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MOTHER'S SCHOOLING, FERTILITY, AND CHILDREN'S EDUCATION: EVIDENCE FROM A NATURAL EXPERIMENT

Victor Lavy
Alexander Zablotsky

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ABSTRACT

This paper studies the effect of mothers' education on their fertility and their children's schooling. We base our evidence on a natural experiment that sharply reduced the cost of attending school and, as a consequence, significantly increased the education of affected cohorts. This natural experiment was the result of the de facto revocation in October 1963 of the military rule that had been imposed on Arabs in Israel, immediately creating free access to institutions of schooling. Military rule, in effect from 1948 to 1966, imposed severe restrictions on movement and travel and therefore disrupted access to schools for residents of localities that had no schools. The change in access to schools affected mainly girls, increasing schooling by 1.02 years for women who were aged 4–8 in 1964 and by 0.58 year for those who were aged 9–13 at the time. These very large effects triggered a sharp decline in completed fertility, measured at 0.61 child for the younger affected cohorts and 0.47 for the older cohorts. Implied 2SLS estimates show that a one-year increase in maternal schooling caused a decline in fertility of 0.5–0.6 child in the younger cohorts. Additional evidence that we present suggests that labor-force participation, age upon marriage, marriage and divorce rates, and spousal labor-force participation and earnings played no role in this fertility decline. However, since spousal education increased sharply through assortative matching, it did play a role in the decline in fertility. These results are robust to checks against various threats to our identification strategy. We also estimate that the increase in mother's schooling led to an increase in the education of children, amounting to about one-third of the increase in their mothers' schooling.

Victor Lavy
Department of Economics
Hebrew University
Mount Scopus
Jerusalem 91905
ISRAEL
and NBER
msvictor@mscc.huji.ac.il

Alexander Zablotsky
The Hebrew University of Jerusalem
Department of Economics
Mount Scopus Jerusalem Israel 91905
alex.zablotsky@mail.huji.ac.il

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). Also, under conditions of positive assortative mating, a woman's education is causally connected to that of her mate (Behrman and Rosenzweig, 2002), so the effect of education on household permanent income is augmented by a multiplier effect. Some societies, however, experience fertility transition without these economic forces 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] However, the increase in education may have impacted Arab's women fertility through other channels. First, education may improve an individual's knowledge of, and ability to process information regarding, fertility options and healthy pregnancy behaviors (Grossman 1972). Glewwe (1999) argues that the most important mechanism for gains in health knowledge is not direct, via curricula; instead, skills obtained in school facilitate the acquisition of health knowledge. Second, education may enhance females' ability to process information, potentially increasing their knowledge of contraception options (Rosenzweig and Schultz, 1989; Thomas, Strauss, and Henriques, 1991). Education may also improve a wife's bargaining power inside her marriage (Thomas, 1990). Moav (2005) suggests that educated mothers may have a technical advantage in producing educated children, tilting the tradeoff from the number of children to their quality. 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.

This paper studies the role of female education in reducing fertility at a time when tradition still kept women far from the labor market. In particular, we present evidence that the strong negative relationship between women's fertility and education reflects a causal effect and shows that potential mechanisms such as women's labor-force participation, age upon marriage, marriage rate, and divorce rates did not play a major role in this fertility decline. The impact of women's education remained very large after we accounted for spouse's employment.

[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).

Furthermore, spouse's education increased immensely through assortative matching and, therefore, probably played a major role in the decline in demand for children. The last section of the paper shows that the increase in mothers' schooling also induced an increase in the education of children concurrent with a decline in their number that we calculated at about two-fifths of the increase in mothers' schooling.

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, primarily the South (Negev), the North (Galilee), and the center East (the "Triangle"). The Arab residents of these areas were subject to measures that placed tight controls on all aspects of their lives, including severe restrictions on movement and the requirement of a permit from the Military Governor to travel more than a given distance from a person's registered domicile.[3] The travel restrictions were revoked in October 1963 following the unexpected resignation in June of that year of the Prime Minister, David Ben-Gurion, who together with his ruling Labor Party strongly supported the continuation of the Military Government. 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 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

[3] A recent historical episode of similar restrictions on perceived "enemy" populations is the United States Government's internment and forced relocation of approximately 110,000 Japanese Americans and Japanese residing along the Pacific coast of the United States to so-called 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, which allowed local military commanders to designate "military areas" as "exclusion zones," from which "any or all persons may be excluded." This power was invoked to declare the exclusion of all people of Japanese ancestry from the entire Pacific coast, including all of California and most of Oregon and Washington, except for those in internment camps. 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.

lived in predominantly Jewish cities. The latter population group was also placed under military rule at first (1948) but was exempted de facto from some of the restrictions a short time later.

The change 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 change of language of instruction on the return to schooling (Angrist and Lavy, 1997), the effect of college education on earnings (Card and Lemieux, 1998), the effect of school construction on education and earnings (Duflo, 2000), and the effect of school choice on pupils’ academic achievements (Lavy, 2010). 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 1983, the affected cohorts were aged 24–35, making it possible to study the effect of education on early-age fertility. In 1995, the affected control cohorts were already aged 36–45, 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 4–8 in 1964 and by 0.58 for women aged 9–13 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 these women's time did not change much during the same period. It also seems that changes in the education, employment, and earnings of affected women' spouses did not play a significant role in the fertility decline. We also provide some evidence that the increase in mothers' schooling impacted positively their children's education even though this intergenerational transmission of human capital may have involved channels other than the pure effect of schooling.

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. This proposition may be violated if the removal of travel restrictions in late 1963 allowed access to other services that may have affected fertility directly. An example would be access to healthcare services, particularly pre- and post-natal services that Israel provides at special public well-baby centers and general clinics. Access to these services would confound our results if they were available in localities that had schools and not in localities that lacked schools and affected fertility directly or indirectly. Another condition for the confounding effect of these services is that they must have affected fertility among the young (2–13) but not among the older (14–18) cohort. Below we provide evidence of no correlation between the availability of schools and the availability of either type of healthcare clinic in the community, making it very unlikely that our estimate of the effect of mother's education on fertility is caused by other factors. Another identification concern is the treatment estimates may be biased due to a pre-existing control–treatment differential time trend in the fertility rate. We use pre-reform data relating to the localities' mean fertility rate for cohorts aged 13–24 in 1964 to estimate different time trends in treatment and control localities. This evidence suggest that there is a time trend in the fertility rate but that this trend is identical in treatment and control localities.

An extensive literature documents associations among education, fertility (Strauss and Thomas. 1995). However, whether these associations 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 (2004) 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. Most recently 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 the lack of a consensus about the causal effect of women's education on fertility. The last section of this paper presents our results about the effect of mothers' education on children's schooling. First, however, we provide a brief summary of the recent relevant literature, which also suggests inconsistency in the evidence about the causal effect of parental education on children's schooling.

The rest of the paper is organized as follows: Section 2 provides a brief review of recent studies on the effect of mother's schooling on fertility. Section 3 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 4, we discuss our identification strategy and present the results of our estimation of the effect of schooling on fertility in Section 5. In Section 6, we check the robustness of the results and discuss possible threats to our identification strategy. Section 7 presents evidence of the intergenerational transmission of human capital and discusses a variety of important interpretation issues. Section 8 concludes.

2. Recent Literature on the Effect of Mother's Education on Fertility

Empirically, there is a strong positive correlation between education and delay of the onset of fertility and a strong negative correlation between education and completed fertility. (See Strauss and Thomas (1995) for a review of the literature.) However, this may not indicate a causal relationship running from education to fertility due to the potential for reverse causality and possible omitted variables, e.g., girls who drop out of school early are also probably those most at risk of having children early. Several studies attempt to address this identification issue. Some of them cite school expansion as the source of an exogenous decrease in the cost of schooling. Breirova and Duflo (2002) use a large school-building program in Indonesia to construct instruments for years of education of both women and men. Their instrumental variable estimates suggest that women's education does not reduce total fertility but increases the age upon marriage and decreases the number of children born before the woman turns 15. Using a similar strategy, Osilii and Long (2008) also find a causal effect of education on fertility in Nigeria. Both papers focus on primary education; the effect of secondary education on early fertility may be much larger if some of this effect increases the opportunity cost of young women's time.

Similar results of schooling expansion have been found for secondary schools in developed countries. Black, Devereux, and Salvanes (2004) find that education gains resulting from compulsory-schooling laws decreased teenage pregnancy in the U.S. and Norway and that the effect in both countries was highly similar in magnitude. 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 admission to school, find that young women who obtain extra schooling because they are born a few days before the cutoff for school admission are equally likely to become mothers at the same age. While they conclude that "education does not affect fertility," their results can in fact be reconciled with those of the earlier studies. McCrary and Royer identify the effect of more years of education obtained in early childhood for people who drop out of school at around the same age (e.g., 16, the earliest age permitted). Duflo, Kremer, and Dupas (2010) suggest that this is a different conceptual experiment from asking girls (or giving them the opportunity) to stay in school one more year, say, from age 16 to age 17. When we compare two girls who dropped out at age 16 but were born on opposite sides of the September 1 cutoff, we find that one of them has one more year of schooling than the other by having started school earlier. Thus, the two sets of results may be reconciled if what affects the probability of teenage pregnancy is the fact of being in school during teenage years rather than the content being taught.

In a recent paper, Duflo, Kremer, and Dupas (2010) provide experimental evidence on the relationship between education and early fertility in Kenya. Girls that randomly received free uniforms in their last three years of primary school (2003–2005) were 2.4 percentage points less likely to drop out of primary school by 2005 and 4.5 percentage points more likely to have graduated from primary school by 2007. By the end of 2005, girls who received uniforms were 1.7 percentage points less likely to be married and 1.5 percentage points (10 percent) less likely to have started childbearing. The effects persisted after the end of the education subsidy: at the end of 2007, when most of these adolescents had left school, girls in the treatment group were still 2.6 percentage points (8 percent) less likely to have started childbearing. These results imply a surprisingly large effect of access to education among adolescent girls on early fertility, at least among girls who are likely to drop out of school.

Kirdar, Tayfur, and Koç (2009) estimate the impact of schooling on the timing of marriage and early fertility in Turkey. The source of exogenous variation in schooling, they report, was the extension of compulsory schooling in Turkey in 1997. Their findings indicate that at age 17—three years after the completion of compulsory schooling—the proportion of married women drops from 15.2 percent to 10 percent and the proportion who gave birth falls from 6.2 to 3.5 percent as a result of the policy. This implies that the impact of increased schooling on marriage and early fertility persists beyond the completion of compulsory schooling for an important duration. In addition, the delay in the timing of first birth is driven by the delay in the timing of marriage. Once a woman is married, schooling has no effect on the time that passes until her first childbearing.

3. The 1948–1966 Military Government and Restricted Movement of Arab Citizens of Israel

During Israel's 1948 War of Independence, the Provisional Council of State decided to impose a special military governmental authority on areas populated by Palestinian Arabs.[4] This population, the enemy against which the Jewish population fought its war of independence, became citizens of the new Jewish state overnight. The Military Government, inaugurated on October 21, 1948, and disbanded in 1966, was legally based on defense regulations enacted in 1945 by the British Mandate Government that ruled Palestine at the time.[5] From then until the cessation of the enforcement of these regulations, the Military Government was the dominant Israeli governmental authority among the Israeli Arab minority. At first, the Military Government worked together with the Ministry of Minorities, which was responsible for humanitarian aspects of the treatment of the Arab population, but this ministry was abolished in 1949. Thereafter, the Military Government held sole responsibility for all affairs of the Arab population. Although all 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.

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, that 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 medical treatment in the cities, importation of capital goods (tractors), access to work or educational opportunities, and practically every other purpose. It has been claimed that getting these permits involved side payments to Arab collaborators.

The travel restrictions and the permit requirement imposed a heavy burden on the Arab population and increased its cost of travel. The Arab-populated "enclosed areas" were divided into three separate army commands: north, south, and center. Each such area was isolated from the other and most Arab citizens were, of course, isolated from the majority Jewish population as well. Enclosure orders controlled movement by means of permits—issued by military commanders—to leave the enclosed area and sometimes to move about within it, too. Residents in different geographical areas (Galilee/north, "Triangle"/center, and Negev/south) were physically isolated from each other and travel restrictions kept apart even those in the same region but in different villages, towns, or tribal areas.

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[4] Much of the material in this section is based on Bauml (2002).

[5] These regulations strongly resembled those that were enacted in England during World War II and annulled at the end of the war (Jiryis, 1966). In 1948, the provisional Government of Israel decided to leave all Mandatory laws, including the Emergency Defense Regulations, in place.

Apart from the practical hardships, the travel restrictions took an emotional toll on their subjects and created 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. For example, on October 29, 1956, on the eve of the Suez War, the Government changed the curfew to an earlier hour. Border Guard forces entered 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. Fouzi himself had to drop out from the boarding school in Nazareth after a year because the family could not afford to keep all its sons in schooling. Ziad Mahjena tells much the same story.[6] He completed primary school in 1957/58 in his home town and wanted very much to continue in nearby schools in Nazareth or 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 do away with the Military Government. In February 1962 and February 1963, four political parties (including Menachem Begin's right-wing

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

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 by Government order Arabs could once again circulate freely around the country.[7] This change removed one of the most burdensome restrictions, one that had profoundly affected the daily lives of Arabs in Israel since the creation of the state. In 1966, the Military Government was abolished altogether; all that remained were several specific restrictions, such as traveling to the nuclear plant in Dimona, the vicinity of the Jordanian border in the Arava Valley, and the 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 became more costly because of the longer travel time (passing checkpoints, etc.) and the financial cost of obtaining permits and/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. The Appendix presents a list of all relevant localities, including those that had schools and therefore allowed their populations to retain unrestricted access to schooling institutions. 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 (Acco, Haifa, Lod, Ramla, and Tel Aviv-Jaffa) were cities that had Jewish majorities and Arab minorities. These cities 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

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

composed of non-Arab minorities (Druze and Circassians); 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 school rose commensurably with the distance to the nearest school. We will exploit this variation in the empirical work to demonstrate that the effect of the lifting of the travel restrictions in late 1963, thus lowering the cost of attending school, had the strongest positive effect on children in localities that were farthest away from 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 social and healthcare services and labor-market opportunities 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 five largest towns (Nazareth, Tamra, Shefar'am, Tayibe, Umm al-Fahm); 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.

4. 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.[8] 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. A particularly important point for our research is that these data identify small geographic areas and localities as well as details of age, occupation, household income, marriage, and education, as well as residential and household details. Due to statistical confidentiality requirements, the data file available from the 1995 census, which includes detailed geographic codes down to code of locality (localities with populations of 2,000+), contains other variables that have been extensively grouped. Thus, age is reported in five-year cohorts and years of schooling are reported in seven cohorts (0, 1–4, 5–8, 9–10, 11–12, 13–15, 16+). 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+. There is a version of the 1995 census that does not include detailed locality code and provides all detailed values (not grouped) 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.

Table 1 presents the 1983 and 1995 mean demographic and economic outcomes for two cohorts, those aged 14–18 and 19–23 in 1964. These cohorts were unlikely to have been affected by the change in travel policy at the end of 1963 (see below). 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 14–18 cohort was 5.79 in the control group and 4.36 in the treated group. Mean fertility in the age 14–18 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 14–18 cohort strongly resemble the treatment–

[8] 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.

control differences based on the age 19–23 cohort. For example, mean years of schooling of the age 19–23 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 14–18 cohort. Also, the treatment–control difference for fertility in 1995 was 1.03 for the age 14–18 cohort and 1.10 for the 19–23 cohort. 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. 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.

5. 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 14 years old or younger in 1964, 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 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 existence/non-existence of a school in locality of residence before 1964 as exogenous variables and as an instrument for an individual's education. This identification strategy may be presented in an interaction-terms analysis of the first-stage relationship between the education (S_{ijk}) of individual *i*, who resided in locality *j* in year *t*, and her exposure to the program:

(1) $$S_{ijt} = a + \alpha_l + \mu_t + \sum_{l=2}^{18} \left(A_l T_{il}\right) \delta_l + \varepsilon_{ijt}$$

where T_{il} is a dummy that indicates whether individual i is age l in 1964 (a cohort dummy), α is a constant, μ_k is a cohort of birth fixed effect, a_l 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 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 time trend in fertility for cohorts age 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 average estimated coefficient for ages 14+ is about 0.02 and is not significantly different from zero. In contrast, the six estimates in the younger than 14 age groups is significantly different from zero and more precisely estimated for cohorts age 9 and younger.

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. Therefore, we impose the restriction that the treatment effect is equal to 0 for cohorts older than 13 in 1964.

5a. Simple Difference-in-Differences Estimates of Access to Schooling on Education

Given these results, we move on to the use of data from both censuses, 1983 and 1995, to estimate the effect of the change in travel restrictions in 1963 on schooling and fertility. In 1983, our youngest treated group was aged 24–28 and the oldest was aged 29–33. In 1995, the youngest treated group was aged 34–38 and the oldest was aged 39–43. 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. For estimation precision, the first treatment group that we examine includes individuals who were aged 4–8 in 1964 and the next (older) affected group, comprising those aged 9–13 in 1964. We also define two unaffected groups, aged 14–18 and aged 19–23 in 1964. Using these age groups, we first present in Table 2 the means of years of schooling of different cohorts by exposure to the regained access to schooling, which we use to analyze an uncontrolled difference in the difference estimates. In Panel A, we compare the schooling attainments of individuals in the control group (women aged 14–18 in 1964) with those of the women who were exposed the longest to treatment (aged 4–8 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 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, mean reversion may confound the estimated effect of interest. However, an implication of the identification assumption may be tested because the schooling of individuals aged 14+ 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 19–23 in 1964 with cohorts aged 14–18 in 1964. The estimated difference in differences is 0.026 (SE=0.344) year of schooling on the basis of 1983 census data and –0.384 (0.374) when the 1995 census data are used. We also analyzed a control experiment based on older cohorts and obtained similar results. These outcomes provide some suggestive evidence that the difference-in-differences 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 individual characteristics and locality fixed effects.

Table 3 presents the elements of the difference-in-differences 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 4–8 age cohorts based on the 1983 census data is 0.122 (SE=0.07). The corresponding difference between treatment and control unaffected cohorts aged 14–18 is 0.677 (SE=0.166), implying a difference-in-differences 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 14–18 age group in 1964 to 5.088 in the 9–13 age group and 4.115 for women in the 4–8 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 difference-in-differences estimate of the effect of the removal of travel restrictions is –0.727 (SE=0.195) for women aged 4–8 in 1964 and –0.543 (SE=0.227) for women aged 9–13 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 age group was almost 40 years old by 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 difference-in-differences 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. These estimates support the assumption that otherwise the decline in fertility would not have been systematically different in affected and unaffected areas.

We may use the difference-in-differences 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 difference-in-differences estimates of the first-stage and reduced-form relationships. For example, the Wald estimate based on the sample of the young and oldest affected cohorts in 1983 is –0.74 (–0.555 divided by 0.751) and –0.57 (–0.279 divided by 0.490), respectively.

Did the control and treatment localities experience different convergence in the fertility rate before the policy reform?

The difference-in-differences estimates may be biased due to a pre-existing control–treatment differential convergence in the fertility rate. We use pre-reform data (from the 1983 census) relating to the localities' mean fertility rate for cohorts aged 13–24 in 1964 to estimate different time trends in treatment and control localities. We employ two methods for this estimation. First, we estimate a constant linear time-trend model while allowing for interaction of the constant linear trend with the treatment indicator. Second, we estimate a model with cohort dummies and include in the regression an interaction of each of these cohort dummies 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. These results are presented in Columns 1–2 of Table 4.

Panel A 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). Panel B presents the estimates of the cohort dummies model. The evidence in Panel B is fully consistent with the linear trend model. The interaction terms of the treatment indicator with the year dummies are all small and not significantly different from zero; furthermore, some are positive and others are negative, lacking any consistent pattern. Columns 3–4 of Table 4 present a similar time-trend estimation from models that also include locality fixed effects as controls. These results strongly resemble those in Columns 1–2. 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. The mean trend among cohorts aged 14–23 in 1964 is an annual increase of 0.271 in years of schooling. The estimated coefficient of the interaction of this trend with the treatment indicator is practically zero, 0.012 (SE=0.021). These results are fully consistent with the evidence in Figure 1.

5b. Controlled DID Estimates of Access to Schooling on Education

The simple difference-in-differences estimates 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) $$S_{ijt} = \alpha + a_{lj} + \mu_t + (A_j\, Y_i)\, \delta + \varepsilon_{ijt}$$

where S_{ijt} is the education of individual i who lives in locality j in year t, Y_i is a dummy indicating whether the individual belongs to the ‘‘young” cohort in the subsample, α is a constant, μ_t is a year-of-birth (cohort) fixed effect, a_{lj} is a locality-of-birth fixed effect, and A_j denotes areas that were under military rule.

Columns 1–3 in Table 5 present estimates of Equation (1) for three subsamples. In Panel A, we compare children aged 4–8 in 1964 with children aged 14–18 in 1964 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 difference-in-differences estimates presented in Table 2. Recall that this specification controls only for the cohort-of-birth dummy of the population aged 4–8 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 4–8 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 individual characteristics as controls. The resulting conditional difference-in-differences 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 9–13 in 1964; again, the control group is children aged 14–18 in 1964. Here as well, we report results based on 1983 and 1995 census data. The estimated effect of treatment on the older cohorts, as expected, is lower than the estimated effects obtained 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 14–18 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 (spurious) positive coefficients. The impact of “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. Even if the identification assumption is satisfied, the coefficient may slightly overestimate the effect of the program on average education.

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 (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 4–8 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 9–13 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 4–8 and 8–13 age cohorts, are based on 1995 census data but are not different when 1983 census data are used. Much as in our earlier results, the estimates for women are positive and significant in all three specifications. However, the estimated effect of treatment on men is practically zero in both the 4–8 and the 9–13 age cohorts. For the 9–13 age 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. 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 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. Another possible explanation for 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.

5c. 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) $$F_{ijt} = \alpha + a_{lj} + \mu_t + (A_j\, Y_i)\, \delta + \varepsilon_{ijt}$$

where F_{ijt} is the number of children in 1995 of individual *i*, who was born in locality *j* in year *t*, in the Military Government zone and without free access to schooling due to the travel restrictions. The results of the estimates of parameter δ based on the three specifications of Equation (2) are presented in Table 5, Columns 4–6. Panel A compares the fertility of women who were aged 4–8 in 1964 with that of women aged 14–18 in 1964. In Column 4, the specification controls only for the interaction of a cohort of birth dummy and the population of the young cohort in 1964. Adding individual characteristics as controls 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 9–13 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 last-mentioned 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, higher than those based on the 1983 census data but lower 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 difference-in-differences 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.

5d. 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) $F_{ijt} = \alpha + l_{lj} + \mu_t + S_{ijt}\,\mu + \varepsilon_{ijt}$

where l_{lj} and μ_t denote locality-of-birth and cohort-of-birth effects, respectively. Ordinary least-squares (OLS) estimates of the relationship between fertility and education may lead to biased estimates if there is a correlation between ε_{ijt} and S_{ijt}. 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.

The 2SLS results 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.

To further substantiate the evidence that our estimated effect of mother's schooling is not confounded by the direct effect of father's education, Table 6 presents evidence on the basis of two subsamples differentiated by spouse's age in 1964. The first subsample is restricted to women who were aged 4–8 in 1964 and whose 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 9–13 (37% of the full sample). The second subsample is restricted to women whose their 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.

5e. 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 4–8 and 9–13 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), than in localities closer to the nearest school, 0.612 (SE=0.333). Symmetrically, the reduced-form effect in the localities that are farther from a school is also larger: –0.694 (SE=0.228) versus –0.426 (SE=0.232). However, both corresponding estimated 2SLS effects are very similar to the IV estimate reported in Table 5 on the basis of the combined full sample. This evidence, we think, strengthens the interpretation of the effect of the travel-policy change in 1963 on schooling as reflecting a decline in the cost of attending school.

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 stratify the sample by size of locality and present estimates on this basis. 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 noting first that our 2SLS estimated effects of education on completed fertility (–0.598 or –0.730) are higher than but not significantly different from the OLS estimate. First, our IV estimate is larger (more negative) 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 as the latter falls within the confidence interval of the IV estimate. 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. Another related explanation is that the effect increase in years of schooling due to the natural experiment led to sharp increase in primary school completion rate. Primary school completion may have however a much larger effect on fertility than infra-marginal gain in primary schooling. 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 be 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.

5f. Mediating Factors of Effect of Education on Fertility

As discussed in the Introduction and in Section 2, 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.

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. The absence of an effect of education on 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 effect of education on these two outcomes is small 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.

We summarize the above evidence by concluding that the increase in education had no significant effect on any of the mechanisms through which female schooling could have reduced fertility and that we could examine with our available data. 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 the other channels. One such potential mediating factor is spouse selection, to which we turn next.

5g. Spouse's Education and Earnings

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 4–8 age cohort may have been affected by the annulment of the travel restrictions whereas the spouses of those in the affected older age cohort (9–13) were too old to have been affected by the regained access to schooling. However, since the travel-policy change had little effect on men in general (as shown in Appendix 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 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; 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. 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.

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.

6. Robustness Checks and Threats to Identification

The identification assumption for estimating the causal effect of mother’s schooling on fertility is that the removal of travel restrictions in late 1963 had neither a direct nor an indirect effect on fertility except by allowing access to schooling. This assumption may be violated if the removal of travel restrictions enabled access to other services that could affect fertility directly. An example would be access to healthcare services, particularly pre- and post-natal services at special public well-baby centers. These centers provide pre- and post-natal healthcare on site, checkups and immunizations for children in kindergarten and schools, and family-planning services and contraceptive information. 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 led to access to such centers and not only to schools. Such access could have reduced infant mortality, for example, and, in turn, fertility. These centers may also increase exposure to contraceptives, but this factor is unlikely to be relevant to the Arab and mostly Muslim population that we study in this paper. This direct effect of exposure to these centers on fertility would have coincided with the potential 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, 40 of which had schools, that did not have well-baby centers and that the population did not receive these services in any other way. This suggests a low or even zero correlation between access to schools and access to well-baby centers.

Another 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, 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. Consequently, the reduction in fertility that we estimate is very unlikely to have been caused by improved access to well-baby centers or more general health services that were unique to the treated localities in our sample.

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 9 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 9 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.

Before concluding this section, we believe 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 infant mortality, for example, for young mothers (cohorts aged 13 or younger in 1964) but not those in older cohorts (aged 14+ 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.

Another possibility is that when the government cancelled the travel restrictions it also expanded its investments in well-baby health services precisely in treatment localities. Our evidence suggests that this did not happen because large public investments and other types of government initiatives to improve social and economic infrastructure in the Arab sector were not evidenced until the 1980s, partly due to the severe economic recession in 1966 and partly due to the heavy military burden of the 1967 and the 1973 wars. The Arab population did, of course, benefit from improvements in health, education, economic, and social services down the years since 1948, but this trend did not spike in 1963 or in 1966. Again, even if such improvements occurred only in the treatment localities, it is very unlikely that they affected only the cohorts up to age 13 in 1964 and not those aged 14–18 that year. One exception is the possibility that improved health services improved the schooling outcomes of the young and not the old cohorts because the latter had already left school by 1964. As noted above, however, the 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.

Potential Mean Reversion and Results Based on Alternative Control Groups

Another possible threat to identification is that our foregoing DID estimates may reflect reversion to mean. To alleviate this concern, 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 A4, which present the mean characteristics of this control group. For example, the control-treatment difference in fertility rate among those in the 19–23 age group 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 group for the older cohorts (14–18 and 19–23) are much better than those of the treatment group. (See Columns 5–8 in Table A4.)

The results based on these two alternative comparison groups, presented in the first two panels of Table 10 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 10 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 regression to mean or differential time trends in unobserved heterogeneity between treatment and control.

Panel C in Table 10 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 72 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 10 is –0.602 (SE=0.258) while that in Table 5 is –0.598. This result is not surprising because the very few who left their locality of birth most likely moved to a nearby village or town that had the same treatment status as their locality of birth.

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 reached 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 4–8 age cohort and –0.585 (SE=0.176) for the 9–13 age cohort.

7. Effect of Mother's Education on Children's Schooling

In this section, we assess whether the change in mothers' schooling affected the educational outcomes of the next generation. Here we use only 1995 census data in order to allow children to advance to the age where their education reflects completed schooling as closely as possible. For the same reason, we focus on the human capital of children who were born to mothers aged 18–26. This selection rule guarantees that the sample will include the oldest children, those most likely to have already attained post-secondary or even tertiary schooling, and that those in the sample will have a comparable mother's age at birth.[9] To assure meaningful sample size, we merge the younger and the older affected cohorts, from age 4 to age 13 in 1964.

7a. Recent Relevant Literature

Recent studies that aim to estimate the causal link between the education of parents and that of their children provide evidence that is far from conclusive. One strand of the literature tries to identify total causal effects of parents' education on children's education via twin datasets (Berhman and Rosenzweig, 2002), adoptee datasets (Plug, 2004), or school reforms (Black et al., 2005) in order to control for parents' unobserved endowments. These studies assume that parents of twins apply the same childraising abilities to both (Berhman and Rosenzweig, 2002) or, in the case of adoptee datasets, that the process of adoption is random (Plug, 2004), or that no selection comparable to inheritable abilities takes place.

The instrumental-variables (IV) approach has been much more widely used to explore the causal relationship between parents' and children's education. Black et al. (2005) apply this approach using a 1960s reform that extended compulsory schooling in Norway from seventh grade to ninth grade, adding two years of required schooling. Despite strong OLS relationships,

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

this study finds little evidence of a causal relationship between parent education and child education. In some specifications, however, they find a positive causal impact of mothers' education on sons' education. Oreopoulos, Page, and Stevens (2006) use a similar methodology to examine the influence of parental compulsory schooling on grade retention status for children aged 7–15 using the 1960, 1970 and 1980 U.S. censuses. Studying changes in U.S. law (in different states at different times) to identify the effect of parents' educational attainments on children's school performance (proxied by grade-for-age), they find that a one-year increase in parental education attainments reduces the probability of a child's repeating a grade by 2–7 percentage points, and their IV estimates are more negative than the OLS ones. Maurin and McNally (2008) use variation in college attendance induced by the May 1968 student riots in Paris. In the aftermath of the unrest, students and authorities negotiated for more lenient exam standards for the Baccalaureat exam (which, if successfully completed, guarantees access to university) for that year alone. As a result, the pass rate increased significantly that year, allowing more students to attend college and leading to a significant increase their wages (about 14%). In addition, these returns were passed on to the next generation in that grade repetition among children of those in the affected cohort declined significantly. Carneiro, Meghir, and Parey (2007) use the NLSY79 and variation in maternal education induced by variation in schooling costs at the time the mother was growing up to identify the effect of maternal education on a variety of children's outcomes, including behavioral problems, achievements, grade repetition, and obesity. They find that, among children aged 7–8, a one-year increase in mother's education increases math standardized-test performance by 0.1 of a Standard Deviation and reduces the incidence of behavioral problems. Page (2009), using cohort-level variation in schooling levels induced by the G.I. Bill to identify the intergenerational transmission of education, argues that this variation traced to the timing of the draft and not to unobservable individual characteristics or underlying trends. She finds that a one-year increase in father's schooling reduces the probability of his child's repeating a grade by 2–4 percentage points. This is quite consistent with Oreopoulos, Page, and Stevens (2006), suggesting that the timing of the additional year—either in high school, due to the prolongation of compulsory schooling, or in college through GI benefits—does not affect the estimates.

Since maternal education can affect children's schooling through several different channels and the intensity of these channels may not be the same for all levels of education or for all subpopulations, the effect of education on child's schooling may differ across studies. Currie and Moretti (2003), for example, use the opening of new colleges to study the effect of maternal education on infant health. The women whose schooling attainments at motherhood were affected by college openings were those who had a high level of education generally.

7b. Reduced-Form and 2SLS Estimates of Intergenerational Transmission of Education

In this section, we report results of estimating the effect of mother's education on her children's schooling. The unit in the sample is now the child and not the mother and there are some mothers who have more than one child in the sample. We use the following educational attainments as outcome measures: completed years of schooling on census day, completed at least primary schooling, completed at least secondary schooling, and obtained a post-secondary certificate (academic degree or other). The sample includes 5,094 mothers of 10,847 children. Using this sample we estimate the following model:

$$E_{ijt} = \alpha + l_{lj} + \mu_t + S_{ijt}\,\delta + \varepsilon_{ijt} \tag{5}$$

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

Table 11 presents estimates from the three specifications of the reduced-form relationship between mother's schooling and her children's education. For each specification and child's-education outcome we present estimates based on the quasi-experimental contrast between children of affected mothers from the 4–13 age cohorts, the unaffected 14–18 age cohorts, and the control experiment of contrasting two unaffected groups of cohorts, 14–18 and 19–23.

Several results stand out in Table 11. The access to schooling that Arab mothers gained in 1964 caused an increase in the schooling of their children. This is reflected in higher attainments in secondary and post-secondary schooling, both of which reflected in an increase in total completed years of schooling. The effect on the probability that the affected mothers' children would complete secondary schooling is modest: an increase of 4 percent. The effect on the likelihood of completing post-secondary certification is an increase of 2.3 percent.

Table 12 presents the OLS and 2SLS estimates of the effect of mother's schooling on children's educational outcomes based on the use of the 14–18 cohorts as a control group. The OLS estimates are all positive and highly significant with large t-values. The 2SLS estimates surpass the OLS estimates except for primary schooling but are much less precisely estimated, suggesting that the OLS estimates are biased downward by a large factor.[10] For example, the OLS estimate of the effect on completing at least secondary schooling is 0.009 while the 2SLS

[10] Maurin and McNally (2008). estimating the effect of parental schooling on children's grade repetition, also report IV estimates (–0.33) that are four times larger than the OLS estimate (–0.08). Oreopoulos, Page, and Stevens (2006), who report a significant negative effect of parental education attainments on the probability of a child's repeating a grade, also report IV estimates that are more negative than the OLS ones. Carneiro, Meghir, and Parey (2007) report a similar pattern.

estimate is 0.067. The gap between the two respective estimates for the effect on obtaining at least a matriculation certificate is somewhat smaller.

An interesting and important question addressed in the literature is the intensity of the intergenerational transmission of human capital from mothers to children. We can measure this parameter by calculating the ratio between the reduced-form effects of the treatment on the years of schooling that the mother and the child completed. The estimated effect on the mothers' years of schooling is 1.018 for the young affected cohorts and 0.575 for the older ones. Since the mothers of the children in our sample of analysis come from both affected groups, we can use the average of the two group-specific effects, i.e., 0.80 year of schooling. Since the reduced-form gain in children's schooling is 0.387, the ratio is 0.48, marginally higher of what we might have expected given the abundant literature on the intergenerational correlation in economic status: the central tendency of estimates of the intergenerational-correlation coefficient is 0.3–0.4 (Solon, 1999). The estimated effect that we report here, however, is more in line with evidence of studies that used instrumental-variable estimation to study the effect of parental schooling on children's education.

8. Conclusions

This paper studied the effect of women's education on their fertility and their children's schooling, an important question with implications for economic development and growth and for social change. The evidence on this question remains mixed and inconclusive; we extend it 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 and on the next generation's education. The estimated effect on fertility is large as well; it explains some of the dramatic decline in the fertility of Israel's Arab-Muslim population. Our evidence 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 reported a 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 and, since they are derived from a population-based sample, they provide a desired dimension of external validity. The evidence may leave further room for generalization also because the Arab population we studied is mostly Muslim and, at the time, had characteristics well representative of the populations of many other developing countries.

Another point worth emphasizing is that the estimated effect of education on fertility does not operate through the opportunity cost of mother's time, as the labor-force participation rate among Arab women has not increased very much in the past 50 years. We also find very little change over this period in other demographic variables such as marriage, age upon marriage, and divorce. Therefore, the increase in female education must have impacted Arab women's fertility through other channels, such as knowledge and ability to process information about fertility options, healthy pregnancy behaviors, contraception options, and spousal education through assortative mating. Our evidence shows that the latter mechanism was effective. Another possible channel is an improvement in wife's bargaining power inside the marriage—although this may be less likely among Muslim families in Israel than among others.

Focusing on a subsample of mothers who had children above age 18 by 1995, we show that the increase in mothers' schooling increased the education of their children. While the gain in mothers' schooling was evinced mainly at the primary and early secondary levels, the gain in children's education was manifested in higher completion rates of secondary and post-secondary schooling. The effect on children's schooling, of course, also reflects the increase in father's (spouse's) education, occasioned by assortative mating. Given that the decline in fertility reduced family size, the improvement in children's education may also reflect a tradeoff effect between the number and quality of children.

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Table 1: Descriptive Statistics, 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	
	14-18	19-23	14-18	19-23	14-18	19-23	14-18	19-23
	(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 4-8 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 14-18 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 9-13 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 14-18 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 14-18 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 19-23 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 4-8 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 14-18 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 9-13 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 14-18 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 14-18 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 19-23 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 4: Differences in Fertility Trends between Treated and Control Localities for PretreatmentCohorts aged 14-23 in 1964 (1983 census data)

	Model without locality fixed effects		Model with locality fixed effects	
	Coeff.	S.E.	Coeff.	S.E.
	(1)	(2)	(3)	(4)
A. Cohort dummies model				
Treatment X Age 15	-0.414	(0.579)	-0.294	(0.559)
Treatment X Age 16	0.354	(0.540)	0.281	(0.522)
Treatment X Age 17	-0.709	(0.555)	-0.116	(0.537)
Treatment X Age 18	-0.233	(0.551)	-0.193	(0.531)
Treatment X Age 19	-0.031	(0.529)	0.316	(0.511)
Treatment X Age 20	0.217	(0.517)	0.385	(0.501)
Treatment X Age 21	-0.398	(0.571)	-0.225	(0.551)
Treatment X Age 22	0.337	(0.557)	0.687	(0.539)
Treatment X Age 23	-0.305	(0.604)	-0.151	(0.584)
Treatment	0.887	(0.356)		
B. Linear Trend Model				
Time trend	-0.241	(0.025)	-0.229	(0.024)
Treatment X time trend	-0.014	(0.046)	-0.038	(0.045)
Treatment	0.884	(0.296)	-	-

Notes: Standard errors are presented in parentheses. The dependent variable is the fertility rate. Panel A reports the coefficient of a treatment status dummy and the interaction coefficients between treatment status and year dummies. The additional regressors are cohort dummies. Panel B reports the coefficients of a linear time trend model, a treatment status dummy, and an interaction between them. The estimates in Columns 3-4 include locality fixed effects. N=2,860.

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 4-8 and 14-18 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 9-13 and 14-18 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 14-18 and 19-23 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 characteristics	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.

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 4-8 and 14-18 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 14-18 and 19-23 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 4-8 and 14-18 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 14-18 and 19-23 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 characteristics	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

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 ofCohorts aged 4-8 and 9-13 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 individual characteristics (religion dummy: Muslim or Christian), a cohort dummy for age 4-8, 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 4-8 and 14 -18 in 1964				Cohorts aged 9-13 and 14 -18 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: 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)
A. Experiment of interest:Cohorts aged 4-8 and 14-18 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 characteristics	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. Individual characteristics include a religion dummy (Muslim or

Table 10: 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 4-8 and 14-18	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 14-18 and 19-23	-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 4-8 and 14-18	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 14-18 and 19-23	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 4-8 and 14-18	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 14-18 and 19-23	-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)		
<u>Control variables</u>								
Individual characteristics	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 11: Estimated Effect of Mother's Access to Schooling on Children's Education When Mother Was Aged 18-26

	Years of schooling			Primary school			Secondary school			Academic degree		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. Experiment of interest:Cohorts aged 4-13 and 14-18 in 1964												
(N=8,127)	0.305	0.252	0.232	-0.005	-0.007	-0.005	0.051	0.042	0.040	0.027	0.025	0.023
(N mothers=3,645)	(0.131)	(0.130)	(0.129)	(0.017)	(0.017)	(0.017)	(0.026)	(0.025)	(0.026)	(0.008)	(0.008)	(0.008)
B. Control experiment :Cohorts aged 14-18 and 19-23 in 1964												
(N=2,720)	-0.136	-0.097	-0.196	0.032	0.032	0.027	-0.014	-0.006	-0.012	-0.059	-0.058	-0.056
(N mothers=1,449)	(0.288)	(0.285)	(0.283)	(0.029)	(0.029)	(0.029)	(0.045)	(0.044)	(0.044)	(0.026)	(0.026)	(0.026)
Control variables												
Individual characteristics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Locality fixed effects	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

Notes: Estimation based on 1995 census data. Standard errors are presented in parentheses. Individual characteristics include a religion dummy (Muslim or Christian) and a cohorts dummy in Panel A that indicatesCohorts aged 4-8 versus those aged 9-13.

Table 12: OLS and 2SLS Estimates of the Effect of Mother's Education on Schooling Attainment of Children Born When Their Mother Was Aged 18-26 ***(1995 census data)***

	Experiment of interest: cohorts aged 4-13 and 14-18	
	OLS	2SLS
	(1)	(2)
Years of schooling	0.102	0.387
	(0.007)	(0.231)
Primary school	0.006	-0.009
	(0.001)	(0.028)
Secondary school	0.007	0.067
	(0.001)	(0.046)
Academic degree	0.001	0.039
	(0.000)	(0.018)

Notes: Standard errors are presented in parentheses. Sample includes 8,127 children born to 3,645 mothers.

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 4-8 and 14-18	Experiment of interest: cohorts aged 9-13 and 14-18	Control experiemnt: cohorts aged 14-18 and 19-23
	(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	3,798	3,190	2,351

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 4-8 and 14-18 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 9-13 and 14-18 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 14-18 and 19-23 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 characteristics	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: 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	
	14-18	19-23	14-18	19-23	14-18	19-23	14-18	19-23
	(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