

DISCUSSION PAPER SERIES

IZA DP No. 16037

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Resulting from an Extra Year of Primary  
School: Evidence from Egypt**

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## ABSTRACT

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# Wage Returns to Human Capital Resulting from an Extra Year of Primary School: Evidence from Egypt\*

In this paper, we examine the wage returns to an extra year of primary school using a policy reform in Egypt, which *reduced* compulsory primary schooling from 6 to 5 years. Since this policy changed the duration of primary school while providing the same diploma, we can estimate the human capital effects holding the sheepskin effects constant. We find that the wage returns to an extra year of primary school for Egyptian men aged 24–44 is a statistically insignificant 2–4 percent. Despite the low returns for the overall population, the returns are much higher for men born in rural areas and men whose fathers have low levels of education—indicating important human capital effects for underprivileged boys. Consistent with this result, we find that the policy effects of a one-year reduction in primary schooling on schooling attainment at various levels are more adverse for underprivileged boys. Our findings, therefore, suggest that such a policy could be particularly detrimental for students from lower socioeconomic groups—contributing to increased inequality.

**JEL Classification:** J18, J31, I21, I28

**Keywords:** returns to schooling, early human capital investment, instrumental variables, compulsory education duration, Egypt

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## 1. Introduction

The causal relationship between schooling and wages is one of the most studied topics in labor economics. Establishing this causal relationship is challenging due to the presence of unobserved factors such as ability and motivation that can confound the effect of schooling on wages. Several studies in the literature use institutional features of schooling systems as a source of exogenous variation in years of schooling to estimate the returns to schooling consistently.<sup>1</sup> Most of the existing research is for developed country contexts and estimates returns at grade levels that correspond to the end of secondary school. However, early environmental conditions play an important role in the evolution of cognitive and noncognitive skills, and ability gaps across socioeconomic groups open up at early ages (e.g., Cunha and Heckman, 2007; Knudsen et al., 2006; Cunha et al., 2006). Therefore, it is important to assess the role of human capital acquisition during early stages of education on adult outcomes and understand how these effects vary by socioeconomic background. In this paper, we study wage returns to an extra year of schooling at the primary level and estimate differences in returns by students' socioeconomic background. For this purpose, we use a unique compulsory schooling policy in the Egyptian context that led to an exogenous variation in years of schooling.

The compulsory schooling policy we study is unusual in that it reduced the duration of primary schooling, unlike other contexts where reforms usually lead to an expansion of compulsory school at later stages of education. The law, which was passed in 1988, resulted in a decrease in the duration of compulsory primary schooling from 6 to 5 years. Following the new policy, an abridged version of the curriculum of grades 4 and 5 were given in grade 4 and the curriculum of (former) grade 6 was given in grade 5. This change resulted in a decline in the amount of material taught to students without affecting the degree received upon the completion of primary school. In this context, the human capital model predicts a decline in wages due to the reduction in schooling while the signaling model predicts no change since the degree received remains the same. Hence,

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<sup>1</sup> Angrist and Kruger (1991), in their pioneering study, use compulsory schooling laws; as do Acemoglu and Angrist (2000), Devereux and Hart (2010), Harmon and Walker (1995), Oreopoulos (2006), Lang and Kropp (1986), Spohr (2003), and Stephens and Yang (2014). Card (1995) use proximity to college whereas Kane and Rouse (1995) exploit the variation in tuition fees.

this provides us a setting where estimated wage effects are more likely to reflect the productivity enhancing role of schooling rather than the signaling role.

We estimate the effect of schooling on wages using the 2012 round of the Egypt Labor Market Panel Survey (ELMPS), which is nationally representative. For estimation, we use a regression discontinuity design (RDD), in which we compare the birth cohorts affected and unaffected by the policy—using month-year of birth as the running variable. Our setting also provides an important advantage in the generalization of our findings to the total population of Egypt. Since the set of compliers with the policy forms almost 85 percent of our sample, our LATE estimate is much closer to the ATE estimate than that typically reported in the literature about returns to schooling.

Our analysis of the policy effect on school outcomes indicates a substantial reduction in schooling. We find that the policy reduces years of schooling by about 0.9 years for men and about 0.7 to 0.9 years for women. We find no policy effect on primary, lower secondary (called preparatory in Egypt), and upper secondary school completion. There is also no policy effect on participation in wage employment, which indicates that there are no compositional effects on the sample of wage earners used for the estimation of school returns. Due to the low rate of wage employment among women (15.1 percent), we carry out our returns to schooling analysis only for men. Our results show that the returns to schooling for men aged 24–44 in Egypt is about 2–4 percent. Since there are no sheepskin effects of the policy, these effects reflect the human capital effect of an extra year of schooling in primary school. Although the estimated average effects are small, we find much larger effects for men from underprivileged backgrounds, in particular for men born in rural areas and men whose fathers have low levels of education. For example, we find that the wages of men born in rural areas increase by 9–12 percent due to an extra year of primary school. In accordance with this finding, we also find that the policy had adverse effects on underprivileged boys' schooling attainment at various levels.<sup>2</sup>

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<sup>2</sup> Using a different compulsory schooling policy, also in Egypt, that increased the duration of compulsory schooling from five to eight years, Elsayed (2019) reports an increase in years of completed schooling. Moreover, he finds an improvement in the reading and self-reported writing skills of male individuals---which is consistent with our finding of important human capital effects of extra years of schooling for underprivileged boys. In contrast, Elsayed and Marie (2021) find that the policy that reduced the duration of primary school from six to five years, which we also use in our study, increased girls' completion rate of the next schooling level.

Our paper has three main contributions to the literature. The first is to the literature on causal returns to schooling. To overcome the endogeneity problem in schooling, several studies use institutional features of schooling systems as a source of exogenous variation in the duration of schooling. Most of these studies are in developed country contexts—presumably because changes in schooling systems that really matter for schooling outcomes are rarer in developing countries, due to a lack of enforcement of compulsory schooling laws. Our study adds to the growing evidence from developing country contexts. Moreover, Egypt is poorer than most of the other countries for which the causal effect of years of schooling on wages has been estimated (see e.g. Aromolaran (2007) for Nigeria; Duflo (2001) for Indonesia; Fang et al. (2012), La (2014), Eble and Hu (2019), and Chen et al. (2020) for China; Aydemir and Kirdar (2017) for Turkey; Bravo (2019) for Mexico).<sup>3</sup> While there is variation in the reported estimates of returns to schooling, estimates generally suggest low wage returns to schooling in developing country contexts.<sup>4</sup> Our imprecisely estimated estimate of 2–4 percent return for men corroborates the evidence provided in developing-country contexts, as well as the recent studies in developed-country contexts,<sup>5</sup> that returns to schooling are either small or zero.

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<sup>3</sup> Duflo (2001) uses the massive school construction program in Indonesia in the 1970s to estimate the returns to schooling. In the Chinese context, Fang et al. (2012) and La (2014) use the 1986 policy change that mandated 9 years of schooling. Using an earlier policy change in China in 1978, Eble and Hu (2019) use the extension of primary school from five to six years and Chen et al. (2020) use the extension of middle school from two to three years. Aydemir and Kirdar (2017) use the increase in the duration of compulsory schooling from 5 to 8 years in Turkey. Bravo (2019) use the 1993 educational reform in Mexico that increased minimum school-leaving age from 12 to 15 while Aromolaran (2007) uses the introduction of a free universal basic education program in Nigeria in 1999.

<sup>4</sup> Duflo (2001) estimates returns that range between 6.8-10.6% in Indonesia. In the Chinese context reported estimates range between zero percent to 20 percent. Fang et al. (2012) find a 20 percent return while La (2014) finds no statistically significant effect using the same policy change. Eble and Hu (2019) find that the policy increases years of primary school by about 0.5 years in urban areas and raises average monthly income by 2.6%, implying a rate of return of about 5% while Chen et al. (2020) estimates a 12.7% return. In the Turkish context Aydemir and Kirdar (2017) an imprecisely estimated 2–2.5% return for an extra year of schooling for men. Bravo (2019) finds no statistically significant effect on wages in the Mexican content while Aromolaran (2007) finds estimates for both men and women that are small at primary and secondary levels, 2–3% and 4%, respectively in Nigeria.

<sup>5</sup> Black, Devereux and Salvanes, 2005; Devereux and Hart, 2010; Grenet, 2013; Meghir and Palme, 2005; Pischke and von Wachter, 2008; Stephens and Yang, 2014.

The setting in our paper provides a critical methodological advantage in the estimation of returns to schooling. The studies that use instrumental variables for this purpose, as we do, estimate the local average treatment effect (LATE), which is the effect of schooling on earnings for a subset of the population called *compliers*, who may not be representative of the whole population.<sup>6</sup> Therefore, the LATE might be substantially different from the average treatment effect (ATE),<sup>7</sup> particularly when compliers form a small subset of the population—which is in fact the case in most empirical studies. The exceptions include Oreopoulos (2006) and Aydemir and Kirdar (2017). These studies use compulsory schooling law changes in the UK and Turkey, respectively, where almost half of the population is affected. Hence, their LATE comes closer to the ATE. In this paper, we go a step further in bringing the LATE and ATE together—as more than 85 percent of the population alter their behavior due to the policy.

The second contribution of this paper is to the literature that studies the role of early years in children’s life cycle on the evolution of cognitive and noncognitive skills and later life outcomes. Numerous studies consider the roles of early environmental conditions, early childhood education, and preschool intervention programs.<sup>8</sup> However, there is little evidence on the effect of primary school on later school and adult outcomes. In contexts where preschool programs are not widely available, which is the case in many developing countries, primary schools are likely to constitute the first phase of education where skill development in a formal setting occurs. An assessment of the importance of early school years on adult outcomes is not feasible in the existing studies, in which individuals are generally induced to complete more years of schooling at grade levels that

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<sup>6</sup> Since compliers are would-be-drop-outs in the context of a compulsory schooling policy, they are likely to be different in terms of their preferences and abilities. Eckstein and Wolpin (1999) estimate that high school drop-outs in the US have in fact lower ability and/or motivation, have a comparative advantage in jobs that are done by non-graduates, and place a higher value on leisure and have a lower consumption value of school attendance.

<sup>7</sup> Imbens and Angrist (1994) note that if there are nonlinearities in the return to schooling, estimating the return via a policy-induced increase at a given schooling level may lead to differences between the estimated LATE and the ATE.

<sup>8</sup> See Cunha and Heckman (2007), Knudsen et al. (2006), Bleakley (2010) for the role of role of early environmental conditions and Elango et al. (2016) for a review the evidence on early childhood programs. For a review of preschool intervention programs see Currie (2001) and Currie and Blau (2006).

correspond to the end of secondary school.<sup>9</sup> In contrast, the policy in our study creates an exogenous change in years of schooling during primary school, thus allowing for an analysis of impacts at an earlier stage.

The literature on early environmental conditions also provides evidence that ability gaps across socioeconomic groups open up at early ages (e.g., Cunha et al., 2006) and the productivity of educational investments is especially high among young and disadvantaged children (Heckman, 2007). Therefore, changing the duration of schooling at primary school level may have differential effects on individuals from different socioeconomic backgrounds. Our analysis of returns to schooling by socio-economic characteristics, such as parental education and urban/rural area of birth, adds to the slim literature in this context. Our findings indicate much higher returns for underprivileged groups despite low average returns,<sup>10</sup> which highlights the importance of improving the educational outcomes of underprivileged groups in a developing country like Egypt, using targeted interventions.

The third contribution of the paper is to the literature on the effect of instructional time on student performance. Most studies on this topic find a positive impact of instructional time on school effectiveness. For instance, Aguero and Beleche (2013) find that a longer school year improved student performance in Mexico.<sup>11</sup> The evidence regarding the impact on long-term labor market outcomes is more mixed. Pischke (2007) finds no effect of the length of the school year on earnings or employment in Germany, although he finds an effect on schooling outcomes. On the other hand,

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<sup>9</sup> In the British context the minimum school leaving age increased from 14 to 15 with the 1947 law change, further increasing it to 16 in 1973. In the German context changes in compulsory schooling laws in West German states affected secondary schools increasing compulsory schooling from 8 to 9 years. In the US, the mean of the minimum school-leaving age across states were 15.3 in 1914 and increased to 16.2 by 1964 (Acemoglu and Angrist, 2000). Similarly, compulsory school law changes in developing country contexts targeted secondary schooling level.

<sup>10</sup> Several studies report higher returns to schooling for underprivileged children. For instance, Clay et al. (2016) find higher returns to schooling for white men at the bottom of the distribution in the US, and Meghir and Palme (2005) report that a compulsory education reform in Sweden raised the schooling and labor market earnings of individuals with unskilled fathers. In addition, Balestra and Backes-Gellner (2017) find higher returns to schooling at lower quantiles of the wage distribution. Altonji and Dunn (1996), on the other hand, find mixed results regarding the interaction between mother's education and return to education in the US.

<sup>11</sup> Dobbie and Fryer (2013) and Lavy (2015) also find that instructional time increases school effectiveness.

Parinduri (2014) finds that a longer school year increases both the probability of working in the formal sector and wages in Indonesia. Eble and Hu (2019) find a positive effect of an additional school year in primary school on monthly income in China. Similarly, we find that the duration of primary schooling has a larger effect on future wages for students from lower socioeconomic backgrounds. Some developing countries—needing to accommodate a large student body with limited classroom and teacher capacity—might want to reduce the duration of compulsory schooling. However, our findings from Egypt show that such a policy is particularly detrimental for students from lower socioeconomic groups—contributing to increased inequality.

The remaining part of this study is organized as follows. We provide an overview of the education system in Egypt in Section 2. Section 3 presents the data used in the analysis and Section 4 discusses the identification strategy and estimation. Results are provided in Section 5. Section 6 concludes.

## 2. Education System in Egypt and the New Policy

Before the 1988–89 school year, the formal schooling system in Egypt was built on 6+3+3 system, which meant six years of primary schooling, three years of lower secondary (*preparatory*) schooling, and three years of upper secondary schooling. The first 9 years of this system, which is termed basic schooling in Egypt, was compulsory. Law No. 233 of 1988, which is the basis for the identification used in this study, changed the duration of compulsory basic education starting from the 1988-89 school year from nine to eight years such that ‘the primary stage’ became five-years long while the ‘the preparatory stage’ remained three-years long.<sup>12</sup>

This law was implemented in the following way. The curriculum of the first three grades in the primary stage did not change. However, the curriculum of the fourth grade was changed to also include an abridged version of the fifth grade curriculum, and the fifth grade curriculum was changed to the pre-existing sixth grade curriculum. Essentially, grade 6 was recoded to grade 5, grades 5 and 4 were recoded to grade 4, and the first three grades did not change. It is important to

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<sup>12</sup> This decision was reversed in 1999 increasing the duration of primary schooling back to six years and compulsory basic schooling to nine years.

note that the duration of the school year, the duration of the school day, the curricula and the textbooks used in the primary as well as the preparatory stage remained essentially the same.<sup>13</sup>

The policy was first implemented in the 1988–89 school year and affected students starting the fifth or earlier grades in this school year. Accordingly, the duration of schooling of students who started grade six in this school year and earlier cohorts of students—the 1977 and earlier birth cohorts—did not change, whereas the duration of schooling of the 1978 and later birth cohorts was curtailed by one year. It is also important to note the following fact about this policy. Students who started the sixth grade in the 1988–89 school year—the 1977 birth cohort—took the sixth grade curriculum under the old system and started the preparatory stage in the 1989–90 school year. Students who started the fifth grade in the 1988–89 school year—the 1978 birth cohort—took the sixth grade curriculum according to the new policy and also started the preparatory stage in the 1989–90 school year—with the earlier birth cohort. In other words, two birth cohorts started the preparatory stage at the same time—which we call the double batch. Obviously, this could have adverse effects on the school success of students in the double batch, which we omit from the analysis as discussed further below.

In essence, while the 1978 and later birth cohorts are similar in the way that they face an abridged curriculum and five years of primary schooling, the 1977 and earlier birth cohorts had six years of primary schooling. At the same time, the two birth cohorts around the cutoff—the 1977 and 1978 birth cohorts—are similar in the way that they suffer from being in the double batch. None of the other birth cohorts face the same problem.

The reduction in compulsory schooling by one year was motivated by the increase in the student population in the primary and preparatory stages and the resulting pressures on the schooling infrastructure. By cutting compulsory schooling by one year, it was hoped that class sizes and

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<sup>13</sup> Although the law also stipulated that the total number of weeks of schooling would be increased from 32 weeks to 38 weeks and the school day would be extended by a maximum of 30 percent, which would be achieved by cancelling the multiple-shift system and moving to a full-school-day system—these changes were not actually implemented. These goals were unrealistic anyway. Since the curriculum of the first three grades did not change, would the school duration be extended for grades four and five and not for others? Moving to a single shift system would have been impossible even with the elimination of one grade level given the number of students and available seats—unless an immense sudden investment in infrastructure was done.

number of students per teacher could be reduced. Statistics on pupil per teacher do indeed show some improvement due to the policy change. Statistics on pupil per teacher show a slight improvement from 31.3 in 1987 to 29.9 in 1988 because of the elimination of the 6<sup>th</sup> grade.<sup>14</sup> However, public investment in education did not show much of a change, remaining at around 4.5 percent of the GDP in the late 80s and early 90s. Smaller classes and fewer students per teacher could potentially lead to higher student achievement for the cohorts affected by the new policy. We will assume that this effect is negligible compared to the effect of one full-year of reduced exposure to schooling—which is highly plausible given the modest drop in average class size.

### 3. Data

We use the 2012 round of the Egypt Labor Market Panel Survey (ELMPS, 2012), which provides detailed schooling and labor market information on individuals. The 2012 ELMPS follows individuals originally interviewed in the 1998 survey, with refreshment samples added both in 2006 and 2012 so that the 2012 cross-section is representative of the country at large (Assaad and Kraft, 2013).

We restrict our sample to a 10-year interval on each side of the birth-year cutoff, in accordance with our RDD. This corresponds to the 1968-87 birth cohorts. In addition, as explained above, the 1977 and 1978 birth cohorts—the double batch—are different from the other cohorts in the way that they are not only treated in terms of schooling duration but also in terms of class size. Therefore, we drop the double-batch birth cohorts from our sample. Our final sample, hence, includes 1968-1976 and 1979-87 birth cohorts. This also means that the individuals in our sample are 24- to 44-year-old. This age range is also convenient for two reasons. First, having younger individuals in the sample could be problematic because they might still be in school, resulting in censoring of the years of schooling variable. In fact, most college students in Egypt earn their degrees by age 24.<sup>15</sup> Second, we could not include individuals older than 45 because detailed schooling information was not elicited from these individuals in the survey. This sample

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<sup>14</sup> UNESCO data base: <http://data.uis.unesco.org/#>.

<sup>15</sup> About 0.5% of our sample have 18 or more years of schooling.

constitutes the largest one we use in our analysis, as most of our RDD samples include fewer individuals as we zoom in around the policy cutoff.

Generating the years of schooling variable – which is key for this study – is not straightforward because what is reported in the data is not the years of schooling but rather the highest level of schooling attended and the highest grade completed in that level. The difficulty of generating years of schooling from this information lies in figuring out who was subject to 5 years vs. 6 years of primary schooling. This information is directly elicited for only a small subset of respondents. Although we know when the law went into effect and the birth date of respondents, due to late or early school start and possible class repetition, two individuals sharing the same birth date might be subject to different rules. The advantage of the ELMPS over other data sets such as the Labor Force Survey or the Demographic Health Survey for Egypt is that it provides information on the year that the person entered primary school as well as the number of grades repeated in all stages of schooling. We use these two pieces of information – the year in which the individual started school and the information on class repetition – to determine the year in which the individual reached the 5<sup>th</sup> grade in primary school and, hence, whether this individual was subject to 5 or 6 years of primary schooling.

The wage information concerns both regular and casual wage earners. For regular wage earners, ELMPS collects information on both the basic monthly wage as well as any supplementary wages received such as bonuses over a period of a year, net of taxes. That share of the lump-sum wage supplement corresponding to a month is computed and added to the basic monthly wage to arrive at full-compensation from work. For casual wage earners, the ELMPS inquires about daily wages. The reference period for hours of work for both the regular and casual workers is the three months preceding the survey date. Information on the number of weeks worked in the past three months, days worked per week, and hours worked per day and week is collected. Hourly wages are computed by dividing monthly and daily wages into hours worked per month and per day, respectively, which are the average values for the three-month period.<sup>16</sup> Reported wages are from the individual's primary job.

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<sup>16</sup> We use the monthly and hourly wages that already exist in the public-use dataset.

### 3.1 Descriptive Statistics

In Table 1, we provide the basic descriptive statistics concerning our operational sample. The total number of 24-44-year-olds in our data is 13,144 (6,675 men and 6,439 women). However, for a small group of individuals (5.0 percent of females and 3.7 percent of males), we lack information on their school start age and hence on their years of schooling. The average years of schooling are about 10 years for men and 8.8 years for women. While 9.2 percent of men have no schooling, this incidence is much higher among women at 22.3 percent. The fraction completing primary school is 82.3 percent among men and 72.1 percent among women. Once they finish primary school, Egyptian children tend to complete lower secondary and upper secondary school. The fraction completing lower secondary school is 72.7 percent for men and 65.9 for women, and the fraction completing upper secondary is 66.6 percent for men and 60.3 for women. However, college graduation rates are much lower, at 21.2 percent for men and 20.3 percent for women. In essence, the school drop-out hazard rate in Egypt is high at school start and just after the upper secondary level, but much lower in between. This can also be seen in Figure 1, which gives the histogram for the years of schooling variable by gender.

As can be seen from Figure 1, among men, the largest group is upper secondary school graduates (11 or 12 years of schooling), followed by college graduates (15 or 16 years of schooling), primary school graduates (5 or 6 years of schooling) and those with no schooling—in that order. Among women, the largest group is also upper secondary school graduates; however, the order changes after that. The second largest group is those with no schooling, followed by college graduates.

Table 1 also shows that the wage employment rate is 72.5 percent for men and only 15.1 percent for women. The low rate of wage employment among women is the reason we restrict our analysis of returns to schooling to men. Of men who are wage-earners, more than 90 percent work full time and less than half (about 46 percent) work formally (i.e., with either a legal contract or social insurance coverage).

Next, we examine the policy effect on certain schooling outcomes using RDD graphs. Figure 2 does this for mean years of schooling; Figure 3 for completion status of primary school, lower secondary school, and upper secondary school; and Figure 4 for wages. In each graph, we provide plots using the global 10-year bandwidths as well as 5-year bandwidths to assess robustness. In

the estimation part, we take a wider set of alternative bandwidths, as here we only provide some suggestive evidence. In each graph, the middle vertical line indicates the time of the discontinuity in January 1978. The two vertical lines surrounding it stand for January 1977 and January 1979; we drop the observations in between these cutoffs because they belong to the double-batch cohort. While quadratic polynomials are fit before and after the discontinuity with the global data, linear polynomials are fit with the data including 5-year intervals on each side of the cutoff—as it is more appropriate to use lower orders of polynomials when the bandwidth is shorter (see Cattaneo et al. [2017]).

As can be seen in Figure 2, a substantial drop exists in average years of schooling with the policy for both men and women. In fact, the upper 95 percent confidence interval on the right-hand side of the cutoff lies below the mean level on the left-hand side of the cutoff for both men and women. While this fact is preserved in panel (II) of Figure 2, with 5-year bandwidths, for men; it vanishes for women. However, as we will see in later sections, this finding for women is peculiar to this particular bandwidth and cannot be generalized.

In Figure 3, we examine the potential impact of the policy on school completion at various schooling levels. Here, our goal is to examine whether there were unintended consequences of the school reform, which shortened the primary school duration, on school completion rates. However, as can be seen in panels (A) to (C) of Figure 3, no suggestive evidence exists for such an effect; in all panels, the fit on the left-hand side of the cutoff lies in between the 95 percent confidence intervals on the right-hand side of the cutoff. Moreover, this holds for both 5-year and 10-year bandwidths. Finally, Figure 4 allows us to examine the existence of a jump in wages at the cutoff. This is given only for men, as the returns to schooling is estimated only for them. Figure 4 suggests no evidence of a jump in wages at the cutoff with either bandwidths.

#### **4. Identification Method and Estimation**

It is well established that the schooling variable in the Mincer earnings equation is endogenous due to omitted variables like ability, motivation, parental connections, and so forth. The method to overcome this problem is the utilization of an instrumental variable for the schooling variable. In this study, we use the reduction of primary school in Egypt from 6 to 5 years as an instrumental

variable for years of schooling. More specifically, we exploit the variation in schooling across birth-cohorts induced by this institutional change.

The structure of our data, as illustrated in Figure 2, fits an RDD. The relationship between the treatment variable (years of schooling) and the running variable (month-year of birth) can be taken to be continuous as the number of discrete values of the running variable is high, and there is a significant jump at the cutoff point (when month-year of birth is equal to January 1978).

Since we exclude the double-batch cohorts of 1977 and 1978, we in fact use a donut-hole RDD. Donut-hole RDD, which excludes certain points in the immediate vicinity of the cutoff, has been used in a number of papers (Almond et al., 2011; Bajari et al., 2011; Barreca et al., 2011; Card and Giuliano, 2014; Goodman et al., 2019a). Cattaneo et al. (2017) discusses donut-hole RDD as a falsification check, where robustness of the findings to the points immediately around the cutoff is examined.<sup>17</sup> It is important to note a few features of our donut-hole. First, since we use a polynomial approach with a rectangular kernel, we do not put more weight on points around the cutoff. Second, while RDD requires extrapolation beyond the support of the data by definition, donut-hole RDD requires further extrapolation within the donut-hole. Therefore, it becomes even more important to assess the robustness of our findings to alternative bandwidths and polynomials—which we present at the end of this section.

The key identifying assumption in RDD is that potential outcome distributions are smooth around the cutoff. While this assumption is not directly testable, three diagnostic checks are typically used to assess its plausibility: (i) continuity of the score density around the cutoff, (ii) absence of treatment effects on pre-treatment covariates, (iii) absence of treatment effects at artificial cutoff values. Next, we present our findings on these diagnostic checks.

Since the running variable (month-year of birth) is set before the cutoff point is determined in our case, no manipulation in the running variable is possible. In addition, the cutoff point was not determined in order to include or exclude certain individuals from treatment. Hence, we can claim that the running variable and the cutoff are independently set. Nonetheless, to check the continuity of the score density around the cutoff, we use the test developed by Cattaneo, Jansson and Ma

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<sup>17</sup> This is applied, for instance, in Goodman et al. (2009b) and Scott-Claydon and Zafar (2019).

(2017), which is based on the comparison of the density of observations near the cutoff. According to the test, we fail to reject the null that no difference exists in the density of treatment and control groups at the cutoff; the p-value of the test is 0.733. Figure A1 in the Appendix provides a graphical illustration of this test, where a histogram of the running variable is given in panel (A) and estimated densities on both sides of the cutoff are given in panel (B).

Second, we examine any potential effects of the policy on pre-treatment covariates at the cut-off. In the absence of sorting around the cutoff, we would expect no jump for the pre-treatment covariates at the cutoff. In fact, the estimates provided in Table A1 in the Appendix show that a jump that is statistically significant at the 10-percent level is observed only for 2 of the 53 covariates—which is expected given the statistical significance level.

Third, we search for discontinuities in the relationship between the outcome variable and the running variable away from the cut-off. For this purpose, we divide the sample into two according to the actual cutoff. For each subsample, we take alternative cutoffs by gradually sliding the actual cut-off by one year each time and estimate policy effects using the counterfactual cutoffs. The results given in Table A2 in the Appendix show that the potential outcome distributions for both the years of schooling and log wages variables are smooth. Only one of the 32 estimates result in a statistically significant estimate at the 10 percent level.

In RDD, the difference between the limiting values of the outcome variable as the running variable approaches the cut-off from the right and from the left, respectively, gives us the average effect of assignment to treatment (intention to treat effect) at the cutoff point. However, we are not interested in the effect of assignment to treatment but the effect of receiving the treatment (the effect of more schooling on wages). To estimate this, we simply need to divide the jump in the relationship between the expected outcome and running variables at the cut-off (that in Figure 4) with the jump in the relationship between the probability of treatment and running variables at the cut-off (that in Figure 2) – as in a Wald estimate. This gives us the LATE estimate (Imbens and Angrist, 1994), which is the average treatment effect for the subpopulation who receive the treatment only because they are assigned to it (who would not receive it in the absence of treatment) – called *compliers*. In RDD, this will be the LATE for the cutoff population – which gives the impact of schooling on wages for those whose schooling is altered by policy.

When there is heterogeneity in the returns to schooling across various subpopulations, the local average treatment effect may be quite different from the average treatment effect for the total population because the former is only for compliers. As discussed in Oreopoulos (2006), the larger the set of compliers is, the closer is LATE to ATE. The policy in Egypt affects all men except those who drop out before earning a primary school degree—who constitute 15.4 percent of our sample; therefore, the set of compliers forms almost 85 percent of our sample. However, the fraction of compliers decreases for certain subpopulations, such as those with a rural birthplace and with less-educated fathers, which we use in the heterogeneity analysis. In addition, since there are no “never takers”—those who are treated by the policy but do not change their behavior—due to the nature of the policy, our LATE estimate is equal to the treatment effect on the untreated—as shown in Angrist and Pischke (2009).

Hahn et al. (2001) show that, under certain assumptions, fuzzy RDD identifies the LATE at the cutoff. They show that the interpretation of this ratio as the causal effect requires the same assumptions as in Imbens and Angrist (1994). A key assumption of the LATE theorem is the exclusion restriction assumption. There is no reason to expect date-of-birth to be correlated with unobserved variables that may affect wages such as ability, motivation, or parental connections. Therefore, we expect date of birth to affect wages through its effect on schooling but we do not expect it to have a direct effect on wages.<sup>18</sup> Another important assumption in the LATE theorem is monotonicity, which requires the policy not to increase the schooling of certain individuals (say, for instance, due to the shorter schooling time to a primary school degree) while cutting the schooling duration of the majority by one year. To check this, we examine the policy effect on various levels of school attainment in Section 5 and find that the monotonicity assumption holds.

The estimation of LATE in a fuzzy RDD is carried out by a 2SLS procedure.

$$s_i = \alpha_0 + \alpha_1 D_i + f(X_i) + u_i, \quad (1)$$

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<sup>18</sup> The fact that the policy decreases the duration of schooling by one year implies that it increases potential work experience by one year as well. However, this problem is common to all studies using compulsory schooling laws and school-leaving age as instruments. At the same time, since we observe most individuals in our sample during the middle of their work-cycle—when the age-earnings profile is rather flat—this would be less of a problem.

$$\log w_i = \beta_0 + \beta_1 s_i + g(X_i) + v_i, \quad (2)$$

where  $w$  denotes the wage rate and  $s$  denotes the years of schooling.  $D$  is a dummy variable for the policy, which takes the value of one if an individual is born after January 1978 and zero otherwise. The running variable is shown by  $X$ ,  $f(\cdot)$  denotes the relationship between the treatment and running variables, and  $g(\cdot)$  denotes the relationship between the outcome and running variables. The error terms in the first-stage and second-stage are shown by  $u$  and  $v$ , respectively. The key parameter of interest is  $\beta_1$ , which denotes the percent change in wages when years of schooling is raised by one. We cluster the standard errors at the month-year of birth level.

The key issue in the estimation of equations (1) and (2) via 2SLS is the correct specification of the functional forms,  $f(\cdot)$  and  $g(\cdot)$ . Since we use donut-hole RDD, we do not use an optimal-bandwidth approach. Hence, we take two different approaches to show the robustness of our findings to alternative bandwidths and alternative polynomials. In our main approach, we gradually narrow the bandwidth while taking linear polynomials on each side of the cutoff.<sup>19</sup> We start with 10-year intervals on each side of the cutoff and gradually zoom in around the cutoff up to 5-year intervals on each side. We do not zoom in further, given our donut-hole, not to extrapolate within the donut-hole based on relatively few points outside of it. We also check robustness of our estimates to the order of polynomials on each side of the cutoff. For this purpose, using the 10-year intervals, we examine how the estimates differ taking first and second order polynomials. We do this only with the wider bandwidth, as the polynomial order would matter more with a wider bandwidth.

In essence, the first approach fixes the degree of polynomial for trends and checks the robustness to alternative bandwidths, whereas the second approach fixes the bandwidths and checks the robustness to alternative degrees of polynomials for trends. Although we do not use an optimal-bandwidth approach, we use relatively narrow bandwidths as is typical in this approach. In fact, parametric and nonparametric approaches are quite related. A parametric approach can be

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<sup>19</sup> This approach is in line with the suggestion of Cattaneo et al. (2017), who recommend using high-order polynomials with global bandwidths but lower order polynomials with restricted bandwidths.

interpreted as a nonparametric method with a very large bandwidth, and a nonparametric approach can be interpreted as a parametric approach with low-order polynomials.<sup>20</sup>

## 5. Results

We first examine the first stage—the policy effect on schooling outcomes—in Section 5.1. Section 5.2 shows our main estimates on returns to schooling. Section 5.3 presents how returns to schooling vary across key subpopulations; in particular, we examine how returns to schooling differ for underprivileged groups. In Section 5.4, we aim to understand the observed differences in returns to schooling among subgroups by examining the variation in the policy impact on various schooling outcomes among them. Finally, Section 5.5 examines the existence of policy impact on the population of wage earners to check for any compositional effects on our sample.

### 5.1 Policy Effect on Schooling Outcomes

Table 2 shows the RDD estimates regarding the policy effect on men’s schooling outcomes. Here, we gradually reduce the bandwidth from 10 to 5 years using split linear polynomials on each side of the cutoff, while excluding the double-batch cohorts. The results indicate that the policy reduces men’s years of schooling by about 0.9 years. No evidence of a policy effect on other schooling outcomes exists, except for the effect on having no schooling with one particular bandwidth. Table 3 provides the same analysis for women. The evidence of a policy effect on years of schooling, at about 0.7 to 0.9 years, exists for all bandwidths wider than 5 years on each side. In comparison to the estimates for men, the policy effect on years of schooling is somewhat smaller for women. Also for women, there is no evidence of a policy effect on other schooling outcomes.

Appendix Table A3 presents the estimates with alternative orders of polynomials, where we now fix the bandwidth at 10-year intervals, excluding the double-batch cohorts. These results indicate

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<sup>20</sup> At the same time, these methods take a different approach in the tradeoff between bias and precision. Nonparametric are less precise as they typically use narrow bandwidths around the cutoff with fewer observations, whereas parametric methods risk larger biases due to functional form misspecification. As stated by Lee and Lemieux (2010), “Nonparametric estimation does not represent a ‘solution’ to functional form issues raised by RD designs. It is therefore helpful to view it as a complement to—rather than a substitute for—parametric estimation.”

a policy effect on years of schooling that is about 0.9 years for both men and women. While the coefficients are statistically significant with linear and quadratic trends for men, they are statistically significant only with the linear trend for women. The estimates in the other rows indicate no evidence of a policy effect on other schooling outcomes either for men or women.

## 5.2 Returns to Schooling

Next, we discuss our results on returns to schooling estimates for men. Table 4 shows the OLS estimates based on equation (2) and the 2SLS estimates based on equations (1) and (2) for alternative bandwidths. We first examine the first-stage results of the 2SLS estimates. The first stage coefficients are statistically significant at the 1-percent level regardless of the bandwidth. The F-statistic is above ten for 6- to 10-year bandwidths, but 7.3 with 5-year bandwidths. Overall, we have evidence for a sufficiently strong first stage.

The OLS estimates in Table 4 range from 0.019 to 0.021 and are highly statistically significant, whereas the 2SLS estimates range from 0.019 to 0.044 and are all imprecisely estimated. The magnitude of these coefficients suggests that the 2SLS estimates are higher than the OLS estimates. According to the estimates in Appendix Table A5 based on 10-year bandwidths, the 2SLS estimate of 0.038 with linear trends drops to 0.029 with quadratic trends, and it remains imprecisely estimated. The corresponding OLS estimate is 0.019.

Table 5 presents the robustness of our findings to alternative sample restrictions. As can be seen in panel (A), with the sample restricted to full-time workers, the 2SLS estimates range between 0.023 and 0.034 and are similar to the baseline estimates in Table 4. In panel (B), where we trim the observations in the bottom and top 1 percent of wages, the 2SLS estimates are also highly similar to the baseline estimates in Table 4. In essence, our results are quite robust to alternative sampling restrictions.

## 5.3 Heterogeneity in Returns to Schooling

In this section, we examine the heterogeneity in returns to schooling by urban/rural place of birth and fathers' educational attainment. The results are given in Table 6. As can be seen in panel (A), for men born in rural places, the returns to schooling estimates are much higher. Moreover, the

2SLS coefficients are statistically significant at the 10 percent level with 8-year and with 6-year intervals on either side. The estimates suggest that an extra year of schooling increases wages of men born in rural areas by about 8.5–12.7 percent.

Panels (B) and (C) show the results by father’s educational attainment. Panel (B) takes fathers with no school degree and panel (C) takes fathers with an upper secondary or lower educational attainment. The estimated coefficients range between 0.079 and 0.095 in panel (B) and between 0.040 and 0.056 in panel (C). The estimated returns to schooling for men with less-educated fathers are higher than those reported for the full sample in Table 5. In addition, the returns to schooling in panel (B)—for individuals with the lowest level of father’s educational attainment—are the highest. In essence, we find that men coming from less-privileged backgrounds (both in terms of rural/urban status and father’s educational attainment) have higher returns to schooling.

Our findings are consistent with those in Balestra and Backes-Gellner (2017), Clay et al. (2016), and Meghir and Palme (2005) in that the effects of schooling on earnings is small overall but much higher for underprivileged groups. This result also indicates that depriving individuals of educational opportunities during early ages translates into inequities in adult earnings. Thus, this result from a developing country context lends further support to the evidence in the literature that ability gaps across socioeconomic groups open up at early ages and the productivity of investments is especially high among young disadvantaged children (Cunha et al., 2006, Heckman, 2007).

The gap between the 2SLS and OLS estimates grows as fathers’ educational attainment decreases. The change in the fraction of non-compliers by parental education would partly explain this pattern. The fraction of non-compliers is higher among children with less-educated fathers; therefore, the distance between the 2SLS estimates (for compliers) and the OLS estimates (for all) increases as parental education decreases.

#### **5.4 Understanding Higher Returns to Schooling for Underprivileged Groups**

The shortening of primary school duration could certainly have adverse effects on school success during primary school years and afterward, at least for certain subgroups. If this change in the structure of primary school affects school success, we could expect a change in primary school completion rates. In addition, the policy potentially reduces the amount of knowledge accumulated

at the end of primary school due to reduced contact time and an abridged curriculum. If human capital accumulation depends on existing human capital, the elimination of one grade level would mean less human capital accumulation in later school years—which could translate into diminished success in lower secondary and upper secondary school years. On the other hand, countervailing behavioral responses might exist. Students who graduate with shorter primary school duration, as well as their parents and teachers, might invest more heavily in their human capital acquisition during lower and upper secondary school years.<sup>21</sup> We check evidence for these other potential effects of the policy for the full sample and the subgroups in heterogeneity analysis, to understand the estimated differences in returns to schooling among subgroups.

Table 7 illustrates the estimates of the policy impact on men’s schooling attainment for the subsamples in our heterogeneity analysis, as well as the full sample.<sup>22</sup> Here, we restrict the analysis to wage earners because our aim is to understand the patterns observed in Table 6, which is also for wage earners. For the full sample of wage earners, the policy impacts on primary school, lower secondary school, upper secondary school, and college completion are all negative but statistically insignificant. For the sample of wage earners with a rural birth place, the magnitudes of the estimated negative impacts on school completion are much larger. For instance, the policy reduces the upper secondary school completion rate by 13.1 percentage points. In addition, the estimated impact on the completion of other schooling levels is at least 6 percentage points. The estimated policy impacts on wage earners whose fathers have lower levels of schooling are also higher overall,<sup>23</sup> albeit not as high as for those with a rural place of birth. These patterns are consistent with the findings in Table 6 that the returns to schooling are higher for men with less-educated fathers and much higher for men with a rural place of birth. Essentially, the results suggest that boys coming from underprivileged backgrounds were hurt more in the completion of various schooling levels—which is reflected in their wages.

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<sup>21</sup> These potential behavioral responses are not specific to our study as it uses a reduction in compulsory schooling years. In the case of an extension of compulsory schooling, individuals might respond by changing their post-schooling investments in job training—as argued by Rosenzweig and Wolpin (2000).

<sup>22</sup> Table 7 provides the estimates with 5-year bandwidths. The patterns with other bandwidths are similar.

<sup>23</sup> For instance, the policy decreases the college completion rate by 7.8 percentage points for wage earners whose fathers have an intermediate school or lower degree.

## 5.5 Policy Effect on Wage Employment

Finally, we examine the policy effect on wage employment of men to make sure that the policy does not have a compositional impact on our sample of male wage earners. Based on alternative bandwidths, Table 8 provides the results for the full sample and for the three samples used in heterogeneity analysis. As can be seen in panel (A), for the full sample, column (1) shows evidence of a positive policy impact on wage employment that is statistically significant at the 10-percent level. However, as we take narrower bandwidths than 10-year intervals on both sides, no evidence of a policy effect on wage employment remains with any bandwidth. In fact, the coefficient estimates are quite close to zero. In addition, Appendix Table A4 shows that the positive policy impact on wage employment with 10-year bandwidths also vanishes when we take second-order instead of linear polynomials. Therefore, we can conclude that the policy has no compositional effects on our sample. Finally, panels (B) to (D) of Table 8 show no evidence of a policy impact for any of the subsamples used in our heterogeneity analysis. Therefore, no evidence exists that our findings on returns to schooling are contaminated by a sample selection bias.

## 6. Conclusion

The reduction in the duration of primary schooling in 1988 from six to five years in Egypt offers a unique opportunity to estimate the wage returns to human capital resulting from an extra year of primary school. While an extensive literature studies the wage returns to schooling, our setting is peculiar in certain ways. First, since we study a reduction in the duration of compulsory schooling, instead of an increase in it, the compliers with the policy constitute a very large fraction of the population, namely 85 percent. Therefore, our LATE estimate is much more representative of the total population. Second, most studies on wage returns to schooling estimate the returns from an extra year of schooling at higher education levels, mostly at the upper secondary school level, because they study policy changes in developed countries. In contrast, we measure the returns to an extra year at the primary school level—for which evidence is relatively rare.

We find modest wage returns to human capital acquired at the primary school level in a developing country. We find that the returns to schooling for men aged 24–44 in Egypt is about 2–4 percent. It is also important to note that these estimates stand for the human capital effect of an extra year

of schooling in primary school because there are no sheepskin effects of the policy—as the students affected by the policy continue to receive the same degree.

The wage returns to an extra year of primary school in Egypt are much higher for men coming from underprivileged backgrounds, such as men born in rural areas and men whose fathers have low levels of education. For instance, an extra year of primary school increases wages of men with a rural birthplace by 9–12 percent. This finding is important for two different reasons. First, it implies that interventions targeting underprivileged groups to improve their educational attainment—such as cash transfers and development of schooling infrastructure in rural areas—can have very high returns. Moreover, this policy would decrease inequality. Second, the evidence for heterogeneity in returns to schooling increases the importance of having an instrument that affects a very large section of the population—as in our case. In fact, were Egypt to increase the duration of compulsory schooling, it is the group of underprivileged children who would form the majority of compliers. In this case, the LATE of returns to schooling that we estimate would be much higher than the ATE.

An important question is why the returns to schooling in a country with low levels of human capital are low on average but higher for underprivileged groups. We find that the reduction in the duration of primary school lowers the overall schooling attainment of children with lower socioeconomic backgrounds. In contrast, no such effect exists for the total population of children. Hence, the drop in wages for the underprivileged is higher. At the same time, there could be other contributing factors to the stronger impact for underprivileged individuals. In primary school, students acquire not only cognitive skills but also non-cognitive skills such as working together, being patient, and the ability to talk in front of others. Children from privileged backgrounds may be already acquiring these habits at home but underprivileged children may not be. In addition, to the degree that cognitive skills matter in the labor market, children from privileged backgrounds are more likely to compensate for the reduced learning—via further investments in their education by their parents. Another potential factor is that acquiring skills (both cognitive and non-cognitive) at school in Egypt might matter more for underprivileged children’s labor market outcomes because they lack family connections that are enjoyed by privileged children in securing a good job (as found in Assaad et al., 2017).

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# Figures and Tables

Figure 1: Distribution of Years of Schooling by Gender

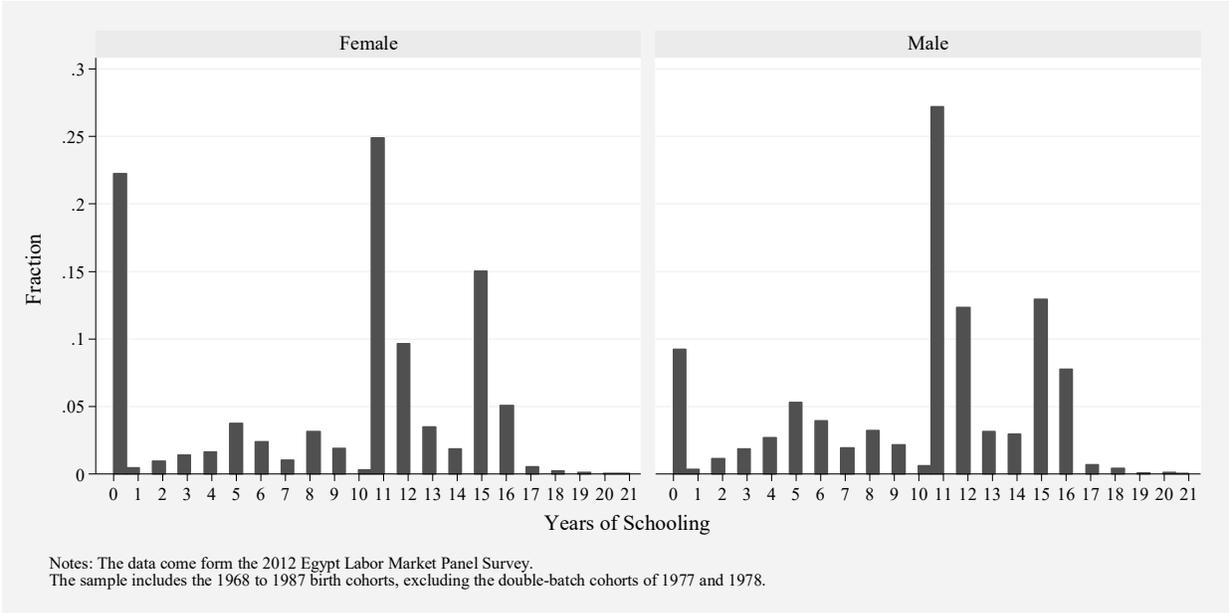
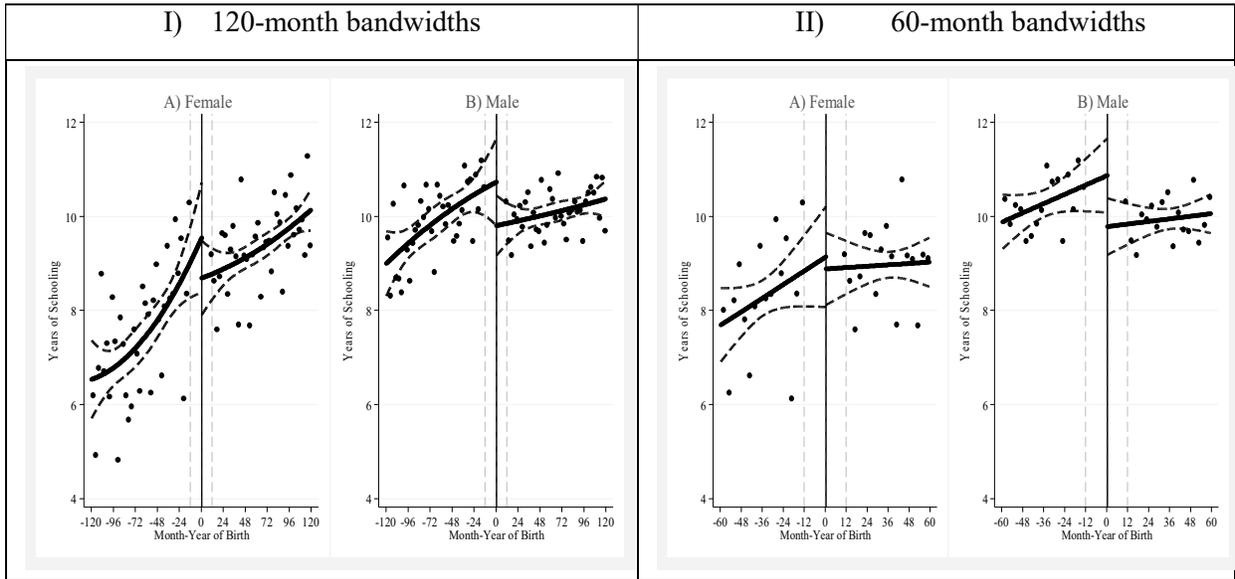
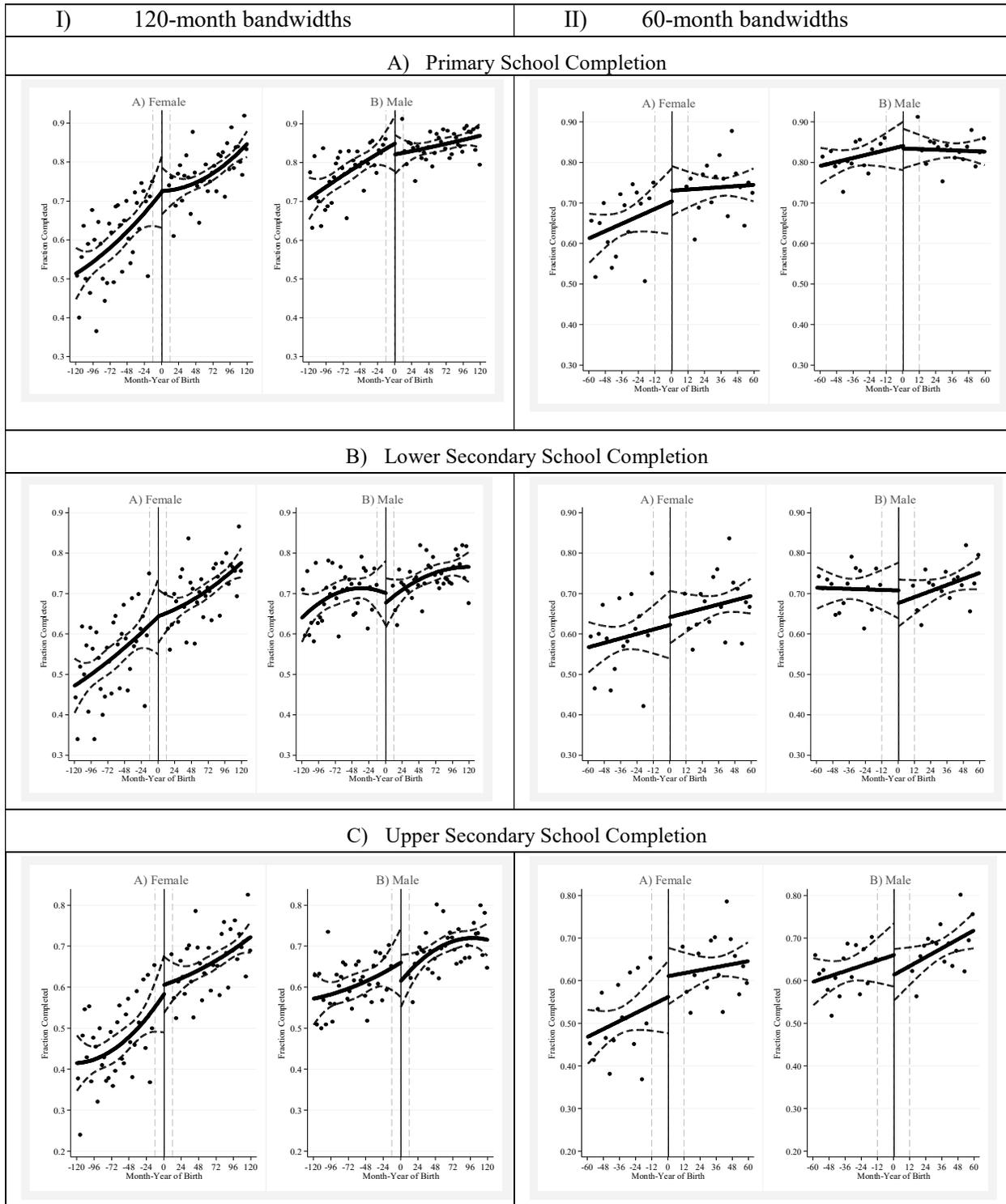


Figure 2: Mean Years of Schooling for Females and Males



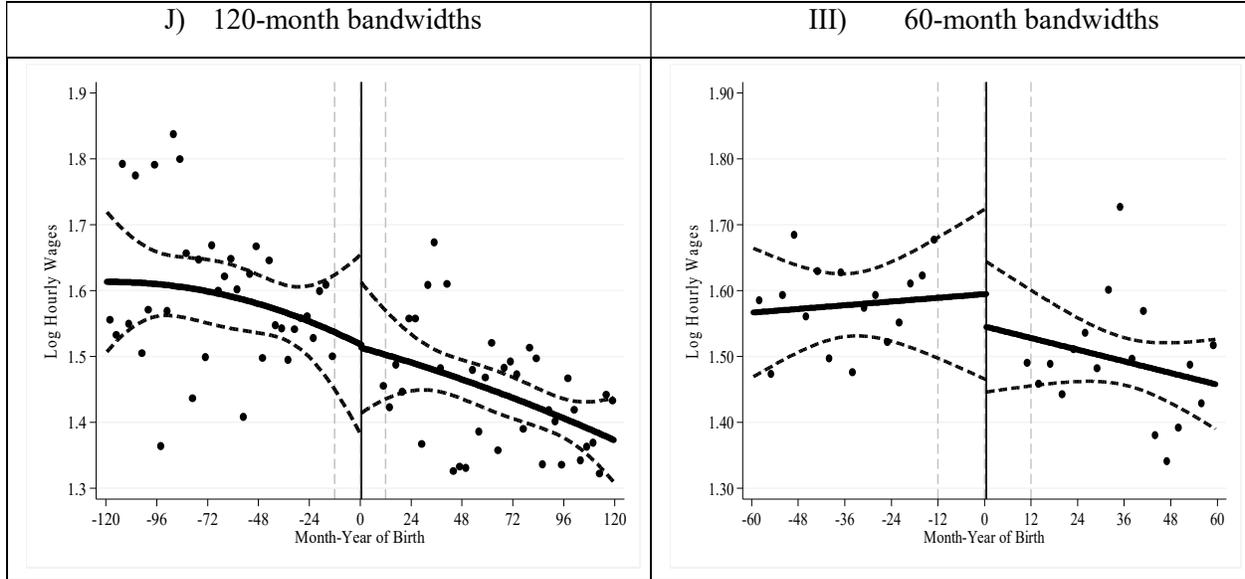
Notes: The double-batch cohorts, 1977 and 1978 birth cohorts, are excluded. Hence, no points exist within the dashed vertical lines at adjusted -12 and 12 months. Each dot shows the average value for three month-year of births. On each side of the cutoff, quadratic polynomials are fit in panel (I) whereas linear polynomials are fit in panel (II)—where the bandwidth is narrower.

Figure 3: Fraction Completing Primary, Lower-Secondary, and Upper-Secondary School for Females and Males



Notes: The double-batch cohorts, 1977 and 1978 birth cohorts, are excluded. Hence, no points exist within the dashed vertical lines at adjusted -12 and 12 months. Each dot shows the average value for three month-year of births. On each side of the cutoff, quadratic polynomials are fit in panel (I) whereas linear polynomials are fit in panel (II)—where the bandwidth is narrower.

Figure 4: Log Hourly Wage Rate for Men



Notes: The double-batch cohorts, 1977 and 1978 birth cohorts, are excluded. Hence, no points exist within the dashed vertical lines at adjusted -12 and 12 months. Each dot shows the average value for three month-year of births. On each side of the cutoff, quadratic polynomials are fit in panel (I) whereas linear polynomials are fit in panel (II)—where the bandwidth is narrower.

Table 1 – Descriptive Statistics

	A) Men			B) Women		
	Mean	S.D.	No obs.	Mean	S.D.	No obs.
Years of Schooling	10.047	4.768	6,430	8.816	5.658	6,118
Treated	0.643	0.479	6,675	0.679	0.467	6,439
No School	0.092	0.289	6,430	0.223	0.416	6,118
Illiterate	0.144	0.352	6,672	0.258	0.437	6,419
Primary School	0.823	0.382	6,670	0.721	0.449	6,411
Lower Secondary School	0.727	0.446	6,672	0.659	0.474	6,415
Upper Secondary School	0.666	0.472	6,672	0.603	0.489	6,415
College	0.212	0.409	6,672	0.203	0.402	6,419
Wage Employment	0.725	0.447	6,605	0.151	0.358	6,409
Full Time Wage Employment, Def. 1	0.675	0.468	6,516	0.137	0.344	6,390
Full Time Wage Employment, Def. 2	0.665	0.472	6,605	0.137	0.343	6,409
Formal Wage Employment	0.332	0.471	6,605	0.124	0.330	6,409
Log Wage	1.507	0.681	4,785	1.412	0.777	968
Month of Birth						
January	0.112	0.316	6,675	0.112	0.315	6,439
February	0.077	0.267	6,675	0.079	0.270	6,439
March	0.083	0.276	6,675	0.086	0.280	6,439
April	0.071	0.258	6,675	0.063	0.243	6,439
May	0.064	0.245	6,675	0.065	0.247	6,439
June	0.064	0.245	6,675	0.064	0.244	6,439
July	0.138	0.345	6,675	0.141	0.349	6,439
August	0.078	0.269	6,675	0.074	0.261	6,439
September	0.079	0.270	6,675	0.079	0.270	6,439
October	0.083	0.276	6,675	0.080	0.271	6,439
November	0.080	0.271	6,675	0.080	0.272	6,439
December	0.070	0.255	6,675	0.077	0.266	6,439
Father's Educational Attainment						
Illiterate	0.536	0.499	6,675	0.528	0.499	6,439
Literate with no Degree	0.178	0.382	6,675	0.174	0.379	6,439
Primary School	0.124	0.330	6,675	0.116	0.320	6,439
Lower Secondary School	0.082	0.275	6,675	0.096	0.295	6,439
Upper Secondary School	0.016	0.124	6,675	0.017	0.130	6,439
College	0.062	0.240	6,675	0.066	0.249	6,439
Post-Graduate	0.002	0.049	6,675	0.003	0.054	6,439
Mother's Educational Attainment						
Illiterate	0.779	0.415	6,675	0.766	0.423	6,439
Literate with no Degree	0.067	0.250	6,675	0.073	0.260	6,439
Primary School	0.067	0.249	6,675	0.064	0.245	6,439
Lower Secondary School	0.057	0.231	6,675	0.060	0.237	6,439
Upper Secondary School	0.006	0.078	6,675	0.008	0.090	6,439
College	0.024	0.153	6,675	0.028	0.166	6,439
Post-Graduate	0.001	0.024	6,675	0.001	0.028	6,439
Urban Birth Place	0.437	0.496	6,643	0.440	0.496	6,398

Notes: The data come from the 2012 Egypt Labor Market Panel Survey. The sample includes the 1968-1987 birth cohorts, excluding the double-batch cohorts of 1977-78. The individuals in the sample are 24- to 44-year-olds. In full-time employment, definition one takes a 1-week reference period whereas definition two takes a 3-month period.

Table 2 – Policy Effect on Schooling Outcomes for Men

	Number of Years around the Cutoff			
	10	8	6	5
A) Years of Schooling	-0.907*** [0.218]	-0.879*** [0.251]	-1.071*** [0.309]	-0.847** [0.337]
No obs.	6,398	4,980	3,562	2,878
B) Primary School Completion	-0.028 [0.018]	-0.030 [0.020]	-0.035 [0.026]	-0.001 [0.027]
No obs.	6,638	5,186	3,716	3,009
C) Lower Secondary School Completion	-0.022 [0.021]	-0.011 [0.024]	-0.044 [0.030]	-0.013 [0.034]
No obs.	6,640	5,187	3,717	3,010
D) Upper Secondary School Completion	0.015 [0.021]	0.012 [0.023]	-0.032 [0.028]	-0.029 [0.031]
No obs.	6,640	5,187	3,717	3,010
E) Lower Secondary School Completion conditional on Primary School Completion	0.004 [0.020]	0.018 [0.024]	-0.016 [0.028]	-0.014 [0.034]
No obs.	5,458	4,260	3,053	2,473
F) No Schooling	-0.001 [0.014]	-0.010 [0.016]	-0.013 [0.020]	-0.046** [0.020]
No obs.	6,398	4,980	3,562	2,878
G) Illiterate	0.005 [0.017]	0.004 [0.019]	-0.002 [0.026]	-0.040 [0.027]
No obs.	6,640	5,187	3,717	3,010

Notes: The data come from the 2012 Egyptian Labor Market Panel Study. The sample is restricted to various bandwidths around the cutoff, as specified in the column headings. The double-batch birth cohorts of 1977 and 1978, which are immediately around the cutoff, are excluded. The dependent variable is given in panel headings (A) to (G). Each cell comes from a separate regression. All regressions include a control for the policy dummy -- which takes the value of one for cohorts born after 1978 -- and split linear polynomials around the cutoff in the running variable (month-year of birth). In addition, all regressions include dummies for the birth month, dummies for various levels of the father's and mother's educational attainment, and dummies for rural/urban status and the governorate of birthplace. Clustering is done at the month-year of birth level. Statistically significant: \*\*\* 1 percent level; \*\* 5 percent level, \* 10 percent level.

Table 3 – Policy Effect on Schooling Outcomes for Women

	Number of Years around the Cutoff			
	10	8	6	5
A) Years of Schooling	-0.903*** [0.308]	-0.870** [0.369]	-0.847* [0.450]	-0.682 [0.563]
No obs.	6,080	4,519	3,119	2,479
B) Primary School Completion	-0.038 [0.024]	-0.020 [0.028]	-0.011 [0.035]	-0.012 [0.045]
No obs.	6,370	4,761	3,287	2,614
C) Lower Secondary School Completion	-0.030 [0.027]	-0.014 [0.031]	-0.014 [0.039]	-0.016 [0.048]
No obs.	6,374	4,765	3,290	2,616
D) Upper Secondary School Completion	0.022 [0.029]	0.024 [0.033]	0.013 [0.042]	0.014 [0.054]
No obs.	6,374	4,765	3,290	2,616
E) Lower Secondary School Completion conditional on Primary School Completion	0.008 [0.024]	0.007 [0.028]	-0.006 [0.034]	-0.009 [0.041]
No obs.	4,584	3,368	2,327	1,840
F) No Schooling	0.007 [0.023]	-0.001 [0.028]	0.009 [0.033]	0.001 [0.041]
No obs.	6,080	4,519	3,119	2,479
G) Illiterate	0.025 [0.023]	0.018 [0.028]	0.017 [0.034]	0.013 [0.042]
No obs.	6,378	4,767	3,291	2,616

Notes: The data come from the 2012 Egyptian Labor Market Panel Study. The sample is restricted to various bandwidths around the cutoff, as specified in the column headings. The double-batch birth cohorts of 1977 and 1978, which are immediately around the cutoff, are excluded. The dependent variable is given in panel headings (A) to (G). Each cell comes from a separate regression. All regressions include a control for the policy dummy -- which takes the value of one for cohorts born after 1978 -- and split linear polynomials around the cutoff in the running variable (month-year of birth). In addition, all regressions include dummies for the birth month, dummies for various levels of the father's and mother's educational attainment, and dummies for rural/urban status and the governorate of birthplace. Clustering is done at the month-year of birth level. Statistically significant: \*\*\* 1 percent level; \*\* 5 percent level, \* 10 percent level.

Table 4 - Returns to Schooling for Men

	Number of Years around the Cutoff			
	10	8	6	5
OLS-RDD	0.019*** [0.002]	0.021*** [0.002]	0.021*** [0.003]	0.021*** [0.003]
IV-RDD	0.038 [0.045]	0.044 [0.045]	0.031 [0.046]	0.019 [0.071]
First-Stage Results	-1.024*** [0.285]	-1.179*** [0.315]	-1.475*** [0.388]	-1.128*** [0.417]
F-Statistic	12.903	13.962	14.429	7.322
No obs.	4,586	3,644	2,621	2,106

Notes: The data come from the 2012 Egyptian Labor Market Panel Study. The sample is restricted to various bandwidths around the cutoff, as specified in the column headings. The double-batch birth cohorts of 1977 and 1978, which are immediately around the cutoff, are excluded. All regressions include a control for the policy dummy -- which takes the value of one for cohorts born after 1978 -- and split linear polynomials around the cutoff in the running variable (month-year of birth). In addition, all regressions include dummies for the birth month, dummies for various levels of the father's and mother's educational attainment, and dummies for rural/urban status and the governorate of birth-place. Clustering is done at the month-year of birth level. F-statistic is adjusted for the number of clusters. Statistically significant: \*\*\* 1 percent level; \*\* 5 percent level, \* 10 percent level.

Table 5 - Returns to Schooling for Men with Alternative Samples

	Number of Years around the Cutoff			
	10	8	6	5
<i>A) Only Full-time Workers</i>				
OLS-RDD	0.019*** [0.002]	0.020*** [0.003]	0.020*** [0.003]	0.021*** [0.003]
IV-RDD	0.033 [0.051]	0.034 [0.052]	0.023 [0.056]	0.033 [0.090]
No obs.	4,204	3,352	2,409	1,938
<i>B) Bottom and Top 1 Percentile of Wages Trimmed</i>				
OLS-RDD	0.017*** [0.002]	0.019*** [0.002]	0.019*** [0.003]	0.019*** [0.003]
IV-RDD	0.030 [0.039]	0.046 [0.037]	0.027 [0.041]	0.022 [0.064]
No obs.	4,497	3,578	2,574	2,065

Notes: The data come from the 2012 Egyptian Labor Market Panel Study. The sample is restricted to various bandwidths around the cutoff, as specified in the column headings. The double-batch birth cohorts of 1977 and 1978, which are immediately around the cutoff, are excluded. The dependent variable is given in panel headings (A) to (B). Each cell comes from a separate regression. All regressions include a control for the policy dummy -- which takes the value of one for cohorts born after 1978 -- and split linear polynomials around the cutoff in the running variable (month-year of birth). In addition, all regressions include dummies for the birth month, dummies for various levels of the father's and mother's educational attainment, and dummies for rural/urban status and the governorate of birthplace. Clustering is done at the month-year of birth level. Statistically significant: \*\*\* 1 percent level; \*\* 5 percent level, \* 10 percent level.

Table 6 - Heterogeneity in Returns to Schooling for Men

	Number of Years around the Cutoff			
	10	8	6	5
<i>A) Rural Birth Place</i>				
OLS-RDD	0.015*** [0.003]	0.016*** [0.003]	0.014*** [0.003]	0.013*** [0.004]
IV-RDD	0.127 [0.085]	0.126* [0.073]	0.114* [0.067]	0.085 [0.065]
No obs.	2,570	2,009	1,403	1,106
<i>B) Fathers with No Degree (Illiterate or Reads and Writes)</i>				
OLS-RDD	0.016*** [0.002]	0.017*** [0.002]	0.016*** [0.003]	0.015*** [0.003]
IV-RDD	0.088 [0.059]	0.093 [0.061]	0.079 [0.055]	0.095 [0.102]
No obs.	3,267	2,608	1,863	1,515
<i>C) Fathers with an Upper Secondary School Degree or Lower Schooling</i>				
OLS-RDD	0.018*** [0.002]	0.019*** [0.002]	0.019*** [0.003]	0.019*** [0.003]
IV-RDD	0.049 [0.044]	0.056 [0.046]	0.048 [0.046]	0.040 [0.071]
No obs.	4,219	3,349	2,399	1,934

Notes: The data come from the 2012 Egyptian Labor Market Panel Study. The sample is restricted to various bandwidths around the cutoff, as specified in the column headings. The double-batch birth cohorts of 1977 and 1978, which are immediately around the cutoff, are excluded. The sample is further restricted according to the criteria given in panel headings (A) to (C). Each cell comes from a separate regression. All regressions include a control for the policy dummy -- which takes the value of one for cohorts born after 1978 -- and split linear polynomials around the cutoff in the running variable (month-year of birth). In addition, all regressions include a polynomial in the running variable (year of birth) whose order is specified in the last row of the table, dummies for the birth month, dummies for various levels of the father's and mother's educational attainment, and dummies for rural/urban status and the governorate of birthplace. Clustering is done at the month-year of birth level. Statistically significant: \*\*\* 1 percent level; \*\* 5 percent level, \* 10 percent level.

Table 7 – Heterogeneity in Schooling Outcomes for Male Wage Earners

	Full Sample	Rural Birth	Father with No School Degree	Father with an Upper Secondary School or Lower Degree
A) Primary School Completion	-0.022 [0.035]	-0.070 [0.061]	-0.028 [0.048]	-0.023 [0.038]
No obs.	2,201	1,151	1,589	2,025
B) Lower Secondary School Completion	-0.018 [0.040]	-0.061 [0.065]	-0.007 [0.054]	-0.018 [0.044]
No obs.	2,201	1,151	1,589	2,025
C) Upper Secondary School Completion	-0.053 [0.035]	-0.131** [0.061]	-0.028 [0.051]	-0.056 [0.039]
No obs.	2,201	1,151	1,589	2,025
D) College	-0.056 [0.040]	-0.089 [0.059]	-0.076 [0.048]	-0.078* [0.042]
No obs.	2,201	1,151	1,589	2,025

Notes: The data come from the 2012 Egyptian Labor Market Panel Study. The sample is restricted to 5-year bandwidths around the birth-cohort cutoff. The double-batch birth cohorts of 1977 and 1978, which are immediately around the cutoff, are excluded. The dependent variable is given in panel headings (A) to (D). Each cell comes from a separate regression. All regressions include a control for the policy dummy -- which takes the value of one for cohorts born after 1978 -- and split linear polynomials around the cutoff in the running variable (month-year of birth). In addition, all regressions include dummies for the birth month, dummies for various levels of the father's and mother's educational attainment, and dummies for rural/urban status and the governorate of birth-place. Clustering is done at the month-year of birth level. Statistically significant: \*\*\* 1 percent level; \*\* 5 percent level, \* 10 percent level.

Table 8 – Policy Effect on Male Wage Employment for Alternative Samples

	Number of Years around the Cutoff			
	10	8	6	5
A) Full Sample	0.039*	0.024	0.003	-0.011
	[0.023]	[0.025]	[0.032]	[0.038]
No obs.	6,573	5,136	3,688	2,985
B) Rural Birth Place	0.028	0.019	0.004	-0.046
	[0.032]	[0.039]	[0.050]	[0.057]
No obs.	3,696	2,856	1,990	1,591
C) Father with No School Degree	0.011	0.024	0.020	-0.023
	[0.029]	[0.031]	[0.043]	[0.050]
No obs.	4,709	3,734	2,671	2,179
D) Father with an Upper Secondary School Degree or Lower Schooling	0.024	0.022	0.002	-0.017
	[0.025]	[0.027]	[0.036]	[0.043]
No obs.	6,062	4,752	3,403	2,759

Notes: The data come from the 2012 Egyptian Labor Market Panel Study. The sample is restricted to various bandwidths around the birth-cohort cutoff, as specified in the column headings. The double-batch birth cohorts of 1977 and 1978, which are immediately around the cutoff, are excluded. The dependent variable is given in panel headings (A) to (D). Each cell comes from a separate regression. All regressions include a control for the policy dummy -- which takes the value of one for cohorts born after 1978 -- and split linear polynomials around the cutoff in the running variable (month-year of birth). In addition, all regressions include dummies for the birth month, dummies for various levels of the father's and mother's educational attainment, and dummies for rural/urban status and the governorate of birth-place. Clustering is done at the month-year of birth level. Statistically significant: \*\*\* 1 percent level; \*\* 5 percent level, \* 10 percent level.

# Online Appendix

Figure A1: Histogram and Estimated Density of the Running Variable

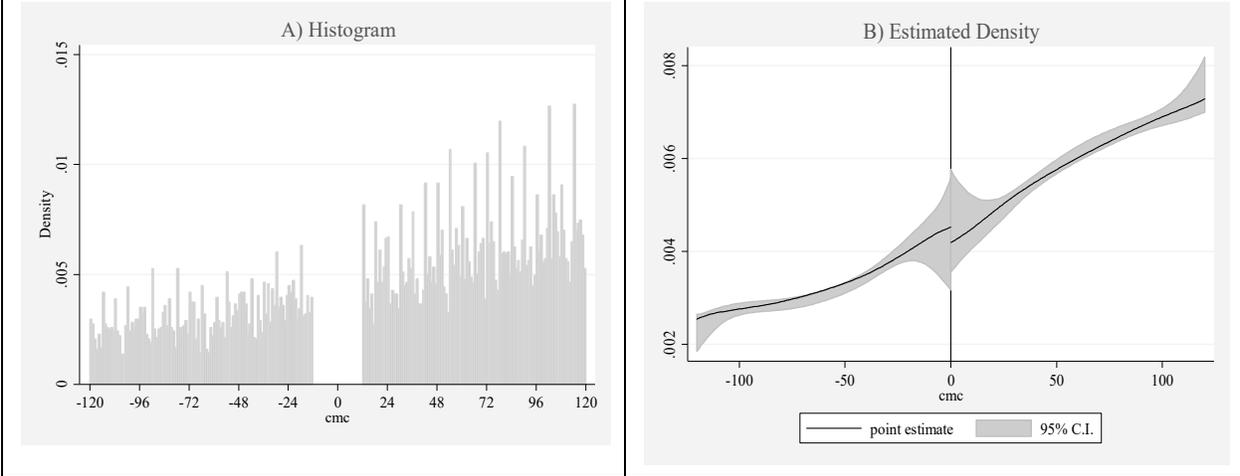


Table A1: Check of Discontinuity at the Cutoff for Other Covariates

	(1)	(2)	(3)	(4)	(5)	(6)
		RD Effect	p-value		RD Effect	p-value
Month of Birth				Urban Birth Place	0.023	0.361
January		0.120	0.295	Governorate of Birth Place		
February		0.018	0.827	Cairo	-0.002	0.866
March		0.017	0.851	Alexandria	0.004	0.720
April		-0.002	0.978	Port-Said	0.003	0.277
May		0.000	0.999	Suez	0.000	0.952
June		-0.028	0.698	Damietta	-0.003	0.696
July		-0.008	0.954	Dakahlia	0.010	0.350
August		-0.002	0.977	Sharkia	0.004	0.746
September		-0.006	0.941	Kalyoubia	0.007	0.425
October		-0.048	0.589	Kafr-Elsheikh	-0.008	0.424
November		-0.021	0.806	Gharbia	0.014	0.194
December		-0.040	0.662	Menoufia	0.011	0.151
Father's Educational Attainment				Behera	0.000	0.959
Illiterate		0.026	0.322	Ismailia	-0.001	0.920
Literate with no Degree		-0.004	0.801	Giza	-0.003	0.691
Primary School		-0.007	0.619	Beni-Suef	0.000	0.964
Lower Secondary School		-0.009	0.391	Fayoum	-0.016	0.056
Upper Secondary School		-0.008	0.088	Menia	-0.003	0.771
College		0.004	0.722	Asyout	-0.020	0.126
Post-Graduate		-0.001	0.490	Suhag	0.013	0.166
Mother's Educational Attainment				Qena	-0.002	0.834
Illiterate		-0.013	0.525	Aswan	-0.002	0.857
Literate with no Degree		0.004	0.714	Luxur	-0.004	0.227
Primary School		0.008	0.405	Red Sea	-0.002	0.124
Lower Secondary School		-0.005	0.587	El Wadi El-Gidid	0.000	0.692
Upper Secondary School		0.003	0.294			
College		0.003	0.703			
Post-Graduate		-0.001	0.619			

Notes: The data come from the 2012 Egyptian Labor Market Panel Study. The sample is restricted to 10-year intervals on each side of the birth-year cutoff, 1968-1987 birth cohorts, excluding the double-batch cohorts of 1977 and 1978. Individuals in these birth cohorts are 24- to 44-year-olds. The dependent variable is the pretreatment covariate given in columns (1) and (4). All regressions include a control for the policy dummy -- which takes the value of one for cohorts born after 1978 -- and a split linear polynomial (according to the cutoff) in the running variable (month-year of birth). Clustering is done at the month-year of birth level. Statistically significant: \*\*\* 1 percent level; \*\* 5 percent level, \* 10 percent level.

Table A2: Continuity-based Analysis for Alternative Cutoffs

*A) Sample is Restricted to the Right Hand Side of the Actual Cutoff*

	Location of the Alternative Cutoff relative to the Actual Cutoff in Months							
	+24	+36	+48	+60	+72	+84	+96	+108
Years of Schooling	0.465 [0.815]	0.456 [0.388]	-0.150 [0.289]	-0.131 [0.296]	-0.239 [0.415]	0.060 [0.779]	1.645 [1.209]	-1.461 [4.960]
Log Wages	-0.070 [0.126]	0.172** [0.080]	0.004 [0.078]	0.046 [0.092]	0.050 [0.108]	0.026 [0.158]	-0.096 [0.349]	-1.025 [0.628]

*B) Sample is Restricted to the Left Hand Side of the Actual Cutoff*

	Location of the Alternative Cutoff relative to the Actual Cutoff in Months							
	-24	-36	-48	-60	-72	-84	-96	-108
Years of Schooling	0.712 [1.055]	-0.537 [0.487]	-0.125 [0.446]	-0.080 [0.534]	-0.049 [0.670]	0.278 [0.978]	-0.851 [2.676]	-9.041 [5.539]
Log Wages	0.205 [0.254]	0.095 [0.133]	0.018 [0.123]	-0.015 [0.134]	-0.074 [0.203]	-0.014 [0.332]	-0.185 [0.584]	-2.176 [1.624]

Notes: The data come from the 2012 Egypt Labor Market Panel Survey. The sample is restricted to birth cohorts on the right hand side of the actual cutoff in panel (A) and to birth cohorts on the left hand side of the actual cutoff in panel (B). The double-batch birth cohorts of 1977 and 1978, which are immediately around the cutoff, are excluded. Alternative cutoffs are taken as given in column headings, by gradually shifting the actual cutoff to the right in panel (A) and to the left in panel (B). Each cell comes from a separate regression. All regressions include a control for the policy dummy -- which takes the value of one according the alternative cutoff taken -- and split linear polynomials around the cutoff in the running variable (month-year of birth). In addition, all regressions include dummies for birth month, dummies for various levels father's and mother's educational attainment, and dummies for rural/urban status and the governorate of birth-place. Clustering is done at the month-year of birth level. Statistically significant: \*\*\* 1 percent level; \*\* 5 percent level, \* 10 percent level.

Table A3 - Policy Effect on Schooling Outcomes for Men and Women – Alternative Degrees of Polynomials

	Men			Women		
	(1)	(2)	No obs.	(3)	(4)	No obs.
A) Years of Schooling	-0.907*** [0.218]	-0.873** [0.394]	6,398	-0.903*** [0.308]	-0.911 [0.633]	6,080
B) Primary School Completion	-0.028 [0.018]	-0.025 [0.034]	6,638	-0.038 [0.024]	-0.001 [0.048]	6,370
C) Lower Secondary School Completion	-0.022 [0.021]	-0.017 [0.038]	6,640	-0.030 [0.027]	-0.001 [0.054]	6,374
D) Upper Secondary School Completion	0.015 [0.021]	-0.036 [0.037]	6,640	0.022 [0.029]	0.023 [0.056]	6,374
E) Lower Secondary School Completion conditional on Primary School Completion	0.004 [0.020]	0.003 [0.037]	5,458	0.008 [0.024]	0.000 [0.046]	4,584
F) No Schooling	-0.001 [0.014]	-0.038 [0.027]	6,398	0.007 [0.023]	-0.009 [0.047]	6,080
G) Illiterate	0.005 [0.017]	-0.007 [0.034]	6,640	0.025 [0.023]	0.011 [0.047]	6,378
Degree of Split Polynomials	First	Second		First	Second	

Notes: The data come from the 2012 Egyptian Labor Market Panel Study. The sample is restricted to 10-year intervals on each side of the birth-year cutoff, 1968-1987 birth cohorts, excluding the double-batch cohorts of 1977 and 1978. Individuals in these birth cohorts are 24- to 44-year-olds. The dependent variable is given in panel headings (A) to (G). Each cell comes from a separate regression. All regressions include a control for the policy dummy -- which takes the value of one for cohorts born after 1978 -- and a split polynomial (according to the cutoff) in the running variable (month-year of birth) whose order is specified in the last row of the table. In addition, all regressions include dummies for the birth month, dummies for various levels of the father's educational attainment, and dummies for rural/urban status and the governorate of birthplace. Clustering is done at the month-year of birth level. Statistically significant: \*\*\* 1 percent level; \*\* 5 percent level, \* 10 percent level.

Table A4 – Policy Effect on Wage Employment – Alternative Degrees of Polynomials

	(1)	(2)	No obs.
A) Wage Employment	0.039* [0.023]	-0.010 [0.043]	6,573
B) Full Time Wage Employment	0.038 [0.025]	-0.013 [0.048]	6,573
C) Formal Wage Employment	0.017 [0.025]	-0.018 [0.044]	6,573
Degree of Split Polynomials	First	Second	

Notes: The data come from the 2012 Egyptian Labor Market Panel Study. The sample is restricted to 10-year intervals on each side of the birth-year cutoff, 1968-1987 birth cohorts, excluding the double-batch cohorts of 1977 and 1978. Individuals in these birth cohorts are 24- to 44-year-olds. The dependent variable is given in panel headings (A) to (C). Each cell comes from a separate regression. All regressions include a control for the policy dummy -- which takes the value of one for cohorts born after 1978 -- and a split polynomial (according to the cutoff) in the running variable (month-year of birth) whose order is specified in the last row of the table. In addition, all regressions include dummies for the birth month, dummies for various levels of the father's educational attainment, and dummies for rural/urban status and the governorate of birthplace. Clustering is done at the month-year of birth level. Statistically significant: \*\*\* 1 percent level; \*\* 5 percent level, \* 10 percent level.

Table A5 - Returns to Schooling for Men – Alternative Degrees of Polynomials

	(1)	(2)	(3)	(5)
	OLS-RDD	IV-RDD	IV-RDD	No Obs.
	0.019*** [0.002]	0.038 [0.045]	0.029 [0.064]	4,586
Degree of Split Polynomials	Second	First	Second	
First-Stage Results		-1.024*** [0.285]	-1.444*** [0.511]	
F-Statistic		12.903	7.987	

Notes: The data come from the 2012 Egyptian Labor Market Panel Study. The sample is restricted to 10-year intervals on each side of the birth-year cutoff, 1968-1987 birth cohorts, excluding the double-batch cohorts of 1977 and 1978. Individuals in these birth cohorts are 24- to 44-year-olds. The dependent variable is log hourly wages. Years of schooling is the endogenous variable, which is instrumented by the policy dummy -- which takes the value of one for cohorts born after 1978 and zero otherwise. All regressions include a polynomial in the running variable (year of birth) whose order is specified in the last row of the table, dummies for the birth month, dummies for various levels of the father's and mother's educational attainment, and dummies for rural/urban status and the governorate of birthplace. Clustering is done at the month-year of birth level. F-statistic is adjusted for the number of clusters. Statistically significant: \*\*\* 1 percent level; \*\* 5 percent level, \* 10 percent level.