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ABSTRACT

This paper studies the effect of mothers‘ education on fertility in a population with very low female labor force participation. The results we present are particularly relevant to many countries in the Muslim world where 70-80 percent of women are still out of the labor force. For identification we exploit the abrupt end of the military rule which greatly restricted the mobility of Arabs in Israel until the mid-1960's. This change improved access to schooling in communities that lacked schools and, as a consequence, significantly increased the education of affected cohorts, mainly of girls. The very large increase in schooling attainment triggered a sharp decline in completed fertility. We show that no other changes explain these findings and that the results are robust to checks against various threats to identification. We rule out convergence in fertility and schooling, changes in labor-force participation, age upon marriage, marriage and divorce rates, and spousal labor-force participation and earnings as mechanisms in this fertility decline. Spousal education increased however sharply through assortative matching and played a role in the fertility decline. We also show that the increase in mother‘s education was significantly and positively correlated with several potential mechanisms such as a reduction in the desired number of children, better knowledge and higher probability of using contraceptives, recognition that family size can compromise children quality, larger role for women in family decision making, less religiosity, and positive attitude towards modern health care and modernism in general.

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1. Introduction

In the 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 quality (Becker and Lewis, 1973; Willis, 1973). In these models, the link between education and fertility crucially depends on labor force participation. However, it appears that some societies have experienced a fertility transition without this mechanism playing a major role. 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 over those years; the respective rates were only 15 percent in 2000 and 18 percent in 2009.[2] Whether education plays a role in lowering fertility in the absence of the labor market mechanism is of great importance since in most of the Arab and Muslim world women are practically absent from the labor force. For example, the most recent World Bank statistics[3] 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. However, female education has increased to various degrees in all these countries and this change could have lowered fertility through other channels.[4]

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 particular, we present evidence that the strong negative relationship between women's fertility and education of Arab women in Israel reflects a causal effect. It shows that women's labor-force participation, as well as other potential mechanisms such as age upon marriage, marriage rates, and divorce rates, did

[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] http://data.worldbank.org/indicator/SL.TLF.CACT.FE.ZS
[4] The increase in education may impact women 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 contraception 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 (forthcoming, 2011) present an insightful summary of how education may affect fertility and children outcomes and discuss the related empirical evidence. However, there is little evidence of the importance of these channels in the absence of meaningful increases in women's employment and the opportunity cost of their time.

not play any role in this fertility decline. The impact of women‘s education remained very large after we accounted 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.

We base the evidence presented in this paper on a natural experiment that increased sharply the education of affected cohorts of children as a result of 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 to outside of a person‘s registered domicile.[5] The travel restrictions were revoked in October 1963 following unexpected political and government change.[6]

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.

[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 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.

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 her 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 her 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, leaving older cohorts to be used in controlled experiments. We used data from the 1983 and 1995 censuses. 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.

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 4–9 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 schooling and also of the completion of at least some years of secondary schooling. 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 induced a sharp decline in fertility, measured at 0.61 children in the younger affected cohorts and 0.47 children in the older cohorts. Implied 2SLS estimates show that a one-year increase in maternal schooling caused a 0.6-child decline in fertility. This fertility decline, however, was not accompanied by discernible changes in women‘s age upon marriage, divorce rate, labor-force participation, and spouse‘s employment, earnings, and age upon marriage. Spouse‘s education, however, did increase through assortative marriage matching, although not directly through the change in access to schooling, and therefore may have had an effect on 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. We also find that mother‘s education was highly correlated with other potential mechanisms, in particular a change in fertility preferences, changes in contraceptive details, a preferences shift towards quality children and reduced child and infant mortality, higher bargaining power of women as reflected in their larger role in family decisions, less religiosity, and positive attitude towards modern health care and modernism in general.

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. We support this assumption with broad range of evidence demonstrating that the removal of the travel restrictions did not have differential impacts on cohorts aside from their effects on education. For example, we show that the travel changes did not affect differentially the labor market opportunities, measured by the probability of working outside of the locality (after the movement restrictions had been removed), number of weeks of work, and wages and earnings, of the affected cohorts. We also present evidence that the changes did not affect differentially measures of family wealth and income. Regarding other potential confounding effects, we show that the removal of travel restrictions in late 1963 did not improve differentially access to services that may have affected fertility directly. For example, we demonstrate that the changes did not lead to differential improved access to healthcare services, particularly pre- and post-natal services that the state provided at special public well-baby centers and general clinics. Another identification concern that we rule out is that the treatment estimates may be biased due to a pre-existing control–treatment differential time trend in the fertility rate and female education. We use pre-reform data relating to the localities‘ mean fertility rate and years of schooling for cohorts aged 14–24 in 1964 and show that the treatment and control localities had similar fertility and female education time trends. We also show that our results are robust to various sensitivity and falsification tests.

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 Norway. Also in Norway, Monstad, Propper and Salvanes (2008) find that increases in education did not lead to decreased fertility but did lead to childbirth 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 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 evidence obviously suggests a lack of consensus regarding the causal effect of women‘s education on fertility. As we noted above, maternal education can affect fertility through many different 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 identify separately 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.

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, we discuss our identification strategy and present the results of our estimation of the effect of schooling on fertility in Section 4. In Section 5, we check the robustness of the results and discuss possible threats to our identification strategy. Section 6 concludes.

2. The 1948–1966 Military Government and Restricted Mobility of Arabs in Israel[7]

On 14 May 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 what the borders of the new state were. On the following day, 15 May, most of the remaining British troops departed and five Arab 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

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

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 Arab citizens were subject to military rule, those who lived in mixed Arab-Jewish cities such as Haifa and Jaffa enjoyed greater freedom than the 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. 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. Since Fouzi's 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; 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.

In Israel's first years 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 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

[8] Retrieved from a memoire website: http://www.Sochrot.org.index.php?id+164.

[9] The populations of five Arab villages adjacent to the frontier 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.

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

As noted above, Arabs who lived under military rule and were confined to specific geographic areas faced severe restrictions in their ability to travel in pursuit of educational and training opportunities and compete for better jobs in the labor market (Okun and Friedlander, 2005). This increased the cost of schooling for Arab children who resided in villages and towns that had no schools. Commuting to the nearest school was complicated due to the need for a travel permit and costly because of the longer travel time (passing checkpoints, etc.) and the financial cost of obtaining permits or enrolling in residential schools. Sometimes travel was also dangerous due, for example, to potential altercations with border police and soldiers at checkpoints and on the roads, changes in curfew, and so on. 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 had Arab primary schools; three of them also had Arab secondary schools. As noted above, however, the Arab populations of these cities were 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 school nor a secondary school. The control group includes all localities under military rule that had at least one primary school. 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.

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 treated 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. 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, which 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.[10] 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, marriage, 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 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 exclude 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

[10] For documentation, see the Israel Social Sciences Data Center web site: http://isdc.huji.ac.il/mainpage_e.html (data sets 115 [1995 demographic file] and 301 [1983 files]). 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.

not allowed to relocate and that on average this population tend to remain leaving in their village, town or city of birth. I 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.

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. For years of schooling and number of children in 1995, we used the midpoints in each range. 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.

Table 1 presents the 1983 and 1995 mean demographic and economic outcomes for two cohorts, those aged 14–19 and 19–24 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 in 1983 of the age 14–19 cohorts was 5.79 in the control group and 4.36 in the treated group. Mean fertility in the age 14–19 cohort in 1983 was 4.8 in the control group and 5.5 in the treated group, a difference of 0.7 children. In 1995, the same difference was 1.0, reflecting the gap in completed fertility. However, the gaps between treated group and control group based on the age 14–19 cohort strongly resemble the treatment–control differences based on the age 19–24 cohort. For example, mean years of schooling of the age 19–24 cohort in 1983 was 4.16 in the control group and 2.71 in the treated group; the difference, 1.44, is identical to the corresponding difference in the age 14–19 cohort. Also, the treatment–control difference for fertility in 1995 was 1.03 for the age 14–19 cohort and 1.10 for the 19–23 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 age 14–18. We expect children of primary-school or early secondary school age in 1964 to have benefited from the regaining of 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 lifting of the 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 in 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_{ilj}) of individual i, who resided in locality j and belonged to cohort l, and her exposure to the program:

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

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 the cohorts by 2-years cohorts and impose the same δ_l on each of the following age 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 groups 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. In Figure 1, the estimated coefficients are small, similar in size, and not statistically different from 0 for the 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 14 or older in 1964. The estimated δ_l then jumps to about 0.75 at age 12–13, reaches 1.0 at age 8–10, and remains at this level or higher for the youngest age cohorts, 2-6. The six estimates in the younger than 14 groups are significantly different from zero and more precisely estimated for cohorts age 9 and younger. In contrast, the average estimated coefficient for cohorts over 14 is about 0.02 and is not significantly different from zero.

The evidence presented in Figure 1 suggests, as expected, that the treatment had no effect on the education of cohorts older than 13 years in 1964 and had a positive effect on the education of younger cohorts. This shows 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.

4a.Simple Difference-in-Differences Estimates of Access to Schooling on Education

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 four 5-years cohorts. Two cohorts, those who were born in the periods 1955-1960 and 1950-1955, were young enough to be affected by the treatment, since their ages at the end of 1963 were 4-9 and 9-14, respectively. The other two, who were born in the years 1945-1950 and 1940-1945, were too old to be affected, as their ages were 14-19 and 19-24. At the time of the earlier census in June 1983, our youngest treated group was aged just over 23–28 years old, and the older treated cohort was 28–33. By the later census in November 1995, the youngest treated group was aged 36–41 and the oldest was aged 41–46. The unaffected cohorts were 33-38 and 38-43 in mid-1983 and 46-51 and 51-56 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.

We first present in Table 2 the means of years of schooling of the four cohorts by exposure to the regained access to schooling, which we use to analyze an uncontrolled difference in the differences estimates. In Panel A, we compare the schooling attainments of individuals in the control group (women aged 14–19 in 1964) with those of the women who were exposed the longest to treatment (aged 4–9 in 1964) in affected and unaffected areas. In both cohort groups, mean years of schooling were higher in areas not affected by the travel restrictions than elsewhere. Note that years of schooling increased in both treated and control areas but increased much more in localities included in the former group. For example, on the basis of the 1983 census data, average schooling in the treatment group increased from 4.4 years among the older group to 8.2 years among the younger group, a difference of 3.8 years of schooling. In the control group, average schooling increased from 5.8 years among the older group to 8.9 years among the younger group, a difference of 3 years of schooling. The exact difference of these differences amounts to a relative increase of 0.75 years of schooling in the treatment group, with a 0.279 standard error. Performing the same analysis on the basis of 1995 census data (shown in Columns 4–6 of Panel A), we obtain an increase of 1.078 years of schooling (SE=0.297).

Panel B of Table 2 presents a similar analysis for the older cohorts that were affected by regained access to schooling. The comparison group again comprises the cohorts closest in age that were hardly exposed to this change. The mean of years of schooling remains higher in areas that were not affected by regained access to schooling. As in the comparison presented in Panel A, years of schooling increased in both groups but more so in treated communities. However, the relative estimated gain on the basis of 1983 census data is only 0.49 year of schooling, about two-thirds of the corresponding average gain among the younger cohorts. The difference-in-differences (DID) estimate of the gain in schooling among the older cohort, on the basis of 1995 census data, is 0.605 year of schooling, again about two-thirds of the corresponding estimate for the younger affected age cohorts.

The two simple DID estimates presented above may be interpreted as the causal effect of treatment under the assumption that in the absence of the change towards free access to schooling the increase in years of schooling would not have been systematically different in affected and unaffected areas. This identification assumption should be checked because the pattern of increase in education may vary systematically across areas. For example, convergence may confound the estimated effect of interest. However, an implication of the identification assumption may be tested because the schooling of individuals aged 14 and above in 1964 cannot have been affected by the removal of the travel restrictions and the restoration of access to schooling in the Military Government regions. The increase in education among cohorts older than 14 in 1964 should not differ systematically across affected and unaffected areas. In Table 2, Panel C, we present one example of such a control experiment, in which we contrast cohorts aged 19–24 in 1964 with cohorts aged 14–19. The estimated DID is 0.026 (SE=0.344) year of schooling on the basis of 1983 census data, which is very small (and statistically insignificant), whereas based on the 1995 data it is even negative, –0.384 (0.374). We also analyzed a control experiment based on older cohorts and obtained similar results. These outcomes provide some suggestive evidence that in spite of the lower initial levels of education among the treatment localities, there was no tendency towards convergence that was responsible for greater improvements among the treated students, and thus the DID estimates presented in Panels A and B are not driven by inappropriate identification assumptions. In the next section, we present more precise results after conditioning the regression on individuals‘ religion and locality fixed effects.

Table 3 presents the elements of the DID estimates for the two treatment groups of the effect of access to schooling on the average number of children per woman. The treatment–control difference in number of children among the 4–9 age cohorts based on the 1983 census data is 0.122 (SE=0.07). The corresponding difference between treatment and control unaffected cohorts aged 14–19 is 0.677 (SE=0.166), implying a DID estimate of a decline of 0.555 (SE=0.155) child in the affected cohorts. Similarly, based on the 1995 census data, fertility declined in both treated and control areas but much more in the former. In the treated group, fertility declined from 6.049 children per women in the 14–19 age group in 1964 to 5.088 in the 9–14 age group and 4.115 for women in the 4–9 age group in 1964. In the control group, the respective fertility rates were 5.023, 4.606, and 3.816 children per women. The implied DID estimate of the effect of the removal of travel restrictions is –0.727 (SE=0.195) for women aged 4–9 in 1964 and –0.543 (SE=0.227) for women aged 9–14 in 1964. The changes estimated on the basis of the later census, naturally, are closely related to changes in completed fertility because the youngest treated cohort was over 36 years old by the census day in 1995.

The causal interpretation of the estimated decline in fertility due to the reopening of access to schooling is supported by the evidence of no change in fertility based on estimates from the control experiment presented in Panel C of Table 3. The DID estimate is –0.193 (SE=0.263) based on the 1983 census data and –0.092 (SE=0.285) on the basis of the 1995 census data; the two estimates have different signs and both are statistically insignificant, which supports the assumption that otherwise the decline in fertility would not have been systematically different in affected and unaffected areas, despite the greater initial levels of fertility in the treated localities. In the next section we show more evidence that this is indeed the case.

We may use the DID estimates of the change in education and fertility to compute a Wald estimate of the effect of mother's schooling on fertility. This estimate is obtained as computed for each affected cohort on the basis of the simple DID estimates of the first-stage and reduced-form relationships. For example, the Wald estimate based on the sample of the young affected cohorts in 1983 is –0.74 (–0.555 divided by 0.751) and for the old it is –0.57 (–0.279 divided by 0.490).

Testing for convergence

As suggested above, the DID estimates of the effect 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 indictor (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 1of Table 4a. 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=0.046). 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 treated and control localities that might confound the estimated treatment effects that we present below.

We also extended the time-trend analysis to show that 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 4a 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=0.033) in years of schooling. The estimated coefficient of the interaction of this trend with the treatment indicator is practically zero, 0.017 (SE=0.061). Overall, the estimates presented in column 2 are fully consistent with the evidence in Figure 1 for cohorts older than 13 in 1964.

Before moving to the controlled DID estimation, we present in Table 4b 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.

4b.Controlled DID Estimates of Access to Schooling on Education

The simple DID estimate may be generalized to 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) $S_{ilj} = \alpha + a_{ij} + \mu_l + (A_j\ Y_i)\ \delta + \varepsilon_{ilj}$

where S_{ilj} 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 1–3 in Table 5 present estimates of Equation (2) for three subsamples. In Panel A, we compare children aged 4–9 in 1964 with children aged 14–19 on the basis of the 1983 census data (first row) and the 1995 census (second row). In Column 1, we replicate for convenience of comparison the simple DID estimates presented in Table 2. Recall that this specification controls only for the cohort-of-birth dummy of the population aged 4–9 in 1964 and a dummy indicator for localities without schools until 1964. The treatment indicator is the interaction of these two variables, and its estimates show that treatment increased the education of female children aged 4–9 in 1964 by 0.751 year by 1983 and by 1.078 years by 1995. This interpretation relies on the identification assumption that there are no omitted time-varying and area-specific effects that correlate with the removal of travel restrictions. Column 2 presents estimates that add individuals' religion as control. The resulting conditional DID estimates are 0.694 by 1983 and 0.921 by 1995, only marginally lower than the uncontrolled DID estimate. Column 3 adds locality fixed effects as controls, eliciting DID estimates of 0.738 and 1.018 for 1983 and 1995, respectively—almost identical to the uncontrolled DID estimates in Column 1. Since the estimated standard errors hardly change when we add these controls, all three estimates are equally precise. The similarity of these three alternative estimates, especially the first and the third, is reassuring because they show that no local or regional effects that might confound the treatment effect of interest have been omitted.

Panel B of Table 5 shows the results of the cohort aged 9–14 in 1964; again, the control group is children aged 14–19 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 from the younger sample cohorts. The 1995 simple DID estimate, based on the later census, is 0.605, just over half as large as the corresponding estimate for the young cohorts. The controlled DID estimates presented in Columns 2–3 are 0.533 and 0.575, respectively. Again all three estimates are very similar, giving further evidence that omitted confounding factors do not affect our simple DID estimates.

Panel C of Table 5 presents the results of the control experiment based on comparing the 14–19 cohorts with those aged 19–24 in 1964. If education had increased faster in affected areas before the removal of the travel restrictions, Panel C would show positive coefficients (which can not reflect an actual treatment effect). The impact of this false ―treatment," however, is very small or even negative and never significant. Each coefficient in Panel C is statistically different from its corresponding coefficient in Panel A and from two of the corresponding estimates in Panel C. For example, the control-experiment estimate in Row 1 and Column 3 of panel C is 0.039 (SE=0.291), 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.

As noted in the data section, the age, education and the fertility variable 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 5. The results from the 1983 grouped data are identical to the 1983 results presented in Table 5 and are available from the authors.

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. Table A2 presents estimates of reduced-form Equation (2), in which the dependent variable is now a dummy indicator of the education level attained. We consider the following educational thresholds that individuals attained at least: 5–8 years of schooling, primary school (6 years of schooling), 9–10 years of schooling, secondary-school diploma (12 years of schooling), matriculation certificate, and post-secondary certificate. The estimated equation includes individual controls and locality fixed effects and is based on 1995 census data.

The first column of Table A2 presents the estimated reduced-form effect for the 4–9 age cohort. 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. 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 2 for the older affected cohort suggests that the gain for the 9–14 age group originated mainly in an increase in post-primary schooling, but these effects are not precisely measured. Column 3 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

Before presenting the results concerning the effect of mother‘s education on fertility, we should note that the travel-policy change may also have affected the education of Arab men. Appendix Table A3 presents results of the estimation of Equation (2) based on a pooled sample of men and women. The results, calculated for the 4–9 and 9–14 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 cohort, for example, the effect on women‘s schooling is 0.620 (SE=0.245) and that on men‘s schooling is 0.117 (SE=0.256).

The very small and insignificant effect on men‘s schooling as against the strong effect on women‘s schooling is not surprising for two reasons. First, we expect females‘ schooling investment to be much more sensitive to cost shocks due to its expected low return.[11] The strong effect on women and the near-absence of an effect on men is related to the expectation that women will not participate in the labor market and, therefore, will not earn a financial market return on their schooling. When the cost of schooling went up sharply because of the travel restrictions, parents might have preferred to keep girls at home and invest all their resources on the schooling of their sons, all of whom were expected to participate in the labor force and obtain a return on their education.

Second, in the context of a traditional Arab-Muslim society, travel restrictions are much more onerous for women than for men because alternative ways of accessing schooling, such as walking long distances daily or living with relatives or in residential schools, are less likely for girls than for boys. Of course, the personal danger related to travel under military rule and the risk of friction with soldiers and other security forces would affect girls more than boys, again especially in a religious Muslim community that often confines girls and women to home and does not permit them to travel alone. Interestingly, too, Gould, Lavy, and Paserman (2010) report that a low-quality childhood environment had a large negative effect only on the education of

[11] All it may takes to withdraw girls from schooling is a small increase in cost while for boys there is a large enough margin in the cost-benefit comparison of investment in education to absorb such changes without withdrawing them from schooling.

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.

4c.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. As for education, we can write an unrestricted reduced-form relationship between exposure to the travel-policy change and women‘s fertility women. Therefore, we estimate:

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

where F_{ilj} 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 5, Columns 4–6. Panel A compares the fertility of women who were aged 4–9 in 1964 with that of women aged 14–19 in 1964. In Column 4, the specification controls only for the interaction of a cohort of birth dummy and the population of the young cohort in 1964. Adding individuals‘ religion as control lowers the estimate to –0.533. When we add the locality fixed effects to the regression estimated, the estimate is practically unchanged. The estimates based on the 1995 census data and these three specifications are marginally higher than the estimates reported above. However, the 1995 reduced-form estimate based on the third specification (with individual controls and locality fixed effects) is –0.609 (SE=0.188), very similar to the corresponding 1983 estimate (–0.539). 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 5 presents DID estimates based on age 9–14 cohort 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 simple DID estimate is –0.279, the controlled DID estimate is –0.346, and the full DID estimate with locality fixed effect is –0.342 (SE=0.181). The latter estimate is 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) but 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 6 of Panel C, based on the 1995 census data is –0.124 (SE=0.271), not allowing us to reject that it is not statistically different from zero.[12]

4d.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) $$F_{ilj} = \alpha + l_{ij} + \mu_l + S_{ilj}\,\lambda + \varepsilon_{ilj}$$

where l_{ij} denotes locality-of-birth fixed effects, and λ is the marginal effect of education on fertility. Ordinary least-squares (OLS) estimates of the relationship between fertility and education may lead to biased estimates if there is a correlation between ε_{ilj} and S_{ilj}. However, under the assumptions that the cross-cohort differences in fertility would not have been systematically correlated with the removal of barriers to access to schools in the absence of the removal of travel restrictions in October 1963 and that this policy change had no direct effect on fertility, the interaction between belonging to young cohorts in 1964 and exposure to regained access to schooling in the locality of residence may be used as an instrument for Equation (4). This instrument has been shown to have good explanatory power in the first stage presented in Table 5.

[12] 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.

The 2SLS results of estimating λ are shown in Table 5—the OLS estimates in column 7 and the 2SLS results in column 8. The OLS estimate for the youngest affected cohort based on the 1983 data, presented in Row 1 of Panel A, is negative at –0.240 and very precisely measured (SE=0.009). The IV estimate is also negative, –0.730, and significantly different from zero and larger than the OLS estimate. This suggests that the OLS estimate is upward-biased, implying less sensitivity of fertility to changes in mothers' education. 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, 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.

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 6 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 4–9 in 1964 and their husbands were older than 8 in that same year; it includes 60% of the full sample of women. In Table A3 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 6 is 0.683 (SE=0.312), very similar to the estimate based on the full sample of women in these age cohorts (0.598, SE=0.238). It is also reassuring to note that the first-stage and reduced form effects reported in Table 6 are also almost identical to their corresponding estimates in Table 5. 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 5. 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.

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

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. To test this prediction, we divided the treated localities into two groups differentiated by 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 4–9 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 7. 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=0.329 of the effect of the travel-policy change in 1963 on schooling as reflecting a decline in the cost of attending school.

To check whether the differences in first-stage and reduced-form effects by distance to nearest school do not reflect some other heterogeneity, Panels B and C of Table 7 presents evidence based on stratification of the sample by size of locality. In Panel B, the treatment group is divided into small and large localities while the full control group is used; Panel C 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 C are even identical, at –0.683 and –0.686, respectively.

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 bias), 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 this direction of bias in the OLS estimate is that we are a estimating a LATE and that 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 natural experiment was primarily among students who otherwise wouldn't have completed primary school. It is reasonably possible that an increase in the lower levels of education (say, 5 to 6 years) is much more effective in reducing fertility than in the higher level of schooling (say, 10 to 11). Since the treated localities initially had lower levels of education, this can explain why the LATE is different from the OLS estimate. Finally, potential measurement error in the schooling variable may have biased the OLS estimate downward, a bias 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 downward bias in the 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.[13]

4f. 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 upon 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 outcomes and the labor-market outcomes of the spouse. These results, presented in Table 8, 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.

[13] 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.

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 therefore practically not different from zero with the exception of the estimated effect on the young age group in the 1983 census which is -0.139 with a standard error of 0.070. 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 *ab initio* 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 upon marriage, are positive and highly significant but the IV estimates show no such relationship in either outcome. The estimated effects of education on these two outcomes is relatively small,[14] 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 8 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 4–9 age cohorts may have been affected by the annulment of the travel restrictions whereas the spouses of those in the affected older age cohort (9–14) 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 A3), 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

[14] 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.

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 also traces to better schooling on the part of their husbands. 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.

Finally, we note that while the OLS effects of mother's schooling on spouse's labor-force participation and earnings (Table 8) are positive and significant for both affected cohorts in both census datasets, 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.

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.[15] 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 natural experiment we used in this paper. However, the rich information the survey provides allows relating the potential mechanisms to level of schooling of women in regressions with different number of controls. We than regress mother schooling on number of children and examine how the estimate of this coefficient changes as we add as controls measures of potential mechanisms. This strategy does not amount to a clean identification of mechanisms. Nevertheless, as will be shown below, mother schooling is highly correlated with almost all of the potential mechanisms and the estimate of mother education in the fertility equation is eroded substantially when these potential channels of effect are added.

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

We grouped questionnaire items under the following six mechanisms: Fertility preferences[16], Contraceptive details[17], Beliefs about the effect of family size on quality of children and about gender differences in schooling investment[18], Child mortality[19], Religiosity[20], Role of women in family decision making[21], Health knowledge and modernism[22].

Table A4 presents the estimated coefficients of mother and father 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 husband and wife are currently working and indicators of whether they have ever worked. In the third specification we add a measure of standard of living, and number rooms, and availability of electricity, running water and toilet in the woman home. The parameters in Table A4 suggest that mother 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 mother schooling are much larger than those of father schooling. The latter are often small and not significantly different from zero.

In Table 9 we report the estimated coefficients of mother schooling in a fertility equation in four 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 mother schooling from age 40-55 sample and the first specification (only mother‘s age included as a control) is -0.444. Note that this OLS estimate is much higher than the OLS estimate reported in Table 5 and it is much closer to the IV estimates reported in that table. Adding to the regression as controls all the measures of potential mechanisms reduces the coefficient of mother schooling to -0.285, a decline of 36 percent from -0.444. The R^2 is on the other hand goes up from 0.188 to 0.401. This is evidence that more than a third of the correlation between mother‘s education and

[16]Measured by these items: *respondent considered desired number of children; number of children desired; it is important to have at least one son; it is important to have at least one daughter*.

[17] Measured by these items: *respondent consulted with a doctor about birth control; consulted with anyone else about birth control; ever done anything to prevent pregnancy; used a particular contraceptive method to prevent pregnancy (birth control pills, I.U.D, condom, diaphragm, coitus interruptus, rhythm method), knows how to prevent pregnancy*.

[18] Measured by these items: *limiting the number of children influences chances of their advance in life; should 14 years old boy continue studying; should 14 years old girls continue studying.*

[19] Measured by these items: *ever experienced child mortality; number of children who died; number of miscarriages and abortions*.

[20] Measured by these items: *degree of religiosity in current home; respondents' observe religious laws and rituals*.

[21] Measured by these items*: who decides on daily expenses, who decided on large expenses; who decided on children's education; who decided on shopping for children*.

[22] Measured by these items: *what are the causes of sickness? woman wears traditional or religious clothes).*

fertility operates through these mechanisms.[23] 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.

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 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 lifting the travel restrictions and here we add more related evidence based on data from the 1972 and 1983 censuses.

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 A5, 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

[23] Adding the personal characteristics (husband's age, age of marriage, and current and past labor force participation, woman age of marriage, current and past labor force participation as controls reduced further the estimate of mother‘s schooling to -0.150 but it does not change further when the 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.

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 travel changes did not affect the labor market opportunities of adults.

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 at home of electricity, running water and toilet, and log of family income.[24] The cohorts of age 12-21 in 1972 are our treatment group (those who were of age 4-13 in 1964) and the cohorts of age 22-26 in 1972 are our control group (individuals who were of age 14-19 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 A6. 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

[24] 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 to in Israel. The 1972 census data includes other measures 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.

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 24 when the restrictions were removed, and so almost all of them had access to these services while giving births. This evidence are 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 0.41and 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 decline 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 10 presents evidence based on adding to the regression a control for localities that had a general health clinic. In the first specification, we include a main effect for this control and its interaction with the cohort dummy variable. In the second specification, we include only the main effect of clinic availability. Although neither specification includes locality fixed effects, our earlier results showed that these controls did not affect the treatment point estimates in any way. The results presented in the table are based on data for the 4–8 age cohort and the 1995 census data. The first-stage, reduced form, the OLS and 2SLS estimates presented in Table 10 are almost identical to those in Table 5. The corresponding results that we obtained using the 9–13 age cohort are identical to those in Table 5; 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.[25] Yet, it is it important to note again that even if the cancelation of travel restrictions created access to other services such as healthcare services, these services cannot threaten the identification strategy in this paper unless they affected young mothers (cohorts aged 13 or younger in 1964) differently than slightly older mothers (aged 14 and above in 1964). This is very unlikely because there is a large overlap in the time periods at which women in both cohorts gave birth, particularly in the 9–13 and 14–18 age groups. For this reason, women in both groups would most likely have experienced the same improvement in pre-natal and general healthcare as well as in family planning and contraceptive information.

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 alleviate this concern. The evidence presented in this section is a further 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 seven and not more localities due to sample-size considerations. The results of excluding the largest five or largest eight localities, however, are very similar to those obtained after the exclusion of seven. 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 A7, which present the mean characteristics of this control group. For example, the control-treatment difference in fertility rate among those in the 19–24 age cohort

[25] 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.

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 19–24) are much better than those of the treatment group. (See Columns 5–8 in Table A7)

The results based on these two alternative comparison groups, presented in the first two panels of Table 11 and based on the youngest affected cohorts only (ages 4–8) and on 1995 census data, strongly resemble those reported in Table 5. The first panel reports estimates when the control group is the original less the observations from the largest seven localities, causing the sample of the control group to fall by about half. The first-stage effect is 1.171, the reduced-form effect is –0.705, and the 2SLS estimate is –0.602 (SE=0.251), remarkably similar to the estimate obtained based on the original control group (–0.598, SE=0.238). Note that the corresponding OLS estimate is lower than that reported in Table 5. This is expected because the population eliminated from the control group (that of the largest towns) is better educated and also has fewer children. Since the latter characteristic may trace to reasons other than education, the OLS estimate become less negative when this group is excluded from the sample. Panel B in Table 11 reports estimates when the control group is the Arab population of the mixed cities. The 2SLS estimate is –0.485 (SE=0.140) whereas that in Table 5 is –0.598 (SE=0.238).

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 because of convergence due to differential time trends.

Panel C in Table 11 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 5. For example, the 2SLS estimate in Table 11 is –0.602 (SE=0.258) while that in Table 5 is –0.598. This result is not surprising because 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 as a comparison group the Jewish population of towns and small cities in the geographical region of the Arab treated localities. 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 5: based on 1995 census data, the 2SLS estimates were –0.494 (SE=0.052) for the 4–9 age cohort and –0.585 (SE=0.176) for the 9–14 age cohort.

6. 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. The evidence about the effect of women‘s education on fertility in general is mixed and inconclusive, and there is even less related evidence for populations where women are practically absent from the labor market. We extend this literature in a few directions by making several unique contributions. The policy change/natural experiment that we used pointed to a large change in women‘s education: a gain of over a year of schooling among affected children who were young enough to have benefited from the opening of access to primary schools. This is a large enough change to allow us to determine precisely its effect on fertility, which turns out to be positive and large; it explains some of the dramatic decline in the fertility of Israel‘s Arab-Muslim population which was correlated with increasing levels of women's education.

To justify our identification strategy we provide evidence that makes it seem very unlikely that the effect of education on fertility that we estimated merely reflects other changes that impacted the fertility of our treatment group differentially. In particular, we show that the travel changes did not affect the labor market opportunities of adults, did not lead to a change in the proportion of men and women working outside the locality when the students came to labor age, which was after the restrictions had been removed, and did not affect differentially measures of family wealth and income of affected and unaffected cohorts. Similarly, 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. We also show that our results are robust to various sensitivity and falsification tests.

A central feature of the paper is that the estimated effect of education on fertility does not operate through the opportunity cost of mother‘s time. Since this result is derived from a population-based sample of mostly Muslim women with very low levels of labor force participation, they provide a desired dimension of external validity for many other settings of Muslim countries in the Middle East and Asia where female labor force participations is still very low. We also find very little change over this period in other demographic variables such as marriage, age upon marriage, and divorce, but we do find evidence that the increase in women‘s education led to an increase in spousal education through assortative mating. We also provide evidence on other mechanisms and show that education of Arab women in Israel is highly correlated with changes in fertility preferences, knowledge and ability to process information about fertility options, healthy pregnancy behaviors, contraception options, less religiosity, more modern attitudes, and larger role for women in household decision making which might signal an increase in wife‘s bargaining power inside the marriage.

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

	Treatment				Control			
	1983 census		1995 census		1983 census		1995 census	
	Age in 1964		Age in 1964		Age in 1964		Age in 1964	
	14-19	19-24	14-19	19-24	14-19	19-24	14-19	19-24
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Women								
Years of schooling	4.364	2.709	3.935	2.864	5.791	4.162	5.864	4.409
	(4.186)	(3.540)	(3.651)	(3.468)	(4.359)	(4.164)	(4.388)	(4.105)
Fertility	5.486	6.869	6.049	6.763	4.809	5.999	5.023	5.646
	(2.911)	(3.539)	(3.016)	(3.082)	(2.875)	(3.437)	(3.009)	(3.262)
Labor-force participation	0.155	0.108	0.081	0.040	0.152	0.151	0.180	0.151
	(0.362)	(0.311)	(0.273)	(0.197)	(0.359)	(0.358)	(0.385)	(0.358)
Marriage	0.920	0.947	0.927	0.960	0.893	0.919	0.915	0.916
	(0.271)	(0.224)	(0.260)	(0.197)	(0.309)	(0.274)	(0.278)	(0.278)
Age upon marriage	20.45	20.86	21.06	19.53	20.53	20.94	21.48	19.92
	(3.944)	(4.658)	(5.922)	(8.027)	(3.949)	(4.587)	(6.060)	(8.062)
Divorce	0.002	0.005	0.019	0.007	0.008	0.014	0.008	0.015
	(0.048)	(0.071)	(0.136)	(0.082)	(0.088)	(0.117)	(0.088)	(0.123)
Observations	426	398	371	298	1,029	1,007	898	784
B: Spouse								
Years of schooling	7.366	6.125	6.279	5.991	8.063	6.745	7.442	6.723
	(3.778)	(3.611)	(3.520)	(3.961)	(3.881)	(3.902)	(3.897)	(3.926)
Labor-force participation	0.924	0.816	0.701	0.579	0.919	0.886	0.743	0.672
	(0.265)	(0.388)	(0.458)	(0.495)	(0.273)	(0.318)	(0.437)	(0.470)
Ln (monthly earnings)	9.783	9.755	8.193	8.163	9.876	9.811	8.186	8.152
	(0.625)	(0.639)	(0.535)	(0.551)	(0.616)	(0.616)	(0.550)	(0.567)
Observations	382	359	308	235	887	870	725	600

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: Simple Difference-in-Differences Estimates of the Effect of Access to Schooling on Female Education of Affected Cohorts

	1983 census			1995 census		
	Treatment	Control	Difference	Treatment	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: experiment of interest						
Cohorts aged 4-9 in 1964	8.178	8.854	-0.676	8.032	8.883	-0.851
	(0.130)	(0.084)	(0.155)	(0.137)	(0.092)	(0.164)
Cohorts aged 14-19 in 1964	4.364	5.791	-1.427	3.935	5.864	-1.928
	(0.209)	(0.134)	(0.248)	(0.217)	(0.140)	(0.258)
Difference	3.814	3.063	0.751	4.097	3.019	1.078
	(0.225)	(0.154)	(0.279)	(0.238)	(0.165)	(0.297)
Panel B: experiment of interest						
Cohorts aged 9-14 in 1964	6.317	7.253	-0.937	5.869	7.193	-1.324
	(0.162)	(0.107)	(0.195)	(0.165)	(0.108)	(0.197)
Cohorts aged 14-19 in 1964	4.364	5.791	-1.427	3.935	5.864	-1.928
	(0.209)	(0.134)	(0.248)	(0.217)	(0.140)	(0.258)
Difference	1.953	1.462	0.490	1.934	1.329	0.605
	(0.260)	(0.171)	(0.313)	(0.262)	(0.177)	(0.321)
Panel C: control experiment						
Cohorts aged 14-19 in 1964	4.364	5.791	-1.427	3.935	5.864	-1.928
	(0.209)	(0.134)	(0.248)	(0.217)	(0.140)	(0.258)
Cohorts aged 19-24 in 1964	2.709	4.162	-1.453	2.864	4.409	-1.545
	(0.200)	(0.126)	(0.237)	(0.228)	(0.141)	(0.268)
Difference	1.655	1.629	0.026	1.071	1.455	-0.384
	(0.271)	(0.189)	(0.344)	(0.278)	(0.208)	(0.374)

Notes: Standard errors are presented in parentheses.

Number of observations for 1983 census data: Panel A: 4,226; Panel B: 3,553; Panel C: 2,860.

Number of observations for 1995 census data: Panel A: 3,798; Panel B: 3,190; Panel C: 2,351.

Table 3: Simple Difference-in-Differences Estimates of the Effect of Access to Schooling on Fertility of Affected Cohorts

	1983 census			1995 census		
	Treatment	Control	Difference	Treatment	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: experiment of interest						
Cohorts aged 4-9 in 1964	1.769	1.647	0.122	4.115	3.816	0.298
	(0.059)	(0.038)	(0.071)	(0.084)	(0.056)	(0.101)
Cohorts aged 14-19 in 1964	5.486	4.809	0.677	6.049	5.023	1.025
	(0.140)	(0.090)	(0.166)	(0.156)	(0.100)	(0.186)
Difference	-3.717	-3.162	-0.555	-1.934	-1.207	-0.727
	(0.130)	(0.084)	(0.155)	(0.166)	(0.105)	(0.195)
Panel B: experiment of interest						
Cohorts aged 9-14 in 1964	3.840	3.442	0.398	5.088	4.606	0.482
	(0.092)	(0.061)	(0.110)	(0.115)	(0.075)	(0.138)
Cohorts aged 14-19 in 1964	5.486	4.809	0.677	6.049	5.023	1.025
	(0.140)	(0.090)	(0.166)	(0.156)	(0.100)	(0.186)
Difference	-1.646	-1.367	-0.279	-0.960	-0.417	-0.543
	(0.164)	(0.104)	(0.192)	(0.198)	(0.122)	(0.227)
Panel C: control experiment						
Cohorts aged 14-19 in 1964	5.486	4.809	0.677	6.049	5.023	1.025
	(0.140)	(0.090)	(0.166)	(0.156)	(0.100)	(0.186)
Cohorts aged 19-24 in 1964	6.869	5.999	0.870	6.763	5.646	1.117
	(0.174)	(0.109)	(0.205)	(0.186)	(0.115)	(0.219)
Difference	-1.383	-1.190	-0.193	-0.715	-0.623	-0.092
	(0.225)	(0.140)	(0.263)	(0.237)	(0.153)	(0.285)

Notes: Standard errors are presented in parentheses.

Number of observations for 1983 census data: Panel A: 4,226; Panel B: 3,553; Panel C: 2,860.

Number of observations for 1995 census data: Panel A: 3,798; Panel B: 3,190; Panel C: 2,351.

Table 4a: 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	0.642
	(0.579)	(0.757)
Treatment X Age 16	0.354	0.310
	(0.540)	(0.707)
Treatment X Age 17	-0.709	1.453
	(0.555)	(0.726)
Treatment X Age 18	-0.233	-1.025
	(0.551)	(0.720)
Treatment X Age 19	-0.031	0.450
	(0.529)	(0.692)
Treatment X Age 20	0.217	-0.223
	(0.517)	(0.677)
Treatment X Age 21	-0.398	1.140
	(0.571)	(0.747)
Treatment X Age 22	0.337	-0.239
	(0.557)	(0.728)
Treatment X Age 23	-0.305	0.367
	(0.604)	(0.790)
Treatment	0.887	-1.706
	(0.356)	(0.466)
B. Linear Trend Model		
Time Trend	-0.241	0.290
	(0.025)	(0.033)
Treatment X Time Trend	-0.014	0.017
	(0.046)	(0.061)
Treatment	0.884	-1.575
	(0.296)	(0.388)

Notes: Standard errors are presented in parenthesis. The dependent variables are the fertility rate and years of schooling. Panel A reports the coefficients of a linear time trend variable, a treatment status dummy and an interaction between them. Panel B reports the coefficient of a treatment status dummy and the coefficients of the interactions between treatment status and cohort dummies. The additional regressors are cohort dummies. N=2,860.

Table 4b: Pre- and Post-Treatment Differences in Education and

	Fertility	Education
	(1)	(1)
Time Trend, Age 14-23	-0.297	0.271
	(0.016)	(0.027)
Time Trend, Age 2-13	-0.302	0.288
	(0.006)	(0.010)
Treatment X Time Trend, Age 14-23	-0.010	0.012
	(0.029)	(0.049)
Treatment X Time Trend, Age 2-13	-0.037	0.045
	(0.011)	(0.018)
Treatment	0.861	-1.530
	(0.179)	(0.300)
Constant	7.145	3.493
	(0.096)	(0.161)

Notes: Standard errors are presented in 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 coeficients 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.

Table 5: 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)	(7)	(8)
A. Experiment of interest:Cohorts aged 4-9 and 14-19 in 1964								
1983 census	0.751	0.694	0.738	-0.555	-0.533	-0.539	-0.240	-0.730
(N=4,226)	(0.279)	(0.262)	(0.257)	(0.155)	(0.147)	(0.147)	(0.009)	(0.303)
1995 census	1.078	0.921	1.018	-0.727	-0.651	-0.609	-0.119	-0.598
(N=3,798)	(0.297)	(0.283)	(0.276)	(0.195)	(0.190)	(0.188)	(0.010)	(0.238)
B. Experiment of interest:Cohorts aged 9-14 and 14-19 in 1964								
1983 census	0.490	0.545	0.514	-0.279	-0.346	-0.342	-0.134	-0.665
(N=3,553)	(0.313)	(0.289)	(0.283)	(0.192)	(0.183)	(0.181)	(0.011)	(0.480)
1995 census	0.605	0.533	0.575	-0.543	-0.507	-0.465	-0.088	-0.808
(N=3,190)	(0.321)	(0.300)	(0.293)	(0.227)	(0.220)	(0.218)	(0.013)	(0.536)
C. Control experiment:Cohorts aged 14-19 and 19-24 in 1964								
1983 census	0.026	0.028	0.039	-0.193	-0.189	-0.251	-	-
(N=2,860)	(0.344)	(0.305)	(0.291)	(0.263)	(0.250)	(0.246)		
1995 census	-0.384	-0.367	-0.334	-0.092	-0.101	-0.124	-	-
(N=2,351)	(0.374)	(0.342)	(0.335)	(0.285)	(0.273)	(0.271)		
Control variables								
Individual level religion dummy	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Locality fixed effects	No	No	Yes	No	No	Yes	No	Yes

Notes: Standard errors are presented in parentheses. The religion dummy indicates Muslim or Christian. In the 1983 census data, columns (2), (3), (5), (6), (7) and (8) include cohort dummies.

Table 6: Estimated Effect of Female Education on Fertility: First Stage, Reduced Form, OLS and 2SLS Estimates, Sample of Women Married to Older Spouses

	Years of schooling			Fertility			Fertility	
	First stage			Reduced form			OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Spouse older than 8 in 1964								
a. Experiment of interest:Cohorts aged 4-9 and 14-19 in 1964								
1995 census	1.006	0.934	0.889	-0.670	-0.631	-0.607	-0.165	-0.683
(N=2,239)	(0.365)	(0.345)	(0.340)	(0.212)	(0.202)	(0.201)	(0.011)	(0.312)
b. Experiment of interest:Cohorts aged 14-19 and 19-24 in 1964								
1995 census	-0.541	-0.477	-0.439	0.202	0.171	0.142	-	-
(N=1,856)	(0.418)	(0.379)	(0.375)	(0.281)	(0.267)	(0.267)		
B. Spouse older than 13 in 1964								
a. Experiment of interest:Cohorts aged 4-9 and 14-19 in 1964								
1995 census	0.800	0.746	0.793	-0.569	-0.542	-0.594	-0.155	-0.749
(N=1,338)	(0.544)	(0.506)	(0.503)	(0.339)	(0.323)	(0.324)	(0.016)	(0.573)
b. Experiment of interest:Cohorts aged 14-19 and 19-24 in 1964								
1995 census	-0.650	-0.574	-0.522	0.360	0.323	0.305	-	-
(N=1,785)	(0.425)	(0.385)	(0.381)	(0.286)	(0.272)	(0.271)		
Control variables								
Individual level religion dummy	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Locality fixed effects	No	No	Yes	No	No	Yes	No	Yes

Notes: Standard errors are presented in parentheses. The religion dummy indicates Muslim or Christian. In the 1983 census data, columns (2), (3), (5), (6) and (8) include cohort dummies.

Table 7: 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 4-9 and 9-14 in 1964)

	Years of schooling	Fertility	Fertility
	First stage	Reduced form	2SLS
	(1)	(2)	(3)
A. Sample stratified by distance to nearest school			
a. Distance to nearest school < 4 km			
(N=4,809)	0.612	-0.426	-0.696
	(0.333)	(0.232)	(0.518)
b. Distance to nearest school >= 4 km			
(N=4,896)	1.023	-0.694	-0.679
	(0.329)	(0.228)	(0.300)
B. Control-group sample stratified by size of locality			
a. Larger localities			
(N=4,831)	0.891	-0.675	-0.758
	(0.334)	(0.232)	(0.518)
b. Smaller localities			
(N=4,874)	0.759	-0.452	-0.596
	(0.328)	(0.230)	(0.383)
C. Treatment and control samples stratified by size of locality			
a. Larger localities			
(N=2,911)	0.782	-0.536	-0.686
	(0.355)	(0.250)	(0.433)
b. Smaller localities			
(N=2,808)	0.886	-0.605	-0.683
	(0.160)	(0.253)	(0.387)

Notes: Standard errors are presented in parentheses. Control variables in each column include a religion dummy indicates Muslim or Christian, a cohort dummy for age 4-9, and locality fixed effects.

Table 8: 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 4-9 and 14 -19 in 1964				Cohorts aged 9-14 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)
Panel A: own outcomes								
Labor-force participation	0.032	-0.139	0.039	-0.040	0.030	-0.035	0.036	-0.007
	(0.001)	(0.070)	(0.001)	(0.034)	(0.001)	(0.063)	(0.002)	(0.051)
Marriage	-0.007	0.055	0.003	-0.011	0.005	-0.068	0.004	-0.061
	(0.002)	(0.042)	(0.001)	(0.024)	(0.001)	(0.063)	(0.001)	(0.051)
Age upon marriage	0.115	-0.107	0.216	0.506	0.150	-0.091	0.157	-0.490
	(0.014)	(0.230)	(0.023)	(0.472)	(0.016)	(0.331)	(0.028)	(1.084)
Divorce	-0.000	0.004	-0.001	-0.009	-0.000	0.002	-0.001	-0.028
	(0.000)	(0.006)	(0.000)	(0.008)	(0.000)	(0.011)	(0.000)	(0.020)
Panel B: spouse outcomes								
Years of schooling	0.498	0.579	0.545	0.537	0.502	0.464	0.466	0.538
	(0.014)	(0.223)	(0.015)	(0.283)	(0.015)	(0.285)	(0.017)	(0.449)
Labor-force participation	0.007	0.006	0.019	-0.018	0.007	-0.019	0.017	-0.007
	(0.001)	(0.017)	(0.002)	(0.033)	(0.001)	(0.026)	(0.002)	(0.056)
Ln (monthly earnings)	0.027	0.067	0.034	-0.034	0.033	0.092	0.030	0.001
	(0.003)	(0.042)	(0.003)	(0.058)	(0.003)	(0.076)	(0.003)	(0.102)

Notes: Standard errors are presented in parentheses.

Table 9: Effect of Mother Education on Fertilitiy with Controls for Potential Mechanisms, Using Data from 1974-75 Fertility Survey

	Age Group		
	Cohorts aged 40-55	Cohorts aged 30-55	Cohorts aged 20-55
Controls	(1)	(2)	(3)
I. Age dummies	-0.444	-0.342	-0.260
	(0.028)	(0.017)	(0.012)
	[0.188]	[0.259]	[0.499]
II. I + Mechanisms	-0.285	-0.215	-0.156
	(0.039)	(0.021)	(0.014)
	[0.401]	[0.453]	[0.630]
III. II + Personal Characteristics	-0.150	-0.105	-0.074
	(0.044)	(0.022)	(0.014)
	[0.481]	[0.546]	[0.703]
IV. III + Famliy wealth	-0.149	-0.105	-0.072
	(0.044)	(0.022)	(0.014)
	[0.501]	[0.554]	[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 mechansims includes measures related to fertility preferences, contraceptives details, views about quantity versus quality of children, expereince of child mortality, religiousity, 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 tokilet at woman's home. and an index of family standard of living.

Table 10: 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 4-9 and 14-19 in 1964								
	1.046	0.992	-0.660	-0.663	-0.088	-0.100	-0.631	-0.669
(N=3,798)	(0.296)	(0.289)	(0.199)	(0.194)	(0.011)	(0.010)	(0.250)	(0.265)
Control variables								
Individual level religion dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clinics dummy	Yes	No	Yes	No	Yes	No	Yes	No
Clinics dummy* cohort dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are presented in parentheses. The religion dummy indicates Muslim or Christian..

Table 11: 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)	(7)	(8)
A. Original control group excluding seven largest localities								
Experiment of interest:Cohorts aged 4-9 and 14-19	1.245	1.011	1.171	-0.856	-0.734	-0.705	-0.100	-0.602
(N=2,283)	(0.353)	(0.338)	(0.328)	(0.232)	(0.226)	(0.225)	(0.013)	(0.251)
Control experiment:Cohorts aged 14-19 and 19-24	-0.608	-0.667	-0.667	0.114	0.149	0.151	-	-
(N=1,577)	(0.399)	(0.373)	(0.369)	(0.318)	(0.307)	(0.305)		
B. Control group includes only Arabs from mixed cities								
Experiment of interest:Cohorts aged 4-9 and 14-19	2.424	2.155	2.249	-1.210	-1.094	-1.091	-0.131	-0.485
(N=1,751)	(0.430)	(0.408)	(0.399)	(0.275)	(0.269)	(0.266)	(0.015)	(0.140)
Control experiment:Cohorts aged 14-19 and 19-24	0.124	0.148	0.053	0.041	0.028	0.101	-	-
(N=1,065)	(0.514)	(0.471)	(0.461)	(0.381)	(0.365)	(0.363)		
C. Sample of Table 5 restricted to persons born in current locality								
Experiment of interest:Cohorts aged 4-9 and 14-19	1.149	0.966	1.092	-0.822	-0.720	-0.657	-0.140	-0.602
(N=2,729)	(0.337)	(0.324)	(0.313)	(0.232)	(0.226)	(0.224)	(0.012)	(0.258)
Control experiment:Cohorts aged 14-19 and 19-24	-0.770	-0.662	-0.672	0.138	0.074	0.007	-	-
(N=1,714)	(0.414)	(0.384)	(0.377)	(0.326)	(0.313)	(0.311)		
Control variables								
Individual level religion dummy	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Locality fixed effects	No	No	Yes	No	No	Yes	No	Yes

Notes: Standard errors are presented in parentheses. Individual characteristics include a religion dummy (Muslim or Christian). In the 1983 census data, Columns (2), (3), (5), (6) and (8) include cohort dummies. Experiment of interest:Cohorts aged 4-8 and 14-18 in 1964. Control experiment:Cohorts aged 14-18 and 19-23 in 1964.

Table A1: Localities and Schools in 1964

Locality	Group	Primary schools	Secondary schools	Distance to school
AR'ARA	Treatment	0	0	4.3
ARRABE	Treatment	0	0	7.8
BI'INA*	Treatment	0	0	2.2
BIR EL-MAKSUR*	Treatment	0	0	5.3
DEIR AL-ASAD*	Treatment	0	0	2.1
DEIR HANNA*	Treatment	0	0	5.1
FASSUTA	Treatment	0	0	5.0
JUDEIDE-MAKER	Treatment	0	0	3.5
KABUL*	Treatment	0	0	1.9
KAFR KANNA	Treatment	0	0	3.0
KAOKAB ABU AL-HIJA*	Treatment	0	0	2.5
MAZRA'A	Treatment	0	0	7.0
MUAWIYYE**	Treatment	0	0	5.0
MUQEIBLE	Treatment	0	0	2.7
MUSHAYRIFA**	Treatment	0	0	4.1
MUSMUS**	Treatment	0	0	3.0
NAHEF*	Treatment	0	0	4.0
REINE	Treatment	0	0	3.1
SAKHNIN	Treatment	0	0	6.8
SHA'AB*	Treatment	0	0	3.9
TUBA-ZANGARIYYE	Treatment	0	0	15.4
ZALAFA**	Treatment	0	0	4.6
ZEMER	Treatment	0	0	3.8
ABU SINAN	Control	1	0	0.0
BAQA AL-GHARBIYYE	Control	2	1	0.0
EIN MAHEL	Control	1	0	0.0
I'BILLIN	Control	1	0	0.0
IKSAL	Control	1	0	0.0
JALJULYE	Control	1	0	0.0
JATT	Control	1	0	0.0
KAFR MANDA	Control	1	0	0.0
KAFR QARA	Control	2	1	0.0
KAFR QASEM	Control	1	0	0.0
KAFR YASIF	Control	1	1	0.0
MA'ALOT-TARSHIHA	Control	1	1	0.0
MAGHAR	Control	3	0	0.0
MAJD AL-KRUM	Control	2	0	0.0
MI'ELYA***	Control	1	0	0.0
NAZARETH	Control	13	2	0.0
PEQI'IN (BUQEI'A)***	Control	1	0	0.0
QALANSAWE	Control	2	0	0.0
RAME	Control	1	2	0.0
SHEFAR'AM	Control	3	0	0.0
TAMRA	Control	3	0	0.0
TAYIBE	Control	4	1	0.0
TIRE	Control	3	1	0.0
TUR'AN	Control	1	0	0.0
UMM AL-FAHM	Control	5	0	0.0
YAFI	Control	1	0	0.0
ACCO	Mixed	2	1	0.0
HAIFA	Mixed	2	1	0.0
LOD	Mixed	1	0	0.0
RAMLA	Mixed	1	0	0.0
TEL AVIV-YAFO (JAFFA)	Mixed	2	1	0.0

* Localities grouped in 1983 as West Lower Galilee census natural area. ** Localities grouped in 1983 as Alexander Mountain census natural area. *** Localities grouped in 1983 as Yechiam census natural area.
Source: Central Bureau of Statistics Census of Schools, 1963.

Table A2: Estimated Effect of Access to Schooling on Female Own Educational Attainment (1995 census data)

	Sample		
	Experiment of interest: cohorts aged 4-9 and 14-19	Experiment of interest: cohorts aged 9-14 and 14-19	Control experiemnt: cohorts aged 14-19 and 19-24
	(1)	(2)	(3)
5-8 years of schooling	0.128	0.042	-0.030
	(0.027)	(0.034)	(0.041)
Primary school	0.079	0.006	-0.052
	(0.033)	(0.037)	(0.040)
9-10 years of schooling	0.064	0.028	-0.058
	(0.033)	(0.032)	(0.032)
Secondary school	0.012	0.019	-0.043
	(0.030)	(0.027)	(0.026)
Matriculation certificate	-0.003	0.022	-0.047
	(0.028)	(0.025)	(0.023)
Post-secondary diploma	0.013	0.039	-0.033
	(0.022)	(0.020)	(0.018)
Observations	902	1,872	2,868

Notes: Standard errors are presented in parentheses.

Table A3: Estimated Effect of Access to Education on Female and Male Education, 1995 Census

	Years of schooling					
	Women	Men	Women	Men	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)
A. Experiment of interest:						
Cohorts aged 4-9 and 14-19 in 1964						
	1.562	0.105	1.451	0.031	1.530	0.110
(N=8,238)	(0.267)	(0.249)	(0.260)	(0.243)	(0.254)	(0.237)
A. Experiment of interest:						
Cohorts aged 9-14 and 14-19 in 1964						
	0.633	0.052	0.607	0.019	0.620	0.117
(N=6,923)	(0.291)	(0.270)	(0.281)	(0.261)	(0.275)	(0.256)
C. Control experiment :						
Cohorts aged 14-19 and 19-24 in 1964						
	0.059	0.131	0.035	0.084	0.024	0.103
(N=5,223)	(0.334)	(0.295)	(0.320)	(0.283)	(0.313)	(0.277)
Control variables						
Individual level religion dummy	No	No	Yes	Yes	Yes	Yes
Locality fixed effects	No	No	No	No	Yes	Yes

Notes: Standard errors are presented in parentheses. Individual characteristics include a religion dummy (Muslim or Christian).

Table A4 : Education and Fertility Preferences, Contraceptives Details, Quality of children, Child Mortality, Religiousity, Family Decision Making, and Health knowledge, Using Data from 1974-75 Fertility Survey

	Conrol 1		Conrol 2		Conrol 3	
	Mother's Education	Father's Education	Mother's Education	Father's Education	Mother's Education	Father's Education
	(1)	(2)	(3)	(4)	(5)	(6)
Woman's fertility preferences						
Considered desired number of children	0.027	0.022	0.025	0.020	0.021	0.017
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Number of children desired	-0.063	-0.018	-0.051	-0.011	-0.046	-0.012
	(0.016)	(0.016)	(0.017)	(0.017)	(0.017)	(0.017)
Important to have at least one son	-0.004	0.003	-0.003	0.003	-0.003	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Important to have at least one daughter	-0.003	0.005	-0.002	0.003	-0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
Contraceptives details						
Consulted wth doctor about birth control	0.027	0.006	0.028	0.005	0.021	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Consulted with anyone about birth control	0.029	0.006	0.029	0.005	0.024	0.000
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Ever done anything to prevent pregnancy	0.030	0.007	0.030	0.007	0.025	0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Used any method to prevent pregnancy	0.031	0.006	0.031	0.007	0.026	0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Respondent knows how prevent pregnancy	0.025	0.000	0.024	-0.003	0.022	-0.004
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Attidude about Quantity versus quality of children						
Limiting number of children influences chan	0.010	0.001	0.010	-0.003	0.011	-0.001
of their advance	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Should 14 years old boy continue studying	-0.001	0.003	-0.000	0.003	-0.000	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Should 14 years old girl continue studying	0.005	0.007	0.004	0.006	0.004	0.005
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)

Notes: on next page.

Table A4: Continued

	Conrol 1		Conrol 2		Conrol 3	
	Mother's Education	Father's Education	Mother's Education	Father's Education	Mother's Education	Father's Education
	(1)	(2)	(3)	(4)	(5)	(6)
<u>Incidence of child mortality</u>						
Ever experienced child mortality	-0.011	-0.006	-0.010	-0.006	-0.009	-0.005
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Number of children deaths	-0.013	-0.012	-0.015	-0.009	-0.014	-0.007
	(0.005)	(0.007)	(0.005)	(0.006)	(0.005)	(0.006)
Number of miscarriages and abortions	0.007	-0.017	0.008	-0.011	0.009	-0.009
	(0.009)	(0.010)	(0.009)	(0.010)	(0.009)	(0.010)
<u>Religiousity</u>						
Degree of religiosity in current home	-0.009	-0.006	-0.010	-0.003	-0.009	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Observe religious laws/rituals	-0.006	-0.007	-0.008	-0.004	-0.011	-0.006
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
<u>Woman role in family decision making</u>						
Respondent decides on daily expenses	0.004	-0.000	0.005	-0.002	0.005	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Respondent decides on large expenses	0.017	-0.002	0.016	-0.006	0.014	-0.006
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Respondent decides on childen's education	0.011	0.003	0.014	0.002	0.012	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Respondent decides on shopping for childre	0.007	0.003	0.010	0.004	0.009	0.003
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
<u>Health knowledge/ modernity</u>						
Sickness is due to physical/medical reasons	0.030	0.005	0.028	0.004	0.023	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Wears traditional (religious) clothes	-0.035	-0.014	-0.037	-0.013	-0.031	-0.011
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)

Notes: Standard errors are presented in parenthesis. The table presents estimates from three different specifcations, each different set of controls, defined as follows:

Control 1: religion and wife's age.

Control 2: Control 1 + husband's age, age of marriage of husband/wife, husband and wife current working status indicators, husband and wife ever worked indicators.
Control 3: Control 2 + number of rooms, electricity, running water and toilet in woman's home, and an index of standard of living.

Table A5: Difference in Differences Treatment Effect Estimates of the Change in Travel Restrictions on Labor Market Outcomes (sample includes indidividuals age 24-33, data from the 1972 and 1983 censuses)

	Female	Male
	(1)	(2)
Labour Force Participation	-0.006	0.003
	(0.012)	(0.009)
	{7,319}	{6,822}
Number of Weeks Worked	-0.842	0.221
	(0.529)	(0.575)
	{7,107}	{6,573}
Work in Locality	-0.005	-0.011
	(0.008)	(0.015)
	{7,268}	{6,123}
Log Wage	-0.071	0.005
	(0.060)	(0.022)
	{940}	{5,195}

Notes: Standard errors are presented in parenthesis. Number of observations in swivel parenthesis. The sample includes indidividuals age 24-33, data from the 1972 and 1983 censuses.

Table A6: Difference in Differences Treatment Effect Estimates of the Change in Travel Restrictions on Family Wealth and Assets at Ages 12-21 and 22-26 (data from the 1972 census)

	Female	Male
	(1)	(2)
Number of Rooms at Home	0.061	-0.037
	(0.078)	(0.077)
Running Water at Home	0.012	0.037
	(0.025)	(0.024)
Electricity at Home	0.001	-0.001
	(0.025)	(0.023)
Toilet at Home	0.037	0.013
	(0.022)	(0.020)
Log Family Income	0.010	0.017
	(0.061)	(0.055)

Notes: Standard errors are presented in parenthesis.

Table A7: Descriptive Statistics, 1983 and 1995 Census Data

	Original control group Excluding seven largest localities				Control group includes only Arabs from mixed cities			
	1983 census		1995 census		1983 census		1995 census	
	Age in 1964		Age in 1964		Age in 1964		Age in 1964	
	14-19	19-24	14-19	19-24	14-19	19-24	14-19	19-24
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Women								
Years of schooling	5.233	3.739	5.804	4.135	6.673	6.068	7.626	6.679
	(4.285)	(3.991)	(4.318)	(3.797)	(4.950)	(4.547)	(4.929)	(4.466)
Fertility	4.847	6.156	5.197	6.041	4.319	5.019	3.890	4.646
	(3.034)	(3.531)	(3.028)	(3.286)	(2.934)	(3.139)	(2.797)	(3.005)
Labor-force participation	0.147	0.119	0.185	0.160	0.255	0.257	0.402	0.302
	(0.354)	(0.324)	(0.389)	(0.367)	(0.437)	(0.438)	(0.491)	(0.460)
Marriage	0.863	0.916	0.926	0.916	0.947	0.898	0.930	0.901
	(0.344)	(0.278)	(0.262)	(0.278)	(0.225)	(0.303)	(0.256)	(0.299)
Age upon marriage	20.56	21.26	21.49	19.88	20.50	20.54	22.19	20.85
	(4.162)	(5.205)	(6.425)	(8.059)	(4.194)	(4.306)	(7.512)	(8.588)
Divorce	0.005	0.010	0.011	0.012	0.034	0.044	0.089	0.044
	(0.068)	(0.099)	(0.102)	(0.107)	(0.182)	(0.205)	(0.285)	(0.206)
Observations	430	403	378	344	263	206	214	182
B: Spouse								
Years of schooling	7.810	6.283	7.490	6.625	7.403	6.840	7.896	6.950
	(4.041)	(3.789)	(4.021)	(3.827)	(4.737)	(4.834)	(4.910)	(4.698)
Labor-force participation	0.926	0.856	0.739	0.703	0.872	0.877	0.752	0.655
	(0.263)	(0.352)	(0.440)	(0.458)	(0.335)	(0.330)	(0.433)	(0.477)
Ln (monthly earnings)	9.805	9.747	8.196	8.150	9.836	9.956	8.398	8.215
	(0.622)	(0.505)	(0.527)	(0.544)	(0.660)	(0.591)	(0.625)	(0.688)
Observations	363	353	306	273	226	162	149	119

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 for 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.

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