte (2011) establish that compulsory schooling leads to a decline in the drop-out rate in Netherlands, but again only for those students not directly affected by the policy reform. Lleras-Muney (2002) demonstrates that compulsory education in US had a strong positive effect on years of education for white students but had no impact on black students. Bell, Costa, Machin (2016) determine that recently adopted compulsory schooling laws in US have no consistent effect on the age at which students drop out, although they find at least some positive impact on black student drop-out rates. Oreopoulos (2007) shows that compulsory schooling regulations have a strong and positive impact on number of years of schooling, highest grade attended, and age of leaving school in US, Canada, and UK, respectively. In the only existing developing country study, Xiao, Li, and Zhao (2017) provide evidence that compulsory schooling policy, when combined with the elimination of school fees, positively affects student enrolments and years of schooling in China. Amid the substantial variation in results a common finding across many of the studies concerns the importance of strong enforcement practices in assuring successful implementation of policies.

Other studies have used compulsory education as an instrument for educational attainment to assess the impact of the latter on other outcomes of interest, such as, for example: employment and income (Angrist and Krueger, 1991; Acemoglu and Angrist, 2000; Pischke and von Wachter, 2005); teenage births (Black, Devereux, and Salvanes, 2008); crime (Lochner and Moretti, 2004; Machin, Marie, and Vujic, 2011); adult health and mortality (Lleras-Muney, 2002; Clark and Roayer, 2013); and children's health (Gunes, 2015). All these studies find that compulsory schooling has positive (first stage) effects on educational achievement.

The imposition of spending mandates by higher levels of government or judiciaries on lower level administrations is also common world-wide. The academic literature on this topic includes, but it is not restricted to, the education sector. It examines three main questions: the impact of spending mandates on lower level government autonomy and budgets, the availability and efficacy of methods for forcing the discontinuance of mandates set by higher level authorities, and lower level administration compliance with mandates. The research emphasizes that many spending mandates compromise lower level governments' autonomy and routinely constrain their fiscal positions (Dabla-Norris, 2006). It finds, not surprisingly, that the most effective methods of ending mandates are direct legal interventions, such as those employed by more modern countries, including US and Australia, as opposed to the imploring admonishments often used by local authorities in developing countries, such as South Africa (Basdeo, 2012). Although legal interventions may be preferred, that is no assurance of their success, as higher-level authorities may find alternative means of imposing their interests (Dilger, 2017). In any case, lower level administrations have been unenthusiastic in responding to mandated expenditures and central authorities have found it difficult to ensure enforcement, especially in developing countries such as China, for example (Gong and Wu, 2011; Fan, 2015). Thus, policy enforcement is a key concern here as well.

Like most countries, Indonesia has also instituted compulsory schooling policies. It issued regulations in 1984 and 1994 declaring that education was obligatory through primary and junior secondary school, respectively. In 2015, based on the perceived positive effects of such policies, Indonesia extended compulsory education to cover senior secondary school.[2]

Indonesia also makes noteworthy use of expenditure mandates in the education sector. In 2002, the nation amended its constitution to require that all levels of government—central, provincial, and local—spend at least 20 percent of their budgets on education. The provision effectively requires each province and local government to abide by the "20 percent rule".

We find that Indonesia's 1994 nine-year compulsory schooling initiative had no impact on child educational attainment among either intent-to-treat or treated cohorts. The nonexistent effect also obtains across an exhaustive set of child subgroups, as defined by gender, religion, ethnicity, location, and socio-economic status. The findings are robust across a wide range of model specifications and assumptions. The total absence of impact is a unique result across studies that have examined such reforms. We argue that the underlying reasons for the lack of effect in Indonesia are that government provided insufficient financial support for the effort and, especially, that it paid inadequate attention to policy enforcement.

We determine that Indonesia's education spending mandate policy has been largely ineffective in influencing school participation. First, most local governments budget more for education than the instructed 20 percent share and have done so since before the policy was introduced, suggesting that the mandate is to a large extent redundant. Second, the reform has had no clear impact on encouraging recalcitrant local governments to increase their education

[2] We do not attempt to evaluate either the 1984 or 2015 compulsory schooling policies. The first is made difficult by several potentially confounding policies, especially as related to school construction efforts, that were implemented during the same period, as discussed in more detail below, while the second is not possible because of a lack of data.

budget shares to required levels, due in part to weak enforcement of the policy. Third, total local government spending on all local functions, including health and infrastructure, is more important than the portion of budgets devoted just to education in determining enrolments, implying that targeting education budget shares is less than optimal. Finally, we find that the roll-out of expenditure mandates to other sectors has the potential to reduce local government spending on education and constrain the efficient delivery of services across all sectors.

Our article makes two contributions to the literature. First, we empirically assess education spending mandates and their impact on children's participation in education. To our knowledge no other study has empirically investigated the impact of mandates on service outcomes. Second, we innovatively compare two specific and classic education policy initiatives—compulsory schooling and spending mandates—to obtain a better understanding of the common and disparate features of these internationally popular policies and to assess the likely effectiveness of new and similar efforts going forward in Indonesia. Our findings may be especially relevant for other developing countries that, like Indonesia, have rather casually adopted education policy initiatives of the kind that have become ubiquitous across countries of the world.

The rest of the paper proceeds as follows. First, we provide some background information on Indonesia's system of education and the major policy initiatives it has undertaken in the education sector. Second, we review the data and variables used in the study. Third, we outline our identification strategy. Fourth, we investigate the impact of Indonesia's 1994 compulsory schooling policy on educational attainment and its 2002 constitutionally-imposed education spending mandate on enrolments and discuss the results. The last section of the paper summarizes and concludes.

2. Background: Indonesia's Education System and Policy Interventions

2.1 Education System

The education system in Indonesia is the world's fourth largest, following those of China, India, and the US. At the pre-tertiary level, it includes around 250,000 schools, 50 million students, and 2.6 million teachers. Pre-tertiary education consists of early childhood, primary, and secondary school. Early childhood education in Indonesia comprises two years of schooling and primary and secondary education spans 12 years, including six years for primary and three years each for junior and senior secondary (World Bank, 2013).

Public and private schools dominate the Indonesian primary and secondary education system. They comprise about 85 percent of the total. Islamic schools make up the remaining 15 percent. Public and private schools are under the authority of the Ministry of Education and Culture while the Ministry of Religion oversees Islamic schools (OECD, 2015).

Prior to 2001 schools were managed by central government ministries and their geographically deconcentrated field offices. In 2001, Indonesia decentralized authority over most public services of a local nature, including education, to subnational governments.[3] At that time districts became responsible for financing and managing early childhood, primary, junior secondary, and senior secondary public schools (Lewis, 2014). In 2016, financial and managerial authority for public senior secondary schools was reassigned to the provincial level. Islamic schools continue to be administered by the Ministry of Religion.

Before 1979, the academic and calendar years were coincident. In 1979 the school year began to operate from July and run through June the following year. Students who started the school year in January of 1978 remained in the same grade until July of 1979

[3] Subnational governments include provincial and local governments. The latter are also called districts. We use the terms local government and district interchangeably.

(Parinduri, 2014). Government regulations deem that children should start primary school at age seven, although some begin a year earlier or a year later (Barakat, 2016). The official cut-off date for determining a child's school age is 31 August.

2.2 Policy Interventions

Perhaps Indonesia's best-known education policy intervention is its massive school construction effort carried out in the 1970s. Between 1973 and 1979 government built over 60,000 primary schools under its SD *Instruksi Presiden* (INPRES) program.[4] This translates into about one school per 500 children, aged 5-14 (Duflo, 2001). The school construction initiative was accompanied by a large-scale teacher recruitment and training exercise.[5] Duflo (2001) provides evidence to show that each primary school built per 1,000 children led to an increase of 0.12 to 0.19 years of education for the first cohort exposed to SD INPRES.

In 1984 government implemented its National Compulsory Education program, requiring all children to complete primary school. Suryadarma, Suryahadi, Sumarto, and Rogers (2006) argue that the six-year compulsory education initiative, along with the school construction program, played a strong role in increasing enrolments at the primary level and improving educational attainment more generally. They note that government reported that 99.6 percent of appropriately aged children were either enrolled in or had completed primary school by 1988. Based on this perceived success, government introduced nine-year

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[4] SD is *sekolah dasar* or primary school. *Instruksi Presiden* (INPRES) refers to the presidential instruction under which the school construction program was authorized.

[5] Duflo (2001) also reports that government eliminated primary school fees in 1978. Suryadarma, Suryahadi, Sumarto, and Rogers (2006) suggest, however, that school fees continue to be levied, where required (and varied) payments are determined by school committees, comprising teachers, parents and other community members.

compulsory schooling in 1994, insisting that all children finish junior secondary school at least. Indonesia extended compulsory schooling to cover senior secondary school in 2015.

In 2002, Indonesia amended its constitution to specify that 20 percent of national and subnational budgets should be devoted to expenditure on education. In the following year, Education Law 20/2003 was issued to clarify that the 20 percent spending figure should not include teacher salaries. Government quickly realized, however, that spending 20 percent of national and subnational budgets on education, above and beyond teacher salaries, was infeasible. Thus, despite the law, in practice the 20 percent rule became interpreted to apply to all education expenditure. This practical interpretation of the 20 percent rule became operative in 2004. It effectively mandates all provinces and districts to spend a minimum of 20 percent of their budgets on education (OECD, 2015). World Bank (2013) argues that the expenditure mandate has led to large increases in public education spending and that student enrolments in junior and senior secondary school have risen significantly as a result.

3. Data

3.1 Compulsory Schooling

To examine the impact of Indonesian compulsory schooling policy on educational attainment we use data from the fourth and fifth waves of the Indonesia Family Life Survey (IFLS). IFLS is a social-demographic-economic household panel survey implemented by RAND. IFLS1 was executed in 1993; it constituted a representative sample of about 83 percent of the population at that time. IFLS4 and IFLS5 were fielded in 2007 and 2014, in turn. These two waves collected information on all original 1993 IFLS1 households along with any newly formed households derived from the originals. The number of households interviewed in the fourth and fifth IFLS waves totaled 13,535 and 16,204, respectively.

Our main dependent variable of interest from this data set is the educational attainment of Indonesians. We construct a dummy variable, relevant for the 1994 reforms, that indicates whether an individual has completed at least junior secondary school or not. We distinguish between individuals that would have been exposed to the compulsory schooling policy and those that would not have been exposed to the reforms. We (initially) assume that all children start school at age seven and that the cut-off date for determining a child's school age is 31 August. Given that assumption, any person born after 31 August 1978 would have been enrolled in the last year of junior secondary school when the policy was in place and those born before that date would not have been exposed to the reform. We include individuals born 15 years before and after the cut-off date to form our sample.

In addition to educational attainment, we employ data on several other covariates drawn from IFLS. Some of these variables focus on sociodemographic characteristics of children that are likely to be important in the context of education: gender, religion, ethnicity, and geographic and urban-rural location of residence. We also use a number of broad proxies of a child's economic situation, including whether the child's residence has access to electricity, piped water, and toilet facilities or not and the employment status of the head of household in which the child lives: wage employed, self-employed, or employed in the agricultural sector. Data on all the above variables are fixed at the time when the child was 12 years of age, that is, during what would have been his/her last year of primary school. Table 1 provides variable summary statistics for the compulsory schooling analysis, where the sample is divided into two groups: those exposed (the treated group) and those not exposed (the control group) to the relevant reform, according to official policies.

Table 1. Summary statistics for 1994 compulsory schooling analysis

	Control		Treatment	
Variable	Mean	Std. Dev	Mean	Std. Dev
Educational attainment	0.582	0.493	0.743	0.437
Child is male	0.518	0.500	0.478	0.500

Child is Islamic	0.899	0.301	0.900	0.300
Child is Javanese	0.430	0.495	0.407	0.491
Child lives on Java	0.575	0.494	0.528	0.499
Child lives in urban area	0.341	0.474	0.356	0.479
Household has electricity	0.349	0.477	0.604	0.489
Household has piped water	0.088	0.283	0.134	0.341
Household has toilet	0.271	0.444	0.399	0.490
Head is wage-employed	0.224	0.417	0.229	0.420
Head is self-employed	0.469	0.499	0.440	0.496
Head is employed in agriculture	0.100	0.300	0.099	0.299

Source: IFLS4/5. Control and treatment groups include all children born 15 years before and 15 years after 1 September 1978, respectively. Educational attainment of the child is defined as having completed at least junior secondary school. Child, household, and head of household characteristics are those for when the child was 12 years old.

3.2 Education Spending Mandate

To investigate the effects of Indonesia's constitutional spending mandate on district education outcomes we employ two data sets. The first is the Central Statistics Agency (BPS) annual socio-economic household survey (SUSENAS) and the second comprises local government fiscal data collected by the Ministry of Finance (MoF). Here our main dependent variables of concern are junior and senior secondary school net enrolment rates and the relevant data come from BPS/SUSENAS. [6] Principal fiscal data include those on total local government spending, the share of local government spending devoted to education, and household spending on education; these three variables are our main independent variables of interest. We also use data on other (control) variables: population, percent of population that is urban, percent of population that is poor, and gross regional domestic product (GRDP). All data are compiled at the district level. We have these data for three years before the mandate was implemented, 2001-2003, and for nine years after the policy took effect, 2004-2012.

[6] We focus on junior and senior secondary school enrolments and do not consider primary school enrolments in this analysis. During the period of concern to this study the latter were already quite high on average and there was relatively minor variation across districts.

There were 610 districts in operation at one time or another during those two periods. Table 2 supplies summary statistics on the above-mentioned variables, for each of the two periods.

Table 2. Summary statistics for education spending mandate analysis

	2001-2003		2004-2012	
Variable	Mean	Std. Dev.	Mean	Std. Dev
Net junior secondary school enrolment rate	61.0	13.7	65.8	11.6
Net senior secondary school enrolment rate	38.5	16.4	46.6	14.1
Average net secondary school enrolment rate	49.8	14.3	56.2	11.8
Total district spending per capita	3,161,480	3,495,709	3,811,372	3,881,810
District education budget share (percent)	34.4	14.3	32.8	11.4
HH education spending per school-aged child	43,903	48,983	93,699	99,452
Population	540,925	550,884	524,524	570,069
Percent of population that is urban	32.3	28.8	38.2	31.7
Percent of population that is poor	21.1	15.0	16.0	10.6
GRDP per capita (mln)	45.0	36.2	2.4	611.1

Source: SUSENAS and MoF. All economic and fiscal variables are measured in rupiah in constant 2010 terms.

4. Identification

4.1 Compulsory Schooling

We use regression discontinuity (RD) methods to identify the causal effects of Indonesia's 1994 compulsory schooling policy on junior secondary educational attainment. As mentioned above, Indonesia introduced nine-year compulsory schooling (that is, through junior secondary) in July 1994. Under the assumption that students start school at age seven and that the cut-off date for determining school age is 31 August, we can use an individual's month and year of birth to determine if he or she was exposed to the compulsory schooling policy.[7] All individuals born on or after 1 September 1978 would have been subjected to the nine-year compulsory schooling regulation and all individuals born before that date would not have

[7] We test the robustness of the empirical results to both assumptions below.

been exposed to the policy. In the RD model, an individual's date of birth (month and year) serves as the running variable and the specific date 1 September 1978 provides the threshold or cut-off. In RD parlance, students exposed to the compulsory schooling policy make up the treatment group and those not exposed constitute the control group.

Following Imbens and Lemieux (2007), define $Y_i(0)$ and $Y_i(1)$ to be potential educational attainments for child i where $Y_i(0)$ is educational attainment without exposure to compulsory schooling policy and $Y_i(1)$ is education attainment with exposure to compulsory schooling. In this case, the impact of compulsory schooling is given by $Y_i(1) - Y_i(0)$. Unfortunately, $Y_i(0)$ and $Y_i(1)$ cannot be observed simultaneously and so attention turns to the average effects of treatment, $Y_i(1) - Y_i(0)$, across subgroups of the population. Let $D_i=0$ if a child was not exposed to compulsory schooling and $D_i = 1$ if he or she was exposed to compulsory schooling. Observed outcomes, Y_i, are therefore $= Y_i(0)$ if $D_i = 0$ and $= Y_i(1)$ if $D_i = 1$. The average causal effect of compulsory schooling, τ, at the cut-off, c, is given by:

$$\tau = \mathrm{E}\left[Y_i(1) - Y_i(0) \mid X_i = c\right] = \mathrm{E}\left[(Y_i(1) \mid X_i = c\right] - \mathrm{E}\left[Y_i(0) \mid X_i = c\right] \quad (1)$$

The key identifying assumption in this framework is that $\mathrm{E}[Y_i(1) \mid X_i]$ and $\mathrm{E}[Y_i(0) \mid X_i]$ are continuous in X, individual's date of birth. This implies that all other unobserved determinants of educational attainment, Y, are also continuously related to X (Imbens and Lemieux, 2007). The implication allows one to use outcomes just below the cut-off as valid counterfactuals for those just above the cut-off (Skovron and Titiunik, 2015).

The general form of the estimating equation is:

$$Y_i = \tau D_i + g(X_i) + \mu_i \quad (2)$$

In equation (2), g(X) is a polynomial function of the running variable X, individual's date of birth; μ is the error term, and τ is the treatment effect, which is to be estimated.

In theory, equation (2) can be estimated by either non-parametric or parametric methods. Non-parametric estimation relies on continuously shrinking the bandwidth within which estimates are made and comparing observed outcomes just above the threshold with those just below. However, when the running variable is discrete, as is the case here, although there may be observations exactly at the cut-off there are no observations just below the threshold and therefore the needed comparison cannot be made (Lee and Card, 2008). In this instance, suggested practice is to estimate a parametric regression of Y on lower order polynomials of X, where identification is achieved through extrapolation, as based on the assumed functional form of the relationship between Y and X (Dong, 2015).

Recent research argues for the use of lower order polynomials in regressions of Y on X (Skovron and Titiunik, 2015; Gelman and Imbens, 2017) and we employ polynomials of degree one and two in our analysis. We estimate the following two equations.

$$Y_i = \alpha_1 + \tau_1 D_i + \beta_{11} X_i + \beta_{12} D_i X_i + \mu_{1i} \tag{3}$$

$$Y_i = \alpha_2 + \tau_2 D_i + \beta_{21} X_i + \beta_{22} D_i X_i + \beta_{23} X_i^2 + \beta_{24} D_i X_i^2 + \mu_{2i} \tag{4}$$

where all variables have been previously defined. Note that D_i interacts with polynomials in X_i, as usual. Parameters τ_1 and τ_2 provide the estimated treatment effects, that is, the causal impact of compulsory schooling on educational attainment.

We estimate equations (3) and (4) using both ordinary least squares (OLS) and probit regression methods around narrow windows (bandwidths) on each side of the cut-off. We select our bandwidths based on data driven procedures developed by Calonico, Cattaneo, Farrell, and Titiunik (2017). Two types of bandwidth selection criteria are employed: one that minimizes the mean square error of the point estimate and another that minimizes the asymptotic coverage error of the confidence intervals associated with the point estimate. After implementing the two procedures across all specifications we find that selected bandwidths vary between 24 and 48 months and therefore decide to estimate all our equations within different three bandwidths: 24, 36, and 48 months. Within the bandwidths we can say, heuristically at least, that the allocation of children across treatment and control groups is “as good as random" (Skovron and Titiunik, 2015). We cluster standard errors on the running variable, individual's date of birth, as is commonly done when the variable is discrete.

The methods described here identify a local average treatment effect (Lee and Lemieux, 2010). It is perhaps useful to emphasize the local character of estimated treatment effects. While the internal validity of the estimated effects is typically argued to be strong, external validity is usually thought to be relatively weak. This suggests that it may be unreasonable to generalize about the impact of compulsory schooling on educational attainment at values of the running variable outside a narrow range around the cut-off.

4.2 Education Spending Mandate

As previously mentioned, the recent amendment to Indonesia's constitution insists that the national government and all subnational governments—provinces and districts—spend at least 20 percent of their budgets on education. The spending mandate's implicit assumption is that rising education budget shares should lead to improvements in education outcomes. This study focuses on local governments and, in this context, we examine the impact of education spending shares on school enrolments at the district level.

Estimating the causal effects of education allocations and spending on enrolments is not straightforward, however, for two reasons. First, enrolments in one year are likely to be a function of enrolments in the previous year. That is, the determination of school enrolments should be specified as dynamic. Second, district spending and education budget shares are probably endogenous to enrolments, as a function of reverse causality. Districts with relatively low (high) enrolments, for example, may spend comparatively more (less) on education. Given these two issues, we specify a dynamic panel data (DPD) model[8], estimated

[8] The methods used here are based on an extensive line of research. See Arellano and Bond (1991), Arellano and Bover (1995), and Blundell and Bond (1998) for introductions to the literature.

by difference-generalized method of the moments (diff-GMM) techniques, to investigate the causal effects of endogenous education spending on school enrolments.

The DPD model can be specified as follows.

$$S_{it} = \alpha S_{it-1} + \beta_1 W_{it} + \beta_2 X_{it} + \upsilon_i + \varepsilon_{it} \quad (5)$$

In equation (5), i represents the district and t is year, respectively; S is the net enrolment rate of students, for either junior or senior secondary school; W is a set of endogenous variables; X is a collection of strictly exogenous variables; υ are unobserved time invariant fixed effects, ε is the random error term; and α, β_1, and β_2 are the parameters to be estimated, where attention focuses on $\beta_{1,}$ which provides the causal effects of the various endogenous variables on enrolments.

Enrolment rates in the present period are posited to be a function of enrolment rates in the previous period. Endogenous variables in W include the log of total local government expenditure per capita and the share of total spending devoted to education. Also included in W is the log of household expenditure on education per student-aged person; the variable is also likely to be endogenous due to reverse causality. Exogenous variables in X comprise log of population, percentage of population that is urban, percent of the population that is poor, and log of GRDP per capita. Time dummies are also included in the model.

To avoid dynamic panel bias, i.e. the bias that results from the fact that S_{ti-1} is endogenous to the fixed effects, we estimate equation (5) in first differences (Nickell, 1981).

$$\Delta S_{it} = \alpha \Delta S_{it-1} + \beta_1 \Delta W_{it} + \beta_2 \Delta X_{it} + \Delta \varepsilon_{it} \quad (6)$$

Differencing removes the fixed effects, but the lagged dependent variable is still endogenous since S_{it-1} in $\Delta S_{it-1} = S_{it-1} - S_{it-2}$ is correlated with ε_{it-1} in $\Delta\varepsilon_{it} = \varepsilon_{it} - \varepsilon_{it-1}$. In addition, ΔW_{it} is endogenous since both W_{it} and W_{it-1} in $\Delta W_{it} = W_{it} - W_{it-1}$ are correlated with ε_{it} and ε_{it-1} in $\Delta\varepsilon_{it} = \varepsilon_{it} - \varepsilon_{it-1}$. The endogeneity can be accommodated by using lagged values of the untransformed variables in question as instruments. Derived moment conditions associated with the diff-GMM estimation of equation (6) imply that lagged levels (first lags and beyond) of S_{it-1} can serve as instruments for ΔS_{it-1} and lagged levels (second and beyond) of W_{it} can serve as instruments for ΔW_{it}. To avoid over-instrumentation, our model estimation employs only three lags of the relevant variables to construct instruments and collapses those instruments to reduce their number further (Roodman, 2009a).[9,10]

5. Results and Discussion

5.1 Compulsory Schooling

We now evaluate the impact of Indonesia's 1994 compulsory schooling policy on individuals' junior secondary educational attainment. First, we test for manipulation of the forcing variable, individual's date of birth. Second, we estimate intent-to-treat (ITT) effects of compulsory schooling on educational attainment. Third, we examine treatment-on-the-treated (TOT) effects. Finally, we estimate ITT effects on a broad range of child subgroups.

5.1.1 Forcing Variable Manipulation

For the RD approach to be valid there must be no manipulation of the forcing variable, individual's month and year of birth, around the cut-off point. Such manipulation is unlikely under the current circumstances, since an individual's date of birth was determined well

[9] Over-instrumentation can result in two main problems. First, the use of too many instruments may over-fit the endogenous variables, thus inadequately expunging the endogeneity and biasing coefficient estimates in the process. Second, the employment of an excessive number of instruments may also create problems for the Hansen test of instrument exogeneity; that is, if the number of instruments becomes too large, the test is invalidated. More information on the use of Hansen's test will be provided later. See Roodman (2009a).

[10] Diff-GMM estimation of the DPD model is carried out using the Stata command 'xtabond2' developed by Roodman (2009b).

before the announcement and execution of the policy. Still, the distribution of individuals' birth dates across the months of any given year is not uniform (which may, in part, be a function of parental preferences) and so some manipulation may have occurred. To be safe, we test for forcing variable manipulation.

Figure 1 shows the distribution of individuals' date of birth. The latter have been normalized around the month of September 1978, the earliest month and year of birth for which children would have been exposed to the 1994 policy, under the assumption that children start school at age seven and the cut-off for determining a child's school age is 31 August. Visual inspection of the distribution does not suggest any discontinuity around the cut-off (shown by the vertical line at zero). A formal test of the null hypothesis that no discontinuity exists at the threshold, using a procedure developed by Cattaneo, Jansson, and Ma (2016), indicates that the null cannot be rejected. Specifically, the robust bias-corrected test statistic, using a polynomial of degree two, a triangular kernel, with jack-knifed standard errors (the default procedures) is -1.322 and the p value is 0.186. The evidence presented here strongly implies no manipulation of the forcing variable at the threshold.

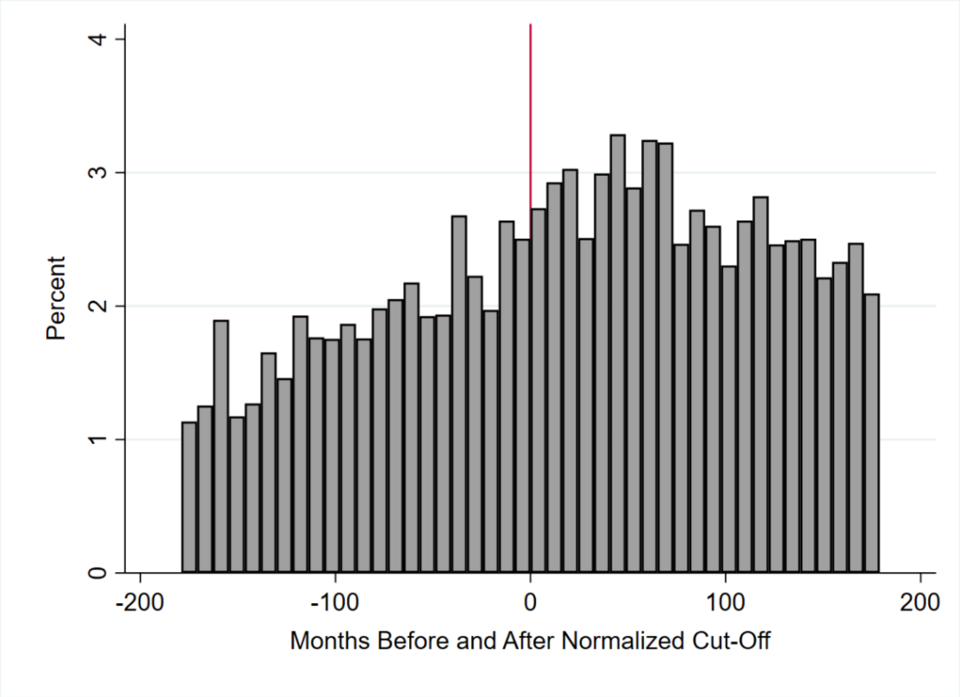

Figure 1. Distribution of normalized running variable, individuals' date of birth

5.1.2 Intent-to-Treat Effects

We continue our assessment of the impact of Indonesia’s 1994 compulsory schooling policy on educational attainment by inspecting the standard RD plot. See Figure 2. The plot shows the relationship between educational attainment, as proxied by a dummy variable indicating whether the individual has completed junior secondary school or not, and individuals’ date of birth. The dots in the plot are termed bins. The bins represent average values of educational attainment over some range, where bin range or length is chosen based on data driven methods to mimic the underlying regression function. See Calonico, Cattaneo and Titiunik (2014) for a detailed explanation. Plots are drawn using a uniform kernel[11] with polynomial of degree four, as suggested by Skovron and Titiunik (2015).

Attention is drawn to the variables’ association at the cut-off, that is, the point in time at which first cohort—born in September 1978—would have been exposed to the policy reform. As the figure shows, there is no obvious jump in educational attainment at the cut-off. This implies that compulsory schooling policy had no impact on child educational attainment. It’s merely a suggestion, however; a firm conclusion can only be reached after a formal estimation of treatment impact. We now turn to a proper estimation of those treatment effects.

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[11] Some analysts prefer to use a triangular kernel, which weights observations near the cut-off more heavily. We use a uniform kernel to be consistent with our OLS and probit regressions, which weight observations equally.

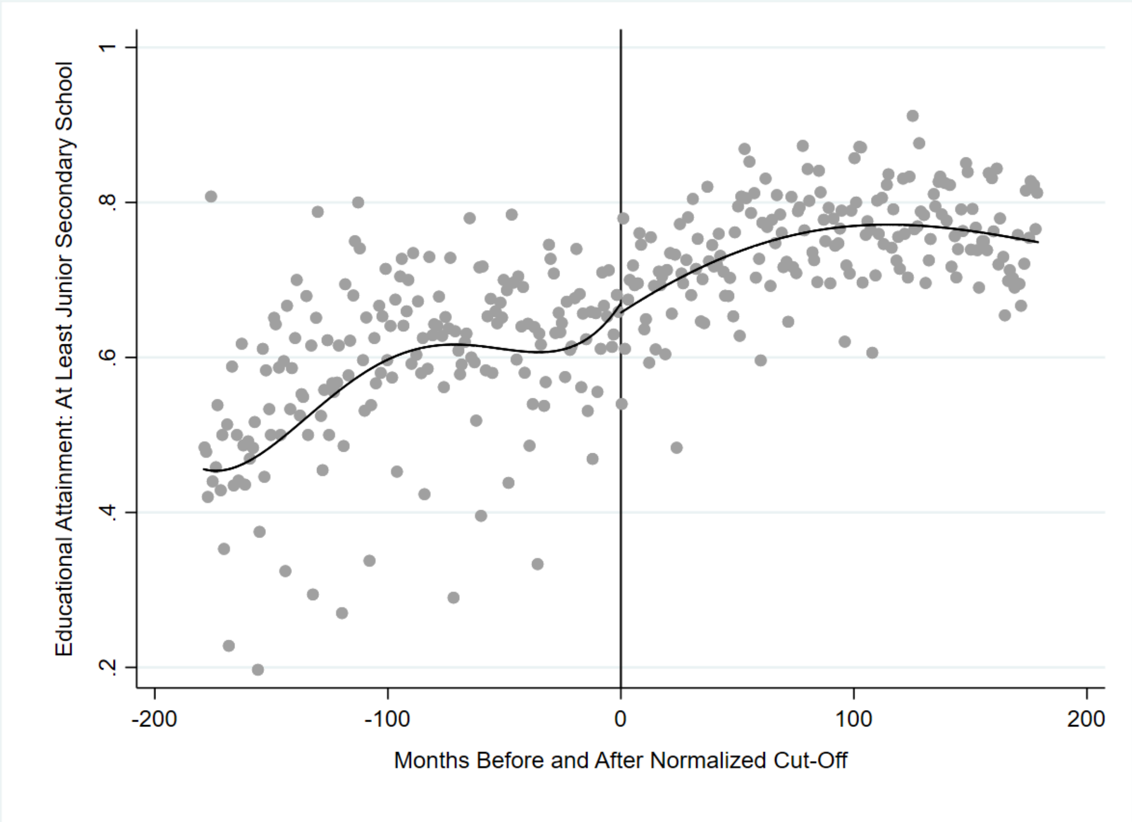


Figure 2. RD plot for compulsory education and educational attainment

To formally determine the treatment effects, we estimate equations (3) and (4). We estimate each equation by OLS and probit methods. We estimate both specifications within narrow bandwidths around the cut-off. Table 3 presents the results. It shows the estimated treatment effects, associated robust standard errors (clustered on the running variable), and an indication of statistical significance of the estimates for all specifications mentioned above.

These estimates represent intent-to-treat (ITT) effects since they concern the impact of the policy as designed or intended, assuming rules regarding the determination of a child's school age and child's age at entry have been followed. It is important to note at this early stage that not all individuals assigned to the treatment group will have been exposed to the compulsory schooling reform, in fact, and some individuals assigned to the control group will, in the event, have been subjected to the policy reform. We take this point up in further detail below.

Table 3. ITT effects of 1994 compulsory schooling policy

Polynomial Degree One					
		OLS		Probit	
BW	Observations	τ	SE	τ	SE
24	4,059	0.029	0.039	0.030	0.040
36	5,984	0.007	0.035	0.008	0.035
48	8,079	0.019	0.027	0.019	0.027

Polynomial Degree Two					
		OLS		Probit	
BW	Observations	τ	SE	τ	SE
24	4,059	-0.049	0.055	-0.048	0.054
36	5,984	0.031	0.052	0.030	0.051
48	8,079	0.001	0.041	0.000	0.041

BW is the bandwidth in months. τ is the estimated treatment effect. SE is the standard error. ** and * indicate that the estimated coefficient is statistically significant at the 0.01 and 0.05 level, respectively.

As the table indicates, none of the estimated treatment effects is statistically significant at the 0.05 level. Coefficient signs are mostly positive, the two exceptions being those for the OLS and probit estimations using second degree polynomials and 24-month bandwidths, but none of the ITT estimates is statistically significant at conventional levels. [12] We tentatively conclude that the 1994 national compulsory schooling policy did not have the intended impact on educational attainment, as the RD plot suggested might be the case.

A possible criticism regarding the estimates presented in Table 3 and the derived conclusion that the compulsory schooling policy reform had no impact concerns the nature of the running variable. As Dong (2015) has shown, standard treatment effects estimates are inconsistent when the running variable is discrete and rounded, as ours is here. She

[12] We also used the covariates discussed in the data section (see Table 1) in ITT effects estimation to gauge the extent to which they might improve the precision of the estimate effects and render them statistically significant. Their inclusion in the regressions did not cause any of the ITT estimates to become statistically significant.

demonstrates that consistent adjusted estimates of the treatment effects, as based on estimated equations (3) and (4) for example, can be derived as follows.

$$\tau_{adj} = \hat{\tau}_1 - \tfrac{1}{2}\hat{\beta}_2 \tag{7}$$

$$\tau_{adj} = \hat{\tau}_2 - \tfrac{1}{2}\hat{\beta}_2 + \tfrac{1}{6}\hat{\beta}_4 \tag{8}$$

A sufficient condition for no statistically significant rounding bias is that $\hat{\beta}_2$ in estimated equation (7) is equal to zero and $\hat{\beta}_2$ and $\hat{\beta}_4$ in estimated equation (8) are jointly equal to zero (Dong, 2015). We perform standard t and F tests for the null hypotheses that $\hat{\beta}_2$ is equal to zero and $\hat{\beta}_2$ and $\hat{\beta}_4$ are jointly equal to zero, respectively, across relevant estimated equations and are unable to reject the null in any case. We conclude that the estimates presented in Table 3 are not significantly affected by any bias that results from the discrete nature of our running variable.

5.1.3 Treatment-on-the-Treated Effects

Another potentially problematic issue concerns the ITT nature of the estimated treatment effects. As noted above, not all children assigned to the treatment group would have been exposed to the compulsory schooling policy. For example, if some students started school earlier than age seven, as determined by the school-age entry rules[13], then some proportion of children who were assigned to treatment may have already graduated from junior secondary school by the time the policy took effect. In this case, children classified as treated will not have been treated. Likewise, some children assigned to the control group may have nevertheless been exposed to the reform. This could have happened, for example, if children started school later than age seven. In this instance, a portion of children assigned to the control group would have received treatment.

[13] See Barakat (2016) for the claim that many students start school earlier than age seven.

These examples suggest that it may be useful to estimate the treatment-on-the-treated (TOT) effects as well. The TOT effects provide the impact of treatment on those who have, in fact, received treatment as opposed to those who were only intended to be treated by the policy. While the ITT is typically claimed to be more important for the evaluation of policy, the TOT may be more relevant on pure substantive grounds (Angrist and Pischke, 2008).

The relationship between estimated ITT and TOT effects can be written as follows (Angrist and Pischke, 2008).

$$TOT = \frac{ITT}{\Pr(Treated \mid X \geq c) - \Pr(Treated \mid X < c)} \tag{9}$$

The equation shows that the TOT effects are equal to scaled-up ITT effects, where the scaling factor is a function of the probability that a child was treated, given assignment to the treatment group, minus the probability that a child was treated, given assignment to the control group (i.e. the compliance rate). The estimated TOT effect will be larger in magnitude (in absolute value terms, that is) than the estimated ITT effect since it assesses the impact just on those children who received treatment while the latter includes some targeted children that have not, in fact, been exposed to treatment.

Our data allow us to identify individuals who were actually in junior secondary school during the time of the reform and those who were not. As such, we can estimate the two probabilities in the denominator of equation (8). The probabilities are shown in Table 4, along with their standard errors, for each of the three bandwidths used in estimating treatment effects here. Note that the probability that an individual was treated given assignment to the treatment group rises as the bandwidth increases, from 54 to 72 percent, and the probability that he or she was treated given assignment to the control group declines as bandwidth expands, from nine to six percent. These outcomes are logical given the underlying reasons for incorrect group assignment outlined above, i.e. as related to early- or late-aged school start. As the bandwidth expands after the cut-off it will include a smaller and smaller share of students that would have started school early enough to have already graduated from junior high school, thus increasing the probability that students have been correctly assigned to treatment; and as the bandwidth rises before the cut-off it will comprise a smaller and smaller portion of students who started school late enough to still be in junior secondary school, decreasing the probability that students have been incorrectly assigned to the control group.

Table 4. Probability of actually receiving treatment around the cut-off

BW	$Pr(T\|x\geq c)$	SE	$Pr(T\|x<c)$	SE	Difference
24	0.543	0.011	0.089	0.007	0.454
36	0.646	0.008	0.070	0.005	0.576
48	0.725	0.007	0.061	0.004	0.664

BW is bandwidth in months. $Pr(T|x\geq c)$ and $Pr(T|x<c)$ are the probabilities that students actually received treatment, given assignment to treatment and control groups, respectively. SE is the standard error.

With the data provided in Table 4 we can easily calculate the magnitude of the TOT effects. They are equal to the ITT effects as shown in Table 3 divided by the relevant difference in probabilities shown in Table 4. We apply bootstrapping methods (with 1,000 repetitions) to determine the associated standard errors of the ratio shown in equation (9). Table 5 provides the estimated TOT effects, along with the bootstrapped standard errors, for each our models.

Table 5. TOT effects of 1994 compulsory schooling policy

Polynomial Degree One					
		OLS		Probit	
BW	Observations	τ	BSE	τ	SE
24	4,059	0.065	0.065	0.066	0.065
36	5,984	0.013	0.043	0.013	0.043
48	8,079	0.028	0.031	0.028	0.032
Polynomial Degree Two					
		OLS		Probit	
BW	Observations	τ	BSE	τ	SE
24	4,059	-0.108	0.096	-0.106	0.099
36	5,984	0.054	0.063	0.052	0.065
48	8,079	0.001	0.047	0.001	0.048

BW is the bandwidth in months. τ is the estimated treatment effect. BSE is the bootstrapped standard error. ** and * indicate that the estimated coefficient is statistically significant at the 0.01 and 0.05 level, respectively.

Not surprisingly, given that all estimated ITT effects were not significantly different from zero, we find that none of the estimated TOT effects is statistically significant either. We infer that the 1994 national compulsory schooling policy, in general, had no impact on either those children who were intended to be treated or those who received treatment. [14]

5.1.4 Intent-to-Treat Effects on Child Subgroups

Finally, we estimate the ITT effects across important subgroups of the child population. As noted in the introduction to this paper, past research has shown that children's demographic, social, and economic characteristics can be important in determining the extent to which they benefit from compulsory schooling policies (Lleras-Muney, 2002; Bell, Costa, Machin, 2016). We focus on the child's gender, religion, ethnicity, and geographic and urban-rural location; the child's household's access to electricity, piped water, and toilet facilities; and the child's household head's employment status, that is, whether he or she was wage-employed, self-employed, or employed in the agricultural sector. All variables are dummies and separate estimations are made for the dummies at values equal to zero and one.

Table 6 provides the output. We show only the OLS estimation results at 36-month bandwidths only to conserve space. As the table demonstrates, again, none of the estimated treatment effects is statistically significant at the 0.05 level. Conclusions regarding the lack of statistical significance based on the estimation results presented in the table are robust with respect to estimation method (OLS or probit) and to bandwidth choice (24, 36, or 48 months).

[14] Individuals may have also been incorrectly assigned to treatment or control if the cut-off date for determining a child's school age was ignored or if it varied across schools. To test the robustness of results regarding changes to the age cut-off date we reran our model using 6 different end-of-month thresholds, three before end of August and three after: May, June, July and September, October, and November. We found no significant ITT or TOT effects for any of the alternative thresholds.

As before, estimated TOT effects are also nonexistent, although we do not show those results here due to lack of space. We surmise that the compulsory schooling reform had no impact on major subgroups of the student population either.

Table 6. ITT effects of 1994 compulsory schooling policy for subgroups

Variable	Value	Observations	τ	SE
Child is male	0	2,926	0.002	0.039
	1	3,058	0.010	0.043
Child is Islamic	0	590	-0.051	0.069
	1	5,394	0.015	0.039
Child is Javanese	0	3,462	0.005	0.044
	1	2,522	0.007	0.038
Child lives on Java	0	2,699	0.031	0.062
	1	3,234	0.036	0.033
Child lives in urban area	0	3,852	0.008	0.042
	1	2,073	0.001	0.030
Household has electricity	0	3,043	-0.024	0.046
	1	2,941	0.032	0.028
Household has piped water	0	5,297	0.007	0.035
	1	687	0.018	0.058
Household has toilet	0	4,005	0.015	0.038
	1	1,979	-0.027	0.030
Head is wage employed	0	4,628	-0.006	0.036
	1	1,356	0.055	0.037
Head is self-employed	0	3,271	0.012	0.038
	1	2,713	0.010	0.042
Head is employed in agriculture	0	5,412	0.007	0.036
	1	572	0.021	0.077

Estimation is by OLS. BW (bandwidth) is 36 months for all regressions. τ is the estimated treatment effect. SE is the standard error. ** and * indicate that the estimated coefficient is statistically significant at the 0.01 and 0.05 level, respectively.

Why did Indonesia’s 1994 compulsory schooling policy fail to such a considerable extent? We argue that the failure can be explained by two main reasons. First, government did not provide any additional funds to support policy implementation, say for example, to build additional schools or reduce school fees. Indonesia’s own experience with school construction shows that the availability of a greater number of schools can spur enrolments (Duflo, 2001). And the China case demonstrates that reducing school fees can facilitate positive impact of compulsory education policy (Xiao, Li, and Zhao, 2017). Second, and perhaps more importantly, government made insufficient effort to enforce the policy. No school-level procedures were instituted to encourage child compliance with the policy and no fines or other penalties were legally established for parents of children who did not complete the requisite number of school years, as they have been in other countries. A significant amount of research demonstrates the importance of enforcement of one kind or another in the successful implementation of compulsory schooling initiatives (Lang and Kropp, 1986; Lleras-Muney, 2002; Black, Devereux, and Salvanes, 2008; Cabus and De Witte, 2011)

5.2 Education Spending Mandate

This subsection of the paper examines Indonesia's education expenditure mandate at the district level. We address four main questions. First, do district education budget shares matter in the determination of secondary school enrolment rates? Second, has the spending mandate in and of itself affected district education budgeting behavior? Third, has central government effectively enforced the education spending mandate at the district level? Fourth, what would be the impact of education budget shares on secondary school enrolments if all districts consistently complied with the mandate, by either allocating at least 20 percent of their expenditure budgets to education or precisely 20 percent of their budgets to education?

5.2.1 Budget Share Impact on Enrolments

The constitutional amendment codifying the expenditure mandate implicitly assumes education budget shares are a relevant target in the context of attempts to improve education outcomes. To investigate the impact of education budget shares on student enrolment rates we estimate equation (5). We estimate three versions of the specification, whereby the dependent variable is set equal to net junior secondary school enrolment, net senior secondary school enrolment, and the simple average of the two enrolment rates, in turn.

Table 7 presents the estimation output. In the current context, the result of principal concern is the impact of district education budget shares on enrolment rates. As the table shows, an increase of one percentage point in the budget share causes about a one percentage point rise in net enrollment rates for all three regressions. The estimated effects are quite large, and the coefficients are all statistically significant at the 0.01 level. We conclude that the share of district budgets devoted to education matters in the determination of net junior and senior secondary school enrolment rates, all else being equal. The initial supposition is that the mandate's focus on budget shares may indeed be reasonable. The output also suggests that a one percent increase in total district spending per capita leads to a 0.21 percentage point increase in the enrolment rate and that a one percent rise in household expenditure on education per school-aged child causes a 0.06 percentage point increase in the enrolment rate. The results for the other two regressions are similar.

Table 7. Explaining school enrolment rates, 2004-2012[1]

	Average Enrolments			Junior Secondary Enrolments			Senior Secondary Enrolments		
Independent Variables	Coef.	SE		Coef.	SE		Coef.	SE	
Lagged enrolment	0.480	0.062	**	0.370	0.052	**	0.367	0.054	**
Log of total local government spending per capita	20.620	6.169	**	23.240	7.895	**	26.815	7.425	**
Local government education budget share	0.940	0.308	**	1.086	0.391	**	1.098	0.345	**
Log of household spending on education per school-aged child	6.018	2.812	*	7.297	3.633	*	7.253	3.319	*
Exogenous controls	yes			yes			yes		
Time fixed effects	yes			yes			yes		
Number of observations	3,104			3,109			3,104		
Number of cross section units	531			533			531		
Number of instruments	25			25			25		
	Stat	p		Stat	p		Stat	p	
Wald	407.97	0.000		183.08	0.000		371.60	0.000	
Arellano-Bond test for AR(2) in first differences	-0.29	0.771		-0.37	0.709		0.04	0.969	
Hansen test of overriding restrictions	9.36	0.313		10.82	0.212		6.98	0.539	
Difference-in-Hansen tests of exogeneity of instrument subsets:									
GMM for lagged enrolments	4.25	0.236		5.00	0.172		5.04	0.169	
GMM for log of total local government spending per capita	3.24	0.356		1.30	0.728		2.55	0.466	
GMM for local government education budget share	0.82	0.846		1.31	0.726		0.10	0.991	
GMM for log of household spending on education per school-aged child	5.38	0.146		7.81	0.052		4.22	0.220	

[1] Dependent variable is listed across the first row. All economic and fiscal variables are measured in constant 2010 terms. Standard errors (SE) are cluster-robust. ** and * indicate statistical significance at the 0.01 and 0.05 levels, respectively.

The question naturally arises as to the relative importance of the three spending variables in determining enrolments. We estimate the standardized beta coefficients for total spending per capita, education budget shares, and household spending per school aged-child and find that they equal 1.241, 0.909, and 0.426, respectively. Total local government spending is the most important variable, by a reasonably large margin, in explaining average enrolments, followed by education budget shares and household spending on education.[15]

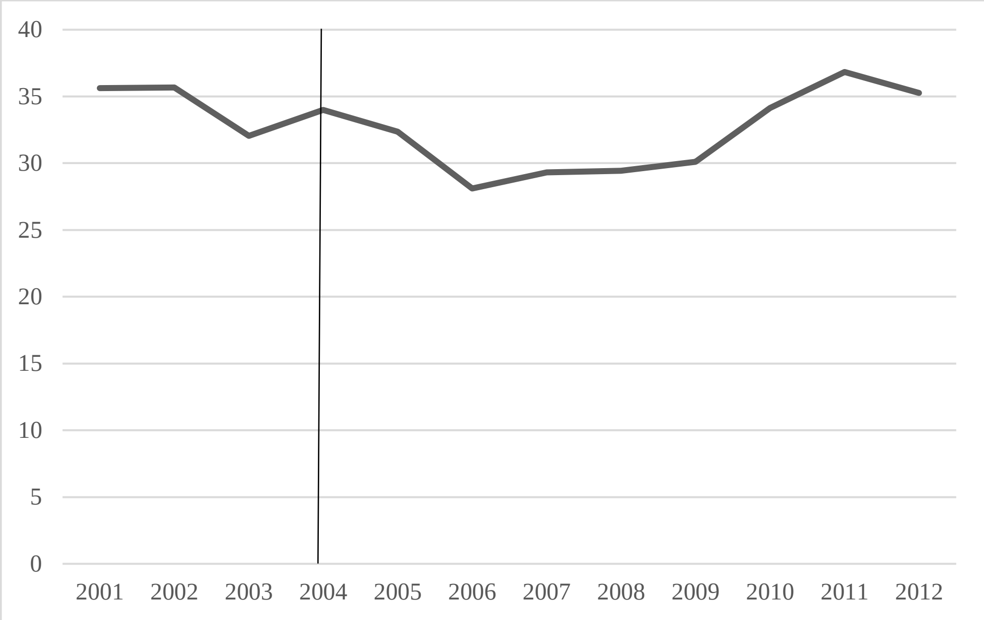
Finally, the diagnostic statistics shown at the bottom of the table imply that the models are well behaved. The Wald statistics are highly significant in all regressions, suggesting good fits. The Arellano-Bond test statistics are insignificant for all models, indicating that the null hypothesis that the error terms are not serially correlated in the second degree cannot be rejected. The various Hansen's statistics imply that the null hypothesis that instruments are exogenous, collectively or by subset, cannot be rejected.

5.2.2 Expenditure Mandate Impact on Local Government Budgeting

To gauge the extent to which the expenditure mandate may have affected district budgeting behavior, we begin by inspecting district education budget shares before and after the mandate took effect. Figure 3 shows the average share of districts budgets devoted to education since decentralization began implementation in 2001. The figure demonstrates that average budget shares have been reasonably steady over the 12-year period, ranging from just lower than 30 percent to slightly higher than 35 percent, significantly more than the mandated share. The average budget share between 2001 and 2003, that is, the period before the mandate became effective was 34.4 percent, while the average budget share between 2004 and 2012, when the mandate was in force, was not much different, at 32.8 percent. We draw

[15] The estimated standardized beta coefficients of the exogenous control variables were substantially smaller than those of total district spending, district education budget shares, and household education spending.

two conclusions: first, the spending mandate has proved ineffective in stimulating increases in education expenditure relative to other types of outlays since it was introduced and second, it is largely redundant.



Source: MoF.

Figure 3: Average district education budget shares

5.2.3 Central Government Enforcement of Mandate

While local governments in the aggregate appear to have been largely unaffected by the mandate and average district budget shares are significantly greater than the targeted 20 percent, it would be useful to know more precisely the extent to which districts complied with the 20 percent target before and after it became policy. Table 8 shows the total number of districts existing in a year and the number and percent of districts that did not meet the target, both for any given year and cumulatively for 2001-2003 and 2004-2012.

The information suggests two points. First, most districts met the target across the years shown; the non-compliance rate for any individual year varied between only 10 and 20 percent over the entire period and did not diverge much across the two sub-periods. Second, the cumulative noncompliance rate before the mandate was effective was lower, 24 percent, than the cumulative rate after the mandate was implemented, 39 percent. These results support the argument made above that the spending mandate was largely ineffective in positively influencing local government education budgeting behavior and demonstrates that central government enforcement of the mandate has been lax.

Table 8. District compliance with 20 percent rule, 2001-2012

	Number of Districts		
Year	Total	Ed Share < 20%	Percent
2001	348	69	19.8
2002	385	43	11.2
2003	434	56	12.9
2001-2003	472	111	23.5
2004	434	47	10.8
2005	434	52	12.0
2006	434	75	17.3
2007	459	50	10.9
2008	477	75	15.7
2009	491	68	13.8
2010	491	47	9.6
2011	491	49	10.0
2012	491	51	10.4
2004-2012	535	208	38.9

Source: MoF. The cumulative figures for total number of districts and total number of non-complying districts at the end of the indicated period represent the total number of individual districts and the total number of individual non-complying districts existing during the indicated period.

5.2.4 Local Government Compliance with the Mandate

Given that not all districts have complied with the education spending mandate it is natural to wonder what the impact on enrolments would be if they did minimally conform to the mandate by allocating at least 20 percent of their budget to education. A related question concerns the impact on enrolments if all districts were to allocate precisely 20 percent of their budgets to education. Regarding the latter, suppose the central government were to impose additional spending mandates and, at the same time, it became more adept at enforcing those mandates. At some point the new mandates might begin to crowd out budget shares for education. Government has already imposed a similar mandate for health spending—10 percent of budgets (Lewis 2017b). Current policy discussions in Indonesia suggest that a new mandate may be enacted for infrastructure spending, amounting to 25 percent of budgets. In an environment with many mandates and sound enforcement, it is possible that mandated budget share floors may at the same time become ceilings as well (World Bank, 2013).

We address these two concerns by simulating the impact of transformed budget shares on enrolments, as based on predictions from estimated equation (5). We perform two counterfactual simulations. First, we predict average enrolment rates for the counterfactual scenario under which all districts spend at least 20 percent of their budgets on education. Second, we predict average enrolments for a scenario under which all districts spend exactly 20 percent of their budgets on education. The results are presented in Table 9.

Table 9. Actual and predicted average district school enrolment rates, 2004-2012

	Actual	Predicted		
		Actual Ed Budget Shares	Ed Budget Shares At Least 20%	Ed Budget Shares All 20%
Mean	56.2	56.2	57.1	44.2
SE	0.212	0.236	0.225	0.226
95% CI	55.8 --- 56.6	55.8 --- 56.7	56.6 --- 57.5	43.8 --- 44.6

SE is the standard error. CI is confidence interval.

The table shows the actual average enrolment rates for junior and senior secondary school—56.2 percent—and three sets of predictions. The first is a prediction of average enrolment rates using actual values of all right-hand side variables. This is done just to confirm that the average predicted enrolment rate is the same as the actual average enrolment rate, as would be expected in a regression. The other predictions simulate the two counterfactuals described above. The first predicts average enrolment rates by fixing all budget shares that are less than 20 percent to precisely 20 percent, while using actual values for all other variables. The second predicts average enrolment rates by setting all budget shares exactly equal to 20 percent, while employing actual values for all other variables.

The first simulation suggests that insisting that all districts comply with the mandate would have only a minor impact on average enrolment rates. In this case, the enrolment rate increases by less than one percentage point compared to the actual rate, to 57.1 percent. This outcome derives from the fact that, on average, districts already spend considerably more than the mandated share, and increasing all budget shares to at least 20 percent does not appreciably affect that, and because other variables—total local government spending—are more important than budget shares in determining enrolments, as shown above.

On the other hand, the second simulation demonstrates the possibility of quite substantial effects if all districts were to precisely adhere to the mandated education budget share. In this case, the enrolment rate declines by 12 percentage points, to 44.2 percent. This implies that significant enrolment losses may obtain if the mandated floor for education were to become a ceiling, as government implements (and enforces) new spending mandates in other sectors and education spending is crowded out.

Of course, any school enrolment loses would have to be weighed against possible service access gains in newly targeted sectors. Furthermore, any relative changes in access to education and other services that may result from the introduction of new mandates can only be assessed properly in the context of citizen preferences for all services. In this framework, the above simulation highlights a general and well-known problem with mandates: by specifying levels of sectoral spending, they reduce local authorities' ability to allocate funds across budget categories in a manner that delivers the mix of public services demanded by their voters. That is, mandates constrain the efficient provision of services (Mikesell, 2011).

6. Conclusions

This paper has examined the effectiveness of two of Indonesia's major education policy initiatives in encouraging higher participation rates in secondary education: compulsory schooling and education sector spending mandates.

We find that Indonesia's 1994 nine-year compulsory schooling policy had no effect on child educational attainment. The program had no substantive impact on children it intended to expose to the reform or on those who were, in the event, actually exposed to it. The policy had no effect on any subgroup of children either, as classified by gender, religion, ethnicity, location, or socio-economic status. The derived result is robust across a wide range of model specifications and assumptions. The complete lack of effect is an exceptional finding among studies that have examined such reforms. We argue that the primary reasons for compulsory education policy failure in Indonesia are that government did not support the initiative with sufficient additional funding and that it was lax in enforcing the policy.

In 2015, Indonesia launched its 12-year compulsory schooling program. As with the 1994 effort, it is also not supported by any significant increases in funding and no mechanisms have been put in place to ensure it is enforced. If the status quo prevails then effective implementation of this effort is also in doubt.

We determine that Indonesia's education spending mandate policy has been largely ineffective in influencing child participation in school. First, the bulk of local governments budget more for education than the required share of 20 percent and have done so since well before the policy was introduced; this suggests that the spending mandate is simply redundant for many districts. Second, the reform has had no discernable effect on encouraging lagging districts to increase their education budget shares to mandated levels, due in part to weak enforcement of the policy. Third, total district spending on all local service delivery functions, including health and infrastructure, is more important than the portion of budgets devoted just to education in determining enrolments; and this implies that targeting budget shares in the education sector is less than optimal.

Undaunted by the experience in education, government has started to employ expenditure mandates more broadly across other sectors as well. It has already introduced a health spending mandate—10 percent of public budgets—and it is considering the application of an even larger mandate in infrastructure as well.

A common feature of compulsory schooling and education spending mandate policies in Indonesia is their laissez-faire enforcement. Government made no apparent effort to enforce either policy. Better enforcement would probably be beneficial in the case of compulsory schooling—especially if accompanied by additional fiscal resources for local governments. That is, improved enforcement has the potential, at least, to positively affect secondary school participation, as has been found in many other countries.

The probable impact of better enforced spending mandates is dissimilar. The major risk in this instance is that government begins to enforce mandates more rigorously, as they are applied to an ever-growing number of sectors: proliferation of strictly enforced mandates would likely severely constrain the efficient delivery of education and other local services. The research in this paper strongly implies that Indonesia's expenditure mandate policies are ill-advised, and that government may wish to reconsider their use, both in education and in other sectors.

The results of this study may be particularly relevant for other countries, especially in the developing world, that have embraced education policies of the kind that have become standard across nations but have done so in an unpremeditated manner. We suspect that the casual application of such efforts may have achieved similarly disappointing results in at least some other like cases. We cannot be certain of this, however, given the lack of pertinent research. The examination here suggests, therefore, that further comparative work to assess the effects of these prevalent policies, especially in developing nations, would be useful.

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