

Women's Schooling and Fertility under Low Female Labor Force Participation: Evidence from Mobility Restrictions in Israel*

Victor Lavy
UNIVERSITY OF WARWICK, HEBREW UNIVERSITY,
AND NBER

Alexander Zablotsky
HEBREW UNIVERSITY

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Abstract

This paper studies the effect of mothers' education on fertility in a mostly Muslim population with very low female labor force participation. We first show that a removal of travel restrictions on Israeli Arabs had raised female education and had almost no effect on male education. Next, we show that it lowered fertility rates among exposed women, which we interpret as an effect of female education on fertility. We rule out labor-force participation, age at marriage, marriage and divorce rates and spousal labor-force participation and earnings as confounding factors or as mechanisms but find that spousal education and children quality play a role in the fertility decline. We provide a variety of robustness tests that rule out other channels by which the removal of the travel restrictions could have affected fertility directly. These results are particularly interesting and important for the context of many Muslim countries with low rates of female labor force participation.

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1. Introduction

In the classic economic model of fertility (Becker, 1960; Mincer, 1963), education increases the opportunity cost of women's time, prompting them to have fewer children but also raising their permanent income through earnings and tilting their optimal fertility choices toward higher children's quality (Becker and Lewis, 1973; Willis, 1973). In these models, the link between education and fertility crucially depends on labor force participation.

This paper studies the role of female education in reducing fertility through mechanisms other than the labor market and its implied female value of time. In the past half-century, for example, the total fertility rate of Muslim women in Israel fell sharply, from over 9.8 children in the mid-1950s to 3.9 in 2008.¹ Concurrently, Israeli-Arab women's average years of schooling increased more than threefold, from three years in 1951 to over ten in 2008. This change, however, hardly affected their labor-force participation and employment behavior; the respective rates were only 15 percent in 2000 and 18 percent in 2009.² Whether education plays a role in lowering fertility rates in the absence of the labor market mechanism is of great importance since in most of the Arab and Muslim world, it is common for women to be absent from the labor force.³ However, female education has increased to various degrees in Arab and Muslim countries and this change could have lowered fertility rates via other channels.⁴

An extensive literature documents associations between education and fertility (Strauss and Thomas, 1995). However, whether they represent causal relationships has been the subject of debate. Breirova and Duflo (2002) and Osili and Long (2008) use school expansion as a source of exogenous decrease in the cost of schooling and find a negative causal effect of education on early age fertility in Indonesia and Nigeria. Black, Devereux, and Salvanes (2008) find that gains in education resulting from compulsory-schooling laws decreased teenage pregnancy in the U.S. and

¹ Israel Central Bureau of Statistics (hereinafter: CBS) website, online tables and figures.

² CBS (2002), State of Israel Prime Minister's Office, and Yashiv and Kasir (2009).

³ The most recent World Bank statistics show that in 2009 the labor force participation rate of women over 15 years old was 20-24 percent in Egypt, Jordan, Lebanon, and Yemen, and it was 14-17 percent in Iraq, Saudi Arabia, and the West Bank and Gaza. In Pakistan and Turkey, Muslim though not Arab countries, female labor force participation is also very low, 23-24 percent (<http://data.worldbank.org/indicator/SL.TLF.CACT.FE.ZS>).

⁴ The increase in education may impact women's fertility by improving an individual's knowledge of, and ability to process information regarding fertility options and healthy pregnancy behaviors (Grossman, 1972). Second, education may enhance females' ability to process information and contraceptive options (Strauss and Thomas, 1995). Education may also improve a wife's bargaining power inside her marriage (Thomas, 1990) and may also tilt the tradeoff from the number of children to their quality (Moav (2005). McCrary and Royer (2011) present an insightful summary of how education may affect fertility and children's outcomes and discuss the related empirical evidence. However, there is little evidence regarding the importance of these channels in the absence of meaningful increases in women's employment and the opportunity cost of their time.

Norway. Also in Norway, Monstad, Propper and Salvanes (2008) find that increases in education did not lead to decreased fertility rates, yet did lead women to give birth at older ages. In contrast, McCrary and Royer (2011), using exact cutoff dates for school entry, find that education does not affect fertility. Kirdar, Tayfur, and Koç (2009) use the extension of compulsory schooling in Turkey in 1997 and find that it increased the average age of marriage and reduced fertility at young ages. Duflo, Kremer, and Dupas (2010) provide experimental evidence that access to education for adolescent girls reduced early fertility among girls who were likely to drop out of school. This mixed evidence obviously suggests a lack of consensus regarding the causal effect of women's education on fertility. Furthermore, maternal education can affect fertility through various channels, and as such it is not evident that there should be one universal effect of maternal education on fertility. Therefore, it is important to separately identify the different channels through which the effect works, and in particular those channels which do not operate through the labor market; thus, the main contribution in the evidence we present is in studying a case in which the level of education had increased without changes in the labor market taking place. This evidence is not only important in abstracting from the labor market effects; it is also highly relevant for understanding the fertility transition in the Muslim and the Arab world, where women's education had increased significantly, yet their labor force participation remained low.

We base the evidence presented in this paper on the de facto revocation in October 1963 of military rule over Arabs in Israel, which immediately allowed some of the Arab population to regain access to schooling institutions. Military rule was in effect from 1948 to 1966 in several geographical areas of Israel that had large Arab populations. Since 1948, the Arab residents of these areas were subject to measures that placed tight controls on all aspects of their lives, including restrictions on mobility and the requirement of a permit from the Military Governor to travel outside of a person's registered domicile.⁵ The travel restrictions were revoked in October 1963 following unexpected political and government change.⁶

⁵ A recent historical episode of similar restrictions on perceived "enemy" populations is the United States Government's internment and forced relocation of Japanese Americans and Japanese residing along the Pacific coast of the United States to War Relocation Camps in the wake of Japan's attack on Pearl Harbor. President Franklin Roosevelt authorized the internment by Executive Order on February 19, 1942. On January 2, 1945, the exclusion order was totally rescinded. Another example is the arrest in camps of Germans in England during World War II.

⁶ In June 1963 the Israeli Prime Minister, David Ben-Gurion, who together with his ruling Labor Party strongly supported the continuation of the Military Government, resigned unexpectedly. The change was also a response to the mounting pressure from the Israeli public and many political parties, including the right-wing party Herut, to annul military rule over Israeli Arabs. This effort led in 1966 to the complete revocation of military rule and the equalization of Arab citizens' rights with those of other citizens.

This historical episode sharply increased the education of affected cohorts of children. The Military Government restricted de facto access to schools for children in localities and villages that had no primary or secondary schools while not affecting access in localities in the relevant regions that already had such institutions. By so doing, it created two zones in the Arab-populated areas, one in which school attendance required travel that had become difficult if not impossible and one in which schooling access was not disrupted at all. In the latter group, we distinguish between Arab localities that were under military rule and the Arab population that lived in predominantly Jewish cities. The latter population group was also placed under military rule at first (1948) but was exempted de facto from some of the restrictions a short time later.

The change which took place in late 1963 reduced the cost of primary or secondary schooling for children in localities that lacked schools. Therefore, the exposure of an individual to this “treatment” was determined both by location and by her year of birth. After controlling for locality and year of birth fixed effects, we use the interaction between a dummy variable indicating the age of the individual in 1964 and whether or not the locality was part of the Military Government zone and had no schools as an exogenous variable and as an instrument for an individual’s education. This is a similar identification strategy as that used to estimate the effect of school quality on returns to education (Card and Krueger, 1992), the effect of college education on earnings (Card and Lemieux, 1998) and the effect of school construction on education and earnings (Duflo, 2001). We allowed the affected cohorts to include children aged 4–13 in 1964, using older cohorts to perform placebo tests.

We used data from the 1983 and 1995 Israeli censuses which include information on labor force participation, education, fertility, and locality of residence of all family members. In the 1983 census, the affected cohorts were just over 23–33 years old, making it possible to study the effect of education on early-age fertility. In 1995, the affected cohorts were already aged 36–46, allowing estimation of the effect of education on completed fertility.

We first show evidence that the removal of travel restrictions imposed on part of the Arab population of Israel during the 1950’s and early 1960’s, raised female education sharply with almost no effect on men education. We then present evidence that this sharp increase in Arab women’s education accounts for part of the fertility decline in the affected cohorts. We also present evidence that women’s labor-force participation, as well as other potential mechanisms such as age at marriage, marriage rates, and divorce rates, did not play any role in this fertility decline. The estimated impact of women’s education on fertility remains very large even after we account for spouse’s employment. Furthermore, spouse’s education increased immensely through assortative matching and, therefore, probably played a major role in the decline in demand for children. Other

mechanisms that seem to be relevant for the role of education in reducing fertility of Arab women in Israel are changes in fertility preferences, knowledge and use of contraceptives, higher bargaining power within the household and role of women in family decisions, reduced religiosity, and positive attitude towards modern health care and modernism in general.

The evidence we present below suggests that the decline in the cost of attending primary and secondary schooling from 1964 onward increased females' years of schooling by 1.02 for women who were aged 4–8 in 1964, and by 0.58 for women aged 9–14 at that time. These educational gains are associated with a large increase in the probability of a woman's completing primary school and also of the completion of at least some years of secondary school. Much smaller effects are estimated for men, suggesting that the travel restrictions did not limit boys' access to schooling as badly.

These very large effects on girls' schooling levels are associated with a sharp decline in completed fertility, measured at 0.61 children in the younger affected cohorts and 0.47 children in the older cohorts. Under the assumption that the historical episode provides a valid instrumental variable for women's schooling, the implied 2SLS estimates show that a one-year increase in maternal schooling caused a 0.6-child decline in fertility. This evidence suggests that the increase in mothers' schooling had a large and negative effect on fertility even though the actual opportunity cost of their time did not change much. Using data from a fertility survey conducted in 1974/75 among a representative sample of some 3,000 currently married Arab women under age 55 in Israel, we also find that maternal education was highly correlated with other potential mechanisms, in particular a change in fertility preferences, changes in contraceptive details, a shift in preferences towards quality children and reduced child and infant mortality, higher bargaining power of women as reflected in their larger role in family decisions, decline in religiosity, and positive attitude towards modernism.

The identification assumption in estimating the causal effect of mother's schooling on fertility is that the removal of the travel restrictions had neither a direct nor an indirect effect on fertility except for its effect on creating access to schooling. However, we cannot rule out completely the possibility that the lifting of mobility restrictions could have affected fertility not only through women's education. For example, it could have affected the childhood environment of girls in mobility-restricted areas in ways that could have direct effects on girls' subsequent fertility as adults, independent of their schooling attainment. Examples of potential threats to the exclusion restriction assumption could be that families of affected girls experienced relatively larger income gains, or that information about family planning and contraceptives reached more easily the affected areas. We examine a large number of potential threats to identification and

discuss and test relevant counterfactuals. We do this using a wide range of evidence, including placebo tests, similarity in control and treatment pre-reform time trends, and extensive variety of specifications and validity tests which we will outline later in detail. This evidence suggests that the removal of the travel restrictions did not have differential impacts on cohorts aside from their effect on education nor was it correlated with pre-reform trends in demographic and socioeconomic characteristics in treated and non-treated localities.

The rest of the paper is organized as follows: Section 2 describes the political and policy context of the Military Government and the mechanisms that it could have used to affect education. After describing the data in Section 3, Section 4 presents our identification strategy, the reduced form effect of the historical episode on women's and men's schooling, and then the estimation results of the effect of schooling on fertility. In Section 5, we check the robustness of the results and discuss possible threats to our identification strategy. In Section 6, we discuss and present evidence on a variety of mechanisms, and Section 7 concludes.

2. The 1948–1966 Military Government and Restricted Mobility of Arabs in Israel⁷

On May 14 1948, the day that Britain had announced it would end its Mandate in Palestine, the Jewish community in Palestine published a Declaration of Independence which announced the creation of the State of Israel. The declaration was based on the United Nations Partition Plan for Palestine adopted as a resolution on 29 November 1947 by the General Assembly of the United Nations. The declaration did not define the borders of the new state. On the following day, 15 May, most of the remaining British troops departed and five Arab countries' armies crossed the borders of what had formerly been Mandate Palestine. This event marked the beginning of the 1948 Arab–Israeli War. The Palestinian Arabs, against which the Jewish population fought its war of independence, became subjected to the new Jewish state at the end of the war. During the war, the Jewish Provisional Council of State decided to impose a special military governmental authority on areas populated by Palestinian Arabs. The Military Government was extended after the war and disbanded only in 1966. It was legally based on defense regulations enacted in 1945 by the British Mandate Government that ruled Palestine at the time. From then until the cessation of the enforcement of these regulations, the Military Government was the dominant Israeli governmental authority exercising control over the Israeli Arab minority. At first, the Military Government worked together with the Ministry of Minorities, which was responsible for humanitarian aspects of the treatment of the Arab population, but this ministry was abolished in 1949. Thereafter, the Military Government held sole responsibility for all affairs of the Arab population. Although all

⁷ Much of the material in this section is based on Bauml (2002), Abu-Saad (2006) and Al-Haj (1995).

Arab citizens were subject to military rule, those who lived in mixed Arab-Jewish cities such as Haifa and Jaffa enjoyed greater freedom than others from the early 1950s on, largely because the travel restrictions were harder to enforce in predominantly Jewish cities.

A separate school system was developed for the Arab population in Israel, even in towns that had mixed Jewish and Arab populations. The conditions of the school facilities in Arab schools were extremely bad, and classrooms were over-crowded, even though in some places students were taught in two shifts (Abu-Saad, 2006, Kopelevitch, 1973). Essential supplies were lacking, such as desks and chairs, blackboards and textbooks. The Free Compulsory Education Law that was passed in 1949 without exclusion of any ethnic group was not applied practically to the Arab population until the mid-1960's (Abu-Saad (2006) and Al-Haj (1995)). However, the most important element of this regime for the purposes of our study was the special travel permits, issued on a daily or weekly basis, which the Military Government required Arab citizens to obtain in order to leave their villages and towns by day or night. Such permits were needed for receiving medical services in the cities, for travel to port cities for importation of capital goods (such as tractors), access to work or educational opportunities, and practically every other purpose which required travelling outside the locality. It has been claimed that obtaining these permits often involved side payments to Arab collaborators. The Arab-populated "enclosed areas" were divided into three separate army commands: north (Galilee), south (Negev), and center (the "Triangle"). Each area was isolated from the other and most Arab citizens were, of course, isolated from the majority Jewish population as well. Enclosure orders controlled mobility by the required permits.

Apart from the practical hardships, the travel restrictions took a toll on their subjects by creating a sense of uncertainty and personal risk. The army set up checkpoints and inspected Arabs regularly for their passes. Those found with an expired pass or no pass at all were fined or imprisoned. The Military Government also imposed a regular curfew from dark to sunrise or, at times, before dawn. The public was not always aware of changes in curfew, resulting in several tragic events. In one notorious case, on October 29, 1956, on the eve of the Suez War, the Government changed the curfew to an earlier hour. Border Guard forces entered the large village of Kafr Qasem and imposed this curfew on the village while many of its residents were out working their fields some distance away, unaware of the revised curfew; some children were still in school. By the end of the Border Guard operation, 51 villagers had been killed, including women and young boys and girls, seven aged 8–13, along with others who were wounded (Hadawi, 1991). This event and lesser tragedies created a climate of fear and insecurity, especially when travel outside the village or town was needed.

There are plenty of stories and anecdotal evidence from personal diaries about the effect of the increase in the cost of school attendance on school enrollment during the tenure of the Military Government. El-Asmar (1975) recounts an experience typical of many youngsters at this time. He gives the example of Fouzi, a young child during the period the military regime, whose home town had no complete eight-grade primary school, "[Families that] wanted their sons to continue their schooling had to send them to Nazareth or to the Triangle area. My father had to send me and my big brother away to a residential school in Nazareth, which cost him a fortune."

To avoid the dangerous and costly daily trip, some boys were sent to residential schools at a much higher cost than attending the nearest school. Importantly, this solution was available for boys only, for example for both primary and secondary age children from Arab Christian families in Church owned boarding schools; girls had to drop out of school in such cases because there were no boarding schools for girls. Ziad Mahjena tells much the same story.⁸ He completed primary school in 1957/58 in his home town and aspired to continue in nearby schools in Nazareth or the nearby Jewish town of Hadera but could not due to the state of military rule and the dearth of family resources. He recounts the story of his three male friends who could afford to enroll in a residential high school. It is important to note however that the travel restrictions did not forbid completely any travel; they simply required permits that were not granted easily. However, children could walk, or ride a donkey or a horse to the nearest school and some did that, including girls, especially of older age. This possibility to attend schooling even when there was no school in the village or town can explain the positive trend we document below in years of schooling of affected cohorts of boys and girls.

During Israel's first years as an independent state, but mainly after 1957, some criticism and reservations were expressed among the Israeli public, the Knesset (parliament) and Mapai (the ruling party) about the need for the Military Government. The critics' main argument—that the Military Government damaged Israeli democracy—led to many initiatives to abolish it. In February 1962 and February 1963, four political parties (including Menachem Begin's right-wing Herut Party) presented parliamentary motions to revoke the entity's status. All the motions were voted down by a close margin. However, the resignation of Prime Minister David Ben-Gurion on June 16, 1963, and the appointment of Levi Eshkol as his successor led immediately to a dramatic and unexpected change. In a speech to the Knesset in October 1963, Eshkol announced that the Arab population would no longer need travel permits and that Arabs could once again move freely around

⁸ Retrieved from a memoir website: <http://www.Sochrot.org.index.php?id+164>.

the country.⁹ This change removed one of the most burdensome restrictions, one that had profoundly affected the daily lives of Arabs in Israel since the creation of the state. In 1966, the Military Government was abolished altogether; all that remained were several specific restrictions, such as traveling to the nuclear plant in Dimona, and to the vicinities of the Jordanian border in the Arava Valley and the Egyptian Sinai Peninsula.

The Military Government and Restricted Access to Schooling

Table A1 lists the Arab localities that were under military rule and travel restrictions as of 1948 and the number of primary and secondary schools in each locality in 1964/65, the first year for which such information was available (Central Bureau of Statistics, 1966). Five of the localities (Acre, Haifa, Lod, Ramla, and Tel Aviv-Jaffa) were mixed cities with a Jewish majority and an Arab minority. All five cities had Arab primary schools; three of them also had Arab secondary schools. As noted above, however, the Arab population of these cities was exempted from military rule and the travel restrictions from the mid-1950s on; we exclude them from our analysis. Five other localities—small villages—were also exempt from military rule because most of their populations were of other minorities (Druze and Circassians) which were not perceived to be a threat; the analysis excludes them, too. This leaves us with 49 Arab localities. Twenty-three of them had neither a primary school nor a secondary school by 1964/65; the other 26 had at least one primary school and eight had one or more secondary school. Thus, the treatment group includes all localities that were under military rule and had neither a primary nor a secondary school. The control group includes all localities under military rule that had at least one primary school.¹⁰

Column 4 of Table A1 lists the distance from each such locality to the nearest Arab locality that had a school. This distance ranges from 3 to 15 kilometers, and it is likely that the cost of attending a school rose commensurably with the distance to the nearest school. We will exploit this variation in the empirical work to assess whether the effect of lifting the travel restrictions in late 1963 is sensitive to the distance to the nearest school. The map in Figure A1 in the online appendix presents the location of each of the localities included in Table A1. It shows that the Arab population

⁹ The populations of five Arab villages adjacent to the border were excluded from the new free-mobility policy. Another restriction that prohibited all Arabs from entering certain areas intended for Jewish settlement and defined as military zones was not cancelled.

¹⁰ It is important to note that during the 1950's and 1960's very few new schools were constructed in the Arab sector and this situation did not change very much even after the removal of the military regime in the mid 1960's. This is evident from the Ministry of Education publication that lists all schools in the country alongside the school's opening date. The lack of opening of new schools in the Arab sector in the post travel ban period is partly due to the deep recession in the Israeli economy in 1966-1967 and the consequences of the 1967 war and the large increase in defense spending that followed. Only in the mid 1970's do we see more new schools in Arab localities.

in Israel in 1948 was concentrated in the center, with most localities along the border with Jordan and in the North region of Israel (Galilee). The map also shows the significant overlap in the spatial distribution of treatment and control localities.

Another important point to note here is that the control population experienced exactly the same travel and other restrictions due to military rule as did the treatment group. This implies, for example, that the populations in both types of localities experienced the same limitations in access to labor-market opportunities, social and healthcare services outside the locality, etc. In an attempt to eliminate further control-treatment differences in pre-program differences, we will also use two alternative comparison groups, both of which are much more similar to the treatment group in pre-program outcomes (education and fertility). The first group excludes the seven largest towns; the second, whom we use for a robustness check, comprises the Arab population of the mixed cities listed in Table A1. The importance of using this comparison group is that it had much better pre-1964 outcomes, i.e., higher average years of schooling and much lower fertility. We will show that the results based on these two additional control groups are very similar to those obtained from our benchmark comparison group.

3. The Data

Our main source of data is the 20% public-use micro-data samples from the 1983 and 1995 Israeli censuses of population and housing, linked with information about the localities and regions that were under military rule from 1948 to 1966. We also use information from government records about localities that had primary and secondary schools before 1963. The Israeli census micro files are 1-in-5 random samples that include information culled from a fairly detailed long-form questionnaire similar to the one used to create the PUMS files for U.S. censuses.¹¹ The micro data of the 1983 census are available in one version that includes all variables from the extended questionnaire and data from the short questionnaire that was administered to households selected in the sample. These data identify age, occupation, household income, marital status, and education, as well as residential and household details, and importantly for our purpose it identifies the locality in which the household dwells (or the restricted geographic area, for small villages). Both the 1983 and the 1995 census provide the current locality which could in principle be different from the locality of birth. However, these censuses also include a question of whether the current locality is also the place of birth and almost 75 percent of the sample replied positively to this question. We

¹¹ The census enumerates residents of dwellings in Israel proper and Jewish settlements in the occupied territories, including residents abroad for less than one year, recent immigrants, and non-citizen tourists and temporary residents living at the indicated address for more than a year.

will show below in section 5 that the main results we obtain from the full sample are identical to those we obtain from the sample that excludes individuals not living at census day at their place of birth. This insensitivity of the results is probably due to the fact that until the late 1960's, the Arab population in Israel was not allowed to relocate and that on average this population tend to remain in their village, town or city of birth. We will return to discuss this issue in the results' section of the paper.

Due to statistical confidentiality requirements, the data file available from the 1995 census, which includes detailed geographic codes down to code of locality, contains other variables that have been grouped. Thus, age is reported in five-year cohorts and years of schooling are reported in seven groups (0, 1–4, 5–8, 9–10, 11–12, 13–15, 16 and above). Education is also reported by the highest certificate earned: never studied, did not get any certificate, primary or intermediate school, secondary school, matriculation, post-secondary certificate (non-academic), bachelor's degree, and master's degree or above. The number of children born (reported only for mothers) is grouped as follows: 0, 1, 2, 3–4, 5–7, and 8 and above. For years of schooling and number of children in 1995, we used the midpoints in each range.¹²

Table 1 presents the 1983 and 1995 pooled sample mean demographic and economic outcomes for two cohorts, those aged 14–18 and 19–23 in 1964. As we explain below, these cohorts were unlikely to have been affected by the change in travel policy at the end of 1963. Comparison of the means of the control and treatment groups shows that the treated population had lower socioeconomic outcomes. For example, the mean years of schooling of the 14–18 age cohorts was 5.82 in the control group and 4.16 in the treated group. Mean fertility in the 14–18 age cohorts was 4.9 in the control group and 5.7 in the treatment group, a difference of 0.8 children. However, the gaps between treatment and control groups based on the 14–18 age cohorts strongly resemble the treatment–control differences based on the 19–23 age cohorts. For example, mean years of schooling of the 19–23 age cohorts was 4.27 in the control group and 2.78 in the treatment group; the difference, 1.49, is similar to the corresponding difference in the 14–18 age cohorts. Also, the treatment–control difference for fertility was 0.8 for the 14–18 age cohorts and 1.0 for the 19–23

¹² There exists another version of the 1995 census that does not include detailed locality code but provides all detailed ungrouped values of these demographic and education variables. However, since we needed the detailed locality code in order to assign individuals to treatment and control groups, we were constrained to use the grouped demographic data. As noted, however, the 1983 census data fully report the values of each variable and with the exception of completed fertility we can assess and compare the results on the basis of the 1983 detailed data and the 1995 grouped data. We also grouped the 1983 data in the same way the 1995 data is grouped and used it for estimation. The results from the detailed ungrouped 1983 data and those obtained based on the 1983 grouped data are almost identical. We therefore conclude that the grouping of some of the variables in the 1995 data is not an important limitation for our purpose.

age cohorts. The stability of these disparities suggests that there were no dynamic differences between treatment and control during the 1948–1963 period. This pattern is important for our identification strategy; we turn to it in the next section when we discuss the threat of convergence in fertility and education. Finally, as noted above, we also use a subset of the control group that excludes the population of the largest seven towns for a robustness check. This comparison group has the valuable advantage of being almost identical to the treatment group in its pre-1964 characteristics and mean outcomes which eliminates the concern of convergence.

4. Identification, Estimation, and Basic Results

An individual’s exposure to the change in access to schooling due to the cancellation of travel restrictions in late 1963 is determined jointly by two variables: her age in 1964 and her locality of residence. Until the mid-1970s, Israeli children attended primary school (grades 1–8) between the ages of 6 and 13 and secondary school (grades 9–12) at ages 14–18. We expect children of primary-school or early secondary school age in 1964 to have benefited from regaining access to schooling institutions. Therefore, all children born in 1950 or later, i.e., those who were under 14 years at the end of 1963, when the travel restrictions were removed, could benefit from the removal of these restrictions. Older cohorts could not, because they were too old to enroll in primary school or even in secondary school if they had completed primary schooling so long ago. Among the affected cohorts, the youngest in 1964 had the highest exposure to the renewed access to schooling; therefore, we expect the effect to be stronger among the younger members of this group than among the older affected cohorts. However, as described in the previous section, access to schooling could be affected by the annulment of the travel restrictions only in localities that were under military rule and did not have a primary school. Therefore, the second variable of exposure to the change in access to schooling is locality of residence in 1964. After controlling for locality and year-of-birth fixed effects, we use the interactions between a dummy variable for individual’s age in 1964 and the indicator for the existence of a school in locality of residence before 1964 as exogenous variables which can be used as instruments for an individual’s education. This identification strategy may be presented in an interaction-terms analysis of the first-stage relationship between education (S_{ij}) of individual i , who resided in locality j and belonged to cohort l , and her exposure to the program:

$$(1) \quad S_{ij} = \alpha + a_{ij} + \mu_l + \sum_{l=2}^{18} (A_j T_{il}) \delta_l + \varepsilon_{ij}$$

where T_{il} is a dummy that indicates whether individual i is age l in 1964 (a cohort dummy), α is a constant, μ_l is a cohort of birth fixed effect, a_{ij} is a locality-of-residence fixed effect, and A_j denotes

a locality that was exposed to treatment (=under military rule and lacking a primary school). In this equation, we measure the time dimension of exposure to the program with 22 year-of-birth dummies. Individuals aged 22–23 in 1964 constitute the control group; for them, this dummy is omitted from the regression. Each coefficient δ_l can be interpreted as an estimate of the treatment of a given cohort. We expect coefficients δ_l to be 0 for $l > 14$ and to start increasing for l values below some threshold (the oldest age at which an individual could have been exposed to treatment and still could have benefited from it).

Figure 1 plots the δ_l coefficients when, for considerations of sample size and estimation precision, we group age cohorts as follows and impose the same δ_l on all groups: 2–3, 4–5, 6–7, 8–9, 10–11, 12–13, 14–15, 16–17, 18–19 and 20–21. Notably, we use the 1983 census for this estimation because its data provide detailed age information, unlike the 1995 census data, which group individuals' ages. Results based on a separate regression for each group of birth cohorts yield a very similar pattern. Each dot on the solid line represents the coefficient of the interaction between a dummy for being in a given group of age cohorts in 1964 and the dummy indicator of exposure to treatment. The 90 percent confidence interval is plotted by dashed lines and the standard errors are clustered by locality. In Figure 1, the estimated coefficients are small, similar in size, and not different statistically from 0 for the 12–13, 14–15, 16–17, and 18–19 age groups, and clearly suggest no differential time trend in education for those in the treatment group who were 12 or older in 1964. The estimated δ_l then jumps to about 0.88 at age 10–11, and remains at this level (on average 0.95) for the youngest age cohorts, 2–9. The five estimates in the younger than 12 age groups are significantly different from zero and are more precisely estimated for the younger cohorts. In contrast, the average estimated coefficient for cohorts over 12 years old is about 0.3 and is not significantly different from zero.

We also present in Figure 1 the cohort fixed effects (μ 's) in addition to the interaction coefficients. These estimates reveal the important secular trend in completed years of schooling in addition to the differential change in trends among treated women at the 10–11 years old cohort.

The evidence presented in Figure 1 suggests, as expected, that the treatment had no effect on the education of age 14 and older cohorts in 1964 and had a positive effect on the education of younger cohorts. The effect on the 12–13 age cohort is positive but much smaller (=0.49) than younger cohorts and is not precisely measured. This demonstrates that the identification strategy is reasonable and that the change in travel policy that led to a change in access to schooling affected girls' education. By implication, we may use the unaffected older cohorts as a comparison group for estimation of the effect of treatment on the affected cohorts.

Given these results, we move on to the use of data from the 1983 and 1995 censuses to estimate the effect of the change in travel restrictions in 1963 on schooling and fertility. We focus our analysis on the following age groups. The first group includes those born in 1956-1960 (were 4-8 years old in 1964), the second includes those born in 1951-55 (9-13 years old in 1964). Individuals in these two groups were young enough to be affected by the treatment. Two other age groups are older and therefore could not be affected by the program: the first age group includes those born in 1946-1950 (their ages were 14-18 in 1964) and the second group includes those born in 1942-1945 (their ages were 19-23 in 1964). At the time of the earlier census in June 1983, our youngest treated group was aged just over 24–27 years old, and the older treated cohort was 27–33. By the later census in November 1995, the youngest treated group was aged 36–40 and the oldest was aged 41–45. The unaffected cohorts were 33-37 and 38-41 in mid-1983 and 46-50 and 51-55 by the end of 1995. On the basis of this range of treated groups, we may estimate the effect of treatment on women in various age groups, including one that is definitely old enough (over age 40) to have completed its education and, in all likelihood, its fertility as well.

Testing for convergence

The estimates of the effect of education on fertility may be biased due to pre-existing differences in fertility rates which led to differential rates of convergence. We use pre-reform data (from the 1983 census) relating to the localities' mean fertility rate for cohorts aged 14–24 in 1964 to estimate different time trends in treatment and control localities. We employ two methods for this estimation. First, we estimate a model with cohort dummies and include in the regression an interaction of each of these cohort dummies with the treatment indicator. Second, we estimate a constant linear time-trend model while allowing for interaction of the constant linear trend with the treatment indicator. In both models, we also include a main effect for the treatment group indicator (treatment group dummy). Both models suggest that there is a time trend in the fertility rate but that this trend is identical in treatment and control localities. This result is presented in Column 1 of Table 2. Panel A presents the estimates of the model that includes the cohort dummies and their interaction with the treatment indicator. The interaction terms are all small and not significantly different from zero; furthermore, some are positive and others are negative, lacking any consistent pattern. The omitted cohort in this regression is age 14 but regardless of which cohort is omitted the important point is that the interaction terms are not changing in way which is consistent over time. Panel B presents the estimates of the linear trend model. The mean trend is an annual decline of 0.241 in the fertility rate. The estimated coefficient of the interaction of this trend with the treatment indicator is practically zero, -0.014 ($SE=0.054$). This evidence is fully consistent with

the results presented in Panel A. Therefore, we are confident that there were no pre-reform differential time trends in treatment and control localities that might confound the estimated treatment effects that we present below.

We also extended the time-trend analysis to show that there was no pre-reform treatment-control differential time trend in mean years of schooling. These results are presented in column 2 of Table 2 and they fully confirm that there was no treatment-control differential time trend in female education before 1964. For example, the estimated coefficients of the interaction terms between the treatment status and cohort dummies are sometime positive and sometime negative and these changes are not consistent over time. These estimates are also not statistically different from zero. The estimates presented in Panel B of columns 2 are consistent with the estimates presented in panel A. For example, the mean trend among cohorts aged 14–23 in 1964 is an annual increase of 0.290 (SE=0.032) in years of schooling. The estimated coefficient of the interaction of this trend with the treatment indicator is practically zero, 0.017 (SE=0.065). Overall, the estimates presented in column 2 are fully consistent with the evidence in Figure 1 for cohorts older than 13 years old in 1964.

Before moving to the DID estimation, we present in Table 3 time trend estimates where we pool together data for ages 2-23 in 1964 and allow for trend differences for affected cohorts (age 2-13) and unaffected cohorts (ages 14-23). Strikingly, the linear trend estimates for the two age groups in control group are identical, both for the fertility and the years of schooling trend models. However, the estimates of the interaction between time trend and treatment indicator are very different for the two age groups. These interactions in the fertility equations are negative and significantly different from zero and they are positive and significantly different from zero in the education equation. Extrapolating these trend estimates for say, a decade, implies an increase of almost 0.5 year of schooling and a fertility decline of 0.4 children. In the next section, we sharpen the estimation of the sharp trend break in fertility and education in treatment localities and the implied changes in women’s education and fertility.

4a. DID Estimates of Access to Schooling on Education

We estimate DID models in a regression framework in order to allow the addition of controls that will improve estimation efficiency and precision of estimates. This suggests running the following regression:

$$(2) \quad S_{ijl} = \alpha + a_j + \mu_l + (A_j Y_i) \delta + \varepsilon_{ijl}$$

where S_{ijl} is the education of individual i from cohort l who lives in locality j , Y_i is a dummy indicating whether the individual belongs to the “young” cohort in the subsample, α is a constant,

μ_l is a year-of-birth (cohort) fixed effect, a_{lj} is a locality-of-birth fixed effect, and A_j denotes areas that were exposed to the treatment.

Columns 1–2 in Table 4 present estimates of Equation (2) for three subsamples. In Panel A, we compare children aged 4–8 in 1964 with children aged 14–18 on the basis of the 1983 census data (first row) and the 1995 census (second row). The standard errors are clustered by locality in this table as well as in all results presented in the following tables. We also present the heteroskedasticity robust standard errors. The treatment indicator is the interaction of the ‘Young’ and the A_j indicators, and its estimates show that the treatment increased the education of female children aged 4–8 in 1964 by 0.694 by 1983 and 0.921 by 1995 (column 1). In column 2, we add locality fixed effects as controls, eliciting DID estimates of 0.738 and 1.018 for 1983 and 1995, respectively. The estimated standard errors are lower when we add these controls and the point estimates are statistically significantly different from zero.

Panel B of Table 4 shows the results of the 9–13 age cohorts in 1964; again, the control group consists of children aged 14–18 in 1964. Here as well, we report results based on 1983 and 1995 census data. The estimated effect of treatment on the older cohorts, as expected, is lower than the estimated effects obtained for the younger cohorts. The 1995 DID estimates presented in Column 2 is 0.575 (SE=0.346).

Panel C of Table 4 presents the results of the control experiment based on comparing the 14–18 age cohorts with those aged 19–23 in 1964. If education had increased faster in affected areas before the removal of the travel restrictions, Panel C would show positive coefficients (which cannot reflect an actual treatment effect). The impact of this false “treatment,” however, is very small or even negative and never significant. For example, the control-experiment estimate in Row 1 and Column 2 of panel C is 0.039 (SE=0.375), practically zero and much lower than the respective estimates in Panel A and Panel B. Although this is not definitive evidence (the education level could have started converging precisely after 1963), it is reassuring.¹³

Which levels of education were affected by the change in access to schooling?

To interpret the estimates of the effect of education on fertility and children’s schooling, we need relevant evidence about the levels of education at which the policy change had this effect. We present in Table 5 estimates of reduced-form Equation (2), in which the dependent variable is

¹³ As noted in the data section, the age, education and the fertility variables in the 1995 census are grouped and we used the mid points in each range of grouping. To assess how the grouping affects our results, we also grouped similarly the 1983 data and used it for estimating all models of Table 4. The results from the 1983 grouped data are identical to the 1983 results presented in Table 4 and are available from the authors.

now a dummy indicator of the education level attained. We use data from 1995 census that includes information on completed years of schooling (grouped to 1-4, 5-8, 9-10, 11-12, 13-15, 15+), information of highest achieved diploma and an indicator on obtaining a matriculation certificate which is distinct from graduating high school. We use as level of education both the grouped completed years of schooling and the diploma attainment in order to have a more complete range of schooling achievement. We consider the following educational thresholds that individuals attained at least: 5–8 years of schooling, primary school (exactly 6 years of schooling), 9–10 years of schooling, secondary-school diploma (exactly 12 years of schooling), matriculation certificate, and post-secondary certificate. The estimated equation includes individual controls and locality fixed effects. In the online appendix we present summary statistics that show the fraction of men and women in the treatment group (Table A2) and in the control group (Table A3) reaching these various education levels.

The second column of Table 5 presents the estimated reduced-form effect for the 4–8 age cohorts. The effect is positive and significant for attainment of three of these thresholds. The estimates indicate that the policy change allowing access to schools increased the probability of completing at least primary school by 8 percent and of attaining at least 9–10 years of schooling by 6.4 percent. Given the mean presented in column 1 of Table 5, these two gains imply a 12 and 14 percent increase, respectively. Overall, these estimates suggest that the mean gain in years of schooling included individuals who reached high school but did not complete it. Conversely, the evidence in Column 3 for the older affected cohorts suggests that the gain for the 9–13 age group originated mainly in an increase in post-primary schooling, but these effects are not precisely measured. Column 4 presents estimates based on the control experiment. Although the evidence overall shows mostly negative estimates for all educational-attainment thresholds, most of the estimates are not statistically different from zero.

The effect of access to schooling on men's education

The travel-policy change may also have affected the education of Arab men. Appendix Table A4 presents results of the estimation of Equation (2) based on a pooled sample of men and women. The results, calculated for the 4–8 and 9–13 age cohorts, are based on 1995 census data but are not different when 1983 census data are used. Much as in our earlier results, the estimates for women are positive and significant in all three specifications. However, the estimated effect of treatment on men is practically zero in both the 4–8 and the 9–13 age cohorts. For the 9–13 age cohorts, for example, the effect on women's schooling is 0.620 (SE=0.292) and that on men's schooling is 0.117 (SE=0.216).

The very small and insignificant effect on men’s schooling as against the strong effect on women’s schooling is not too surprising because it could be that the school access cost shock was much greater for female, perhaps because girls have fewer safe travel options. This may be particularly relevant in the context of a traditional Arab-Muslim society that often confines girls and women to home and does not permit them to travel alone and would be reluctant to expose girls and women to the risk of friction with soldiers and other security forces. For the same reasons, living with relatives or in residential schools, are less likely for girls than for boys. Interestingly, too, Gould, Lavy, and Paserman (2011) report that a low-quality childhood environment had a large negative effect only on the education of girls from traditional Jewish families in Israel during the 1950s and 1960s and did not affect the schooling attainments of boys in the same families at all. The gain in years of schooling from access to a better childhood environment estimated in this study was almost 0.75 year, very similar to our estimate for Arab women in this study.

4b. Effect of Access to Schooling on Fertility

The same reduced-form identification strategy can be applied to estimate the effect of access to schooling on fertility. The identification assumption—that the change in fertility and education across cohorts would not have varied systematically across affected and unaffected areas in the absence of the removal of the travel restrictions—suffices to estimate the reduced-form impact of the change in travel policy. Additionally, if we assume that the change in access to schooling had no effect on fertility other than by increasing educational attainment, we may use this policy change to construct instrumental-variable estimates of the impact of additional years of education on fertility.

We can write an unrestricted reduced-form relationship between exposure to the travel-policy change and women’s fertility as:

$$(3) \quad F_{ij} = \alpha + a_{ij} + \mu_l + (A_j Y_i) \delta + \varepsilon_{ij}$$

where F_{ij} is the number of children in 1995 of individual i of cohort l , who was born in locality j . A_j is an indicator for the localities without a school and Y_i indicates the young affected cohorts. The results of the estimates of parameter δ based on the three specifications of Equation (3) are presented in Table 4, Columns 3–4. Panel A compares the fertility of women who were aged 4–8 in 1964 with that of women aged 14–18 in 1964. In Column 3, the specification controls for the interaction of a cohort of birth dummy, the population of the young cohort in 1964 and individuals’ religion and the estimate is -0.533 ($SE=0.324$). When we add locality fixed effects to the regression estimated, the estimate is practically unchanged. The estimates based on the 1995 census data and these two specifications are marginally higher and more precisely estimated than the estimates

reported above. The 1995 reduced-form estimate based on the second specification (with individual controls and locality fixed effects) is -0.609 ($SE=0.211$). This estimate implies that the removal of the travel restrictions reduced these women’s completed fertility by just over half a child.

Panel B of Table 4 presents DID estimates based on 8–14 age cohorts as the treatment group. The estimated effect of the improved access to schooling is, as expected, lower among older cohorts than among younger ones. Based on the 1983 census data, the full DID estimate with locality fixed effect is -0.342 ($SE=0.260$), about 40 percent lower than the reduced-form estimated effect obtained for the younger cohorts. Given that the reduced-form effect on the older group’s education is also 50 percent lower than that on the younger cohorts, we should expect the 2SLS estimate of the effect of education on fertility obtained from the young and older age cohorts to be very similar. The estimates obtained while using the 1995 census data are, again as expected, greater than those based on the 1983 census data (because it captures complete fertility) and they are more precisely estimated but they are smaller than the corresponding estimates of the younger affected cohorts.

The evidence obtained from the control experiment presented in Panel C supports the identification assumption that there are no omitted time-varying and area-specific effects correlated with the removal of travel restrictions. If fertility decreased faster in affected regions before the removal of the travel restrictions, Panel C would show (spurious) negative coefficients. The impact of “treatment,” however, is very small and never significant. For example, the DID estimate in Column 4 of Panel C, based on the 1995 census data is -0.124 ($SE=0.277$), not allowing us to reject that it is not statistically different from zero.¹⁴

4c. IV Estimates of the Effect of Mother’s Education on Completed Fertility

The estimates of Equations (2) and (3) are first-stage and reduced-form equations that can be used for instrumental variable (IV) estimation of the impact of female education on fertility. Consider the following equation, which characterizes the causal effect of education on fertility:

$$(4) \quad F_{ij} = \alpha + l_{ij} + \mu_i + S_{ij} \lambda + \varepsilon_{ij}$$

where l_{ij} denotes locality-of-birth fixed effects, and λ is the marginal effect of education on fertility. Under the assumptions that, the removal of barriers to access to schools in the absence of the removal of travel restrictions in October 1963 had no direct effect on fertility, the interaction

¹⁴ We also estimated another placebo regressions looking at the effects of the removal of the travel restrictions on the Jewish population of towns and small cities in the geographical region of the Arab treated and control localities. We note that no Arab resides in these localities so spillover effects are very unlikely. These estimates show no first stage and reduce form effects.

between belonging to young cohorts in 1964 and regained access to schooling in the locality of residence may be used as an instrument for Equation (4).

The 2SLS results of estimating λ are shown in Table 4—the OLS estimates in column 5 and the 2SLS results in column 6. The OLS estimate for the youngest affected cohort based on the 1983 data, is presented in Row 1 of Panel A, is negative at -0.240 and very precisely measured ($SE=0.015$). The IV estimate is also negative, -0.730 , larger than the OLS estimate and significantly different from zero based on the robust standard error. Row 2 of Panel A presents the results for the young cohort based on the 1995 census data. The 2SLS estimate here is -0.598 , significantly different from zero at the ten percent significance level and only marginally lower than the estimates obtained from the 1983 data. The latter 2SLS estimate reflects a relatively short-term effect, as the affected cohorts were less than 30 years old on the survey date while the former estimate (based on 1995 census data) reflects the effect of education on completed fertility, as all affected women were already close to or older than 40 years at survey date.¹⁵

4d. IV Effects by Distance to Nearest School and Implied 2SLS Estimates

Removing the travel ban can have two different effects on the demand for schooling. On the one hand, we expect the effect on years of schooling to be smaller in localities near schools because the post-1963 decline in the cost of attending school is lower in such localities. However, if women were not allowed to travel at all under the ban, regardless of distance, after the ban is lifted women within a short distance of a school might enroll at higher rate than women who are far away from school and therefore travel cost is still high. To test the net effect of these two opposing potential changes, we divided the treated localities into two groups differentiated by

¹⁵ We have shown above evidence that the removal of travel restrictions did not affect male years of schooling. However, in order to further substantiate the evidence that our estimated effect of mother's schooling is not confounded by a direct effect of father's education, Table A5 in the online appendix presents evidence on the basis of two subsamples differentiated by spouse's age in 1964. This estimation is subject to the caveat that the age gap between spouses can be endogenous. The first subsample is restricted to women who were aged 4–8 in 1964 and their husbands were older than 8 in that same year; it includes 60% of the full sample of women. In Table A4 we showed that the change in travel restriction had no effect on the schooling of men aged 9–14 (37% of the full sample). The second subsample is restricted to women whose husbands were older than 13 in 1964; it includes 35% of all women in this sample. This group of men could not have benefited from the change in access to schooling in 1964 because they were simply too old at the time. The IV estimate based on the first sample and presented in Panel A of Table A5 is -0.683 ($SE=0.399$), very similar to the estimate based on the full sample of women in these age cohorts (-0.598 , $SE=0.332$). It is also reassuring to note that the first-stage and reduced form effects reported in Table A5 are also almost identical to their corresponding estimates in Table 4. Finally, the estimates obtained from the second restricted subsample (based on spouse's age) are also very similar to the corresponding estimates reported in Table 4. These results support the interpretation of our estimates of the effect of mother's schooling on fertility as causal, net of the direct effect of her spouse's schooling.

distance to the nearest (control) locality that had a school. The first group included all localities with a distance of less than 4 kilometers; the second group included all other localities (distance of 4 kilometers or more). We then estimated first-stage reduced-form OLS and IV models separately for each sample, leaving the control group the same as before. To assure a meaningful sample size for the two treatment groups, we combined the two age groups (the 4–8 and 9–14 age cohorts) into one sample but added an indicator to the regression to distinguish between them.

The results are presented in Panel A of Table 6. The first row in this panel includes the estimates from the regressions based on the first sample (treatment localities at shorter distances from schools); the second row shows localities that are farther from schools. The first-stage estimated effect on schooling is larger in localities farther from the nearest school, 1.023 (SE=0.388), than in localities closer to the nearest school, 0.612 (SE=0.466). Symmetrically, the reduced-form effect in the localities that are farther from a school is also larger: -0.694 (SE=0.218) versus -0.426 (SE=0.212). However, both corresponding estimated 2SLS effects are very similar to the IV estimate reported in Table 4 on the basis of the combined full sample.¹⁶ This evidence, we believe, strengthens the interpretation of the effect of the travel-policy change in 1963 on schooling as reflecting a decline in the cost of attending school.¹⁷

We conclude this section by discussing the differences between the 2SLS and the OLS estimates. First, our IV estimate is greater than the OLS estimate (Leon, 2004, reports a similar direction of this gap), although we cannot reject the hypothesis that the IV estimate is not different from the OLS estimate based on the confidence interval of the IV estimate and an Hausman test. One explanation for why the OLS estimate is different is that we are estimating a LATE and that

¹⁶ We did the same robustness test for boys and found that there is no effect on boys' schooling regardless of the distance to the closest school.

¹⁷ To check whether the differences in first-stage and reduced-form effects by distance to nearest school do not reflect some other heterogeneity, Table A6 presents evidence based on stratification of the sample by size of locality. In Panel A, the treatment group is divided into small and large localities, while the full control group is used; Panel B also divides the control group into small and large localities and matches both groups with their respective treatment groups. The evidence clearly shows no apparent differences in the first-stage and reduced-form estimates for the small and large treatment localities, irrespective of the control group used. The estimated 2SLS estimates are also similar for the small and large localities and in Panel B are even identical, at -0.683 and -0.686 , respectively. This is a very important result because the treated and non-treated localities in the small localities sample are practically identical in many important observed dimensions such as level and pre-reform trends in demographic, socioeconomic characteristics, local labor markets. This evidence also rules out the possibility that the need to travel outside the locality to attend schooling is correlated with area and family characteristics and it supports our claim that the instrument operates through a single causal channel – schooling. In addition, the evidence based on the sample of small localities also dispel the concern that the end of the military rule had differential developmental impact on treated and non-treated areas, above and beyond access to schooling, because the enforcement of the military rules and their relaxation should have been similar in localities of similar size and nature, such as small urban towns and rural villages.

the population affected most by the IV is also more vigorous about its children's education and, in particular, more concerned about that of its daughters. Another explanation of the high LATE estimate is that primary schooling has a stronger effect on fertility than gains in secondary or tertiary schooling. As we saw, the increase in years of schooling due to the historical episode was primarily among students who otherwise wouldn't have completed primary school. It is possible that an increase in the lower levels of education (say, 5 to 6 years) is more effective in reducing fertility than in the higher level of schooling (say, 10 to 11). Since the treated localities had lower levels of education to begin with, this can explain why the LATE is different from the OLS estimate. We used the 1995 census data to run OLS regressions of educational level on fertility and it yielded evidence that does not support this hypothesis. The estimated coefficient of 1-4 years of schooling on fertility in this OLS regression is practically zero; it is negative but small for 5-8 years and there are large jumps in the negative effect of 9-10, 11-12, and 16+ years of schooling. Finally, potential measurement error in the schooling variable may have lowered the OLS estimate, and this could have been corrected by our instrumental variable estimation. A different explanation for the higher IV estimate may come from the fertility hypothesis regarding minority-group status and fertility (Goldscheider and Uhlenberg, 1969, Ritchey, 1976). This hypothesis posits that a deprived minority group that also experiences discrimination will adopt a higher fertility rate as a strategy to strengthen itself against an external threat. Keyfitz and Flieger (1990) use this hypothesis to explain the high fertility rates in Northern Ireland and among the black and white populations of South Africa. Anton and Meir (2002) suggest that the fertility of Muslims in Israel reflects a survival strategy inspired by radical nationalism. However, if radicalism and education are correlated but the latter does not cause the former, it may induce a lower OLS effect of education on fertility. Having provided these possible explanations, we reiterate that our IV estimate is not significantly higher than the OLS. Finally, we note that our estimate represents an effect size only marginally higher than Leon's (2004) estimates, based on 1950–1990 U.S. census data. Leon reports an instrumental variable estimate of -0.35 using changes in state compulsory-schooling laws as a source of exogenous variation in women's education.¹⁸

5. Robustness Checks and Threats to Identification

Our identification assumption for estimating the causal effect of mother's schooling on fertility may be violated if the removal of travel restrictions caused other changes that could have affected fertility directly or indirectly. Below we address such potential threats due to improved

¹⁸ Leon's (2004) study is about much more educated cohorts. This can support the explanation that the effect is stronger among lower levels of education.

access to labor market opportunities, pre- and post-natal health care and general health care services and show evidence that suggest that they cannot account for our results.

Improved access to labor market opportunities that might have impacted differentially the younger cohorts in treated localities could have caused the decline in fertility that we documented above. However, we have shown above that the labor force participation (as measured in 1983 and in 1995 censuses) of the affected cohort was not affected by the removal of the travel restrictions and here we add more related evidence based on data from the 1972 and 1983 censuses. The advantage of using the 1972 and 1983 censuses data over the 1983 and the 1995 censuses data for this analysis is that the former pair of censuses is closer to the date at which travel restrictions were removed and therefore it could better reflect improved employment opportunities.

The affected cohorts who were 4-13 years old in 1963 came to the age of 24-33 in 1983. Based on these cohorts and those of similar age in 1972, we estimated DID treatment effect on four labor market outcomes: labor force participation, number of weeks worked in the last 12 months, an indicator of working outside the locality and the natural log of wages. Note that both in 1983 and in 1972 the travel restrictions have already been removed, and so there was no differential change in accessibility to the labor market between the two censuses. What we test here is whether being released from the restrictions while being at school earlier on has had an effect on the later labor market outcomes. The results are presented in appendix Table A7, separately for men and women. All estimates presented in the table are very small and none are significantly different from zero. Of particular importance is the zero effect on the probability of working outside the locality, which is further evidence that the removal of the travel restrictions did not have differential effect at a later time on the cohorts that were subjected to the treatment while being at school age, aside from their effects on education. This is evidence that the additional effect of lifting the travel restrictions when the cohort was still in school did not have a differential effect on labor market outcomes.

We can further check for potential confounders based on the 1972 census data which includes measures of family wealth and income. We used the following variables as outcome measures: number of rooms at home, indicators of availability of electricity at home, running water and toilet, and log of family income.¹⁹ The 12-21 age cohorts in 1972 are our treatment group (those who were 4-13 years-old in 1964) and the cohorts 22-26 age cohorts in 1972 are our control group

¹⁹ We focus on access to electricity, running water and indoor toilet because they were shown in Gould, Lavy and Paserman (2011) to be important determinant of long term human capital outcomes and fertility of immigrants from Yemen in Israel. The 1972 census data includes other measures of ownership of appliances such as television, telephone, cooking oven, car and more but very few families owned such appliances at the time, especially among the Arab population of Israel.

(individuals who were 14-19 years-old in 1964). We estimated DID regressions based on these definitions of treatment and control groups and the definition of treatment and control localities. These estimates are presented appendix Table A8. All estimates are small and none is significantly different from zero.

Another possible important confounder is the access to pre- and post-natal services that could have improved after 1964 and perhaps more so in the treated localities. These services are provided in Israel on site at special public well-baby centers, who also provide family-planning services and contraceptive information as well as checkups and immunizations for children in kindergarten and schools. If, for example, such centers existed in localities that had schools before 1964 and not in localities that lacked them until after 1964, then the cancelation of travel restrictions in 1963 could have facilitated access to such centers. Such access could have reduced infant mortality, for example, and, in turn, fertility and it could have increased exposure to contraceptives which could also lower fertility. Such direct effect on fertility would have coincided with the fertility decline occasioned by the increase in mother's schooling and would make the two difficult to disentangle. The 1965 annual report of the Israel State Comptroller and Ombudsman, however, provides information indicating that this concern is not relevant in our case. The report notes that in 1964 there were 46 Arab localities that did not have well-baby centers and where the population did not receive these services locally in any other way and 40 of them had schools. This suggests a low or even zero correlation between access to schools and access to well-baby centers. Another possibility is that when the government cancelled the travel restrictions it also expanded its investments in well-baby health services precisely in treatment localities. Our evidence suggests that this did not happen because large public investments and other types of government initiatives to improve social and economic infrastructure in the Arab sector were not evidenced until the 1980s, partly due to the severe economic recession in 1966 and partly due to the heavy military burden of the 1967 and the 1973 wars.

Another argument why differential access to pre- and post-natal services wasn't likely to have been a major issue is that the oldest girls in the control group were 23 when the restrictions were removed, and so almost all of them had access to these services while giving births. This evidence is further supported by the fact that in the early 1960's there were no significant differences in infant mortality rates between treatment and control localities in our sample. The 1960 census included a question on infant mortality. The mean infant mortality per women aged 18-30 in 1960 was 0.41 and 0.32 in treatment and control localities, respectively and the difference (0.09) is not statistically different from zero (SE=0.08). Controlling for exact age, this difference declines to 0.06 (SE=0.07). This pattern is similar when older age groups are considered.

Consequently, the reduction in fertility that we estimate is very unlikely to have been caused by improved access to well-baby centers.

Another similar potential concern is that localities that had schools had also general health clinics and that those lacking the former also lacked the latter. If such was the case, the exposure of the treated population to lower cost of schooling may be correlated with lower cost of visiting general health clinics, which could have reduced infant mortality and improved adult health. Both potential effects may have affected fertility directly, although it is not clear to which direction, confounding our estimates of the effect of mothers' schooling on fertility. The State Comptroller's report cited above, however, also provides information about the location of general health clinics and we used these data to investigate this concern about our identification. The report shows that while there were 54 clinics in Arab localities in Israel in 1964, the two regions where most of the Arab population in Israel lived at the time—Acco (north) and Hadera (center)—had no such clinics at all in any of the Arab localities. Thirteen of our treated localities and 11 of our control localities were in Acco region. The nearest school for each of the 13 treated localities was in one of the 11 control localities. By implication, in all 13 cases the nearest locality with a school did not have a health clinic. A similar pattern emerges in the Hadera region, which included five of our treated and four of our control localities. However, to further study the potential confounding effect of access to general health clinics, we obtained data from the main provider of healthcare in Israel at the time about the exact location of its clinics in the localities in our sample. Thirteen of the control localities and five of the treated localities had such clinics in 1964. Table 7 presents evidence based on adding to the regression a control for localities that had a general health clinic. The first-stage, reduced form, the OLS and 2SLS estimates presented in Table 7 are almost identical to those in Table 4. The corresponding results that we obtained using the 9–13 age cohorts are identical to those in Table 4; we do not present them here due to space considerations. This evidence permits us to conclude that the reduction in fertility was not caused by improved access to general health services that were unique to the treated localities in our sample.²⁰

More on Convergence and Results Based on Alternative Control Groups

We discussed above the issue of convergence as a threat to identification and presented evidence in Table 4 that alleviates this concern. The evidence presented in this section is a further

²⁰ Additional evidence suggests that the health improvements were not unique to the population in localities that had no schools. The *Israel Government Yearbook* for 1995, for example, provides details on health improvement programs for the Arab population that were implemented in all localities, such as a campaign to stamp out tuberculosis, scalp ringworm (jointly with UNICEF), and trachoma among schoolchildren.

check against the threat of convergence. We present estimates based on two alternative control groups. The first is a subsample of the original control group, excluding the population of the seven largest localities in the sample. We excluded only seven localities due to sample-size considerations. The results of excluding the largest five or largest eight localities, however, are very similar to those obtained when excluding seven localities. In any case, this modification produced a control group that is more similar to the treatment group in terms of the characteristics and pre-reform outcomes of unaffected cohorts of both groups. This change may be seen in Columns 1–4 of Table A9, which present the mean characteristics of this control group. For example, the control-treatment difference in fertility rate among those in the 19–23 age cohorts declines from 1.12 based on the full sample of control localities to 0.72 based on the control group that excludes the seven largest localities.

A second alternative comparison group is the Arab population of mixed cities. Recall that this population was not subject to the travel restrictions and all these cities had primary schools and all but two also had secondary schools. The mean characteristics and outcomes of this comparison group for the older cohorts (14–19 and 19–23) are much better than those of the treatment group. (See Columns 5–8 in Table A9)

The results based on these two alternative comparison groups, presented in the first two panels of Table 8 and based on the youngest affected cohorts only (ages 4–8) and on 1995 census data, strongly resemble those reported in Table 4. The first panel reports estimates when the control group is the original less the observations from the largest seven localities, causing the sample size of the control group to fall by about half. The first-stage effect is 0.953, the reduced-form effect is -0.775 , and the 2SLS estimate is -0.814 ($SE=0.513$), similar to the estimate obtained based on the original control group (-0.598 , $SE=0.332$). Note that the corresponding OLS estimate is lower than that reported in Table 4. This is expected because the population eliminated from the control group (that of the seven largest localities) is more educated and also has fewer children. Since the latter characteristic may trace to reasons other than education, the OLS estimate becomes smaller when this group is excluded from the sample. Panel B in Table 8 reports estimates when the control group is the Arab population of the mixed cities. The 2SLS estimate is -0.485 ($SE=0.153$), not so different from the respective estimate in Table 4.

The fact that two alternative sets of DID estimates, one based on a comparison group that has much better characteristics and outcomes than the treated group and another based on a comparison group that has marginally better characteristics and outcomes, yield the same qualitative results is reassuring, given the possibility that the DID estimates are biased due to convergence arising from differential time trends.

Panel C in Table 8 presents estimates based on a sample that includes only individuals who were living in their locality of birth at the time of the 1995 census. This sample includes 75 percent of the original sample. The first-stage, reduced-form, and 2SLS estimates are almost identical to the corresponding estimates reported in Table 4. For example, the 2SLS estimate in Table 8 is -0.602 ($SE=0.344$) while that in Table 4 is -0.598 . This result is not surprising because the very few who left their locality of birth most likely moved to a nearby village or town that had the same treatment status as their locality of birth. Another, and perhaps more important, explanation for the similarity of results is that the pattern of movement from the place of birth is not different between the affected and unaffected older cohorts that we include in the treatment group and similarly across cohorts in the control group.

We also estimated treatment effects using the Jewish population of towns and small cities in the geographical region of the Arab treated localities as a comparison group. This alternative control group includes mainly Jewish immigrants from Arab countries who had arrived to Israel after 1948. The results based on this sample (not shown here) strongly resemble those reported in Table 4: based on 1995 census data, the 2SLS estimates were -0.494 ($SE=0.169$) for the 4–8 age cohorts and -0.585 ($SE=0.252$) for the 9–13 age cohorts.

6. Mechanisms of Effect of Education on Fertility

As discussed in the introduction (footnote 4), education may affect fertility in various ways, including labor-force participation and wages that figure in the shadow cost of children, age at marriage, and marriage and divorce rates. Through assortative matching, education can also affect fertility via spousal outcomes, e.g., spouse's education, and labor-market outcomes. To examine these potential mechanisms, we estimated IV equations similar to Equation (4), in which the outcome is one of these own demographic and labor market outcomes and the labor-market outcomes of the spouse. These results, presented in Table 9, suggest overall that the increase in women's education had no discernible effect on any of the own economic and demographic outcomes shown in the table.

Furthermore, the OLS estimated effect on labor-force participation is positive and highly significant for both affected cohorts, while the IV estimates are all negative but very imprecisely measured and are therefore practically no different from zero. The absence of a positive effect of education on female labor-force participation may trace to the preponderance of primary schooling in the gain in total schooling in a traditional society, which may induce little or no change in market participation. Recall also that average female labor-force participation is very low in this population

group and that the employment of Arab women, especially Muslims, is largely local, with no out-of-town travel. These constraints narrow the potential effect of education on female employment.

The OLS relationships between women's education and marriage and between women's education and age at marriage, are positive and highly significant but the IV estimates show no such relationship in either outcome. The estimated effect of education on these two outcomes is relatively small,²¹ inconsistent across samples, and given their estimated standard errors, not statistically different from zero. Conversely, the effect of education on the probability of divorce is small and insignificant in both the OLS and the IV estimation.

Summarizing the above evidence we note that the most important finding is that education had no effect on mothers' labor-force participation, a clear indication that the decline in fertility is not due to an increase in the effective cost of children resulting from an increase in cost of mother's opportunity time. Education must have affected fertility through other channels to which we turn next. One potential mediating factor is spouse selection. Panel B of Table 9 presents OLS and IV estimates of the effect of women's education on spouse's education, labor-force participation, and earnings. The spouses (husbands) in our sample are on average five years older than their wives and 30 percent of them are seven or more years older. This marital age gap implies that the spouses of those in our 4–8 age cohorts may have been affected by the annulment of the travel restrictions whereas the spouses of those in the affected older age cohort (9–13) were too old to have been affected by the regained access to schooling. However, since the travel-policy change had little effect on men in general (as shown in Table A4), we may conclude that the spouses of the women in our samples were not affected directly by the travel-policy change. These facts help interpret our finding that the increase in female education led to marriage with better educated men, i.e., one additional year of schooling enabled women to marry men who had an additional half-year of schooling. Note that the OLS and IV estimates of this effect are almost identical. This large magnitude of assortative mating suggests that some of the reduction in fertility of women in the young and older affected cohorts can be traced to better schooling on the part of their husbands.²²

²¹ Note that columns 6 and 8 on the marriage row show a negative effect of 6%, which may not be small when only 10% of the women are unmarried.

²² Although marrying better educated men may be at the 'expense' of women from older cohorts, this supply constraint (of educated spouses) was probably less binding in our context for two different reasons. The first is polygamy, which was prevalent among the Muslim population at the time; if polygamy prevalence has indeed increased among individuals in our treatment sample, it could also be a mechanism for the decline in fertility. However, we cannot assess this possibility due to data limitation about the practice of polygamy in our sample. The second is the removal of the travel restrictions, which probably expanded the "geographic coverage" of the marriage market, and expanded the range of mating options for both genders. Another possibility that will allow younger women to marry more educated men is to add non-linear complementarity in the matching-fertility model.

Higher father’s education would not lead necessarily to a decline in fertility, unless there is some sort of quality/quantity tradeoff in the demand for children and in the next section we provide evidence for such tradeoff in the population we study.

Finally, we note that while the OLS effects of mother’s schooling on spouse’s labor-force participation and earnings (Table 9) are positive and significant for both affected cohorts in both censuses, the respective IV estimates are much smaller, sometimes change signs, and are always not significantly different from zero. Therefore, it seems that neither outcome is a mediating channel through which the increase in mothers’ education reduced their fertility.

Effect of mother’s education on children’s schooling as a mechanism

In this section, we assess whether the change in mothers’ schooling affected the educational outcomes of their children, another potential mechanism working through a tradeoff between quality and number of children.²³ Here we use only 1995 census data in order to allow children to advance to the age where their education reflects completed schooling as closely as possible. For the same reason, we focus on the human capital of children who were born to mothers aged 18–26. This selection rule guarantees that the sample will include the oldest children, those most likely to have already attained post-secondary or even tertiary schooling, and that those in the sample will have a comparable mother’s age at birth.²⁴ To assure meaningful sample size, we merge the younger and the older affected cohorts, from age 4 to age 13 in 1964. Note that the unit in this sample is the child and not the mother and there are some mothers who have more than one child in the sample. We use the following educational attainments as outcome measures: completed years of schooling on census day, completed at least primary schooling, completed at least secondary schooling, and obtained a post-secondary certificate (academic degree or other). The sample includes 5,094 mothers of 10,847 children. Using this sample we estimate the following model:

$$(4) \quad E_{ijt} = \alpha + l_j + \mu_t + S_{ijt} \delta + \varepsilon_{ijt}$$

where E_{ijt} is child i ’s education outcome, j denotes the locality, and t denotes mother’s year of birth.

²³ Recent studies that aim to estimate the causal link between the education of parents and that of their children provide evidence that is far from conclusive. For example, Black et al. (2005) used for identification 1960’s school reform that extended compulsory schooling in Norway from 7th to 9th grade. Despite strong OLS relationships, this study finds little evidence of a causal relationship between parent education and child education. Oreopoulos, Page, and Stevens (2006) use a similar methodology and find that a one-year increase in parental education attainments reduces the probability of a child’s repeating a grade by 2–7 percentage points, and their IV estimates are more negative than the OLS ones.

²⁴ Notably, however, the education outcome is probably truncated for some children in our sample because they are still in school; this may bias downward the estimate of mother’s education on child’s schooling.

Table 10 presents estimates from the three specifications of the reduced-form relationship between mother's schooling and her children's education. For each specification and child's-education outcome we present estimates based on the quasi-experimental contrast between children of affected mothers from the 4–13 age cohorts, the unaffected 14–18 age cohorts, and the control experiment of contrasting two unaffected groups of cohorts, 14–18 and 19–23. Several results stand out in Table 10. In panel I, we present the results based on the full sample. The access to schooling that Arab mothers gained in 1964 caused an increase in the schooling of their children. This is reflected in higher attainments in secondary and post-secondary schooling, both of which reflected in an increase in total completed years of schooling. The effect on the probability that the affected mothers' children would complete secondary schooling is modest: an increase of 4 percent. The effect on the likelihood of completing post-secondary certification is an increase of 2.3 percent. In panels II and III of Table 10, we present the estimates from separate samples of boys and girls. The effect on girls is stronger with significant increase in completed years of schooling (0.679, SE=0.292), driven largely by an increase of 4.5 percent in enrolment in post-secondary schooling and a smaller, imprecisely measured, increase in secondary school attainment. For boys, the increase is large in secondary school attainment (0.052, SE=0.038).

Table 11 presents the OLS and 2SLS estimates of the effect of mother's schooling on children's educational outcomes based on the use of the 14–18 age cohorts as a control group. The OLS estimates are all positive and highly significant with large t-values. The 2SLS estimates surpass the OLS estimates except for primary schooling but are much less precisely estimated, suggesting that the OLS estimates are smaller by a large factor.²⁵ For example, the OLS estimate of the effect of completing at least secondary schooling is 0.007 while the 2SLS estimate is 0.067. The gap between the two respective estimates for the effect of obtaining at least a matriculation certificate is somewhat smaller.²⁶

Additional potential mechanisms

²⁵ Oreopoulos, Page, and Stevens (2006), who report a significant negative effect of parental education attainments on the probability of a child's repeating a grade, also report IV estimates that are more negative than the OLS ones.

²⁶ A question addressed in the literature is the intensity of the transmission of human capital from mothers to children. We can measure this parameter by calculating the ratio between the reduced-form effects of the treatment on the years of schooling that the mother and the child completed. The estimated effect on the mothers' years of schooling is 1.018 for the young affected cohorts and 0.575 for the older ones. Since the mothers of the children in our sample of analysis come from both affected groups, we can use the average of the two group-specific effects, i.e., 0.80 years of schooling. Since the reduced-form gain in children's schooling is 0.387, the ratio is 0.48, an estimate in line with evidence of studies that used instrumental-variable estimation to study the effect of parental schooling on children's education.

For evidence on additional potential mechanisms, we resort to data from a very detailed fertility survey conducted in 1974/75 among a representative sample of some 3,000 currently married Arab women under age 55 in Israel.²⁷ The women were asked about their childbirth histories, use of family planning, socio-economic characteristics and other topics which were thought to be relevant to reproductive behavior. Regretfully, this data source does not include information on locality of residence and therefore we could not link women in the sample to the historical episode we used in this paper. However, we can regress fertility on schooling and examine how the estimate of this coefficient changes as we add measures of potential mechanisms as controls. The strategy here does not amount to a clean identification of these additional potential mechanisms and the evidence below should be viewed as suggestive and not conclusive.

We grouped questionnaire items under the following six mechanisms: Fertility preferences, Contraceptive details, Beliefs about the effect of family size on quality of children and about gender differences in schooling investment, Child mortality, Religiosity, Role of women in family decision making, Health knowledge and modernism. The online appendix describes the individual items that we grouped under these six mechanisms in Table A10.

Table A10 presents the estimated coefficients of women and husband years of schooling on each of these 23 items by three different regression specifications. The first specification includes only woman's age and a religion dummy as controls. In the second, we add the husband's age and age of marriage, wife age of marriage, indicators of whether the woman and husband are currently working and indicators of whether they have ever worked. In the third specification, we add a measure of standard of living, number of rooms, and availability of electricity, running water and toilet in the woman's home. The parameters in Table A10 suggest that the woman's schooling is highly correlated with almost all of the 23 potential mechanisms, even in the regressions that includes all the controls (specification 3). The estimates of the woman's schooling are much larger than those of the husband's schooling. The latter are often small and not significantly different from zero.

In Table 12, we report the estimated coefficients of a woman's schooling in a fertility equation in three different age samples: 40-55, 30-55 and 20-55. We use four different specifications that vary by the set of control variables included in the regression. The estimated parameter of woman's schooling from the age 40-55 sample and the first specification (including only mother's age as a control) is -0.444. Note that this OLS estimate is much higher than the OLS estimate reported in Table 4 and it is much closer to the IV estimates reported in that table. Adding

²⁷ Details about the survey and variables for analysis are presented in the following link: <http://geobase.huji.ac.il:8080/catalog/?dataset=0187>.

to the regression all the measures of potential mechanisms described above as controls reduces the coefficient of woman's schooling to -0.285, a decline of 36 percent from -0.444. The R-squared, on the other hand, goes up from 0.188 to 0.401. This is evidence that more than a third of the correlation between a woman's education and fertility operates through these mechanisms.²⁸ A similar pattern is seen based on the estimates from the other two samples. We view these results as evidence that the increase in education of Arab women had an impact on women fertility through mechanisms that capture most of the channels suggested in the economic literature and summarized here in footnote 3. In our context, these mechanisms include fertility preferences, knowledge and use of contraceptives, some awareness to the effect of family size on quality of children, degree of religiosity, bargaining power of women in the household as reflected by her role in family decision making, reduced infant and child mortality and degree of modernism.

7. Conclusions

This paper studied the effect of women's education on their fertility in an economic environment with very low levels of female labor force participation. This is an important question with implications for economic development and growth and for social change, particularly among Muslim populations where many women are still out of the labor force. We extend this literature in a few directions by making several unique contributions. The policy change that we study generated large change in women's education: a gain of more than one year of schooling among affected cohorts who were young enough to have benefited from the removal of access restrictions to primary schools. The effect on fertility is negative and large, and explains some of the dramatic decline in the fertility of Israel's Arab-Muslim population.

We provide evidence that the effect of education on fertility that we estimated does not merely reflect other changes that differentially impacted the fertility of our treatment group. In particular, we show that the travel changes did not differentially affect the labor market opportunities of adults in treatment localities, their probability of working outside the locality, or their wealth and income. We find very low correlation between the availability of schools in the community and the availability of pre- and post-natal services and general health clinics and we show our results are robust to various sensitivity and falsification tests.

²⁸ Adding personal characteristics (husband's age, age of marriage, current and past labor force participation, and woman's age of marriage) as controls reduces further the estimate of a woman's schooling to -0.150, but it does not change further when family wealth variables (number of rooms, electricity, water and toilet at woman's home, and an index of family standard of living.) are added as well.

8. References

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Table 1: Pre-Program Mean Outcomes, 1983 and 1995 Pooled Census Data

	Treatment		Control	
	Age in 1964		Age in 1964	
	14-18	19-23	14-18	19-23
	(1)	(2)	(3)	(4)
<i>A: Women</i>				
Years of schooling	4.164 (3.949)	2.775 (3.507)	5.825 (4.372)	4.270 (4.139)
Fertility	5.748 (2.971)	6.824 (3.349)	4.909 (2.940)	5.845 (3.365)
Labor-force participation	0.120 (0.326)	0.079 (0.270)	0.165 (0.371)	0.151 (0.358)
Marriage	0.923 (0.266)	0.953 (0.213)	0.903 (0.295)	0.917 (0.275)
Age upon marriage	20.73 (4.960)	20.29 (6.353)	20.98 (5.062)	20.50 (6.330)
Divorce	0.010 (0.100)	0.006 (0.076)	0.008 (0.088)	0.015 (0.120)
Observations	797	696	1,927	1,791
<i>B: Spouse</i>				
Years of schooling	6.881 (3.702)	6.072 (3.750)	7.784 (3.899)	6.736 (3.911)
Labor-force participation	0.825 (0.381)	0.722 (0.448)	0.840 (0.367)	0.799 (0.401)
Ln (monthly earnings)	9.210 (0.968)	9.216 (0.970)	9.195 (1.018)	9.264 (0.985)
Observations	690	594	1,612	1,470

Notes: Standard deviations are presented in parentheses. The fertility measure is a woman's total number of live births until the census year. Log monthly earnings is measured in Israel shekels in census-year prices. Number of observations is presented for all variables except age upon marriage and log monthly earnings of spouse. Because data on these variables are lacking for some women in the sample, the corresponding number of observations is slightly lower

Table 2: Differences in Fertility and Schooling Trends between Treated and Control Localities for Pretreatment Cohorts, Age 14-23 in 1964

	Fertility	Education
	(1)	(2)
A. Cohort Dummies Model		
Treatment X Age 15	-0.414 (0.950) {0.530}	0.642 (1.131) {0.825}
Treatment X Age 16	0.354 (0.500) {0.487}	0.310 (0.715) {0.704}
Treatment X Age 17	-0.709 (0.521) {0.471}	1.453 (0.993) {0.745}*
Treatment X Age 18	-0.233 (0.599) {0.524}	-1.025 (0.615) {0.663}
Treatment X Age 19	-0.031 (0.815) {0.503}	0.450 (0.983) {0.653}
Treatment X Age 20	0.217 (0.550) {0.535}	-0.223 (0.700) {0.620}
Treatment X Age 21	-0.398 (0.789) {0.601}	1.140 (0.777) {0.757}
Treatment X Age 22	0.337 (0.695) {0.571}	-0.239 (0.820) {0.634}
Treatment X Age 23	-0.305 (0.822) {0.644}	0.367 (0.852) {0.753}
Treatment	0.887 (0.548)* {0.319}***	-1.706 (0.950)* {0.459}***
B. Linear Trend Model		
Time Trend	-0.241 (0.022)*** {0.025}***	0.290 (0.032)*** {0.033}***
Treatment X Time Trend	-0.014 (0.054) {0.047}	0.017 (0.065) {0.058}
Treatment	0.884 (0.462)* {0.324}***	-1.575 (0.665)** {0.356}***

Notes: Standard errors adjusted for clustering at the level of locality are presented in parentheses. Robust standard errors are presented in swivel parenthesis. The dependent variables are the fertility rate and years of schooling. Panel A reports the coefficient of a treatment status dummy and the coefficients of the interactions between treatment status and cohort dummies. Panel B reports the coefficients of a linear time trend variable, a treatment status dummy and an interaction between them. The additional regressors are cohort dummies. N=2,860. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.10.

Table 3: Pre- and Post-Treatment Differences in Education and Fertility Trends between Treated and Control Localities, Age 2-23

	Fertility	Education
	(1)	(2)
Time Trend, Age 14-23	-0.297 (0.020)*** {0.020}***	0.271 (0.029)*** {0.029}***
Time Trend, Age 2-13	-0.302 (0.015)*** {0.007}***	0.288 (0.015)*** {0.010}***
Treatment X Time Trend, Age 14-23	-0.010 (0.046) {0.037}	0.012 (0.055) {0.051}
Treatment X Time Trend, Age 2-13	-0.037 (0.019)* {0.013}***	0.045 (0.021)** {0.017}***
Treatment	0.861 (0.423)* {0.243}***	-1.530 (0.642)** {0.297}***
Constant	7.145 (0.324)*** {0.129}***	3.493 (0.533)*** {0.169}***

Notes: Standard errors adjusted for clustering at the level of locality are presented in parentheses. Robust standard errors are presented in swivel parenthesis. The dependent variables are the fertility rate (column 1) and female schooling (column 2). The table reports the coefficient of a treatment status dummy, the coefficients of time trend variables for age 14-23 and 2-13 and the coefficients of the interaction between treatment status and each of the time trend variables. N=9,059. *** = p-value <

Table 4: Estimated Effect of Female Education on Fertility: First Stage, Reduced Form, OLS and 2SLS Estimates

	Years of schooling		Fertility		Fertility	
	First Stage		Reduced form		OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
A. Experiment of interest: Cohorts aged 4-8 and 14-18 in 1964						
1983 census (N=4,226)	0.694 (0.534) {0.268}***	0.738 (0.560) {0.261}***	-0.533 (0.324) {0.174}***	-0.539 (0.340) {0.173}***	-0.240 (0.015)*** {0.010}***	-0.730 (0.523) {0.326}**
1995 census (N=3,798)	0.921 (0.406)** {0.277}***	1.018 (0.438)** {0.272}***	-0.651 (0.212)*** {0.208}***	-0.609 (0.211)*** {0.207}***	-0.119 (0.013)*** {0.011}***	-0.598 (0.332)* {0.252}**
1983 and 1995 Pooled census (N=8,024)	0.730 (0.416)* {0.190}***	0.781 (0.436)* {0.187}***	-0.575 (0.251)** {0.138}***	-0.551 (0.259)** {0.137}***	-0.180 (0.011)*** {0.007}***	-0.706 (0.494) {0.233}***
B. Experiment of interest: Cohorts aged 9-13 and 14-18 in 1964						
1983 census (N=3,553)	0.545 (0.397) {0.291}*	0.514 (0.378) {0.282}*	-0.346 (0.266) {0.193}*	-0.342 (0.260) {0.190}*	-0.134 (0.013)*** {0.011}***	-0.665 (0.522) {0.490}
1995 census (N=3,190)	0.533 (0.319) {0.297}*	0.575 (0.346) {0.292}**	-0.507 (0.180)*** {0.228}**	-0.465 (0.169)*** {0.226}**	-0.088 (0.013)*** {0.013}***	-0.808 (0.514) {0.548}
1983 and 1995 Pooled census (N=6,743)	0.457 (0.279) {0.205}**	0.472 (0.293) {0.20}**	-0.385 (0.171)** {0.149}***	-0.368 (0.175)** {0.148}**	-0.116 (0.009)*** {0.008}***	-0.780 (0.523) {0.433}*
C. Control experiment: Cohorts aged 14-18 and 19-23 in 1964						
1983 census (N=2,860)	0.028 (0.347) {0.302}	0.039 (0.375) {0.285}	-0.189 (0.236) {0.256}	-0.251 (0.221) {0.251}	-	-
1995 census (N=2,351)	-0.367 (0.385) {0.325}	-0.334 (0.367) {0.320}	-0.101 (0.283) {0.275}	-0.124 (0.277) {0.274}	-	-
1983 and 1995 Pooled census (N=5,211)	-0.159 (0.284) {0.216}	-0.123 (0.275) {0.209}	-0.125 (0.192) {0.186}	-0.172 (0.178) {0.184}		
Control variables						
Individual level religion dummy	Yes	Yes	Yes	Yes	Yes	Yes
Locality fixed effects	No	Yes	No	Yes	No	Yes

Notes: Standard errors adjusted for clustering at the level of locality are presented in parentheses. Robust standard errors are presented in swivel parenthesis. The religion dummy indicates Muslim or Christian. In the 1983 census data, columns (2), (3), (5), (6), (7) and (8) include cohort dummies. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.10.

Table 5: Estimated Effect of Access to Schooling on Female Own Educational Attainment (1995 census data)

	Sample			
	Means:	Experiment of	Experiment of	Control
	Treatment Group Cohort	interest:	interest:	experimnt:
	aged 4-8	cohorts aged	cohorts aged	cohorts aged
	(1)	(2)	(3)	(4)
5-8 years of schooling	0.868 (0.339)	0.128 (0.052)** {0.032}***	0.042 (0.043) {0.036}	-0.030 (0.048) {0.042}
Primary school	0.688 (0.464)	0.079 (0.046)* {0.033}**	0.006 (0.037) {0.036}	-0.052 (0.037) {0.038}
9-10 years of schooling	0.461 (0.499)	0.064 (0.040) {0.029}**	0.028 (0.030) {0.029}	-0.058 (0.031)* {0.028}**
Secondary school	0.254 (0.435)	0.012 (0.030) {0.025}	0.019 (0.026) {0.023}	-0.043 (0.018)** {0.021}**
Matriculation certificate	0.175 (0.380)	-0.003 (0.019) {0.022}	0.022 (0.018) {0.021}	-0.047 (0.019)** {0.018}***
Post-secondary diploma	0.094 (0.292)	0.013 (0.018) {0.017}	0.039 (0.018)** {0.017}**	-0.033 (0.016)** {0.014}**
Observations	785	3,798	3,190	2,351

Notes: Standard errors adjusted for clustering at the level of locality and year are presented in parentheses. Robust standard errors are presented in swivel parenthesis. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.10.

Table 6: Estimated Effect of Female Education on Fertility in Samples Stratified by Distance to Nearest School and by Size of Locality (1995 census data, sample of Cohorts aged 4-8 and 9-13 in 1964)

	Years of schooling	Fertility	Fertility
	First stage	Reduced form	2SLS
	(1)	(2)	(3)
Sample stratified by distance to nearest school			
<i>a. Distance to nearest school < 4 km</i>			
(N=4,809)	0.612	-0.426	-0.696
	(0.466)	(0.212)*	(0.650)
	{0.325}*	{0.268}	{0.574}
<i>b. Distance to nearest school >= 4 km</i>			
(N=4,896)	1.023	-0.694	-0.679
	(0.388)**	(0.218)***	(0.357)*
	{0.326}***	{0.248}***	{0.307}**

Notes: Standard errors adjusted for clustering at the level of locality are presented in parentheses. Robust standard errors are presented in swivel parenthesis. Control variables in each column include a religion dummy indicates Muslim or Christian, a cohort dummy for age 4-9, and locality fixed effects. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.10.

Table 7: Estimated Effect of Female Education on Fertility with Control for Access to Health Services using 1995 Census Data

	Years of schooling		Fertility		Fertility			
	First stage		Reduced form		OLS		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)

Experiment of interest: Cohorts aged 4-8 and 14-18 in 1964

(N=3,798)	1.046	0.992	-0.660	-0.663	-0.088	-0.100	-0.631	-0.669
	(0.398)**	(0.461)**	(0.220)***	(0.231)***	(0.015)***	(0.014)***	(0.336)*	(0.386)*
	{0.296}***	{0.287}***	{0.217}***	{0.211}***	{0.011}***	{0.011}***	(0.263)**	(0.278)**

Control variables

Individual level religion dummy	Yes							
Clinics dummy	Yes	No	Yes	No	Yes	No	Yes	No
Clinics dummy* cohort dummy	Yes							

Notes: Standard errors adjusted for clustering at the level of locality are presented in parentheses. Robust standard errors are presented in swivel parenthesis. The religion dummy indicates Muslim or Christian. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.10.

Table 8: Estimated Effect of Female Education on Fertility Based on Alternative Control Groups and Samples (1995 Census Data)

	Years of schooling		Fertility		Fertility	
	First stage		Reduced form		OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
A. Original control group excluding seven largest localities						
Experiment of interest:Cohorts aged 4-8 and 14-18 (N=2,550)	0.841 (0.437)* {0.321}***	0.953 (0.452)** {0.315}***	-0.828 (0.253)*** {0.234}***	-0.775 (0.249)*** {0.232}***	-0.106 (0.015)*** {0.013}***	-0.814 (0.513) {0.355}**
Experiment of interest:Cohorts aged 9-13 and 14-18 (N=2,118)	0.458 (0.369) {0.342}	0.475 (0.387) {0.336}	-0.585 (0.225)** {0.257}**	-0.556 (0.207)*** {0.255}**	-0.092 (0.017)*** {0.015}***	-1.169 (0.992) {0.949}
Control experiment:Cohorts aged 14-18 and 19-23 (N=1,577)	-0.667 (0.411) {0.365}*	-0.667 (0.407) {0.363}**	0.149 (0.268) {0.307}	0.151 (0.259) {0.306}	-	-
B. Control group includes only Arabs from mixed cities						
Experiment of interest:Cohorts aged 4-8 and 14-18 (N=1,751)	2.155 (0.344)*** {0.421}***	2.249 (0.352)*** {0.414}***	-1.094 (0.325)*** {0.276}***	-1.091 (0.316)*** {0.273}***	-0.131 (0.018)*** {0.016}***	-0.485 (0.153)*** {0.138}***
Experiment of interest:Cohorts aged 9-13 and 14-18 (N=1,476)	1.189 (0.291)*** {0.444}***	1.296 (0.289)*** {0.442}***	-0.617 (0.243)** {0.296}**	-0.667 (0.222)*** {0.292}**	-0.119 (0.018)*** {0.018}***	-0.515 (0.204)** {0.260}**
Control experiment:Cohorts aged 14-18 and 19-23 (N=1,065)	0.148 (0.364) {0.489}	0.053 (0.374) {0.482}	0.028 (0.225) {0.356}	0.101 (0.221) {0.354}	-	-
C. Sample of Table 5 restricted to persons born in current locality						
Experiment of interest:Cohorts aged 4-8 and 14-18 (N=2,729)	0.966 (0.457)** {0.314}***	1.092 (0.497)** {0.307}***	-0.720 (0.253)*** {0.242}***	-0.657 (0.245)*** {0.240}***	-0.140 (0.015)*** {0.013}***	-0.602 (0.344)* {0.266}**
Experiment of interest:Cohorts aged 9-13 and 14-18 (N=2,327)	0.505 (0.417) {0.341}	0.549 (0.446) {0.334}*	-0.572 (0.227)** {0.264}**	-0.523 (0.238)** {0.262}**	-0.096 (0.017)*** {0.015}***	-0.953 (0.828) {0.710}
Control experiment:Cohorts aged 14-18 and 19-23 (N=1,714)	-0.662 (0.471) {0.370}*	-0.672 (0.446) {0.366}**	0.074 (0.308) {0.316}	0.007 (0.299) {0.313}	-	-
Control variables						
Individual level religion dummy	Yes	Yes	Yes	Yes	Yes	Yes
Locality fixed effects	No	Yes	No	Yes	No	Yes

Notes: Standard errors adjusted for clustering at the level of locality are presented in parentheses. Robust standard errors are presented in swivel parenthesis. Individual characteristics include a religion dummy (Muslim or Christian). Experiments of interest: Cohorts aged 4-8 and 14-18 in 1964, Cohorts aged 9-13 and 14-18 in 1964. Control experiment:Cohorts aged 14-18 and 19-23 in 1964. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.10.

Table 9: OLS and 2SLS Estimates of the Effect of Education on Woman's Labor-Force Participation, Marriage, Age upon Marriage, Divorce, and Spouse's Outcomes

	Cohorts aged 4-8 and 14 -19 in 1964				Cohorts aged 9-13 and 14 -19 in 1964			
	1983 census		1995 census		1983 census		1995 census	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: own outcomes</i>								
Labor-force participation	0.032 (0.003)*** {0.002}***	-0.139 (0.136) {0.068}**	0.039 (0.003)*** {0.002}***	-0.040 (0.029) {0.029}	0.030 (0.002)*** {0.002}***	-0.035 (0.044) {0.062}	0.036 (0.003)*** {0.002}***	-0.007 (0.041) {0.045}
Marriage	-0.007 (0.003)** {0.002}***	0.055 (0.063) {0.037}	0.003 (0.002) {0.001}**	-0.011 (0.024) {0.022}	0.005 (0.001)*** {0.001}***	-0.068 (0.070) {0.062}	0.004 (0.002)** {0.001}***	-0.061 (0.055) {0.051}
Age upon marriage	0.115 (0.015)*** {0.014}***	-0.107 (0.274) {0.253}	0.216 (0.030)*** {0.024}***	0.506 (0.511) {0.502}	0.150 (0.020)*** {0.016}***	-0.091 (0.435) {0.344}	0.157 (0.024)*** {0.030}***	-0.490 (1.222) {1.105}
Divorce	-0.0002 (0.0002) {0.0002}	0.004 (0.006) {0.006}	-0.001 (0.0005)** {0.0005}***	-0.009 (0.008) {0.009}	-0.0004 (0.0002)** {0.0003}	0.002 (0.010) {0.009}	-0.001 (0.0004)* {0.0004}**	-0.028 (0.018) {0.020}
<i>Panel B: spouse outcomes</i>								
Years of schooling	0.498 (0.020)*** {0.017}***	0.579 (0.277)** {0.224}***	0.545 (0.018)*** {0.015}***	0.537 (0.337) {0.286}*	0.502 (0.020)*** {0.017}***	0.464 (0.256)** {0.288}	0.466 (0.019)*** {0.019}***	0.538 (0.503) {0.453}
Labor-force participation	0.007 (0.001)*** {0.001}***	0.006 (0.017) {0.017}	0.019 (0.001)*** {0.002}***	-0.018 (0.035) {0.037}	0.007 (0.001)*** {0.001}***	-0.019 (0.023) {0.025}	0.017 (0.002)*** {0.002}***	-0.007 (0.055) {0.060}
Ln (monthly earnings)	0.027 (0.003)*** {0.003}***	0.067 (0.055) {0.044}	0.034 (0.004)*** {0.003}***	-0.034 (0.045) {0.057}	0.033 (0.003)*** {0.003}***	0.092 (0.103) {0.079}	0.030 (0.004)*** {0.003}***	0.001 (0.102) {0.103}

Notes: Standard errors adjusted for clustering at the level of locality are presented in parentheses. Robust standard errors are presented in swivel parenthesis. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.10.

Table 10: Estimated Effect of Mother's Access to Schooling on Children's Education When Mother was Aged 18-26

	Years of schooling			Primary school			Secondary school			Academic degree		
	(1)	(2)	(3)	(3)	(4)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<i>I. Full Sample:</i>												
<i>A. Experiment of interest: Cohorts aged 4-13 and 14-18 in 1964</i>												
(N=8,127)	10.649	0.252	0.232	0.898	-0.007	-0.005	0.412	0.042	0.040	0.022	0.025	0.023
(N mothers=3,645)	(2.399)	(0.196)	(0.192)	(0.302)	(0.021)	(0.021)	(0.492)	(0.033)	(0.032)	(0.147)	(0.012)**	(0.012)**
		{0.152}*	{0.151}		{0.016}	{0.016}		{0.026}	{0.026}		{0.011}**	{0.011}**
<i>B. Control experiment : Cohorts aged 14-18 and 19-23 in 1964</i>												
(N=2,720)	11.170	-0.097	-0.196	0.893	0.032	0.027	0.643	-0.006	-0.012	0.086	-0.058	-0.056
(N mothers=1,449)	(3.091)	(0.382)	(0.371)	(0.309)	(0.035)	(0.035)	(0.479)	(0.054)	(0.052)	(0.280)	(0.036)	(0.036)
		{0.324}	{0.318}		{0.031}	{0.031}		{0.045}	{0.045}		{0.031}*	{0.031}*
<i>II. Sample: Boys Only</i>												
<i>C. Experiment of interest: Cohorts aged 4-13 and 14-18 in 1964</i>												
(N=4,600)	10.591	0.009	-0.009	0.897	-0.007	-0.003	0.415	0.055	0.052	0.022	0.010	0.009
(N mothers=2,140)	(2.397)	(0.219)	(0.214)	(0.304)	(0.024)	(0.024)	(0.493)	(0.039)	(0.038)	(0.148)	(0.015)	(0.015)
		{0.187}	{0.185}		{0.020}	{0.020}		{0.034}	{0.033}		{0.015}	{0.014}
<i>D. Control experiment : Cohorts aged 14-18 and 19-23 in 1964</i>												
(N=1,650)	11.313	-0.024	0.074	0.911	0.009	0.011	0.659	0.025	0.028	0.098	-0.050	-0.046
(N mothers=898)	(2.997)	(0.438)	(0.426)	(0.285)	(0.038)	(0.037)	(0.474)	(0.064)	(0.063)	(0.297)	(0.048)	(0.048)
		{0.395}	{0.390}		{0.035}	{0.035}		{0.058}	{0.058}		{0.045}	{0.045}
<i>III. Sample: Girls Only</i>												
<i>E. Experiment of interest: Cohorts aged 4-13 and 14-18 in 1964</i>												
(N=3,527)	10.725	0.666	0.679	0.900	-0.003	-0.005	0.407	0.030	0.037	0.022	0.048	0.045
(N mothers=1,505)	(2.400)	(0.303)**	(0.292)**	(0.300)	(0.033)	(0.033)	(0.491)	(0.049)	(0.048)	(0.145)	(0.018)***	(0.018)**
		{0.258}***	{0.254}***		{0.028}	{0.028}		{0.041}	{0.041}		{0.017}***	{0.018}***
<i>F. Control experiment : Cohorts aged 14-18 and 19-23 in 1964</i>												
(N=1,070)	10.950	-0.375	-0.643	0.865	0.058	0.055	0.618	-0.063	-0.082	0.068	-0.079	-0.079
(N mothers=551)	(3.221)	(0.563)	(0.557)	(0.341)	(0.063)	(0.064)	(0.486)	(0.080)	(0.078)	(0.252)	(0.040)**	(0.045)*
		{0.528}	{0.523}		{0.055}	{0.056}		{0.071}	{0.070}		{0.037}**	{0.0414}*
<i>Control variables</i>												
Individual characteristics		Yes	Yes		Yes	Yes		Yes	Yes		Yes	Yes
Locality fixed effects		No	Yes		No	Yes		No	Yes		No	Yes

Notes: Standard errors adjusted for clustering at the level of locality are presented in parentheses. Robust standard errors are presented in swivel parenthesis. Estimation based on 1995 census data. Individual characteristics include a religion dummy (Muslim or Christian) and a cohorts dummy in Panel A that indicates Cohorts aged 4-8 versus those aged 9-13. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.10.

Table 11: OLS and 2SLS Estimates of the Effect of Mother's Education on Schooling Attainment of Children Born When Their Mother Was Aged 18-26 (1995 census data)

	Experiment of interest: cohorts aged 4-13 and 14-18	
	OLS	2SLS
	(1)	(2)
Years of schooling	0.102 (0.015)*** {0.008}***	0.387 (0.353) {0.264}
Primary school	0.006 (0.001)*** {0.001}***	-0.009 (0.042) {0.028}
Secondary school	0.007 (0.002)*** {0.001}*	0.067 (0.062) {0.046}
Academic degree	0.001 (0.001)** {0.001}**	0.039 (0.032) {0.022}*

Notes: Standard errors adjusted for clustering at the level of locality are presented in parentheses. Robust standard errors are presented in swivel parenthesis. Sample includes 8,127 children born to 3,645 mothers. *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.10.

Table 12: Effect of Mother Education on Fertility with Controls for Potential Mechanisms, Using Data from 1974-75 Fertility Survey

Controls	Age Group		
	Cohorts aged 40-55	Cohorts aged 30-55	Cohorts aged 20-55
	(1)	(2)	(3)
I. Age dummies	-0.444 (0.028) [0.188]	-0.342 (0.017) [0.259]	-0.260 (0.012) [0.499]
II. I + Mechanisms	-0.285 (0.039) [0.401]	-0.215 (0.021) [0.453]	-0.156 (0.014) [0.630]
III. II + Personal Characteristics	-0.150 (0.044) [0.481]	-0.105 (0.022) [0.546]	-0.074 (0.014) [0.703]
IV. III + Family wealth	-0.149 (0.044) [0.501]	-0.105 (0.022) [0.554]	-0.072 (0.014) [0.708]
Observations	3,798	3,190	2,351

Notes: Standard errors are presented in parentheses. R-square of each regression is presented in square brackets. The mechanisms includes measures related to fertility preferences, contraceptives details, views about quantity versus quality of children, experience of child mortality, religiosity, role of women in family decision making, and women health knowledge and modernity. See table A4 for the detailed items that are included under these headings of mechanisms. The characteristics include husband's age, age of marriage, and current and past labor force participation, woman age of marriage, current and past labor force participation. The family wealth includes number of rooms, electricity, water and toilet at woman's home. and an index of family

Figure 1: Coefficients of the interaction of age in 1964 and access to schooling and the cohorts FE in the education equation

