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

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Abstract: Exploiting a unique policy reform in Egypt that *reduced* the number of years of compulsory schooling, we show how it unexpectedly *increased* education attainment as more students chose to complete the next school stage. This impact is almost entirely driven by girls from more disadvantaged households. Treated women later experienced important positive improvements in labor market opportunity and marriage quality, as measured by bride price received and household bargaining power. We attribute the increased investment in daughters' human capital to changes in the behavior of credit-constrained families facing reduced school costs combined with strongly non-linear returns to female education.

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

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1 Introduction

Investment in human capital is essential for economic growth and escaping poverty (Becker 1995). Nonetheless, education investment decisions in developing countries are often constrained by the high cost that schooling entails for families. These come not only in the form of direct expenses (such as school fees, materials, transportation, uniforms, etc.) but also opportunity costs in terms of income foregone when children could instead work at home or in the labor market. For girls, families also have to consider the costs of continued school enrolment relative to getting married, which often entails them leaving the household. The poorer the household, the higher the opportunity costs of children's education due to the lower ability of credit-constrained households 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 is prevalent across many developing countries also implies that daughters' education is more negatively affected by credit constraints compared with sons (Jayachandran 2015; Evans et al. 2020). In such contexts, policies aiming 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' investment in human capital for children in the setting of a developing country with a strong preference for sons. The identification of the causal relationship comes from a rather unusual policy change in the pre-university education system of Egypt 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 remained unchanged. 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 be able to accommodate students), while also aiming 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 of its implementation 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 the school individuals had been to during the different stages of their education, and – for a subset of individuals – whether they received compulsory schooling under the old or the new regime of reduced years (i.e. actual treatment). We use this information to assess the timing of take-up of the policy for each individual school to then assign treatment at the cohort-school to implement a staggered difference-in-differences identification approach, which we extensively validate.

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 not obvious to predict *a priori* the overall impact of the policy. On the one hand, the policy made the completion of compulsory as well as secondary school and the receipt of a diploma easier for students and less costly for parents. 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 the course of subsequent levels of education and afterwards in the labor market.[1]

[1] There is no school-leaving age in Egypt. 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, there were no serious enforcement measures in place to make students remain enrolled in school until they completed the compulsory schooling stage of education.

We find evidence that compressing compulsory schooling into a shorter period was an effective policy to reduce dropout in the basic education stage. Prior to the policy, only 66% of the students managed to finish compulsory education. The policy raised this share by about 8 percentage points in our preferred specification to 74%. More interestingly, we show that the impact did not stop at the compulsory stage, but rather the share of those who enrolled 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 61%, whereby the policy increased this share by 6.4 percentage points. This resulted in an overall rise in the age at which students leave school by about 0.73 years, corresponding to an increase of about 5% compared with the average age of fifteen before the policy. The impact is robust to several robustness checks and mainly driven by individuals from rural areas, as well as those from poorer families. This suggests that the reduced education costs that the policy entails is the main reason behind the positive impacts.

We further show that there is a strong gender dimension, whereby the impact is all coming from 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 having sons, such as Egypt. In such contexts, households choose to 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 families being 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 on the Egyptian labor market compared with men. We also show that it significantly improved women's marriage quality as it reduced the probability of being married as a minor, increased the value of jewelry received (bride price) at marriage, and enhanced the intra-household decision-making power of wives.

We further explore alternative mechanisms that may explain our findings of improved educational and later-life outcomes stemming 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 provided was compensated by an increase in the quality of the education that pupils received, which in turn would have increased their probability to stay enrolled in school for longer. We test this by checking whether the policy changed preparatory education quality using three self-reported measures related to whether the respondent had ever studied in single-shift classes, if they had ever used IT equipment, and if their teachers had ever resorted to corporal punishment. We reject the notion that any of these significantly changed and thus that education improvement could explain our findings. This thus leaves changes in investment decisions from previously credit-constrained families as the most likely underlying mechanism, although it is still necessary to rationalize why this would have particularly affected girls in our context.

Another possibility could be if returns to education were 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, which increase much more non-linearly for women compared with men at each educational stage. These non-linearities are also found when considering the bride price received and the probability of patrilocal residence at marriage (i.e., co-residnce with the in-laws as opposed to independent place of residence). This indicates that they are also present for 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.[2] The fourth strand deals with long-term returns to education and the role that culture context plays

[2] 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).

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 polygyny have been shown to play an important role in households' decision to invest in the human capital of their children (Jacoby 1995; Levine and Kevane 2003; Tertilt 2005; 2006; Gaspart and Platteau 2010; Jayachandran and Pande 2017; Ashraf et al. 2018). One example is the practice of bride price: according to Becker (1981), it not only compensates households for the transfer of their 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 when bride price traditions still exist (Ashraf et al. 2018).

Our paper is one of the first to investigate the effects of reducing the number of years of schooling as a policy to reduce the costs of education.[3] The findings suggest that cutting the costs of schooling induces households to invest more in children's human capital even when education is theoretically for free, as in the case of Egypt.[4] Having the impact mainly driven by poorer families provides strong suggestive evidence that credit constraints are a major barrier to investment in education. Egypt provides a unique context to study 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.[5] The finding that the impact is

[3] Germany has recently undergone a reduction in the number years of schooling, where most of the German states have abolished the last year of secondary schooling. The literature shows that this reduction has 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 compared with later towards the end of secondary stage, which enables us to study how households adjust their behavior over a longer period of time regarding investment in their children's human capital. Second, the implementation of the policy change in Germany is quite recent, which makes evaluating the impact on longer-term outcomes rather difficult. Third, and more importantly, the implications regarding cost reducation of education are more relevant in the context of a developing country with strong gender bias .

[4] 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 that keeping children at school entail (Assaad and Krafft 2015).

[5] 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).

mainly driven by poorer girls provides strong evidence that reducing 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-complete 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 factors are all likely to be present in a large number of developing countries, we believe that our findings hold relevance for a large number of poor young girls across 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, after which 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 is divided into three levels: primary, preparatory, and secondary. Between the early-1980s and the reform that we study, compulsory (basic) education entailed a total of nine years: the six grades of primary school and the three grades of preparatory school. Upon successful completion of these two education levels, students could optionally join the secondary stage, which comprises two alternative tracks: the vocational (technical) track or the general secondary track, both of which take three years to complete. School legally starts 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 – of school dropouts.[6]

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 (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 work under the supervision of the ministry of education,[7] which supervises 27 directorates, one per governorate (province), which in turn oversee local education departments across districts.[8] 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 to understand why there was staggered roll-out of the policy change that we present in the next section and exploit for our identification strategy.

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[6] The government could in theory attempt to more strictly enforce the completion of compulsory schooling, although this would come at a high price as it would imply 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 transfer to families conditional on attendance or of large regional investments in school buildings or teachers do show that these are policy option that many decision makers have considered (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, even when the financial resources may have been present, to consider such costly options to improve its educational system performance.

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

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

2.2 Policy Change

Since the mid-1970s, Egypt has experienced almost unprecedented population growth, which led to a rapid increase in the enrolment of students in compulsory education (Barro and Lee 2013). This placed huge pressure on the school system, which struggled to accommodate eligible students. Between 1980 and 1989, there was more than 50% increase 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.[9] One concrete example of how schools attempted to cope with this large influx of new students was to increasingly 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*. Beginning in the 1989-1990 school year, primary education now comprised 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.

[Figure 1 about here]

According to the ministry of education, the reduction in years of schooling would allow for the absorption of a larger number of children in an over-stretched school system, as well as reducing pressures coming from the large class sizes in primary schools, and eventually reducing the number of schools running on a daily-shift basis (Eldahshan, 1992). It was also

[9] https://databank.worldbank.org/Egypt-Education/id/ab0784a2

estimated that abolishing the final year of the primary stage of education would – at the very least – save millions of dollars yearly in the form of authoring and printing costs of the curriculum books of the sixth primary grade, whereby this money could be better 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 keeping the core of the academic content taught almost unaltered within the five years of primary school. This would be achieved by removing some elements of the curricula that were deemed to be unnecessary by education experts, as well as increasing the length of the academic year from 32 to 38 weeks, which – coupled with increased number of hours in every school day (after cutting down on school shifts) – would mostly lead to no alteration in the total instruction time during which pupils received compulsory education, and it could potentially even increase it.[10]

The law relating to this policy was passed by the parliament in June 1988, with a view to starting implementation three months later at the beginning of the new academic year in September (Abdelkerim 2009). The sixth year of the primary stage would be immediately abolished beginning with pupils enrolled in grade 5 in the previous academic year.[11] 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

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

[11] 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 to further stages of education and then into the labor market. We have extensively looked 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 justifiably not detect any specific impact on outcomes for these particular individuals. Also, including a 'double cohort' dummy in all our specifications does not change any of our results.

policy, only a small proportion 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 happen gradually each year until the last year of the primary stage was completely abolished in all schools (Abdelkarim 2009).[12]

[Figure 2 about here]

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.[13] Figure 2 plots the percentage of individuals born every quarter between 1971 Q3 and 1984 Q3 who give answer to this question. We first observe a strong increase in the proportion treated starting with individuals born in 1977 Q3, namely those 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 increasing relatively smoothly in all subsequent quarters as more schools change to five years of primary education, until plateauing at two-thirds of a cohort being treated. This confirms the staggered manner in which the policy was introduced over time across schools, which guides the identification strategy that we will implement to obtain causal estimates of its effects on individual outcomes.[14]

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[12] 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.

[13] This variable will be used to assign treatment status across different cohorts within schools. Section 3.2 explains in detail how the assignment is done.

[14] An earlier study by Ali and Gurmu (2016) had wrongly assumed that the policy was introduced to all students born simultaneously and consequently erroneously applied a cohort-based regression discontinuity (RD) approach to estimate its (reduced) form effect on female labor market participation and fertility outcomes. Its inconclusive findings on both of these fronts are thus unsurprising as they are unlikely to actually pick up the causal impact of the reform properly. We also show in Figure A1 of the Online Appendix that schools which were bigger, had a larger proportion of girls or pupils with poor fathers among their student bodies took longer to introduce the policy. 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 econometrically unadequate to study

3 Data and Descriptive Statistics

3.1 Dataset and sample

To investigate the impact of this policy on short- and long-run outcomes, we make use of 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 have 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 and thus limit our sample to those born between September 1971 and September 1984 – six years before and seven years after its announcement – for whom we have complete information on education and background characteristics.[15] This leaves us with a population of 8,945 survey respondents (4,830 men and 4,115 women) for our statistical analysis.

3.2 Assignment to treatment

The 2012 wave of the ELMPS includes the question on actual 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,

this policy. The within school comparison staggered difference-in-difference approach we take should in contrast account for potential school level selection into treatment on observable and unobservable characteristics.

[15] We have this complete information for almost nine out of ten survey respondents of either wave.

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 – which we have for all individuals and have matched 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- instead of six-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.[16]

Using this assigned treatment variable along with information on the location of each primary school, Figure 3 illustrates geographical disparities in the roll-out of the policy introduction across districts. A district is considered to be treated if a majority of its schools are treated in a certain year. The figure clearly shows the existence of variations in the timing of policy implementation across districts.

[Figure 3 about here]

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 – which was now reduced from nine to eight years – and that most individuals could

[16] 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 five students before and after the policy within schools answer the survey question (See Table A1 in Online Appendix). There remains the issue concerning 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 matter and 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.

now complete 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 takes one fewer 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, namely total years of schooling. On one hand, one might think that reducing the number of years of compulsory schooling would automatically lead to a reduction in the completed years of education. On the other hand, if the reform pushes a sufficient number of individuals on the margin of dropping out of the next phase of education to update their decision – as the costs of doing so are reduced – and complete it, we could observe increases in the total years of schooling. Our statistical analysis will reveal whether a policy reducing compulsory schooling indeed led to increases in total education attainment. However, we can already posit that if this was the case, this effect would have been stronger for those with more biases against and/or constraints when a family makes human capital investment decisions in the Egyptian context, namely girls from poor backgrounds.

We also look at longer-term individual outcomes that education may have affected in turn, which 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 to study 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 also look at three further outcomes that are more specific to the context of a developing country with traditional gender roles and a form of 'bride price' practice at marriage such as Egypt. 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.[17] 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.[18] 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 that may result from changes in the education attainment of treated girls after the policy was introduced. The third outcome is social empowerment, captured by the intra-household decision-making index, which is estimated by women being asked whether they usually have a say in making a number of

[17] 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 could be more able to 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).

[18] 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 instead contributing its value towards setting up 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).

different decisions within the household.[19] Table A1 in the Online Appendix provides details on how all relevant variables are defined.

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 men and women separately, and Columns (4) – (7) for different groups, which we will use later in our heterogeneity analyses. The table shows that only two-thirds of students finished what was supposed to be '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 only half among students from rural areas or with uneducated fathers). Almost two-fifth of students did not then go on 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 93 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 the lowest for individuals for uneducated fathers (13.4).

[Table 1 about here]

[19] For each decision, women are assigned the value of one if they make it on their own or together with their husband (or their family if they are not married), and zero otherwise. Following Duflo et al. (2007) and Kling et al. (2007), we aggregate information from the 10 decision-making items to create an index related to intra-household decision-making. We construct this index by averaging the z-scores of the underlying measures. See Table A1 in the Online Appendix for the list of the 10 decision items.

Table 1 also reports means for a number of labor market outcomes that we will study the impact of the education policy on, and here the difference by gender becomes even starker. While almost all men (92%) work in Egypt, less than one-fifth of women report being employed at the time of the survey, and these women are much more likely to be doing ‘unpaid jobs’ (e.g. working in the household of a relative without financial compensation). Finally, marriage outcomes are reported for women. 15% of the women marry before the legal age of 18, whereby this percentage is considerably higher in rural areas, accounting for more than one-fifth of women. The table also shows that 44% of women in rural areas and one-third of those from less-advantaged families (less-educated fathers) co-reside with their in-laws (or parents). Bride price seems to be significantly higher in urban areas and among relatively advantaged families, and women in these groups are more socially empowered with more freedom of decision-making within their households compared with women in rural areas and those with less-educated fathers.

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:

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

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 $Treat_{cs}$, 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, and all following ones where at least 50% of individuals reported having followed five years of education in a particular school), and zero otherwise. X_{ics} is a set of controls for individual characteristics,[20] some of which we will also use interacted with treatment status in our heterogeneity analysis to understand who reacted more strongly to the policy change. ε_{ics} is the error term, which is assumed to be independent and normally distributed across individuals i, and which is clustered at the governorate (province) times cohort level.[21] We present some results using a basic specification which does not account for cohort (α)- and school (γ)-specific fixed effects, but rely for our interpretation of estimates of β on the more complete variant of equation (1) which includes both. 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.[22] 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 as the policy was implemented for students who had

[20] These are namely gender, uneducated father and mother (i.e. cannot read and write), low-income father (i.e. below-median income when the respondent was aged 15), working mother (i.e. mother was working when the respondent was aged 15), number of siblings, month of birth, and finally a dummy variable that takes the value of 1 if the primary school identifier is missing – replaced then by district – and zero otherwise.

[21] We cluster at the governorate level because this 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 non-randomly and also at the cohort level to account for potential unobserved shocks in each school year around the policy introduction. The results, however, are robust to other levels of clustering such as school or district.

[22] 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 this could be an issue 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 of 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.

already 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 education decisions to selectively avoid treatment.[23] 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 (linear, quadratic, and cubic). We can also visually show – in Figure 4 – 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. We believe that this is already strong evidence validating our staggered difference-in-difference approach, although we will further address this school-level pre-trend issue from the start of the subsequent results section.

[Figure 4 about here]

5 Education Outcomes

[23] 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. 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 absolutely 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.

5.1 Average Policy Impact

Pre-Trends?

Before showing policy impact estimates from equation (1), we graphically show 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 β coefficients obtained from equations (2) below for five pre- and post-policy introduction cohorts (with the reference cohort being k = -1), which also includes the same set of X_{ij} of control variables as previously defined. This exercise – akin to taking an event study difference-in-differences approach – is also a good way to visually check whether we should still worry about school selection into treatment based on educational outcome pre-trends. Specifically, we estimate the following model:

$$E_{iks} = \sum_{k=-5}^{5} \beta_k \cdot Cohort\_Treat_{iks} + \gamma_j X_{ij} + \varepsilon_{iks} \quad (2)$$

Figure 5 reports the estimates for the three education outcomes of interest: the probability of finishing compulsory education (Figure 5.1), the probability of finishing secondary education (Figure 5.2), and the age at which the student left school (Figure 5.3). 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 the policy can be considered as a relatively exogenous shock to students across cohorts within school and thus further validates the staggered difference-in-differences approach taken. Once the policy is implemented, there appears to be a significant increase in not only the probability of finishing compulsory schooling for all subsequent cohorts, but also completing the next secondary education stage, which seems to lead to students leaving school later on average.[24]

[Figure 5 about here]

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 A3 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 present placebo estimates on the same education outcomes. Figure A4 in the Online Appendix presents these coefficients from these placebo estimates and again clearly shows no pattern of any irregular trend or jump prior to the policy introduction.

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 controls for an individual's background characteristics,

[24] There appears to be a somewhat stronger policy impact for the first treated cohort ($k = 0$). We believe this is because 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.

while Columns (2)–(4) gradually add cohort fixed effects, school fixed effects, and different forms of school-specific time trends. The most complete and final specification includes cohort and school fixed effects, as we as a cubic school-specific time trend. To benchmark the magnitude of the effects, we report the policy impact in the percentage of the control group means.

[Table 2 about here]

Overall, the estimated impacts are always positive and statistically significant and insensitive to the exact set of controls, fixed effects, or time trend included. We, therefore, focus on our preferred specification in column (4), which suggests that the policy increased the probability of finishing preparatory education by 7.5 percentage points, which corresponds to an 11.5 percent increase given the average baseline completion rate of two-thirds pre-reform. This is a strong but perhaps unsurprising impact given the mechanical effect that reducing the number of years that 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 6.4 percentage points (10.6% of an average of the 61% who finished secondary education prior to the reform).[25] The age at which individuals finish education increased by 0.73 years, corresponding to a 4.8% increase of the average age of 15 years prior to the policy.

While we will further explore in the rest of the paper what might initially seem to be surprising results – less compulsory schooling resulting in more completed years of education –, we should already give some intuition as to why this could have happened. We can first

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

safely state that having one year less to complete compulsory education will have reduced costs for families of pupils enrolled in a school at the time of the policy introduction, at least relative to students from non-treated cohorts. More of these students would then not only have completed compulsory education but also entered secondary school earlier than their families had planned. As a result, some would have re-considered their investment decision to now push their children to keep on going and finish secondary education. This would consequently lead to more total years of completed education for the students affected by the policy. If this 'cost channel' is at play, we would then 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 the definition of treatment or the sample used, as reported in Table A2 of the Online Appendix. First, we present in Panel A results when an individual's treatment is assigned at the district-cohort level (i.e. more than half of respondents born the same year in a district report having followed the five-year primary curriculum) and these estimates are very similar to 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 B and C, respectively). Next, we limit the analyses to schools for which we have at least two treated and two untreated individuals. This exercise entails dropping about 60 percent of schools from the sample (keeping 1,004 out of 2,634) but since these are the small-sized schools, we remain with 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 school produces a somewhat stronger policy impact (Panel D) than our baseline estimates. 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 find that the reform

5.2 Heterogeneity in Policy Impact

Gender Differences

Given that Egypt is a country with a strong son preference, one could expect a differential impact for boys and girls in terms of reducing the costs of completing compulsory schooling.

It is not obvious which gender will benefit most as there are two competing basic mechanisms at play here: on the one hand, preference in investment for boys could be so strong that all cost reduction benefits go towards their education, while on the other hand, 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 then see that this gender-neutral education policy benefited girls more than boys.[26] We test this by re-estimating a version of equation (1) that interacts the treatment dummy with an indicator for the student being a girl.

Table 3 presents these estimates for the three education outcomes of interest using our preferred specification. The main finding here is that almost all of the previously-observed average positive policy impact on education appears to be driven by increases in attainment by girl students. While girls start from lower average schooling levels (apparent here with the large negative coefficients on the female dummy), they strongly improve their probability of completing not only compulsory education but also the next stage of three-year secondary education, which translates into an extra year before they left school once the policy was introduced. This leads us to conclude that the cost reduction brought about by the compulsory schooling curriculum compression prompted families to especially re-assess education investment decisions for their daughters, who may otherwise have dropped out of school at an earlier stage. If this cost channel for investing in the marginal girl that we put forward is what drives these results, we should then expect it to be more pronounced for all children – but especially daughters – from families who are more likely to face credit constraints. We turn to investigate heterogeneities on this margin.

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

Family Background Differences

We propose two ways to identify families that are likely to face relatively more financial limitations when having to make an investment decision for their children's education, namely those from rural areas and those whose father had a below-median income.[27] If the policy leads to changes in investment in the education of the marginal child driven by cost considerations, we would expect stronger impacts for those from families with such background characteristics. We would also expect this to be particularly true for girls in these families, for whom the decision might be even more strongly influenced by costs due to the combined effect of prevalent gender bias and credit constraints.

[Figure 6 about here]

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 report these coefficients for the three education outcomes graphically in Figure 6 (left graphs for all students and right graphs for girls), along with 95 percent confidence intervals, all benchmarked against the mean population effect (vertical dashed line). The striking pattern that emerges from this picture is that the policy effect at all stages is driven by changes in education attainment among children from rural and poorer households, whereby no significant impact is found from urban and richer families. This is true for all students as well as when we focus on girls exclusively. Accordingly, on top of the previous evidence that the positive policy response was almost fully explained by education investment in daughters, this stronger impact for girls from more credit-

[27] The father's occupation question is asked for the time when the survey respondent was aged 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.

constrained households further suggests that cost consideration for the marginal child lies behind our findings. We next turn to explore how this might have affected their longer-run life outcomes.

6 Labor Market and Marriage Outcomes

Since we have uncovered strong evidence that education attainment improved – especially for poorer girls – after the compulsory school curriculum was compressed, we naturally turn to investigating whether this translated into better long-term life outcomes in terms of their labor market situation and marriage quality.

6.1 Labor Market

We produce estimates of the impact of the policy on employment probability and – if working – measures of job quality. These are presented in Table 4 for all treated students (Panel A) and for women in particular (Panel B). On average, we detect positive and significant effects on the probability of having ever worked (+6.2%), being in a paid job (+3.5%) or in the non-agricultural sector (+5.1%) if currently working, and earning much better wages (+19%) for those who report one. This applies for both men and women and since the policy impact on education was mostly apparent for girls, focusing on women might be more relevant, especially as they start from a much lower situation on the labor market and there is thus much room for improvement. These baseline differences by gender are well illustrated by the magnitude of the female dummy coefficients presented in Panel B of the table; for example, men are almost 70 percent more likely to currently work and women are 30 percent less likely to be paid if employed. Turning to the *Treatment*Female* interaction, we again see that most of the policy impact gains observed on average are coming from large significant enhancements of women's labor market situation. Those from cohorts that experienced the shorter primary curriculum as students work more and are in better quality jobs with higher wages than older girls who attended the same school.

[Table 4 about here]

We delve deeper into the heterogeneity of this labor market outcome policy effect by estimating the interacted *Treatment*Female* model[28] for the family background characteristics previously used, namely rural or urban and having a low- or high-income father. Table A3 in the Online Appendix presents the estimated coefficients obtained. The general picture is that poor girls benefited the most in terms of labor market situation improvements. There are also some visible gains for more well-off treated women, although these are smaller in almost all dimensions than for those from families living in the countryside or those who are relatively poorer. Since these more disadvantaged women started from even lower labor market conditions than their richer counterparts, some of the gains from the education policy can be extremely large: e.g. an increase of +11 percentage points in the probability of currently working on a baseline of 21% or a more than doubling of the probability of being in a paid job if currently working.

6.2 Marriage

For many women, labor market success may not be the margin on which enhanced education attainment will have changed life outcomes, and thus we look at how the quality of their marriage might have also improved. This is also reflected in the fact that we have information

[28] We choose to run this interaction specification rather than restricting our sample to women and then cutting it by family background separately again as this would leave us for very few working women and thus means that we lack sufficient power to obtain accurate estimates (note that they still proceed in the exact same direction as those presented here if we do so).

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 about here]

Table 5 reports estimates of the policy impact on our measures of marriage quality. All age indicators strongly indicate that treated women married later in life – the probability of doing so before the age of eighteen was cut by more than half and the average age of 22 increased by almost a whole year – and that far fewer did so with men much older than them (i.e. more than 6 years older). The probability of living with parents-in-laws after being married – rather than forming a new household – decreased, albeit not statistically significantly. We have available in our context a good (continuous) proxy for a woman's value on the marriage market: the reported monetary amount of the jewelry given by the groom to the wife's family in Egypt, the *Shabka* in Arabic. Using this measure,[29] we find that women from treated cohorts received on average a 13 percent higher 'bride price' than their non-treated counterparts from the same schools, showing that the extra education that they received translated into a higher valuation when it came to marriage. Finally, to assess whether they experienced better and more equal relationships once married, we create an index indicating how much say a woman has when the household makes important decisions. In the final column of Table 5, this

[29] 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).

standardized intra-household decision-making index is shown to be 16.5 percent higher for the women who were treated by the policy, confirming that their improved education attainments also helped them to become involved in relationships where they benefited from stronger bargaining power.

[Figure 7 about here]

Figure 7 graphically illustrates estimates of policy impact coefficients of our measures of marriage quality decomposed by women's family background characteristics. It is initially apparent 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- compared with high-income households, although the picture is somewhat patchier. An interesting result is that the probability of patrilocal residence – which is very common in the countryside, with almost 45 percent of couples living with the husband's parents – now significantly decreased by about 24%. Moreover, all of the gains from the marriage market value seem to be concentrated among women from the more disadvantaged family backgrounds. This is also the case for intra-household decision-making, which jumps up to almost 30 percent for both treated rural women and those who had low-income fathers. This provides further evidence that the policy we study especially improved the life outcomes of poorer girls whose family were most likely to update education investment decision made in view of the son preferences that they held and the tight budget constraints that they faced.

7 Mechanisms

We now explore two important mechanisms that could be behind the perhaps unforeseen impact of a policy reducing the number of years of compulsory schooling leading to an increase in the total amount of education attained by treated students. First, we consider whether the savings that primary schools made from dropping a year were used to improve education quality, which in turn could have increased the chance of certain students staying on in later school years. Second, we ask whether the very marked gender difference that we observe could be explained by differences in the perceived costs and/or benefits that families expect when investing in various stages of education for their sons or daughters.

7.1 Increased Quality of Education

A potential channel that could explain the positive impact of the policy is via improvements in the quality of education. As explained earlier, while the main aim of the reform was to reduce the cost of education on 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 was 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 outcomes, including the probability of not dropping out early from school. 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 – could be potentially erroneous.

Even if this only occurred in an extreme case scenario as it is likely it would take longer time to implement a non-shift teaching structure than to drop one year of education. We test for this by checking for change in the probability that treated student cohorts were taught in single-shift classes. We also 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) the teacher having ever resorted to physical punishment at school.

[Table 6 about here]

Table 6 reports the estimated policy impact on our three measures of education quality when students were in either primary or preparatory school, which together form the compulsory education stage. 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 at least that some schools combined dropping the final year with altering their teaching structure, although the magnitude of this effect – a 4.6 percentage point decrease on a baseline of 41.3 percent – is far too small for this to be considered a systematic change, and most primary education remained taught in shifts after the policy introduction. We also detect no impact on shift classes for the next stage, something we would not have expected to happen in any case. In terms of other quality measures, there is 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 we can mostly 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.

7.2 Costs and Returns to (Female) Education

All of the heterogeneity analysis conducted pointed to a much stronger policy impact for girls than boys at all levels of education. This suggests that families reacted more strongly to the reduction in costs to complete various stages of schooling for their daughters than for their sons. This is despite strong gender discrimination in human capital investment, 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 tend 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 A4 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), although these are perceived to be substantially higher for girls than boys during secondary school for girls (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.

[Figure 8 about here]

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 proceeding and 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. 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 of the cost reduction entailed by compressing the compulsory curriculum by a year mostly benefiting 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's decision to reduce 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. This revealed that it not only resulted in (mechanical) increases in compulsory schooling completion but also led to significant increases in the probability of treated individuals completing the following education stage of secondary schooling. The overall estimated effect of the policy was consequently to increase the number of total years of completed schooling.

We hypothesized that this was most likely to stem from adjustments in investment in a child's completion of secondary education among poorer families for whom the reduction of a year to do so tilted the cost-benefit calculation positively, and especially for their daughters. We substantiated this by showing that almost all of the education increases came from treated girls and that these were 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 of more disadvantaged women. To further explain the strong gender differences of the policy effect, we highlight the much larger jumps in returns to education to finishing a certain school stage that are prevalent for girls compared with boys.

In terms of their 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 extra expenditures towards implementing this policy, and – if anything – savings for the government existed, this makes it potentially an extremely cost-effective option to increase poor girls' education attainment. Our estimated policy impact on education across gender is actually almost identical to that found by Beaman et al (2012), who studied the effect female leadership has 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 through role model effects as directly, we also believe that changes in information about female returns to education may still have played an important role. Once more girls were enrolled in secondary school, they may have learned – along with their family – more about the strong benefits to finishing that education stage and consequently decided not to drop out before completion. This is in fact 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, our findings provide important insights for the role of education policies aiming to make education thresholds easier to achieve and reduce the cost of schooling in spurring higher levels of investment in human capital and reducing gender inequalities (especially among disadvantaged groups). This has positive implications for 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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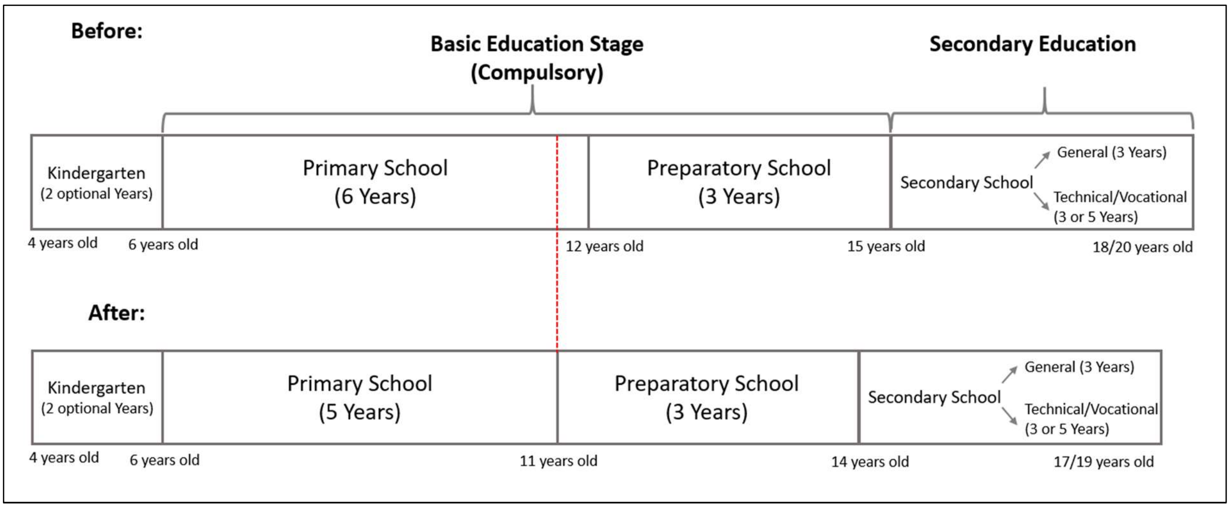
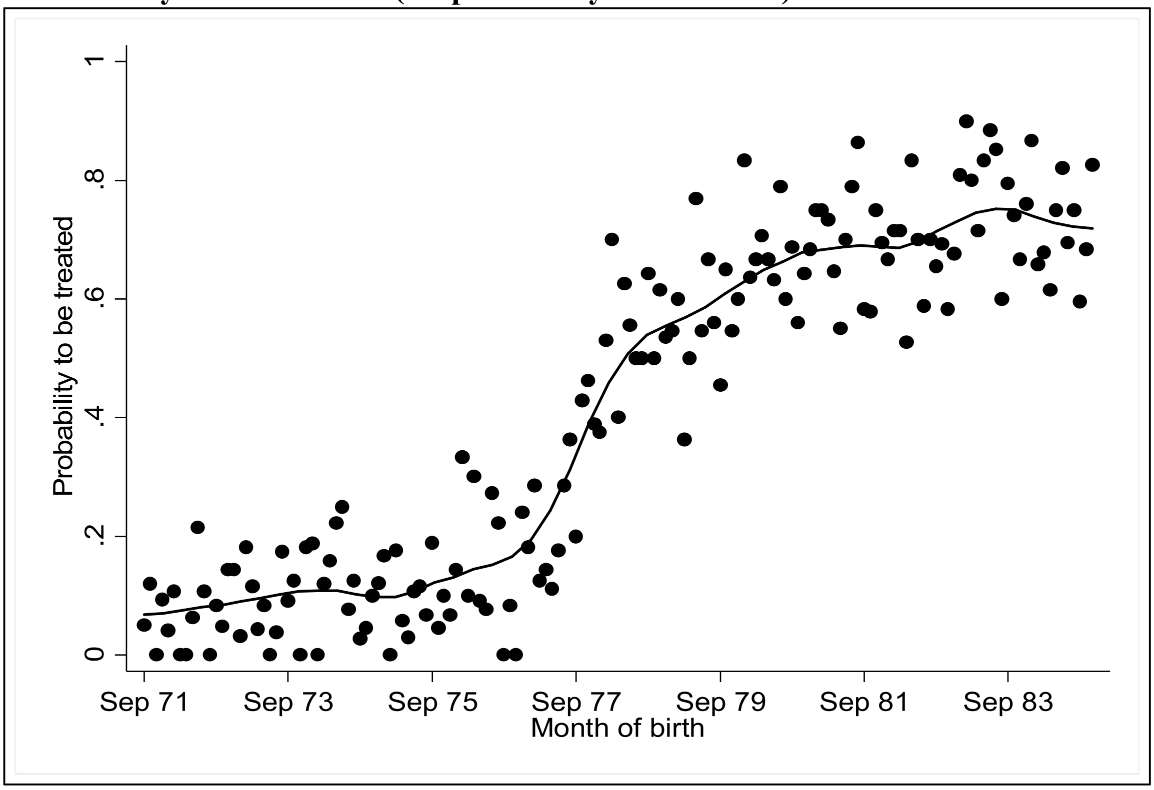
Figures

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



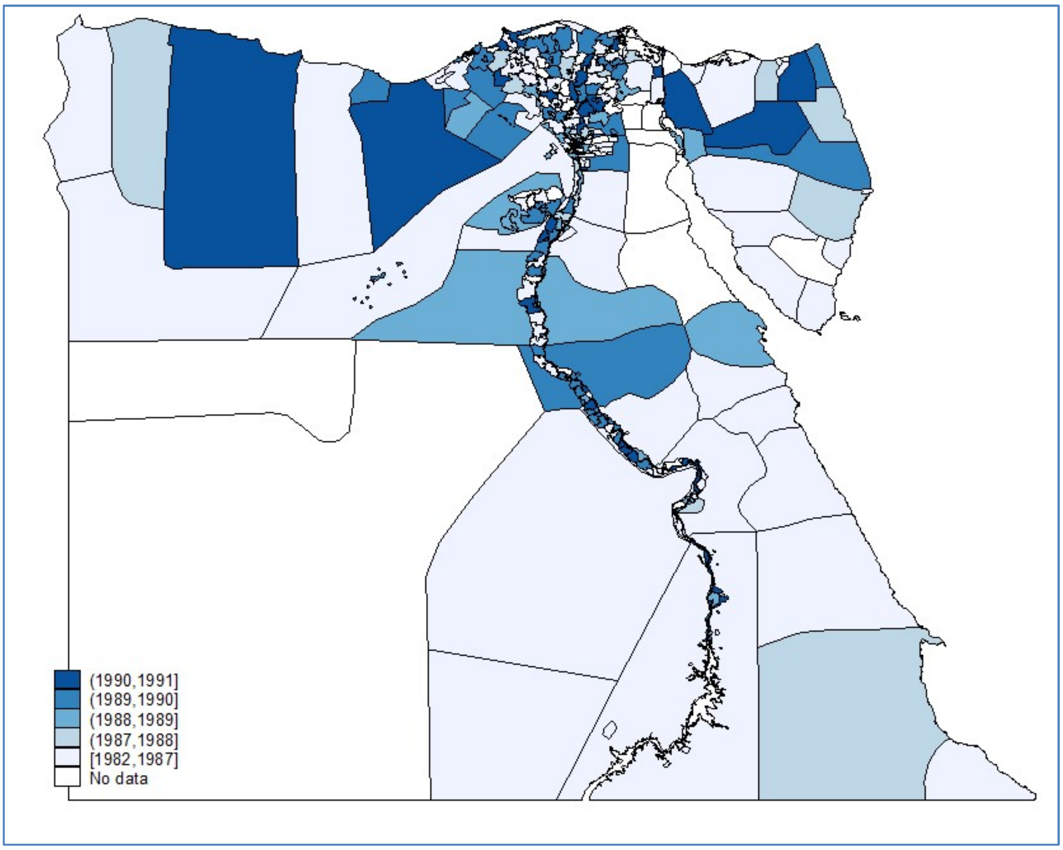
Note: The figure shows the pre-university education system before and after the policy. The vertical line shows the policy change.

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



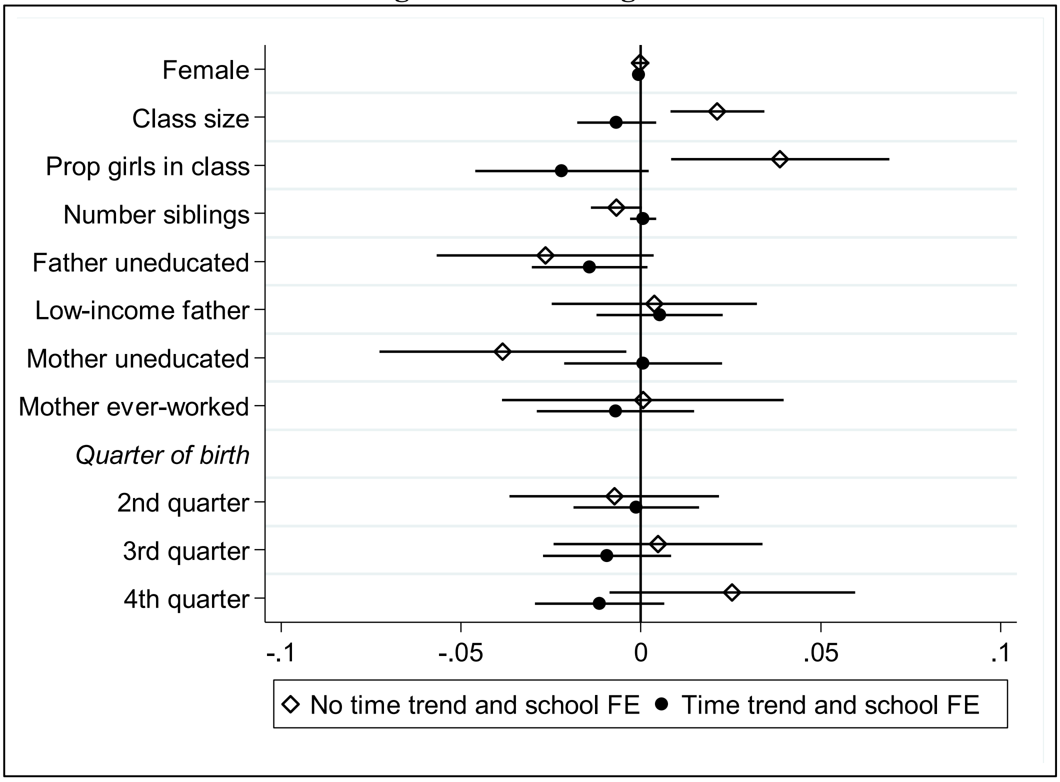
Note: The probability of treatment (i.e. having primary education on a five-year basis instead of six) for each month of birth using the question on actual treatment in ELMPS 2012.

Figure 3: 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 at 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 analyses.

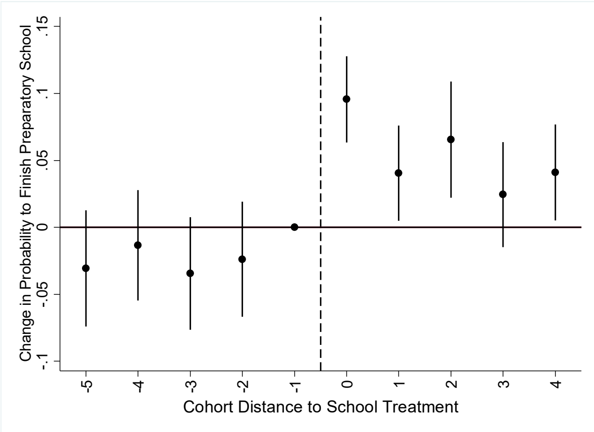
Figure 4: Balancing test



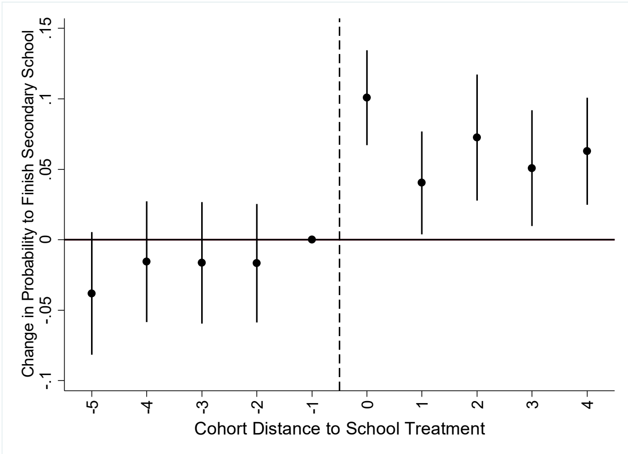
Note: The figure displays the estimated coefficients and confidence intervals of two separate regressions. The number of observations is 8,945. The dependent variable is a dummy variable that takes the value of one if the individual is treated, and zero otherwise. The independent variables are displayed in the vertical axis. Standard errors clustered at the governorate-cohort cells in both regressions.

Figure 5: Effect of the policy on cohorts relative to the time of implementation

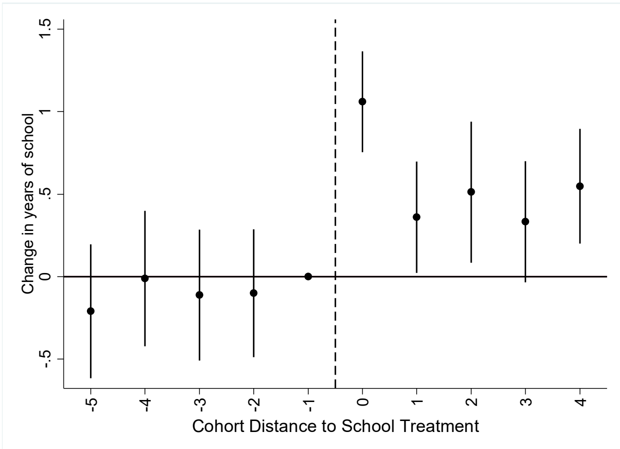
5.1: Probability of finishing compulsory education



5.2: Probability of finishing secondary education

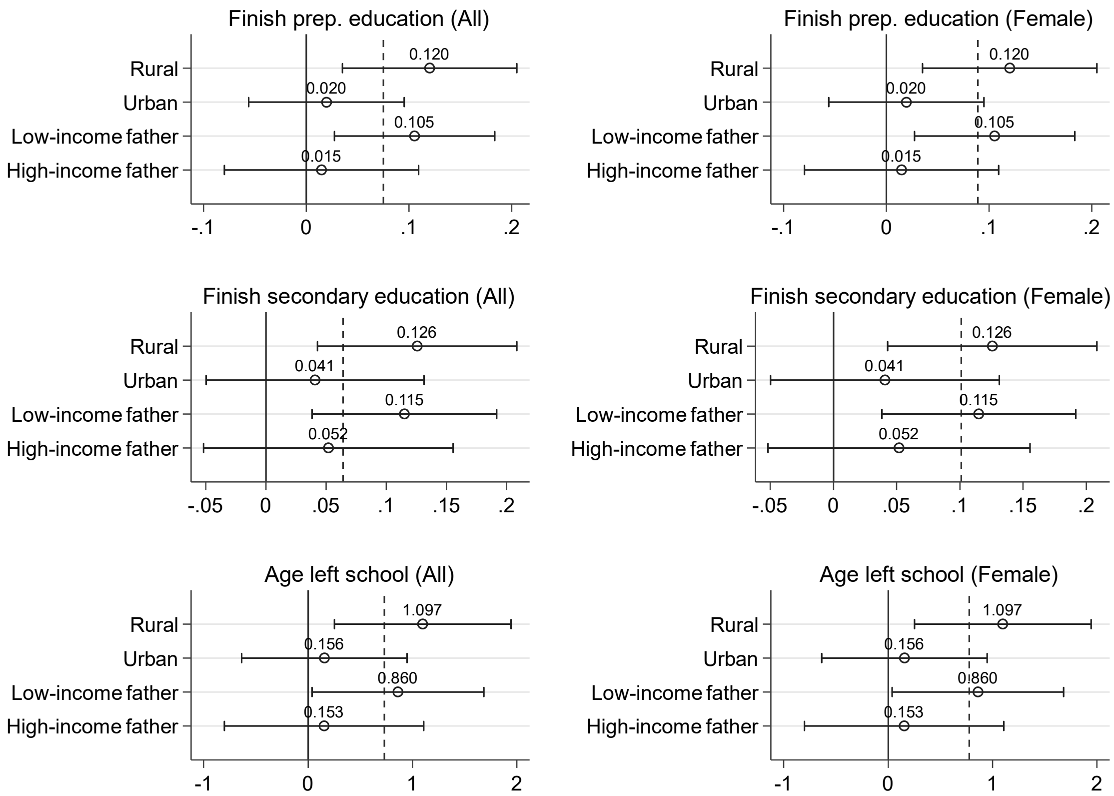


5.3: Age left school



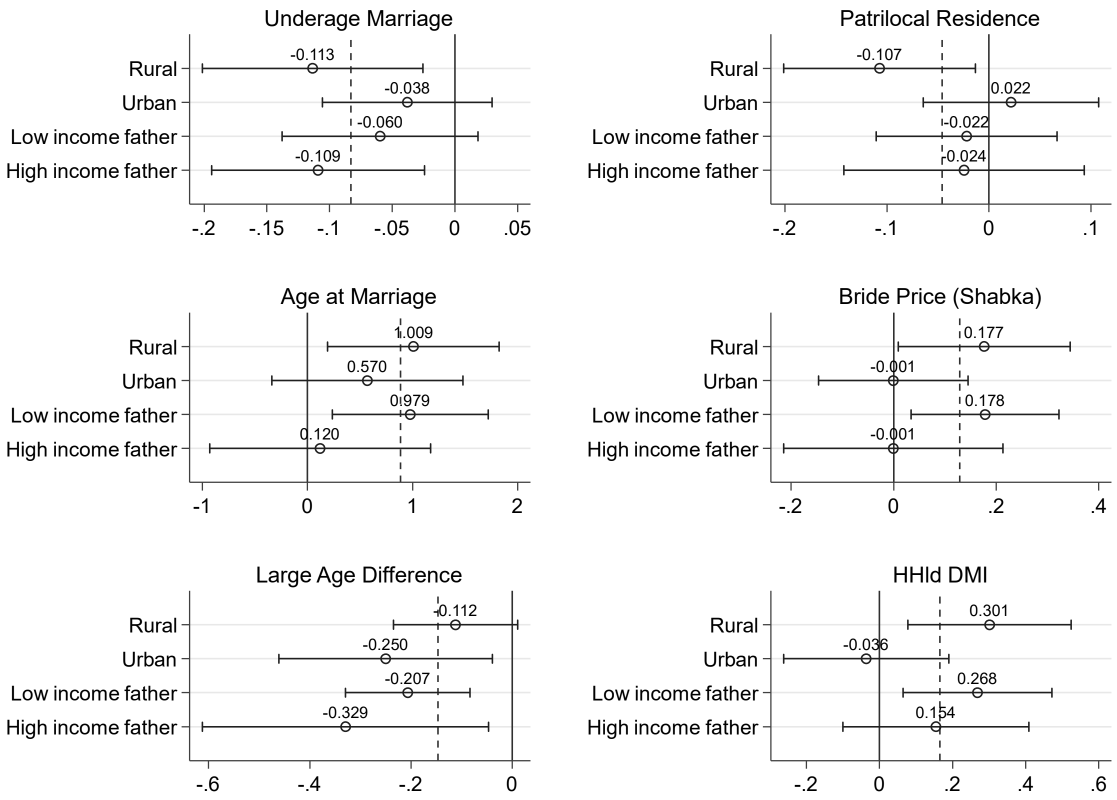
Note: Coefficient estimates and confidence intervals for school-specific cohort dummies from Equation 2. Each point represents the coefficient of relative to cohort -1. The vertical line represents the implementation of the policy.

Figure 6: Policy impact on education by family background and gender



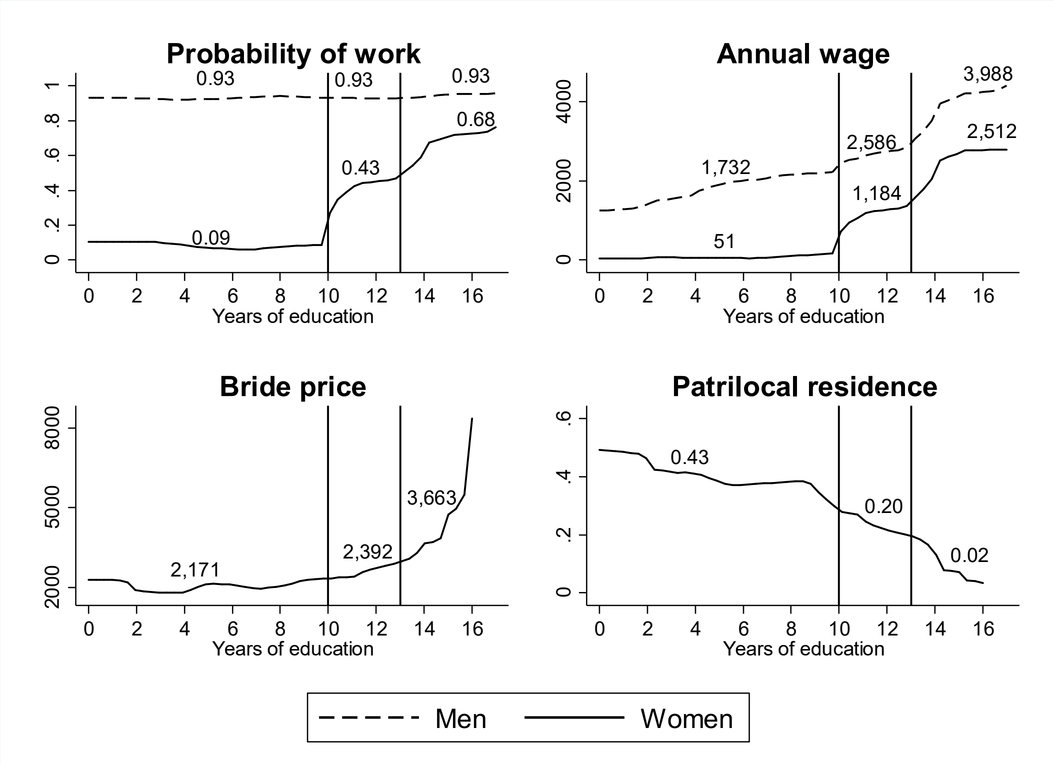
Note: Coefficients and confidence intervals of estimating Equation 1 for different groups separately. Controls are similar to Table 1. The dashed line shows the estimated coefficients for the whole student sample (left) and the whole female sample (right).

Figure 7: Policy impact on marriage outcomes by family background



Note: Coefficients and confidence intervals of estimating Equation 1 for different groups separately. Controls are similar to Table 1. The dashed line shows the estimated coefficients for the whole female sample.

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



Note: Outcomes of education prior to the policy using ELMPS data. 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 E£ at the time of marriage.

Table 1: Descriptive statistics for untreated school cohorts

Means (Standard Deviation)	(1) All	(2) Men	(3) Women	(4) Rural	(5) Urban	(6) Low-income father	(7) High-income father
Education outcomes							
Finish compulsory education	0.66	0.70	0.60	0.51	0.78	0.59	0.77
	(0.48)	(0.46)	(0.49)	(0.50)	(0.41)	(0.49)	(0.42)
Finish secondary education	0.61	0.64	0.56	0.47	0.73	0.55	0.72
	(0.49)	(0.48)	(0.50)	(0.50)	(0.44)	(0.50)	(0.45)
Age left school	15.07	15.74	14.24	13.47	16.48	14.33	16.46
	(4.65)	(4.06)	(5.18)	(5.13)	(3.65)	(4.94)	(3.68)
Labor Market outcomes							
Ever worked	0.66	0.94	0.30	0.64	0.67	0.65	0.67
	(0.48)	(0.24)	(0.46)	(0.48)	(0.47)	(0.48)	(0.47)
Employed	0.61	0.92	0.24	0.61	0.62	0.61	0.62
	(0.49)	(0.28)	(0.42)	(0.49)	(0.49)	(0.49)	(0.48)
Paid job	0.57	0.89	0.17	0.55	0.59	0.55	0.61
	(0.49)	(0.31)	(0.38)	(0.50)	(0.49)	(0.50)	(0.49)
Non-agricultural job	0.87	0.88	0.83	0.77	0.96	0.82	0.97
	(0.33)	(0.33)	(0.37)	(0.42)	(0.20)	(0.39)	(0.17)
Log wage	3.41	3.48	3.09	3.31	3.48	3.37	3.48
	(0.72)	(0.70)	(0.76)	(0.66)	(0.76)	(0.73)	(0.71)
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	-	-	22.06	20.85	23.21	21.54	23.06
	-	-	(4.39)	(4.25)	(4.20)	(4.33)	(4.32)
Large age difference	-	-	0.12	0.12	0.11	0.12	0.12
	-	-	(0.32)	(0.33)	(0.32)	(0.32)	(0.32)
Patrilocal residence	-	-	0.31	0.44	0.18	0.35	0.21
	-	-	(0.46)	(0.50)	(0.38)	(0.48)	(0.41)
Log jewelry (std.)	-	-	0.06	0.03	0.09	0.01	0.15
	-	-	(0.99)	(1.10)	(0.86)	(0.98)	(0.99)
Intra-HH decision-making	-	-	0.02	-0.15	0.18	-0.06	0.16
	-	-	(1.02)	(1.10)	(0.91)	(1.07)	(0.91)
Independent variables:							
Female	0.45	0.00	1.00	0.45	0.44	0.45	0.43
	(0.50)	(0.00)	(0.00)	(0.50)	(0.50)	(0.50)	(0.50)
Low-income father	0.65	0.65	0.66	0.80	0.53	1.00	0.00
	(0.48)	(0.48)	(0.47)	(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)	(0.48)
Number of siblings	4.60	4.49	4.74	5.12	4.14	4.79	4.25
	(2.22)	(2.19)	(2.26)	(2.20)	(2.14)	(2.23)	(2.17)

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

Table 2: Policy impact on education oucomes

	(1)	(2)	(3)	(4)
Panel A - Finished Compulsory Education				
Treatment (5-year primary school)	0.065***	0.063***	0.062***	0.075***
	(0.012)	(0.018)	(0.017)	(0.020)
Mean of Outcome	0.656	0.656	0.656	0.656
Effect size	9.9	9.66	9.48	11.49
Panel B - Finished Secondary Education				
Treatment (5-year primary school)	0.065***	0.055***	0.054***	0.064***
	(0.013)	(0.019)	(0.019)	(0.022)
Mean of Outcome	0.606	0.606	0.606	0.606
Effect size	10.77	9.13	8.93	10.62
Panel C - Age Left School				
Treatment (5-year primary school)	0.574***	0.561***	0.544***	0.730***
	(0.118)	(0.173)	(0.173)	(0.201)
Mean of Outcome	15.068	15.068	15.068	15.068
Effect size	3.81	3.72	3.61	4.84
Sample size	8,945	8,945	8,945	8,945
Controls	Yes	Yes	Yes	Yes
Cohort and School Fixed Effects	No	Yes	Yes	Yes
Quadratic school-specific time trends	No	No	Yes	Yes
Cubic school-specific time trends	No	No	No	Yes

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, dummies for the number of siblings, dummies for month of birth, and a dummy variable that takes the value of 1 if the primary school identifier is missing (replaced then by district), and zero otherwise. There are a total of 2,634 schools in the sample. Robust standard errors clustered by governorate and cohort (313 clusters) reported in parenthesis. *, **, and *** denote significance at the 1, 5, and 10 percent level, respectively.

Table 3: Policy impact on education oucomes – female

	Finish Compulsory Education (1)	Finish Secondary Education (2)	Age Left School (3)
Treatment * Female	0.095***	0.093***	1.043***
	(0.022)	(0.023)	(0.223)
Treatment	0.034	0.023	0.271
	(0.023)	(0.023)	(0.216)
Female	-0.122***	-0.119***	-1.858***
	(0.020)	(0.021)	(0.212)
Mean of Outcome Women	0.602	0.558	14.24
Mean of Outcome Men	0.699	0.645	15.74
Controls	Yes	Yes	Yes
Cohort and School Fixed Effects	Yes	Yes	Yes
Cubic School-Specific time trend	Yes	Yes	Yes
Sample size	8,945	8,945	8,945

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, dummies for the number of siblings, dummies for month of birth, and a dummy variable that takes the value of 1 if the primary school identifier is missing (replaced then by district), and zero otherwise. There are a total of 2,634 schools in the sample. Robust standard errors clustered by governorate and cohort (313 clusters) reported in parenthesis. *, **, and *** denote significance at the 1, 5, and 10 percent level, respectively.

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

Panel A: Average Policy Effect	Ever worked	Currently working	Currently in Paid job	Currently in Non-agr. job	Log wage
Treatment	0.041**	0.013	0.033**	0.044**	0.189***
	(0.016)	(0.016)	(0.014)	(0.021)	(0.069)
Mean value of outcome	0.656	0.613	0.931	0.872	3.090
Effect size at mean	6.22	2.07	3.49	5.06	6.13
Panel B: Policy Effect for Women	Ever worked	Currently working	Currently in Paid job	Currently in Non-agr. job	Log wage
Treatment * Female	0.048**	0.084***	0.057	0.074**	0.283**
	(0.021)	(0.020)	(0.041)	(0.029)	(0.132)
Treatment	0.020	-0.024	0.024*	0.033	0.135*
	(0.017)	(0.018)	(0.014)	(0.021)	(0.072)
Female	-0.648***	-0.688***	-.0284***	-0.062***	-1.280***
	(0.015)	(0.014)	(0.028)	(0.020)	(0.097)
Mean of Outcome Women	0.304	0.236	0.733	0.832	2.160
Mean of Outcome Men	0.938	0.917	0.972	0.88	3.340
Controls	Yes	Yes	Yes	Yes	Yes
Cohort and School Fixed Effects	Yes	Yes	Yes	Yes	Yes
Cubed school-specific time trends	Yes	Yes	Yes	Yes	Yes
Sample size	8,945	8,945	5,037	5,037	3,649
Number of schools	2,634	2,634	1,772	1,772	1,480

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, dummies for the number of siblings, dummies for month of birth, and a dummy variable that takes the value of 1 if the primary school identifier is missing (replaced then by district), and zero otherwise. Robust standard errors clustered by governorate and cohort (313 clusters) reported in parenthesis. *, **, and *** denote significance at the 1, 5, and 10 percent level, respectively.

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)	-0.083***	0.887***	-0.147***	-0.046	0.129**	0.165**
	(0.029)	(0.311)	(0.055)	(0.034)	(0.057)	(0.075)
Mean of Outcome	0.150	22.1	0.116	0.306	-	-
Effect size (%)	-55.1	4.02	-126	-15.1	12.9	16.5
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cohort and Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Cubic school-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	3,577	3,577	1,115	1,115	3,528	3,517
Number of schools	1,552	1,552	611	611	1,540	1,545

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, dummies for the number of siblings, dummies for month of birth, and a dummy variable that takes the value of 1 if the primary school identifier is missing (replaced then by district), and zero otherwise. Robust standard errors clustered by governorate and cohort (313 clusters) reported in parenthesis. *, **, and *** denote significance at the 1, 5, and 10 percent level, respectively.

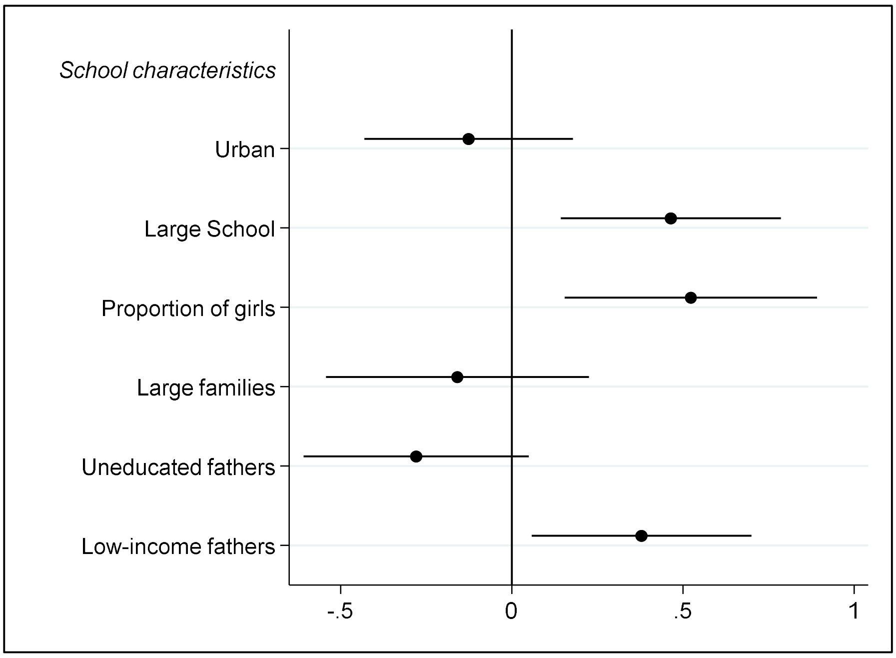
Table 6: Policy impact on quality of education

Quality Measure:	**School in Shifts**		**Computer Use**		**Physical Punishment**	
Education Stage:	Primary	Preparatory	Primary	Preparatory	Primary	Preparatory
Treatment (5 Year primary school)	-0.046**	-0.014	-0.020	0.012	0.003	0.016
	(0.023)	(0.028)	(0.025)	(0.033)	(0.018)	(0.024)
Mean of Outcome	0.413	0.411	0.194	0.204	0.857	0.843
Effect size	-11.1	-3.43	-10.4	5.66	0.41	1.87
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cohort and School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
School-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	7,548	6,429	5,133	4,781	7,507	6,381
Number of schools	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, dummies for the number of siblings, dummies for month of birth, and a dummy variable that takes the value of 1 if the primary school identifier is missing (replaced then by district), and zero otherwise. There are a total of 2,634 schools in the sample. Robust standard errors clustered by governorate and cohort (313 clusters) reported in parenthesis. *, **, and *** denote significance at the 1, 5, and 10 percent level, respectively.

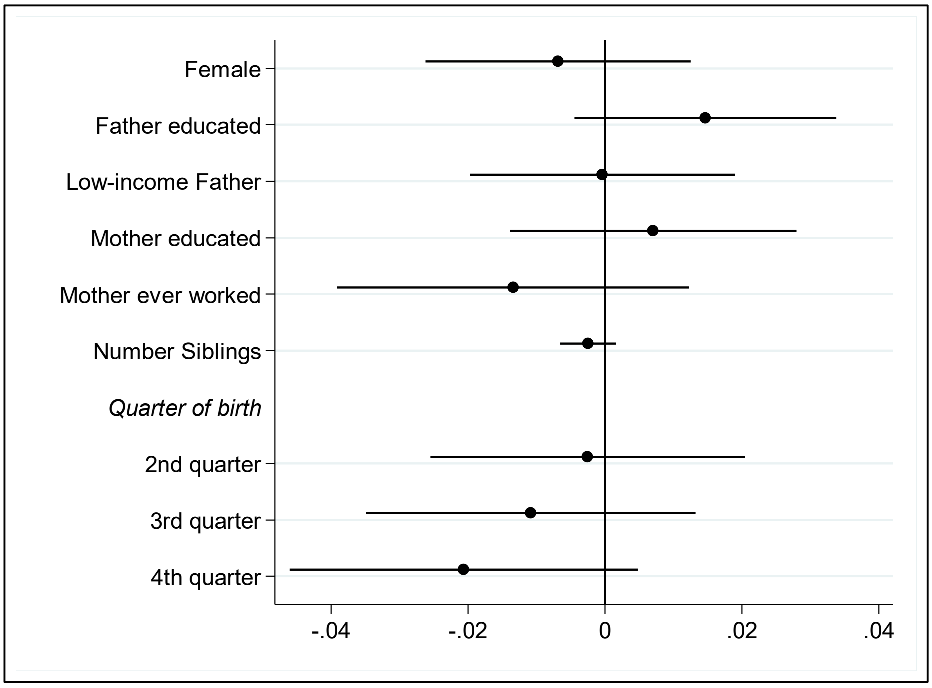
Online Appendix

Figure A1: Timing of policy introduction by school characteristics



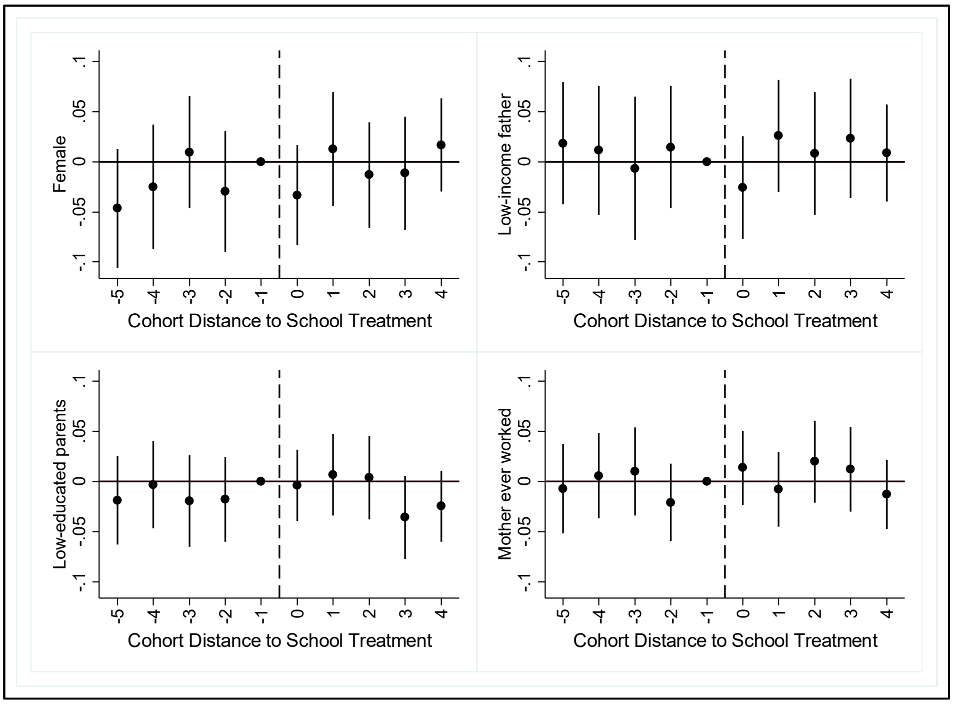
Note: The figure displays the estimated coefficients, and 95 percent confidence intervals, from a regression of the year in which a school introduced the policy on the characteristics of its student body as described on the vertical axis.

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



Note: The figure displays the estimated coefficient, and 95 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 already reported, and zero otherwise. The independent variables are displayed in the vertical axis. Standard errors clustered at the governorate-cohort cells. The number of observations for whom the actual treatment is known is 3,332.

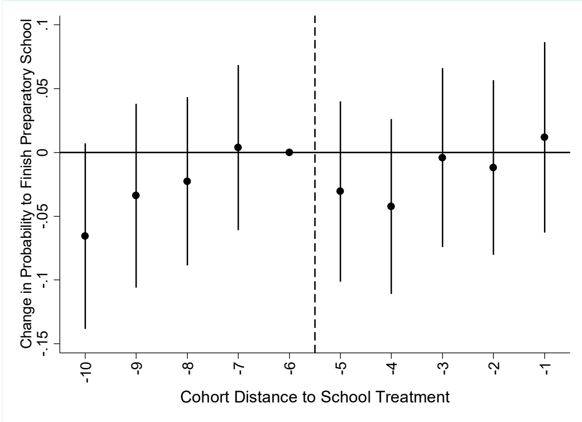
Figure A3: Testing for pre-trend and policy impact on covariates



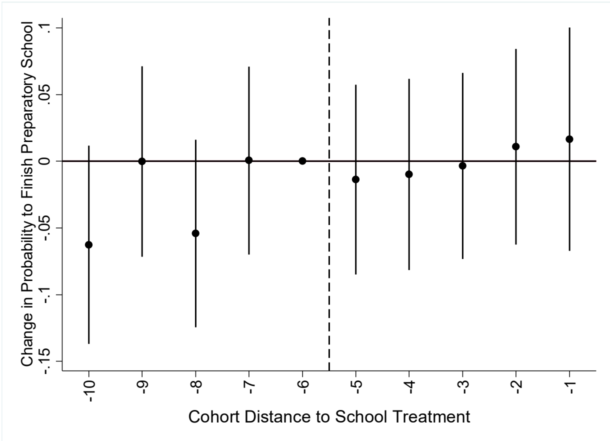
Note: The figure displays the coefficient estimates and confidence intervals for the impact of the policy on different covariates for cohorts around the time of implementation. Each point represents the coefficient relative to cohort -1. The vertical line represents the implementation of the policy.

Figure A4: Placebo policy effect on education using earlier cohorts

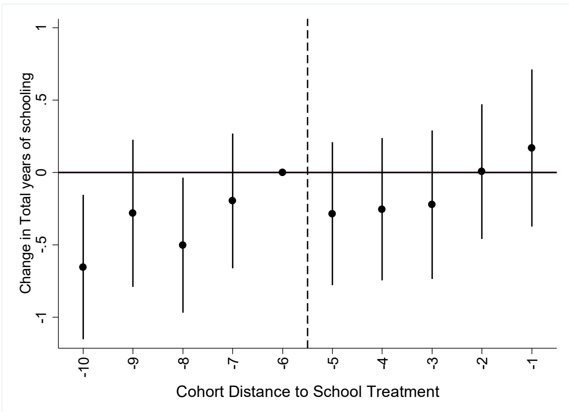
A4.1: Probability of finishing compulsory education



A4.2: Probability of finishing secondary education



A4.2: Probability to finish secondary education



Note: Placebo regression for the impact on earlier cohorts. Coefficient estimates and confidence intervals for school-specific cohort dummies from Equation 2. Each point represents the coefficient relative to cohort -6 The vertical line represents six years before 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 got married before the legal age of 18, zero otherwise.
Age of marriage	- Age at which a woman got 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) Treatment assigned at district and cohort level			
Treatment	0.070***	0.066***	0.656***
	(0.018)	(0.019)	(0.162)
Mean of Outcome	0.656	0.597	14.99
Effect size	10.8	11.1	4.38
Sample size	8,945	8,945	8945
B) School cohort treated if 1/3rd or more of surveyed respondents report treatment			
Treatment	0.064***	0.065***	0.701***
	(0.018)	(0.019)	(0.186)
Mean of Outcome	0.656	0.604	15.06
Effect size	9.69	10.8	4.65
Sample size	8,945	8,945	8945
C) School cohort treated if 2/3rd or more of surveyed respondents report treatment			
Treatment	0.070***	0.060***	0.685***
	(0.020)	(0.022)	(0.200)
Mean of Outcome	0.655	0.606	15.07
Effect size	10.71	9.83	4.55
Sample size	8,945	8,945	8,945
D) Limit to schools with at least two individuals before and after treatment			
Treatment	0.096***	0.081***	0.869***
	(0.021)	(0.023)	(0.215)
Mean of Outcome	0.614	0.569	14.58
Effect size	15.65	14.17	5.96
Sample size	6,810	6,810	6,810
E) Limit to schools with at least five individuals before and after treatment			
Treatment	0.088***	0.065**	0.763***
	(0.026)	(0.027)	(0.253)
Mean of Outcome	0.594	0.553	14.323
Effect size	14.8	11.83	5.33
Sample size	5,442	5,442	5,442
F) Use actual treatment variable			
Actual Treatment	0.085***	0.080***	0.650***
	(0.026)	(0.028)	(0.210)
Mean of Outcome	0.753	0.686	16.38
Effect size	11.35	11.67	3.97
Sample size	3,332	3,332	3,332
G) IV: actual treatment instrumented by assigned treatment			
Actual treatment	0.128***	0.109**	0.711*
	(0.050)	(0.052)	(0.378)
Mean of Outcome	0.753	0.686	16.38
Effect size	16.99	15.89	4.34
First stage			
Assigned treatment	0.883***	0.883***	0.883***
	(0.033)	(0.967)	(1.967)
Sample size	3,332	3,332	3,332

Note: Controls are as in Table 2. Overall, there are 583 school district in Panel A's specification. The numbers of schools are otherwise: 2,634 in Panels B and C, 855 in Panel D, 410 in Panel E, and 1,860 in Panels F and G. Robust standard errors clustered by governorate and cohort (313 clusters) reported in parenthesis. *, **, and *** denote significance at the 1, 5, and 10 percent level, respectively.

Table A3: Heterogeneity of policy impact on female labor market outcomes by family background characteristics

VARIABLES	(1) Rural	(2) Urban	(3) Low-income father	(4) High-income father
A) Ever worked				
Treatment * Female	0.084***	0.014	0.044*	0.039
	(0.025)	(0.030)	(0.025)	(0.041)
Mean of Outcome Women	0.248	0.354	0.285	0.342
Mean of Outcome Men	0.956	0.923	0.946	0.923
Sample size	4,327	4,618	5,816	3,129
B) Currently working				
Treatment * Female	0.111***	0.059**	0.080***	0.090**
	(0.026)	(0.028)	(0.024)	(0.039)
Mean of Outcome Women	0.212	0.258	0.224	0.259
Mean of Outcome Men	0.938	0.898	0.926	0.901
Sample size	4,327	4,618	5,816	3,129
C) Paid job				
Treatment * Female	0.144***	0.080***	0.118***	0.091**
	(0.022)	(0.029)	(0.021)	(0.040)
Mean of Outcome Women	0.113	0.227	0.14	0.238
Mean of Outcome Men	0.902	0.882	0.893	0.888
Sample size	2,492	2,545	3,272	1,765
D) Non-agr. job				
Treatment * Female	0.125***	0.018	0.115***	-0.012
	(0.047)	(0.027)	(0.041)	(0.035)
Mean of Outcome Women	0.673	0.951	0.758	0.959
Mean of Outcome Men	0.791	0.960	0.83	0.973
Sample size	2,492	2,545	3,272	1,765
E) Log wage				
Treatment * Female	0.519***	0.135	0.365**	0.065
	(0.167)	(0.160)	(0.157)	(0.178)
Mean of Outcome Women	1.253	2.76	1.689	2.943
Mean of Outcome Men	3.152	3.478	3.25	3.47
Sample size	1,648	2,001	2,245	1,404

Note: Controls similar to Table 4, except the variable for which heterogeneous effects are estimated.

Table A4: Costs and returns to different stages of education

	All			Men			Women		
	Primary	Preparatory	Secondary	Primary	Preparatory	Secondary	Primary	Preparatory	Secondary
Prob. higher education level	0.15	0.10	0.44	0.16	0.10	0.43	0.14	0.09	0.44
Average annual education cost	478.22	758.51	1322.02	488.1	733.56	1175.35	467.79	785.04	1,482
Employed	0.68	0.62	0.61	0.99	0.96	0.93	0.14	0.09	0.17
Paid job	0.65	0.59	0.59	0.97	0.94	0.90	0.06	0.06	0.14
Annual wage	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	0.9	0.91	0.92	0.91
Received Shabka							0.99	0.99	0.99
Shabka real value (in E£)							6162.03	7939.13	8484.54

Note: Authors' calculations from 2006 and 2012 ELMPS waves.