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THE IMPACT OF COMPULSORY SCHOOLING EXPANSION ON EDUCATIONAL OUTCOMES: THE CASE OF INDONESIA

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Title: The Impact of Compulsory Schooling Expansion on Educational Outcomes: The Case of Indonesia

Abstract: Compulsory schooling reforms have been frequently used for expanding access to higher levels of education. However, these laws may not translate immediately into human capital gains. In this paper, we assess the impact of compulsory schooling expansion in Indonesia on a set of educational outcomes. The identification strategy exploits the discontinuity in the exposure to the reform according to individual's month and year of birth adopting a sharp regression discontinuity design. Our main results indicate that the reform successfully increased expected years of education and the probability of completing junior secondary education in the middle and long term. The reform also shaped the propensity to enroll and complete senior secondary schooling. However, the policy did not affect university attendance. Upon closer examination of heterogeneous effects, we found that the reform's impact was more substantial on individuals living in urban compared to those living in rural areas across both Java and Non-Java Islands. Moreover, when we investigate the heterogenous effects of parental education along with the gender of the child, male children with a low parental education experienced a greater effect from the reform. In contrast, female children of highly educated parents were more affected by the reform.

JEL Codes: I21, I25, I28

Keywords: Compulsory education, educational outcomes, regression discontinuity

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1. Introduction

Compulsory education laws are a tool for enhancing access to education. These laws substitute the decision-making capacity of individuals in order to correct information failures, maximize social positive externalities, and feed the economic system with skilled workers. The starting age and extension of compulsory education varies widely, the former being in most developed countries being ages 5-6 and, the latter, between 9 and 12 years (*UIS*, 2022). In developing countries, compulsory education laws have also been used as tools for expanding access to higher levels in a sequential way (Choi, 2009). Typically, expansions in the compulsory education age are usually accompanied by policies –such as building new facilities, hiring new teachers- intending to facilitate the compliance of the law and to scaffold the achievement of its goals. However, in spite of the widespread use of compulsory education acts, evidence on their effects on educational attainment and outcomes are relatively scarce for developing countries.

We contribute to the literature by evaluating the medium to long-term impacts of the extension of compulsory education from six to nine years on a set of educational outcomes. We explore the average and heterogeneous effects by gender, parental education, area (urban/rural) and place of residence of a law passed in Indonesia in 1994. Furthermore, we also assess crossed heterogenous analysis over area and land of living and across parental educational background and gender. It is noteworthy that most of the existing literature focuses on short term average effects and omits the assessment of heterogeneous effects, differences by gender being the main exception.

We follow a sharp regression discontinuity strategy using data from three Indonesia Family Life Survey (IFLS)[1] waves, 2000, 2007, and 2014 – six, thirteen, and twenty years,

[1] IFLS contains detailed information on education, individual characteristics, and place of residence during the school age.

respectively after the reform-, in order to assess the average and heterogeneous effects of this compulsory education act on a set of educational outcomes.[2] Our findings suggest that the reform successfully increased junior secondary completion, enrollment on senior secondary schooling, completion on 12 years of schooling, and years of education, but not university attendance in general. The findings further demonstrate that the reform has a more pronounced impact on girls compared to boys. Additionally, individuals from backgrounds with lower paternal education are more influenced by the reform, as are those with a high level of maternal education. However, our findings showed a discrepancy in the impact of the policy based on geographical factors. Individuals residing in urban areas, along with those who lived outside of Java Island when they were 13, experienced a more pronounced effect of the reform. Upon closer examination, we discovered that in both Java and Non-Java Islands, the impact of the reform was greater for urban residents as compared to rural residents. Furthermore, it was found that male children from families with low parental education experienced a stronger impact from the reform. On the other hand, female children of highly educated parents were more affected by the reform. Lastly, we observed that the effect of the reform appears to decrease over time. This implies a "catch-up effect" is occurring within education: as time goes on, the older generations (those who did not experience the reform) are progressively attaining similar educational standards as those generations who were affected by the reform. Falsification exercises and sensitivity checks provide evidence in favor of the causal interpretation of our main findings.

The remainder of the paper is organized as follows. Section 2 briefly reviews the causal evidence between compulsory education laws and educational outcomes. Section 3 overviews Indonesia's 1994 compulsory education law. Section 4 describes the data, descriptive statistics

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[2] The outcomes are junior secondary completion, senior secondary enrollment, senior secondary completion, university attendance, and years of education.

and empirical models used in this study, and Section 5 presents the main findings. Section 6 concludes.

2. Compulsory Schooling Laws and Educational Outcomes

The evidence on the impact of mandatory education laws on educational outcomes in developed countries usually finds that compulsory education laws lead to significant gains in the accumulation of human capital. For example, Angrist and Krueger (1991) and Acemoglu and Angrist (1999), using the 1960 - 1980 US censuses and taking advantage of variation in the timing of law changes across states over time, showed that compulsory schooling laws were effective in compelling students to stay in school until they reach the legal dropout age. Using a one-percent sample of the 1960 Census too, Lleras-Muney (2002) found that the Compulsory Attendance Law -which specified the age at which a child had to enter school- and the Child Labor Laws increased educational attainment. Similarly, Goldin and Katz (2011) found that the state compulsory schooling and child labor laws from increased secondary school enrollment rate and years of completed schooling. In the United Kingdom (UK), Harmon and Walker (1995) exploited the experimental nature of two changes in the minimum school-leaving age -the 1947 and 1973 shifts, which expanded compulsory education from 14 to 15 years and from 15 to 16, respectively-which increased years of schooling. Oreopoulos (2006a, 2006b) conducted cross-country analyses and concluded that the minimum school leaving age reform increased educational attainment in the UK, the US, and Canada. More recent research conducted in the US comes to similar conclusions, suggesting that laws prohibiting child work and mandatory schooling attendance increase the number of years spent in school, the enrolment rate, and the likelihood of being enrolled (Stephens and Yang, 2014; Lleras-Muney and Shertzer, 2015; Clay et al., 2021; Shanan, 2021).

In contrast to the extensive literature on developed countries, the evidence for emerging countries is relatively scarcer and its results less clear. Pischke and Von Wachter (2008) utilized the changes in Germany's obligatory schooling law following World War II and discovered that the law only boosted the average number of years of education by a small amount. In Turkey, the 1997 school compulsory reform increased the likelihood of completing eight-grade and high school and effectively enhances years of education (Kirdar et al., 2016; Dayioglu and Kirdar, 2022). In 2012, the law was once again revised and compulsory education expanded from eight to twelve years. Utilizing the latter, Erten and Keskin (2019) found that the reform increased high school attendance. In Egypt, Elsayed (2019) used a natural experiment to analyze the additional one-year extension to primary education in 1999 and found that the reform had a large positive effect on educational attainment.[3] Momo et al. (2021) found that extension of compulsory education to lower secondary education significantly increased compulsory education completion and improved the average number of post-compulsory graduates in Senegal.

Moving to Asia, Spohr (2003) and Tsai et al. (2009) discovered that the Taiwan's 1968 compulsory education expansion from six to nine years increased the average years of schooling. Korwatanasakul (2019) found that the mandatory reforms of 1978 in Thailand resulted in a high average number of years of schooling (nearly twice the additional schooling required by the law). Similarly, Fang et al. (2012a) analyzed the 1968 nine years of compulsory education reform in China and found that this policy increased years of completed education by almost one year. However, in 2006 to 2007, the Chinese government initiated the free compulsory education reform on a nationwide basis for enforcing the nine years compulsory

[3] Assaad et al. (2016) also assess the impact of compulsory schooling reform in Egypt which was implemented in 1988. However, the policy was reducing primary school from 6 to 5 years instead of extending the duration. The result indicate that the reform led to a substantial decline in completed years of schooling.

schooling and this initiative had no significant impact on school enrollment (Tang et al., 2020). As it may be seen, the relationship between compulsory education laws and its impact on educational outcomes is far from being straightforward.

The analysis of heterogeneous effects of compulsory education laws have been assessed and obtained mixed result. In terms of gender, some studies have concluded that the policy change has a stronger effect on females than on males (Lleras-Muney, 2002; Tsai et al., 2009; Fang et al., 2012; Dayioglu and Kirdar, 2022) but also vice versa (Spohr, 2003; Goldin and Katz, 2011; Elsayed, 2019). In terms of area of residence, the law has a larger impact on individuals who lived in rural in China, Egypt and Turkey (Fang et al., 2012; Elsayed, 2019; Dayioglu and Kirdar, 2022, respectively). Finally, in terms of parental education, the 1997 mandatory schooling reform in Turkey affected individuals whose parents had completed over five years of education more than those whose parents had less. However, the effect became insignificant for those whose parents had completed education at least ten years of schooling (Dayioglu and Kirdar, 2022).

In Indonesia, investigation related to compulsory education laws is very scarce. A similar education policy that was assessed was a natural experiment which consisted in the extension of the academic year that Indonesian children experimented due to the change of the start of the academic year from January to July in 1978 (Parinduri, 2014, 2017). According to our knowledge, the only study that has evaluated the causal impact of compulsory schooling policies in Indonesia is Lewis and Nguyen (2020). Even though previous studies suggest that implementing mandatory educational law may effectively increase educational outcomes both in developed and emerging nations, Lewis and Nguyen (2020) found that the 1994 nine-year compulsory schooling initiative had no discernible impact on child educational attainment. Lewis and Nguyen (2020) argue that the failure of the mandatory schooling policy in Indonesia was due to the apparent shortcomings of government implementation of the program and acknowledge potential weaknesses[4] in their analysis, particularly as regards lack of data on possible socioeconomic determinants of school participation. In this paper using the same database, we are able to add these missing socioeconomic characteristics and overcome other shortcomings[5]. We are additionally able to estimate the medium and long-term impact of the law and provide a number of heterogeneous effects.

3. The 1994 National Compulsory Education Law

The schooling system in Indonesia has a 6+3+3+4 structure, with students spending 6 years in elementary school; 3 in junior high; 3 in senior high; and 4 years in college, and has remained unaltered since the enactment of the first curriculum in 1946 (Al-samarrai and Lewis, 2021). Up to 85% of the Indonesian primary and secondary school students attend public or private institutions, while the remaining 15% are enrolled in Islamic institutions.

After years of a gradual expansion at the primary education level[6], the government of Indonesia turned to boosting secondary graduates during the late 1980s. By that time, the six-year compulsory schooling policy (ages 7 to 12)[7], in force since 1988, had succeeded in making primary school almost universal, with completion rates exceeding 95% (World Bank, 2022a; Suryadarma et al., 2006)). However, by 1990, junior secondary school graduation rates were still below 50% (World Bank, 2022a). Since then, the government, private institutions and international donor agencies responded by implementing several programs, including training

[4] Lewis and Nguyen (2020) did not have access to data on socioeconomic characteristics (i.e., parents' education, household income, household size, the number and gender of child's siblings, child's birth order or school costs) that may influence the positive effects of government's compulsory schooling.

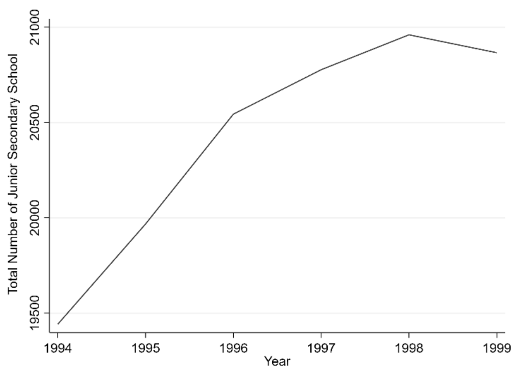
[5] Lewis and Nguyen (2020) assume that all children start school at the age of seven, with the cut-off date for determining school age being August 31st. However, in Indonesia, children may start school at the age of six, which creates uncertainty in identifying whether they should be classified as part of the treatment or control group. Therefore, in our identification strategy, we exclude individuals who are exactly at the cut-off point to improve precision of the estimated treatment effect and to ensure the sharp RDD is not violated.

[6] One policy that was phenomenal in making primary education universal was the *Sekolah Dasar* INPRES program. From 1973 to 1978, the Indonesian government undertook one of the most extensive school construction initiatives under this program (Duflo, 2001).

[7] Although, by law the compulsory age for children to start elementary school is at age seven, some begin a year earlier or a year later (Barakat and Bengtsson, 2018)

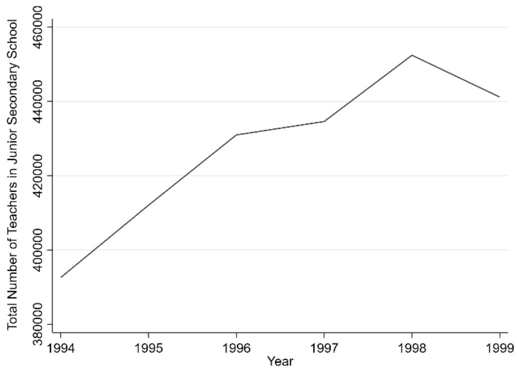
for teachers, providing textbooks for students and teachers, supplying and distributing science equipment to schools, and building more buildings for secondary schools (Yeom et al., 2002). Those early 1990s programs[8], intended to support the imminent move to a nine-years compulsory education system. As reported by Indonesia Statistics (Badan Pusat Statistik, BPS), the number of junior secondary schools and teachers in junior secondary schools increased by around 7% (Figure 1) and 12% (Figure 2), respectively, within five years of the 1994 reform. These trends underline that the support of educational infrastructure development accompanied the extension of compulsory schooling.

Figure 1: Number of Junior Secondary School



Source data: Indonesian Statistics (Badan Pusat Statistik).

Figure 2: Number of Teaches in Junior Secondary School



Source data: Indonesian Statistics (Badan Pusat Statistik).

Indeed, in April 1994, the President of Indonesia issued Presidential Instruction Number 1, which extended compulsory schooling from 6 to 9 years. The reform was effectively implemented at the beginning of the 1994/95 academic year, that is, in September 1994. Students who had not graduated from junior secondary school by the end of the 1993/1994 academic year were bound to complete nine years of education. This extension aimed to reduce

[8] Based on Law Number 2 of 1989 concerning the National Education System, article 14, paragraph 2, states that citizens aged seven must attend primary school or equivalent education until graduation. However, the implementation of nine-years compulsory education was only effective nationally in the academic year of 1994/1995 after the issue of Presidential Instruction Number 1 of 1994.

child labor and keep kids in school, especially those who could not afford to pursue higher education (Yeom et al., 2002).

4. Data and empirical methodology

4.1. Data

We exploit data from Indonesia Family Life Survey (IFLS)[9], covering more than 80% of the Indonesian population within the survey area (Strauss et al., 2016). The IFLS is a longitudinal micro-level survey conducted in 1993, 1997, 2000, 2007, and 2014. The survey provides information about individuals' characteristics, educational attainment, and the locations (province and district) of the respondents' birthplace, current residence and entire migration history. As our main aim is to analyze the impact of the 1994's nine years compulsory schooling reform on the completion of nine years of compulsory schooling, enrollment in senior secondary schooling, completion of 12 years of schooling, university attendance, and years of education, we exploit on the third, fourth, and fifth wave of the IFLS survey (2000, 2007, and 2014).

For the outcome of completion rate of nine years compulsory schooling, we create a dummy variable that captures whether an individual has completed at least junior secondary school, which is the most relevant goal for the 1994 reform. For measuring enrollment in senior secondary schooling, completion of 12 years of schooling, and university attendance, we also create indicator variables according to whether an individual was enrolled for at least one year in senior secondary schooling, completed at least senior secondary schooling or been enrolled for at least one year at the university level, respectively. As for years of education, we exploit

[9] IFLS data can be obtained from https://www.rand.org/well-being/social-and-behavioral-policy/data/FLS/IFLS.html.

information about the highest level of schooling attended and the highest grade ever completed by the respondents. Using both these data, we can calculate the years of completed education.[10]

We distinguish between individuals who were subjected to the compulsory schooling policies (treated) and those who were not (control) by using month and year of birth. In accordance with the directives of the President, the educational reforms were initiated during the academic year 1994/1995, compelling students who had not attained completion of their junior high school to pursue their studies until attaining graduation. Adopting the identification strategy proposed by Lewis and Nguyen (2020), individuals belonging to the young cohort, born in September 1978 and onwards, were mandated to fulfill a compulsory nine-year educational period, whereas individuals from the old cohort, born prior to September 1978, had the option to discontinue their education prior to the completion of junior secondary schooling.

Table 1[11] illustrates summary statistics for individuals aged between 14 and 17 in 1994 because the estimated optimal bandwidths in our local regression analyses fall into this range. The table provides sample means and standard deviations of the variable used in the empirical analysis (outcome and controls). The table is divided into two groups: those how were exposed (treatment) and those who were not exposed (control) to the reform. The table indicates that individuals subjected to the reform have a higher (unconditional) probability of completing junior secondary education, enrollment in senior secondary education, completion of senior secondary schooling, and years of education. Regarding control variables, we only use a set of characteristics which are relevant factors in the education production function (Hanushek, 2020): gender, fathers' and mothers' education, number of siblings, religions, and place of

[10] For instance, if an individual’s highest level of schooling is junior high school and his/her highest grade ever completed is 2, then his/her years of completed education is equal to 8 years. This calculation already considers the grade repetition.

[11] Please note that some of the young cohorts that belong in the treatment group were born in 1983 and 1984. In 2000 they were still 16- and 17-years old, implying that they were still too young to complete 12 years of schooling or attending university. Therefore, we exclude them. In addition, we also incorporate work experiences, which we do not treat as predetermined individual characteristics, but rather employ it in section 5.1 to provide explanation for the declining effect of the reform across subsequent waves.

residence when the respondents aged 13 (urban vs. rural and Java vs. Non-Java Island). The treated and non-treated samples are balanced at both sides of the threshold except for the relevant outcome variables (as expected), but also gender, religiosity, and island of residence when they were 13. However, the significance differences in those predetermined characteristics are the overall differences between treated and control groups. These differences are no longer there when we assess these variables along with other predetermined characteristics around the cutoff point (refer to section 4.3) which make us feel confident about the comparability of both subgroups as suggested in De La Cuesta and Imai (2016).

Table 1: Summary statistics of 14-17 years old individual in 1994 - Wave 2000, 2007, and 2014

	Control			Treatment			Diff		
	2000	2007	2014	2000	2007	2014	2000	2007	2014
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Junior Secondary Completion	0.568	0.652	0.598	0.637	0.692	0.675	0.069***	0.040**	0.077***
	(0.496)	(0.476)	(0.490)	(0.481)	(0.462)	(0.468)	(0.020)	(0.019)	(0.015)
Years of Education	8.712	9.199	8.598	8.950	9.451	9.054	0.238*	0.252	0.456***
	(3.587)	(4.025)	(4.315)	(3.520)	(3.831)	(4.066)	(0.144)	(0.162)	(0.130)
Senior Secondary Enrollment	0.286	0.339	0.293	0.348	0.379	0.331	0.061***	0.040**	0.038***
	(0.452)	(0.473)	(0.455)	(0.476)	(0.485)	(0.471)	(0.019)	(0.020)	(0.014)
Senior Secondary Completion		0.329	0.282		0.365	0.306		0.036*	0.024*
		(0.470)	(0.450)		(0.482)	(0.461)		(0.020)	(0.014)
University Attendance		0.170	0.148		0.160	0.152		-0.010	0.004
		(0.376)	(0.355)		(0.367)	(0.359)		(0.015)	(0.011)
Male	0.483	0.537	0.506	0.449	0.512	0.491	-0.034*	-0.025	-0.016
	(0.500)	(0.499)	(0.500)	(0.498)	(0.500)	(0.500)	(0.020)	(0.021)	(0.015)
Fathers' Educ >= 9 Years	0.272	0.343	0.329	0.296	0.341	0.321	0.023	-0.002	-0.008
	(0.445)	(0.475)	(0.470)	(0.456)	(0.474)	(0.467)	(0.018)	(0.020)	(0.014)
Mothers' Educ >= 9 Years	0.210	0.299	0.294	0.228	0.308	0.288	0.018	0.009	-0.006
	(0.408)	(0.458)	(0.456)	(0.420)	(0.462)	(0.453)	(0.017)	(0.019)	(0.014)
Number of Siblings	7.628	8.352	11.352	7.601	8.284	11.419	-0.027	-0.068	0.066
	(4.654)	(5.072)	(5.990)	(4.849)	(5.449)	(6.096)	(0.193)	(0.217)	(0.187)
Islam	0.892	0.899	0.884	0.869	0.876	0.883	-0.024*	-0.023*	-0.001
	(0.310)	(0.301)	(0.320)	(0.338)	(0.330)	(0.321)	(0.013)	(0.013)	(0.010)
Christian	0.061	0.059	0.068	0.083	0.082	0.074	0.023**	0.023**	0.007
	(0.239)	(0.235)	(0.251)	(0.277)	(0.275)	(0.262)	(0.010)	(0.011)	(0.008)
Other Religion	0.047	0.042	0.048	0.048	0.042	0.042	0.001	0.000	-0.006
	(0.211)	(0.200)	(0.214)	(0.213)	(0.200)	(0.202)	(0.009)	(0.008)	(0.006)
Individual Live in Urban at age 13	0.439	0.479	0.562	0.454	0.484	0.555	0.015	0.005	-0.008
	(0.496)	(0.500)	(0.496)	(0.498)	(0.500)	(0.497)	(0.020)	(0.021)	(0.015)
Individual Live in Java Island at age 13	0.589	0.558	0.534	0.537	0.517	0.509	-0.052***	-0.042**	-0.025
	(0.492)	(0.497)	(0.499)	(0.499)	(0.500)	(0.500)	(0.020)	(0.021)	(0.015)
Work Experience	4.694	5.346	5.176	4.106	4.852	4.535	-0.588**	-0.494	-0.642***
	(5.970)	(6.259)	(6.457)	(4.715)	(7.104)	(5.882)	(0.276)	(0.339)	(0.236)
Observations	6227	5584	5964	5631	6272	5663			

Note: *** significant at 1% , ** significant at 5% , * significant 10%.
Source: Indonesian Family Life Survey Wave 2000, 2007, and 2014

4.2. Identification strategy

The compulsory education reform implemented in 1994 stipulates that individuals born in September 1978 and subsequent months were mandated to complete a minimum of nine years of formal education. In contrast, those born prior to this cut-off date had the provision to discontinue their education earlier (before completing 9 years of education). We exploit the individual's month and year of birth to create the cut-off point and running variable in a sharp regression discontinuity (RD) design for estimating the impact of the reform on a set of educational outcomes. Individuals born on and after September 1978 were mainly in grade eight at the end of the 1993/1994 academic year and exposed to the reform. Thus, in our RD design those born after September 1978 represent the treatment group. However, due to the variability in the starting age of children for schooling, whether at the age of six or seven, a potential ambiguity arises regarding the assignment of pupils to the treatment or control group. Therefore, to mitigate this confusion, we exclude individuals precisely at the cut-off point.

Following Imbens and Lemieux (2008) and Cattaneo et al. (2020) we use local linear regressions in our RD estimations and implement the optimal bandwidth selection using the Imbens and Kalyanaraman (2012) procedure to minimize bias and maximize precision (Lee and Lemieux, 2010). Nevertheless, we also perform sensitivity analyses using 0.5, 1.5, and twice the optimal bandwidth selection. Our main RD estimate is as follow:

$$Y_i = \alpha + \beta treat_i + f(x_i) + \varepsilon_i$$

$$\forall x_i \in (c - h, c + h)$$

where Y_i is the dependent variable, $treat_i$ is the treatment status, x_i is the forcing variable, h is the bandwidth around the cut-off point c, and ε_i is the error term. Following Lee and Card (2008), we cluster standard errors at the month-year of birth level to accommodate for specification errors in the forcing variable. We conduct our analysis by both excluding and including individual covariates as control variables. These controls include a set of dummy variables indicating gender, whether the individual's father or mother has completed nine years of schooling, number of siblings, the individual's birth order, religion, and the individual's place of residence at age 13 (either urban or rural, and either on the island of Java or another island). Before conducting our main analysis, we first perform preliminary checks. The first check is performing density test of the running variable to check for manipulation and ensure the validity of our analysis (McCrary, 2008). If the density of the running variable changes sharply at the cutoff point, then it would invalidate our RDD approach. The second test is performing estimation on the predetermined characteristics of the individuals to verify that there is a balance in the characteristics on both sides around the cutoff point. We do this by setting these characteristics as outcome variables and assessing them against the running variable (Cattaneo et al., 2020). We use data from the waves 2000, 2007, and 2014 for both our main estimates and preliminary tests, allowing us to evaluate the short-, medium-, and long-term impacts of the reform.

We also conduct a falsification exercise, in which we treat a group of older people who are part of the control group in our baseline model (1971–1975) as if they were treated (fake treatment) and much older individuals, who were born between 1964 and 1970, as if they were in the control group (fake control). Then, we set the fake policy reform as if it happened in 1987 by estimating a placebo regression instead of the actual one in 1994.

4.3. Preliminary checks

We begin by presenting validity checks for the RD design by conducting manipulation test of our running variable as suggested by McCrary (2008) and examining whether the predetermined individual's characteristics in our analysis are continuous at the discontinuity (Imbens and Lemieux, 2008). Following the procedure described in Cattaneo et al. (2018), we do not find substantial evidence of a discernible discontinuity in the density of our running variable. This suggests that the underlying distribution of the data remains consistent across the spectrum of the running variable, thereby upholding the validity of the Regression Discontinuity Design (RDD) approach in our study (Figure 3 and Table 2).

Figure 3: Local density plots of month-and-year of birth around the cutoff point

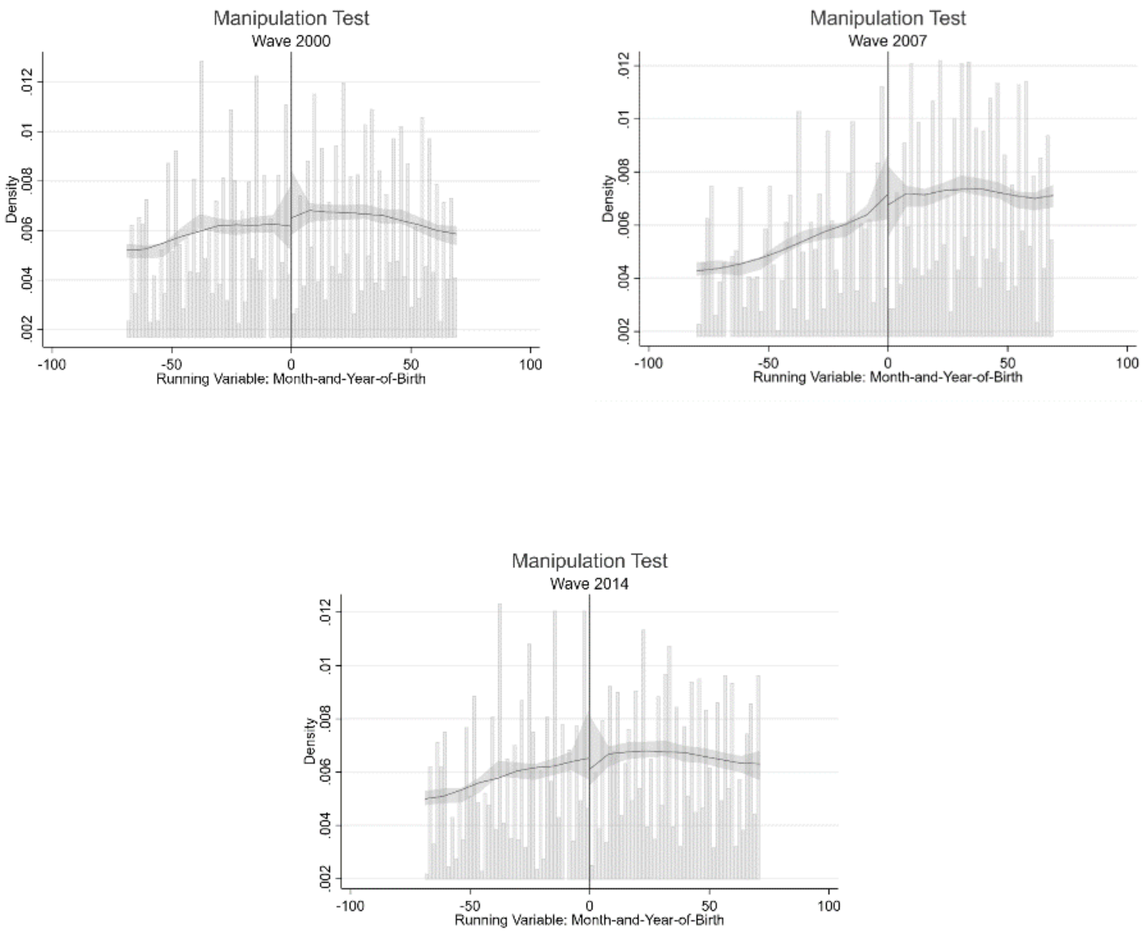


Table 2: Manipulation Test of Running Variable
Wave 2000, 2007, and 2014

Method:	2000	2007	2014
T	0.630	-0614	-0.262
P>|T|	0.5285	0.539	0.793
Bandwidth Left	23.000	26.635	23.000
Bandwidth Right	23.000	23.000	23.725
Observations	11858	11856	11627

Note: This table presents the findings from the manipulation test of the running variable following the procedure of Cattaneo et al. (2018). The running variable is individual's month and year of birth.

Furthermore, Table 3 shows estimations of the local averages of the individual's predetermined characteristics against the running variable, allowing for a discontinuity at the threshold. The predetermined characteristics that we plot are a set of dummy variables indicating gender, whether individuals' fathers and mothers have completed at least nine years of schooling, number of siblings, religion, and individual's place of residence when they aged 13 (living in urban or rural and Java or non-Java Island). The results indicate that there are no significant jumps at the cutoff point for any predetermined individual characteristic, which implies that the changes we see in the main estimation are likely due to the impact of the reform rather than individuals' characteristics.[12]

Table 3: RDD estimate for pre-determined characteristics
Wave 2000, 2007, and 2014

	2000	2007	2014
	(1)	(2)	(3)
Male	-0.018	-0.005	-0.039
	(0.041)	(0.053)	(0.043)
Father's Education	0.029	0.004	0.040
	(0.031)	(0.027)	(0.032)
Mother's Education	0.020	-0.023	-0.024
	(0.032)	(0.039)	(0.031)
No. Siblings	-0.011	-0.010	0.477
	(0.440)	(0.26)	(0.496)
Religion	0.700	0.146	0.308
	(0.577)	(0.400)	(0.566)
Individual Live in Urban at age 13	0.075	-0.0004	0.006
	(0.049)	(0.038)	(0.037)
Individual Live in Java Island at age 13	0.019	0.016	-0.018
	(0.033)	(0.057)	(0.033)
Observations	11858	11856	11627

Note: This table presents the results of preliminary checks using a Regression Discontinuity Design (RDD) to assess the balance of pre-determined characteristics across the treatment threshold. Each row in the table represents an estimated treatment effect on a pre-determined characteristic, with separate models run for each characteristic. Standard errors, in parentheses, are clustered at the month-year-of-birth. *** significant at 1% , ** significant at 5% , * significant 10%.

[12] The graphical analysis of the local averages of the individual's predetermined characteristics against the running variable are depicted in Figure A1. It indicates that there are no significant jumps around the cutoff point for predetermined characteristics in each wave.

5. Results

5.1. The average impact of the 1994 compulsory schooling reform

Table 4: RD Treatment Effect on Educational Outcomes Waves 2000, 2007, and 2014 (Optimal Bandwidth)

	Panel A: Completion of nine years of compulsory schooling		Panel B: Enrollment in senior secondary schooling		Panel C: Completion of 12 years of schooling		Panel D: University attendance		Panel E: Years of education		N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Wave 2000	0.193***	0.200***	0.148***	0.139***					1.794***	1.928***	11858
	(0.065)	(0.044)	(0.054)	(0.035)					(0.587)	(0.367)	
Wave 2007	0.123***	0.132***	0.098**	0.123***	0.111**	0.141***	0.012	0.022	1.401***	1.753***	11856
	(0.043)	(0.028)	(0.047)	(0.040)	(0.051)	(0.040)	(0.031)	(0.030)	(0.382)	(0.264)	
Wave 2014	0.086***	0.070***	0.058	0.052	0.069	0.065	0.026	0.018	0.802**	0.650**	11627
	(0.028)	(0.026)	(0.044)	(0.039)	(0.049)	(0.044)	(0.030)	(0.026)	(0.342)	(0.300)	
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	

Notes: Data is from Indonesian Family Life Survey Wave 2000, 2007 and 2014. Each column (1 - 10) reports the RD treatment effect of being born on and after September 1978 with a linear control function in month-year-of-birth on each side of the discontinuity and exploits the optimal bandwidth selection based on Imbens and Klayanaraman (2009) algorithm. The dependent variable in: Panel A is a dummy variable equal to one if the individual completed junior secondary school; Panel B is a dummy variable equal to one if the individual enrolled to senior secondary school; Panel C is a dummy variable equal to one if the individual completed senior secondary school; Panel D is a dummy variable equal to one if the individual enrolled to university; and Panel E is the years of education attained. Control variables are gender, religion, father's and mother's education, number of siblings, and place of residence based on Java or non-Java islands and urban or rural areas. Standard errors, in parentheses, are clustered at the month-year-of-birth. *** significant at 1% , ** significant at 5% , * significant 10%.

We initiate our analytical work by quantifying the estimated average impact of the policy shift on the educational outcomes of individuals in the years 2000, 2007, and 2014 (refer to Table 4). For each respective outcome, we introduce two specifications, without and with control variables, defined in accordance with the optimal bandwidth selection described in section 4. Table 4 report the results from our Regression Discontinuity (RD) design, showing the effects of the intervention on various educational achievements: fulfilling nine years of obligatory schooling (Panel A); enrollment in senior secondary education (Panel B); completion of a 12-year schooling period (Panel C); matriculation into university (Panel D); and cumulative years of education (Panel E).[13] It is noteworthy that we abstain from employing the 2000 wave data for the outcomes concerning the completion of 12 years of schooling and

[13] Graphical analyses are reported in Figure A2.

university admission, due to the consideration that certain younger clusters within the treatment group were insufficiently matured at the survey period.[14]

Our findings suggest that the obligatory schooling reform had a fruitful impact in fostering an increase in the accomplishment of nine years of education (19.3 percentage points), the enrolment in senior secondary education (14.8 percentage points), the completion of a 12-year educational cycle (11.1 percentage points) and completed years of education (1.79 years). However, the influence on the probability of university enrollment is statistically insignificant. The results displayed in table 4 are in line with those documented in different countries. For instance, Momo et al. (2021) observed that the impact of the reform exhibits greater magnitude in Senegal, with an increased likelihood of completing grade 10 by seven percentage points. This effect size is comparatively smaller when compared to the outcomes observed in Thailand, where a similar reform initiative resulted in an approximate extension of four additional years of schooling, nearly twice the duration mandated by the corresponding legislation (Korwatanasakul, 2019). Furthermore, the findings align closely with those observed in other developing nations, where the range of outcomes varied from 0.8 to 2.14 years (Tsai et al., 2009; Fang et al., 2012; Elsayed, 2019).

We also detect that the effect of the reform decreases across subsequent waves. This is shown by the decreasing size of the positive and statistically significant impact on various educational outcomes. This implies a "catch-up effect" is occurring within education: over time, older generations (those who did not experience the reform) are progressively attaining similar educational standards as those generations who were influenced by the reform. To support this argument, we introduce work experience as an alternative outcome. That is, we analyze whether individuals affected by the reform spent more time working than their

[14] Some of the young cohorts in our sample were born in 1983 and 1984. Therefore, in 2000 they were still 16- and 17-years old implying that they were still too young to complete 12 years of schooling or attending university.

counterparts in the control group. The purpose of this exercise is to shed light on the possible channel behind the diminishing effect of the reform, where initially, the older and younger groups have a significant difference in work experience, but the difference becomes insignificant across time. As we can see in Table A2, the significant negative difference in work experiences between the older and younger cohorts (around 0.47 years) observed in 2000 is no longer present by 2007. This indicates that the older group has caught up in terms of work experience, which indirectly reflects their progress in educational achievements between 2000 and 2007.

Table 5: RD Treatment Effect on Educational Outcomes using same individuals across waves (Waves 2000, 2007, and 2014)

	Panel A: Completion of nine years of compulsory		Panel B: Enrollment of senior secondary schooling		Panel C: Completion of 12 years of schooling		Panel D: University attendance		Panel E: Years of education		N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Wave 2000	0.136***	0.117***	0.103***	0.079***					1.058***	0.933***	9410
	(0.035)	(0.025)	(0.036)	(0.029)					(0.335)	(0.233)	
Wave 2007	0.091***	0.066***	0.084**	0.057*	0.104**	0.078**	0.004	-0.012	0.539**	0.322*	9410
	(0.029)	(0.023)	(0.042)	(0.033)	(0.045)	(0.036)	(0.030)	(0.019)	(0.261)	(0.171)	
Wave 2014	0.063*	0.040	0.052	0.031	0.075*	0.052	0.022	0.008	0.514*	0.311	9410
	(0.037)	(0.033)	(0.044)	(0.037)	(0.043)	(0.035)	(0.023)	(0.018)	(0.281)	(0.229)	
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	

Notes: Data is from Indonesian Family Life Survey Wave 2000, 2007 and 2014. Each column (1 - 10) reports the RD treatment effect of being born on and after September 1978 with a linear control function in month-year-of-birth on each side of the discontinuity and exploits the optimal bandwidth selection based on Imbens and Klayanaraman (2009) algorithm. The dependent variable in: Panel A is a dummy variable equal to one if the individual completed junior secondary school; Panel B is a dummy variable equal to one if the individual enrolled to senior secondary school; Panel C is a dummy variable equal to one if the individual completed senior secondary school; Panel D is a dummy variable equal to one if the individual enrolled to university; and Panel E is the years of education attained. Control variables are gender, religion, father's and mother's education, number of siblings, and place of residence based on Java or non-Java islands and urban or rural areas. Standard errors, in parentheses, are clustered at the month-year-of-birth. *** significant at 1% , ** significant at 5% , * significant 10%.

Next exercises, we performed a set of robustness checks to ensure the reliability of our findings in the baseline estimation. Specifically, we examine the sensitivity of our results to three factors: the composition of the selected sample, the bandwidth, and a falsification analysis using a fake placebo reform. To begin, we replicated the previous analysis by focusing solely on individuals who in waves 2000, 2007, and 2014 were participated and ruled out those who were not present in one or two of these three waves. The aim of this exercise is to check whether the changes in the composition of the IFLS sample may be leading to the results. For this exercise, we retained 9,410 individuals from our original baseline sample, which initially consisted of approximately 11,800 individuals. Our findings, as shown in Table 5, indicate that the observed effect is slightly lower compared to the baseline results. However, it is important to note that the impact of compulsory schooling reform remains positive and statistically significant. Furthermore, the observed decrease in coefficients across waves, which suggests the existence of an inter-cohort catch-up effect, persists even when only considering the same individuals who participated in all waves.

The second test involves the utilization of varying bandwidth values. As delineated in section 4.2, we implemented bandwidths of 0.5, 1.5, and twice the optimal bandwidth selection. Table A1 presents these results, demonstrating that the estimated effects remain stable under different bandwidths.

Finally, we performed a falsification exercise to further strengthen the validity of our main findings. In this exercise, we considered a fictitious compulsory schooling reform and replicated our baseline analysis using never-treated individuals. We take a cohort of older individuals, born between 1964 and 1970 who were excluded from our main analysis, as a fake control cohort, and we use our original control cohort of individuals born between 1971 and 1975 as a fake treated cohort. We then estimate a placebo regression by assuming that the compulsory reform took place in 1987 rather than the actual year of 1994. The RD estimates of these placebo estimations are presented in Table 6 (columns 1 to 6). Results are not statistically different from zero for all waves and all educational outcomes.[15] These null results of the placebo estimations reinforce the causal interpretation of our main findings which further bolsters the robustness of our conclusions and emphasizes the credibility of the estimated relationship between the compulsory schooling reform and the educational outcomes.

[15] The graphical illustrations are displayed in Figure A3.

Table 6: Falsification Test for Educational Outcomes Using Old and Very Old Cohorts)

	2000		2007		2014	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Completion on Nine Years of Compulsory Schooling						
RD Estimate	0.000	0.008	-0.015	0.010	0.015	-0.016
	(0.095)	(0.077)	(0.068)	(0.060)	(0.091)	(0.074)
Panel B: Enrollment on Senior Secondary Schooling						
RD Estimate	-0.019	-0.007	-0.009	0.007	0.027	0.014
	(0.051)	(0.043)	(0.073)	(0.061)	(0.050)	(0.044)
Panel C: Completion on 12 Years of Schooling						
RD Estimate	-0.019	-0.009	0.014	0.032	0.037	0.022
	(0.049)	(0.042)	(0.076)	(0.064)	(0.043)	(0.037)
Panel D: University Attendance						
RD Estimate	-0.004	0.003	0.042	0.044	0.036	0.024
	(0.028)	(0.026)	(0.041)	(0.031)	(0.029)	(0.027)
Panel E: Years of Education						
RD Estimate	-0.285	-0.188	-0.018	0.285	0.217	-0.048
	(0.758)	(0.637)	(0.828)	(0.748)	(0.738)	(0.608)
Observations	7974	7974	6500	6500	7316	7316
Controls	No	Yes	No	Yes	No	Yes

Notes: Data is from Indonesian Family Life Survey Wave 2000, 2007, and 2014. Each column reports falsification analysis of the RD treatment effect for each wave stored in column (1) to (6). For each wave, we consider a cohort of older individuals, initially excluded from our estimation sample born between 1964 and 1970 as a fake control cohort and we use our original control cohort of individuals born between 1971 and 1975 as a fake treated cohort. All results are estimated using the optimal bandwidth selection computed using Imbens and Klayanaraman (2009) algorithm. Control variables are gender, religion, father's and mother's education, number of siblings, and place of residence based on Java or non-Java islands and urban or rural areas. Standard errors, in parentheses, are clustered at the month-year-of-birth. *** significant at 1% , ** significant at 5% , * significant 10%.

5.2. The heterogeneous effects of the 1994 compulsory education law

We then move to the analysis of heterogenous effects by different individual characteristics and the place of residence. Panel A in Table 7 presents the results regarding the impact on the probability of completing nine years of compulsory schooling, senior secondary enrollment, and years of education using data from the 2000 wave (The complete results along with 2007 and 2014 waves are stored in appendix Table A3). In columns (1) and (2), the effect is slightly greater for female than male in 2000, but the difference becomes wider in 2007 (columns (1) to (4) in Panel A of Table A3) and the effect is insignificant for female in 2014 (columns (1) to (4) in Panel A of Table A3). Furthermore, in columns (3) and (4) of Table 7, the impact is higher for individuals whose father has less than nine years of education with consistent results across waves (columns (5) to (8) in Panel A of Table A3). In columns (5) and (6) of Table 7, the effect is slightly higher for individuals whose mother has at least completed junior secondary schooling in 2000, but in 2007 and 2014, the impact is higher and statistically significant for individuals whose mother has less than nine years of education (columns (9) to

(12) in Panel A of Table A3). However, the impact is not significant for individuals with high educated mother in 2007 and 2014 (columns (9) to (12) in Panel A of Table A3).

Table 7: Heterogenous Analysis of RD Treatment (Individual Covariates) - Wave 2000

	Male	Female	Dad Educ ≥ 9	Dad Educ < 9	Mom Educ ≥ 9	Mom Educ < 9
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Completion on Nine Years of Compulsory Schooling						
RD Estimate	0.147***	0.196**	0.145**	0.192***	0.230**	0.153***
	(0.049)	(0.080)	(0.069)	(0.047)	(0.113)	(0.037)
Optimal Bandwidth ($\hat{h}$)	19	16	14	16	14	16
Observations	5813	6045	3404	8454	2698	9160
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Senior Secondary Enrollment						
RD Estimate	0.123***	0.124**	0.014	0.203***	0.370***	0.082***
	(0.039)	(0.056)	(0.062)	(0.042)	(0.137)	(0.031)
Optimal Bandwidth ($\hat{h}$)	21	21	18	16	16	22
Observations	5813	6045	3404	8454	2698	9160
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: Years of Education						
RD Estimate	1.267***	1.952***	0.866	2.115***	2.929***	1.286***
	(0.365)	(0.620)	(0.653)	(0.338)	(0.935)	(0.325)
Optimal Bandwidth ($\hat{h}$)	18	16	18	14	14	15
Observations	5813	6045	3404	8454	2698	9160
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Data is from Indonesian Family Life Survey Wave 2000. Each column reports RD treatment effect of being born on and after September 1978 with a linear control function in month-year-of-birth on each side of the discontinuity. Column (1) to (6) reports local RD regression with controls, using optimal bandwidth selection. Control variables are gender, religion, father's and mother's education, number of siblings, and place of residence based on Java or non-Java islands and urban or rural areas. Standard errors, in parentheses, are clustered at the month-year-of birth. *** significant at 1% , ** significant at 5% , * significant 10%

Moving on to Panel B in Table 7, we examine the influence on senior secondary enrollment. In columns (1) and (2), we observe that the effect is relatively similar for both boys and girls in 2000. However, the estimated effect remains positive and significant only for males in 2007 and 2014 (columns (1) to (4) in Panel B of Table A3). In columns (3) and (4) of table 7 and in column (5) to (8) of table A3, we find that the impact is greater for individuals whose fathers have less than nine years of education across all waves. Similar to the findings in Panel A of table 7, we also see a similar pattern in column (5) and (6), where the impact is higher for individuals whose mothers have completed nine years of education or more in 2000. However, the situation is reversed in 2007 and 2014 (column (9) to (12) in Panel B of Table A3), where the impact becomes higher for individuals with mothers who have less than nine years of education.

Columns (1) to (4) in Panel C of Table A3, we observe that the impact on completing 12 years of schooling has a significant effect on males in 2007. However, in 2014, this effect is insignificant for males but significant for females. The reform also has a significant impact on individuals whose fathers (columns (5) to (8) in Panel C of Table A3) and mothers (columns (9) to (12) in Panel C of Table A3) have less than nine years of education. Moreover, in Panel D of Table A3, the effect on university attendance only affects female and individuals with low educated mother.

Panel C in Table 7 exhibits a comparable pattern to Panel A in Table 7, wherein the effect is more pronounced for females, individuals whose fathers have less than nine years of schooling, and individuals whose mothers have completed at least nine years of education in the year 2000. When we examine the data from wave 2007 and 2014 (Panel E in Table A3), we observe consistent impacts on years of education across different waves. However, in 2007 and 2014, the reform only had a positive influence on individuals with mothers who have lower levels of education (columns (5) to (12) in Panel E of Table A3).

In a subsequent stage, we undertake a heterogenous analysis rooted in the geographical location of residency when individuals reached the age of thirteen, as outlined in Table 8 (The complete tables are presented in appendix Table A4). In Panel A, the effect on accomplishing the nine-year mandatory education period appears to be more pronounced for individuals who were residing in urban settings as compared to their counterparts in rural areas when they were thirteen years old, as seen in column (1) and (2). In contrast, the impact is larger for those who resided outside the boundary of Java Island, as noted in column (3) and (4). Similar results were obtained in Panel B and C of Table 8 and Panel E of Table A4, wherein the effect is relatively more substantial for individuals who resided in urban locations than those who dwelt in rural areas, and likewise, for those who lived outside Java Island. However, it is important to note a contrast between our results and the conclusions drawn by Elsayed (2019), who found the effect to be significantly stronger in rural areas as opposed to urban ones. This effect is consistent across waves (columns (1) to (8) of Table A4). Lastly, the education reform appears to affect university enrollment solely for individuals who resided in a rural area (columns (3) and (4) in Panel D of Table A4).

Table 8: Heterogenous Analysis of RD Treatment (Place of Residence at aged 13) - Wave 2000

	Urban	Rural	Java Island	Non-Java Island
	(1)	(2)	(3)	(4)
Panel A: Completion on Nine Years of Compulsory Schooling				
RD Estimate	0.281***	0.117**	0.150***	0.184***
	(0.047)	(0.056)	(0.041)	(0.071)
Optimal Bandwidth ($\hat{h}$)	15	17	16	20
Observations	5281	6577	6717	5141
Controls	Yes	Yes	Yes	Yes
Panel B: Senior Secondary Enrollment				
RD Estimate	0.160**	0.101**	0.110**	0.164***
	(0.065)	(0.043)	(0.043)	(0.055)
Optimal Bandwidth ($\hat{h}$)	20	22	20	22
Observations	5281	6577	6717	5141
Controls	Yes	Yes	Yes	Yes
Panel C: Years of Education				
RD Estimate	2.133***	1.178***	1.842***	1.799***
	(0.555)	(0.433)	(0.344)	(0.523)
Optimal Bandwidth ($\hat{h}$)	17	16	16	17
Observations	5281	6577	6717	5141
Controls	Yes	Yes	Yes	Yes

Notes: Data is from Indonesian Family Life Survey Wave 2000. Each column reports RD treatment effect of being born on and after September 1978 with a linear control function in month-year-of-birth on each side of the discontinuity. Column (1) to (4) reports local RD regression with controls, using optimal bandwidth selection. Control variables are gender, religion, father's and mother's education, number of siblings, and place of residence based on Java or non-Java islands and urban or rural areas. Standard errors, in parentheses, are clustered at the month-year-of birth. *** significant at 1% , ** significant at 5% , * significant 10%

We also carry out a robustness test of our heterogenous analysis, by applying a fixed bandwidth around the cutoff point. The fixed bandwidth corresponds to the optimal bandwidth selection for the entire sample for each educational outcome. The estimation results displayed in tables A5 and A6 are very similar to those in table A3 and A4, implying that the estimates are robust to alternative specifications with different bandwidths.

Several hypotheses could be suggested to rationalize these heterogeneous effects. One argument by Lewis and Nguyen (2020) is that the Indonesian government may not have had sufficient power to fully apply the compulsory schooling law. Importantly, Bellettini and Ceroni (2004), Justman and Peyton (2018) and Oreopoulos (2009) have argued that measures for compulsory school enrolment and attendance are only effective if they are credible and are accompanied by measures that increase children's and parental appreciation of the value of schooling. Murtin and Viarengo (2011) showed that the effectiveness of compulsory schooling laws in rising school attendance in the rural areas has historically depended on the power of the state to enforce compulsion. Additionally, Parker and Sudibyo (2022) indicated that in places like Papua, outside of Java, students often leave school early not solely due to financial issues or lack of law enforcement, but also due to their unique cultural and ethnic backgrounds. Mare and Maralani (2006) and Mazumder et al. (2021) have also stressed the salient role of mothers in the intergenerational transmission of education in Indonesia.

Thus, to better grasp the heterogenous effects shown in Tables 7 and 8, we perform further analyses by combining areas (urban and rural) with islands (Java and Non-Java) of residence at age 13. As presented in table 9, the impact of the compulsory schooling reform affects heavily on individuals who were living in urban than in rural areas both in Java and Non-Java Island. The subpar educational outcomes among children who were resided in rural areas can be attributed to their limited access to necessary educational resources (Suryadarma and Jones, 2014). In particular, students from rural backgrounds in Indonesia often face various challenges that hinder their education, including difficulties with school enrollment and limited access to high-quality schools (Turwelis et al., 2022). The poor infrastructure of school buildings and the subpar learning environment within the classrooms make it hard for effective learning to take place in these rural areas. In contrast, schools in urban areas have superior infrastructure and provide more conducive learning environments. This noticeable disparity causes a migration of highly qualified teachers from rural to urban areas, drawn by the availability of better facilities (World Bank, 2020). This further exacerbates the educational problems experienced by students in rural Indonesia which led to suboptimal of the impact on educational outcomes.

Table 9: Heterogenous Analysis - Java/Non-Java * Urban/Rural - Wave 2000

	Java*Urban	Java*Rural	Non-Java*Urban	Non-Java*Rural
	(1)	(2)	(3)	(4)
Panel A: Completion on Nine Years of Compulsory Schooling				
RD Estimate	0.215***	0.069	0.241***	0.135*
	(0.040)	(0.078)	(0.084)	(0.078)
Observations	3178	3539	2103	3038
Controls	Yes	Yes	Yes	Yes
Panel B: Senior Secondary Enrollment				
RD Estimate	0.208***	0.077	0.204**	0.149**
	(0.060)	(0.051)	(0.080)	(0.068)
Observations	3178	3539	2103	3038
Controls	Yes	Yes	Yes	Yes
Panel C: Years of Education				
RD Estimate	2.726***	0.676*	1.391**	1.536**
	(0.553)	(0.398)	(0.610)	(0.718)
Observations	3178	3539	2103	3038
Controls	Yes	Yes	Yes	Yes

Notes: Data is from Indonesian Family Life Survey Wave 2000. Each column reports RD treatment effect of being born on and after September 1978 with a linear control function in month-year-of-birth on each side of the discontinuity. Column (1) to (4) reports local RD regression with controls, using optimal bandwidth selection. Control variables are gender, religion, father's and mother's education, number of siblings, and place of residence based on Java or non-Java islands and urban or rural areas. Standard errors, in parentheses, are clustered at the month-year-of birth. *** significant at 1% , ** significant at 5% , * significant 10%

Furthermore, we have also combined information about parental educational background with children's gender. As illustrated in Table 10, the compulsory schooling reforms positively affect males, especially when both parents have a low level of education. However, the impact is slightly larger for boys when the interaction is with paternal education (compared to maternal education). In contrast, females derive a more substantial advantage from the reform when their parents boast a high educational background. As expected, the impact is greater for those with highly educated mothers. The considerable disparity in the coefficient between males and females arises from gender inequality in secondary school participation prior to the reform.[16] Therefore, in the context of the enacted compulsory educational reforms, the implications appear to be more pronounced for females.

Table 10: Heterogenous Analysis - Parental Educational Background * Children's Gender - Wave 2000

	Male*High Educ Dad	Male*Low Educ Dad	Male*High Educ Mom	Male*Low Educ Mom	Female*High Educ Dad	Female*Low Educ Dad	Female*High Educ Mom	Female*Low Educ Mom
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Completion on Nine Years of Compulsory Schooling								
RD Estimate	0.042	0.174***	0.062	0.160***	0.229**	0.134*	0.363***	0.083
	(0.080)	(0.056)	(0.084)	(0.056)	(0.116)	(0.071)	(0.136)	(0.063)
Observations	1629	4184	1167	4646	1775	4270	1531	4514
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Senior Secondary Enrollment								
RD Estimate	-0.100	0.255***	0.218	0.133***	0.135	0.124**	0.448**	0.036
	(0.073)	(0.042)	(0.153)	(0.049)	(0.146)	(0.052)	(0.225)	(0.052)
Observations	1629	4184	1167	4646	1775	4270	1531	4514
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: Years of Education								
RD Estimate	-0.386	1.810***	1.036	1.220***	2.182*	1.720***	3.736***	0.741*
	(0.387)	(0.399)	(0.813)	(0.381)	(1.244)	(0.525)	(1.444)	(0.427)
Observations	1629	4184	1167	4646	1775	4270	1531	4514
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Data is from Indonesian Family Life Survey Wave 2000. Each column reports RD treatment effect of being born on and after September 1978 with a linear control function in month-year-of-birth on each side of the discontinuity. Column (1) to (8) reports local RD regression with controls, using optimal bandwidth selection. Control variables are gender, religion, father's and mother's education, number of siblings, and place of residence based on Java or non-Java islands and urban or rural areas. Standard errors, in parentheses, are clustered at the month-year-of birth. *** significant at 1% , ** significant at 5% , * significant 10%.

However, the varying result by parental education of the effect of the reform introduces an intricate discussion which is inherently linked to the intergenerational transmission of education. Many studies have yielded diverse findings, potentially hinging on an array of elements such as parental involvement, sociocultural background, children's gender, and the application of twin-based methodologies or instrumental variables in the analysis. For instance, Chevalier (2004), Black et al. (2005), Holmlund et al. (2011) and Stella (2013) conclude that the importance of maternal education appears to supersede that of paternal education. Further dissecting these findings reveals a particularly pronounced effect on daughters than sons (Daouli et al., 2010; Amin et al., 2015; Pastor et al., 2021). On the other hand, Behrman and Rosenzweig (2005), Pronzato (2012), and Amin et al. (2015) argued that the significance of paternal education appears to outweigh that of maternal. Indeed, in our case, the impact is

[16] In 1993, the Gender Parity Index (GPI) demonstrated a relatively equitable distribution of male and female students at the primary school level, with a GPI of 0.98. However, the GPI for secondary school enrollment was less balanced in the same year, presenting a value of 0.81. By the year 2000, the secondary school GPI had significantly improved to a value of 0.97, nearly achieving gender parity (World Bank, 2023a, 2023b).

particularly substantial on sons rather than daughters, in line with Magnani and Zhu (2015) and Chen et al. (2019).

6. Conclusion

This paper analyzed the effect of compulsory schooling reform on educational outcomes in Indonesia. Beginning in the school years of 1994/1995, the Indonesian government expanded the mandatory education system from six to nine years. Using database surveyed six, thirteen, and twenty years, respectively after the reform, the main results indicate that the reform successfully increased a set of educational outcomes, mainly for the junior secondary completion and years of education. The reform also shaped the propensity to enroll and complete senior secondary schooling. However, we do not find that this policy was strong enough to support university attendance.

Our results rely on the identification assumption that individuals at the two sides of the thresholds are identical in terms of observable and unobservable characteristics and cannot manipulate their location with respect to the cutoff. We tested this by conducting a manipulation test and the individual's predetermined characteristics against our running variable. The results suggest that we did not find evidence of a significant break in density in our running variable and no significant jumps at the cutoff point for each individual's predetermined characteristics. Furthermore, the results are robust to sensitivity analysis and falsification exercises that point towards the internal of our identification strategy and validate the causal interpretation of the results. Therefore, the evidence reported in this paper is consistent with previous results from the existing literature, which indicate that compulsory schooling reforms are a great instrument for expanding access to higher levels of education. This finding is relevant especially for developing countries since education represents one of the main factors through which they can foster economic growth and achieve the desired economic and social development level.

The heterogeneous analysis also reveals that the reform has a more pronounced impact on girls compared to boys. Additionally, individuals from backgrounds with lower paternal education are more influenced by the reform, as are those with a high level of maternal education. Individuals who lived in urban areas at 13 had a greater impact than those who lived in rural areas and those who lived outside Java Island than those who lived on Java Island. Upon closer examination, we found that the impact was more substantial on individuals living in urban compared to those living in rural areas across both Java and Non-Java Islands. Moreover, when we combined parental educational attainment with the gender of the child, we found that male children with a low parental education experienced a greater effect from the reform. On the other hand, female children of highly educated parents were more affected by the reform.

Indeed, we see that the effect diminishes over time (from wave 2000 to 2007 to 2014). However, we interpret this decrease as an encouraging sign indicating that the older cohorts (the non-treated group) are gradually catching up in their educational attainment. We substantiate this argument by examining work experiences between older and younger cohorts. While there was a significant negative change in work experiences in 2000, the effect disappeared by 2007.

All in all, the results displayed along this paper show that compulsory education laws have the potential for expanding the human capital endowment for the population. However, the Indonesian experience suggests that previous investment in educational facilities and enforcement capacity help to the success of the law. Indeed, our results showed that there was a clear need for further policy interventions that supported individuals living in rural areas, as well as those students -especially boys- living in households with a lower socioeconomic background -and who may be more prone, in developing countries, to abandon school and join the labor market at very young ages. While increasing the enforcement capacity in remote areas may be an immediate measure to be taken, interventions aiming to reduce the opportunity cost of schooling, such as lowering or removing transportation costs to schools (Adukia et al., 2020) or providing school meals (Aurino et al., 2023) may be promising policies which may scaffold a compulsory education law, especially among disadvantaged students. Other experiences in low and middle income countries have also shown the potential of conditioned scholarship programs -such as Progresa in Mexico (Attanasio et al., 2012) or Bolsa Familia in Brazil (de Brauw et al., 2015) - to increase school attendance in rural and low-income areas.

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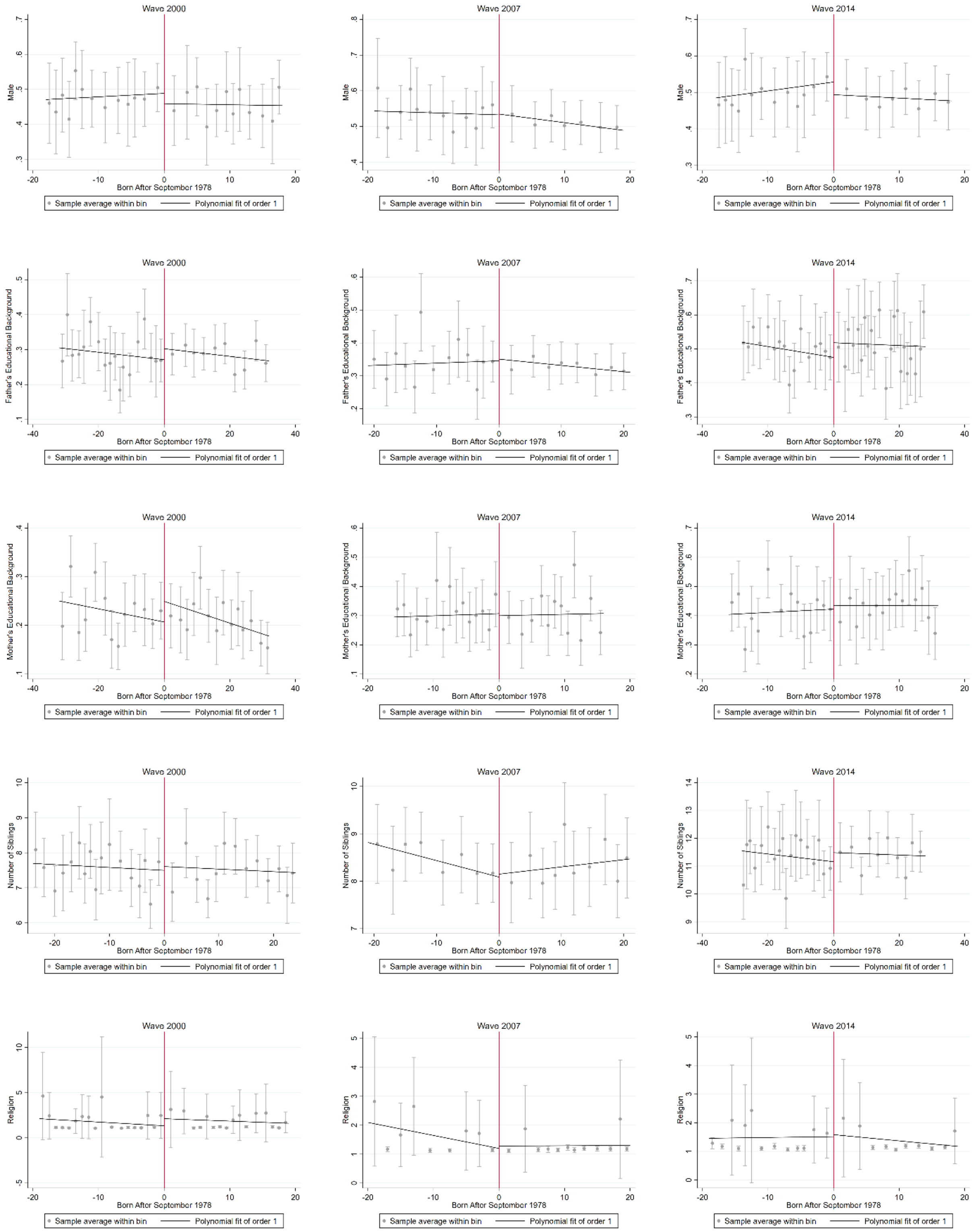
Appendix

Figure A1: Figures plot predetermined individual's characteristics in monthly bins against the month-year-of-birth of being born on and after September 1978



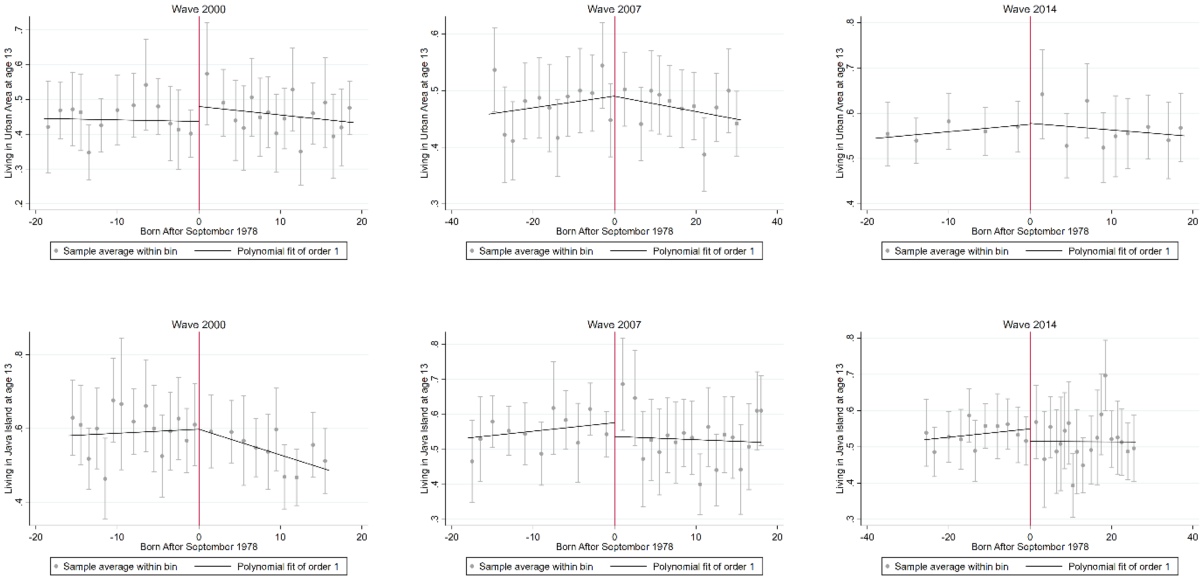
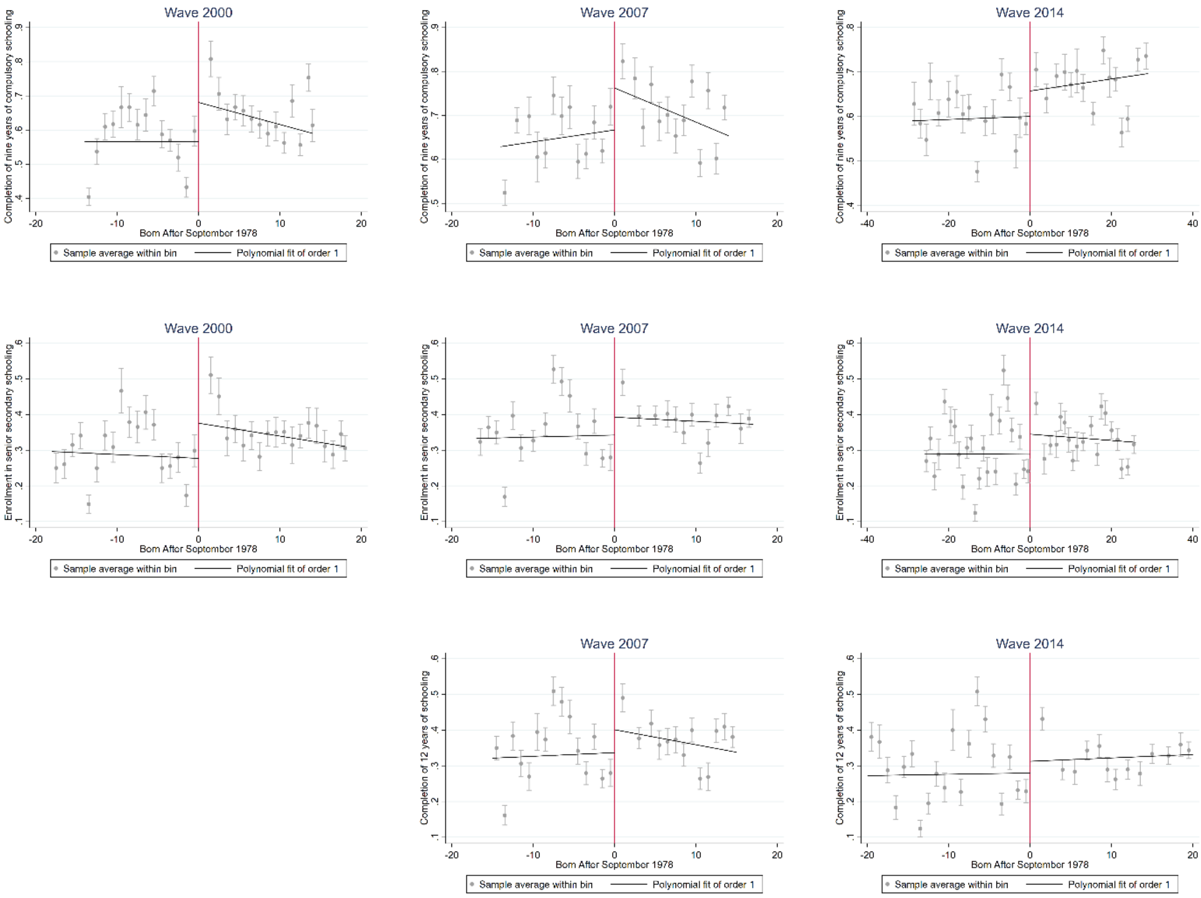


Figure A2: The figures plot a set of educational outcomes in monthly bins against the month-year-of-birth of being born on and after September 1978



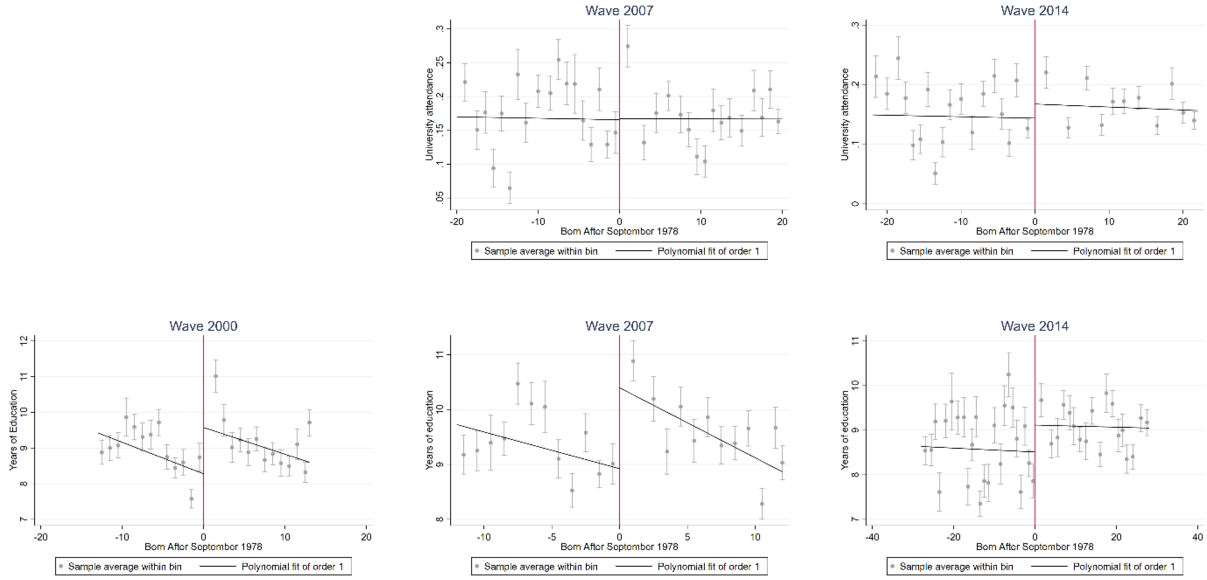
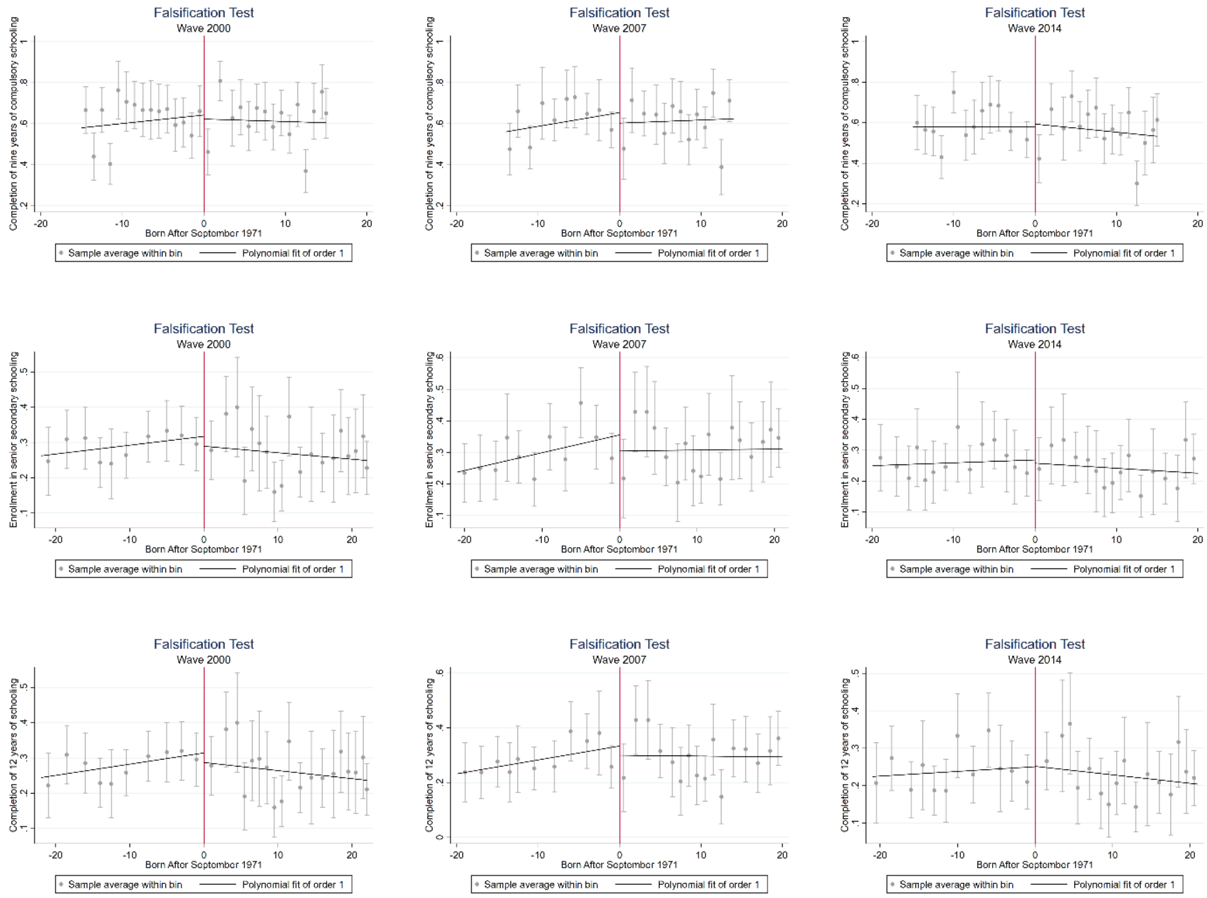
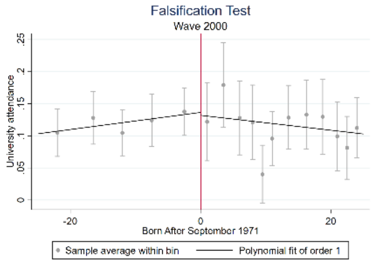
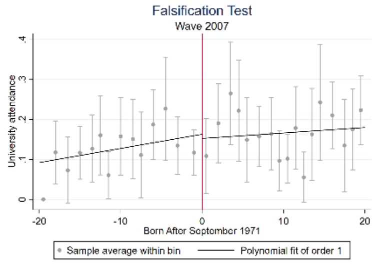
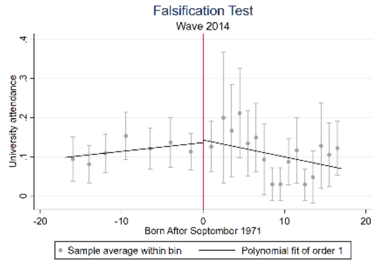


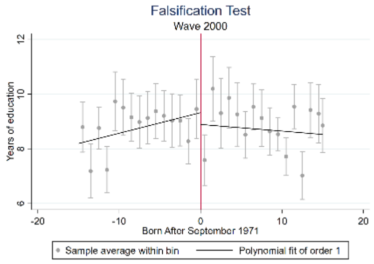
Figure A3: Falsification Analysis Using Fake Compulsory Schooling Reform in 1987

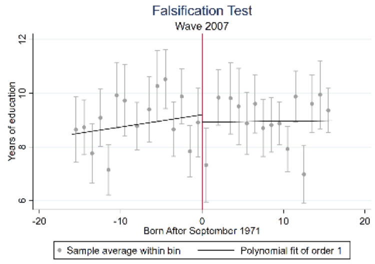



Falsification Test
Wave 2000
University attendance
0
.05
.1
.15
.2
.25
-20
0
20
Born After September 1971
Sample average within bin
Polynomial fit of order 1


Falsification Test
Wave 2007
University attendance
0
.1
.2
.3
.4
-20
-10
0
10
20
Born After September 1971
Sample average within bin
Polynomial fit of order 1


Falsification Test
Wave 2014
University attendance
0
.1
.2
.3
.4
-20
-10
0
10
20
Born After September 1971
Sample average within bin
Polynomial fit of order 1


Falsification Test
Wave 2000
Years of education
6
8
10
12
-20
-10
0
10
20
Born After September 1971
Sample average within bin
Polynomial fit of order 1


Falsification Test
Wave 2007
Years of education
6
8
10
12
-20
-10
0
10
20
Born After September 1971
Sample average within bin
Polynomial fit of order 1

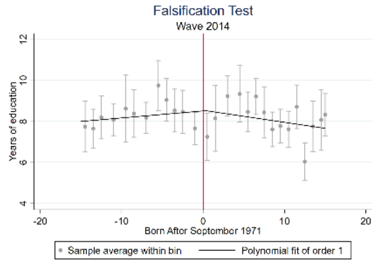

Falsification Test
Wave 2014
Years of education
4
6
8
10
12
-20
-10
0
10
20
Born After September 1971
Sample average within bin
Polynomial fit of order 1

Table A1: RD Treatment Effect on Educational Outcomes Waves 2000, 2007, and 2014 - Complete Table

Bandwidth	RD (ĥ) Without Controls	RD (ĥ)	RD (0.5 ĥ)	RD (1.5 ĥ)	RD (2 ĥ)	ĥ	Mean	N
		With Controls						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Completion on Nine Years of Compulsory Schooling								
Wave 2000	0.193*** (0.065)	0.200*** (0.044)	0.279*** (0.065)	0.129*** (0.041)	0.111*** (0.035)	16	0.563	11858
Wave 2007	0.123*** (0.043)	0.132*** (0.028)	0.151*** (0.028)	0.098*** (0.028)	0.077*** (0.026)	16	0.679	11856
Wave 2014	0.086*** (0.028)	0.070*** (0.026)	0.098*** (0.035)	0.060** (0.025)	0.059*** (0.023)	29	0.631	11627
Panel B: Enrollment on Senior Secondary Schooling								
Wave 2000	0.148*** (0.054)	0.139*** (0.035)	0.242*** (0.034)	0.121*** (0.029)	0.105*** (0.026)	19	0.270	11858
Wave 2007	0.098** (0.047)	0.123*** (0.040)	0.246*** (0.034)	0.072** (0.036)	0.057* (0.033)	19	0.344	11856
Wave 2014	0.058 (0.044)	0.052 (0.039)	0.150*** (0.040)	0.052 (0.034)	0.037 (0.031)	24	0.299	11627
Panel C: Completion on 12 Years of Schooling								
Wave 2007	0.111** (0.051)	0.141*** (0.040)	0.245*** (0.031)	0.080** (0.037)	0.058* (0.034)	18	0.331	11856
Wave 2014	0.069 (0.049)	0.065 (0.044)	0.194*** (0.047)	0.059 (0.037)	0.045 (0.034)	21	0.285	11627
Panel D: University Attendance								
Wave 2007	0.012 (0.031)	0.022 (0.030)	0.093** (0.043)	0.004 (0.023)	0.002 (0.020)	24	0.155	11856
Wave 2014	0.026 (0.030)	0.018 (0.026)	0.062* (0.033)	0.027 (0.021)	0.023 (0.019)	24	0.138	11627
Panel E: Years of Education								
Wave 2000	1.794*** (0.587)	1.928*** (0.367)	2.577*** (0.448)	1.164*** (0.364)	0.938*** (0.307)	15	8.507	11858
Wave 2007	1.401*** (0.382)	1.753*** (0.264)	1.988*** (0.283)	1.022*** (0.296)	0.726*** (0.268)	15	9.264	11856
Wave 2014	0.802** (0.342)	0.650** (0.300)	1.136*** (0.352)	0.573** (0.268)	0.430* (0.242)	28	8.704	11627

Notes: Data is from Indonesian Family Life Survey Wave 2000, 2007, and 2014. Each column reports the RD treatment effect of being born on and after September 1978 with a linear control function in month-year-of-birth on each side of the discontinuity. Column (1) and (2) reports local RD regression without and with controls, respectively, using optimal bandwidth selection. Columns (3), (4), and (5) report local RD regressions with a linear control function using half, one-half, and double of the optimal bandwidth, respectively. Column (6) reports the optimal bandwidth estimated by the Imbens and Klayanaraman (2009) algorithm. Column (7) reports the outcome mean within the optimal bandwidth, and column (8) reports the number of observations used in the estimations. The dependent variable in: Panel A is a dummy variable equal to one if the individual completed junior secondary school; Panel B is a dummy variable equal to one if the individual enrolled to senior secondary school; Panel C is a dummy variable equal to one if the individual completed senior secondary school; Panel D is a dummy variable equal to one if the individual enrolled to university; and Panel E is the years of education attained. Control variables are gender, religion, father's and mother's education, number of siblings, and place of residence based on Java or non-Java islands and urban or rural areas. Standard errors, in parentheses, are clustered at the month-year-of-birth. *** significant at 1% , ** significant at 5% , * significant 10%.

Table A2: Work Experiences between control and treatment group

	2000		2007	
	(1)	(2)	(3)	(4)
RD Estimate	-0.476**	-0.432*	-0.102	-0.043
	(0.242)	(0.235)	(0.518)	(0.525)
Observations	9410	9410	9410	9410
Controls	No	Yes	No	Yes

Notes: Data is from Indonesian Family Life Survey Wave 2000 and 2007. Each column reports RD treatment effect of work experiences. All results are estimated using the optimal bandwidth selection computed using Imbens and Klayanaraman (2009) algorithm. The dependent variable is work experiences (in years). Control variables are gender, religion, father's and mother's education, number of siblings, and place of residence based on Java or non-Java islands and urban or rural areas. Standard errors, in parentheses, are clustered at the month-year-of-birth. *** significant at 1% , ** significant at 5% , * significant 10%.

Table A3: Heterogenous Analysis of RD Treatment (Individual Covariates) - Complete Table

	Male		Female		Dad Educ ≥ 9		Dad Educ < 9		Mom Educ ≥ 9		Mom Educ < 9	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Completion on Nine Years of Compulsory Schooling												
Wave 2000	0.148***	0.147***	0.217**	0.196**	0.125*	0.145**	0.226***	0.192***	0.211*	0.230**	0.202***	0.153***
	(0.056)	(0.049)	(0.095)	(0.080)	(0.075)	(0.069)	(0.056)	(0.047)	(0.120)	(0.113)	(0.049)	(0.037)
Optimal Bandwidth (ĥ)		19		16		14		16		14		16
Observations	5813	5813	6045	6045	3404	3404	8454	8454	2698	2698	9160	9160
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2007	0.069	0.080**	0.209***	0.219***	0.033	0.037*	0.169***	0.155***	0.061	0.067	0.164***	0.133***
	(0.045)	(0.041)	(0.064)	(0.045)	(0.021)	(0.022)	(0.050)	(0.036)	(0.049)	(0.055)	(0.034)	(0.029)
Optimal Bandwidth (ĥ)		21		15		26		15		17		15
Observations	6111	6111	5745	5745	3887	3887	7969	7969	3579	3579	8277	8277
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	0.069	0.083*	0.113**	0.049	-0.009	-0.002	0.113**	0.106***	0.069	0.053	0.063	0.068**
	(0.052)	(0.047)	(0.051)	(0.046)	(0.056)	(0.053)	(0.046)	(0.038)	(0.050)	(0.048)	(0.039)	(0.031)
Optimal Bandwidth (ĥ)		32		29		21		21		22		23
Observations	5822	5822	5805	5805	3747	3747	7880	7880	3448	3448	8179	8179
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel B: Senior Secondary Enrollment												
Wave 2000	0.123**	0.123***	0.154**	0.124**	0.012	0.014	0.241***	0.203***	0.325**	0.370***	0.109***	0.082***
	(0.049)	(0.039)	(0.073)	(0.056)	(0.065)	(0.062)	(0.051)	(0.042)	(0.139)	(0.137)	(0.036)	(0.031)
Optimal Bandwidth (ĥ)		21		21		28		16		16		22
Observations	5813	5813	6045	6045	3404	3404	8454	8454	2698	2698	9160	9160
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2007	0.096	0.118*	0.046	0.048	0.059	0.069	0.155***	0.154***	0.073	0.098	0.137***	0.092**
	(0.065)	(0.060)	(0.051)	(0.045)	(0.084)	(0.081)	(0.038)	(0.032)	(0.066)	(0.068)	(0.050)	(0.043)
Optimal Bandwidth (ĥ)		19		29		22		15		21		17
Observations	6111	6111	5745	5745	3887	3887	7969	7969	3579	3579	8277	8277
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	0.065	0.080*	0.083	0.048	-0.018	-0.017	0.090**	0.084**	0.045	0.029	0.049	0.056
	(0.047)	(0.043)	(0.055)	(0.044)	(0.080)	(0.082)	(0.046)	(0.041)	(0.057)	(0.051)	(0.044)	(0.041)
Optimal Bandwidth (ĥ)		32		20		28		21		26		24
Observations	5822	5822	5805	5805	3747	3747	7880	7880	3448	3448	8179	8179
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel C: Completion on 12 Years of Schooling												
Wave 2007	0.107	0.131*	0.061	0.049	0.064	0.059	0.140***	0.140***	0.074	0.102	0.151***	0.117***
	(0.074)	(0.068)	(0.049)	(0.044)	(0.079)	(0.074)	(0.046)	(0.038)	(0.066)	(0.068)	(0.053)	(0.045)
Optimal Bandwidth (ĥ)		18		27		24		15		21		15
Observations	6111	6111	5745	5745	3887	3887	7969	7969	3579	3579	8277	8277
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	0.059	0.082	0.109**	0.077*	0.011	0.006	0.099**	0.095**	0.052	0.036	0.073	0.072
	(0.055)	(0.051)	(0.054)	(0.043)	(0.073)	(0.072)	(0.050)	(0.044)	(0.069)	(0.059)	(0.047)	(0.047)
Optimal Bandwidth (ĥ)		24		18		32		20		23		20
Observations	5822	5822	5805	5805	3747	3747	7880	7880	3448	3448	8179	8179
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel D: University Attendance												
Wave 2007	-0.036	-0.027	0.067**	0.087***	0.006	-0.017	0.023	0.021	-0.036	-0.031	0.087**	0.072**
	(0.051)	(0.051)	(0.032)	(0.027)	(0.069)	(0.063)	(0.020)	(0.021)	(0.051)	(0.045)	(0.038)	(0.033)
Optimal Bandwidth (ĥ)		20		25		22		22		25		15
Observations	6111	6111	5745	5745	3887	3887	7969	7969	3579	3579	8277	8277
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	-0.026	-0.006	0.070*	0.048	-0.034	-0.031	0.044	0.039	-0.004	-0.010	0.016	0.018
	(0.038)	(0.042)	(0.038)	(0.032)	(0.097)	(0.084)	(0.034)	(0.028)	(0.084)	(0.075)	(0.026)	(0.027)
Optimal Bandwidth (ĥ)		18		25		17		22		17		27
Observations	5822	5822	5805	5805	3747	3747	7880	7880	3448	3448	8179	8179
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel E: Years of Education												
Wave 2000	1.127**	1.267***	2.176***	1.952***	0.494	0.866	2.476***	2.115***	2.497***	2.929***	1.684***	1.286***
	(0.465)	(0.365)	(0.806)	(0.620)	(0.720)	(0.653)	(0.444)	(0.338)	(0.955)	(0.935)	(0.432)	(0.325)
Optimal Bandwidth (ĥ)		18		16		18		14		14		15
Observations	5813	5813	6045	6045	3404	3404	8454	8454	2698	2698	9160	9160
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2007	0.830*	1.017**	1.835***	2.183***	0.380	0.090	1.626***	1.553***	0.366	0.478	2.079***	1.754***
	(0.434)	(0.413)	(0.485)	(0.350)	(0.470)	(0.455)	(0.313)	(0.279)	(0.525)	(0.523)	(0.405)	(0.362)
Optimal Bandwidth (ĥ)		18		16		22		15		23		13
Observations	6111	6111	5745	5745	3887	3887	7969	7969	2698	2698	9160	9160
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	0.370	0.505	1.334***	0.900**	-0.262	-0.219	1.143***	1.059***	0.272	0.129	0.726*	0.729*
	(0.466)	(0.453)	(0.499)	(0.374)	(0.716)	(0.652)	(0.410)	(0.374)	(0.537)	(0.483)	(0.403)	(0.401)
Optimal Bandwidth (ĥ)		27		23		21		23		22		27
Observations	5822	5822	5805	5805	3747	3747	7880	7880	3448	3448	8179	8179
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Data is from Indonesian Family Life Survey Wave 2000, 2007, and 2014. Column (1) – (12) report the RD treatment effect of being born on and after September 1978 with a linear control function in month-year-of-birth on each side of the discontinuity and exploits the optimal bandwidth selection based on Imbens and Klayanaraman (2009) algorithm. The dependent variable in: Panel A is a dummy variable equal to one if the individual completed junior secondary school; Panel B is a dummy variable equal to one if the individual enrolled to senior secondary school; Panel C is a dummy variable equal to one if the individual completed senior secondary school; Panel D is a dummy variable equal to one if the individual enrolled to university; and Panel E is the years of education attained. Control variables are gender, religion, father's and mother's education, number of siblings, and place of residence based on Java or non-Java islands and urban or rural areas. Standard errors, in parentheses, are clustered at the month-year-of-birth. *** significant at 1% , ** significant at 5% , * significant 10%.

Table A4: Heterogenous Analysis of RD Treatment (Place of Residence at aged 13) - Complete Table

	Urban		Rural		Java Island		Non-Java Island	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Completion on Nine Years of Compulsory Schooling								
Wave 2000	0.232***	0.281***	0.111*	0.117**	0.138**	0.150***	0.202**	0.184***
	(0.053)	(0.047)	(0.066)	(0.056)	(0.055)	(0.041)	(0.089)	(0.071)
Optimal Bandwidth (ĥ)		15		17		16		20
Observations	5281	5281	6577	6577	6717	6717	5141	5141
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2007	0.113***	0.142***	0.130**	0.099***	0.077	0.084	0.160	0.184**
	(0.027)	(0.029)	(0.054)	(0.038)	(0.056)	(0.053)	(0.106)	(0.083)
Optimal Bandwidth (ĥ)		15		18		19		17
Observations	5610	5610	6246	6246	6311	6311	5545	5545
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	0.092	0.083*	0.052	0.064	0.067	0.035	0.091*	0.124***
	(0.056)	(0.049)	(0.062)	(0.056)	(0.042)	(0.036)	(0.054)	(0.042)
Optimal Bandwidth (ĥ)		20		21		19		20
Observations	6530	6530	5097	5097	6113	6113	5514	5514
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Panel B: Senior Secondary Enrollment								
Wave 2000	0.140**	0.160**	0.098**	0.101**	0.111*	0.110**	0.192***	0.164***
	(0.066)	(0.065)	(0.042)	(0.043)	(0.058)	(0.043)	(0.068)	(0.055)
Optimal Bandwidth (ĥ)		20		22		20		22
Observations	5281	5281	6577	6577	6717	6717	5141	5141
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2007	0.118*	0.152**	0.067	0.044	0.083**	0.105***	0.076	0.086
	(0.066)	(0.071)	(0.047)	(0.043)	(0.040)	(0.033)	(0.082)	(0.074)
Optimal Bandwidth (ĥ)		19		23		24		20
Observations	5610	5610	6246	6246	6311	6311	5545	5545
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	0.092*	0.077*	0.019	0.025	0.037	0.014	0.086	0.105**
	(0.048)	(0.044)	(0.053)	(0.043)	(0.041)	(0.039)	(0.057)	(0.050)
Optimal Bandwidth (ĥ)		25		27		32		20
Observations	6530	6530	5097	5097	6113	6113	5514	5514
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Panel C: Completion on 12 Years of Schooling								
Wave 2007	0.123*	0.159**	0.091*	0.060	0.088**	0.114***	0.101	0.128*
	(0.070)	(0.075)	(0.049)	(0.047)	(0.040)	(0.034)	(0.082)	(0.073)
Optimal Bandwidth (ĥ)		18		18		22		17
Observations	5610	5610	6246	6246	6311	6311	5545	5545
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	0.102**	0.094**	0.022	0.031	0.029	0.005	0.132**	0.165***
	(0.051)	(0.047)	(0.063)	(0.051)	(0.043)	(0.042)	(0.059)	(0.051)
Optimal Bandwidth (ĥ)		20		24		29		18
Observations	6530	6530	5097	5097	6113	6113	5514	5514
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Panel D: University Attendance								
Wave 2007	-0.023	-0.006	0.052*	0.042*	0.029	0.026	-0.013	-0.012
	(0.048)	(0.055)	(0.029)	(0.025)	(0.047)	(0.040)	(0.039)	(0.039)
Optimal Bandwidth (ĥ)		23		22		27		22
Observations	5610	5610	6246	6246	6311	6311	5545	5545
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	0.051	0.031	-0.010	-0.004	0.032	0.017	0.012	0.011
	(0.038)	(0.036)	(0.033)	(0.026)	(0.046)	(0.041)	(0.026)	(0.021)
Optimal Bandwidth (ĥ)		28		20		19		25
Observations	6530	6530	5097	5097	6113	6113	5514	5514
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Panel E: Years of Education								
Wave 2000	1.666***	2.133***	1.172**	1.178***	1.474***	1.842***	1.905***	1.799***
	(0.592)	(0.555)	(0.507)	(0.433)	(0.530)	(0.344)	(0.724)	(0.523)
Optimal Bandwidth (ĥ)		17		16		16		17
Observations	5281	5281	6577	6577	6717	6717	5141	5141
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2007	1.005**	1.477***	1.531***	1.346***	0.855**	1.074***	1.419**	1.986***
	(0.469)	(0.516)	(0.377)	(0.316)	(0.409)	(0.395)	(0.642)	(0.506)
Optimal Bandwidth (ĥ)		17		15		22		15
Observations	5610	5610	6246	6246	6311	6311	5545	5545
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	1.178***	1.028***	0.174	0.228	0.883**	0.596	0.573	0.770**
	(0.382)	(0.320)	(0.544)	(0.452)	(0.424)	(0.388)	(0.437)	(0.332)
Optimal Bandwidth (ĥ)		21		26		21		21
Observations	5813	5813	6045	6045	3404	3404	8454	8454
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Data is from Indonesian Family Life Survey Wave 2000, 2007, and 2014. Column (1) – (8) report the RD treatment effect of being born on and after September 1978 with a linear control function in month-year-of-birth on each side of the discontinuity and exploits the optimal bandwidth selection based on Imbens and Klayanaraman (2009) algorithm. The dependent variable in: Panel A is a dummy variable equal to one if the individual completed junior secondary school; Panel B is a dummy variable equal to one if the individual enrolled to senior secondary school; Panel C is a dummy variable equal to one if the individual completed senior secondary school; Panel D is a dummy variable equal to one if the individual enrolled to university; and Panel E is the years of education attained. Control variables are gender, religion, father's and mother's education, number of siblings, and place of residence based on Java or non-Java islands and urban or rural areas. Standard errors, in parentheses, are clustered at the month-year-of-birth. *** significant at 1% , ** significant at 5% , * significant 10%.

Table A5: Heterogenous Analysis of RD Treatment (Individual Covariates) Using A Static Bandwidth - Complete Table

	All	Male		Female		Dad Educ>=9		Dad Educ<9		Mom Educ>=9		Mom Educ<9	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Panel A: Completion on Nine Years of Compulsory Schooling													
Wave 2000	0.200***	0.177***	0.175***	0.259***	0.223***	0.125*	0.127*	0.252***	0.222***	0.207*	0.208*	0.221***	0.177***
	(0.044)	(0.064)	(0.051)	(0.097)	(0.080)	(0.076)	(0.068)	(0.057)	(0.047)	(0.121)	(0.113)	(0.048)	(0.036)
Optimal Bandwidth ($\hat{h}$)							16						
Observations	11858	5813	5813	6045	6045	3404	3404	8454	8454	2698	2698	9160	9160
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2007	0.123***	0.060	0.078*	0.208***	0.205***	0.037	0.045	0.173***	0.157***	0.066	0.069	0.164***	0.135***
	(0.040)	(0.054)	(0.044)	(0.066)	(0.046)	(0.027)	(0.027)	(0.052)	(0.037)	(0.052)	(0.059)	(0.034)	(0.030)
Optimal Bandwidth ($\hat{h}$)							16						
Observations	11856	6111	6111	5745	5745	3887	3887	7969	7969	3579	3579	8277	8277
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	0.070***	0.065	0.083*	0.108**	0.058	0.003	0.005	0.114***	0.102***	0.082*	0.068	0.068**	0.065**
	(0.026)	(0.054)	(0.048)	(0.051)	(0.048)	(0.045)	(0.045)	(0.036)	(0.031)	(0.044)	(0.043)	(0.035)	(0.030)
Optimal Bandwidth ($\hat{h}$)							29						
Observations	11627	5822	5822	5805	5805	3747	3747	7880	7880	3448	3448	8179	8179
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel B: Senior Secondary Enrollment													
Wave 2000	0.139***	0.133**	0.133***	0.179**	0.145**	-0.010	0.005	0.222***	0.196***	0.296**	0.313**	0.119***	0.085**
	(0.035)	(0.054)	(0.040)	(0.078)	(0.062)	(0.090)	(0.077)	(0.051)	(0.042)	(0.134)	(0.128)	(0.042)	(0.035)
Optimal Bandwidth ($\hat{h}$)							19						
Observations	11858	5813	5813	6045	6045	3404	3404	8454	8454	2698	2698	9160	9160
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2007	0.123***	0.106	0.117*	0.140***	0.140***	0.109	0.129	0.126***	0.116***	0.106	0.133*	0.133***	0.115**
	(0.040)	(0.070)	(0.061)	(0.050)	(0.045)	(0.095)	(0.092)	(0.039)	(0.032)	(0.073)	(0.072)	(0.051)	(0.045)
Optimal Bandwidth ($\hat{h}$)							19						
Observations	11856	6111	6111	5745	5745	3887	3887	7969	7969	3579	3579	8277	8277
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	0.052	0.048	0.068	0.077	0.046	-0.020	-0.016	0.091**	0.085**	0.043	0.030	0.052	0.056
	(0.039)	(0.052)	(0.048)	(0.049)	(0.042)	(0.085)	(0.085)	(0.041)	(0.038)	(0.058)	(0.051)	(0.044)	(0.043)
Optimal Bandwidth ($\hat{h}$)							24						
Observations	11627	5822	5822	5805	5805	3747	3747	7880	7880	3448	3448	8179	8179
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel C: Completion on 12 Years of Schooling													
Wave 2007	0.141***	0.123	0.137**	0.161***	0.157***	0.145	0.167*	0.136***	0.125***	0.134*	0.165**	0.149***	0.128***
	(0.040)	(0.081)	(0.069)	(0.049)	(0.045)	(0.096)	(0.092)	(0.047)	(0.039)	(0.074)	(0.073)	(0.054)	(0.046)
Optimal Bandwidth ($\hat{h}$)							18						
Observations	11856	6111	6111	5745	5745	3887	3887	7969	7969	3579	3579	8277	8277
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	0.065	0.055	0.080	0.086	0.057	0.011	0.016	0.094*	0.090**	0.052	0.036	0.066	0.072
	(0.044)	(0.061)	(0.058)	(0.054)	(0.044)	(0.098)	(0.096)	(0.050)	(0.044)	(0.076)	(0.064)	(0.047)	(0.047)
Optimal Bandwidth ($\hat{h}$)							21						
Observations	11627	5822	5822	5805	5805	3747	3747	7880	7880	3448	3448	8179	8179
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel D: University Attendance													
Wave 2007	0.022	-0.037	-0.034	0.086**	0.091***	0.011	0.033	0.024	0.022	-0.030	-0.013	0.045	0.037
	(0.030)	(0.051)	(0.049)	(0.034)	(0.027)	(0.072)	(0.074)	(0.021)	(0.022)	(0.054)	(0.048)	(0.034)	(0.029)
Optimal Bandwidth ($\hat{h}$)							24						
Observations	11856	6111	6111	5745	5745	3887	3887	7969	7969	3579	3579	8277	8277
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	0.018	-0.024	-0.007	0.074*	0.055	-0.025	-0.023	0.043	0.038	0.011	0.004	0.018	0.022
	(0.026)	(0.036)	(0.039)	(0.042)	(0.035)	(0.078)	(0.070)	(0.034)	(0.028)	(0.070)	(0.063)	(0.029)	(0.031)
Optimal Bandwidth ($\hat{h}$)							24						
Observations	11627	5822	5822	5805	5805	3747	3747	7880	7880	3448	3448	8179	8179
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel E: Years of Education													
Wave 2000	1.928***	1.449***	1.427***	2.856***	2.423***	1.160	1.386**	2.515***	2.151***	2.743***	2.916***	1.954***	1.565***
	(0.367)	(0.548)	(0.376)	(0.834)	(0.606)	(0.889)	(0.700)	(0.451)	(0.341)	(1.005)	(0.952)	(0.431)	(0.313)
Optimal Bandwidth ($\hat{h}$)							15						
Observations	11858	5813	5813	6045	6045	3404	3404	8454	8454	2698	2698	9160	9160
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2007	1.753***	1.121**	1.285***	2.482***	2.424***	1.490***	1.745***	1.870***	1.697***	1.133*	1.390**	2.071***	1.809***
	(0.264)	(0.520)	(0.433)	(0.450)	(0.342)	(0.493)	(0.479)	(0.307)	(0.285)	(0.615)	(0.641)	(0.414)	(0.378)
Optimal Bandwidth ($\hat{h}$)							15						
Observations	11856	6111	6111	5745	5745	3887	3887	7969	7969	2698	2698	9160	9160
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Wave 2014	0.650**	0.396	0.559	1.219***	0.822**	-0.043	-0.069	1.103***	0.999***	0.458	0.311	0.708*	0.741*
	(0.300)	(0.463)	(0.431)	(0.468)	(0.379)	(0.582)	(0.557)	(0.376)	(0.350)	(0.460)	(0.414)	(0.399)	(0.409)
Optimal Bandwidth ($\hat{h}$)							28						
Observations	11627	5822	5822	5805	5805	3747	3747	7880	7880	3448	3448	8179	8179
Controls	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Data is from Indonesian Family Life Survey Wave 2000, 2007, and 2014. Column (1) is the copy from the baseline estimation. Column (2) – (13) report the RD treatment effect of being born on and after September 1978 with a linear control function in month-year-of-birth on each side of the discontinuity and employing a static bandwidth around the cutoff, which is the optimal bandwidth estimated for the entire sample based on Imbens and Klayanaraman (2009) algorithm. The dependent variable in: Panel A is a dummy variable equal to one if the individual completed junior secondary school; Panel B is a dummy variable equal to one if the individual enrolled to senior secondary school; Panel C is a dummy variable equal to one if the individual completed senior secondary school; Panel D is a dummy variable equal to one if the individual enrolled to university; and Panel E is the years of education attained. Control variables are gender, religion, father's and mother's education, number of siblings, and place of residence based on Java or non-Java islands and urban or rural areas. Standard errors, in parentheses, are clustered at the month-year-of-birth. *** significant at 1% , ** significant at 5% , * significant 10%.