

Education as Opportunity? The Causal Effect of Education on Labor Market Outcomes in Jordan

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Abstract:

This paper studies the impact of the 1988/1989 educational reform in Jordan which extended mandatory schooling from nine to ten years and restructured secondary schooling. Despite weakness in the Jordanian labor market, our estimates suggest that an additional year of required schooling in the late 1980s was sufficient to improve labor force participation, employment, and wages. These effects were initially largest for women, while males with more education were also slightly more likely to be self-employed, work longer hours, and earn higher wages. We show that the extensive margin labor market gains we observe for women dissipate rapidly over the life cycle. In contrast, the impacts we observe for men strengthen with age. These patterns are consistent with a persistent influence of traditional gender norms in Jordanian society influencing labor market decision making.

JEL Codes: O12, I21, E24

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1. Introduction

Rapid gains in education without corresponding improvements in job prospects have been cited as one of the leading causes of the Arab Spring (Campante and Chor, 2012). Several nations across the Middle East and North Africa region have experienced remarkable expansions in educational attainment over the past three decades without correspondingly large economic gains in the labor market. The Hashemite Kingdom of Jordan is no exception. Despite having the 9th largest increase in years of education around the globe from 1980 to 2010, today the nation exhibits double-digit unemployment rates (15% for men and 33% for women), a female labor force participation rate around 15%, and a wage premium for higher education lower than that for secondary school completion. Like many of its neighbors, Jordan underwent protests during the Arab Spring in 2011 and continued weakness in the labor market remains a catalyst for new demonstrations taking place in 2018.

In this paper we analyze the labor market effects of recent educational reforms in Jordan which extended the length of compulsory schooling. Although compliance with the compulsory schooling reform was imperfect, our fuzzy regression discontinuity estimates suggest that the 1988 educational reform increased schooling attainment among the population obtaining an educational degree by nearly a quarter of a year in the first year, and implementation improved gradually in the years after.¹ Our estimates suggest that the additional education is responsible for a small increase in the probability of entering the workforce and being employed, an increase in the use of self-employment, and gains in hours worked and wages.

We disaggregate these gains by gender, showing that the impacts on Jordanians at the time of their entrance into the labor market of additional schooling are significantly larger for women, particularly on the extensive margin. Males instead realize gains in hours worked, which is consistent with the fact that nearly all men already participate in the labor market in by their mid-20s. Exploiting novel features of our data to analyze affected cohorts at later points in their life, we then show that the labor market gains realized by women are transitory, disappearing as women exit the labor market in their 30s and 40s. In other words, the gains in labor force participation, employment prospects, and wages, are insufficient to

¹ The average increase in educational attainment differs depending on whether the sample includes graduates with secondary and higher degrees. While compulsory preparatory schooling was lengthened by a year, this only impacted attainment for those stopping with this as a terminal degree, as secondary school (which was non-compulsory) was concurrently shortened from 3 to 2 years.

outweigh the opportunity cost of exiting the labor market as women progress over the life cycle in the Jordanian setting. In contrast, males appear to engage more strongly over this window and to benefit both at the intensive and extensive margin over the course of their careers.

Analysis of schooling reform in Jordan presents particularly interesting case study for a number of reasons. First, while many of the causal studies of education in the empirical economics literature have relied on extensions to compulsory schooling or changes to school leaving age laws, this research has demonstrated heterogeneous effects. Some papers have found positive, significant, and sizeable estimates of the returns to schooling (Oreopoulos, 2006, 2007), while other studies have uncovered contexts in which the return to schooling appears to have been either quite low or even zero (Albouy and Lequien, 2008; Pischke, and Wachter, 2008). These variations highlight the sizeable degree to which educational investments exhibit heterogeneous returns across settings and time. Furthermore, research has historically been focused primarily on high income settings, for example, studying reforms in the United States or Europe, which are largely secular educational systems. Less is known about how well the estimates produced in these studies generalize to regions, particularly those in the developing world, with dramatically different curricula and educational agendas with non-standard objective functions, as well as under different sets of economic conditions and societal norms of behavior.

Our work also relates to a number of papers which have examined the causal impact of education on non-labor market outcomes. For example, studies have demonstrated an impact of increased education on the probability of voting (Milligan et al., 2004), health and mortality (Silles, 2009; Kippersluis et al., 2010; Powdthavee, 2010), teenage childbearing (Black et al., 2008; Silles, 2011), and intergenerational human capital accumulation (Oreopoulos, 2006; Assaad and Saleh, 2016).

The remainder of this paper is organized as follows. Section 2 provides the background on the economy as well as on the history of education reform in Jordan. Section 3 describes the data used in the analysis. Section 4 details our empirical strategy. Section 5 presents results for the impact of education on labor market outcomes both initially and over the life cycle. Section 6 presents a series of robustness checks regarding the empirical strategy and data assumptions. Section 7 concludes.

2. Background

2.1 The Government and Economy of Jordan

The Hashemite Kingdom of Jordan gained independence as a hereditary constitutional monarchy from the British in 1946. Jordan's economy in the post-independence period has been affected by regional instability and conflict, including a series of international conflicts with Israel (1948/1949 First Arab Israeli War; 1967 Six Day War; 1967-1970 War of Attrition; 1973 October War) and domestically, through confrontations with the PLO, most notably the events surrounding Black September which persisted from 1970 to 1971 (Robbins, 2004). By comparison, our study period which spans education and labor market participation in the 1980s, 1990s, and 2000s has been one of relative stability for the county. As of the mid 2010s, the nation again finds itself front and center in a refugee crisis, with inflows from socio-economic and political turbulence in neighboring countries.

These periods of conflict have complicated provision of education in Jordan, as large refugee populations have sporadically entered Jordan. Notably, during the 1948 Palestinian Exodus – some 250,000 to 300,000 Palestinians arrived in Jordan, with smaller migration flows continuing over the period 1949-1956 and again another quarter million individuals fled the West Bank and Gaza for Jordan during the 1967 Exodus. As Jordan did not have a sizeable population of Sephardi or Mizrahi, it also did not experience a similar outmigration during the corresponding Jewish exodus from Arab countries from 1948 into the 1970s, such as that from Yemen, Libya, or Iraq. Periodic inflows of population both strained resources and acted as catalyst for the expansion of educational infrastructure in the state. Today Jordan is home to the second largest refugee population in the world on a per capita basis. These inflows, coupled with high fertility rates, have led to a rapid population expansion in the country, with some of the largest gains in the urban centers such as Amman.

Economically, Jordan is still considered a developing nation. The economy expanded rapidly from the 1970s through the mid-1980s, with GDP growth often exceeding 10% a year. Jordan has occasionally been labelled a rentier state because of its large reliance on external support. This pattern peaked in the 1970s when dramatically increased aid flows arrived from a boom in Middle East oil revenues. These began to decline in late 1980s saw a decline in culminating in a federal debt crisis. Slower growth returned in the 1990s,

accelerating into the 2000s, and today, Jordan has a per capita income of roughly \$4,000 PPP. In spite of the economic progress over the period, unemployment in the country has remained relatively high.

2.2 Educational Reforms in Jordan

A number of elements make Jordan a novel setting in which to examine educational reform. Having only existed as a formal polity for around 70 years, the country is still in the process of defining and shaping its educational policy as well as expanding access to and resources for the educational system. While seen as a strong performer in comparison to peers in the MENA Region, Jordan is still rather unique for the sheer volume of refugee flows with which the country has had to cope. Driven by conflicts in surrounding countries, Jordan is now second only to Lebanon for refugees on a per capita basis, with particularly large Palestinian and Syrian populations.

The Educational reform law of 1952 established education as a right of every Jordanian citizen and introduced compulsory schooling for a period of 7 years (this was subsequently reduced to 6 years in the 1954/1955 school year). Education Law no. 16 of 1964 extended schooling to 9 years (known at the time as preparatory school) as compulsory and ushered in a broad expansion to education - including more schools, more teachers, increased teacher training, and a unified philosophy of education (Abbas, 2012). This reform would ultimately have to cope with a large increase in demand for schooling, given large numbers of Palestinians arriving from the West Bank into Jordan in the aftermath of the 6 day war in 1967.

In this paper, we focus on the impact of provisional Education Act No. 27 in 1988 whose primary effect was to increase free compulsory schooling from 9 to 10 years (renamed from Preparatory to Basic education). All children 6 years and older are required to attend school and must continue through ten years of basic education (Hodges, 2005). While the previous reforms are larger increases in years of education, two things complicate the analysis of their impact on labor market outcomes. First, they were implemented gradually as a result of limited state resources for education, and second, they occurred far enough in the past that many respondents in the Census and EUS surveys affected by the reforms may have exited the labor market at the time survey.

Compulsory schooling in Jordan is equivalent in duration to completing primary school in the U.S. as well as two years of high school. After completing Basic education, students are eligible to enter secondary school. Prior to the reform, secondary school was heterogeneous in nature and lasted 3 years. After the reform, a “new secondary” program was implemented which standardized the curriculum, created specific tracks of study, and decreased the length of secondary school to 2 years. This means that the combined effect of Education Act No. 27 was to change the nature of secondary school, but not to alter the total number of years required to reach a secondary degree before and after the reform.

The next significant revision to the educational system would not occur until 1994, when Educational Act No. 3 reformed the philosophy and objectives of education in the country.

3. Data and Strategy

We combine data from multiple sources in our analysis. Individual level educational attainment and labor force outcomes are derived both from the 2004 Jordanian Census (IPUMS, 2018) as well as from the Jordanian Employment and Unemployment Surveys (EUS) for 2006, 2007, 2008, 2009, 2011, 2013, 2014, and 2016. The 2004 Census is a nationally representative, stratified, 10%, sample of the Jordanian population, representing about half a million individuals. Estimates from the Department of Statistics report a response rate of 95.9%. The EUS are representative at both the national and governate level, and are conducted quarterly. We employed the full set of harmonized EUS compiled and disseminated by the Economic Research Forum (OAMDI, 2017).

Table 1 presents summary statistics for our Census sample.² Panel A contains individual level demographics and work outcomes. At the time of enumeration for the 2004 census, the mean individual in the sample is 30 years old, roughly 65% of males are married; while roughly 76% of women are married, reflecting an earlier average age of marriage and child birth for women. Astoundingly, in this age group over 90% of males participate in the labor force, while roughly 30% of women participate. These reflect prime working age in the country as both values are significantly higher than for the population at large. Marriage and large families are common. Roughly three-quarters of women have married by age 30, and for older cohorts this rises above 90%. While the total fertility rate in Jordan has declined

² Corresponding summary statistics for the EUS sample are presented in Appendix Table 1.

since the 1980s when it exceeded 7, it remains high enough for population growth at 3.3 today (WB, 2018).

The 2004 Jordanian Census recorded educational outcomes for those who have completed their education as a categorical variable divided into bins. Figure 1 depicts educational attainment for individuals born in birth cohorts ranging from 1967 to 1980³. As can be seen from the upper right and lower left panels of the figure, a visible discontinuity occurs from the 1973 to 1974 birth cohorts. It is this forced jump in education attainment we exploit to derive causal estimates of educational attainment. It is also clear that the transition from Preparatory to Basic education did not completely transition from one cohort to the next. It is for this reason that we adopt a fuzzy regression discontinuity strategy. We consider a number of explanations for the imperfect adjustment ranging from incomplete compliance with the compulsory schooling rule, to insufficient resources to implement the reform, to survey response error in Section 5.1.

Figure 1 presents mean years of education for individuals in our sample frame who have completed any degree. There are no visibly large discontinuities in the share of the population attaining secondary or higher education after the reform as well; suggesting the increase in education we study is predominantly confined to the impact of increasing the duration of compulsory years from 9 to 10. As discussed in Section 2, at the same time that the reform increased the duration of basic compulsory education, it reduced the duration of secondary school by 1 year, meaning that all higher degrees such as secondary, masters, and bachelor's degrees still represented the same number of years of educational attainment. Panel B of Table 1 contains summary statistics on educational attainment for our sample.

Corresponding statistics for the EUS sample are presented in Appendix Table 1. The two samples generally paint a similar picture of the Jordanian population, with a few nuances worth pointing out. First, the EUS waves begin in 2006 and run through 2016, so they observe our birth cohorts of interest across a series of later points in their lives. Second, the EUS surveys contain additional information on wages, which we discuss and examine in more detail in Section 5.3. A third interesting difference is that the EUS sample suggests 41% of males and 56% of women work in the public sector. In the census the corresponding numbers are 35 and 30%. Some of this difference may be attributable to how the questions

³ We present these figures for a wide band which we use in some robustness checks and which we believe provides useful context on the pre and post reform trends in educational attainment in the country. By the nature of our empirical approach, we typically examine narrower intervals around the educational reform.

are worded, and government statistics suggest the EUS numbers are closer to reality, with women disproportionately seeking the public sector over the private. Anecdotally, public sector employment is often considered more socially acceptable for women.

To assess the availability of educational resources surrounding the reform, we compile a number of statistics from disparate sources. First, using data on the date of construction for the corpus of schools in Jordan from Ministry of Education’s 2010 Census of Schools in Jordan, we retrospectively construct the number of schools present in each district in each year following the general approach of Assaad and Saleh (2016).⁴ From annual statistical yearbooks, we complement data on the number of schools with information on the total number of teachers and pupils in the country. This data allows us to construct average values for the number of teachers per school, students per school, and the teacher/pupil ratio at a regional level over time – a set of proxies for the evolution of educational resources and demands on the educational system.

4. Empirical Strategy

4.1 Methodology

As discussed in Section 2, the educational reform extended *de jure* years of compulsory schooling in Jordan from nine to ten, which theoretically should create a discontinuity in years of education attained across cohorts. In practice, *de facto* enforcement of the new Basic, 10-year, compulsory schooling was imperfect, with some individuals still only attaining nine years of schooling. Nevertheless, compliance was still significant enough to generate the discontinuity visible in Figures 1 and 2, and it is this development that we exploit to examine the causal impact of educational on labor market outcomes.

From an econometric standpoint, these nuances of the data means that the treatment, T_i – an additional year of completed required schooling – is determined only in part by the value a forcing variable, x_i , cohort of birth, since when an individual was born determines whether they were exposed to the schooling reform. Even with imperfect compliance, it is possible to study the causal impact of education reform using regression discontinuity approaches as long as the probability of treatment is discontinuous at a cutoff point, denoted by x_0 . As can be seen in Figure 1, there is a discrete break in educational

⁴ Appendix A discusses this process and the assumptions necessary to construct and use the school census in this manner.

attainment across the 1973 and 1974 birth cohorts in which roughly half of the typical population attaining preparatory suddenly began attaining basic instead. And as discussed previously, the fraction of the population attaining other degrees exhibits no large discontinuities around this time threshold.

We thus employ a fuzzy regression discontinuity design to evaluate the impact of increased educational attainment resulting from the 1988/1989 school reforms.⁵ Following Imbens and Lemieux (2008), we assign $T_i = 1$ when individual i receives an additional year of schooling and $T_i = 0$ when they do not. Probability of assignment to the treatment is then given by:

$$\Pr(T_i = 1 | x_i) = E(T_i | x_i) \quad (1)$$

and the following assumption is satisfied:

$$\lim_{x_i \rightarrow x_0^+} E(T_i | x_i) \neq \lim_{x_i \rightarrow x_0^-} E(T_i | x_i) \quad (2)$$

when the probability of treatment is discontinuous at the threshold x_0 . We further denote y_{0i} to be the outcome if individual i has not been treated and y_{1i} be the treated outcome for individual i . Given y_i as an observed outcome, it can be expressed as follows:

$$y_i = \alpha_i + \rho_i T_i \quad (3)$$

where $\alpha_i = y_{0i}$ and $\rho_i = y_{1i} - y_{0i}$. In this setup, ρ_i is the treatment effect for individual i . However, in practice, we never observe y_{1i} and y_{0i} together. Instead, we can estimate the average effect of the treatment and derive the treatment effect ρ over our sub-population of interest.

An essential property for clean identification is that the conditional mean function $E(\alpha_i | x_i)$ is continuous at $x_i = x_0$. Stated differently, our fuzzy regression discontinuity approach requires that in the absence of the reform, average outcomes would otherwise be similar for individuals just below the cutoff in comparison to those just above the cutoff. As long as this assumption holds, then it can be shown that

$$\begin{aligned} & \lim_{x_i \rightarrow x_0^+} E(y_i | x_i) - \lim_{x_i \rightarrow x_0^-} E(y_i | x_i) \\ &= \rho \left(\lim_{x_i \rightarrow x_0^+} E(T_i | x_i) - \lim_{x_i \rightarrow x_0^-} E(T_i | x_i) \right) \end{aligned} \quad (4)$$

$$\Rightarrow \rho = \frac{\lim_{x_i \rightarrow x_0^+} E(y_i | x_i) - \lim_{x_i \rightarrow x_0^-} E(y_i | x_i)}{\lim_{x_i \rightarrow x_0^+} E(T_i | x_i) - \lim_{x_i \rightarrow x_0^-} E(T_i | x_i)} \quad (5)$$

⁵ Regression discontinuity designs were first introduced by Thistlethwaite and Campbell (1960), while Hahn *et al.*, (2001) contributed the key theoretical framework and concepts for the application of fuzzy RD design.

where ρ provides an estimate of the local average treatment effect. In analysis, we revisit this assumption and discuss its plausibility in the context of this particular Jordanian educational reform. We also present several robustness checks along this dimension.

The estimation of the average treatment effect ρ is typically a nonparametric regression problem. However, Imbens and Lemieux (2008) show that bias for the simple kernel estimator when one non-parametrically estimates a regression function in the interior of the support is relatively high and is related to the bandwidth h . One practical solution to this issue is a local linear regression (Fan and Gijbels, 1996; Imbens and Lemieux, 2008). In a fuzzy RD design, using a local linear regression with a uniform kernel is comparable, under certain conditions, to obtaining the treatment effect via a two-stage least squares (2SLS) approach.⁶ This equivalency also holds when we include additional covariates as discussed in the subsequent analysis (Hahn et al. 2001; Imbens and Lemieux, 2008; Calonico, et al., 2018).

While the theoretical treatment above describes the case of fuzzy regression discontinuity for an indicator treatment variable for simplicity, it also holds in a continuous setting. The existing literature on the evaluation of compulsory schooling changes have widely used a measure of treatment intensity, namely years of education as their measure of treatment assignment (Albouy and Lequien, 2009; Van Kippersluis et al., 2011; Ali and Gurmu, 2016). We follow this approach as it has the advantage of being directly comparable with pre-existing research and because using variable intensity allows us to more easily interpret the marginal effect of schooling on labor market outcomes.

In our 2SLS approach, we use an indicator for the introduction of the 10 year compulsory schooling reform, $Basic_{Reform}_i = 1(x_i > x_0)$, and the first and second order polynomial interactions of the reform with birth cohorts as a set of instruments. The first stage equation is given,

$$T_i = \alpha + \beta_1 Basic_{Reform}_i + \beta_2 x_i + \beta_3 Basic_{Reform}_i * x_i + \beta_4 x_i^2 + \beta_5 Basic_{Reform}_i * x_i^2 + Z_{it} \Gamma + \Omega_i + \varepsilon_{it} \quad (6)$$

and, similarly, the reduced form equation is

$$Y_i = \alpha + \pi_1 Basic_{Reform}_i + \pi_2 x_i + \pi_3 Basic_{Reform}_i * x_i \quad (7)$$

⁶ Formally, the same bandwidth must be employed for the estimation of the conditional expectation of the outcome variable and for the conditional expectation of the treatment variable on both sides of the threshold, which is the case in our analysis.

$$+ \pi_4 x_i^2 + \pi_5 BasicReform_i * x_i^2 + \mathbf{Z}_{it}\mathbf{\Gamma} + \mathbf{\Omega}_i + \omega_{it}$$

where i indexes individuals.⁷ Y_i is our labor market outcomes of interest, which we vary by specification - labor force participation, employment status, an indicator for self-employment, and hours worked per week.⁸ \mathbf{Z}_{it} represents a vector of time variant district and governate specific controls, which we include to help isolate the impact of any correlated investments in the educational system from the impact of the additional year of schooling. $\mathbf{\Omega}_i$ is a vector of district fixed effects.

Omitted factors are a concern when they are (a) heterogeneous across districts, (b) time-variant, and (c) potentially correlated with the implementation of the compulsory schooling component of the reform. Other educational investments occurring concurrently with the compulsory schooling extension are a first order candidate. As detailed in Section 3, we attempt to mitigate this issue with the inclusion of fixed effects and by controlling for the per capita stock of same gender or co-ed schools in the district, as well as governate level student-teacher and class unit-teacher ratios.

4.2 Bandwidth selection

The selection of bandwidth in regression continuity designs presents a tradeoff between bias and variance. A wider bandwidth may have a larger bias because more observations are far from the cutoff, but it will have lower variance because it can employ a larger number of observations. We take two approaches for selecting an appropriate bandwidth. First, we use a cross-validation procedure based on mean-square error to let the data suggest a bandwidth for each outcome variable (Calonico, 2016). Across the outcomes, this technique produces bandwidths which range from 4 to 7 years.

As a second approach, we consider narrower ranges of birth cohorts on a theoretically basis. One issue with allowing for a comparison across more years is that individuals become less and less comparable with time. Factors such as cohort effects and macroeconomic conditions at the time of entry in the labor market have more scope to vary. Perhaps most importantly, other impacts of the Education Act No. 27 in 1988 beyond the

⁷ In this framework, the estimated treatment effect is given by $\rho = \frac{\pi_1}{\beta_1}$.

⁸ All outcomes observed at the time of survey enumeration: 2004 for the Census; various years for the EUS. Further details can be found in the table notes and data appendix regarding treatment of the EUS waves.

compulsory schooling law itself would, over longer time horizons, have had time to have an impact. This could include the reformed structure and content of secondary school as well as any additional educational investments over the period such as school building, teacher training or hiring.

Several things are worth noting in this regard. First, anecdotally, the most commonly documented impact of the reform was the extension of compulsory schooling and the curricula/structural reforms in secondary school. Second, there were no other major educational reforms in the years immediately preceding or following the 1988/1989 reforms. The closest reform occurred in 1994, a year during which a new unified educational philosophy was issued. Using our preferred bandwidth, 4 years, excludes individuals exposed to this reform, and even using a longer bandwidth of 7 years would still only include individuals exposed to the new reform in the final 2 of their 10 years of education. In the analysis, we present results for 4 years as the bandwidth, and then run robustness checks using a range from 3 to 7 years. In all analysis, we employ the same bandwidth for the outcome and treatment regressions and the same bandwidth length on either side of the reform.

4.3 A graphical exploration

As a first pass, it is useful to examine how years of education vary over time in relation to the educational reform. Figure 2 plots years of education across Jordanian birth cohorts using the 2004 Census. A discontinuity in the average years of education is visible between those born in 1973 and 1974.⁹ There is a discernable increase in years of education completed by individuals born after 1973 who attended 10 years compulsory schooling (those affected by the 1988/1989 compulsory schooling reform) compared to individuals born in 1973 and earlier years. This increase is on the order of about one-fifth of a year of education for the entire cohort.

This jump in years of education is consistent with the evidence we have previously presented in two ways. First, the educational attainment of roughly 70% of the population would not be expected to change by the reform. This is because about 5% of the Jordanian population either never attended or completed any formal schooling (and were thus not

⁹ The dots in Figure 2 represent the conditional mean of years of education for each birth cohort group and the solid lines represent fitted quadratic regression lines with confidence intervals.

impacted), while about 10% completed no more than primary before and after the reform, and about 50% completed secondary degrees or higher (recall from the previous discussion that the total years of attainment were unaffected for this group as well). The only affected group should have been those stopping education at the preparatory level, a group comprising about 30% of the population. And within this population, only about half were impacted by the Basic reform.¹⁰

4.4 First-stage estimates

Table 2 presents the first-stage OLS estimates for the sample of male, female, and both. It indicates that the 1988/1989 compulsory schooling reform increased the average years of education of individuals by about two months regardless of whether the cohort trends are specified as linear or quadratic. Even though the coefficients on reform indicator are similar and significant in both models, but the quadratic model has the lowest value of AIC.¹¹

On the basis of AIC, we choose to present results from the quadratic model as the baseline model and simply run alternative specifications of the cohort trends as robustness check. For the separate samples of male and female, coefficients on reform also have similar magnitudes for both linear and quadratic models, and again, the quadratic regressions still produce lower AIC value than the linear models. Those coefficients indicate that the 1988 reform increased the average years of schooling of females and males by about two and half months as well as one month respectively.

5. Results

5.1 Schooling and Engagement with the Labor Market

Table 3 presents the results from the fuzzy regression discontinuity specification described in equations 6 and 7, using a 4 year window on either side of the 1988/1989

¹⁰ In the diagram and the analysis, we exclude individuals with less than primary educational attainment. We do this primarily because estimates of the return to an additional year of school for these individuals, many of whom are illiterate, are unlikely to be representative of that for the reform in question which expanded schooling only among higher attainment levels. We explore alternative subpopulations of educational attainment groups as robustness checks in the next section.

¹¹ The Akaike information criterion (AIC) is a test of model selection to balance the tradeoff between goodness of fit and simplicity of the model. It equals to twice of the difference between the number of estimated parameters and the value of log of maximum likelihood function at the optimal. The optimal model is the one with the lowest value of AIC.

reform (including only birth cohorts spanning 1970 to 1977), and employing a quadratic functional form with interactions as discussed in Section 4.1. Each row and column represents an individual regression, all of which contain district fixed effects. The first four columns provide estimates of labor market status in a sample of individuals from the 2004 Census, while the remaining columns use a pooled sample drawn from the closest EUS waves temporally, 2006 and 2007.¹² These regressions additionally control for a flexible set of polynomials in age to account for the fact that individuals are observed at different times since graduation in each wave.

Panel A presents estimates for both men and women together, while Panel B and C isolate the sample of men and women respectively. Columns (1) and (5) present estimates of the impact of an additional year of compulsory schooling on labor market participation. Looking first at the full sample, the coefficient of an additional year of education on labor force participation is estimated to be positive and significant at 0.032, suggesting that each additional year of schooling is associated with a 3.2% point increase in the likelihood an individual participates in the labor force.

Given a mean labor force participation rate of 64.1%, each additional year of schooling would then be associated with a roughly 5% increase in labor force participation. In practice, because compliance with the actual reform was limited, these estimates imply that in the first year of its implementation, the roughly 0.2 year increase in schooling would have increased labor force participation by about 0.6 percentage points (a 1% gain). The results for employment status in columns (2) and (6) generally mirror those for labor force participation as well.

As can be seen from Panels B and C, these impacts are driven predominantly by women entering the labor force and being more likely to be employed with the additional education. This is both unsurprising and surprising. It is unsurprising because nearly all males participate in the labor market, so there is not a large scope for additional education to increase this. Incredibly, in the age groups in our regression sample, over 95% of males engage in the labor market and between 85 and 90% work.¹³ At the same time, it is surprising to observe because social norms operate to constrain women's opportunities for economic participation in Jordan.

¹² We discuss the reasoning behind this approach in Section 5.2.

¹³ The estimate for male labor force participation is one location where the Census and EUS results diverge slightly, with the impact on LFP of males larger in the EUS and close to zero in Census. 2

Columns (3) and (7) present the impact of additional schooling on self-employment. An extra year of education is associated with a small increase in self-employment, largely driven by males. Similarly, columns (4) and (8) demonstrate an increase in hours worked resulting from an additional year of schooling, driven entirely by males. That we observe such a large impact on the intensive margin rather than the extensive margin for males makes intuitive sense given that nearly all males already participate in the labor force.

Contrasting the two samples produces additional insights. While the Census and the EUS survey estimates are relatively close for the overall labor force impacts and for females, the EUS yields generally larger statistics for the impact of education on male labor force participation, employment status, and hours worked respectively. One consideration is that the EUS provides a snapshot of individuals slightly later in life, and we provide hypotheses and explore this feature of the data in more detail in the following section.

5.2 Education and Labor Market Outcomes over the Life Cycle

The 2004 Census captures individuals on either side of the 1988/1989, meaning we observe individuals with an average 30 years of age, plus or minus the bandwidth we select. A novel feature of using the Employment and Unemployment Surveys to also examine this question is that we have snapshots of the same cohorts of individuals at later periods of their life. Patterns of labor market outcomes over the life cycle are very different for men and women over time in Jordan. These are depicted in Figures 3 and 4 respectively across cross-sections of the population observed in four of the EUS waves.

We focus first on Figure 3. By age 20, roughly 70% of males are in the labor force. Nearly all males are working by 23, and this rate plateaus for men until about age 35. At which point, it begins to decline slowly over the life-cycle. By age 40 it has fallen about 5% points and by age 50 it has declined to a level between 60 and 70%.

This pattern is remarkably different than that for women. As can be seen in Figure 4, women enter the labor force at roughly the same age as men, although only 30 to 40% of them enter at this point (in a promising trend, the highest levels of female labor force entrance are observed in the most recent EUS waves). This is where the similarities end. Female labor force participation peaks at 23, and unlike for men where it plateaus, it immediately begins declining at this point. By age 30, about a quarter of those who entered have left, and by age 40 nearly half have. By age 50, labor force participation rates approach

10%.

These patterns likely could reflect the influence of market factors including higher rates of unemployment for women and lower wages, although these differences are not especially large while the different life cycle patterns are striking. Indeed, the stark patterns are probably more consistent with a set of strong social pressures for women to marry, have children, and provide non-market labor within the household which raise the opportunity cost of remaining in the labor force as women age (which may also manifest in a perceived lack of acceptability of engaging in the labor market).

To examine this further, we utilize the EUS in yet another way in Table 4. Here we run our fuzzy RD design to examine the causal impact of an additional year of schooling from the 1988/1989 reforms in the first two available waves of the EUS 2006 and 2007 and contrast that with those from the 2014 and 2016 EUS waves. The first five columns reproduce those from Table 3, and we observe patterns similar to the Census where extensive margin labor force and employment gains are large for women from the schooling reform. Gains for men exist but are smaller, and we actually see large intensive margin gains for men in terms of hours worked. This is consistent with the patterns we observed in Figures 3 and 4. Men and women are both induced to engage more heavily with the market at this earlier stage of their life (around age 32), just along different margins.

When we turn to the last set of columns, the labor force participation gains for women are essentially estimated to be zero, while those for men are positive. The same is true for wages. This suggests that the additional schooling may have helped men remain in the labor market, but for women the increased schooling was unable to mitigate, even to the slightest degree, the massive decline in labor force participation over the life-cycle for women observed in Figure 4.

6. Discussion and Robustness

6.1 Functional Form of the RD Specification

In this section we explore the robustness of our empirical specification to alternative econometric modeling choices. Section 4.4 discussed the first stage results of the impact of the change to compulsory schooling on years of educational attainment. In our analysis, we used a parametric model with a quadratic polynomial functional form with treatment interaction terms -- on the basis that it produced lower AIC values than those for the linear

models. In Table 5, we replicate these baseline estimates in Panel D following this approach. Panels A, B, and C, present the results if we instead use a linear, linear interaction, or quadratic approach respectively. With the exception of small changes in the estimated impact on wages, the results are qualitatively and quantitatively similar regardless of specification, suggesting that the choice of functional form for the regression discontinuity is not a first order concern in this setting.

A second econometric concern is that a number of our labor market outcomes of interest are constructed as binary variables – including labor force participation, employment status, and our indicator for self-employment. A typically raised issue when conducting analysis on a binary dependent variable is that if this structure is not directly modeled, regression estimates are capable of generating predictions outside the range of 0 to 1. As a robustness check, Appendix Table 4 reproduces the results of Table 3 using an IV Probit specification. Reassuringly, in all cases, these results are remarkably similar to those produced using our baseline specification.

6.2 Alternative Definitions of the Treatment Population

Arguably, the creation of Basic education in 1988 should have had by far the largest impact within the population of individuals who were previously electing to stop school at the end of preparatory education (9 years). In theory, individuals who were stopping school earlier, such as at the end of the 6 year window (primary) or who had preprimary or lower educational attainment could have been induced to stay in school longer by the reform act.¹⁴ This would be the case if, for example, the reform been perceived to increase educational quality and individuals expected this to translate into better labor market outcomes.

In practice, this change seems unlikely in the case of 1988 Jordan for a number of reasons. First, slow implementation of the new Basic program suggests that many schools may not even have immediately known or been capable of enacting the reform. This suggests that most students and parents would likely have been even worse informed of the reform and its repercussions. Second, the share of the population leaving school before

¹⁴ It is also possible that the introduction of the 10th year of compulsory education impacted the decision to attend secondary school or to continue in the educational system beyond ten years into a master's or bachelor's degree in the university system. Although plausible, rigidities in the Jordanian educational system limit the scope for individual and household responses to this change because entrance into secondary school tracks are based on relative performance in the classroom, and acceptance into public universities are based on a competitive exam.

reaching 9 years of education exhibited no visible discontinuity. If individuals were induced to stay in school longer, we might expect to see a more rapid increase in years of educational attainment. It is still possible however that the addition of another year of school in the regular curriculum may have increased earlier dropout, as the time cost associated with obtaining the regular school age stopping degree had been increased.¹⁵

Appendix Table 3 presents results in which we include all individuals, even those who are illiterate or do not complete primary school. In all cases, coefficient magnitudes decrease slightly, yet remain significant and quite similar to those from our main specification in Table 3. This would be as expected since the addition of a larger “untreated” population into the analysis should introduce noise and attenuate the coefficients.

6.3 Robustness to Bandwidth Selection

As discussed in Section 4.2, the selection of bandwidth presents a tradeoff between bias and variance. We elected to present results for a 4 year bandwidth following intuition and both empirical selection procedures. Nevertheless, should unobserved educational investments not captured by our time variant measures of schools per capita and teachers per capita have occurred in the sample period, in longer bandwidth specifications, these could bias our coefficients in either direction depending on whether these investments were positively or negatively correlated with compliance with the new basic requirements.¹⁶ We present the primary results for bandwidths of 3, 4, 5, and 7 years in Appendix Table 2. Fortunately, the primary results are not incredibly sensitive to our choice of bandwidth.

7. Conclusion

This paper studies the causal impact of an educational reform in Jordan during the 1988 to 1989 school year which extended years of compulsory schooling from nine to ten. Our analysis suggests that additional schooling in the late 1980s induced more men and women to enter the labor force participation, raised their chances of finding employment,

¹⁵ While it is possible that both forces could have operated and offset one another, the empirical strategy in Section 4 should be relatively robust to this case. This is because the reform was implemented shortly after being passed, and thus there was not a large period of time in which households would have been able to forecast this change. This means that individuals in the sample only learned of the change to compulsory schooling in the final few years of their preparatory education, at which time many of the early dropouts would have already left school or decided not to enroll in the first place.

¹⁶ Furthermore, the estimates we produce should then be interpreted as the net impact of these other overall educational reforms enacted in the period, not of schooling alone.

and boosted wages. Gains were larger in the intensive margin for men, who realized increased in hours worked as a result. Interestingly, the initial gains in labor force participation for women dissipate rather rapidly over the life cycle, as there is no persistent impact on labor market engagement by age 40 for women. These patterns provide some evidence that educational improvements can still translate into stronger labor markets, but also highlight the strength of traditional gender norms in Jordanian society. Institutions and norms sometimes evolve slowly, and the broader impact of even temporary improvements in labor market participation may be larger than observed today, such as influences on future generations. To this extent, future research may explore factors which affect the extent to which these norms and institutional forces dictate the health of the Jordanian labor market and prospects for female employment.

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Figure 1

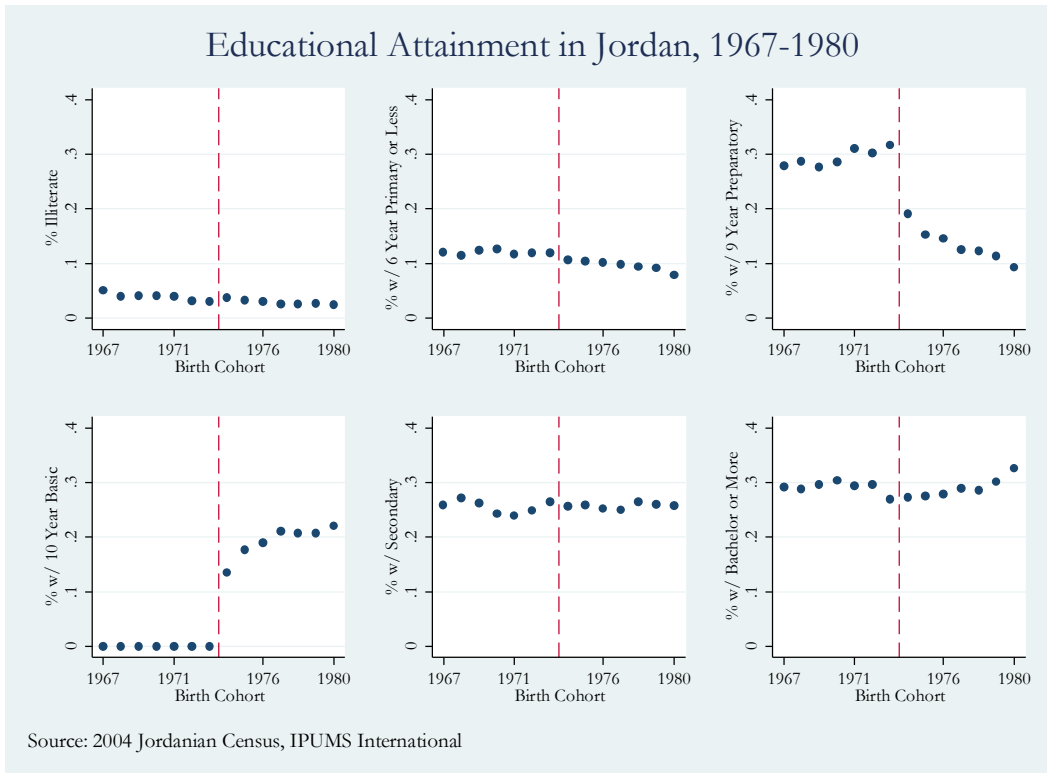


Figure 2

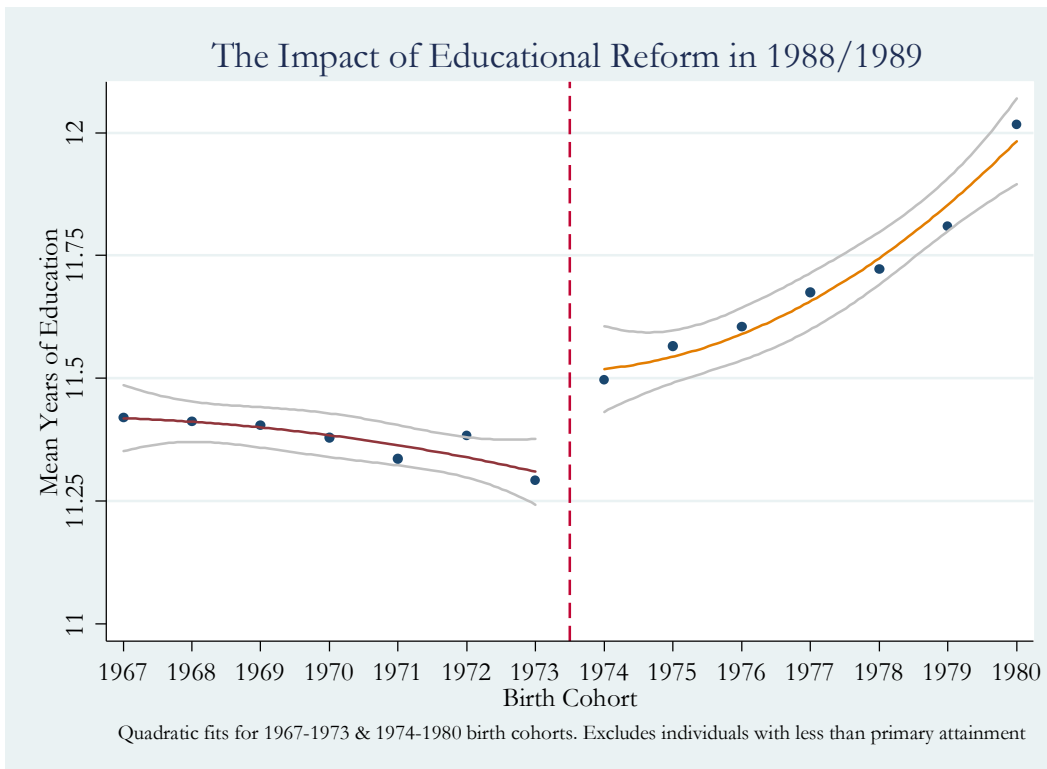
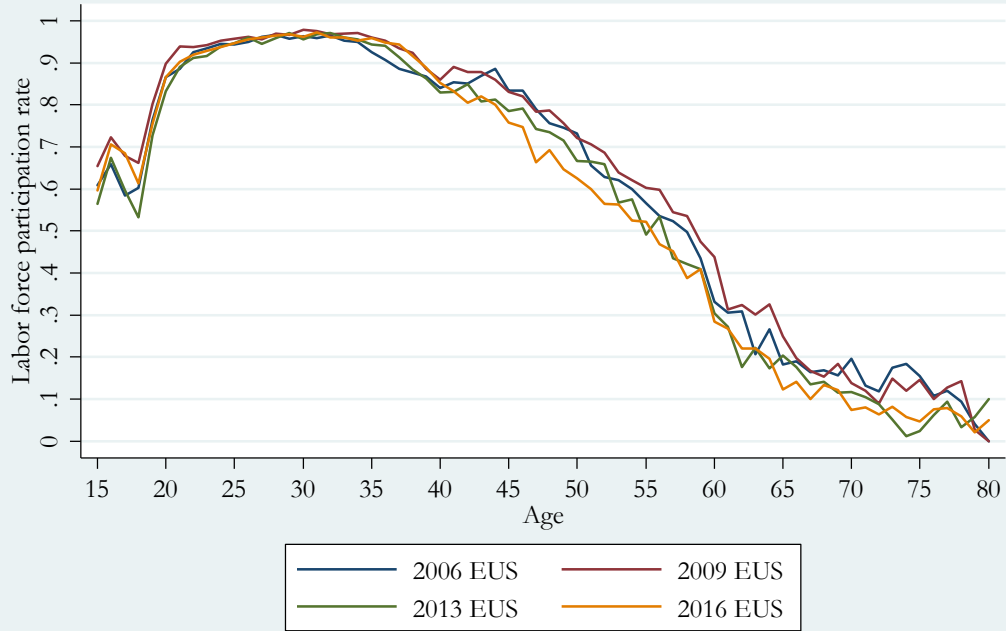
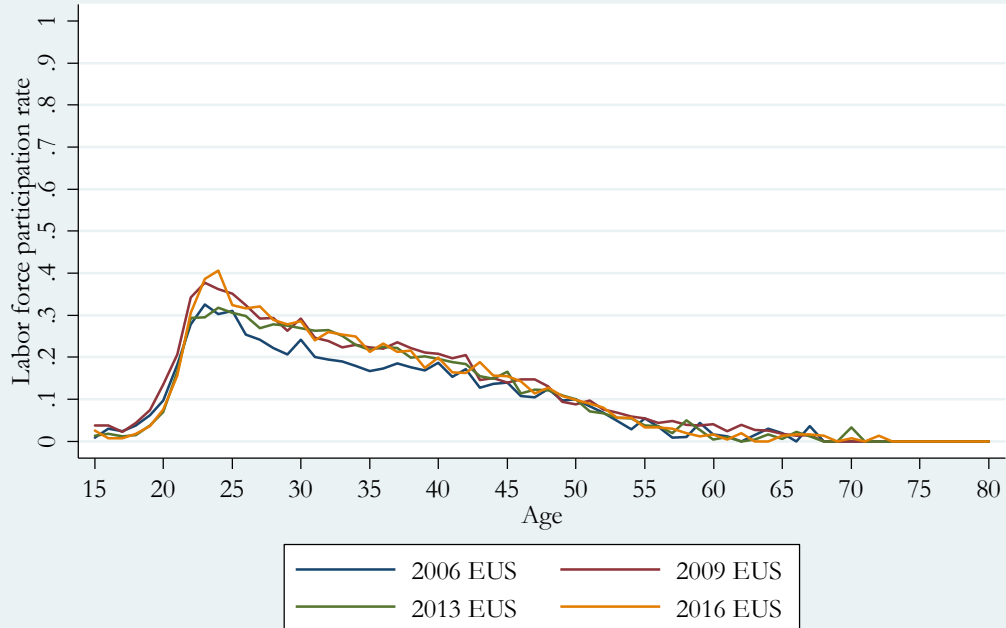


Figure 3: Male Labor Market Engagement over the Life Cycle



Source: Author's calculation.

Figure 4: Female Labor Market Engagement over the Life Cycle



Source: Author's calculations.

Table 1: Census Summary Statistics

Panel A: Individual Characteristics	Full Sample			Men			Women		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Age	101,728	30.06	4.00	51,441	30.03	4.01	50,287	30.08	3.99
Married	101,728	0.70	0.46	51,441	0.65	0.48	50,287	0.76	0.42
Number of children	101,728	1.87	1.99	51,441	1.43	1.74	50,287	2.33	2.12
Age at first birth of child	60,729	24.05	4.17	26,455	26.33	3.48	34,274	22.30	3.80
Labor force participation	101,728	0.62	0.49	51,441	0.93	0.26	50,287	0.31	0.46
Employed	101,728	0.49	0.50	51,441	0.80	0.40	50,287	0.17	0.37
Self employed ^a	63,155	0.09	0.29	47,750	0.12	0.32	15,405	0.01	0.12
Public sector employee ^a	63,155	0.33	0.47	47,750	0.35	0.48	15,405	0.30	0.46
Hours worked ^a	48,512	48.91	13.95	40,167	50.14	14.21	8,345	42.98	10.79
Panel B: Educational Characteristics									
Mean years of schooling ^b	94,460	11.56	2.98	47,719	11.40	3.04	46,741	11.72	3
Educational Attainment									
Primary (6 years)	101,728	0.08	0.27	51,441	0.09	0.29	50,287	0.07	0.26
Preparatory (9 years)	101,728	0.20	0.40	51,441	0.22	0.41	50,287	0.19	0.39
Basic (10 years)	101,728	0.10	0.30	51,441	0.12	0.32	50,287	0.09	0.29
Old Secondary (12 years)	101,728	0.11	0.32	51,441	0.12	0.32	50,287	0.11	0.32
New Secondary (12 years)	101,728	0.14	0.34	51,441	0.14	0.34	50,287	0.14	0.34
Bachelors or More	101,728	0.29	0.45	51,441	0.25	0.44	50,287	0.32	0.47
Panel C: Household Characteristics									
Family size	101,728	5.80	2.71	51,441	5.66	2.79	50,287	5.94	2.61
Own home	101,728	0.78	0.41	51,441	0.78	0.41	50,287	0.78	0.42
Polygamous household	101,728	0.01	0.10	51,441	0.01	0.09	50,287	0.01	0.12
Urban residence	101,728	0.78	0.41	51,441	0.78	0.41	50,287	0.78	0.42

Notes: Full sample of individuals born between 1967 and 1980. Sample excludes the illiterate, individuals with an indeterminate age, current students, and individuals who migrated into Jordan after school starting age (age 6). Mean years of schooling imputed using educational attainment, time to degree based on birth cohort, and length of degree program in years. Appendix A describes additional variable construction and cleaning. (a) Percentages calculated among those in the labor force. (b) Among those with at least primary attainment.

Source: Author's calculations using 2004 Jordanian Census (IPUMS).

**Table 2: First Stage - The Impact of the Compulsory
Schooling Reform on Years of Education**

	Linear			Quadratic		
	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample	Men	Women	Full Sample	Men	Women
Reform Indicator	0.146*** (0.051)	0.081 (0.074)	0.209*** (0.070)	0.151*** (0.051)	0.084 (0.074)	0.216*** (0.070)
Birth Cohort	0.027** (0.011)	0.025 (0.016)	0.031** (0.015)	-36.736*** (10.721)	-29.204* (15.422)	-47.031*** (14.853)
Birth Cohort Squared				0.009*** (0.003)	0.007* (0.004)	0.012*** (0.004)
Number of observations	54,056	27,285	26,771	54,056	27,285	26,771
AIC	268385	136916	131277	268374	136912	131267

Notes: Sample as described in Table 1. Appendix A describes variable construction.

Source: Author's calculations using 2004 Jordanian Census (IPUMS).

*** p<0.01, ** p<0.05, * p<0.1

**Table 3: The Effects of the 1988/1989 Jordanian Educational Reform
on Labor Market Outcomes**

	Census				Employment and Unemployment Survey				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full Sample	LFP	Employed	Self Employed	Hours Worked	LFP	Employed	Self Employed	Hours Worked	Wages
Years of Education	0.032*** (0.003)	0.032*** (0.003)	0.012*** (0.001)	0.641*** (0.107)	0.034*** (0.006)	0.029*** (0.006)	0.013*** (0.002)	3.689*** (0.314)	44.852*** (2.445)
Mean of Dep Var.	0.641	0.521	0.060	48.92	0.571	0.526	0.033	44.78	231.8
Number of Observations	54,056	54,056	54,056	27,622	44,777	44,777	44,777	23,534	23,437
Panel B: Males									
Years of Education	0.003* (0.001)	0.004 (0.003)	0.024*** (0.003)	1.562*** (0.137)	0.031*** (0.004)	0.036*** (0.006)	0.032*** (0.005)	6.755*** (0.532)	61.405*** (3.812)
Mean of Dep Var.	0.967	0.857	0.114	50.12	0.950	0.895	0.064	46.17	234.9
Number of Observations	27,285	27,285	27,285	22,959	21,746	21,746	21,746	19,473	19,412
Panel C: Females									
Years of Education	0.080*** (0.003)	0.077*** (0.003)	0.004*** (0.001)	-0.207 (0.358)	0.073*** (0.005)	0.061*** (0.005)	0.003*** (0.001)	-0.106 (0.571)	36.481*** (3.991)
Mean of Dep Var.	0.308	0.178	0.005	43.02	0.214	0.176	0.005	38.09	216.7
Number of Observations	26,771	26,771	26,771	4,663	23,031	23,031	23,031	4,061	4,025
Age and Age Squared	NA	NA	NA	NA	Yes	Yes	Yes	Yes	Yes
Birth Cohort Polynomials	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Fuzzy RD (Second stage IV regression) estimates including quadratic interaction terms. Sample as described in Table 1, restricted to those with at least 6 years of completed education and born between 1970 and 1977.

Source: Author's calculations using 2004 Jordanian Census (IPUMS) and the 2006 and 2007 EUS Surveys (ERF).

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Short vs. Long-Term Effects of Educational Attainment on Labor Market Outcomes in Jordan

	EUS 2006 and 2007					EUS 2014 and 2016				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Full Sample	LFP	Employed	Self Employed	Hours Worked	Wages	LFP	Employed	Self Employed	Hours Worked	Wages
Years of Education	0.039*** (0.006)	0.035*** (0.006)	0.014*** (0.002)	3.276*** (0.310)	41.567*** (2.443)	0.040*** (0.005)	0.039*** (0.005)	0.004 (0.003)	-2.842*** (0.314)	44.697*** (5.104)
Panel B: Males										
Years of Education	0.026*** (0.004)	0.032*** (0.006)	0.031*** (0.005)	5.857*** (0.505)	55.850*** (3.727)	0.114*** (0.012)	0.120*** (0.013)	0.007 (0.007)	-0.032 (0.295)	30.769*** (6.206)
Panel C: Females										
Years of Education	0.078*** (0.006)	0.068*** (0.005)	0.003*** (0.001)	0.646 (0.641)	45.172*** (5.190)	0.010** (0.004)	0.009** (0.004)	0.001 (0.001)	2.416*** (0.740)	8.745 (9.990)
Age and Age Squared	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Cohort Polynomials	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Fuzzy RD (Second stage IV regression) estimates including quadratic interaction terms. Sample as described in Table 1, restricted to those with at least 6 years of completed education and born between 1971 and 1976.

Source: Author's calculations using the 2006, 2007, 2014, and 2016 EUS Surveys (ERF).

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Sensitivity to Functional Forms of the Regression Discontinuity Design

	Census				Employment and Unemployment Survey					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
	LFP	Employed	Self Employed	Hours Worked	LFP	Employed	Self Employed	Hours Worked	Wages	
Panel A: Linear										
Years of Education	0.031*** (0.003)	0.032*** (0.003)	0.013*** (0.001)	0.648*** (0.107)	0.035*** (0.006)	0.029*** (0.006)	0.013*** (0.002)	3.720*** (0.315)	45.037*** (2.455)	
Panel B: Linear Interaction										
Years of Education	0.032*** (0.003)	0.032*** (0.003)	0.012*** (0.001)	0.641*** (0.107)	0.034*** (0.006)	0.029*** (0.006)	0.013*** (0.002)	3.689*** (0.314)	44.852*** (2.445)	
Panel C: Quadratic										
Years of Education	0.031*** (0.003)	0.032*** (0.003)	0.013*** (0.001)	0.648*** (0.107)	0.035*** (0.006)	0.029*** (0.006)	0.013*** (0.002)	3.720*** (0.315)	45.037*** (2.455)	
Panel C: Quadratic Interaction										
Years of Education	0.032*** (0.003)	0.032*** (0.003)	0.012*** (0.001)	0.641*** (0.107)	0.034*** (0.006)	0.029*** (0.006)	0.013*** (0.002)	3.689*** (0.314)	44.852*** (2.445)	
Age and Age Squared	NA	NA	NA	NA	Yes	Yes	Yes	Yes	Yes	
Birth Cohort Polynomials	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	

Notes: Fuzzy RD (Second stage IV regression) estimates including quadratic interaction terms. Sample as described in Table 1, restricted to those with at least 6 years of completed education and born between 1970 and 1977.

Source: Author's calculations using 2004 Jordanian Census (IPUMS) and the 2006 and 2007 EUS Surveys (ERF).

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 1: EUS Summary Statistics

Panel A: Individual Characteristics	Full Sample			Men			Women		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Age	314,701	36.30	5.14	152,468	36.33	5.17	162,233	36.27	5.11
Married	314,701	0.84	0.37	152,468	0.84	0.37	162,233	0.84	0.37
Number of children	314,701	2.94	2.03	152,468	2.79	1.93	162,233	3.09	2.11
Age at first birth of child	261,144	25.32	5.38	126,740	26.93	5.42	134,404	23.80	4.88
Labor force participation	314,701	0.54	0.50	152,468	0.90	0.30	162,233	0.20	0.40
Employed	314,701	0.50	0.50	152,468	0.84	0.37	162,233	0.17	0.38
Self employed ^a	169,108	0.08	0.27	136,668	0.09	0.29	32,440	0.02	0.14
Public sector employee ^a	169,108	0.44	0.50	136,668	0.41	0.49	32,440	0.56	0.50
Hours worked ^a	156,397	42.54	13.55	128,104	43.98	12.83	28,293	36.01	14.74
Wages	155,803	303.80	155.93	127,723	306.73	157.31	28,080	290.49	148.76
Panel B: Educational Characteristics									
Mean years of schooling ^b	293,352	11.47	2.90	144,813	11.35	2.96	148,539	11.59	2.84
Educational Attainment									
Primary (6 years)	314,701	0.12	0.32	152,468	0.12	0.33	162,233	0.11	0.31
Preparatory (9 years)	314,701	0.25	0.43	152,468	0.27	0.44	162,233	0.22	0.42
Basic (10 years)	314,701	0.17	0.38	152,468	0.18	0.39	162,233	0.16	0.37
Secondary (12 years)	314,701	0.14	0.35	152,468	0.15	0.36	162,233	0.14	0.35
Old Secondary (12 years)	314,701	0.07	0.26	152,468	0.08	0.27	162,233	0.06	0.24
New Secondary (12 years)	314,701	0.07	0.26	152,468	0.07	0.26	162,233	0.08	0.27
Bachelors or More	314,701	0.14	0.34	152,468	0.14	0.35	162,233	0.13	0.34
Panel C: Household Characteristics									
Family size	314,701	5.64	2.18	152,468	5.38	2.11	162,233	5.88	2.21
Polygamous household	314,701	0.00	0.03	152,468	0.00	0.05	162,233	0.00	0.00
Urban residence	314,701	0.72	0.45	152,468	0.72	0.45	162,233	0.71	0.45

Notes: Full sample of individuals born between 1967 and 1980. Sample excludes the illiterate, individuals with an indeterminate age, current students, and individuals who migrated into Jordan after school starting age (age 6). Mean years of schooling imputed using educational attainment, time to degree based on birth cohort, and length of degree program in years. Appendix A1 describes additional variable construction and cleaning. (a) Percentages calculated among those in the labor force. (b) Among those with at least primary attainment.

Appendix Table 2: Alternative Bandwidths

	Census				Employment and Unemployment Survey				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: 3 Years 1971-1976 Cohorts	LFP	Employed	Self Employed	Hours Worked	LFP	Employed	Self Employed	Hours Worked	Wages
Years of Education	0.030*** (0.003)	0.032*** (0.003)	0.014*** (0.002)	0.705*** (0.123)	0.039*** (0.006)	0.035*** (0.006)	0.014*** (0.002)	3.276*** (0.310)	41.567*** (2.443)
Panel B: 4 Years 1970-1977 Cohorts									
Years of Education	0.032*** (0.003)	0.032*** (0.003)	0.012*** (0.001)	0.641*** (0.107)	0.034*** (0.006)	0.029*** (0.006)	0.013*** (0.002)	3.689*** (0.314)	44.852*** (2.445)
Panel C: 5 Years 1969-1978 Cohorts									
Years of Education	0.033*** (0.002)	0.031*** (0.002)	0.011*** (0.001)	0.531*** (0.097)	0.035*** (0.005)	0.027*** (0.005)	0.010*** (0.002)	3.314*** (0.264)	41.904*** (2.055)
Panel D: 7 Years 1967-1980 Cohorts									
Years of Education	0.038*** (0.002)	0.026*** (0.002)	0.006*** (0.001)	0.506*** (0.084)	0.054*** (0.005)	0.037*** (0.005)	0.001 (0.002)	3.658*** (0.294)	53.595*** (2.571)
Age and Age Squared	NA	NA	NA	NA	Yes	Yes	Yes	Yes	Yes
Birth Cohort Polynomials	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Fuzzy RD (Second stage IV regression) estimates including quadratic interaction terms. Sample as described in Table 1, restricted to those with at least 6 years of completed education.

Source: Author's calculations using 2004 Jordanian Census (IPUMS) and the 2006, and 2007 EUS Surveys (ERF).

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 3: Full Population Results

	Census				Employment and Unemployment Survey				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full Cohorts	LFP	Employed	Self Employed	Hours Worked	LFP	Employed	Self Employed	Hours Worked	Wages
Years of Education	0.025*** (0.002)	0.029*** (0.002)	0.010*** (0.001)	0.541*** (0.092)	0.028*** (0.003)	0.024*** (0.003)	0.010*** (0.001)	2.931*** (0.269)	38.167*** (2.205)
Age and Age Squared	NA	NA	NA	NA	Yes	Yes	Yes	Yes	Yes
Birth Cohort Polynomials	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Fuzzy RD (Second stage IV regression) estimates including quadratic interaction terms. Sample as described in Table 1, restricted to those born between 1970 and 1977.

Source: Author's calculations using 2004 Jordanian Census (IPUMS) and the 2006 and 2007 EUS Surveys (ERF).

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 4: IV Probit Estimates

	Census			Employment and Unemployment Survey		
	(1)	(2)	(3)	(5)	(6)	(7)
Panel A: Full Cohorts	LFP	Employed	Self Employed	LFP	Employed	Self Employed
Years of Education	0.025*** (0.002)	0.029*** (0.002)	0.013*** (0.001)	0.029*** (0.003)	0.025*** (0.003)	0.019*** (0.003)
Age and Age Squared	NA	NA	NA	Yes	Yes	Yes
Birth Cohort Polynomials	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Fuzzy RD (Second stage IV regression) estimates including quadratic interaction terms. Sample as described in Table 1, restricted to those born between 1970 and 1977. Numbers are marginal effects.

Source: Author's calculations using 2004 Jordanian Census (IPUMS) and the 2006 and 2007 EUS Surveys (ERF).

*** p<0.01, ** p<0.05, * p<0.1

Appendix A

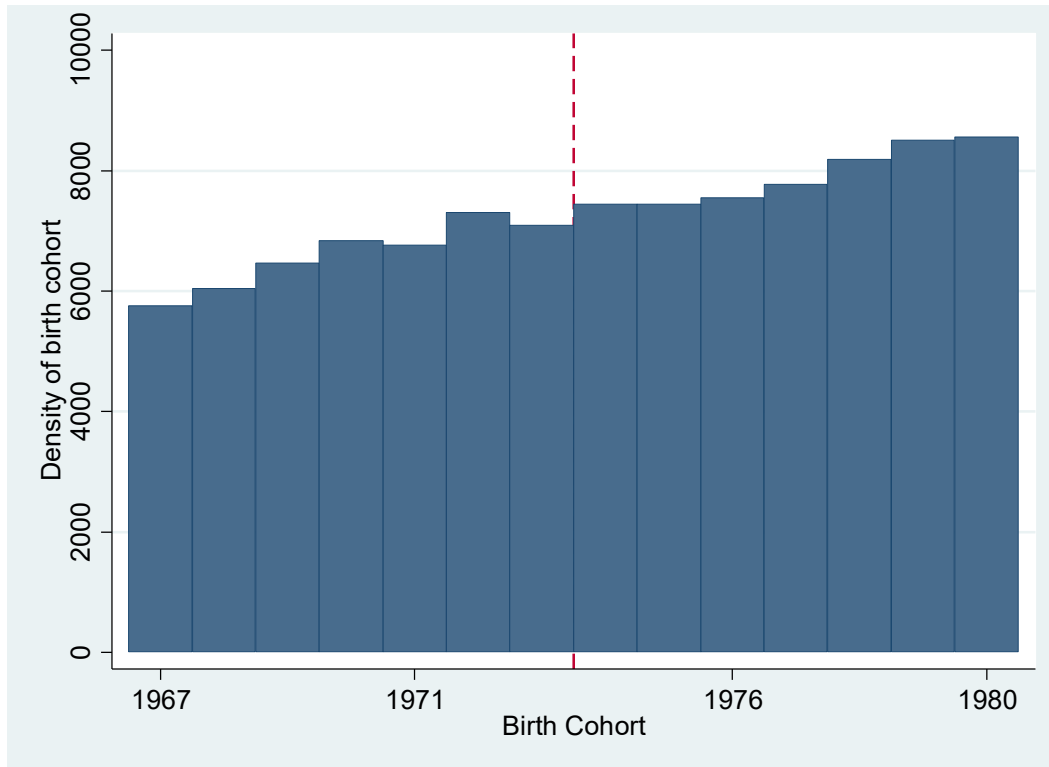
A1: 2004 Jordanian Census Data (IPUMS)

In order to construct an analysis sample that would be reflective of the population of Jordanians who were exposed to the schooling reform we make a number of sample restrictions. We exclude anyone in the sample who does not know or does not report their age (0.08%), as we are unable to assign these individuals to a particular birth cohort. We exclude anyone who reports being a current student (32%). We also exclude those who may not have been schooled in Jordan – including any immigrants with missing values for date of immigration (0.07%) as well as those who arrived after age 6 (5%).

While these populations are arguably of interest and potentially informative – we err on the side of trying to identify the impact of an additional year of schooling generated by the 1988/1989 reform. To the extent that the very large influx of refugees into the country are less likely to have been enrolled and to have received the new Basic education, this population doesn't provide useful causal inference on the returns to schooling. Nevertheless, their absence implies that the effects we observe do not generalize the effects to the complete population of Jordan, as additional resources may have been necessary to more fully expand education and there may be additional general equilibrium effects from more complete compliance and from devoting resources to ensuring broader swathes of the population receive 10 years of education.

Because our regression discontinuity uses birth cohorts as a running variable, one concern is that time variant factors could have influenced cohorts in ways that would bias the results. In Appendix Figure 1, we plot the number of observations in each birth cohort. There is no unusual discontinuity in the distribution of individuals in any cohort around the treatment threshold. This suggests that the composition of households (a) were not able to influence fertility in expectation of educational reform 15 years in the future and (b) population inflows through forces like migration did randomly generate demographic bulges among these birth cohorts large enough to significantly contaminate our analysis.

Appendix Figure 1: Searching for Evidence of Manipulation or Population Irregularities along the Running Variable (Birth Cohort)



A2: Population Data Construction

In an ideal analysis, measures of school and teacher availability at the district level would be matched to population data, in order to construct disaggregated per capita estimates of the evolution of these investments temporally and spatially around Jordan. Unfortunately, consistent population series at this level of aggregation are non-existent for Jordan. This owes both to dearth of estimates in the early period of the sample, as well as to a number of changes in geographic coding over time. In order to overcome this issue, we construct our own, newly consistent estimates of population disaggregated at the district level using estimates for towns, districts, and governates from the 1979, 1994, and 2004 censuses, as well as from a handful of statistical yearbooks.

A3: School Data

using data on the date of construction for the corpus of schools in Jordan from Ministry of Education’s 2010 Census of Schools in Jordan, we retrospectively construct the number of schools present in each district in each year. This is a strategy pioneered by Assaad and Saleh (2016), who note that validity of this approach relies on the assumption that not many schools have closed, or that they have not done so in a disproportionate manner across regions of the country. Given the relative youth of the state, and most schools in general (stats here), this seems plausible. We further cross-validate these backward estimates with contemporaneous estimates of the stock of schools at the aggregate level from annual statistical yearbooks (correlation here). Appendix A discusses this data and these checks in more detail.

A4: EUS Data

One nuance of the Employment and Unemployment Survey data is that this data source reflects repeated cross sections. In this regard, it is possible that by pooling the waves we may occasionally observe the same individual twice. We are unable to identify or exclude any repeated observations in the sample. Nevertheless, we frequently only pool a subset of the EUS samples, making the likelihood of an individual appearing repeatedly quite low. In addition, to the extent that this may have occurred, it should be random in the classical sense of measurement error.

A5: Data Construction Assumptions

A concern in working with household survey data, particularly surveys collected several decades ago, are data limitations and statistical choices these impose in the data construction process. We have endeavored to make conservative choices where possible to limit the extent to which these decisions may have impacted the results we produce.

One of the most important of these decisions concerns a lack of specific years of education attained in the Census data. In order to construct years of education for our Census sample we use the available measures of degree attained and apply to these outcomes the years of education required to obtain this degree. This choice could be problematic if individuals incorrectly remember their specific degree along the preparatory/basic margin, particularly. Indeed, a tiny fraction of individuals report obtaining a basic degree in 15 years (which could also reflect grade skipping or misreporting). More commonly, not all

individuals report obtaining a basic degree after the reform was implemented. It is important for interpretation of our findings to understand to what extent this is the result of imperfect compliance and to what extent forgetfulness could play a factor.