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The Philippine Review of Economics

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The impacts of secondary education reforms on schooling and income of women and men in the Philippines

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In 1988 and 1989, the Philippine government implemented free public and subsidized private secondary schooling through two major policies, namely Republic Act (RA) 6655 and RA 6728. This study investigates the long-run impacts of the two policies on schooling attainment and income using a regression discontinuity design (RDD). It draws data from the Annual Poverty Indicators Survey 2008 and 2011. We present two main findings. First, younger cohorts of women and men, who are policy beneficiaries, have significantly higher educational attainment relative to non-beneficiaries. Second, an additional year of schooling significantly increases individual income of women in the informal sector and men in the formal sector. In brief, our findings suggest that the policies are effective in enhancing schooling attainment, but the downstream impacts appear to have accrued substantially to women employed in the informal sector and men employed in the formal sector.

JEL classification: I21, I25, I26, I28, O1
Keywords: schooling, income, gender, regression discontinuity design

1. Introduction

Education is widely recognized as an important factor in advancing human capital. It plays a crucial role in helping individuals gain marketable skills that may lead to better prospects of employment and higher income [Fasih 2008]. Governments and international organizations have placed high emphasis on prioritizing education in their policy agenda. The United Nations (UN), for instance, has set targets in improving the quality of and access to education through the Millennium Development Goals (MDGs) and the Sustainable

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Development Goals (SDGs). Specifically, Goal 2 of the MDGs aims to ensure that by 2015, children in all countries will be able to complete primary schooling [UN 2015]. Likewise, Goal 4 of the SDGs aims to continue the agenda of the MDGs by ensuring inclusive and equitable quality education for all from 2015 to 2030 [UN 2017]. Such goals of the UN focus on increasing access to quality education that can enhance individual productivity and improve labor market outcomes.

In 1986, the Philippines started to invest more in higher education to enhance the skills of its labor force through two major policies. First, in 1988, the government implemented the Free Public Secondary Education Act or Republic Act (RA) 6655, which eliminates tuition fees in all public high schools. Second, in the following year, it supported private schools through the passing of the Government Assistance to Students and Teachers in Private Education (GASTPE) Act or RA 6728, which subsidizes private schooling. These two policies reduce the relative price of schooling, easing the resource constraints of households in sending their children to school [Revilla and Estudillo 2016]. Notably, the two programs serve as the most important educational reforms since the implementation of free public primary schooling during the American colonial period (1898-1946) ["Historical perspective of the Philippine educational system" n.d.].

In terms of labor market outcomes, the least educated Filipinos, like many others worldwide, are employed in lowly paid jobs, while those with higher educational attainment work in highly paid occupations. Moreover, the less educated continue to earn less than the more educated. Evidently, education serves as the single most important factor that influences employment opportunities and income differentials [Luo and Terada 2009] and the two policies may help in improving labor market prospects for all children.

To our knowledge, no rigorous impact assessment of RA 6655 and RA 6728 had been conducted in the Philippines. This study aims to explore the impact of free and subsidized secondary education on schooling attainment and, in turn, to assess the downstream impacts of schooling on individual earnings. We conduct the analysis for the whole population as well as for the subgroups of women and men. Our main hypothesis is that RA 6655 and RA 6728 are expansionary educational reforms that increase schooling attainment and, eventually, improve individual income across genders.

The study utilizes a quasi-experimental approach called the regression discontinuity design (RDD), which allows us to select a cut-off, based on the year of policy implementation, to observe the effects on the group affected by the policy. In particular, fuzzy RDD uses an instrumental variable based on individuals' exposure to free and subsidized secondary schooling policies to address the endogeneity of schooling. Briefly, the main findings of this study are the following: First, the policies have the impact of significantly increasing the educational attainment of women and men beneficiaries vis-à-vis non-beneficiaries. Second, schooling causally increases individual income of informally employed women and formally employed men.

Our study contributes to the extant literature in several ways. First, in contrast to studies that analyze mostly short-term effects, our analysis captures long-term effects of educational policies on schooling and income. Second, to our knowledge, this study is one of the first in the Philippines to link specific educational policies to earnings using the regression discontinuity approach. Third, while earlier studies show mere associations, our results reflect causal relations between education and individual income. Fourth, the same model in our study may be used to analyze more recent schooling policies such as the K to 12 program and the free college tuition act in the Philippines once data become available.

This study has five remaining sections. Section 2 presents a background on the major public and private secondary education policies implemented in 1988 and 1989. Section 3 provides the literature review of policy impacts. Section 4 describes the empirical strategy and data. Section 5 discusses the results. Finally, Section 6 concludes the study.

2. Background on the major secondary education reforms

With the aim of making secondary education accessible to all, the Philippine government implemented the Free Public Secondary Education Act, also called RA 6655, in May 1988. This law ensures that public secondary schools, including national high schools, general comprehensive high schools, and high schools funded by local government units, are free from tuition and other school fees. RA 6655 took effect in school year 1988-1989. Likewise, in recognition of the importance of the private sector in providing and promoting quality education, the government implemented the GASTPE Act or RA 6728 in June 1989. Specifically, the law provides assistance to students in private secondary schools through tuition fee supplements, the High School Textbook Assistance Fund, and the Educational Service Contracting (ESC) scheme. The ESC scheme allows the Department of Education to enter into contracts with private schools and settle fees of students who cannot be accommodated by public high schools due to congestion or children who live in areas with no public schools. To finance the implementation of the policies, budgets were realigned within the education ministry, additional budget adjustments were incorporated in the succeeding fiscal years, and other budget sources were tapped (i.e., taxes collected from airports, coconut levies, etc.) [Free Public Secondary Education Act of 1988 1988; Government Assistance To Students and Teachers In Private Education Act 1989]. Since the implementation of RA 6655 and RA 6728 in 1988 and 1989, the Philippines has evidently experienced rising gross enrollment rates in secondary school [Revilla and Estudillo 2016]. This is indicative of the positive impact of the two policies on enrollment.

It is worth noting that the Philippine secondary education system has recently gone through another major reform. In 2013, the government passed the Enhanced

Basic Education Act or the K to 12 program which extends secondary education from four to six years. Hence, the country's education system now follows the basic 6-6-4 structure: six years of primary, six years of secondary, and six years of undergraduate. Pre-primary and basic education are compulsory, while public pre-primary, basic, and higher education are tuition-free.

Given this structure and the year of implementation of our major reforms, the cut-off year in our regression discontinuity design should be the year of birth of those individuals who were at least in fourth year high school or around 15 years old in 1989. Thus, those individuals born in and after 1974 are part of our treatment group (beneficiaries of the reform), while those born before 1974 are part of our control group (non-beneficiaries). It is important to mention that the beneficiaries of the program are those who entered the labor force after the economic liberalization in 1986 and, thus, were able to benefit from the rising returns to education due to liberalization.

3. Impacts of education policies

One commonly used method in impact evaluation of education policies is the randomized control trial (RCT). Using this method, Duflo, Dupas, and Kremer [2021] show that Ghana's scholarship program increases educational attainment by 1.3 years and the probability of completing secondary school by 55 percent. The recipients are also more likely to increase their earnings significantly. For vocational students, total earnings rise by about 19 percent, while their rate of returns to education is around 13 percent.

Further, several studies that use instrumental variables (IV) have been conducted to analyze returns to schooling. For instance, Acemoglu and Angrist [2001] estimate the impact of compulsory schooling laws on earnings using quarter of birth (i.e., birth month falls on first, second, third, or fourth quarter of the year) and differences in compulsory attendance and child labor laws across the US as IV. They reveal that a year of compulsory education raises annual earnings of students by approximately ten percent.

Finally, the regression discontinuity design is another method frequently applied in the analysis of education reforms. Filmer and Schady [2014] use sharp regression discontinuity design to explore the effects of a three-year scholarship program in Cambodia. Results indicate a substantial increase in schooling attainment of 0.6 years. However, they show no significant impact on employment and earnings. The study of Ozier [2018] on the impact of secondary school completion on employment in Kenya reveals that men in their 20s, who have completed secondary school, are 50 percent less likely to be in low-skill self-employment. The likelihood of formal employment is positive, although insignificant, in all specifications. In the case of Uganda, Keats [2018] mentions that the universal primary education reform increases women's educational attainment by 0.6 years and improves women's employment outcomes. To be specific, additional schooling increases the

probability of working by 9.2 percent, of having salaried work by 33 percent, and of receiving cash payment by 13 percent.

In the Philippines, analysis of schooling policies and outcomes that utilize experimental and quasi-experimental approaches are still limited. For instance, Maluccio [1998] exploits distance to secondary school as a main instrument in analyzing returns to education in the Bicol region of the Philippines. He observes that estimates of returns to schooling increase substantially when instruments are used to address the endogeneity of schooling. His dataset, however, is not nationally representative. Meanwhile, Sakellariou [2006] uses a national survey dataset from the Philippines in 1999 to examine the causal effect of schooling on wages. For his IV method, he uses the implementation of free secondary schooling policy in 1988 and secondary enrollment levels when the individual was 12 years old as instruments. Consistent with previous evidence, he finds that IV estimates are typically higher than ordinary least squares (OLS) estimates. He specifies that returns to education are around six to eight percent for OLS and 16 percent for IV. The aim of this paper is to supplement these earlier findings using an RDD in the Philippine context.

4. Empirical strategy and data

4.1. Empirical strategy

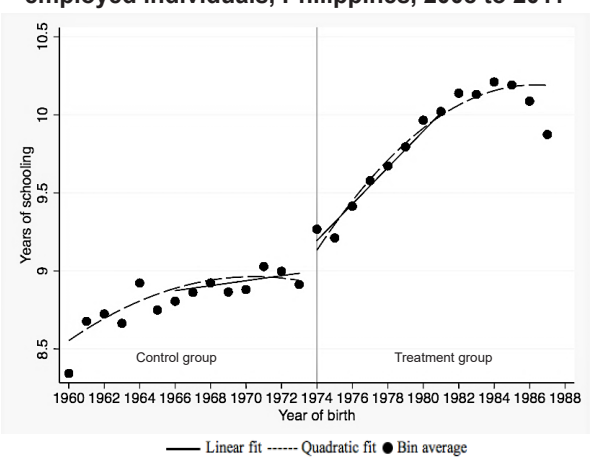
The goals of the study are, first, to investigate the relationship between the free and subsidized secondary education policies and years of schooling attainment, and second, to examine the causality between educational attainment and labor market outcomes (i.e., formal work income and informal work income). However, the main challenge in these kinds of analyses is the endogeneity of schooling. Essentially, this means that crucially important observed and unobserved factors (such as ability, IQ, parental characteristics, etc.) are captured in the error term and their impacts may confound those of years of schooling. This problem may lead to biased estimates of the regression coefficients.

To address this issue, we employ the regression discontinuity design (RDD), which is a quasi-experimental method that measures causal effects of interventions. The main intuition behind the RDD is that those observations near a certain cut-off or threshold are, on average, similar between control and treated groups in most respects (i.e., motivation, exposure to economic and environmental factors, etc.) and are made different solely because of the intervention. Hence, if outcomes exhibit a discontinuity at the cut-off, it might be reasonable to infer that this is due primarily to the intervention. In other words, outcomes would be continuous if it were not for the policy. One limitation of the RDD is that the effect is strictly evaluated only around the threshold. Nonetheless, this method has gained popularity in empirical research in recent decades.

In our estimation of the causal effect of an additional year of schooling, we take advantage of the timing of the free public secondary education and GASTPE reforms to conduct an RDD. Our running variable in this case is year of birth, which is considered exogenous and randomized (as an individual cannot choose or manipulate his or her year of birth).[1] We mention that in all our calculations, years of birth are normalized and re-centered at the discontinuity so that coefficients may be interpreted directly. As for our cut-off (c), we use the year of policy implementation (1989) as basis for selection. Children born in or after 1974 are aged 15 or younger in 1989 and are, thus, able to take advantage of the reforms (treated group). We note that in 1989, children at age 15 are normally at their fourth and last year in high school. Conversely, those born before 1974 are aged 16 or older in 1989 and are expected to have completed secondary school (control group). This older cohort has limited exposure to the policy.

Given our cut-off of 1974, we establish our control group as the cohort at the left-hand side of the cut-off that did not receive treatment and our treatment group as the cohort at the right-hand side that received treatment (Figure 1).

FIGURE 1. Fit of the first stage regression: Year of birth and education, employed individuals, Philippines, 2008 to 2011



Note: Figure drawn using data from the Annual Poverty Indicators Survey 2008 and 2011.

[1] We argue that there is low likelihood of manipulation of year of birth around the cut-off since the Philippine basic education system requires the birth certificate upon enrollment to Grade 1 to verify that the child enters primary school at the required age. In addition, data from the World Bank show that the percentage of primary school repeaters in the Philippines is lower than the world average. In 1989, the percentage of primary school repeaters among girls are 2.0 percent in the Philippines and 6.4 percent in the world, while the percentage among boys are 1.6 percent in the Philippines and 7.7 percent in the world. The same trend persists a decade later. These information ensure that individuals are less likely to move from one side of the cut-off to another [UNESCO Institute for Statistics 2020].

In hindsight, as individuals on the left-hand and right-hand side of the cut-off have similar features and their year of birth is exogenous, the implementation of RA 6655 and RA 6728 yields variations in highest grade completed across age groups, and that is as good as randomized [Lee 2008]. Our RDD hinges on this key assumption. In particular, we follow a fuzzy RDD. In a fuzzy RDD setting, treatment assignment does not mean that individuals actually got treated. This means that there may be some observations at the right-hand side of the cut-off that are untreated and some at the left-hand side that are treated.[2] For instance, if some individuals who are born before 1974 experience a delay in schooling, then they might have been able to avail themselves of the reforms. Likewise, there may be individuals born after 1974 who did not take advantage of the programs. Examples are those who chose to attend expensive private school despite the availability of free public school and subsidized private school. Thus, the probability of treatment jumps by less than 1 at the cut-off [Imbens and Lemieux 2008].

Fuzzy RDD provides a local average treatment effect (LATE), which means that the impact is estimated only for a group of compliers or subgroup of beneficiaries (i.e., in this case, those who avail the free and subsidized schooling upon its implementation). The formal model for the causal effect of an additional year of schooling in the fuzzy RDD is:

$$\tau_{FRD} = \frac{lim_{x\downarrow c} E\,[Y \mid Birthyear = x] - lim_{x\uparrow c} E\,[Y \mid Birthyear = x]}{lim_{x\downarrow c} E\,[Sch \mid Birthyear = x] - lim_{x\uparrow c} E\,[Sch \mid Birthyear = x]} \quad (1)$$

Equation 1 denotes that the causal impact, τ_{FRD}, is the ratio of (i) the difference in the outcome from a regression on the treatment-determining or running variable (year of birth) and (ii) the difference in the treatment (schooling) from a regression on the running variable. Both differences are estimated with respect to the cut-off [Keats 2018].

Hahn, Todd, and Van der Klaauw [2001] show that τ_{FRD} can be estimated using an instrumental variables (IV) approach (i.e., two-stage estimation). In our analysis, we employ the IV two-stage least squares (2SLS) to analyze the impact of schooling on income. The causal effect, τ_{FRD}, is equivalent to the estimator β_{FRD}, provided that the bandwidth and order of the polynomial are the same in both the first and second stages. Correspondingly, the set of equations in the IV 2SLS is:

$$Y = \alpha + \beta_{FRD}\,\widehat{Sch} + \varepsilon \quad (2)$$

$$Sch = \gamma + \delta Z + g\,(Birthyear - c) + v \quad (3)$$

Equation 3 represents the first stage, where *Sch* refers to years of schooling and *Z* is a dummy variable that takes the value of 1 if birthyear ≥ 1974 and 0 if

[2] This is in contrast to sharp RDD where probability of assignment jumps from 0 to 1 at the cut-off.

birthyear < 1974. Note that Z is exogenous and also serves as our instrumental variable. The regression parameter δ captures the impact of Z on *Sch*. We expect δ to be significant to satisfy the correlation condition in the 2SLS. This means that the effect of our IV (Z), which depends on our running variable (*Birthyear*), is only through the treatment variable (*Sch*). In Equation 2, our second stage, we specify Y as the outcome of interest (*income*) and $\widehat{Sch}$ as the predicted values of *Sch* from the first stage. $\widehat{Sch}$ is now independent of the error term (i.e., no longer endogenous) since we estimate it using an exogenous IV that is not correlated with the error term from the main equation. Importantly, our parameter of interest β_{FRD} represents the causal effect of an additional year of schooling on the outcome. Finally, $g(\cdot)$ denotes the polynomial function under consideration, given our cut-off year (c = 1974), and v and ε represent the error terms for the first and second stage.

Further, two critical aspects of the RDD approach are the choice of bandwidth (data window) and polynomial specification. Several methods may be undertaken to determine the optimal bandwidth h^* and the order of the polynomial. In our study, we use the Imbens and Kalyanaraman (IK) approach, which suggests that the optimal bandwidth should minimize the mean squared error [Imbens and Kalyanaraman 2012]. Based on this minimization criterion, IK derived a plug-in equation that estimates the optimal bandwidth in the fuzzy RDD setting. Calonico et al. [2017] note that the IK method works well in realistic settings. To estimate h^* and the coefficient on h^*, we utilize the *rdbwselect* and *rdrobust* commands in Stata which are based on an upgraded version of the IK bandwidth selection approach developed by Calonico et al. [2017].[3] This upgraded version takes into consideration some crucial aspects of our study, including adjustments in estimates and biases when adding covariates, execution time for large sample sizes, and bandwidth selection options for fuzzy RDD.

To determine the order of the polynomial, Lee and Lemieux [2010] emphasize that examining near the cut-off is better because this distance provides higher certainty that observations at the left-hand and right-hand side are similar, except for the exposure to the treatment. In this scenario, the left-hand side group better represents the counterfactual state of not having the treatment. If we estimate close to the cut-off, the number of polynomial terms needed for estimation decreases (i.e., local linear specification). Local linear regression is shown to have attractive properties and proven to be rate optimal[4] [Porter 2003].

In our analysis, we present results only from the optimal bandwidth. We mention, however, that the results from other nearby bandwidths, with linear and

[3] For the IV estimation, we conducted both direct *rdrobust* and manual IV 2SLS. The results across both methods, including the coefficients and significance of coefficients, were consistent. To organize our presentation for both stages, we opted to present the results from the manual IV 2SLS because *rdrobust* does not show the first stage results in detail. Conducting manual IV 2SLS in addition to *rdrobust* also serves as a robustness check.

[4] The optimal rate denotes that the bias is reduced to a level not worse than that commonly found in non-parametric conditional mean estimation [Porter 2003].

quadratic specifications, are generally similar to those of the optimal bandwidth. This verifies the consistency of our findings and contributes as robustness checks. These additional tables are available upon request from the authors.

As summarized by Keats [2018], the validity of fuzzy RDD estimates hinges on the following assumptions. First is the exogeneity of the treatment status. There should be no manipulation in the treatment status of individuals. Second is the smoothness assumption, which ensures that factors that may elicit effects on both educational attainment and outcomes vary smoothly across the threshold. Third, and last, is the significance of β_{FRD}. This means that the additional year of schooling solely and significantly affects the changes in outcomes. As our model addresses the endogeneity issues and tests of assumptions, our estimates could effectively deliver causal effects.

4.2. Data

4.2.1. Datasets

In our main analyses, we utilize the Annual Poverty Indicators Survey (APIS) conducted by the Philippine Statistics Authority (PSA). The APIS is a nationally representative survey that collects household-level and individual-level information on the socioeconomic characteristics and living conditions of Filipinos. The sampling design of the earlier APIS (i.e., 2008 to 2011) is based on the 2003 master sample for household surveys derived from the 2000 Census of Population (Census). It follows a three-stage scheme. The first and second stages are the selection of primary sampling units (PSUs) and sample enumeration areas (EAs). The PSUs and EAs are chosen with probability proportional to the number of households in the Census. The third and final stage is the selection of sample housing units using systematic sampling (Philippine Statistics Authority [2008, 2011, 2017]).

To increase our sample size and reduce noise, we pool data from both the APIS 2008 and 2011.[5] As estimates in the IV approach, while consistent, may be biased, a large sample size is crucial [Angrist and Krueger 2001]. In total, the APIS 2008 has 40,613 households and 190,171 individuals, while the APIS 2011 has 42,063 households and 193,097 individuals. We extract and calculate our individual-level variables, such as years of schooling, employment status, income, age, year of birth, region, location, and gender, from our pooled dataset.

Meanwhile, we choose available data on father's and mother's educational attainment and household location of residence (i.e., urban or rural) to test the smoothness assumption. As mentioned earlier, these selected variables should vary smoothly across the cut-off to ensure that the policies only affect individual

[5] APIS 2008 and 2011 were the full datasets available to us (authors) when the study was conceptualized in 2017. The datasets have a complete set of variables on employment and income, include geographical codes up to the barangay level, and have large sample size (>190,000 individuals) unlike other APIS rounds that we were able to observe at that time.

schooling (our endogenous variable of interest) and that schooling solely drives changes in income. If these other variables jump across the cut-off, then the policies or certain events in 1974 may have also affected other factors which can later contribute to changes in income. As the reforms primarily impact schooling attainment, parental characteristics and location should not vary significantly across the cut-off and, hence, should not affect our long-term outcomes. We again use the pooled APIS 2008 and 2011 to find the educational attainment of parents whose children are born within a given bandwidth. For location of residence, we use only APIS 2011 since APIS 2008 does not have data on urban residence. Lastly, one way to verify the significance of the jump across our cut-off is to find evidence of discontinuity in other datasets. In this case, we use the APIS 2017 (the latest dataset available as of writing).

4.2.2. Years of schooling calculation

To calculate the years of schooling of an individual, we follow a modified version of Barro and Lee's [1993] categorization. Table 1 presents the schooling categories and corresponding years of education and completed grade levels. We consider the economically active population (i.e., those who are 25 years old and above or those who are 24 years old and below but are no longer attending school) in our computations. We note that Filipino households commonly finish investments in children's schooling at age 24.[6]

TABLE 1. Educational attainment levels, Philippines

Category	Years of schooling	Highest grade completed
No schooling	0	No grade completed, Nursery, Kinder, Preparatory
Partial primary	1	Grade 1
	2	Grade 2
	3	Grade 3
	4	Grade 4
	5	Grade 5
Complete primary	6	Elementary graduate
Partial secondary	7	1st year high school
	8	2nd year high school
	9	3rd year high school

[6] Based on Section D1 of the APIS 2008 and 2011 questionnaire, the question on schooling status is asked only among children aged 3 to 24 years old. Hence, we are able to capture those who are no longer attending school in this age group. However, if an individual is 25 years old or older, no variable indicates his or her schooling status. Thus, we consider all individuals aged 25 and above and assume that most of them have completed their schooling and have entered the labor force.

TABLE 1. Educational attainment levels, Philippines (continued)

Category	Years of schooling	Highest grade completed
Complete secondary	10	High school graduate
Partial tertiary	11	1st year post-secondary; 1st year college
	12	2nd year post-secondary; 2nd year college
	13	Post-secondary graduate; 3rd year college
Complete tertiary	14	4th year college or higher
	15	With some units earned or enrolled in graduate school

Note: Adapted from Barro and Lee [1993].

4.2.3. Employment classification

Our employment classification is based on the work status of individuals in the last six months (i.e., January to June 2008 for APIS 2008 and January to June 2011 for APIS 2011). Broadly, an individual is employed if he or she reports working on a job or business in that given time period.

We then classify employment into formal or informal. The formal sector includes corporations and partnerships, cooperatives and foundations, single proprietorships with employment of ten and over, and single proprietorships with branches ["2010 annual survey of Philippine business and industry - construction sector : final results" 2013]. Based on the APIS questionnaire, formal sector workers are mostly those in private households, private establishments, and government offices or corporations. Likewise, they receive wage or salary on a regular basis along with social security provisions. In contrast, the informal sector includes household unincorporated enterprises, which may be informal own-account enterprises or enterprises of informal employers. These establishments do not hire employees on a permanent basis. They may also employ unpaid family members especially women and children ["Informal sector (operational definition)" n.d]. Based on the APIS questionnaire, informal sector workers are largely self-employed without any employee and employed in own family-operated farm or business.

4.2.4. Income calculation

We calculate per capita income based on an individual's type of employment and sources of income. For formal workers or wage earners, income is computed by adding *basic salaries and wages* and *allowances, honoraria, tips, etc.* For informal workers or non-wage earners, income is estimated by dividing *family's total entrepreneurial and other income* by the *number of non-wage earners in the family*. We impute a worker's non-wage income based on the *family's total entrepreneurial and other income* since these data are not available at the individual-level. On a final note, consistent with Mincer's earnings function, we use the logarithm (log) of income in the regressions to deal with outliers.

5. Results and Discussion

The following tables provide our estimates of program effects. We tackle this section by first analyzing the effect on schooling attainment using *t*-test and then evaluating its downstream effects on income across sectors and genders using RDD.

5.1. Effect on schooling

5.1.1. Schooling of members of the labor force

Here we present the effect of the policies on schooling of those in the labor force (employed and unemployed) using *t*-test (Table 2). The *t*-test is a hypothesis test that determines if the difference in means of two groups is statistically different from 0. We utilize the pooled APIS 2008 and 2011 for two sample bandwidths: bandwidth 8 (year of birth: 1966 to 1981) and bandwidth 12 (year of birth: 1962 to 1985). As a demographic overview, when we compare the control and treatment groups in these bandwidths, on average, the control group is 8 to 12 years older than the treatment group. In terms of gender distribution, the proportion of men equals the proportion of women (both at around 50 percent). Meanwhile, based on the *t*-test, the difference in average years of schooling between the control and treatment groups are significant at the one percent level. This means that the average years of schooling of the treated cohort, who benefited from the policy, is statistically higher compared to that of the untreated cohort, who were not exposed to the program. Specifically, for bandwidth 8, years of schooling of the treatment group is 9.61 years, while that of the control group is 8.93 years (0.68-year difference). For bandwidth 12, average years of schooling is 9.80 for the treated and 8.89 for the control (0.91-year difference).

Both women and men exhibit significant increase in years of schooling after policy intervention. In particular, for bandwidth 8, men in the control group have 8.61 years of schooling while those in the treatment group have 9.25 years. Similarly, women in the control group have 9.24 years of schooling while those in the treatment group have 9.98 years. In addition, for bandwidth 12, men in the control group have 8.58 years of schooling while those in the treatment group have 9.42 years. Meanwhile, women in the control group have 9.20 years of schooling while those in the treatment group have 10.19 years. The same rising trend in schooling attainment is true among women and men employed in the formal and informal sectors.

The results imply that the treatment group indeed benefited from the free and subsidized secondary schooling reforms. Decline in the relative price of schooling is the most important pathway through which the reform affect household investments in schooling. First, with the reform, the relative price of schooling declines vis-à-vis other goods (i.e., substitution effect) which lead to the substitution of schooling for other goods, leading to more "purchases of schooling" or higher investments in schooling [Tiongson 2005]. Second, the

decline in the price of schooling leads to increases in household purchasing power (i.e., income effect) that enables household to purchase more schooling. Since schooling is a normal good, the substitution effect and income effect are reinforcing which means that with the reform households will no doubt increase investments in schooling.

Moreover, we note that the average years of schooling for both bandwidths are still less than 10 years. This means that children leave school around their fourth year in high school or right after. They are not able to finish basic education nor proceed to university. This is problematic because individuals in developing countries should acquire more years of schooling to compensate for the low quality of education that they receive [Fasih 2008].

We emphasize that the passing of RA 6655 and RA 6728 substantially improved schooling attainment of recipients. However, the policies are apparently not enough to encourage the most disadvantaged students to stay in school. It appears that complementary programs that address the persisting demand and supply side issues in education should be in place.

TABLE 2. Effect of free and subsidized secondary schooling policies on schooling, by bandwidth, Philippines, 2008 to 2011

		Bandwidth					
		8 (Year of birth: 1966 to 1981)			12 (Year of birth: 1962 to 1985)		
		Control	Treatment	*t*-test	Control	Treatment	*t*-test
Panel A: Demography							
Age	*mean*	40	32	8***	42	30	12***
	sd	(2.70)	(2.75)	(0.02)	(3.74)	(3.77)	(0.02)
	N	37,863	37,448	75,311	54,980	57,047	112,027
Male	*mean*	0.50	0.50	0.00	0.50	0.51	-0.01**
	sd	(0.50)	(0.50)	(0.00)	(0.50)	(0.50)	(0.00)
	N	37,863	37,448	75,311	54,980	57,047	112,027
Panel B: Education							
Years of schooling	*mean*	8.93	9.61	-0.68***	8.89	9.80	-0.91***
	sd	(3.80)	(3.76)	(0.03)	(3.84)	(3.73)	(0.02)
	N	37,863	37,448	75,311	54,980	56,735	111,715
Years of schooling | men	*mean*	8.61	9.25	-0.63***	8.58	9.42	-0.84***
	sd	(3.79)	(3.78)	(0.04)	(3.81)	(3.75)	(0.03)
	N	18,900	18,678	37,578	27,420	28,653	56,073
Years of schooling | women	*mean*	9.24	9.98	-0.73***	9.20	10.19	-0.99***
	sd	(3.78)	(3.70)	(0.04)	(3.85)	(3.67)	(0.03)
	N	18,963	18,770	37,733	27,560	28,082	55,642

TABLE 2. Effect of free and subsidized secondary schooling policies on schooling, by bandwidth, Philippines, 2008 to 2011 (continued)

		Bandwidth					
		8 (Year of birth: 1966 to 1981)			12 (Year of birth: 1962 to 1985)		
		Control	Treatment	*t*-test	Control	Treatment	*t*-test
Years of schooling | formally employed	*mean*	9.57	10.23	-.54***	9.53	10.42	-.89***
	sd	(3.83)	(3.75)	(.05)	(3.88)	(3.70)	(.03)
	N	16,208	16,677	32,885	23,136	25,664	48,800
Years of schooling | informally employed	*mean*	8.14	8.68	-.54***	8.12	8.71	-.59***
	sd	(3.71)	(3.72)	(.05)	(3.75)	(3.71)	(.04)
	N	13,888	10,923	24,811	20,896	14,891	35,787
Years of schooling | men, formally employed	*mean*	9.06	9.64	-0.58***	9.04	9.76	-0.72***
	sd	(3.67)	(3.65)	(0.05)	(3.72)	(3.61)	(0.04)
	N	10,425	11,018	21,443	14,766	16,895	31,661
Years of schooling | women, formally employed	*mean*	10.48	11.38	-0.90***	10.41	11.70	-1.29***
	sd	(3.94)	(3.68)	(0.07)	(4.00)	(3.53)	(0.06)
	N	5,783	5,659	11,442	8,370	8,769	17,139
Years of schooling | men, informally employed	*mean*	7.86	8.26	-0.40***	7.83	8.33	-0.50***
	sd	(3.78)	(3.75)	(0.06)	(3.78)	(3.72)	(0.05)
	N	7,509	6,218	13,727	11,201	8,809	20,010
Years of schooling | women, informally employed	*mean*	8.47	9.24	-0.77***	8.45	9.26	-0.81***
	sd	(3.60)	(3.61)	(0.07)	(3.69)	(3.62)	(0.06)
	N	6,379	4,705	11,084	9,695	6,082	15,777

Note: Datasets used are the Annual Poverty Indicators Survey 2008 and 2011. Standard deviations in parentheses. ***, **, and * indicate significance at the one, five, and ten percent levels, respectively.

5.1.2. Schooling of employed individuals

As part of our RDD, we run the first stage regression which shows the effect of the policies on the schooling of employed individuals only. The estimates show that beneficiaries of the policies attain significantly higher years of schooling compared to non-beneficiaries (Table 3). Specifically, Panel A of Table 3 shows that among the employed, within the optimal bandwidth, exposure to the program leads to about 0.162 to 0.230 more years of schooling. The outcomes are statistically significant at the one and five percent significance level and are consistent with the earlier *t*-test. Graphically, the jump in years of schooling is evident at the 1974 cut-off year (Figure 1).

To explore the variation of the effect on schooling, we disaggregated our sample by sector and gender. Interestingly, the policies have no significant effect on the schooling of formally employed individuals (Panel B, Table 3) but have significant impact on the schooling of the informally employed sample (Panel C, Table 3).

To further break this down, we find statistically significant evidence that men who are currently employed in the formal sector have benefited more in the programs with 0.259 to 0.309 more years of education based on the optimal bandwidth (Panel D, Table 3). On the contrary, the policies appear to have exerted no significant effect on the schooling of formally employed women (Panel E, Table 3).

The results in the informal sector are quite different from those found in the formal sector. Panel F of Table 3 indicates that the policies did not exert significant impact on schooling of currently employed men in the informal sector. In contrast, Panel G of Table 3 reveals that the policies have exerted a statistically significant rise in schooling attainment of women workers in the informal sector. The increase is anywhere between 0.586 and 0.694 years of schooling based on the optimal range.

Based on these findings, different subgroups of women and men respond differently to changes in the price of education (i.e., different price elasticity of demand). We highlight that in the formal sector, employed men appear to have benefited more from the free and subsidized education than employed women, while the opposite is true in the informal sector.

TABLE 3. First stage estimates (IV 2SLS): Effect of free and subsidized secondary schooling policies on schooling, Philippines, 2008 to 2011

Dependent variable: Years of schooling	*Panel A Sample: Employed in the formal and informal sector*		*Panel B Sample: Employed in the formal sector*	
Z	0.230***	0.162**	0.18	0.073
	(0.056)	(0.065)	(0.131)	(0.11)
Optimal Bandwidth	13	9	5	6
With controls	No	Yes	No	Yes
R-squared	0.019	0.111	0.004	0.095
No. of observations	91,173	64,530	20,478	24,714
	Panel C Sample: Employed in the informal sector		*Panel D Sample: Employed men in the formal sector*	
Z	0.435***	0.352***	0.309**	0.259*
	(0.11)	(0.118)	(0.143)	(0.141)
Optimal Bandwidth	7	6	5	5
With controls	No	Yes	No	Yes
R-squared	0.005	0.12	0.003	0.062
No. of observations	21,724	18,834	13,402	13,402

TABLE 3. First stage estimates (IV 2SLS) (continued)

Dependent variable: Years of schooling	*Panel E Sample: Employed women in the formal sector*		*Panel F Sample: Employed men in the informal sector*	
Z	-0.149 (0.217)	-0.226 (0.199)	0.247 (0.16)	0.184 (0.165)
Optimal Bandwidth	5	5	7	6
With controls	No	Yes	No	Yes
R-squared	0.009	0.06	0.003	0.127
No. of observations	7,076	7,076	11,989	10,361
	Panel G Sample: Employed women in the informal sector			
Z	0.694*** (0.205)	0.586*** (0.192)		
Optimal Bandwidth	5	5		
With controls	No	Yes		
R-squared	0.01	0.097		
No. of observations	7,077	7,077		

Note: *Z* is a dummy variable that takes the value of 1 if birth year ≥ 1974 and 0 if birth year < 1974. Years of birth were normalized based on the cut-off (1974). Linear specification includes year of birth and *Z* × year of birth. Datasets used are the Annual Poverty Indicators Survey 2008 and 2011. Control variables include male and regional dummies. Standard errors in parentheses are clustered at the year of birth level. ***, **, and * indicate significance at the one, five, and ten percent levels, respectively.

5.2. Effect on income

We analyze the returns to schooling of employed individuals using fuzzy RDD. While returns to education has been widely studied since the late 1950s, most analyses focus on high-income countries and only few on developing economies [Peet, Fink, and Fawzi 2015]. The few earlier estimations argue that workers in developing countries receive higher returns to education than those in more developed countries (Card [2001]; Duflo [2001]). Also, previous studies mention that estimated returns using the IV approach are commonly higher than those using OLS (Card [1999]; Sakellariou [2006]). Here we provide additional empirical evidence on returns to schooling in a developing country.

Based on our optimal bandwidth, an additional year of schooling increases income by about 17.2 to 23.0 percent, with high level of statistical significance (Panel A, Table 4). Thus, in the Philippines, higher levels of education, brought forth by secondary schooling policies, yield significantly higher income. Consistent with previous research, it is still economically wise to obtain more years of education. The results also imply that the benefits of free and subsidized schooling can only be realized if the individual is employed because labor income is by far the most important source of individual income.

TABLE 4. Second stage estimates (IV 2SLS): Effect of schooling on income, Philippines, 2008 to 2011

Dependent variable: log of Income	*Panel A Sample: Employed in the formal and informal sector*		*Panel B Sample:Employed in the formal sector*	
Years of schooling	0.172*** (0.052)	0.230** (0.095)	0.572 (0.352)	0.858 (1.131)
Optimal Bandwidth	13	9	5	6
With controls	No	Yes	No	Yes
No. of observations	91,172	64,529	20,478	24,714
	Panel C Sample: Employed in the informal sector		*Panel D Sample: Employed men in the formal sector*	
Years of schooling	0.104** (0.053)	0.120* (0.071)	0.361** (0.145)	0.367** (0.177)
Optimal Bandwidth	7	6	5	5
With controls	No	Yes	No	Yes
No. of observations	21,724	18,834	13,402	13,402
Dependent variable: Years of schooling	*Panel E Sample: Employed women in the formal sector*		*Panel F Sample: Employed men in the informal sector*	
Years of schooling	-0.246 (0.708)	-0.091 (0.303)	0.04 (0.132)	0.014 (0.191)
Optimal Bandwidth	5	5	7	6
With controls	No	Yes	No	Yes
No. of observations	7,076	7,076	11,989	10,361
	Panel G Sample: Employed women in the informal sector			
Years of schooling	0.158** (0.064)	0.170** (0.073)		
Optimal Bandwidth	5	5		
With controls	No	Yes		
No. of observations	7,077	7,077		

Note: *Z* is a dummy variable that takes the value of 1 if birth year ≥ 1974 and 0 if birth year < 1974. Years of birth were normalized based on the cut-off (1974). Linear specification includes year of birth and *Z* × year of birth. Datasets used are the Annual Poverty Indicators Survey 2008 and 2011. Control variables include male and regional dummies. Standard errors in parentheses are clustered at the year of birth level. ***, **, and * indicate significance at the one, five, and ten percent levels, respectively.

5.2.1. Effect on income by sector

For an in-depth analysis, we explore where the change in income comes from by dividing our employed sample into formally and informally employed workers by gender.

An additional year of schooling has no significant effect on income of formal wage earners (Panel B, Table 4) but has significant effect on income of informal wage earners (Panel C, Table 4). In particular, an additional year of education increases informal income significantly by 10.4 to 12.0 percent based on the optimal bandwidth. This may indicate that the contraction of the formal sector and low labor demand between 2008 and 2011 forced individuals to venture into the informal sector, even those who are more educated. As data from the Asian Development Bank (ADB) and International Labor Organization (ILO) [2011] reveal, employment in the formal sector contracted while employment in the informal sector, particularly for women, grew during the crisis.

The differences in returns to schooling of individuals in the two sectors are consistent with previous literature which show that the benefits of schooling may be distributed unequally [Fasih 2008]. In the case of the Philippines, the contraction of the aggregate economy during the Asian economic crisis makes the informal job market buoyant, making returns to schooling disproportionately increase in the sector. In the next subsection, we explain the variation in our estimates by investigating the returns to schooling across genders in each sector.

5.2.2. Effect on income by sector and gender

Based on Table 2, informally employed men have the lowest average years of schooling both before and after policy implementation (i.e., 7.83 and 8.33 years for bandwidth 12). In contrast, formally employed women have the highest grades of schooling at 10.41 years before the reforms and 11.70 years after. The educational attainment of informally employed women are almost comparable to that of formally employed men. These may indicate that the formal labor market could be less accommodating to women, even to the more educated ones, partly because of the cost associated with maternal leave, which is most likely borne by formal enterprises. Women may choose to work in the informal sector because they prefer jobs that are flexible which allow them to perform their task in home production. Indeed, in terms of employment, based on the LFS (2007 to 2011), the proportion of men working in the formal sector is consistently way higher than the proportion of women working in the formal sector ("TABLE 3.10 - Employed Persons by Class of Worker and Sex, Philippines: 2007 – 2011" [n.d]). We apply the same RDD strategy in our succeeding analyses of the causal effect of schooling on income gained by men and women in formal and informal occupations.

5.2.2.1. Effect on income in the formal sector by gender

The second stage estimates reveal that an additional year of schooling causes a 36.1 to 36.7 percent increase in the income of men employed in the formal sector (Panel D, Table 4). Moreover, there is no significant relationship between women's schooling and income in this case (Panel E, Table 4). These results

suggest that men in the formal sector appear to have benefited more from the policy by gaining more years of schooling and receiving higher earnings. These may also be reflective of how women in the formal sector are at a disadvantage vis-à-vis men during this period. According to the ADB and ILO [2011], women in the Philippines suffered disproportionately during the global financial crisis for two reasons. First is that women have limited employment opportunities as gender disparities in employment and income have been observed even before the crisis began. Second, female-dominated industries, such as garments, electronics, and export processing zones, were hit the hardest by economic shocks. Most lay-offs occurred in these female-dominated industries. Regardless of schooling policies and educational attainment, if the formal labor market favors men over women, gender inequities in employment and income will continue to persist.

5.2.2.2. Effect on income in the informal sector by gender

The findings in the informal sector are quite the opposite of those found in the formal sector. Based on the optimal bandwidth in the second stage, there is no significant relationship between male schooling and income (Panel F, Table 4). Meanwhile, among women in informal occupations, an additional year of schooling significantly increases their income by about 15.8 to 17 percent (Panel G, Table 4).

These results may imply that the informal sector absorbed even the highly educated women who cannot be accommodated in the formal sector during aggregate economic contraction. This influx of women into informal work is consistent with the findings of ADB and ILO [2011] that when the crisis hit, women strongly felt the burden of meeting immediate family needs (i.e., food, water, and healthcare). Thus, they opted to engage in informal activities, either through self-employment, home-based work, small businesses, or other sidelines, to compensate for income loss and ensure family survival.

5.3. Gender segregation in employment

Wrapping up our discussion on the gender differences in income, we mention the common observation that women tend to settle for low-productivity, low-paying, or informal jobs and are not commonly promoted to higher positions in formal jobs even though Filipino women obtained more years of schooling than their male counterparts. According to the World Bank [2012], this phenomenon may be explained by two factors, namely care responsibilities and market failures.

First, women's productivity and earnings are affected by their household responsibilities and time allocation across activities. Gendered norms and traditions dictate that women should spend significantly more time in housework and care responsibilities than men. Thus, women are more likely to choose jobs with flexible working arrangements (i.e., part-time, informal, or casual work), which in turn offer lower wages. Men also generally spend more time in market

work than women. While the presence of small children in the family increases the amount of care by both men and women, women still clock in more hours than men. Thus, the high fixed costs of market work (i.e., fixed schedules and minimum required hours), particularly formal work, remain a burden for women, causing them to choose jobs that offer flexible schedules. In some cases, women may have to leave work completely.

In the Philippines, the expansion in employment opportunities, as a result of economic growth, has not been inclusive for women. Time-use patterns in domestic, care, and market work reveal gender-based work gaps. In domestic and care work, gendered norms lead women to spend more time in domestic and care work compared to men. Relatedly, in terms of market work, there is a low percentage of Filipino women in formal wage, non-agricultural employment. A higher percentage of women are engaged in vulnerable employment, which includes own account work and unpaid work in family businesses, relative to men [ADB 2013].

Second, market and institutional failures also affect women's choice of employment and employers' ability to assess women's skills and capacities. Market failures in labor market information, for instance, affect women's participation in formal sector jobs and employers' decision-making. Since women have low presence in certain job sectors, employers are not fully informed of their knowledge and skills. This means that employers are not aware of their potential work performance and will, thus, find it difficult to hire and promote more women.

Moreover, institutional failures in terms of infrastructure, especially transportation, lower women's access to economic opportunities due to longer travel time to work and decreased mobility. Poor women, who often reside in remote villages, settle for low-productivity and informal jobs due to the difficulty of traveling to cities or urban areas where better work opportunities are available. Similarly, if a woman is of reproductive age, she may find it difficult to apply for a formal sector job since the costs of maternal leaves may be borne as additional expenses by the company [World Bank 2012].

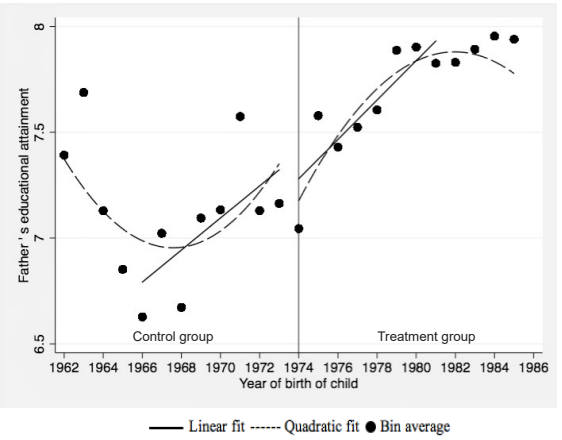
Based on data from the Philippines' LFS, the proportion of women in the informal sector rose from 39 percent in 2007 to 41 percent in 2011, while that of men in the same sector slid from 61 to 59 percent ["Table 3.10 - Employed Persons by Class of Worker and Sex, Philippines: 2007 – 2011" n.d.]. As mentioned earlier, these informal jobs are usually vulnerable and short-term. Workers do not receive social protection and are constantly at risk of being laid off as economic downturns occur. They likewise need to compete largely with new entrants who have been retrenched from the formal sector [ADB and ILO 2011]. Hence, our results also shed light on the need to protect workers in the informal economy, many of whom are women who strive to earn extra income for their families.

5.4. Smoothness assumption and evidence of discontinuity in other datasets

We validate our RDD by testing the smoothness assumption and finding similar discontinuity in other available datasets. For the smoothness assumption, we show that respondents' fathers' and mothers' education do not jump significantly across the cut-off (Figures 2 and 3). We similarly find no signs of discontinuity in individuals' urban/rural location (Figure 4). Since these variables vary smoothly across the cut-off, we verify that the policies only affect schooling and that schooling, in turn, primarily drives changes in our outcomes.

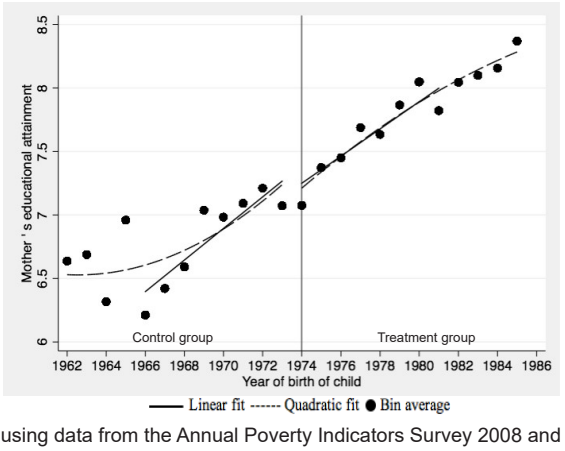
To provide supportive evidence of discontinuity in education, we use APIS 2017 and find that there is also a significant jump in schooling attainment at the cut-off (Figure 5). This means that the treatment group in this dataset also benefited from the policies.

FIGURE 2. Year of birth of child and father's education, Philippines, 2008 to 2011



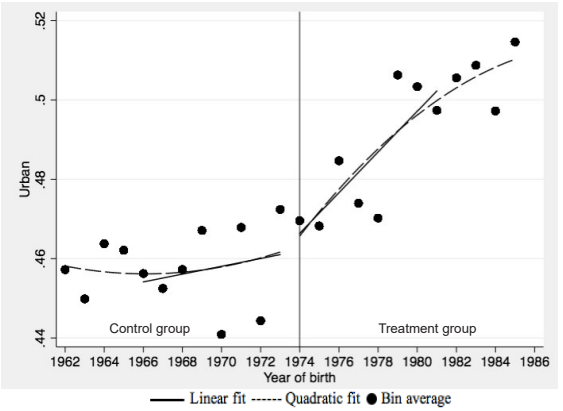
Note: Figure drawn using data from the Annual Poverty Indicators Survey 2008 and 2011.

FIGURE 3. Year of birth of child and mother's education, Philippines, 2008 to 2011



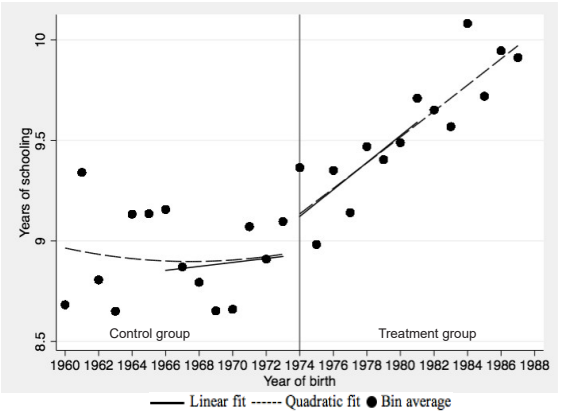
Note: Figure drawn using data from the Annual Poverty Indicators Survey 2008 and 2011.

FIGURE 4. Year of birth and urbanity, Philippines, 2008 to 2011



Note: Figure drawn using data from the Annual Poverty Indicators Survey 2011.

FIGURE 5. Fit of the first stage regression: Year of birth and education, Philippines, 2017



Note: Figure drawn using data from the Annual Poverty Indicators Survey 2017.

6. Conclusion

Schooling attainment remains low in developing countries and many programs were implemented to solve this problem. Free public and subsidized private secondary schooling has been implemented in the Philippines in 1988 and 1989. Through RA 6655, the government eliminated tuition and other school fees in public high schools. Similarly, through RA 6728, it provided tuition fee supplements and textbook funds to private high school students.

In this study, we conducted a rigorous assessment on the long-run impact of these policies on schooling attainment and income using an RDD. Briefly, this study found that the policies have significant positive impacts on schooling attainment and income.

First, beneficiaries of the policies attained significantly more years of schooling than non-beneficiaries. For instance, among members of the labor force, the treatment group gained 0.68 more years of schooling than the control group in bandwidth 8 (9.61 vs. 8.93 average years of schooling), indicating the effectiveness of the programs in enhancing schooling attainment. This rise in years of schooling after policy implementation is true for both women and men. In addition, the first stage estimates among employed individuals indicate an average increase of 0.162 to 0.230 years of education.

Second, in general, schooling significantly increases income. We noticed that an additional year of schooling increases income by about 17.2 to 23.0 percent among our sample of employed individuals after policy implementation. When we divided our sample of workers based on sector and gender, the results are quite different. We found that the policy had a significant impact on schooling attainment and returns to education of women in the informal sector and of men in the formal sector. Specifically, an additional year of schooling causally increases income of informally employed women and formally employed men by 15.8 to 17 percent and 36.1 to 36.7 percent, respectively. These results are observed although women employed in the informal sector attained a higher increase in schooling than men employed in the formal sector (based on the first stage estimates). This may imply that there are culture-driven gender roles and labor market imperfections that put women at a disadvantage when entering formal work.

The study points to the need for approaches that protect and support women workers (i.e., gender employment quotas in the formal sector, social protection provisions in the informal sector, flexible work schedule options, and provision of day care programs). It also emphasizes the importance of enhancing schooling reforms as they are deemed effective in making education more accessible for all.

To sum up, our results contribute to the limited literature on the long-term impact of basic education policies in developing countries. We conclude that the reforms encouraged school participation. Yet their downstream impact on earnings tend to vary across sectors and genders.

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