Does Increasing Mother's Schooling Reduce Fertility and Increase Children's Education: Evidence from a Natural Experiment on Arabs in Israel*

Victor Lavy and Alexander Zablotsky

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Abstract

We study in this paper the causal relationship between women's education and fertility and the transmission of human capital from mothers to children. We base our evidence 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 the Military Government of Arabs in Israel which immediately enabled a large part of the Arab population to regain access to schooling institutions. The Military Government which was in effect from 1948 imposed severe restrictions on movement and travel and therefore disrupted sharply access to schooling of residents in localities that lacked education institutions. Regaining access to schooling increased female years of schooling by 1.02 for women age 4-8 in 1964 and by 0.58 for women age 9-13 at that time. The gain for the young affected cohorts reflects an increase of 8 percent in the probability of completing primary school and of 6 percent of completing at least two years of secondary schooling. These very large effects on schooling levels led to a sharp decline in completed fertility, 0.61 children for the younger affected cohorts and of 0.47 children for the older cohorts. The implied 2SLS estimates show that an increase in one year of maternal schooling caused a decline in fertility of 0.6 children for the younger cohorts. Additional evidence that we present suggests that labor force participation, age when married, marriage and divorce rates as well as spouse's labor force participation and earnings did not play any role in this fertility decline. However, spouse's education increased sharply as well through assortative matching, therefore playing as well a major role in the declining fertility. In the second part of the paper we show that the increase in mother's schooling led also to an increase in the education of children in face of a decline in their number. The increase in schooling of children amounted to just over a third of the increase in schooling of their mothers.

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1. Introduction

In the economic model of fertility (Becker, 1960, Mincer, 1963), education increases the opportunity cost of women's time leading them to have fewer children, but also raises a woman's permanent income through earnings, tilting her optimal fertility choices toward higher quality. (Becker and Lewis 1973, Willis 1973). Second, under positive assortative mating, a woman's education is causally connected to her mate's education (Behrman and Rosenzweig 2002), so the effect of education on household permanent income is augmented through a multiplier effect. However, there are societies that experienced fertility transition without these economic forces playing a major role. For example, during half century the total fertility rate of Moslem women in Israel fell sharply, from over 9.8 children in the mid 1950's to 3.9 children in 2008.[1] During the same period, Israeli Arab women's average years of schooling increased by more than three folds, from 3 years in 1951 to over 10 years in 2008, but this change barely impacted their labor force participation and employment during the same period reaching only 15 percent in 2000 and 18 percent in 2009[2]. However, the education increase could 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). Second, education may equip girls with a better ability to process information, potentially increasing knowledge of contraception options (Rosenzweig and Schultz, 1989, Thomas, Strauss, and Henriques, 1991). Education may also improve the wife's bargaining power inside the marriage (Thomas, 1990). Moav (2005) suggests that educated mothers may have a technical advantage in producing educated children, tilting the trade off from quantity to quality children. However, there is little evidence of the importance of these channels in the absence of a meaningful increase in women's employment and opportunity cost of time.

The objective of this paper is to study the role that female education played in reducing fertility while tradition still kept women far from the labor market. In particular, we present evidence that indicates that the strong negative relationship between women's fertility and education reflects a causal effect and show that potential mechanisms such as women's labor force participation, age when married, marriage, and divorce rates did not play a major role in this fertility decline. The impact of women's education is still very large after accounting for spouse's employment and earnings though spouse's education increased very much through assortative matching and therefore likely played a major role in the decline in the demand for children. In the last section of the paper we show that the increase in mother's schooling led also to an increase in

[1] Central Bureau of Statistics's website, online tables and figures.
[2] Central Bureau of Statistics (2002), State of Israel Prime Minister's Office and Yashiv and Kasir (2009).

the education of children in face of a decline in their number. The size of this decline is 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 the Military Government of Arabs in Israel which immediately enabled a large part of the Arab population to regain access to schooling institutions. The Military Government was in effect from 1948 to 1966 over some geographical areas of Israel having large Arab populations, primarily the South (Negev), the North (Galilee), and the Center (Triangle). The residents of these areas were subject to a number of measures that placed tight controls on all aspects of life of Arabs in Israel. These measures included severe restrictions on movement, and permits from the military governor had to be procured to travel more than a given distance from a person's registered place of residence.[3] Although the Military Government was officially for geographical areas and not people, its restrictions were enforced only on the Arab residents of these areas. The travel restrictions were revoked in October 1963 following the 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 from many political parties, including the Herut right wing party, to annul the Military Government of Israeli Arabs. This effort led in 1966 to the complete cancelation of The Military Government and the Arab citizens of Israel were granted the same rights as other citizens.

The Military Government restricted de facto access to schools for children in localities and villages in which there were no primary or secondary schools while not affecting access in localities in the Military Government regions that had schools already. It therefore created two zones in Arab populated areas, one in which school attendance required some travel which then became difficult or even impossible and one where schooling access was not interrupted at all. In the latter group we distinguish between Arab localities that were under the Military Government

[3] A recent historical episode where similar restrictions were imposed on perceived 'enemy' populations is the Japanese-American internment and forced relocation by the United States government in 1942 of approximately 110,000 Japanese Americans and Japanese residing along the Pacific coast of the United States to camps called "War Relocation Camps," in the wake of Japan's attack on Pearl Harbor. President F. Roosevelt authorized the internment with an 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 used to declare that all people of Japanese ancestry were excluded 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 rescinded entirely. Another example is the arrests in camps of Germans in England during World War II.

and the Arab population living in predominantly Jewish cities that were also placed initially (1948) under military but later were exempted de facto from some of the restrictions.

The change in the end of 1963 could benefit children that were not too old to attend primary school or children who completed primary schooling and could now enroll in secondary schooling. 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. Similar identification strategies were 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 competition on pupil's academic achievement (Lavy, 2010). We allow the affected cohorts to include children from age 4 to age 13 in 1964 while older cohorts are used as controlled experiments. We use data from the 1983 and 1995 census. In 1983 the affected cohorts were 24 to 35 years old which permits studying the effect of education on early age fertility. In 1995 the affected control cohorts were already 36 to 45 years old, allowing estimating the effect of education on completed fertility.

The evidence we present in the paper suggests that regaining access to schooling starting 1964 increased female years of schooling by 1.02 for the women at age 4-9 in 1964 and by 0.58 for women at age 9-13 at that time. These educational gains are associated with a large increase in the probability of woman completion of primary schooling and also of at least some years of secondary schooling. Much smaller effects are estimated for men a finding that suggests that the travel restrictions did not limit as much the access to schooling of boys.

These very large effects on schooling levels of girls led to a sharp decline in fertility of 0.6 children for the younger affected cohorts and of 0.47 children for the older cohorts. The implied 2SLS estimates show that an increase in one year of maternal schooling caused a decline in fertility of 0.6 children. However, this fertility decline is not accompanied by discernible changes in women age of marriage, divorce rate, labor force participation, and their spouse's employment, earnings and age when married. However, the education of the spouse did increase through assortative marriage matching, though not directly through the natural experiment of renewed access to schooling, and therefore could 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 time to these women did not change much during the same period. It also seems that changes in education, employment and earnings of the spouses of affected women did not play a significant role in the fertility decline. We also provide some evidence that the increase in mother's schooling impacted positively the education of her children though this intergenerational transmission of human capital could involve channels other than the pure effect of schooling.

An extensive literature documents associations between education and fertility and children schooling (Strauss and Thomas 1995). However, whether these associations represent causal relationships has been the subject of debate. Breirova and Duflo (2002) and Osilii and Long (2008), use school expansion as a source of exogenous drop in the cost of schooling and find a negative causal effect of education on fertility in Indonesia and Nigeria. Black, Devereux, and Salvanes (2004) find that increases 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 (2008), 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 of adolescent girls reduced early fertility among girls who were likely to drop out of school. This evidence obviously suggests lack of consensus about the causal effect of women's education on fertility. Similar lack of consistency is evident from the evidence about the causal effect of the parental education children schooling. Berhman and Rosenzweig, 2002, use twin datasets and assume that the child rearing abilities of twins are identical and find that maternal schooling does not have a causal effect on a child's education but that father's schooling does. Plug (2004) uses adoptee datasets and assumes that the process of adoption is random and concludes that mother's education has a positive effect on children schooling. Sacerdote (2007) uses Korean-Americans adoptees who were placed with American families and find a much lower estimate of the effect of mother's education on child schooling. Black et al., 2005 use school reforms in Norway to control for parents' unobserved endowments and find no evidence of a causal relationship between parent education and child education. Oreopoulos, Page, and Stevens (2006) use a similar methodology and find that an increase in parental education attainment in the US reduces the probability that a child repeats a grade. 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 and find that mother's education has a positive effect on child cognitive outcomes. Studies that focused on primary school

construction programs in Taiwan (Chou, Liu, Grossman and Joyce 2003) and Indonesia (Breierova and Duflo 2004), and on college openings in the United States (Currie and Moretti 2003), finds that there is a causal effect of mother education on child health.

The remainder of the paper is organized as follows. In Section 2, we provide a brief review of recent studies of the effect of mother schooling on fertility and in Section 3 we describe the political and policy context of the Military Government and the mechanisms by which it could have affected 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 on 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

In broad terms, education may affect a woman's fertility and child-investment choices through either income or learning (Michael 1973). Education increases a woman's income stream through both the labor market and the mating market, the latter through assortative mating. In addition to the income channel, education may improve a woman's stock of knowledge regarding contraceptive technologies or healthy pregnancy behaviors, either because it augments her knowledge directly (i.e., educational curricula are important), or because it improves her ability to absorb and process information generally. We next describe each of these mechanisms in turn.

The income channel operates through the well-documented effect of education on labor earnings. For example, Angrist (1995) shows for both Arab women and men in Israel an average wage premium of about 20 percent for 13-15 years of schooling and of over 44 percent for 16 or more years of schooling. The causal effect of an additional year of education on annual earnings was around 43.3 percent. The notion that an exogenous increase in a woman's income may lead to reduced fertility is present in the earliest neoclassical model of fertility (Mincer 1963, Willis 1973). In these models, households do not value children per se but what Willis terms "child services"—the product of the number of children and the average quality of those children. A key idea is that production of child services is time-intensive relative to other activities for the woman. As the value of a woman's time rises, she generally substitutes away from consumption that is highly time intensive (Becker 1965) and hence desires fewer children. These predicted effects of education on fertility map naturally into predicted effects on child quality. Assuming child services are a normal good, falling fertility in response to rising income requires that child

quality be an increasing function of income. Cross-price effects such as these were first emphasized by Becker and Lewis (1973) and Willis (1973).

Predictions based on the income channel are further sharpened by positive assortative mating or the tendency for men and women of similar education to marry (Behrman and Rosenzweig 2002). Under this type of stratification, an exogenous increase in a woman's education leads to a mate of higher education, further increasing household permanent income through a multiplier effect. In addition to the income channel, the literature has stressed the role of education in augmenting an individual's stock of health knowledge. With respect to fertility, Rosenzweig and Schultz (1989) provide evidence that a woman's education explains ability to effectively use contraception. With respect to infant health, Thomas, Strauss and Henriques (1991) show that education predicts a woman's ability in regards to information acquisition and processing. One of the most frequently cited examples is smoking (Currie and Moretti 2003). Through anti-smoking campaigns in schools or health class, children could learn about the dangers of smoking and be discouraged from adopting the habit. Glewwe (1999) argues that the most important mechanism for knowledge gain is not directly via curricula; but rather the skills obtained in school facilitate the acquisition of health knowledge. Grossman (1972) formalizes these ideas by viewing education as a productivity shifter in the household production function for health.

Empirically there is a strong positive correlation between education and delay in the onset of fertility, and a strong negative correlation between education and the number of children had (see Strauss and Thomas (1995) for a review of the literature). However, this may not indicate a causal relationship running from education to fertility, both due to the potential for reverse causality, and to possible omitted variables: girls who drop out of school early are also probably those most at risk of having children early.

Several studies have tried to address this identification issue. Some studies exploit school expansion as a source of exogenous drop in the cost of schooling. Breirova and Duflo (2002) use a large school construction program in Indonesia to construct instruments for years of education of both women and men. The instrumental variable estimates suggest that women's education does not reduce total fertility but increases the age at marriage and decreases the number of children born before the woman turned 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, and the effect of secondary education on early fertility could potentially be much larger, if part of the effect of secondary education is to increase a young woman's opportunity cost of time.

Similar results of schooling expansion have been found for secondary schools in developed countries; Black, Devereux, and Salvanes (2004) find increases in education resulting from compulsory schooling laws decreased teenage pregnancy in the U.S. and Norway and that the effects in the two countries were of highly similar magnitude. Also in Norway, Monstad, Propper and Salvanes (2008) find increases in education did not lead to decreased fertility but did lead to childbirth at older ages. In contrast, McCrary and Royer (2008), using exact cutoff dates for school entry, find that young women who get extra schooling because they are born a few days before the cutoff for school entry 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 around the same age (for example 16, the earliest age permitted). This is a different conceptual experiment than asking girls (or giving them the opportunity) to stay one more year in school, say, from age 16 to age 17. When we compare two girls who both dropped out at 16 but were born on either side of the September 1 cutoff, one has one more year of schooling than the other by virtue of having had started school earlier. The two sets of results can thus 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 most 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 for the last three years of primary school (from 2003 to 2005) were 2.4 percentages point 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 impact of access to education of adolescent girls on early fertility, at least among girls who are likely to drop out
of school.

Kirdar, Tayfur and Koç, (2009) estimates the impact of schooling on the timing of marriage and early fertility in Turkey. The source of exogenous variation in schooling is the extension of compulsory schooling in Turkey in 1997. The findings indicate that at age 17 –three years after the completion of compulsory schooling –, the proportion of women who are married

drops from 15.2 to 10 percent and the proportion of women who have given 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 from the delay in the timing of marriage. After a woman is married, schooling does not have an effect on the duration until her first-birth.

3. The 1948-66 Military Government and Restricted Movement of Arab Citizens of Israel

During the 1948 War of Independence, the Israeli Provisional State Council decided to enforce a special governmental military authority over the areas populated by Palestinian Arabs.[4] This population was the enemy against which the Israeli fought its independence war and overnight they became citizens of the new Jewish state. The Military Government, which began in October 21, 1948 and ended in 1966, was to be based on the mandatory defense regulations of 1945 that were enacted by the British Government in Palestine which was under British Mandate at the time. These regulations were very similar to those enacted in England during World War II and were annulled at the end of the war (Jiryis, 1968). The provisional government in Israel decided in 1948 to leave in place all Mandatory laws, including the "Emergency Defense Regulations". From that point on, until the cessation of the enforcement of these regulations, the Military Government served as the dominant Israeli governmental authority among the Israeli-Arab minority. Most importantly for our study, the Military Government required the Arab citizens of Israel to obtain special permits in order to travel day or night out of their villages and towns. From 1963 a special travel permit was needed only for night travel. Although the Military Government was imposed on all Arab citizens of Israel, those who lived in mixed cities such as Haifa and Jaffa enjoyed greater freedom since the mid 1950s, largely because it was more difficult to enforce the travel restrictions on Arab citizens living in predominantly-Jewish cities. At first, the military government worked together with the Ministry of Minorities which was responsible for humanitarian aspects of the Arab population, but this ministry was cancelled in 1949. Therefore, the military government remained the only responsible authority on all affairs of the Arab population.

During the first years of the State, but mainly after 1957, some criticism and reservations were raised among the Israeli public, the Knesset (Israeli Parliament) and the MAPAI ruling party concerning the necessity for the Military Government. The main issue of criticism was that it was

[4] The material in this section is based on Bauml (2002).

believed that the Military Government damaged the democracy of Israel and it led to many initiatives to terminate the Military Government. In February 1962 and in February 1963, four political parties proposed to the Israeli parliament to cancel the status of the Military Government. One of the four proposals was by the right-wing party led by Menachem Begin. All proposals were rejected by a close margin in a parliamentary vote. 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 change in the Military Government. In a speech to the Knesset in October 1963, Eshkol announced that the Arab population will no longer need travel permits and the government reinstated their right to move freely in the country.[5] This change has removed one of the most burdening restrictions that affected profoundly the daily life of Arabs in Israel since the creation of the state. In 1966 the Military Government was cancelled altogether except for some specific restrictions that were kept such as travel to the nuclear plant in Dimona and travel to near the border with Jordan at the Arava valley and to Sinai.

3a. Military Government and Restricted Access to Schooling

The Arabs who lived under the military administration and were confined to specific geographic-areas were severely limited in their ability to travel in pursuit of educational and training opportunities and to compete for better jobs in the labor market (Okun and Friedlander, 2005). This meant that Arab children who resided in villages and towns that had no schools were not able to travel to schools elsewhere and therefore lost access to primary and/or secondary schooling. In the appendix we list all these localities and also those that had schools and therefore their population kept free access to schooling institutions. The travel restrictions limited sharply the access to primary and secondary schools in areas under the military government that did not have such schools. Table A1 lists the Arab localities in Israel as of 1948, which were under the Military Government and travel restrictions and the number of primary and secondary schools in each locality in 1964/5, the first year for which such information is available (Central Bureau of Statistics 1966). Five of these were cities (Akko, Haifa, Lod, Ramla, Tel Aviv) with mostly Jewish population and a minority of Arab population. These five cities had Arab primary schools and three of them also had Arab secondary schools. However, as noted above the Arab populations in these cities were exempted from the Military Government and travel restrictions since mid 1950's. There were five more small villages which were also exempted from the

[5] The population of five Arab villages that are located very much near the border were not included in this new free movement policy. Another limitation that was not cancelled prohibited all Arabs from entering certain areas intended for Jewish settlements and defined as military zones.

Military Government because a majority of their population was other non Arab minorities (Druze and Circassians) and we exclude them from our analysis. We are left therefore with 49 Arab localities, 23 of which did not have a primary school or a secondary school by 1964/65. In each of the other 26 localities there was at least one primary school and in 8 of them there was at least one secondary school. We therefore include in the treatment group all localities that were under the Military Government, and did not have a primary school and a secondary school. The control group includes all the localities under the Military Government that had at least one primary school. Since some of the treated localities were relatively close to a control locality, we do not exclude the possibility that some children from a locality that did not have a school travelled or even walked to the nearest locality that had a school. However, the travel restrictions made this possibility much more difficult, expensive and even dangerous.

Another point of importance to note here is that the control population experienced exactly the same travel and other military government restriction as the treated group. This implies for example that the populations in both type of localities experienced the same limitations in access to hospitals and labor markets outside the locality. To tighten even more the comparability of the control and treatment areas in terms of access to social and economic services other than access to schools, we will show evidence based on a comparison group that excludes the five largest cities/towns (Nazareth, Tamra, Shefar'am, Tayibe and Umm al-Fahm). This change makes the control and treatment groups much more similar in all other aspects except in terms of having or not having schools in the locality. For robustness check we also use the Arab population in the mixed cities listed in Table A1 as control group. This group however will have much better pre-1964 characteristics (years of schooling for example) and much lower fertility. We will show that the results based on the three alternative control groups are very similar, a result important in terms of allowing to reject the idea that mean reversion and differential time trends confound the estimates that we present in this paper.

4. Data

The main source of data used here is the 20% public-use micro-data samples from the 1995 and 1983 Israeli censuses, linked with information about the locality and regions in the country that were under the Military Government from 1948 until 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 collected on a fairly detailed long-form questionnaire similar to the one used to create the PUMS files for U.S.

censuses.[6] The micro-data of the 1983 Census of Population and Housing is available in one version that includes all variables from the Census' extended questionnaire and data from the short questionnaire for those households selected in the sample. Particularly important for our research is that it contains the identification of small geographic areas and localities and also detailed data on age, occupation, family income, marriage, education and residential and household details. Due to requirements of statistical confidentiality, the available 1995 census data file that includes detailed geographic codes down to code of locality (localities with 2,000+) includes other variables that have been extensively grouped. Age is reported in 5 year groups. Years of schooling is reported as follows: 0, 1-4, 5-8, 9-10, 11-12, 13-15, 16+. Education is also reported by highest certificate: Never studied, did not get any certificate, primary or intermediate school, secondary school, matriculation (Bagrut), post secondary certificate (not academic), bachelors degree, masters degree or above. The number of children born (reported only for mothers) is grouped as follows: 0, 1, 2, 3-4, 5-7, 8+. A version of the 1995 census that does not include detailed locality code includes all detailed values (not grouped) of these demographic and education variables. However, since we needed the detailed code of locality in order to assign individuals to treatment and control we were constrained to use the grouped demographic data. For years of schooling, and number of children in 1995 we have used the mid points in each range. However, as noted the 1983 census data fully report the values of each variable and except for completed fertility we can assess and compare the results based on the 1983 detailed data and the 1995 grouped data.

Table 1 presents the 1983 and 1995 means of demographic and economic outcomes for two cohorts groups, those of age 14-18 and age 19-23 in 1964. We will claim below that these cohorts were not likely affected by the travel policy change in end of 1963. The comparison of means of the control treatment groups shows that the treated population had lower socio-economic outcomes. For example, mean years of schooling in 1983 of the age 14-18 cohorts is 5.79 in control group and 4.36 in the treated group. The fertility mean in 1983 of the age 14-18 cohorts is 4.8 in control group and 5.5 in the treated group, a difference of 0.7 children. In 1995 the same difference is 1.0, reflecting the gap in completed fertility. However, the gaps between treated and control based on the age 14-18 cohorts are very similar to the treatment-control differences based on the age 19-23 cohorts. For example, mean years of schooling in 1983 of the

[6] Documentation can be found at 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 includes residents of dwellings inside the State of Israel and Jewish settlements in the occupied territories. This includes residents abroad for less than one year, new immigrants, and non-citizen tourists and temporary residents living at the indicated address for more than a year.

age 19-23 cohorts is 4.16 in control group and 2.71 in the treated group, the difference being 1.44 which is identical to the respective difference for the 14-18 age cohorts. Also, the fertility treatment-control difference in 1995 is 1.03 for the age 14-18 cohorts and it is 1.10 for the 19-23 cohorts. The stability of these gaps suggests therefore that there were no dynamical differences between treatment and control over the period from 1948 to 1963, a pattern that is important for the identification strategy we use and to which we turn in the next section. Finally, as noted above we also use for robustness check a subset of the control group that excludes the population of the largest five cities/towns. This comparison group has a valuable advantage of being identical to the treatment group in terms of pre-1964 characteristics and outcomes means.

5. Identification, Estimation and Basic Results

The age in 1964 and the locality of residence jointly determine an individual's exposure to regained access to schooling due to the cancelation of travel restrictions in end of 1963 that previously governed the life many of the Arab citizens of Israel from 1948 to 1966. Israeli children until the mid 1970's attended primary school (from 1st to 8th grade) between the ages of 6 and 13 and secondary schooling (9^{th} to 12^{th} grade) between the ages of 14 and 18. We expect that children of primary school age or at early ages of secondary school in 1964 stood to benefit from regaining access to schooling institutions. Therefore, all children born in 1950 or later were 14 years old or younger in 1964 when the travel restrictions were removed, therefore could benefit from their revocation. Older cohorts could not since 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, and therefore we expect the effect on this group to be larger than on 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 areas under the Military government that also did not have secondary and full primary schools. Therefore locality of residence in 1964 is a second dimension of variation in the exposure to the change in access to schooling. After controlling for locality and year of birth fixed effects, we use the interactions between a dummy variable indicating the age of the individual in 1964 and whether or not her locality of residence regained access to schooling following the lift of travel restriction in end of 1963 as exogenous variables, and as an instrument for an individual's education. This identification strategy can be presented in an interaction terms analysis of the first stage relationship between the education (S_{ijk}) of an individual i, who reside in locality j in year t, and his exposure to the program:

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

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_j is a locality of residence fixed effect and A_j denotes a locality that was exposed to treatment (being under the Military Government rule and not having a primary nor a high school). In this equation we measure the time dimension of exposure to the program with 22 year-of-birth dummies. Individuals aged 19-23 in 1964 form the control group, and this dummy is omitted from the regression. Each coefficient δ_l can be interpreted as an estimate of the treatment on a given cohort. We expect that the coefficients δ_l should be 0 for $1 > 14$ and start increasing for l smaller than some threshold (the oldest age at which an individual could have been exposed to treatment and still benefit from it).

Figures 1 plot the δ_l coefficients when for sample size consideration and estimations precision we group cohorts and impose the same δ_l within each of the following groups: age 2-3, 4-5, 6-7, 8-10, 11-13, 14-16, 17-19, and age 20-21. Results based on separate regression for each group cohorts of birth yield very similar pattern. Each dot on the solid line is 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. Recall that the comparison group in Figure 1 includes only the localities that were under the Military government with exactly the same travel and other restrictions as the treated localities but they had primary and high schools by 1964/65. In Figure 1 the estimated coefficients are small and not statistically different from 0 for the 14-16, 17-19 and 20-21 age groups, it than jumps to about 0.70 at age 11-13, reaches 1.0 for ages 8-10 and remain at this level for the youngest age cohorts 5-7 and 2-4. The average estimated coefficient for ages 14-18 is about zero and it is not significantly different from zero. Each of the four estimates in the younger age groups is significantly different from zero and more precisely estimated for cohorts age 10 and younger.

The evidence presented in Figure 1 suggests, as expected, that the treatment had no effect on the education of cohorts not exposed to it (older than 13 years in 1964), and it had a positive effect on the education of younger cohorts. These figures show that the identification strategy is reasonable and that the travel policy change that led to a change in access to schooling had an effect on education. This suggests that we can use the unaffected older cohorts as a comparison group for estimating the treatment effect on affected cohorts. We therefore impose the restriction that the treatment effect is equal to 0 for cohorts older than 13 in 1964.

Before proceeding it is interesting to estimate the same model as equation (1) while the dependent variable is fertility measured by the number of women born to women by census day in 1983. The plot of the estimated effect on fertility in Figure 1 is almost a mirror image of the plot of the estimated effects on years of schooling. For ages 14-16 and 17-18 the estimated coefficients are negative but not significantly different from zero. For age group 11-13 it is about -0.55, and then it decreases to about -0.70 and remains at this level for all three younger age group. For all the four age group younger than age 14 in 1964 the estimates are significantly different from zero.

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

Given these results we move to use both the 1983 and the 1995 census data to estimate the effect of the travel restrictions change in 1963 on schooling and fertility. In 1983 our youngest treated group is age 24-28 and the oldest is age 29-33. In 1995 the youngest treated group is age 34-38 and the oldest is age 39-43. These various treated groups enable us to estimate the effect of treatment on women of various age groups, including for a cohort group that is old enough (over age 40) to have definitely completed their education and most likely also finished making children. For estimation precision the first treatment group that we examine includes individuals who in 1963 were age 4 to 8 and the second older affected group including those age 9 to 13 in 1963. We also define two unaffected groups, age 14-18 and age 19-23 in 1963. Using these age groups we first present in Table 2 their means of years of schooling for different cohorts by exposure to the regained access to schooling, which we use to perform an analysis of an uncontrolled difference in differences estimates. In panel A, we compare the schooling attainment of individuals in the control group (women who were of age 14-18 in 1964) to women who were exposed the longest to treatment (they were 4 to 8 in 1964) in affected and unaffected areas. In both cohort groups, means of years of schooling are higher in areas that were not affected by the travel restrictions. But note that years of schooling have increased in both treated and control areas but they increased much more in localities included in the former group. For example, based on the 1983 census data, average schooling in the treatment group increased from 4.4 for the older group to 8.2 in the younger group, a difference of 3.75 years of schooling. In the control group the average schooling increased from 5.8 for the older group to 8.9 in the younger group, a difference of 3 years of schooling. The precise 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. When we perform the same analysis based on the 1995 census data (presented in columns 4-6 of panel A) we obtain an increase of 1.078 years of schooling (se = 0.297).

In panel B of Table 2 we present a similar analysis for the older cohorts affected by the regained access to schooling. The comparison group is yet again the closest in age cohorts that were practically not exposed to this change. The mean of years of schooling is still higher in areas that were not affected by the regained access to schooling. As in the comparison presented in Panel A, years of schooling have increased in both groups but more so in treated communities. However, the relative estimated gain based on the 1983 census data is only 0.49 years of schooling, about two thirds of the respective average gain for the younger cohorts. The differences and in differences estimate for the schooling gain for the older cohort based on the 1995 census data is 0.605 years of schooling, again about two thirds of the respective estimate for the younger affected age cohorts.

The above two simple difference in differences estimate can be interpreted as the causal effect of the treatment under the assumption that in the absence of the regained 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 as the pattern of increase in education could vary systematically across areas. For example, there could be mean reversion which can confound the estimated effect of interest. However, an implication of the identification assumption can be tested because the schooling of individuals aged 14 or older in 1964 could not be affected by the removal of the travel restrictions and renewed access to schooling in regions governed by the military administration. The increase in education between cohorts older than 14 years in 1964 should not differ systematically across affected and unaffected areas. In Table 2, panel C, we present one example of such control experiment where we contrast cohorts at age 19 to 23 in 1964 to cohorts at age 14 to 18 in 1964. The estimated difference in differences is 0.026 (sd=0.344) years of schooling based on the 1983 census data and -0.384 based on the 1995 census data. We also analyzed a control experiment based on older cohorts and obtained similar results. These results provide some suggestive evidence that the difference-in differences estimates presented in panels A and B are not driven by inappropriate identification assumptions and in the next section we present more precise results after conditioning the regression on individual characteristics and locality fixed effects.

In Table 3 we present 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 4-8 age cohorts based on the 1983 census data is 0.122 (se=0.07). The respective difference among treatment and control unaffected cohorts age 14-18 is 0.677 (se=0.166), implying a difference and differences estimate of a decline of 0.555 (se=0.155) children for 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 age group 14-18 in 1964 to 5.088 for the 9-13 age group and to 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 the travel restrictions for women age 4-8 in 1964 is -0.727 (se=0.195) and for women of age 9-13 in 1964 it is -0.543 (se=0.227). The changes estimated based on the later census naturally are closely related to changes in completed fertility because the youngest treated age group is almost 40 years old by census day on 1995.

The causal interpretation of the estimated decline in fertility due to the opening 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 based on the 1983 census data is -0.193 (sd=0.263) and based on the 1995 census data it is -0.092 (se=0.285). These falsifications like estimates support the assumption that the decline in fertility would not have been otherwise systematically different in affected and unaffected areas.

We can use the difference in differences estimates of the change in education and fertility to compute the Wald estimate of the effect of mother's schooling on fertility. This estimate is obtained as computed for each affected cohort based on 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 older affected cohorts in 1983 is -0.74 (-0.555 divided by 0.751) and -0.57 (-0.279 divided by 0.490).

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

The simple difference in differences estimates can be generalized to a regression framework in order to allow for controls to be added thereby improving estimation efficiency and the 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 the Military Government.

Table 4, columns 1-3, presents estimates of equation (1) for three subsamples. In panel A, we compare children aged 4 to 8 in 1964 with children aged 14 to 18 in 1964 based on 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 3. Recall that this specification controls only for the cohort of birth dummy of the population aged 4-8 in 1964 and for a dummy indicator for localities without schools until 1964. The treatment indicator is the interaction of these two variables and its estimates show that the treatment increased the education of female children aged 4-8 in 1964 by 0.751 years 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 correlated with the removal of travel restrictions. In column 2 we present estimates adding 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. In column 3 we add locality fixed effects as controls and the estimated DID are 0.738 and 1.018, for 1983 and 1995 respectively, which are almost identical to the uncontrolled DID in column 1. The estimated standard errors do not change much as we add these controls so all three estimates are equally precise. The similarity of these three alternative estimates, especially the first and the third, are reassuring that there are no omitted local or regional effects that potentially can confound the treatment effect of interest.

Panel B of Table 4 shows the results of the cohort age 9-13 in 1964 and the control group is again children aged 14-18 in 1964. Here as well we report results based on 1983 census data and on 1995 census data. The estimated effect of treatment on the older cohorts is lower as expected in comparison to the estimated effects obtained from the sample younger cohorts. Based on the latest census, the 1995 simple DID estimate is 0.605, just over half the size of the respective estimate for the young cohorts. The controlled DID estimates presented in columns 2-3 are 0.533 and 0.575 respectively. Here again all three estimates are very similar suggesting once more that omitted confounding factors do not affect our simple DID estimates.

Panel C of Table 4 presents the results of the control experiment based on comparing the cohorts aged 14-18 to the cohorts aged 19-24 in 1964. If, before the removal of the travel restrictions, education had increased faster in affected areas, panel C would show (spurious) positive coefficients. But the impact of "treatment" is very small or even negative and never significant. Each of the coefficients in panel C is statistically different from the corresponding coefficients in panel A and from two of the corresponding estimates in panel C. For example, the control experiment estimates in the first row and third column of panel C is 0.039 (se=0.291), practically zero and much lower than the respective estimate presented in panel A and in panel B. Although this is not definitive evidence (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.

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

Evidence relevant for the interpretation of the estimates to be presented below about the effect of education on fertility and children schooling is at what level of education was the policy change effective. In Table A2 we present estimates of the reduced form equation (2) where 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 (high) school diploma (12 years of schooling), a matriculation diploma and a post secondary degree. Estimated equation includes individual controls and locality fixed effects and is based on 1995 Census data.

In the first column of Table A2 we present the estimated reduced form effects for the 4-8 age cohorts. The effect is positive for attainment of each of these thresholds but it is precisely estimated only for the first three thresholds. The estimates indicate that the policy change that allowed 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 not its completion. On the other hand the evidence presented in column 2 for the older affected cohort suggests that the gain for the 9-13 age-group originated mainly from an increase in post primary schooling but these effects are not precisely measured. In column 3 we present estimates based on the control experiment and overall these evidence show mostly negative estimates for all educational attainment thresholds but the estimates in most cases are not statistically different from zero.

The effect of change in access to schooling on men's education

Before presenting the results about the effect of mother's education on fertility it is important to note that the travel policy change could have affected also the education of Arab men. In appendix Table A3 we present results of estimating equation (2) based on the men sample. The estimates are positive but they are much smaller than the estimate obtained from the sample of women. Based on the 1995 census data, the travel policy change led to an increase of 0.531 years of schooling for the 4-8 age cohorts, about half the size of the effect on women. The effect on the 9-13 age cohorts is much smaller, 0.203 years of schooling and given its estimated standard error it is also statistically insignificant. However, unlike the case of the estimates for women, the control experiment presented in panel C suggest also positive effects on older cohorts which indicate a possible positive trend in men's years of schooling in treated area that preceded the timing of the lift of travel restrictions. This trend weakens the case for a causal interpretation

of the estimated effects on men, suggesting that they reflect mainly a pre-existing trend and not the effect of the policy change. To further insure that the effect of mother education on fertility is not confounded by and independent though correlated change in spouse education we will estimate the results based on a sample of women who are married to older men, in particular we will restrict the female 4-8 sample to include only women whose husbands are older than 8 and in a second sample even older than 13. In this sample we are certain that husbands were not affected by the lift of travel restrictions and therefore our estimated effect of mother's education will be net of the effect of the education of her spouse.

The smaller and less significant effect on men versus the strong effect on women's schooling is not surprising because it is expected that in a traditional society such as Arab Moslems in Israel travel restrictions will be much more binding for women because alternative mode of access to schooling such as walking long distances daily or living with relatives or in boarding schools in order to get better access to school are less likely for girls than for boys. It is interesting to note that Gould, Lavy and Paserman (2010) also report that low quality childhood environment had a large negative effect only on girls' education in families of traditional Jewish families in Israel during the 1950's and 1960's while not affecting at all the schooling attainment of boys in the same families. The gain in years of schooling from access to better childhood environment estimated in this study was almost 0.75 years of schooling, very similar to our estimate for Arab women in this study. Another possible explanation for large effect on women with practically no effect on men is related to the expectation that women will not participate in the labor market and therefore they 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 the resources on boys schooling because they are all expected to participate in the labor force and get a return on their education.

5c. Effect of Limited 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 is sufficient to estimate the reduced form impact of the travel policy change. Additionally, if we assume that the change in access to schooling had no effect on fertility other than by increasing educational attainment, one can 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 the fertility of women. We therefore estimate:

(3) $F_{ijt} = \alpha + a_{lj} + \mu_k + (A_j Y_i) \delta + \varepsilon_{ijt}$

where F_{ijt} is the number of children in 1995 of an individual i, who was born in a locality j in year t, within the Military Government zone and without free access to schooling because of the travel restrictions. Results of the estimates of the parameter δ based on the three specifications of equations (2) are presented in Table 4, columns 4-6. In panel A, we compare fertility of women who were at age 4 to 8 in 1964 with fertility of women aged 14 to 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 lower the estimate to -0.533. When we add the locality fixed effects to the regression estimated, the estimate is practically unchanged. The estimates obtained based on the 1995 census data and these three specifications are marginally higher than the above reported estimates. However, the 1995 reduced form estimates based on the third specification (with individual controls and locality fixed effects) is -0.609 (se=0.188), which is very similar to the 1983 respective estimate (-0.539). This estimate implies that the removal of travel restrictions reduced the completed fertility of these women by just over half a child.

In Panel B we show DID estimates based on using the cohorts age 9-13 as the treatment group. The estimated effect of the improved access to schooling on the older cohorts is as expected lower than the effect on the younger cohorts. 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 90 (se=0.181). The latter estimate is about 40 percent lower than the reduced form estimated effect obtained for the younger cohorts. Given that the reduced form effect on education for the older group is also 50 percent lower than the effect on schooling of the younger cohorts, we should expect that the 2SLS estimate of the effect of education on fertility obtained from the young and older age cohorts will 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 still lower than the respective 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, before the removal of the travel restrictions, fertility had decreased faster in affected regions, panel C would show (spurious) negative coefficients. But the impact of "treatment" 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

with a standard error of 0.271, which does not allow to reject that it is not statistically different from zero.

5d. IV Estimates of the Effect of Mother's Education on Completed Fertility

Estimates of equation (2) and (3) are the 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 differences in fertility across cohorts 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 being in the young cohorts in 1964 and the exposure to the regained access to schooling in the locality of residence can 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 4.

The 2SLS results are presented in Table 4, the OLS estimates in column 7 and the 2SLS results in column 8. The OLS estimate for the youngest affected cohorts based on the 1983 data, presented in first row of panel A, is negative, -0.240, and measured very precisely (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 biased upward, implying a lower sensitivity of fertility to changes in mothers' education. In the second row of panel A we present the results for the young cohort based on the 1995 census data and the 2SLS estimate here is -0.598, marginally lower than the estimates obtained from the 1983 data. The later reflect a relatively short run effect as the affected cohorts are less than 30 years old at survey date while the former estimate (based on 1995 census data) reflects the effect of education on completed fertility as all affected women are already close to or older than 40 years at survey date.

We also estimated an IV model based on a sample that includes the two unaffected older groups, namely the 14-18 and 19-23 included simultaneously in the sample as part of the control group. The importance of such an enlargement of the control group is that it allows estimating a specification that includes a differential time trend for the treatment and control groups for the years that preceded 1964. It is equivalent to triple difference in differences estimation. These results are presented in Table A5. The 2SLS estimated effects of education on fertility based on

this model are almost identical to those reported in Table 4. For example, OLS estimate for the youngest affected cohorts (4-8) is -0.321 and the respective 2SLS estimate is -0.705 (sd=0.326). Note also that the first stage and the reduced form estimates reported in Table 5 are also very similar to the respective estimates reported in Table 4. This result indicates that differential pre-treatment trends by treatment and control groups and potentially mean conversion do not derive our results of the strong positive effect of mother's education on fertility.

Panel B in Table 4 and in Table A4 presents the results based on the experiment of the older affected cohorts. These results are qualitatively similar to those obtained for the younger affected cohort but they are less precisely measured. When we replicate the estimation separately for cohorts 9-10 and 11-13 based on 1983 Census data we get more precise estimates for the former group, consistent with the evidence presented in Figure 1 where we have shown that the effect on the 11-13 age group is smaller and barely significant. It is also important to note that while the 2SLS estimates for older and young affected cohorts are similar, the respective first stage and reduced from estimated effects are different but in a way that their ratio (and the 2SLS estimate) are still similar.

To further substantiate the evidence that our estimated effect of mother schooling is not confounded by the direct effect of the education of the father, we present in Table 5 evidence based on two subsamples based on the age of the spouse in 1964. The first sample is restricted to women of age 4-8 in 1964 whom their husbands were older than 8 in that same year and it includes 60% of the full sample of women. In Table A3 we have shown that the change in travel restriction did not have any effect on the schooling of men age 9-13 (37% of the full sample). The second sample is restricted to women whom their husbands were older than 13 in 1964 and it includes 35 of all women in this sample. These group of men could have benefitted 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 5 is 0.683 (se = 0.312). It is very similar to estimated 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 5 are also almost identical to their respective estimates in Table 4. Finally, the estimates that we obtained from the second restricted sample 9based on the spouse age) are also very similar to the respective estimates reported in Table 4. These results support the interpretation of the our estimates of the effect of mother schooling on fertility as causal, net of the direct effect of her spouse's schooling.

We conclude this section by first noting that our 2SLS estimated effect of education on completed fertility (-0.598) represents an effect size only marginally higher than the estimates

presented in Leon (2004) based on 1950-1990 US Census data. This study reports instrumental variable estimate of -0.35 using changes in state compulsory schooling laws as a source of exogenous variation in women's education. We also note here that the IV estimate we obtain is larger (more negative) than the OLS estimate (similar direction of bias is reported in Leon, 2004) though we cannot reject the hypothesis that the IV estimate is not different from the OLS estimate as the later falls within the confidence interval of the IV estimate. One explanation for this direction of the bias of the OLS estimate is that we are estimating a LATE and that the population affected most by the travel restrictions have higher sensitivity of fertility to changes in female education. Another argument leading to the same result is that those reacting to the renewed access to schooling are those who are more concerned about their daughters' education. Another explanation of the high LATE estimate is that the effect of primary schooling on fertility is larger than schooling gains in secondary or college schooling. A totally different explanation for the higher IV estimate could be related to the fertility hypothesis regarding minority group status and fertility (Goldscheider and Uhlenberg, 1969, Ritchey, 1976). This hypothesis suggests that a deprived minority group that also experience discrimination will adopt higher fertility rate as a strategy to strengthen the group against an external threat. Keyfitz Flieger (1990) uses this hypothesis to explain the high fertility rate in Northern Ireland and of the black and white population in South Africa. Anton and Meir (2002) suggest that the fertility of Moslems in Israel reflects a survival strategy that is aspired by radical nationalism. However, if radicalism and education are correlated but the later does not cause the former it could lead to a downward bias in the OLS effect of education on fertility. Having provided these possible explanations, we reiterate again the IV estimate is not significantly higher than the OLS.

5e. Mediating Factors of Effect of Education on Fertility

As discussed in the introduction and in section 2, education can affect fertility through various channels including labor force participation and wages that feature in the shadow cost of children, through age when married and also marriage and divorce rates. Through assortative matching education can also affect fertility via spousal outcomes such as their education and labor market outcomes. To examine these potential mechanisms we have estimated IV equations similar to equation (4) when the outcome is one of these own demographic and labor outcomes and the labor market outcomes of the spouse. These results are presented in Table 6 and overall they suggest that the increase in women's education did not have any 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, but the IV estimates are all negative but very imprecisely measured and therefore practically they are not different from zero. The no-effect of education on labor force participation could be because the gain in schooling reflects mainly more years of primary schooling which in a traditional society may induce little or no change in market participation. Recall also that the average female labor force participation is very low to start with and that employment of Arab women, especially Moslems, is mostly restricted to the locality without any 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 of women's education and age when married, are positive and highly significant, but the IV estimates show no such relationship for both outcomes. The estimated effect of education on these two outcomes is small. Given their estimated standard errors they are not statistically different from zero. On the other hand the effect of education on the probability of divorce is small and insignificant both in the OLS and IV estimation.

We can summarize the above evidence by concluding that the increase in education did not significantly affect any of the mechanisms through which female schooling could have reduced fertility and that we could have examined with our available data. Most important is the zero-effect of education on mothers' labor force participation which is 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 the opportunity cost of time of the mother. Education must have affected fertility through the other channels; one such potential mediating factor is spouse selection to which we turn next.

5f. Spouse's Education and Earnings

In Panel B of Table 6 we present OLS and IV estimates of the effect of women's education on spouse education, labor force participation and earnings. The spouses (husband) 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 for our 4-8 age cohorts the spouses might have been affected by the annulment of the travel restrictions. On the other hand, the spouses of the affected older age cohorts (9-13) were too old to be affected by the regained access to schooling. However, as we have shown in appendix Table A3, the travel policy change had little effect on men in general and therefore we can conclude that the spouse of the women in our samples were not affected directly by the travel policy change. These facts help interpret the findings that the

higher female education led to marriage with more educated men: one more years of schooling enabled women to marry a man with an additional of half a year of schooling. Note that the OLS and IV estimates of this effect are almost identical. This large magnitude of assortative mating suggest that some of the reduction in fertility of women in the young and older affects cohorts is also due to higher schooling of their husbands.

Finally, we note that the OLS effect of mother's schooling on spouse's labor force participation and earnings (Table 6) are positive and significant for both cohorts affected and in both censuses data sets but the respective IV estimates are much smaller, sometimes change signs and always not significantly different from zero. Therefore it seems that these two outcomes are not mediating channels through which the increase in mother's education led to a reduction in her fertility.

6. Robustness Checks and Threats to Identification

The identifying assumption for estimating the causal effect of mother's schooling on fertility is that the removal of the travel restrictions did not have a direct or indirect effect on fertility but through its effect on opening access to schooling. This assumption can be violated if the removal of travel restrictions in end of 1963 was accompanied by other measures that could affect fertility. Such measures could include, for example, government investments in health services, prenatal care and centers in the Arab sector in the country which could have directly affected Arab women's fertility. The evidence we have suggest that this possibility (that the exclusion restrictions are not satisfied) is very unlikely. First, note that it is only the travel restrictions that were lifted in end of 1963 while the Military Government, including all its other aspects, was abolished officially only in 1966. Second, there were no public investments and other type of government initiatives to improve social and economic infrastructure in the Arab sector in Israel until later in the 1980's. This is partly due to the deep recession of the Israeli economy in 1966 and the heavy economic burden imposed by the 1967 war against Egypt, Jordan and Syria and the 1973 war against Egypt and Syria. There were of course improvements in health, education, economic and social services that benefitted the Arab population throughout the years since 1948 but there was not a jump in this trend in 1963 or in 1966. Further, even if such a change would have occurred in 1964, in order to confound our results it would have had to be stronger or biased towards the localities that regained access to schooling in 1963 and to affect only the young children and not those who are only few years older. For example, if in the affected localities there was relatively more construction of pre- and post- natal health clinics following the lift of travel restrictions in 1963, we would have no reason to expect that this

improvement in health services will affect only the fertility of those of age 11-13 in 1964 and not of those at age 14-16 in the same year. Individuals in both of these age groups will get married few years after 1963 and could be both affected by this improvements and the effect on these women could last many years until they are about 40 years old. This time pattern of the potential effect of such improvements in health services makes the exact timing of their placement almost irrelevant. Therefore the reasonable conclusion is that such confounding factors even if they existed should not have led to discontinuity in the fertility of Arab women in an around age 12-13 in 1964.

Another possible threat to identification is that the various difference-in-differences estimates may reflect conversion to the mean. To alleviate this concern we present estimates based on two alternative control groups. The first is a sub sample of the original control group after deleting from it the population of the five largest localities in the sample. This modification produces a control group that is more similar to the treatment group in terms of characteristics and outcomes of unaffected cohorts of both groups (see appendix Table A6). The second alternative comparison group is the Arab population in mixed cities. Recall that this population was not subject to the travel restrictions and had both primary and secondary schools in the locality. The mean characteristics and outcomes of this group for the older cohorts (age 14-18 and 19-23) are much better than of the treatment group (see appendix Table A6). The results based on these two alternative comparison groups are very much similar to those reported in Table 4. These alternative estimates are presented in the first two panels of Table 7. I report here results based on the youngest affected cohorts only (ages 4-8) and on the 1995 census data. The first panel report estimates when the control group is the original one less the observations coming from the largest five localities, causing the sample of the control group to fall by half. The first stage effect is 0.953, the reduced form effect is -0.775 and the 2SLS estimate is -0.814 (se=0.342). Note that the respective OLS estimates are lower than those reported in Table 4. This is an expected result because the population we eliminated from the control group (those in large cities) are more educated and they also have fewer children for reasons other than education and therefore when they are excluded from the sample the OLS estimate become less negative.

Panel B in Table 7 reports estimates when the control group is the Arab population in mixed cities. The 2SLS estimate is -0.485 (se=0.140) while that in Table 4 is -0.598.

The fact that two alternative sets of DID estimates, one that is based on a comparison group that has much better characteristics and outcomes than the treated group and a second that is based on a comparison group that has marginally better characteristics and outcomes yield qualitatively the same results is reassuring given the possibility that the DID estimates are biased

because of regression to the mean or due to differential time trends in unobserved heterogeneity between treatment and control.

Panel C in Table 7 presents estimates based on a sample that includes only individuals who are living at the time of the 1995 Census at their place of birth. This sample includes 72 percent of the original sample. The first stage, the reduced form and the 2SLS estimates are almost identical to the respective estimates reported in Table 4. For example, the 2SLS estimate in Table 7 is -0.602 (se=0.258) while that in Table 4 is -0.598. This result is not surprising because the very few who are not leaving at their place of birth have most likely moved to a nearby village or town that shared the same treatment status as their place of birth.

7. Effect of Mother's Education on Children Schooling

In this section, we assess whether the change in mother's schooling affected the educational outcomes of the next generation. We use in this part of the paper only the 1995 census data to allow children to advance to an age where their education reflect as close as possible completed schooling. For the same reason we focus on the human capital of children who were born to mothers of age 18-26. This selection rule guarantees that we include in the sample the oldest children which are more likely to reach already post secondary schooling or even college age. It also assures that the children in this sample have a comparable mother's age at birth. To allow for a meaningful sample size we pull together 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 their children provide evidence that is far from conclusive. There is a strand of literature aimed at identifying total causal effects of the education of parents on the education of their children via twin datasets (Berhman and Rosenzweig, 2002), adoptee datasets (Plug, 2004) or school reforms (Black et al., 2005) to control for parents' unobserved endowments. These studies assume that the child rearing abilities of twins are identical (Berhman and Rosenzweig, 2002) or, in the case of adoptee datasets, that the process of adoption is random (Plug, 2004) or that there is no selection going on which would be comparable to inheritable abilities.

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

study finds little evidence of a causal relationship between parent education and child education. However, in some specifications they find a positive causal impact of mother's education on son's 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 to 15 using the 1960, 1970 and 1980 U.S. Censuses. They study U.S. law changes (that occurred in different states at different times) to identify the effect of parents' educational attainment on children's school performance (as proxied by grade-for-age). They find that an increase in parental education attainment of 1 year reduces the probability that a child repeats a grade by between 2 and 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; because of student protests, 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 for that year and more students were able to attend college. This led to significantly higher wages for the students who were then able to attend college, with an increase of about 14%. In addition, these returns were passed on to the next generation; grade repetition declined significantly for the children of the affected cohort. 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, achievement, grade repetition, and obesity. They find that, among children aged 7-8, an increase in mother's education by one year increases math standardized test performance by 0.1 of a standard deviation and reduces the incidence of behavioral problems. Page (2009) uses cohort level variation in schooling levels induced by the G.I. Bill in order to identify the intergenerational transmission of education. She argues that this variation was due to the timing of the draft and not unobservable individual characteristics or underlying trends. She finds that a one year increase in father's schooling reduces the probability that his child repeats 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 increased compulsory schooling or in college through GI benefits--does not affect the estimates.

Since maternal education can affect children schooling through several different channels and the intensity of these channels may not be the same for all levels of education nor for all subpopulations, the effect of education on child schooling may differ across studies. For example, Currie and Moretti (2003) use college openings to study the effect of maternal education on infant

health. The women whose schooling attainment at motherhood is affected by college openings are those women with 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 schooling. The unit in the sample is now the child and not the mother and there are some mothers with more than one child in the sample. We use the following educational attainment as outcome measures: completed years of schooling at census day, completing at least primary schooling, at least secondary schooling, and obtaining a post secondary academic degree (college or other diploma). The sample includes 5,094 mothers to 10,847 children. Using this sample we estimate the following model:

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

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

In Table 8 we present estimates from the three specifications of the reduced form relationship between mother's schooling and her children education. For each specification and child education outcome we present estimates based on the quasi-experimental contrast between children of affected mothers from cohorts age 4-13, unaffected cohorts age 14-18 and from the control experiment of contrasting two unaffected groups of cohorts, 14-18 and 19-23.

Several results stand out in Table 8. Gaining access to schooling in 1964 of Arab mothers caused an increase in schooling of their children. This gain is reflected in increased attainment in secondary and post-secondary schooling both of which are reflected in an increase in total completed years of schooling. The effect on the probability that the children of affected mothers will complete secondary schooling is modest, an increase of 4 percent. The effect on the likelihood of completing a post secondary degree is an increase of 2.3 percent.

In Table 9 we present the OLS and 2SLS estimates of the effect of mother schooling on children educational outcomes based on using the 14-18 cohorts as a control group. The OLS estimates are all positive and highly significant with large t-values. The 2SLS estimates are higher than the OLS estimates except for primary schooling but they are much less precisely estimated. This suggests that the OLS estimates are biased downward by a large factor.[7] For example, the

[7] Maurin and McNally (2008) estimating the effect of parental schooling on child grade repetition also report IV estimates (-0.33) that are four time larger than the OLS estimate (-0.08). Oreopoulos, Page, and Stevens (2006) who report a significant negative effect of parental education attainment on the probability that a child repeats a grade, report also IV estimates are more negative than the OLS ones. A similar pattern is reported in Carneiro, Meghir and Parey (2007).

OLS estimate of the effect on completing at least secondary schooling is 0.009 while the 2SLS estimate is 0.067 and a somewhat smaller gap exists between the two respective estimates for the effect on obtaining at least a matriculation diploma.

An interesting and important question addressed in the literature is the size 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 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 affected cohorts. Since the mothers of the children in our sample of analysis are from both affected groups we can use the average of the two group specific effects, 0.80 years of schooling. Since the reduced form gain of children schooling is 0.387, the ratio is 0.48, marginally higher of what one would have expected given the large literature on the intergenerational correlation in economic status: the central tendency of estimates of the intergenerational correlation coefficient is between 0.3 and 0.4 (Solon, 1999). However, the estimated effect we report here is more in line with evidence of studies that used instrumental variable estimation to study the effect of parental schooling on child education.

8. Conclusions

We study in this paper the effect of female education on fertility and on the schooling of children. This is an important question with implications for economic development and growth and for social change. The evidence to date on this question is still mixed and inconclusive and we extend it in few directions with several unique contributions. The policy change/natural experiment that we use provides a large change in education of women, a gain of over a year of schooling for affected children who were young enough to benefit from the opened access to primary schools. This is a large enough change that allows detecting precisely its effect on fertility and on education of the next generation. The estimated effect on fertility is large as well and it explains some of the dramatic decline in fertility of Arab Moslem population in Israel. The results we present 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 has perhaps more scope for generalization also because the Arab population we study is mostly Moslem and at the time it had characteristics well representative of the populations of many other developing countries. Another point worth emphasizing is that the effect of education on fertility that we estimated does not operate through the opportunity cost of time of the mother as labor force participation of Arab women did not increase very much over

the last 50 years. We also find very little change over this period in other demographic variables such as marriage, age of marriage and divorce. Therefore, the increase in female education have impacted Arab women fertility through other channels such as knowledge and ability to process information regarding fertility options, healthy pregnancy behaviors, contraception options and spouse education through assortative mating. We have shown evidence that the latter mechanism was effective. Another possible channel is improved wife's bargaining power inside the marriage though this may be less likely in Moslem families in Israel.

9. References

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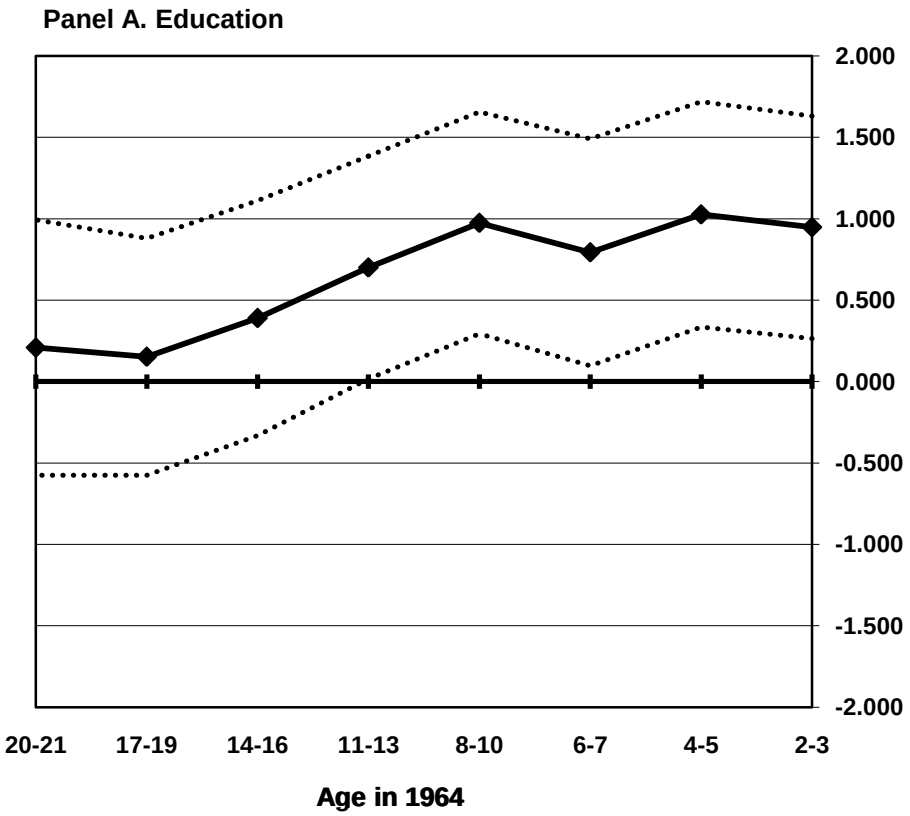
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Figure 1. Coefficients of the interaction of age in 1964 and accsess to schooling in the education and fertility equation



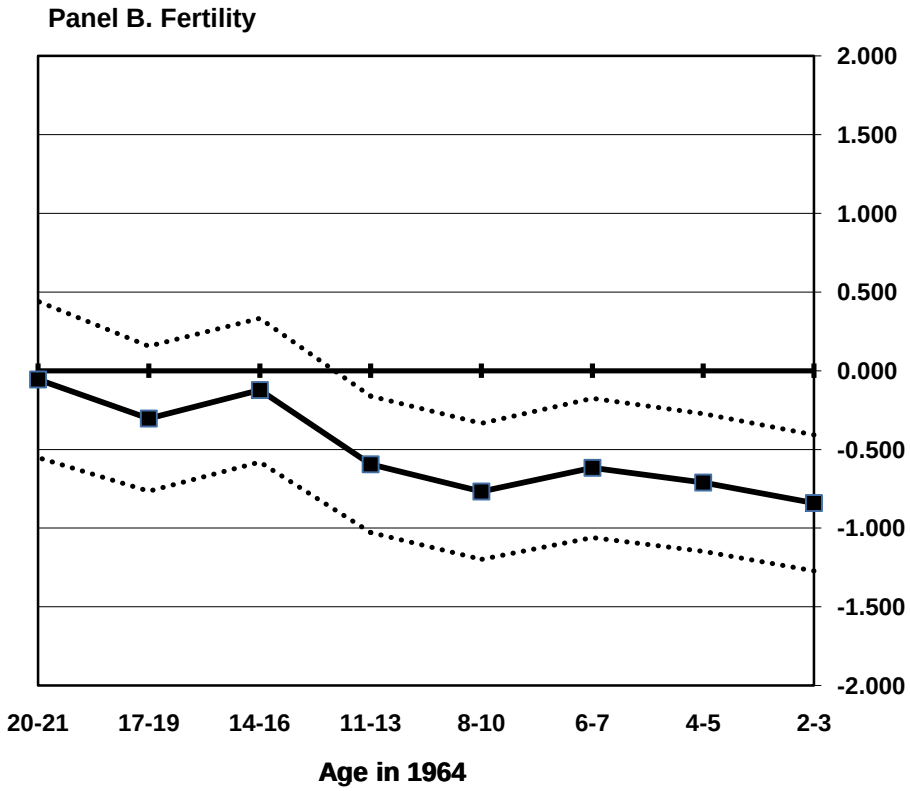


Table 1: Descriptive Statistics, 1983 and 1995 Census Data

	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)
Labour 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 of 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)
Labour 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 parenthesis. The fertility measure is a woman's total number of live births until the Census year. Log monthly earnings is measured in Israely shekels in prices of the Census year. Number of observations is presented for all variables except for age of marriage and log mounthly earnings of spouse. Some women in the sample do not have data on these varibles and therefore 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		
	Treatmen	Control	Difference	Treatmen	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment of Interest						
Cohorts age 4 to 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 age 14 to 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 Age 9 to 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 age 14 to 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 age 14 to 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 age 19 to 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 parenthesis.

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		
	Treatmen	Control	Difference	Treatmen	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment of Interest						
Cohorts age 4 to 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 age 14 to 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 Age 9 to 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 age 14 to 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 age 14 to 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 age 19 to 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 parenthesis.

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

	Years of Schooling			Fertility			Fertility	
	Fist Stage			Reduced Form			OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Experiment of Interest: Cohorts age 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 age 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 age 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 parenthesis. Individual characteristics include religioun dummy (Moslem or Christian). In 1983 Census data columns (2), (3), (5), (6) and (8) include cohort dummies.

Table 5: 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	
	Fist 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 age 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 age 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 age 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 age 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 parenthesis. Individual characteristics include religioun dummy (Moslem or Christian). In 1983 Census data columns (2), (3), (5), (6) and (8) include cohort dummies

Table 6: OLS and 2SLS Estimates of the Effect of Education on Woman's Labour Force Participation, Marriage, Age of Marriage, Divorce and Spouse's Outcomes

	Cohorts age 4-8 and 14 -18 in 1964				Cohorts age 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								
Labour Force Participation	0.033	-0.139	0.038	-0.040	0.031	-0.035	0.034	-0.007
	(0.002)	(0.070)	(0.002)	(0.034)	(0.002)	(0.063)	(0.002)	(0.051)
Marriage	0.006	0.055	0.007	-0.011	0.009	-0.068	0.005	-0.061
	(0.002)	(0.042)	(0.001)	(0.024)	(0.001)	(0.063)	(0.001)	(0.051)
Age of Marriage	0.172	-0.107	0.231	0.506	0.169	-0.091	0.171	-0.490
	(0.015)	(0.230)	(0.026)	(0.472)	(0.017)	(0.331)	(0.030)	(1.084)
Divorce	-0.000	0.004	-0.001	-0.009	-0.001	0.002	-0.001	-0.028
	(0.000)	(0.006)	(0.000)	(0.008)	(0.000)	(0.011)	(0.001)	(0.020)
Panel B: Spouse Outcomes								
Years of Schooling	0.482	0.579	0.513	0.537	0.493	0.464	0.444	0.538
	(0.016)	(0.223)	(0.017)	(0.283)	(0.016)	(0.285)	(0.018)	(0.449)
Labour Force Participation	0.006	0.006	0.012	-0.018	0.007	-0.019	0.015	-0.007
	(0.001)	(0.017)	(0.002)	(0.033)	(0.001)	(0.026)	(0.002)	(0.056)
Ln (Monthly Earnings)	0.035	0.067	0.039	-0.034	0.036	0.092	0.031	0.001
	(0.003)	(0.042)	(0.003)	(0.058)	(0.003)	(0.076)	(0.003)	(0.102)

Notes: Standard errors are presented in parenthesis.

Table 7: Estimated Effect of Female Education on Fertility Based on Alternative Control Groups and Samples (1995 Census Data)

	Years of Schooling			Fertility			Fertility	
	Fist Stage			Reduced Form			OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Original Control Group Excluding Five Largest Localities								
Experiment of Interest	0.976	0.841	0.953	-0.895	-0.828	-0.775	-0.106	-0.814
(N=2,550)	(0.337)	(0.324)	(0.314)	(0.220)	(0.215)	(0.213)	(0.012)	(0.342)
Control Experiment	-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	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	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 4 Restricted to Persons Born in Current Locality								
Experiment of Interest	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	-0.770	-0.662	-0.672	0.138	0.074	0.007	-	-
(N=1,714)	(0.414)	(0.384)	(0.377)	(0.326)	(0.313)	(0.311)		
Control Variables								
Individual 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 parenthesis. Individual characteristics include religioun dummy (Moslem or Christian). In 1983 Census data columns (2), (3), (5), (6) and (8) include cohort dummies. Experiment of interest: cohorts age 4-8 and 14-18 in 1964. Control experiment: cohorts age 14-18 and 19-23 in 1964.

8: Estimated Effect of Mother's Access to Schooling on Education of Children Born When Their Mother Was 18-26 *(1995 Census*

	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 age 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 age 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: Standard errors are presented in parenthesis. Individual characteristics include religion dummy (Moslem or Christian) and cohorts dummy in panel A that indicates cohorts of age 4-8 versus 9-13.

Table 9: OLS and 2SLS Estimates of the Effect of Mother's Education on Schooling Attainment of Children Born When Their Mother Was 18-26 ***(1995 Census data)***

	Experiment of Interest: Cohorts age 4-13 and 14-	
	OLS	2SLS
	(1)	(2)
Years of Schooling	0.098	0.387
	(0.007)	(0.231)
Primary School	0.006	-0.009
	(0.001)	(0.028)
Secondary School	0.009	0.067
	(0.001)	(0.046)
Academic Degree	0.002	0.039
	(0.000)	(0.018)

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

Table A1: Arab Localities and schools in 1963

Locality	Group	Primary schools	Secondary schools
AR'ARA	Treatment	0	0
ARRABE	Treatment	0	0
BI'INA*	Treatment	0	0
BIR EL-MAKSUR*	Treatment	0	0
DEIR AL-ASAD*	Treatment	0	0
DEIR HANNA*	Treatment	0	0
FASSUTA	Treatment	0	0
JUDEIDE-MAKER	Treatment	0	0
KABUL*	Treatment	0	0
KAFAR KANNA	Treatment	0	0
KAOKAB ABU AL-HIJA*	Treatment	0	0
MAZRA'A	Treatment	0	0
MUAWIYYE**	Treatment	0	0
MUQEIBLE	Treatment	0	0
MUSHAYRIFA**	Treatment	0	0
MUSMUS**	Treatment	0	0
NAHEF*	Treatment	0	0
REINE	Treatment	0	0
SAKHNIN	Treatment	0	0
SHA'AB*	Treatment	0	0
TUBA-ZANGARIYYE	Treatment	0	0
ZALAFA**	Treatment	0	0
ZEMER	Treatment	0	0
ABU SINAN	Control	1	0
BAQA AL-GHARBIYYE	Control	2	1
EIN MAHEL	Control	1	0
I'BILLIN	Control	1	0
IKSAL	Control	1	0
JALJULYE	Control	1	0
JATT	Control	1	0
KAFAR MANDA	Control	1	0
KAFAR QARA	Control	2	1
KAFAR QASEM	Control	1	0
KAFAR YASIF	Control	1	1
MA'ALOT-TARSHIHA	Control	1	1
MAGHAR	Control	3	0
MAJD AL-KRUM	Control	2	0
MI'ELYA***	Control	1	0
NAZARETH	Control	13	2
PEQI'IN (BUQEI'A)***	Control	1	0
QALANSAWE	Control	2	0
RAME	Control	1	2
SHEFAR'AM	Control	3	0
TAMRA	Control	3	0
TAYIBE	Control	4	1
TIRE	Control	3	1
TUR'AN	Control	1	0
UMM AL-FAHM	Control	5	0
YAFI	Control	1	0
AKKO	Mixed	2	1
HAIFA	Mixed	2	1
LOD	Mixed	1	0
RAMLA	Mixed	1	0
TEL AVIV - YAFO	Mixed	2	1

* 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, Schools Census, 1963

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

	Sample		
	Experiment of Interest: Cohorts age 4-8 and 14-18	Experiment of Interest: Cohorts age 9-13 and 14-18	Control Experiemnt: Cohorts age 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 Diploma	-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 parenthesis.

ıble A3: Estimated Effect of Access to Schooling on Male Education *(1995 Census dat*

	Years of Schooling		
	(1)	(2)	(3)
Panel A: Experiment of Interest			
Cohorts age 4 to 8 and 14 to 18 in 1964	0.514	0.466	0.531
	(0.263)	(0.260)	(0.255)
Observations: 4,440			
Panel B: Experiment of Interest			
Cohorts age 9 to 13 and 14 to 18 in 1964	0.076	0.069	0.203
	(0.288)	(0.284)	(0.279)
Observations:3,733			
Panel C: Control Experiment			
Cohorts age 14 to 18 and 19 to 23 in 1964	0.508	0.458	0.434
	(0.310)	(0.305)	(0.299)
Observations: 2,872			
Control Variables			
Individual Characteristics	no	yes	yes
Locality Fixed Effects	no	no	yes

Notes: Standard errors are presented in parenthesis.

Table A4: Estimated Effect of Female Education on Fertility: First Stage, Reduced Form, OLS and 2SLS Estimates

	Years of Schooling			Fertility			Fertility	
	Fist Stage			Reduced Form			OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Experiment of Interest: Cohorts age 4-8 and 14-23 in 1964								
1983 Census	0.745	0.685	0.718	-0.635	-0.525	-0.506	-0.321	-0.705
(N=5,631)	(0.234)	(0.259)	(0.252)	(0.152)	(0.174)	(0.173)	(0.009)	(0.326)
1995 Census	0.877	0.908	0.999	-0.755	-0.641	-0.619	-0.158	-0.620
(N=4,880)	(0.249)	(0.280)	(0.273)	(0.172)	(0.200)	(0.198)	(0.009)	(0.251)
B. Experiment of Interest: Cohorts age 9-13 and 14-23 in 1964								
1983 Census	0.484	0.544	0.500	-0.358	-0.345	-0.312	-0.187	-0.625
(N=4,958)	(0.263)	(0.282)	(0.273)	(0.182)	(0.204)	(0.200)	(0.010)	(0.500)
1995 Census	0.404	0.529	0.548	-0.571	-0.504	-0.456	-0.113	-0.831
(N=4,272)	(0.274)	(0.296)	(0.289)	(0.200)	(0.226)	(0.223)	(0.011)	(0.572)
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 parenthesis. Individual characteristics include religioun dummy (Moslem or Christian). In 1983 Census data columns (2), (3), (5), (6) and (8) include cohort dummies.