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Abstract

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JEL Classification: I21, I25, J24, O55

Keywords: School Costs, Education Investment, Gender bias, Female Labor Market, Marriage, Bride Price, Egypt

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Less School (Costs), More (Female) Education?
Lessons from Egypt *Reducing* Years of Compulsory Schooling[1]

Ahmed Elsayed and Olivier Marie

September 2021

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1 Introduction

Investment in human capital is essential for economic growth and to escape from poverty (Becker 1995). Nonetheless, family decisions about how much to invest in education in developing countries are often constrained by the high costs of schooling. These include direct expenses (such as school fees, materials, transportation, uniforms, etc.) and opportunity costs, in income foregone when children are not available to work at home or in the labor market. For girls, another option that is delayed by continuing education is marriage and leaving the household. The poorer the household, the higher the opportunity costs of children's education, because credit-constrained households have less ability to smooth their consumption over longer periods of time (e.g. Rose 2000; Kingdon 2005; Maccini and Yang 2009; Lochner and Monge-Naranjo 2012; Barcellos et al. 2014). The strong preference for boys that prevails across many developing countries also implies that daughters' education is more negatively affected by credit constraints than sons' (Jayachandran 2015; Evans et al. 2020). In such contexts, policies designed to reduce the costs of schooling for all may ultimately benefit girls more than boys (Glick 2008; Evans and Yuan 2019).

In this paper, we investigate the effect of reducing the costs of completing compulsory schooling on households' investments in their children's human capital in a developing country with a strong preference for sons. The identification of the causal relationship comes from a rather unusual policy change in Egypt's pre-university education system at the end of the 1980s, when the number of years of compulsory schooling was reduced from nine years (six in the primary stage plus three in the preparatory stage) to eight (five in the primary stage plus three in the preparatory stage). The curriculum would be compressed but in theory was otherwise the same. The policy was a response to a continuous increase in the number of students enrolled in education, which forced the over-stretched public educational system to run schools on a daily-shift basis to accommodate all students. One aim of the policy was to reduce the financial demands on the public educational system.

To evaluate the causal impact of the policy, our empirical strategy exploits the unplanned staggered roll-out across schools. For this, we use a rich dataset from the Egypt Labor Market Panel Survey (ELMPS), which gives detailed information on the education path of individuals, as well as their labor market and living conditions. Importantly, the data contains information on which school individuals had attended during the different stages of their education, and – for a subset of individuals – whether they received compulsory schooling under the old or new regime (i.e. actual treatment). We use this information to assess the timing of take-up of the policy for each school, to assign treatment at the cohort-school level and implement a staggered difference-in-differences identification approach, which we validate extensively.

We estimate the effect of the policy on subsequent educational attainment, as well as longer-term outcomes in the labor market and marriage. It is difficult to predict *a priori* the overall impact of the policy. On the one hand, the policy made it cheaper for parents and easier for students to complete the compulsory years of education, to finish secondary school and to achieve a diploma. This could encourage families to aim higher for their children, especially when stage completion is associated with high returns in both the labor and marriage markets. Finishing compulsory education early could also push some students to pursue further education if they are perceived as too young for the labor market. On the other hand, the reduction in schooling years could result in a situation where graduates lack important skills that would enable them to succeed in higher education and afterwards in the labor market.[2]

We find evidence that compressing compulsory schooling into a shorter period was effective at reducing the dropout in the basic education stage. Prior to the policy, only 65% of students completed their compulsory education. The policy raised this share by about 7 percentage points in our preferred specification to 72%. Of more interest, we show that the impact did not stop at the compulsory stage: the share of those who enrolled in and finished secondary education (compulsory schooling plus three years) increased in response to the policy. Prior to the policy, the share of students who finished secondary education was 60% and the policy increased this share by about 6 percentage points. This resulted in an overall increase in the age at which students leave school of about 0.56 years, an increase of about 4% over the average age of fifteen before the policy. The effect is robust to several checks and mainly driven by individuals from rural areas and those from poorer families. This suggests that the lower cost achieved by the policy is the main reason for the positive impacts observed.

We further show that there is a strong gender dimension and that the impact is mostly experienced by girls. This is in line with the literature highlighting that gender-neutral policies often affect women more than men (e.g., Glick 2008; Evans and Yuan 2019). This can be particularly expected in a country with strong preference for sons,

[2] Egypt does not have a compulsory schooling age. As will be explained in more detail in Section 2, the rule prior to the policy was to successfully finish nine grades of basic education. However, this rule was not enforcd.

such as Egypt. In such contexts, households invest in the human capital of boys or girls to maximize expected utility, with the male bias prompting them to anticipate higher returns for their sons (Rosenzweig and Schultz 1982). They consequently invest – in the (pre-policy) steady state – relatively less in their daughters, and since the inequality is generated through budget constraint, relaxation of the constraint can potentially lead to reductions in the existing bias (Gaudin 2011). We confirm this by showing that girls from rural areas and poorer households benefit the most from the policy in terms of improved education outcomes. This provides strong evidence that the policy reduced gender disparities especially among the disadvantaged groups, with those families most likely to be credit constrained in terms of education investment.

In the long run, we also find that the policy had strong positive impacts on labor market and marriage outcomes. It increased the probability of employment, improved job quality and raised wages, especially for women, who fare much worse in the Egyptian labor market than men. We also show that it significantly improved women's marriage quality as it reduced the probability of being married as a minor and increased the value of jewelry received (bride price) at marriage.

We explore alternative mechanisms that may explain our findings of improved educational and later-life outcomes from a policy that reduced the number of years of compulsory schooling, and in particular for poorer girls. One possibility is that reducing the quantity of compulsory education was compensated by an increase in the quality of the education that pupils received, which in turn would have increased their probability of remaining in school for longer. We test this by checking whether the policy changed education quality using self-reported measures on school interruptions, grade repetition and, crucially, test scores at all education levels (i.e. primary, preparatory, and secondary). We also use measures of whether the respondent had ever studied in single-shift classes, if they had ever used IT equipment at school, and if their teachers had ever used corporal punishment. We reject the hypothesis that any of these changed significantly and thus that education improvement could explain our findings. This leaves changes in investment decisions from previously credit-constrained families as the most likely mechanism, although it is still necessary to explain why it affected girls more.

Another mechanism we test is whether returns to education are markedly different across gender, with the completion of the secondary stage implying much higher returns for girls compared with boys. This is exactly what we find when we look at the probability of working and the wage received if working, with the increase for women being much more non-linear than for men at each educational stage. These non-linearities are also found in the bride price received and the probability of patrilocal residence at marriage (i.e., co-residence with the in-laws as opposed to independent place of residence). This indicates that they also exist in returns on the marriage market, which strongly supports why the reduced costs of reaching a certain education stage would affect girls more strongly than boys.

With this paper, we contribute to four strands of economic literature. The first deals with households' investment in the education of children and the significant role of the cost of education and credit constraints in hindering investment in children's schooling, especially in developing countries (e.g. Foster 1995; Jacoby and Skoufias 1997; Maccini and Yang 2009; Lochner and Monge-Naranjo 2012). The second strand focuses on gender differences and documents that women in several developing countries fare worse than men across several domains. This pattern of gender disparity is more pronounced in countries with strong "son preferences", where boys are breastfed longer (Jaychandaran and Kuziemko 2011; Chakravarty 2015), are more likely to be vaccinated or given vitamin supplements, and receive more childcare time and education (Barcellos et al. 2014; Choi and Hwang 2015). The literature shows that this pattern of gender bias is much more pronounced under credit and liquidity constraints (Rose 2000; Maccani and Yang 2009; Lafortune and Lee 2014). The third strand relates to a substantial body of economic literature measuring several outcomes related to changes in compulsory schooling age (e.g. Harmon and Walker 1995; Card 1999; Spohr 2003; Oreopoulos 2007; Brunello et al. 2009; Devereux and Harts 2010; Machin et al. 2011; Erten and Keskin 2018; 2019). This literature mostly investigates the impact of increasing the number of years of schooling and documents positive impacts on different outcomes later in life.[3] The fourth strand deals with long-term returns to education and the role that cultural context plays in human capital investment. While marriage is considered to be an important component of the returns to education, especially for women (Goldin 2006; Chiappori et al. 2017), cultural practices such as bride price, son preference, patrilocal residence, and polygamy have been shown to play an important role in households' decisions to invest in the human capital of their

[3] Besides gender-neutral policies, there is also a growing body of economic literature addressing girl-friendly policies in education (e.g. Burde and Linden 2013; Meller and Litschig 2015; Blimpo et al. 2016; Muralidharan and Prakash 2017).

children (Jacoby 1995; Levine and Kevane 2003; Tertilt 2005; 2006; Gaspart and Platteau 2010; Jayachandran and Pande 2017; André and Dupraz 2019; Ashraf et al. 2020). One example is the practice of bride price: according to Becker (1981), it not only compensates households for the future transfer of parent's property to children but also induces them to invest optimally in daughters' education if this investment entails high returns later in life. This practice explains the success of school construction programs in improving girls' education attainment where bride price traditions still exist (Ashraf et al. 2020).

Our paper is one of the first to investigate the effects of a policy to reduce the costs of education by reducing the number of years of schooling.[4] The findings suggest that cutting the costs of schooling induces households to invest more in children's human capital even when education is theoretically free, as it is in Egypt.[5] The impact is mainly driven by poorer families, suggesting that credit constraints are a major barrier to investment in education. Egypt is a unique context for studying such a policy, given the extreme preference for sons (Arnold 1997; Chakravarty 2015) and the unfavorable position of women, who are economically and socially disempowered.[6] The finding that the impact is mainly driven by poorer girls is strong evidence that reducing the cost of schooling reduces gender disparities, especially among disadvantaged groups. The positive long-term impacts on the labor market and marriage outcomes indicate that reducing the time it takes to reach a certain education stage could be a practical policy tool to improve female empowerment via education in the long run. This is especially relevant in contexts where: i) there is non-universal compulsory schooling compliance; ii) the state does not have the financial resources to significantly change this compliance; and iii) returns to girls' education are more non-linear than boys. Since these three

[4] Germany is an exception as it has recently reduced the number years of schooling as most states abolished the last year of secondary education. Evaluations of this policy change has shown that it had negative effects on math grades of the students in the final year of the secondary education (Büttner and Thomsen 2015), increased grade repetition (Huebener and Marcus 2017), delayed their enrollment at university and increased university dropout rates (Marcus and Zambre 2019). There are three major differences that distinguish our paper from the literature on the German case. First, the policy change in Egypt takes place early in the stage of compulsory education rather than towards the end of secondary stage, which enables us to study how households adjust their behavior over a longer period of time. Second, the implementation of the policy change in Germany is quite recent, which makes evaluating the impact on longer-term outcomes more difficult. Third, and more importantly, the implications regarding cost reduction of education are more relevant in the context of a developing country with strong gender bias. Relatedely, Pischke (2007) studies the impact of a the implementation of once occuring shorter school year in Germany in 1966-67 and finds some negative effects on education outcomes but none on longer term labor market ones for treated individuals.

[5] To compensate for education system deficiencies, families still need to incur education costs, although most of the costs come in the shape of the foregone earnings from keeping children at school (Assaad and Krafft 2015).

[6] The 2016 Global Gender Gap Report ranked Egypt 132nd out of 144 countries in terms of the relative disparities between women and men in the four areas of economic opportunity, educational attainment, political participation, and health survival (World Economic Forum 2016).

factors are all likely to be present in a large number of developing countries, we believe that our findings are relevant for a large number of poor young girls around the world.

The remainder of the paper is structured as follows. The next section explains the institutional background of pre-university education in Egypt and the policy change. Section 3 describes the data and provides descriptive statistics. Section 4 presents and validates the empirical strategy. Section 5 discusses the impact of the policy on education outcomes, and Section 6 estimates the longer-term outcomes in labor market and marriage. Section 7 sheds light on mechanisms, and finally Section 8 offers some concluding remarks.

2 Institutional Background

2.1 Egyptian School System

Pre-university education in Egypt has three levels: primary, preparatory, and secondary. Between the early-1980s and the reform that we study, there were nine years of compulsory (basic) education: the six grades of primary school and the three grades of preparatory school. Upon successful completion of these two education levels, students could opt into the secondary stage, which comprises two alternative tracks: the vocational (technical) track and the general secondary track, both of which take three years to complete. Children start school at the age of six, so most pupils would reach the post-compulsory secondary education stage by the age of fifteen. However, there was no strong enforcement mechanism to keep students at school until they finished the compulsory stage, and therefore there was a relatively high share – about one-third – of students who dropped out of school.[7]

The country's education system is characterized by a centralized top-down approach where the ministry of education oversees all general educational policies, chooses the curricula, and allocates funds and teachers to individual schools according to official enrolment counts (Hanushek et al. 2008). With the exception of religious

[7] The government could in theory attempt stricter enforcement of compulsory schooling, but it would come at a high price as it would incur not only costs from increased monitoring and punishment of non-compliant families but also substantial investment in the expensive process of building new schools and employing and training new teachers. Multiple examples of making cash transfers to families conditional on attendance (or of large regional investments in school buildings or teachers) do show that these are popular policy options (see for example https://www.povertyactionlab.org/publication/roll-call-getting-children-school for a review of the efficacy of such schemes). In Egypt at the time, however, the government did not have the political will to undertake such costly options to improve its educational system performance.

(Azhari) schools and some international schools – which jointly represent less than seven percent of the population of pupils in any given year – all public and private schools in Egypt are under the supervision of the ministry of education,[8] which supervises 27 directorates, one per governorate (province), which in turn oversee local education departments across districts.[9] Although the overall policy is set by the central ministry in Cairo, decisions on operational aspects such as allocating students to different schools within districts, the number of students enrolled in each school, capacity decisions in terms of number of students per class, etc. are left to local education authorities at the district level. This de facto partial autonomy to implement new national educational reforms is crucial for understanding why there was a staggered roll-out of the policy change that we present in the next section and exploit for our identification strategy.

2.2 Policy Change

Since the mid-1970s, Egypt has experienced unprecedented population growth, which led to a rapid increase in the number of students enrolling in compulsory education (Barro and Lee 2013). This placed huge pressures on the school system, which struggled to accommodate eligible students. Between 1980 and 1989, there was an increase of more than 50% in the number of pupils enrolled in primary schools (from 4.6 to 7 million students), which was not matched by an increase in public spending, as the Egyptian government spent less than 5% of GDP on education annually over this period.[10] One concrete example of how schools attempted to cope with this large influx of new students was to run classes on a daily two- or sometimes three-shift basis (Abdelkarim 2009). Against this backdrop, the ministry of education proposed – and introduced almost immediately – a radical policy change, reducing the number of years of compulsory schooling. From the beginning of the 1989-1990 school year, primary education lasted five instead of six years, reducing the total years of compulsory schooling – including the three years of preparatory education, which remained unchanged – from nine to eight years. Figure 1 summarizes the change in the structure of the pre-university education system brought about by this policy.

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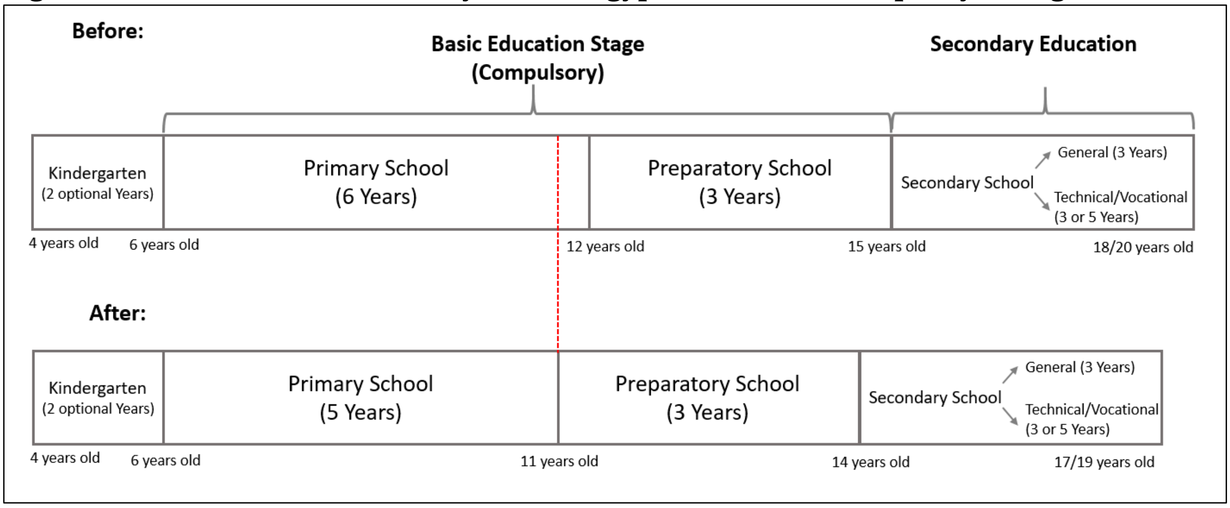
[8] For more detailed information on the structure of pre-university education in Egypt, see Elbadawy (2015) and Hanushek et al. (2008).

[9] *Shiakhas* in Arabic, with often just a single school in a village.

[10] According to the Unesco Institute of Statistics, the government expenditure on education as a percentage of GDP declined continuously from 5.6% in 1982 to 4.5% in 1988 and 1989 and further to about 4% in 1992 and 1993 (see: http://data.uis.unesco.org/). This could explain why, according to the annual book of statistics, the average class size across the country was constant over the years at around 43 students (see: www.capmas.gov.eg).

Figure 1: Structure of education system in Egypt before/after the policy change



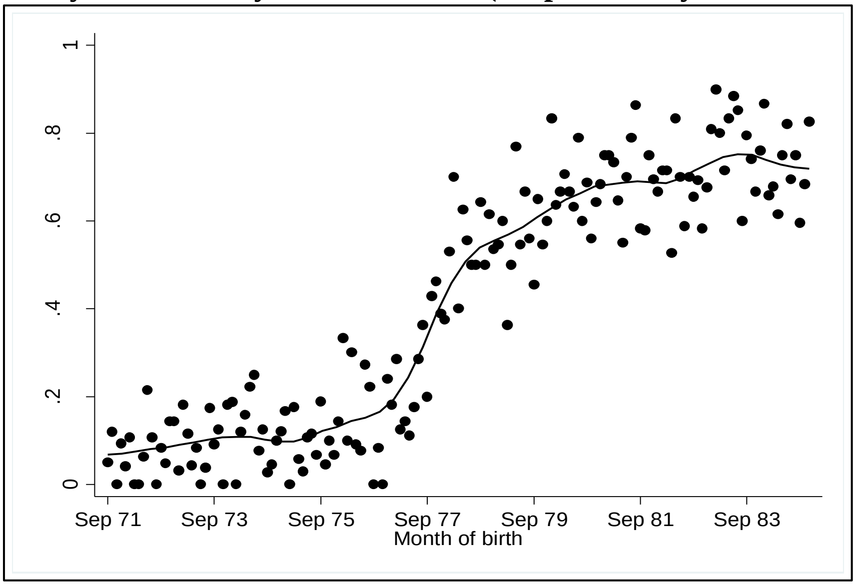
Note: The figure shows the pre-university education system before and after the policy. The vertical line shows the policy change.

According to the ministry of education, reducing the years of schooling would make it possible to admit more children into the over-stretched school system, as well as reducing pressure on class sizes in primary schools, and eventually reducing the number of schools operating in shifts (Eldahshan, 1992). It was also estimated that abolishing the final year of the primary stage of education would – at the very least – save millions of dollars a year on the cost of publishing the books for the sixth grade of primary school. The money could instead be spent on improving the education outcomes of students and enhancing the capacity of teachers (Eldahshan 1992). At the time, there was some public concern that reducing the number of years of compulsory schooling could have a negative impact on the quality of education, although the government argued that this would be avoided by preserving the core of the academic content taught within the five years of primary school. This would be achieved by removing some elements of the curricula that education experts deemed unnecessary. In addition the ministry of education suggested increasing the length of the academic year from 32 to 38 weeks, which – coupled with a longer school day (after cutting down on school shifts) – would maintain or even increase the total instruction time for compulsory education.[11]

[11] According to Abdelkarim (2009), the increase in the length of academic year has not been achieved. However, in theory, even if a school changed the academic year from 32 to 38 weeks each year and operated on a single shift (6 hours) instead of a double shift (5 hours each), this would lead to 800 hours of instruction a year before the policy and 1,140 hours per year thereafter. Over five rather than six years, this would lead to total instruction time being increased from 4,800 to 5,700 hours. Note that this would only apply for students who have completed all of their

The policy was passed into law in June 1988, and commenced with the new academic year in September (Abdelkerim 2009). The sixth year of the primary stage would be abolished immediately, beginning with pupils enrolled in grade 5 in the previous academic year.[12] All future cohorts would run on the new system of compulsory schooling comprising five years of primary and three years of preparatory education. Given the very sudden announcement of the policy, only a small number of schools managed to implement it as early as September 1988 and the rest delayed the start to the following – or even later – academic years. The ministry of education had to accept this situation and gave up on the idea of an immediate national roll-out, but instead insisted that the implementation should be rolled out each year until the last year of the primary stage was completely abolished in all schools (Abdelkarim 2009).[13]

Figure 2: Proportion of students who received five (rather than six) years of primary education by month of birth (i.e. probability of treatment)



Note: The probability of treatment (i.e. having five years of primary education instead of six) for each month of birth using the question on actual treatment in ELMPS 2012

primary education under the new system in a school with a single shift, reflecting two conditions that would take some time to be met.

[12] Students who were in the fifth grade of that year would 'skip' the last year of primary education and join their peers in the sixth grade to integrate together the first year of preparatory education. The pupils of these two cohorts would complete jointly through compulsory education despite the difference in years of schooling and age, and this 'double cohort' would arrive simultaneously at later stages of education and then into the labor market. We have looked closely into potential general equilibrium effects that this could have had in terms of outcomes. Since the policy was eventually implemented at the school level and not nationally as originally planned, we could not detect any specific impact on outcomes for these particular individuals. Including a 'double cohort' dummy in all our specifications does not change any of our results.

[13] In an interview with Al-Akhbar daily newspaper on July 7, 1990, the minister of education stated that the policy would be implemented every year for groups of schools until the sixth year of the primary stage was fully abolished (Abdelkerim 2009).

The Egypt Labor Market Panel Survey (ELMPS; described in detail in the next section) contains a question on actual treatment – i.e. having followed the five- rather than the six-year primary school curriculum – for a subset of individuals.[14] Figure 2 plots the percentage of individuals born every month between September 1971 and September 1984 who give an answer to this question. We first observe a strong increase in the proportion treated starting with individuals born in 1977, who turned 11 years old in the 1988-89 school year (note that treatment for older individuals can be mostly attributed to grade repeaters in primary school). Second, crucially we observe that the probability of treatment continues to increase relatively smoothly in all subsequent months of birth as more schools change to five years of primary education, until it plateaus at two-thirds of a cohort being treated. This confirms that the introduction of the policy was staggered across schools, and guides our identification strategy for obtaining causal estimates of its effects on individual outcomes.[15]

3 Data and Descriptive Statistics

3.1 Dataset and sample

To investigate the impact of this policy on short- and long-run outcomes, we use two waves – 2006 and 2012 – of the ELMPS, a large nationally-representative panel survey that collects detailed information on the family background, educational performance, labor market, and marriage conditions of individuals. Crucially, the ELMPS contains several retrospective questions on education history for those who had already left school at the time of interview, as well as past and current labor market experiences (OAMDI 2016). We focus on individuals who are likely to have been affected by the policy change, limiting our sample to those born between September 1971 and September 1984 – six years before and seven years after its announcement – for whom

[14] Given the absence of administrative data on exact timining of policy implementation across schools, this variable will be used to assign treatment status across different cohorts within schools. Section 3.2 explains in detail how the assignment is made.

[15] Earlier literature assumed that the policy was introduced to all students in a birth cohort simultaneously and consequently applied a cohort-based regression discontinuity (RD) approach to estimate its reduced form effect on female labor market participation and fertility outcomes (e.g., Ali and Gurmu 2018). We show in Figure A1 of the Online Appendix that bigger schools, schools located in urban regions, those with larger proportion of girls and large proportion of students with uneducated mothers, and those that adopted corporal punishment as a practice, introduced the policy sooner. This is indicative of the non-random delays in the policy implementation at school level which is further evidence that a cohort RD approach is not suitable for studying this policy. The within-school staggered difference-in-difference approach we take should, in contrast, account for potential school level selection into treatment on observable and unobservable characteristics.

we have complete information on education and background characteristics.[16] This leaves us with a population of 8,746 survey respondents (4,041 women and 4,705 men) for our statistical analysis.

3.2 Assignment to treatment

The 2012 wave of the ELMPS includes the question on treatment (i.e. whether an individual's primary education was on a five- or six-year basis), which we used to show the temporal variation in the implementation of the policy (in Figure 2 above). This question was only asked to new survey respondents in the 2012 wave or those re-interviewed that year who indicated updates in their education since the 2006 wave. This raises two issues: first, this information is missing for a substantial number of individuals (62% of the sample); and second, it may not be missing randomly since education updates between waves may correlate with other individual characteristics. To deal with these two issues, we use the detailed information on the primary school attended and match it by exact name across waves and the year of birth to create a school-cohort level treatment variable. We assign treatment to an individual if half or more of respondents from their birth cohort (and following cohorts) who attended the same primary school report having followed the five-year curriculum. We will use this school-cohort assigned treatment measure as the main measure for an individual being exposed to the policy throughout the rest of the paper.[17]

Using this assigned treatment variable and information on the location of each primary school, Figure A3 in the Online Appendix illustrates geographical disparities in the roll-out of the policy across districts. A district is considered to be treated if a

[16] We have this complete information for almost nine out of ten survey respondents of either wave. There are no significant differences on observable characteristics between those for whom we have complete information and those for whom we do not.

[17] We use this measure for our statistical analysis, although our results are robust to several sensitivity checks including different levels of threshold definition of the policy, e.g. 35% or 75%. They are also robust to limiting the analyses to individuals where at at least five students within schools answer the survey question before and after the policy introduction (see Table A2 in Online Appendix). There remains the issue of why some individuals answered not having been treated when most of their class-cohort peers indicated that they had been. We mostly attribute this to recall or measurement error, but we still want to check that this is not linked to any other observable individual characteristics that may bias our analysis. Figure A2 in the Online Appendix shows a balancing test for observable characteristics between individuals who were assigned to the category that they chose and those who were assigned to the opposite category. The figure clearly shows no evidence of differences in observable characteristics between the two categories and thus suggests that using this assignment treatment policy does not bias our results. We have this information for over 2/3rd of students. For the others, we match them to the district-cohort they belong to. Crucially, the likelihood to be missing a school identifier is not correlated with treatment probability. We still add an indicator variable to control for this particular group in all specifications and this does not change the results. Importantly, we also show in Table A2 that the results are not affected by taking the alternative approach of assigning treatment at the district-cohort – rather than school-distric – level for all individuals in our sample.

majority of its schools are treated in a certain year. The figure clearly shows that there are variations in the timing of policy implementation across districts.

3.3 Outcome variables

We are interested in exploring the impact of the policy on various education outcomes. We expect an almost mechanical positive effect on the probability of completing compulsory schooling as it is reduced from nine to eight years, and that most individuals could now complete compulsory schooling by the age of fourteen rather than fifteen. The impact of the policy on finishing secondary education is less clear, although one could also expect some increases in the proportion of pupils completing this stage, as it takes one less year to do so (finishing at seventeen rather than eighteen). Therefore, overall the most theoretically-ambiguous effect of this policy relates to our third education outcome, total years of schooling. One might expect that reducing the number of years of compulsory schooling would automatically lead to a reduction in the completed years of education. However, if the reform pushes a sufficient number of individuals who are on the margin of dropping out of the next phase of education to update their decision and complete it (as the costs are reduced), total years of schooling would increase. We test for this outcome using statistical analysis. To avoid confusion with the calculation of years of schooling based on the new (reduced) regime vs. the earlier regime, we report the estimates using the age of leaving school.

We also look at longer-term individual outcomes that might be affected by education, that are linked to the labor market situation and marriage quality (for women). For the former, this includes the probability that the individual has ever worked, as well as the probability that she is currently working. For those who are working, we consider indicators of job quality such as the probability that they are in paid employment, as well as whether the job is in the non-agricultural sector. Finally, we also consider the more standard effect that the education policy may have had on wages. Since female labor force participation remains very low in Egypt (less than one-quarter work among the cohorts that we study), for many women the quality of marriage is a more relevant outcome in terms of how our education policy may have affected their life situation. We consider various measures linked to marital age: the probability of underage marriage (before the legal age of eighteen), the actual age at the time of first marriage, and the probability that the age difference between the married couple is large (the husband being six years older, reflecting the top quartile of age difference).

We look at three further outcomes that are more specific to developing countries with traditional gender roles and a form of 'bride price'. The first outcome is patrilocal residence, estimated by the probability that the married couple co-reside with their in-laws (or parents) instead of having their own place of residence.[18] The second outcome is the value of the jewelry traditionally received by the bride from the groom's family at the moment of marriage in Egypt, the '*Shabka'* in Arabic.[19] This is closely related to the idea that Becker (1981) theorized, whereby a bride price influences human capital investment in girls. This has been empirically tested by Ashraf et al. (2020), who show that the practice of bride price can be crucial to understanding heterogeneities in the effects of education policies in other contexts. Here, we will use the actual monetary amount reported by all married women as a proxy for value on the marriage market. The third outcome is intra-household decision-making, which is estimated by asking women whether they usually have a say in making a number of different household decisions. Table A1 in the Online Appendix provides a detailed list of these decisions. For each item, women are assigned a value of one if they make the decision on their own or together with the husband (or the family if they are not married), and zero otherwise. Following Duflo et al. (2007) and Kling et al. (2007), we use an aggregate index constructed by grouping the 10 decision-making items. We construct this index by averaging the z-scores of the equally weighted underlying measures. The z-scores are calculated by subtracting the control group's mean and dividing by the control group's standard deviation. Therefore, for the control group in our sample, each item in the intra-household decision making index has a mean of zero and standard deviation of one. This has the advantage of reducing the likelihood of type I (i.e., the result for any single item could be due to chance) and type II errors (i.e., risk of low statistical power).

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[18] Despite the potential economic advantages that co-residing with one's parents-in-law may bring when they contribute to household income or share housing and other assets, the literature shows that this form of housing formation usually implies a lesser amount of freedom for the wife as the co-residing in-laws can impose their preferences and expect the daughter-in-law to take part in the housekeeping tasks. This could have negative implications for women's social and economic empowerment (see for example, Chu et al. 2014; Ebenstein 2014; Grogan 2013; Landmann et al. 2018).

[19] We use it as a measure of bride price as it represents a major cost of marriage that the groom and/or his family transfer to the bride at marriage. It is a substantial cost and amounts to about one year's salary. Another item is `*Mahr'*, which is the money paid by the husband and/or his family to the bride. However, unlike Shabka, this part is not essential for every marriage. There are different practices including giving up Mahr and making an equivalent contribution towards establishing the newly-formed household (e.g. buying a house, furniture, etc.) or jewelry for the wife (Shabka). While only 40% of women in our sample reported having received Mahr at marriage, 99% reported having received Shabka. For details on the practice of bride price across different societies, see for example Anderson (2007).

Table 1: Descriptive statistics for untreated school cohorts

Means (Standard Deviation)	(1) All	(2) Women	(3) Men	(4) Rural	(5) Urban	(6) Low-income father	(7) High-income father
Education outcomes							
Finish compulsory education	0.65	0.60	0.69	0.51	0.78	0.59	0.77
	(0.48)	(0.49)	(0.46)	(0.50)	(0.42)	(0.49)	(0.42)
Finish secondary education	0.60	0.55	0.64	0.46	0.72	0.54	0.72
	(0.49)	(0.50)	(0.48)	(0.50)	(0.45)	(0.50)	(0.45)
Age left school	15.01	14.20	15.68	13.43	16.43	14.28	16.41
	(4.66)	(5.18)	(4.08)	(5.12)	(3.67)	(4.94)	(3.70)
Labor Market outcomes							
Ever worked	0.66	0.31	0.95	0.64	0.68	0.65	0.68
	(0.47)	(0.46)	(0.21)	(0.48)	(0.47)	(0.48)	(0.47)
Employed	0.62	0.24	0.93	0.61	0.63	0.61	0.63
	(0.49)	(0.43)	(0.25)	(0.49)	(0.48)	(0.49)	(0.48)
Paid job	0.58	0.18	0.91	0.55	0.60	0.56	0.62
	(0.49)	(0.38)	(0.29)	(0.50)	(0.49)	(0.50)	(0.49)
Non-agricultural job	0.87	0.83	0.88	0.77	0.96	0.82	0.97
	(0.33)	(0.37)	(0.32)	(0.42)	(0.20)	(0.39)	(0.17)
Log wage	3.09	2.17	3.34	2.78	3.32	2.92	3.37
	(1.21)	(1.56)	(0.97)	(1.35)	(1.04)	(1.33)	(0.94)
Marriage outcomes (women)							
Underage marriage (Before 18)	-	0.15	-	0.22	0.08	0.17	0.11
	-	(0.36)	-	(0.42)	(0.27)	(0.38)	(0.31)
Age at marriage	-	22.05	-	20.83	23.20	21.54	23.03
	-	(4.38)	-	(4.25)	(4.20)	(4.33)	(4.31)
Large age difference	-	0.11	-	0.11	0.11	0.11	0.11
	-	(0.31)	-	(0.32)	(0.31)	(0.31)	(0.32)
Patrilocal residence	-	0.31	-	0.44	0.18	0.35	0.21
	-	(0.46)	-	(0.50)	(0.38)	(0.48)	(0.41)
Bride price (Log jewelry std.)	-	0.06	-	0.03	0.09	0.01	0.14
	-	(0.99)	-	(1.10)	(0.86)	(0.98)	(0.99)
Intra-HH decision-making index	-	0.02	-	-0.16	0.18	-0.06	0.16
	-	(1.02)	-	(1.10)	(0.91)	(1.07)	(0.90)
Independent variables:							
Female	0.45	1.00	0.00	0.45	0.44	0.45	0.44
	(0.50)	(0.00)	(0.00)	(0.50)	(0.50)	(0.50)	(0.50)
Rural	0.47	0.48	0.47	1.00	0.00	0.57	0.28
	(0.50)	(0.50)	(0.50)	(0.00)	(0.00)	(0.49)	(0.45)
Low-income father	0.65	0.66	0.65	0.80	0.53	1.00	0.00
	(0.48)	(0.47)	(0.48)	(0.40)	(0.50)	(0.00)	(0.00)
Educated Mother	0.21	0.21	0.21	0.08	0.33	0.14	0.34
	(0.41)	(0.41)	(0.41)	(0.27)	(0.47)	(0.35)	(0.47)
Number of siblings	4.61	4.74	4.50	5.12	4.15	4.79	4.26
	(2.22)	(2.26)	(2.19)	(2.20)	(2.14)	(2.23)	(2.17)

Note: Authors' calculations from ELMPS dataset. See Table A1 in the Online Appendix for definitions of these variables.

3.4 Descriptive statistics

Table 1 reports descriptive statistics – with means and standard deviations in brackets – for all variables used in our analysis at baseline for the untreated school cohorts. Column (1) is for the overall sample, Columns (2) and (3) for women and men separately, and Columns (4) – (7) for different groups, which we will use later in our heterogeneity analyses. The table shows that less than two-thirds of students finished their nominally 'compulsory' education, and that this relatively low compliance rate was even lower for women (60%) and individuals from less-advantaged groups (e.g. with completion probabilities of about 0.5 among students from rural areas and 59% for children of low-income fathers). Two-fifths of students did not proceed to complete secondary schooling, again with similar patterns by gender and socio-economic background. These numbers highlight that most school dropout behavior in Egypt occurs during the compulsory education stage, and that the probability that a student finishes secondary education – conditional on completing compulsory education – is very high, with about 92 percent of pupils doing so. In terms of educational outcomes, we further see that while the average age of leaving school is about 15, it is 1.5 years higher for males (15.7) than females (14.2) and lowest for individuals in rural areas (13.4).

4 Empirical Strategy

To causally evaluate the impact of reducing the number of years of compulsory education on individual outcomes, we exploit the school differences in the timing of implementation of the policy and estimate variations based on the following equation:

$$Y_{ics} = \beta \cdot Treat_{ics} + \delta X_{ics} + \alpha_s + \gamma_c + \varepsilon_{ics} \qquad (1)$$

where Y_{ics} is one of several outcomes of interest (i.e. education, labor market, and marriage) for individual i in cohort c in school s. The treatment variable is $Treat_{ics}$, which is a dummy variable equal to one for individuals from a particular cohort c assigned the treatment status in school s (i.e. individuals from the first cohort within a school, and all following ones, where at least 50% of individuals reported having followed five years of education), and zero otherwise. X_{ics} is a set of controls for individual characteristics. These are namely a dummy variable for gender; a dummy variable that takes the value of 1 if the father is uneducated (i.e. cannot read and write), and zero otherwise; a dummy variable that takes the value of 1 if the mother is uneducated (i.e. cannot read and write), and zero otherwise; a dummy variable that takes the value of 1 if the father was in a low-paying job (below median) at the respondent's age of 15, and zero otherwise; a dummy variable that takes the value of 1 if the mother was working at the respondent's age of 15, and zero otherwise; number of siblings, dummies for month of birth; a dummy variable for the double cohort (the two cohorts between the time of implementation), and a dummy variable that takes the value of 1 if the primary school identifier is missing (replaced then by district), and zero otherwise. ε_{ics} is the error term, which is assumed to be independent and normally distributed across individuals i, and which is clustered at the school level. The results are robust to other levels of clustering such as district or governorate-cohort dummies.[20] Given that we estimate coefficients for a relatively large number of outcome variables (related to education, labor market, and marriage) which use the same source of variation, we correct p-values for inference by the number of tests. For this purpose we estimate the sharpened q-values, which refers to the p-values adjusted for the multiple hypothesis testing suggested by Anderson (2008) and these are presented in each results table.

We control for cohort (γ)- and school (α)-specific fixed effects. The inclusion of these two-way fixed effects means that in practice we compare outcomes for students within the same school across cohorts pre- and post-policy introduction.[21] In an additional specification we also account for the interaction between governorate and cohort dummies given that governorate is the administrative unit level in Egypt at which many education (and other policy) decisions that we may not observe could affect both treatment and outcome groups non-randomly.

[20] Table A2 in the Online Appendix reports different robustness checks including different levels of clustering. We discuss these robustness checks in more detail in Section 5.1

[21] As indicated in the recent literature on staggered difference-in-differences designs (e.g., de Chaisemartin and D'Haultfoeuille 2020), linear regressions with period and group (school) fixed effects estimate a weighted average of treatment effects, where some of the weights could be negative. These negative weights occur in situations when the treatment effect is heterogeneous over time. de Chaisemartin and D'Haultfoeuille (2020) developed a test to estimate how serious an issue this could be and developed an estimator that is valid under these conditions: the Wald-TC, which is the LATE for the switchers (i.e. schools that change treatment status, compared to the yet-to-be-treated) and provide a Stata command for this estimator in Chaisemartin and D'Haultfoeuille (2020). In our case, only 23% of the weights across school and cohort groups are negative. The negative weights sum to -0.25 and the ATT may be of opposite sign to the estimated coefficient if the standard deviation of the ATEs across all the treated (school, cohort) cells is equal to 0.072. This suggests that the issue of negative weights is not a major concern in our exercise. However, as a robustness check we estimated the model using the technique of Chaisemartin and D'Haultfoeuille, and – as expected – the outcomes are qualitatively similar.

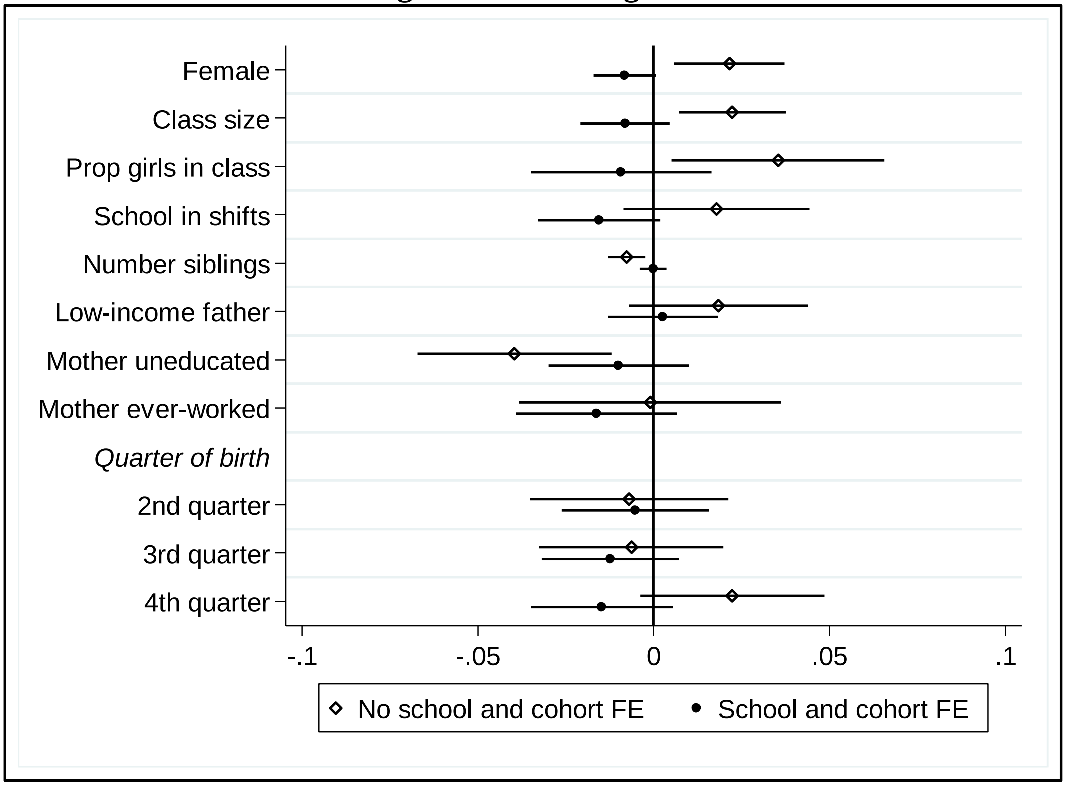
The fundamental threats to identification in the context of this staggered difference-in-differences approach stem from two potential sources of selection into treatment, namely at the (i) individual and (ii) school level. First, if students who received five rather than six years of primary education are different on observable (or unobservable) characteristics, the impact estimated could be biased by these differences. This is unlikely to be the case as the policy was implemented for students who had already been enrolled within their local school from a specific cohort – an event that was not predictable by parents at the time of birth – and it leaves little scope for parents to make strategic decisions to selectively avoid treatment.[22] The second, more serious identification threat comes from potential non-random school-level selection into treatment. The worry is not that early and late compliers are on average systematically different, since the within-school approach taken will eliminate this; rather, our main concern is that school-specific pre-trends could have affected both a school's timing of implementation and student outcomes. We address this dynamic school-level selection issue by estimating model specifications of equation (1) that include school-specific time trends (quadratic and cubic) in the robustness check in Table A2 in the Appendix. We will also show in the next section graphical evidence of the non-existence of school pre-trends using event-study variation of equation (1).

We can also show visually – in Figure 3 – the importance of including both time trends and school fixed effects in our context. Once these are considered, treatment probability appears to be as close to random as possible, since our balancing test rejects any statistical difference with respect to a large number of observable individual characteristics between treated and control students. Moreover, the F statistic for joint significance of the variables shown in the figure drops from 4.78 (*P*-value 0.00) in the model with no school and cohort FE to 0.93 (*P*-value 0.52) in the model with school and cohort FE, suggesting that these variables jointly play a minimal role in explaining who is exposed to treatment once school and cohort are taken care of. We believe that

[22] Parents of children who are already enrolled in schools cannot control the timing of implementation. Moving students to a school that had not yet implemented the policy could have been an option for some families who did not want their children to only receive five years of primary schooling. If there were multiple schools within the district where they lived, they could potentially have done so without moving to a different district. However, school and district policy implementation were very strongly correlated (0.85) and we show later (Table A2) that all our results are unchanged if we use district-cohort level as the definition for treatment. It would still have been possible for some to move to a school out of the residing district as a response to potential treatment. We check this by estimating whether changes in the probability of migration when a child was younger than 15 increased with the local school implementing the policy and find no evidence of this occurring (coefficient = -0.002; standard error = 0.013; mean probability of migration = 0.161). We thus reject the possibility that strategic school change was a phenomenon that could bias the policy estimates.

this is already strong evidence validating our staggered difference-in-difference approach although we will address this school-level pre-trend issue again, later in the paper.

Figure 3: Balancing test



Note: The figure displays the estimated coefficients and 90 percent confidence intervals of two separate regressions. The number of observations is 7,349 for whom complete information about school characteristics exist. The dependent variable is a dummy variable that takes the value of one if the individual is treated, and zero otherwise. The independent variables are displayed in the vertical axis. Standard errors clustered at the school level in both regressions.

5 Education Outcomes

5.1 Average Policy Impact

Pre-Trends? Before showing policy impact estimates from equation (1) we show, using a graph, that its introduction affected the education outcomes of cohorts around the time of its implementation within a school. For this purpose, we estimate a regression of education outcomes on a vector of dummy variables reflecting an individual's cohort distance k to the year of his/her school treatment. These are the φ coefficients obtained from equations (2) below for five pre- and post-policy introduction cohorts (with the reference cohort being k = -1). The model also includes school (α_s) and cohort (γ_c) FE as previously defined. v_{iks} is the error term clustered at the school level and is assumed to be independent and normally distributed across individuals i. This exercise – akin to taking an event study difference-in-differences approach – is also a good way to check visually whether we should still worry about school selection into treatment based on educational outcome pre-trends. Specifically, we estimate the following model:

$$E_{iks} = \sum_{k=-5}^{5} \varphi_k \cdot Cohort\_Treat_{iks} + \alpha_s + \gamma_c + v_{iks} \quad (2)$$

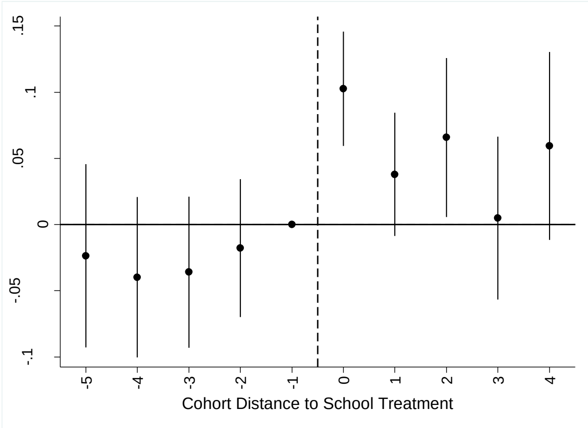
Figure 4 reports the estimates for the three education outcomes of interest: the probability of finishing compulsory education (Figure 4.1), the probability of finishing secondary education (Figure 4.2), and the age at which the student left school (Figure 4.3). The first thing clearly apparent is that there do not appear to be any pre-trends in education outcomes for the cohorts up to the year when a primary school introduced the shorter five-year curriculum. This confirms that it is reasonable to treat the policy as an exogenous shock to students across cohorts within school and thus further validates the staggered difference-in-differences approach. Once the policy is implemented, there appears to be a significant increase in the probability of finishing compulsory schooling for all subsequent cohorts, and of completing the next secondary education stage, which seems to lead to students leaving school later on average.[23]

We present two further pieces of graphical evidence – using the same approach – to check that the post-policy jump was not (i) driven by dynamic changes in other confounding factors or (ii) artificially pushed up by educational trends in these schools. We address the first issue by estimating equation (2) using a number of observable individual characteristics of students as outcome variables. Figure A4 in the Online Appendix clearly shows there are no significant jumps around the cut-off point for any of these characteristics. To address the second issue, we move the policy implementation within schools by five cohorts pre-treatment and publish placebo estimates on the same education outcomes. Figure A5 in the Online Appendix presents the coefficients from these placebo estimates and again clearly shows no pattern of any irregular trend or jump prior to the policy introduction.

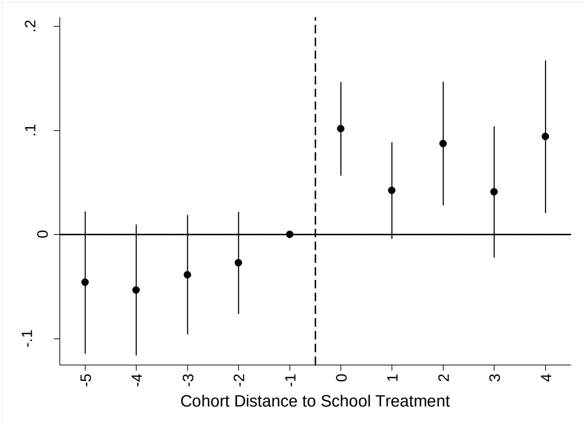
[23] There appears to be a somewhat stronger (although not statistically different) policy impact for the first treated cohort ($k = 0$) relative to later treated cohorts. One simple explanation for this is that the surprise introduction of the policy meant that families of the first-treated students reacted slightly more strongly than those who had more time to adapt to the new educational setting (i.e. potentially re-assessed optimal investment in the education of their children given the new structure of the education system).

Figure 4: Effect of the policy on cohorts relative to the time of implementation

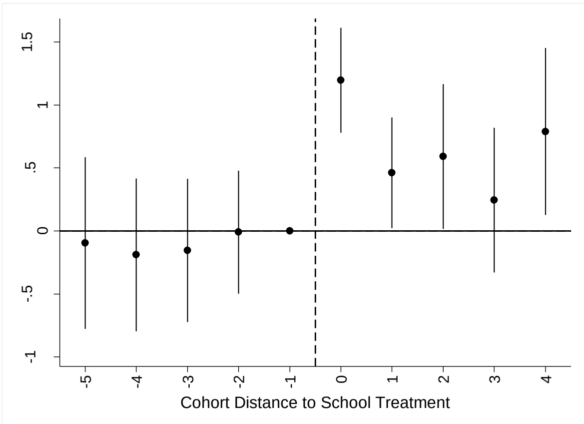
4.1: Probability of finishing compulsory education



4.2: Probability of finishing secondary education



4.3: Age left school



Note: Coefficient estimates and 90 percent confidence intervals for school-specific cohort dummies from Equation (2). Each point represents the coefficient of a school-specific cohort relative to the school-specific cohort -1. The dashed vertical line represents the implementation of the policy.

Estimates

Table 2 reports policy impact coefficients on education outcomes for various specifications of equation (1) for all individuals in our baseline sample. Panel A relates to the probability of finishing the preparatory stage of education (i.e. compulsory schooling), Panel B the probability of finishing secondary school, and Panel C the impact on the number of completed years of education (measured by the age at which the individual left school). Column (1) relates to the most basic model, which only accounts for school and cohort FE without controlling for the individual's background characteristics. Columns (2) adds the detailed set of control variables to the basic model. Column (3), as a robustness check, adds to the model an interaction term between governorate- and cohort-dummies to account for any governorate-specific time trends in education policies that could have affected cohorts within governorates differently. To benchmark the magnitude of the effects across the three model specifications, we report the policy impact as a percentage change relative to the control group means.

The estimated impacts are positive and statistically significant across the different model specifications. We focus on our preferred specification in column (2)[24] which suggests that the policy increased the probability of finishing preparatory education by 6.5 percentage points, corresponding to about a 10 percent increase from the average baseline completion rate of about two-thirds pre-reform. This is a strong but perhaps unsurprising impact given the mechanical effect that reducing the number of years it takes to complete a certain stage of education would have on the probability of doing so. More interestingly, this strong positive impact seems to have carried through to the next stage of education, as the probability of finishing secondary education increased by 5.7 percentage points (9.5% of an average of the 60% who finished secondary education prior to the reform).[25] The age at which individuals finish education increased by 0.56 years, corresponding to about a 3.8% increase over the average age of 15 years prior to the policy. We will show later in Section 7.1 that this is not driven by an increase in grade repetition for the treated relative to the control group.

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[24] We prefer specification 2 over 3 because it gives us sufficient power when we start looking at other outcomes with fewer observations, such as labor market and marriage outcomes for women (especially when we further split the sample across different observable characteristics to investigate channels). Therefore, we use this specification across the paper to be able to compare the results of the different model specifications to the baseline model. However, the results are robust to the inclusion of the interaction term between governorate and cohorts in the specifications with a sufficient number of observations.

[25] As explained above, most of the students who finish compulsory schooling proceed with secondary education, which has higher pay-off in the labor market. The reasons behind this pattern are discussed in Section 7.

Table 2: Policy impact on education outcomes

	(1)	(2)	(3)
Panel A - Finished Compulsory Education			
Treatment (5-year primary school)	0.066	0.065	0.068
	(0.019)	(0.018)	(0.019)
Sharpened *q*-value	[0.004)	(0.003)	(0.003)
Mean of Outcome	0.651	0.651	0.651
Percent increase	10.2	10.03	10.44
Panel B - Finished Secondary Education			
Treatment (5-year primary school)	0.060	0.057	0.058
	(0.019)	(0.019)	(0.019)
Sharpened *q*-value	(0.005)	(0.005)	(0.006)
Mean of Outcome	0.601	0.601	0.601
Effect size	9.92	9.53	9.66
Panel C - Age Left School			
Treatment (5-year primary school)	0.587	0.563	0.608
	(0.184)	(0.173)	(0.177)
Sharpened *q*-value	(0.005)	(0.005)	(0.003)
Mean of Outcome	15.013	15.013	15.013
Effect size	3.91	3.75	4.05
Controls	No	Yes	Yes
Cohort FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Governorate X Cohort FE	No	No	Yes
Sample size	8,746	8,746	8,746
Number of schools	2,563	2,563	2,563

Note: Controls include a dummy variable for gender, a dummy variable that takes the value of 1 if the father is uneducated (i.e. cannot read and write), and zero otherwise; a dummy variable that takes the value of 1 if the mother is uneducated (i.e. cannot read and write), and zero otherwise; a dummy variable that takes the value of 1 if the father was in a low-paying job (below median) at the respondent's age of 15, and zero otherwise; a dummy variable that takes the value of 1 if the mother was working at the respondent's age of 15, and zero otherwise; number of siblings; dummies for month of birth; a dummy variable for the double cohort (the two cohorts between the time of implementation); and a dummy variable that takes the value of 1 if the primary school identifier is missing (replaced then by district), and zero otherwise. Standard errors clustered by schools reported in parentheses. Sharpened *q*-values refers to the *p*-values adjusted for the multiple hypothesis testing suggested by Anderson (2008).

We will briefly consider the intuition behind this result of less compulsory schooling resulting in more completed years of education. Having the years of compulsory education reduced by one year will have reduced costs for families of pupils enrolled in a school at the time the policy was introduced, at least relative to students from non-treated cohorts. These students would have completed their compulsory education and commenced secondary school earlier than their parents anticipated. Some parents would have re-considered their decision about how much (how many years) to invest in their children's education. This would lead to more total years of completed education for the students affected by the policy. If this 'cost channel' is at play, we would expect a stronger impact for those on the margin of dropping out due to lower potential investment from their families in their education. We argue that this could be due to gender bias (girls more marginal) and/or financial constraints (poorer more marginal) and will investigate differences in policy effects across these groups in our heterogeneity analysis section.

Robustness

We check how robust these findings are by producing estimates of the policy impact on education when changing different elements of the baseline analysis. Table A2 in the Online Appendix reports the estimates of these robustness checks. First, we present in Panels A and B results using levels of clustering other than school level. Panel A reports the estimates obtained when we cluster at the district level, while Panel B shows those resulting from clustering at the governorate x cohort level. In both cases the standard errors do not differ much from those obtained when we use schools as a level for clustering, and therefore the statistical significance of the coefficients is not affected.

Panels C and D show estimates of model specifications which account for different forms of school time trends: quadratic in Panel C, and cubic in Panel D. The inclusion of these different forms of time-trends helps to account for the possibility that school-specific pre-trends could have affected both school's timing of implementation and student outcomes. The results show that this is not the case and that the estimates obtained in the baseline specification are robust to the inclusion of these school-specific time-trends. Panel E shows the estimates when the treatment is assigned at the district-cohort level (i.e., individuals in a district are assigned the treatment status when more than half of respondents born in the same year, and the years to follow, report having followed the five-year primary curriculum). The coefficient estimates are quite similar to those when treatment is assigned at the school-cohort level. This was likely to be the case as correlation between these two assigned treatments is 0.85, although it is still an important robustness check since it provides some first evidence that strategic student movement within districts is not an important issue for our identification strategy.

We then return to school-level assignment but change the threshold for the proportion of respondents who need to have answered positively to the treatment question in a school cohort for it to be considered as treated. Instead of one-half, we use either one-third or two-thirds as thresholds and this does not significantly change the magnitude of our results (in Panels F and G, respectively). Next, we limit the analyses to schools for which we have at least two treated and two untreated individuals. For this exercise we drop about 60 percent of schools from the sample (keeping 830 out of 2,634) but since these are the small-sized schools, we retain over three-quarters (keeping 6,810 out 8,945) of the students used in our original analysis. Using this sub-sample of more represented, larger schools produces a somewhat stronger policy impact (Panel D) than our baseline estimates. A similar pattern is obtained when we change the limit to even larger schools with at least five students on each side of treatment status (Panel E) despite the larger drop in the number of schools (only 410 remain out of 2,634) and the number of students (5,442 out of 8,945).

Finally, we run the analyses only for students who have reported 'actual treatment' status, i.e. the potentially selected group of individuals who answered the survey question on the five-year curriculum in the 2012 wave because they were new to the survey or had an education update since 2006. We do this first with a straightforward replication of equation (1) (Panel F) and by using the assigned treatment at the school-cohort level as an instrument for the actual reported treatment in an instrumental variable setup (Panel G, which also reports the first-stage estimates of the correlation between actual and assigned treatment). Reassuringly, despite the smaller sample across these different specifications, we still conclude to a similarly signifiant impact of the reform.

5.2 Heterogeneity in Policy Impact

Gender Differences

Given that there is a strong son preference in Egypt, one could expect a differential impact for boys and girls in terms of reducing the costs of completing compulsory schooling. It is not obvious a priori which gender will benefit most, as there are two basic mechanisms that compete: the preference for investment in boys could be so strong that all cost reduction benefits go towards their education, or there could be more girls with families on the margin of changing investment in their education, resulting in them being more strongly affected by this gender-neutral policy. If the latter effect dominates, we would see girls benefit more than boys from this change in education policy.[26] We test this by re-estimating equation (1) for two genders.

Table 3 presents these estimates for the three education outcomes of interest using our preferred specification. The main finding here is that the previously-observed average positive policy impact on education outcomes is more pronounced for girls than boys. While girls start from lower average schooling levels, they strongly improve their probability of completing both compulsory education and the next stage of three-year secondary education, which translates into almost one extra year of schooling under the new policy. This suggests that the savings from compressing the compulsory curriculum compression prompted families to re-assess education investment decisions for their daughters in particular, who may otherwise have left school at an earlier stage. If this cost channel for investing in the marginal girl is what drives these results, we would expect the impact to be more pronounced for all children – but especially daughters – from families that face credit constraints. We investigate heterogeneities on this margin.

Table 3: Policy impact on education outcomes – by gender

	Finish Compulsory Education		**Finish Secondary Education**		**Age Left School**	
	(1)	(2)	(3)	(4)	(5)	(6)
	Female	Male	Female	Male	Female	Male
Treatment	0.084	0.053	0.093	0.034	0.778	0.409
	(0.029)	(0.026)	(0.029)	(0.027)	(0.296)	(0.244)
Sharpened q-value	(0.004)	(0.576)	(0.002)	(0.703)	(0.009)	(0.576)
Mean of outcome	0.598	0.693	0.554	.638	14.20	15.68
Percent increase	14.01	7.61	16.77	5.32	5.48	2.61
Chow test *P*-value	0.000		0.000		0.000	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	4,041	4,705	4,041	4,705	4,041	4,705
Number of schools	1,640	1,654	1,640	1,654	1,640	1,654

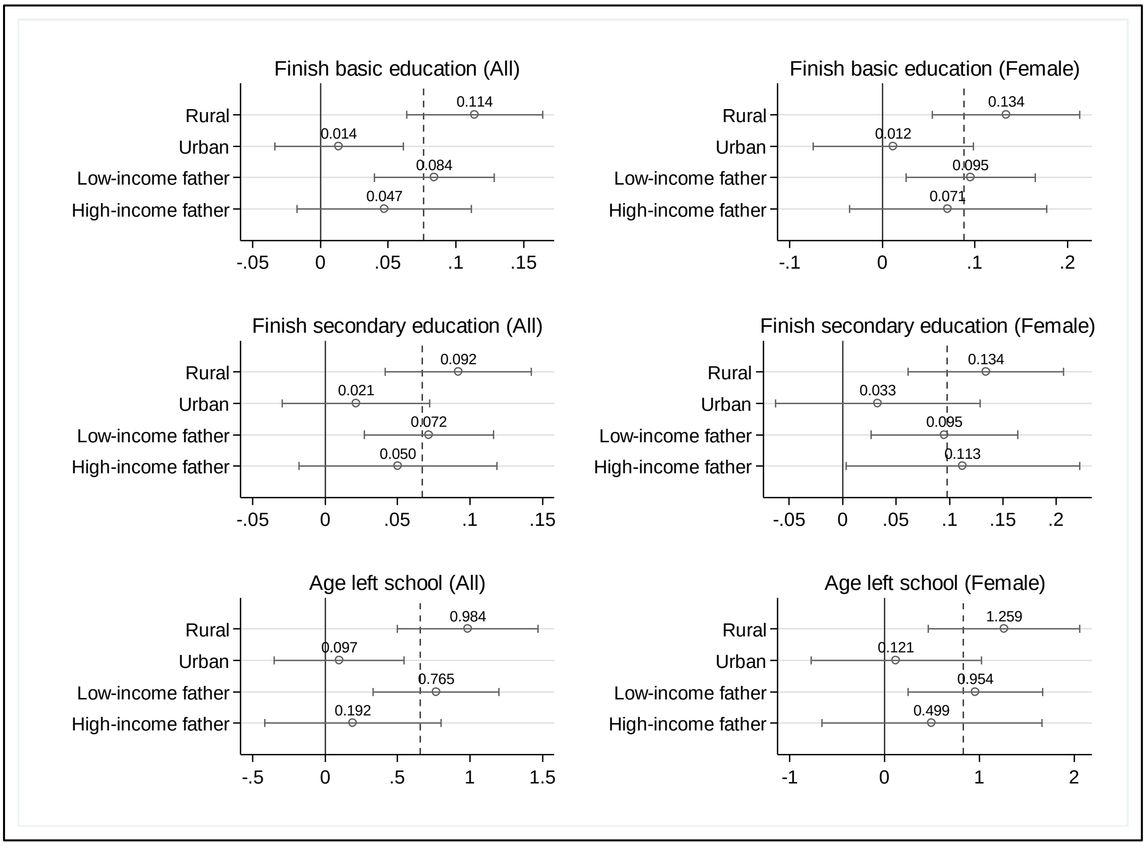
Note: Controls include a dummy variable that takes the value of 1 if the father is uneducated (i.e. cannot read and write), and zero otherwise; a dummy variable that takes the value of 1 if the mother is uneducated (i.e. cannot read and write), and zero otherwise; a dummy variable that takes the value of 1 if the father was in a low-paying job (below median) at the respondent's age of 15, and zero otherwise; a dummy variable that takes the value of 1 if the mother was working at the respondent's age of 15, and zero otherwise; number of siblings; dummies for month of birth; a dummy variable for the double cohort (the two cohorts between the time of implementation), and a dummy variable that takes the value of 1 if the primary school identifier is missing (replaced then by district), and zero otherwise. Standard errors clustered by schools reported in parentheses. Sharpened *q*-values refers to the *p*-values adjusted for the multiple hypothesis testing suggested by Anderson (2008).

[26] This effect could be compounded by a difference in returns to education by gender, especially when it comes to reaching certain stages of schooling, which we will discuss in our findings in Section 7.

Family Background Differences

We propose two ways to identify families that are likely to face relatively more financial constrains on their investments in their children's education, namely those from rural areas and those whose father had a below-median income.[27] If decisions about investment in education are driven by cost considerations, we would expect stronger impacts for those from families on the financial margin. We would expect this to be particularly true for daughters, about whom the decision might be even more strongly influenced by costs, due to the combined effect of prevalent gender bias and credit constraints.

Figure 5: Policy impact on education by family background and gender



Note: Coefficients and 90 percent confidence intervals of estimating Equation 1 for different groups separately. The dashed line shows the estimated coefficients for the pooled sample (left) and the female sample (right). Regressions account for school FE, cohort FE and the list of control variables as in Table 2 for the pooled sample and Table 3 for the female sample.

[27] The father's occupation question is asked for the time when the survey respondent was 15 years old. Rural areas are a good divider between poorer and richer households in Egypt, but they are likely to encompass most students within the same school and thus will only provide heterogeneity estimates at the school level. Different proportions of students with poorer fathers are observed across cohorts within schools, which is thus perhaps more suited to a heterogeneity analysis considering our identification approach.

To test these hypotheses, we produce estimates of the treatment effect depending on family background characteristics for (i) all students and (ii) girls only. We graph these coefficients for the three education outcomes in Figure 5 (left graphs for all students and right graphs for girls), along with confidence intervals, all benchmarked against the mean population effect (vertical dashed line). The striking pattern is that the policy effect at all stages is driven by changes in education attainment among children from rural and poorer households, but no significant impact is found among urban and richer families. This is true as well when we focus on girls exclusively. On top of the previous finding that the positive policy response was almost fully explained by education investment in daughters, this stronger impact for girls from more credit-constrained households further suggests that cost considerations for the marginal child explain our findings. We next explore how this might have affected their long term outcomes.

6 Labor Market and Marriage Outcomes

We have uncovered strong evidence that education attainment improved – especially for poorer girls – after the compulsory school curriculum was compressed. We next look at the effect on labor market and marriage quality outcomes.

6.1 Labor Market

We estimate the impact of the policy on employment probability and measures of job quality. These are presented in Table 4 for the pooled sample (Panel A) and for the two genders separately (Panels B and C).

On average, we detect positive and significant effects on labor market outcomes. The impact is mainly driven by women, for whom the probability of ever having worked increased by 7.9 percentage points, representing an increase of about 26% from the average of 31% in the control group. The share of women who are currently working increased by 5.2 percentage points, corresponding to about 22% increase over the average of 24%. Conditional on being employed, wage increased by 40.5 log points (18.66% increase relative to control group). The probability of being in a paid employment (non-agricultural job) increased by 6.8 (8.5) percentage points, representing an increase of 9.3% (10.2%) however the effect is not statistically significant. The coefficients for men are not economically significant, suggesting that the policy did not have long-term effect on men's labor market outcomes.

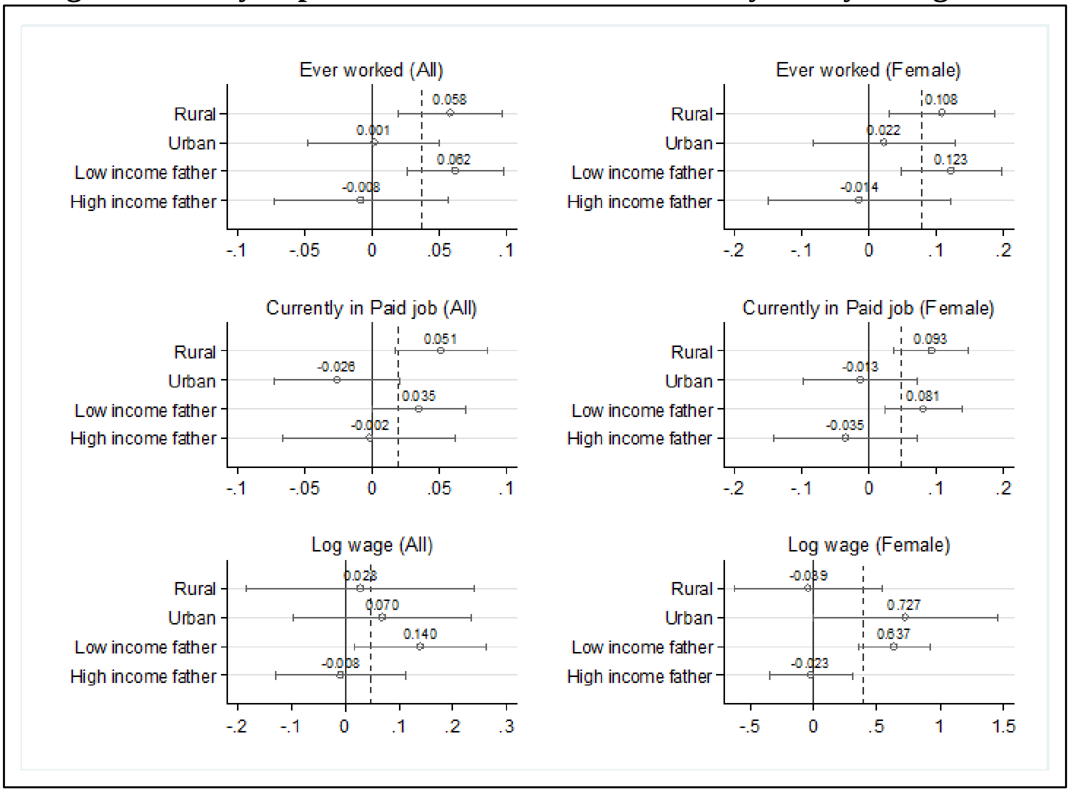
Table 4: Policy impact on labor market outcomes – average and by gender

	Ever worked	Currently working	Currently in Paid job	Currently in Non-agr. job	Log wage
Panel A: Average policy effect					
Treatment	0.035	0.013	0.022	0.023	0.061
	(0.016)	(0.016)	(0.014)	(0.020)	(0.070)
Sharpened *q*-value	(0.038)	(0.258)	(0.206)	(0.206)	(0.258)
Mean value of outcome	0.663	0.621	0.932	0.872	3.092
Percent increase	5.2	2.09	2.4	2.63	1.97
Sample size	8,746	8,746	5,026	5,026	3,641
Panel B: Policy effect on women					
Treatment	0.079	0.052	0.068	0.085	0.405
	(0.032)	(0.029)	(0.071)	(0.061)	(0.209)
Sharpened *q*-value	(0.028)	(0.055)	(0.045)	(0.081)	(0.045)
Mean value of outcome	0.307	0.238	0.736	0.833	2.169
Percent increase	25.7	21.98	9.27	10.19	18.66
Sample size	4,041	4,041	858	858	774
Panel C: Policy effect on men					
Treatment	0.005	-0.008	0.010	0.018	-0.024
	(0.017)	(0.020)	(0.013)	(0.022)	(0.077)
Sharpened *q*-value	(1.000)	(1.000)	(1.000)	(1.000)	(1.000)
Mean value of outcome	0.952	0.932	0.972	0.881	3.338
Percent increase	0.54	-0.91	1.00	2.03	-0.72
Sample size	4,705	4,705	4,168	4,168	2,867
Controls	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes
Chow test *P*-Value	0.000	0.000	0.000	0.009	0.000

Note: Controls include a dummy variable for gender (for the average sample); a dummy variable that takes the value of 1 if the father is uneducated (i.e. cannot read and write), and zero otherwise; a dummy variable that takes the value of 1 if the mother is uneducated (i.e. cannot read and write), and zero otherwise; a dummy variable that takes the value of 1 if the father was in a low-paying job (below median) at the respondent's age of 15, and zero otherwise; a dummy variable that takes the value of 1 if the mother was working at the respondent's age of 15, and zero otherwise; number of siblings; dummies for month of birth; a dummy variable for the double cohort (the two cohorts between the time of implementation), and a dummy variable that takes the value of 1 if the primary school identifier is missing (replaced then by district), and zero otherwise. Standard errors clustered by schools reported in parentheses. Being currently in a paid job, non-agriculture job, and log wage are conditional on being currently employed. Sharpened *q*-values refers to the *p*-values adjusted for the multiple hypothesis testing suggested by Anderson (2008).

We examine the heterogeneity of this policy on labor market outcomes by estimating the effect across the family background characteristics previously used: rural or urban and having a low- or high-income father. Figure 6 presents the estimated coefficients obtained for a selected number of labor market outcomes for the overall sample (left side) and for women (right side). The general picture is that disadvantaged individuals improved their labor market situation more. The impact is mainly driven by rural and poorer girls. Since those disadvantaged girls started from even lower labor market expectations than their richer counterparts, some of the gains from the policy are extremely large. For rural women the policy increases by about 11 percentage points the probability of having ever worked, and about 10 percentage points the probability of being currently in paid employment. Treated women from disadvantaged families are 12 percentage points more likely to have ever worked, and 8 percentage points more likely to be in paid employment. They are also earning about 64 log-points more in wages compared to the untreated. The figure shows gains for urban women especially on wages, though the effect is not statistically significant.

Figure 6: Policy impact on labor market outcomes by family background



Note: Coefficients and 90 percent confidence intervals of estimating Equation 1 for different groups separately. The dashed line shows the estimated coefficients for the pooled sample (left) and the female sample (right). Regressions account for school FE, cohort FE and the list of control variables as in Table 2 for the pooled sample and Table 3 for the female sample.

6.2 Marriage

For many women, labor market success may not be the margin on which enhanced education attainment will have changed life outcomes, so we also look at the quality of their marriage. We have information on marriage situation for all but one percent of married women in our sample and focus on them exclusively (i.e. drop men) for this part of our analysis. We consider how the education policy may have affected six characteristics of a marriage: three are related to age (underage marriage, average age at first marriage, large age difference with husband), and the others relate to living condition (patrilocal residence), 'bride price' (value of jewelry given at the time of marriage), and a wife's bargaining power (intra-household decision-making).

Table 5 reports estimates of the policy impact on our measures of marriage quality. All age indicators show that treated women married later in life – the probability of doing so before the age of eighteen was cut by more than 40 percent (from an average of 15%) and the average age of 22 increased by almost three-quarters of a year. Far fewer women married men much older than them (i.e. > 6 years older).

Far fewer women married men much older than them (i.e. more than 6 years older). The probability of living with parents-in-law after being married – rather than forming a new household – decreased, albeit not statistically significantly. We have a good (continuous) proxy for a woman's value on the marriage market: the reported monetary value of the jewelry given by the groom to the wife's family in Egypt, the *Shabka*. Using this measure,[28] we find that women from treated cohorts received on average a 13% higher 'bride price' than their non-treated counterparts from the same schools, showing a correlation between more years of education and a higher bride price. Finally, to assess whether they experienced better and more equal relationships once married, we use the intra-household decision making index. In the final column of Table 5, this standardized index is shown to be 6.6 percent higher for the women who were treated by the policy, however, this effect is not statistically significant.

[28] We use a log-standardized version of the real value of the *Shabka* in local currency at the time of marriage to avoid giving too much weight to extreme values (logarithm) and for ease of interpretation (standardization).

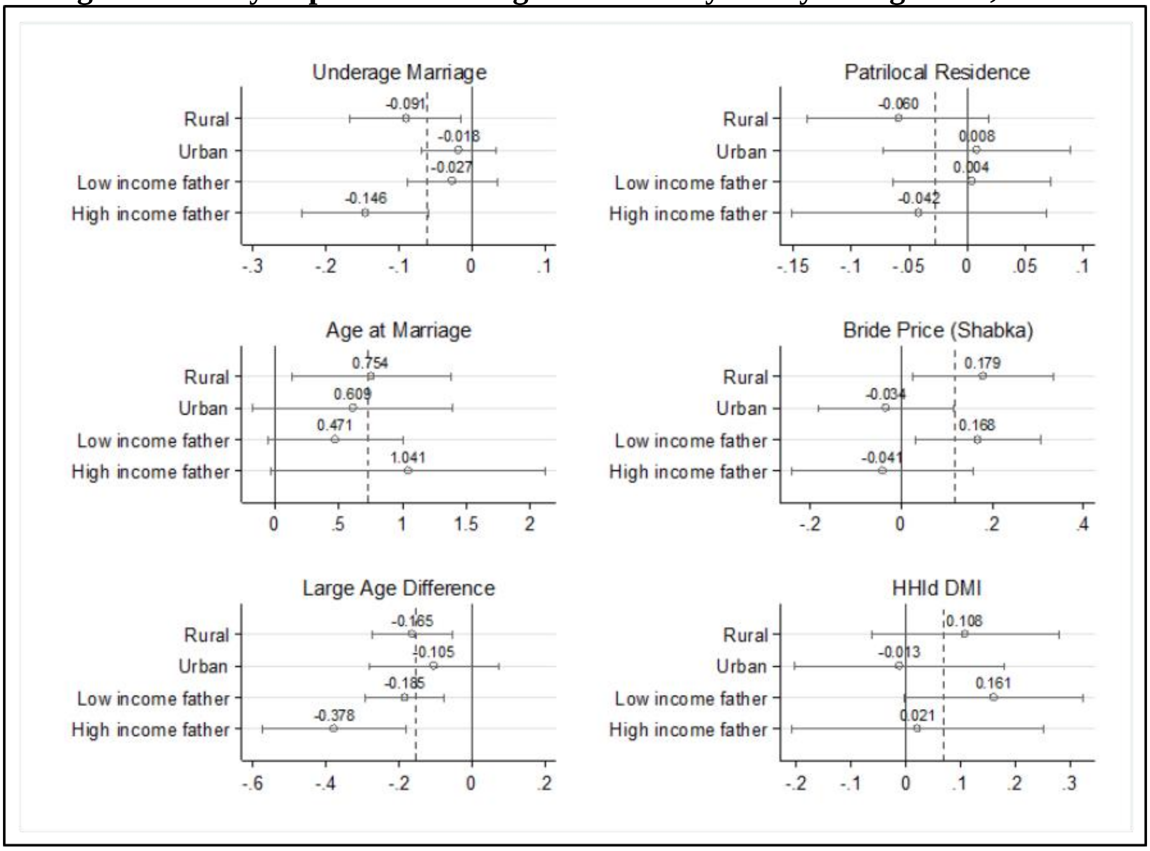
Table 5: Policy impact on marriage outcomes

VARIABLES	(1) Underage Marriage	(2) Age at Marriage	(3) Large Age Difference	(4) Patrilocal Residence	(5) *Shabka* or 'Bride Price'	(6) Intra-Hhld DMI
Treatment (5 Year primary school)	-0.062	0.736	-0.154	-0.031	0.133	0.066
	(0.031)	(0.301)	(0.054)	(0.034)	(0.067)	(0.077)
Sharpened *q*-value	(0.045)	(0.028)	(0.023)	(0.101)	(0.045)	(0.101)
Mean of Outcome	0.151	22.046	0.11	0.307	-	-
Effect size (%)	-41.3	3.34	-139.75	-10.12	13.3	6.6
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
School Fe	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	3,523	3,523	1,101	3,497	3,476	3,464
Number of schools	1,520	1,520	603	1,509	1,514	1,509

Note: The analysis is limited to women. Controls include a dummy variable for gender; a dummy variable that takes the value of 1 if the father is uneducated (i.e. cannot read and write), and zero otherwise; a dummy variable that takes the value of 1 if the mother is uneducated (i.e. cannot read and write), and zero otherwise; a dummy variable that takes the value of 1 if the father was in a low-paying job (below median) at the respondent's age of 15, and zero otherwise; a dummy variable that takes the value of 1 if the mother was working at the respondent's age of 15, and zero otherwise; number of siblings; dummies for month of birth; a dummy variable for the double cohort (the two cohorts between the time of implementation), and a dummy variable that takes the value of 1 if the primary school identifier is missing (replaced then by district), and zero otherwise. Standard errors clustered by schools reported in parentheses. Sharpened *q*-values refers to the *p*-values adjusted for the multiple hypothesis testing suggested by Anderson (2008).

Figure 7 shows estimates of the policy impact coefficients of our measures of marriage quality decomposed by women's family background characteristics. It shows that women from rural areas benefited more than their urban counterparts on all measures. This is also mostly true for girls who grew up in low-income households compared to girls who grew up in high-income households, although the picture is somewhat patchier. An interesting result is that all of the gains from the marriage market value seem to be concentrated among women from the more disadvantaged family backgrounds. This is mostly obvious in the impact on bride price, which is mainly driven by rural women and women whose fathers had low-income jobs.

Figure 7: Policy impact on marriage outcomes by family background, women



Note: Coefficients and 90 percent confidence intervals of estimating Equation 1 for different groups separately. The dashed line shows the estimated coefficients for the overall female sample. Regressions account for school FE, cohort FE and the list of control variables as in Table 5.

7 Mechanisms

We explore two mechanisms that could explain the counter-intuitive result of a reduction in the number of years of compulsory schooling leading to an increase in the total years of education. First, we consider whether the savings that primary schools made from dropping a year were used to improve education quality, which could have increased retention. Second, we ask whether the very marked gender difference that we observe could be explained by differences in the perceived costs and/or benefits of investing in education for sons and daughters.

7.1 Increased Quality of Education

A channel that might explain the positive impact of the policy is an improvement in the quality of education. As explained earlier, while the main aim of the reform was to reduce the cost of education to the public purse, it also stated that reducing the number of years taught could increase the quality of the instruction that students received, primarily by eliminating running schools on a daily two- (or three-) shift basis. If this were systematically implemented when the sixth year was dropped in a primary school, we could expect this to improve teaching quality, which in turn would have positively affected student education outcomes. If this mechanism is sufficiently important, then the family investment channel that we have put forward – stemming from the cost reduction in the number of years to complete various stages of schooling – might not explain the results.

We exploit the rich education module of the ELMPS dataset to look at the impact of the policy on different self-reported quality indicators. Those include the probability of school interruptions for six months or longer, the probability of having ever repeated a grade, and test scores.[29] The data in Table 6 reveals no significant differences between treated and control groups in any of the outcomes, suggesting that the policy did not have direct effects on the quality of education. Given that a major goal of the policy was to eliminate the need to operate shifts within schools, we further investigate the impact of the policy on attending school on a daily-shift basis. All we note here is a small, significant drop in the probability of having attended school on a shift-basis while in primary school. This shows that at least some schools combined dropping the final year with alterations to their teaching structure, although the

[29] At the end of each education stage (primary, preparatory, and secondary), students have to take a high-stakes test. The test in the primary and preparatory stages is unified at the governorate level, while the test at the end of secondary stage is unified at the national level. While test scores are more meaningful when comparing students within the same cohorts, it could provide signals for the relative quality of education. The test scores reported in Table 6 are standardized with an average of zero and standard deviation of one.

magnitude of this effect – a 4.7 percentage point decrease on a baseline of 41.5 percent – is far too small for this to be considered a systematic change. A large share of primary schools continued to run shifts after the the policy was introduced. We also detect no impact on shift classes for the next (preparatory) stage, something we would not have expected to happen in any case.

We assess the possibility that the policy has affected other aspects of education quality by creating indicators for (i) the student having ever used computers while at school, and (ii) teachers having ever resorted to physical punishment at school. The table shows no evidence of any change in the potential positive use of computers in the classroom, nor the negative use of corporal punishment by teachers during the compulsory stage of education. These findings suggest that we can largely reject the possibility that the reduction in the quantity of education was compensated by a substantial increase in its quality as an explanation of our general positive findings.

Table 6: Policy impact on quality of education

Quality Measure:	**Ever interrupted school**	**Ever repeated a grade**	**Test scores (std.)**			**School in Shifts**		**Computer Use**		**Corporal Punishment**	
Education stage	All	All	Primary	Prep.	Sec.	Primary	Prep.	Primary	Prep.	Primary	Prep.
Treatment	0.004	-0.006	-0.340	-0.119	-0.013	-0.047	-0.011	-0.028	0.012	-0.005	0.010
	(0.005)	(0.019)	(0.264)	(0.198)	(0.206)	(0.022)	(0.025)	(0.024)	(0.029)	(0.016)	(0.020)
Mean of Outcome	0.019	0.205	-	-	-	0.415	0.376	0.193	0.301	0.858	0.747
Effect size	20.28	-3.09	-	-	-	-11.44	-2.85	-14.43	3.88	-0.61	1.31
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	7,348	7,349	854	1,175	1,491	7,548	6,429	5,133	4,781	7,507	6,381
Number of schools	2,560	2,560	634	866	1,073	2,633	2,444	2,107	2,045	2,619	2,427

Note: Controls include a dummy variable for gender; a dummy variable that takes the value of 1 if the father is uneducated (i.e. cannot read and write), and zero otherwise; a dummy variable that takes the value of 1 if the mother is uneducated (i.e. cannot read and write), and zero otherwise; a dummy variable that takes the value of 1 if the father was in a low-paying job (below median) at the respondent's age of 15, and zero otherwise; a dummy variable that takes the value of 1 if the mother was working at the respondent's age of 15, and zero otherwise; number of siblings; dummies for month of birth; a dummy variable for the double cohort (the two cohorts between the time of implementation), and a dummy variable that takes the value of 1 if the primary school identifier is missing (replaced then by district), and zero otherwise. Standard errors clustered by school reported in parentheses.

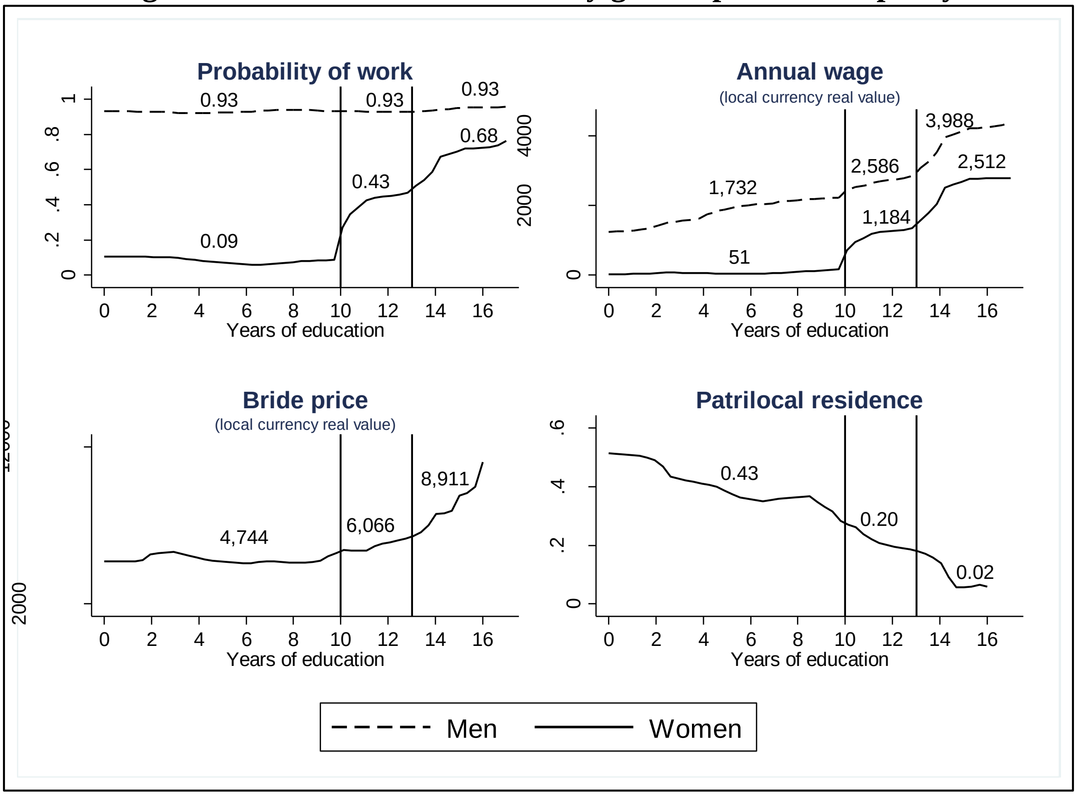
7.2 Costs and Returns to (Female) Education

All of the heterogeneity analysis pointed to a much stronger policy impact for girls than boys, at all levels of education. This suggests that the policy shifted families' decisions about their daughters' education, but less so for their sons'. This is despite strong gender discrimination in human capital investment (in favor of sons), which is evident from the relatively large pre-policy baseline differences in education attainment observed between boys and girls. Given this preference for investment in sons, there must have been a substantial shift in the cost-benefit calculations that families made, especially when it came to pushing their daughters to finish the (non-compulsory) secondary education stage.

On the expenditure side, the marginal cost of education is often perceived to be higher for girls than boys. One argument is that strong gender segmentation in household production, with daughters being submitted to an inflexible schedule of infant care and household chores, tends to clash with school attendance (Meller and Litschig 2015). If this is also the case in Egypt, one less year of school to reach a certain higher education level would mean a larger drop in costs for girls than boys. We assess this by using survey answers in the ELMPS about perceived annual costs per child for each education level. These are reported in Table A3 of the Online Appendix and show that during the compulsory stage there is only a marginal difference by gender (E£ 569 per year for boys and E£ 576 for girls, which is equivalent 90 US$ a year or close to 10% of the average annual male wage), although this is perceived to be substantially higher for girls than boys during secondary school (E£ 1,175 per year for boys and E£ 1,482 per year for girls). Accordingly, in terms of costs, the possibility of finishing secondary education by the age of seventeen rather than eighteen would have had a stronger impact on a family's investment decision for the average daughter compared with a son.

Looking at the benefit side of education investment decisions by gender, our results could be perfectly in line with the stronger non-linearities in returns by stage of schooling for girls than boys. One would then expect that an increase in the probability of entering secondary school would have more of an impact on the probability of completing this higher education stage for the gender with the higher relative benefit of doing so. There is no question in the ELMPS covering expected returns to education, so we use certain labor market and marriage (for women) outcomes by years of schooling for the non-treated population, to evaluate how these may be perceived by gender.

Figure 8: Outcomes of education by gender prior to the policy



Note: Outcomes of education prior to the policy using ELMPS data. Probability of work and annual wage come from the 1998 ELMPS, while bride price and patrilocal residence are from the control group in our analyses above (2012 ELMPS). Annual wage is conditional on work. The vertical lines represent the end of the preparatory/compulsory stage (the left line), and the end of secondary stage (the right line). The numbers above the graph represent the average value for each education stage: primary, preparatory, and secondary. Bride price is estimated in real value of Egyptian pounds at the time of marriage.

These can be seen in the four graphs of Figure 8, which cover the probability of working, average wage, bride price, and patrilocal residence. It is apparent that while almost all men work – independent of their education level – there is a huge jump in the probability of being employed if women have completed secondary education (second vertical line). Conditional on working, men's wages increase with slight non-continuity by education stage, although these non-linearities are not as sharp as those for women's wages. Turning to marriage market value and living conditions – which may be more important measures of expected returns to education for many women – we see that both bride price and patrilocal residence change discontinuously with the number of years of schooling. Almost no women who have completed secondary education live with their parents-in-law and the *Shabka* received at marriage only really non-linearly increases if women finished this stage (i.e. there is almost no difference in the amount received as a bride up to this stage). These clear non-linear returns to female education seem to confirm the mechanism that the cost reduction from compressing the compulsory curriculum by a year mostly benefits investment in the human capital of daughters, and especially those from more disadvantaged families.

8 Concluding Remarks

Our paper has documented the (perhaps surprising) positive impact on education and later-life outcomes of poorer girls after Egypt reduced the number of years of compulsory schooling. To obtain causally interpretable inferences of the policy effect, we exploited its staggered implementation across schools and compared outcomes of treated and non-treated pupils within each school. We found that the policy not only resulted in (mechanical) increases in compulsory schooling completion, but also led to significant increases in the probability of treated individuals completing the subsequent stage of secondary schooling. The policy increased the total years of schooling.

We hypothesized that this was most likely due to adjustments in investment children's secondary education among poorer families, for whom one less year of education expenses tilted the cost-benefit calculation positively, and especially for their daughters. We substantiated this by showing that almost all of the increase in years of schooling came from treated girls and that the effect was especially strong if they belonged to rural or less wealthy households. This is also the case for improvements in longer-term labor and marriage market outcomes. To further explain the strong gender differences in the policy effect, we highlight the much larger jumps in returns to education for girls at each stage of school completion compared to boys.

In terms of economic magnitude, our finding of a 10-percentage-point increase in secondary education completion for girls places it among the mid-range estimates of conditional cash transfer programs (J-PAL Policy Bulletin 2017). Since there were no additional costs of this policy, and possibly even some savings for the government, this makes it a very cost-effective option to increase poor girls' education attainment. Our estimated policy impact on education across gender is almost identical to that found by Beaman et al (2012), who studied the effect of female leadership on adolescent girls' career aspirations and educational attainment in India. However, this did not translate into improvements in labor market opportunities in their setting, while it strongly does in ours. While the Egyptian policy we evaluate is unlikely to have worked directly through role model effects, we also believe that changes to information about returns to female education may still have played an important role. As soon as more girls were enrolled in secondary school, they may have learned – along with their family – about the strong benefits to finishing that education stage. This could have helped with retention. This is very much in line with the model proposed by Altonji (1993) predicting uncertainty about educational outcomes, with individuals learning over time about the non-linear relationship between years of education and earnings.

More generally, we reveal important insights for the role of education policies. Making education thresholds easier to achieve and reducing the cost of schooling can facilitate higher levels of investment in human capital and reduce gender inequalities (especially among disadvantaged groups). This has positive implications for the economic and social empowerment of women. The findings also underscore the role of signaling and sheepskin effects as important determinants of households' investment in education, as well as later success in the labor market and marriage.

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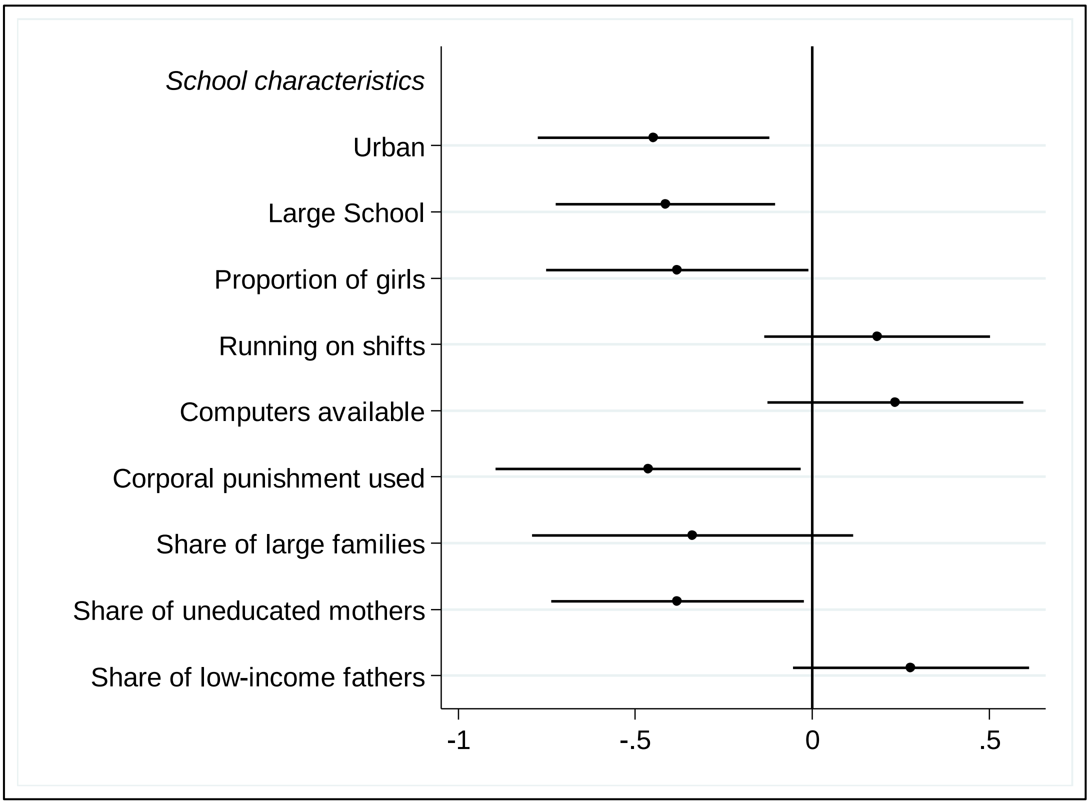
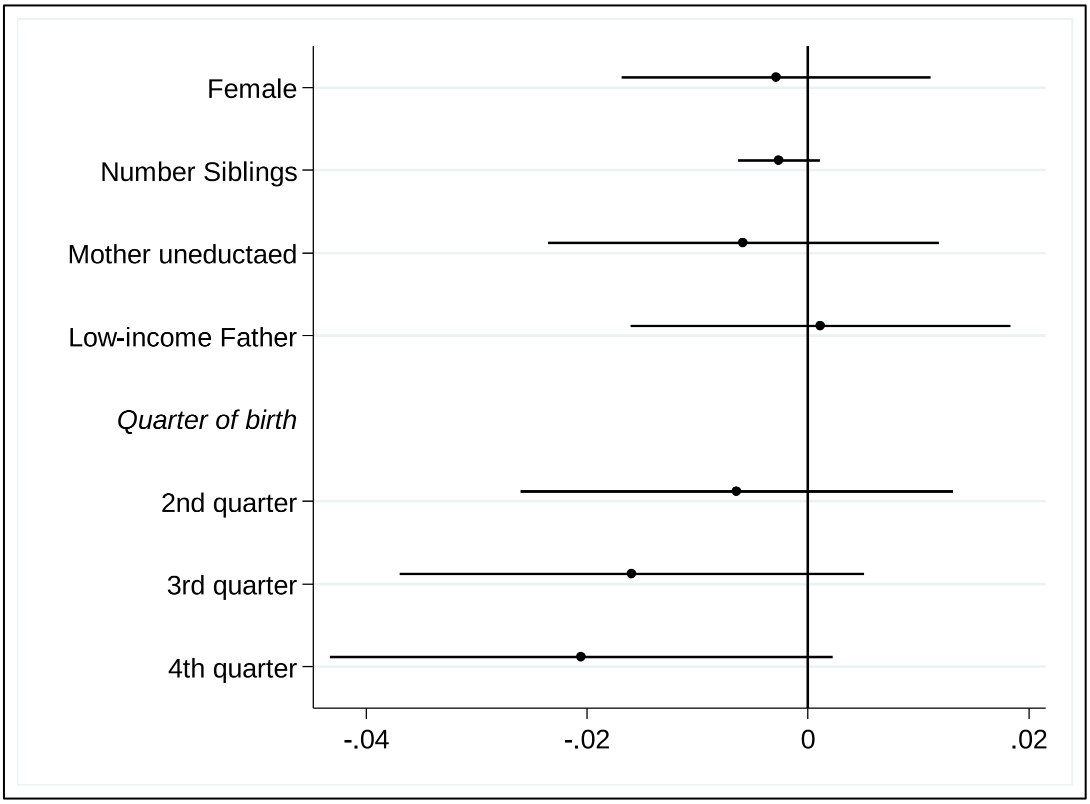
Appendix – For Online Publication

Figure A1: Timing of policy introduction by school characteristics



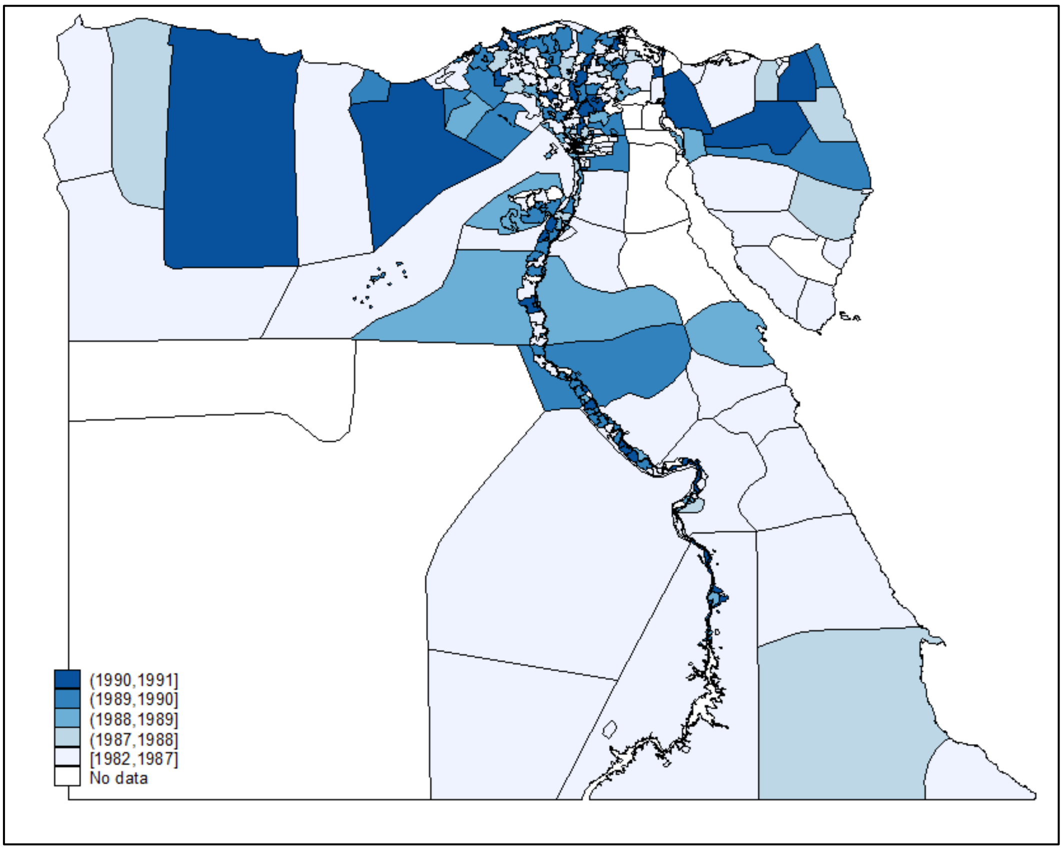
Note: The figure displays the estimated coefficients and 90 percent confidence intervals, from a regression of the year in which a school introduced the policy on the characteristics of the school as described on the vertical axis. The figure clearly shows that urban and large schools adopted the policy earlier. The same also applied to schools that adopted corporal punishment as a practice and schools with higher shares of students with uneducated mothers.

Figure A2: Balancing test for the quality of school-cohort assignment



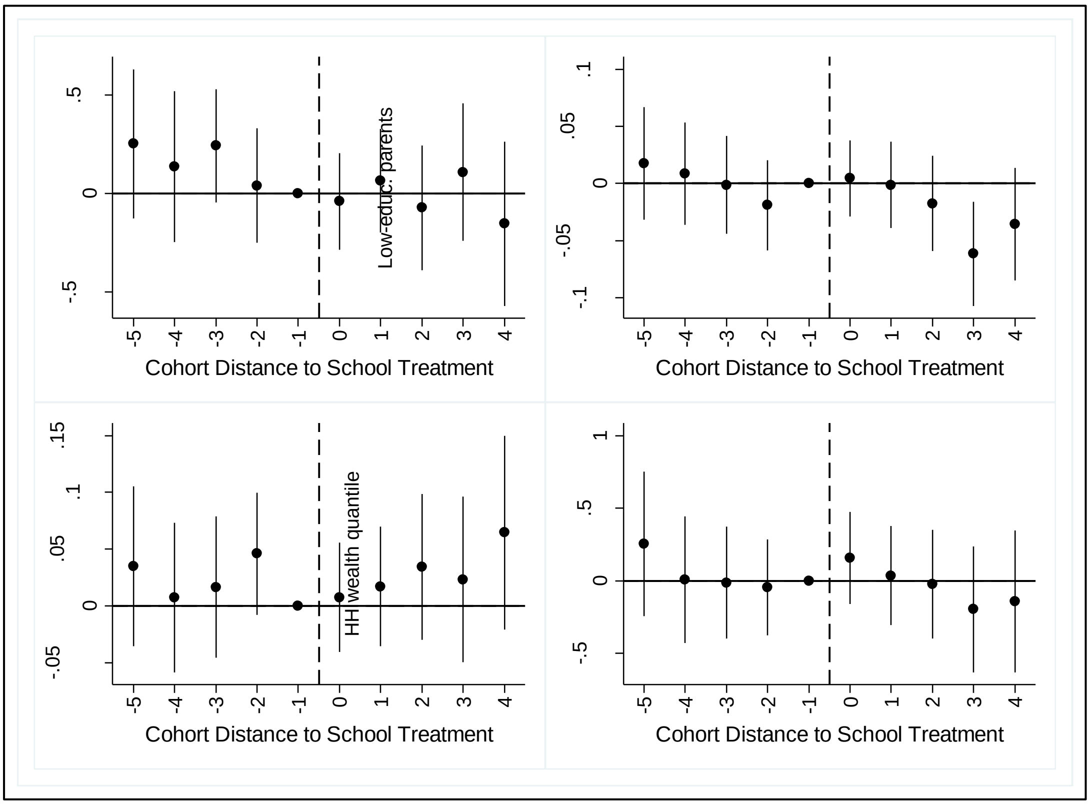
Note: The figure displays the estimated coefficient and 90 percent confidence intervals, a regression where the dependent variable is a dummy taking value one if the individual is assigned to the treatment status he/she already reported, and zero otherwise. The independent variables are displayed in the vertical axis. Standard errors clustered at the school level. The number of observations for which the actual treatment is known is 3,133.

Figure A3: Variation in timing of policy implementation across districts



Note: Map of districts in Egypt with the time of policy implementation. The timing of policy implementation is defined by the first cohort in which the majority (i.e. 50% or more) report having been treated in the ELMPS 2012. It is worth mentioning that the majority of the Egyptian population (95%) lives along the banks of the Nile and in the Nile Delta. The large-sized regions further away from the Nile have only about 5% of the population, making them less relevant for our analysis.

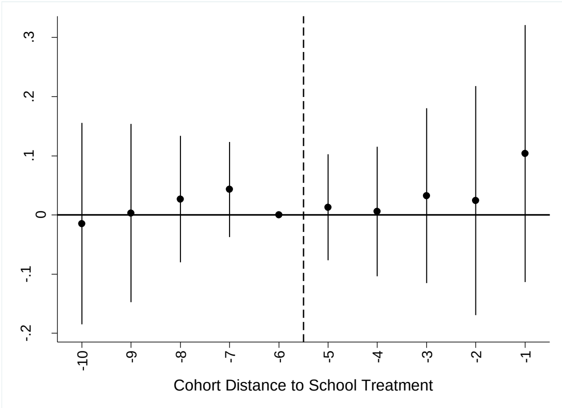
Figure A4: Testing for pre-trend and policy impact on covariates



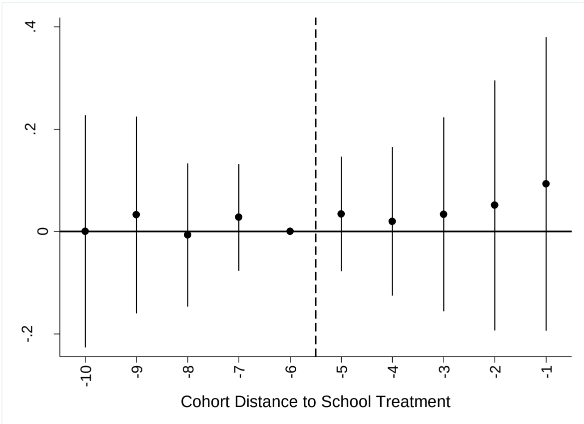
Note: Coefficient estimates and 90% confidence intervals for the impact of the policy on different covariates for cohorts around the time of implementation. Each point represents the coefficient of a school-specific cohort relative to the school-specific cohort -1. The vertical dashed line represents the implementation of the policy.

Figure A5: Placebo policy effect on education using earlier cohorts

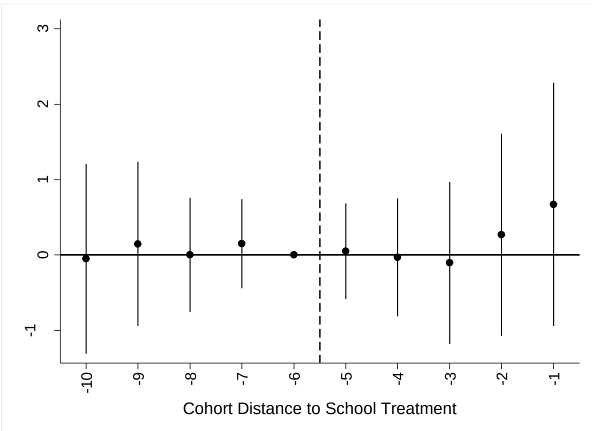
A5.1: Probability of finishing compulsory education



A5.2: Probability of finishing secondary education



A5.3: Probability of finishing secondary education



Note: Coefficient estimates and 90% confidence intervals for placebo regression for the impact of the policy on earlier cohorts. Each point represents the coefficient of a school-specific cohort relative to the school-specific cohort -6. The vertical line represents 5 years prior to the implementation of the policy.

Table A1: Definition of outcome variables

Variables	Definition
Education outcomes	
Finish compulsory school	- One if successfully finished preparatory school, and zero otherwise.
Finish secondary school	- One if successfully finished secondary school, and zero otherwise.
Age left school	- Age at which an individual was in the last year of school.
Labor market outcomes	
Ever worked	- One if the individual has ever worked, and zero otherwise.
Currently working	- One if the individual is currently working, and zero otherwise.
Paid job	- One if the individual's current job is paid, and zero otherwise.
Non-agricultural job	- One if the individual's current job is in a non-agricultural sector, and zero otherwise.
Log wage	- Log hourly wage.
Marriage outcomes	
Early marriage	- One if a woman ever married before the legal age of 18, zero otherwise.
Age of marriage	- Age at which a woman married for the first time.
Large age difference	- One if the age difference between the partners in marriage (husband's age – wife's age) is six years or more, zero otherwise
Bride price or *Shabka*	- Standardized log values of the jewelry received by bride from the husband (and/or his family) at marriage (*Shabka* in Arabic) reported by the respondent in local currency (E£) and estimated with the real value at the time of marriage.
Intra-household decision-making	- Estimated by asking women whether they usually have a say in making different decisions within the household. Choices given are: me alone, me with my partner (or family, if unmarried), my partner (or family, if unmarried alone), or other. Women are assigned the value of one if they make the decision on their own or together with husband (family), and zero otherwise. An index is constructed by averaging the z-scores and then standardizing (Duflo et al. 2007; Kling et al. 2007). The decisions are: Making large household purchases / Making household purchases for daily needs / Visits to family, friends or relatives / What food to be cooked each day / Getting medical treatment or advice for herself / Buying clothes for herself / Taking child to the doctor / Sending children to school / Dealing with school issues (e.g. talking with teachers, etc.) / Buying clothes for children.

Table A2: Education estimates – robustness checks

	Finish compulsory	Finish secondary	Age left school
A) Clustering at the district level			
Treatment	0.065	0.057	0.563
	(0.018)	(0.019)	(0.177)
Mean of outcome	0.651	0.601	15.013
Percent increase	10.03	9.53	3.75
Sample size	8,746	8,746	8,746
B) Clustering at the governorate-cohort level			
Treatment	0.065	0.057	0.563
	(0.018)	(0.019)	(0.178)
Mean of outcome	0.651	0.601	15.013
Percent increase	10.03	9.53	3.75
Sample size	8,746	8,746	8,746
C) Accounting for quadratic school time trend			
Treatment	0.064	0.056	0.553
	(0.018)	(0.019)	(0.174)
Mean of outcome	0.651	0.601	15.013
Percent increase	9.83	9.31	3.68
Sample size	8,746	8,746	8,746
D) Accounting for cubic school time trend			
Treatment	0.074	0.063	0.704
	(0.022)	(0.022)	(0.203)
Mean of outcome	0.651	0.601	15.013
Percent increase	11.35	10.49	4.69
Sample size	8746	8746	8746
E) Treatment assigned at district and cohort level			
Treatment	0.056	0.050	0.489
	(0.015)	(0.017)	(0.148)
Mean of outcome	0.647	0.596	14.977
Percent increase	8.64	8.45	3.26
Sample size	8746	8746	8746
F) School cohort treated if 1/3rd or more of surveyed respondents report treatment			
Treatment	0.061	0.063	0.600
	(0.019)	(0.018)	(0.181)
Mean of Outcome	0.651	0.599	15.008
Effect size	9.39	10.44	4.00
Sample size	8,746	8,746	8,746
G) School cohort treated if 2/3rd or more of surveyed respondents report treatment			
Treatment	0.061	0.054	0.529
	(0.018)	(0.019)	(0.174)
Mean of Outcome	0.651	0.601	15.011
Effect size	9.43	8.91	3.52
Sample size	8,746	8,746	8,746
H) Limit to schools with at least two individuals before and after treatment			
Treatment	0.081	0.069	0.665
	(0.019)	(0.020)	(0.184)
Mean of Outcome	0.609	0.563	14.518
Effect size	13.36	12.33	4.58
Sample size	6,669	6,669	6,669

Table A2 (Continued): Education estimates – robustness checks

I) Limit to schools with at least five individuals before and after treatment			
Treatment	0.074	0.064	0.547
	(0.023)	(0.023)	(0.227)
Mean of Outcome	0.587	0.546	14.267
Effect size	12.53	11.72	3.83
Sample size	4,688	4,688	4,688
J) Use actual treatment variable			
Actual Treatment	0.081	0.076	0.607
	(0.031)	(0.032)	(0.248)
Mean of Outcome	0.743	0.673	16.279
Effect size	10.86	11.29	3.73
Sample size	3,133	3,133	3,133
K) IV: actual treatment instrumented by assigned treatment			
Actual treatment	0.102	0.097	0.617
	(0.046)	(0.050)	(0.351)
Mean of Outcome	0.743	0.673	16.279
Effect size	13.72	14.41	3.79
First stage			
Assigned treatment	0.838	0.838	0.838
	(0.033)	(0.967)	(1.967)
Sample size	3,133	3,133	3,133

Note: Controls in all specifications are as in Table 2. All specifications (except E) control for school FE and cohort FE. Model specification E controls for district FE and cohort FE. Standard errors clustered by school reported in parentheses for all model specifications except E. In model specification E, standard errors clustered by district reported in parentheses.

Table A3: Costs and Returns to Different Stages of Education

	All			**Men**			**Women**		
	Primary	Preparatory	Secondary	Primary	Preparatory	Secondary	Primary	Preparatory	Secondary
Highest Education Level	0.15	0.10	0.44	0.16	0.10	0.43	0.14	0.09	0.44
Average Annual Education Cost	478	758	1,322	488	733	1,175	468	785	1,482
Employed	0.68	0.62	0.61	0.99	0.96	0.93	0.14	0.09	0.17
Paid Employment	0.65	0.59	0.59	0.97	0.94	0.90	0.06	0.06	0.14
Annual Wage (in E£)	4,668	5,316	5,933	4,965	5,578	6,054	790.9	1,150	4,975
Married	0.91	0.90	0.91	0.91	0.88	0.90	0.91	0.92	0.91
Received Shabka at Marriage							0.99	0.99	0.99
Shabka Amount (in E£)							6,162	7,939	8,484

Note: Authors' calculations from the 2006 and 2012 waves of the ELMPS. All monetary amounts are in 2012 E£ and, at the time, 500 E£ were equivalent to about 80 US$.