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

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Preliminary and Incomplete

June 17, 2010

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 1.42 for women age 4-9 in 1964 and by 0.75 for women age 9-13 at that time. These gains reflect an increase of 16.4 and 6.1 percent increase in the probability of completing primary school for the two affected cohorts, respectively. These very large effects on schooling levels led to a sharp decline in completed fertility, 0.6 children for the younger affected cohorts and of 0.4 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 0.5 children. Additional evidence we present suggest that labor force participation, age when married, marriage and divorce rates as well as spouse education and earnings did not play a role in this fertility decline. In the second part of the paper we show that the increase in mother schooling led also to an increase in the education of children in face of a decline in their number.

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 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 from the market place. 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 education and family income. In the last section of the paper we show that the increase in mother schooling led also to an increase in the education of children in face of a decline in their number.

[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).

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), North (Galilee), and the central (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. 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 Heirut 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, theoretically if not always in practice, granted the same rights as Jewish 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 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 enrol 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 languish 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 2 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 40 to 53 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 1.42 for the women at age 4-9 in 1964 and by 0.75 for women at age 9-13 at that time. The gain for the respective cohorts of men is 0.85 and 0.45 years of schooling. These educational gains are associated with a 16.4 percent increase in the probability that a woman in the young affected cohorts completed primary school and a 6.1 percent increase in the same probability for the older affected cohorts.

These very large effects on schooling levels led to a sharp decline in fertility of 0.6 children for the younger affected cohorts and of 0.40 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 0.5 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. Moreover, for the older affected cohorts the education of the spouse did not change either, largely because they were too old to be affected by the natural experiment of renewed access to schooling. However, schooling of spouses of the younger affected cohorts did increase because they were young enough in 1964 and therefore were directly exposed to the improved access to schooling. 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 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 mothers' 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 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. In Section 3, we discuss our identification strategy, and after describing the data in section 4, we present the results of our estimation of the effect of schooling on fertility in Section 5. Section 6 presents evidence on the intergenerational transmission of human capital and discusses a variety of important interpretation issues. Section 7 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 behaviours, 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.

Kırdar, 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.[3] The Military Government, which began in October 21, 1948 and ended in 1966, was to be based on the mandatory defence regulations of 1945 that were enacted by the British Government in Palestine, a British Colony 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 (Gereiss, 1968). The provisional government in Israel decided in 1948 to leave in place all Mandatory laws, including the "Emergency Defence 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 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

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

permits and the government reinstated their right to move freely in the country.[4] 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. The 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 Fridlander, 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 completely the access to secondary schools because there were very few such Arab schools, none of them in affected areas. For example, in 1962/63 there were only 10 Arab high schools in the whole country, none of which was given the status of a State School because of their low passing rate on matriculation exams (The Statistical Abstract of Israel, 1963, pp: 627-634). The exact number of primary schools in the Arab populated regions in Israel during the Military Government is not known exactly. Government documents indicate that there were 47 primary schools in 1948 and that that number increased gradually to 137 in 1962/63. However, based on current Ministry of Education records about the date each school was opened there should have been only 45 Arab primary schools during the 1950's and only a handful in areas under the Military Government. Further, the majority of these schools had classes up to 4th grade and therefore completion of all 8th grades of primary schooling involved attending a school in a different locality. This evidence is consistent with what is known about the quality of education in the Arab sector of Palestine under the British Mandate (The Arabs in Israel, State of Israel, 1955). There were no compulsory schooling laws, and in more than a third of the Arab communities there were no primary schools. Most of these schools had multi-grade classes with only one or two classrooms and children remained in school only until 4th grade. Only in the very large villages and in cities there were complete schools that offered all grades. Secondary schools

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[4] 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.

existed only in the large cities and Kindergarten, special education schools and extra-curricular activities were completely missing in the Arab sector of Palestine. During and after the 1948 war, many teachers left the Arab villages and as a result the education system was impoverished from much of its human capital. The Arab education system therefore needed restructuring and expansion but the young new Jewish country lacked the needed resources at the time (Kopelevitch, 1973). As an immediate solution to the scarcity of teachers, many unqualified staff, some even with only primary education schooling, were recruited. Many of the new and old teachers also lacked any pedagogical training. Additional factors that contributed to the lack of teachers during the first decade and a half after 1948 were the Military Government restrictions on travel that made it difficult for teachers to commute to schools and security considerations that disqualified some veteran teachers to continue working in schools because they were viewed as a security threat (Zameret, 2003).

We therefore include in the treatment group all localities that were under the Military Government, did not have a secondary school and that according to Ministry of Education records these localities did not have either a complete full eight grade primary school. There were 78 such localities which were inhabited by about 65 percent of the Arab population of Israel at the time. The rest resided in two types of localities: those that were under the Military Government but had primary and secondary schools and those that were mixed cities with a majority of Jewish population. We will demonstrate evidence first based on using only the first type of localities which we will term as control group I. We will than add to this group the second type of localities and will term this extended list of localities as control group II. It is important to show evidence based on control group I because the population in this comparison group experienced exactly the same travel and other restriction as the treated group. The importance of using also control group II is that it will allow us to demonstrate that it is the lack of schools in treated areas in conjunction with the travel restrictions and not the Military Government per se that is deriving our results.

3. 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 secondary and/or primary 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.[5] 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 includes 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 constraint 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 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 two defined control groups and the treated group shows that the treated population had lower socio-economic outcomes. For example, mean year of schooling of the age 14-18 cohorts is 7.4 in control group I, 6.8 in control group II and 4.3 in the treated group. 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. This 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 next.

[5] 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.

4. 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 enrol 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:

$$(1) \qquad S_{ijt} = a + \alpha_l + \mu_t + \sum_{l=2}^{21} \left(A_l T_{il}\right) \delta_l + \varepsilon_{ijt}$$

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 22-23 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 and 2 plot the δ_l when for sample size. Because of consideration and estimations precision we group cohorts in pairs of adjacent ages (2-3, 3-4, 5-6, and so on until 20-21). Results based on regression for each year of birth yield very similar pattern. Each dot on the solid line is the coefficient of the interaction between a dummy for being a in given age in 1964 and the dummy indicator of exposure to treatment. The 90 percent confidence interval is plotted by dashed lines. The comparison group in Figure 1includes 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 1948. We note this group as control group I. In Figure 2 the comparison group includes also the mixed localities with Jews and Arabs residents. This later group was also under the Military Government but with less strict mobility restrictions. We note this extended control group as control group II.[6] In Figure 1 the estimated coefficients fluctuate around 0 until age 14-15 and it jumps to about 0.80 at age 12-13 and starts increasing reach its highest value at over 1.00 for the 2-3 age group. The average estimated coefficient for ages 14-21 is about zero and each of the four averaged estimates is not significantly different from zero. Each of the six estimates in the younger age groups is significantly different from zero. Figure 2 shows a very similar pattern, as these estimated coefficients fluctuate at low values and around 0 until age 14-15 and start increasing at age 12-13. Each the four coefficients for ages 14-21 is not significantly different from zero and again the estimate jumps, to over 1.00, for the 12-13 age cohorts. The average for cohorts age 2-12 is 1.20 and each of the six estimates in this group is significantly different from zero.

The evidence presented in Figures 1-2 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 and estimate the following equation:

(2) $$S_{ijt} = \alpha + a_{lj} + \mu_k + (A_j\, T_i)\, \lambda + \varepsilon_{ijk}$$

We estimate this equation separately for each pair of treated cohorts (2-3, 4-5, 6-7, 8-9, 10-11,12-13) while including in each round of estimation the same control group comprising individuals

[6] Table 1 present the means of outcomes for the treatment, control I and control II groups for the age cohorts that were 14 years old or older in 1964. There is a gap in favour of the two control groups but this gap is very stable over the various age cohorts.

aged 14 to 18 in 1964. Columns 1-3 in Table 2 (using control group I) and Table 3 (using control group II) show the coefficients of the treatment effect, λ, of each of the six pairs of affected cohorts in three specifications. The first is without any controls, amounting to a simple difference in differences estimate, the second including controls for religion and cohort of birth and the third including also locality fixed effects. In all three columns, in Table 2 and in Table 3, the estimated effects are positive and they are precisely measured for cohorts age 10 or younger in 1964. All coefficients in both tables are significantly different from 0 from age 2 to age 8. The estimates of simple difference in differences (presented in column 1) are higher but not very different in most cases from the estimates from the specification with individual controls and locality fixed effects which are presented in column 3 of each of these tables. The estimated effect of treatment on years of schooling is highest for the youngest cohort, 1.3 years same in Table 2A and in Table 3, and it is lowest for the oldest treated cohort, 0.6 in Table 2 and 0.5 in Table 3.

4a. Effect of Mother's Education on Fertility

The same identification strategy can be applied to estimate the effect of removing barriers 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 reduce 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.

We first discuss the reduce form estimates. 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:

$$(3) \qquad F_{ijt} = \alpha + a_{lj} + \mu_k + (A_j\, T_i)\, \theta + \varepsilon_{ijk}$$

where F_{ijt} is the number of children in 1983 of an individual i, who was born in a locality j in year t, within the Military Government zone, some with and some without free access to schooling because of the travel restrictions. Results of the estimates of the parameter θ based on the three specifications of equations (3) are presented in Tables 2-3, columns 4-6. All the estimated treatment effects on fertility presented in both tables are negative and significantly different from zero for age 9 and below. The highest reduce form effect is for the youngest cohort, a decline of over one child, and the lowest effect estimated is for the oldest cohort, less than 0.2. Recall that this evidence is based on non-completed fertility since the youngest mothers in the sample are 26 years old.

It is very important to notice that the estimates presented in Table 2 and in Table 3 are very similar because it means that there is no difference between the two groups that form the control group. We have seen from Table 1 that the two groups had not too dissimilar level of schooling for the older unaffected cohorts and the results of Tables 2-3 show that this is also corrected for the younger cohorts of age 2-13 in 1964. The conclusion that we can draw from this similarity is that the existence of the Military Government in areas that had schools did not place their population in disadvantage in terms of investment in education in comparison to the Arab population who lived in mix cities with practically no Military Government. Therefore we can also conclude that the increase in schooling caused by the annulment of travel restriction in treated localities resulted from the renewal of access to schooling and not because of any other changes related to the Military Government after 1964 or 1966 when it was cancelled altogether.

4b. Instrumental Variable Estimates of the Effect of Mother's Education on 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) $F_{ijt} = \alpha + l_{lj} + \mu_k + S_{ijt}\, \mu + \eta_{ijk}$

where l_{lj} and μ_k denote locality-of-birth and cohort-of-birth effects, respectively. Ordinary least-squares (OLS) estimates of equation (4) may lead to biased estimates if there is a correlation between η_{ijk} 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 use as an instrument for equation (4). This instrument has been shown to have good explanatory power in the first stage presented in Tables 2-3.

The OLS and 2SLS results are presented in columns 7-8 of Tables 2-3. The OLS estimates are negative and significantly different from zero for all age cohorts. Remarkably, the size of this estimate does not vary much and in any systematic pattern across age cohorts. For example, the estimated OLS effect in Table 3 for age 2-3 is -0.080 and for age 12-13 it is -0.081. The 2SLS estimates are all negative and they are significantly different from zero for age 8-9 and younger. They estimated for the very youngest affected cohort is highest and its size implies that an increase of one year of schooling caused a decline in fertility of about one child. This is a very large effect but we defer the discussion on this issue to the next section where we will present evidence on the effect of education on completed fertility. We also should note that the 2SLS estimates are much larger (in absolute term) suggesting that the OLS estimates understates the magnitude of the causal relationship of interest. First we should note here that other studies also found similar direction of bias and size of the OLS estimate versus the IV effect of mother's schooling on fertility. For example, Leon (2004) reports based on US data an OLS estimate of mother's years schooling on fertility of -0.131 and a respective IV estimate of -0.344. The direction of the selection bias in the OLS estimates indicates that a positive correlation between mother's schooling and fertility could be induced by some unobserved factor that is likely to have caused the OLS estimate to be biased upwards. One such candidate factor is spouse unobserved characteristics that are correlated positively with the demand for children and with wife's schooling. For example, if more educated women tend to marry relatively wealthier men, and demand for children rise with family income, than the effect of mother's education will be spuriously downwardly biased. If the new access to schooling gained in 1964 did not alter matching decision in the marriage market, then 2SLS estimates will be more negative than OLS because the instrument eliminates this additional source of positive endogeneity bias. We have assessed this potential explanation by controlling for the husband's earnings in the OLS fertility equation. The OLS estimates doubled in size in each of the regressions reported in Table 2 and Table 3, narrowing some of the gap between these and the respective IV estimates.

5. Effect of Completed Schooling on Competed and Fertility

Data from the 1995 Census allows estimating the effect on schooling and fertility, a year when our treated and control cohorts are over age 40, old enough to have definitely completed their education and most likely also finished making children. However, we note here again that in the 1995 data file certain variables are grouped and so we will not be able to replicate the estimation based on each cohort or groups of two cohorts. We therefore will have one young affect group including age 4 to nine, a second older affected group age 9 to 13, and two unaffected groups, age 14-18 and age 19-23. Using these age groups we first present in Table 4 their means of years of schooling and of the number of children per women 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. These estimates are also presented in Table 4. In panel A, we compare the schooling attainment and fertility 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, mean years of schooling is higher in areas that were not affected by the travel restrictions. The opposite holds for fertility which is lower in unaffected areas for both cohorts. But note that years of schooling have increased in both treated and control areas but it increased much more in localities included in the former group. In the control group average schooling increased from 7.1 for the older group to 9.2 in the younger group, a difference of 2.1 years of schooling. In the treatment group the average schooling increased from 4.5 for the older group to 9.2 in the younger group, a difference of 3.6 years of schooling. The precise difference of these differences amounts to a relative increase of 1.566 years of schooling in the treatment group with a 0.254 standard error.

In panel B of Table 4 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 and fertility is lower. 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 gain is only 0.633 years of schooling, less than half the respective average gain for the younger cohorts. The level of precision of these two difference in differences estimates allow rejecting, at a comfortable level of statistical significance, that they are equal. Moreover, these 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.

Columns 4-6 in Table 4 present the ingredients 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. Fertility declined in both treated and control areas but much more in the former. In the treated group fertility declined from 5.68 children per women in the age group 14-18 in 1964 to 5.04 for the 9-13 age group and to 4.11 for women in the 4-8 age group in 1964. In the control group the respective fertility rates were 4.31, 3.96 and 3.37 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 0.622 (sd=0.164) and for women of age 9-13 in 1964 it is 0.279 (sd=0.188). Again, the relative decline in fertility of the two age groups can be interpreted as the causal effect of the regained access to schooling under the assumption that the decline in fertility would not have been otherwise systematically different in affected and unaffected areas.

The Wald estimate of the effect of mother's schooling on fertility can be computed for each affected cohort based on the simple difference in differences estimates of the first stage and reduced form relationships. The ratio of the two parameter estimates is based on the sample of the younger age cohorts implies that an increase of one year of schooling caused a decline in fertility of 0.31 children (0.622 divided by 1.566). For the older cohorts this estimate is 0.44 children (0.279 divided by 0.633). However, the identification assumption needed for these estimates to reflect causal relationships 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 and were not affected by the removal of the travel restrictions and renewed access to schooling in some 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 4, 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, 0.239 (sd=0.307) for schooling and -0.156 for fertility, are very close to 0. We also analysed 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.

5a. Basic Results

As shown with the 1983 Census data, the simple difference in differences 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 equation which similar to equation (2) but Y_i is a dummy that replaces T_i and it indicates whether the individual belongs to the "young" cohort in the subsample:

(5) $$S_{ijt} = \alpha + a_{lj} + \mu_k + (A_j Y_i) \delta + \varepsilon_{ijk}$$

Table 5, 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. In column 1, the specification controls only for the interaction of a cohort of birth dummy and the population aged 4-8 in 1964. The suggested effect is that the treatment increased the education of female children aged 4-8 in 1964 by 1.42 years. 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. This conditional difference in differences estimate is 1.485, only 3.5 percent higher than the uncontrolled DID estimate. In column 3 we add locality fixed effects as controls and the estimated DID is 1.434, almost identical to the uncontrolled DID presented 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 5 shows the results of the cohort age 9-13 in 1964 and the control group is again children aged 14-18 in 1964. 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. The simple DID estimate is 0.633, less than half the size of the respective estimate for the young cohorts. The controlled DID estimates presented in columns 2-3 are 0.726 and 0.723 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 5 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 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 column 3 is 0.078 (sd=0.274), practically zero and much lower than the respective estimate (1.463) presented in panel A and in panel B (0.723). 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.

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 (1) 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, high school diploma (12 years of schooling), a matriculation diploma and a college degree. In the first column 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 not precisely estimated for the high school and matriculation diploma thresholds. The estimates indicate that the policy change that allowed access to schools increased the probability of completing at least primary school by 16 percent and of attaining at least 9-10 years of schooling by 5.5 percent. Overall these estimates suggest that the mean gain in years of schooling included individuals who reached high school and even beyond to college 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 the probability of completing primary school and not much beyond this level of schooling.

In column 3 we present estimates based on the control experiment and over all these evidence show no clear pattern across the various educational attainment thresholds.

Before presenting the results about the effect of mother's education on fertility it is important to note that the travel policy change affected also the education of Arab Men. In appendix Table A3 we present results of estimating equation (1) based on the men sample. The estimates are similar to those of women but the sizes of the effects are much smaller. The travel policy change led to an increase of 0.711 years of schooling for the 4-8 age cohorts and of 0.382 years of schooling for the 9-13 age cohorts. The estimated effects are very similar in all three specifications, especially for the older cohorts affected, again suggesting that omitted relevant locality characteristics do not confound the estimates of the experiment effect on schooling. The control experiment presented in panel C suggest close to zero effects on older cohorts which reinforce the causal interpretation of the estimated effects on men.

The smaller effect on men than on women is not surprising because it is expected that in a traditional society of Arabs in Israel travel restrictions will be much more binding for women because alternatives such as walking long distances daily or living with relatives or in boarding schools in order to get better access to education are less likely for girls than for boys.

5b. Effect of Mother's Education on Completed Fertility

We first discuss the reduce form estimates obtained from estimating equation (3) with the 1995 data. These results are presented in Table 5, columns 4-6. In panel A, we compare fertility of women who were at age 4-8 in 1964 with fertility of women aged 14-18 in 1964. In column 1, the specification controls only for the interaction of a cohort of birth dummy and the population of the young cohort in 1964. This estimate suggests that the removal of travel restrictions reduced the fertility of young female cohort by 0.622 children. Adding individual characteristics as controls lower the estimate to 0.586. When we add the locality fixed effects to the regression estimated, the DID estimate bounced back to 0.615. It is important to note here as well how similar are the three DID estimates obtained from these three specifications.

In Panel B we show the DID estimates based on using the cohorts age 9-13 as the treatment group. The estimated effect of the regained access to schooling on the older cohorts is as expected lower than the effect on the younger cohorts. The simple DID estimate is -0.279, the controlled DID estimate is -0.324 and the full DID estimate with locality fixed effect is –0.371. 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 of fertility obtained from the young and older age cohorts will be very similar.

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. But the difference in differences estimate in column 6 of Panel 6 is -0.167 with a standard error of 0.217, not allowing rejecting that it is not statistically different from zero.

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

The OLS estimates are presented in column 7 of Table 5 and the 2SLS results are presented in column 8 of the same table. The OLS estimate for the youngest affected cohorts, presented in panel A, is negative and significantly different from zero but it is very small, -0.05. Results obtained from specifications without controls are very similar and not shown here. IV estimate is also negative, -0.429, and significantly different from zero and is almost ten times larger than the OLS estimates. This suggests that the OLS estimate is very much upward biased. We also estimated an IV model based on a sample that includes the cohorts age 19-23 in 1964 as part of the control group. The importance of this 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. The estimated effect of education on fertility based on this model is -0.416 (sd=0.125), and it is not different from the estimate based on the sample that included only the 14-18 age cohorts as a comparison group. 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 presents the results based on the experiment of the older affected cohorts. The OLS estimate is negative and significantly different from zero but very small, -0.062. Results obtained from specifications without controls are similar and not shown here. IV estimate is also negative, -0.513, significantly different from zero, and it much larger than the OLS estimate. The estimated effect based on including in the control group the 19-23 age group and allowing for a pre-treatment differential trend for treatment and control areas is -0.452 (sd=0.272), just marginally lower than the estimate obtained when the control group include only the 14-18 age group.

In concluding this section we note that the IV estimates obtained for the young and older affected cohorts are very much identical, 0.429 and 0.513 respectively, even though the first stage effect on schooling is twice as large for the younger affected cohorts. These estimates imply that an increase in one year of schooling causes a reduction in fertility of half a child. This effect size is very similar to that estimates presented in Leon (2004) based on 1950-1990 US Census data. This study report instrumental variable estimates in the range 0.30-0.35 using changes in state compulsory schooling laws as a source of exogenous variation in women's education.

5d. 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 both negative though imprecisely measured and are therefore not practically 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. 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 positive: an increase of one year of schooling increases it by 1.1 percent for the 4-8 age cohorts and by 2.1 percent for the 9-13 age cohorts. The latter is larger probably due the longer time period under hazard of divorce. Note however that the OLS estimates of the effect on the divorce rate are zero.

We can summarize the above evidence by concluding that the increase in education did not significantly affect the mechanisms through which female schooling could have reduced fertility. 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. Education must have affected fertility through the other channels; one such potential mediating factor is spouse selection to which we turn next.

5e. 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 were 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. These facts help interpret the findings that for the young affected cohorts, higher female education led to marriage with more educated men: one more years of schooling enabled women to marry a man with 0.428 additional years of schooling. Note that the OLS and IV estimates of this effect are identical. This positive effect may suggest that some of the reduction in fertility of women in this cohort is also due to higher schooling of their husbands. However, for the cohorts age 9-13 the IV estimated effect of women's education on spouse education is much smaller and actually it is not significantly different from zero even though the respective OLS estimate is positive and large and of equal magnitude to the OLS and IV estimates of this coefficients obtained from the young affected cohorts. We think that it can therefore be safely concluded that the effect of mother schooling on fertility is not enacted also through father's education.

Finally, we note that the OLS effect of mother's schooling on spouse's labor force participation and earnings are positive and significant for both cohorts affected but the respective IV estimates are much smaller and 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. 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 focus on the human capital of the oldest children age 18-19) because the other children are most likely too young to complete their schooling by 1995. We also focus on the older women affected by the policy change because women who were of age 4-8 in 1964 will most likely not have children of age 18-19 in 1995.

6a. 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 in 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 behavioural 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 behavioural 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.

6b. 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. We focus our estimation on the sample of older cohorts among those affected by the removal of travel restrictions in 1963. The age cohorts 4-8 in 1964 are too young in 1995 and do not have old enough children to complete their schooling by 1995 (the census date from which we draw our data). The cohorts of age 9-13 in 1964 are 40-44 years old in 1995 and therefore are more likely to have children that reached post secondary schooling or even college age. Recall that the census reports a grouped age and therefore we will focus on the individuals in the 18-19 years old group. Since a woman might have more than one child in this age group we use as an outcome of children educational attainment the proportion of children in this age (18-19) who attained at least certain education levels. In particular, we focus on attainment of at least primary schooling, secondary schooling, a matriculation diploma and a college degree. We include in the sample only mothers with children in this age range. The sample includes 1,756 mothers with over 2,459 children. Using this sample we estimate the following model:

$$(6) \quad E_{ijt} = \alpha + l_{lj} + \mu_k + S_{ijt}\,\delta + \eta_{ijk}$$

Where E_{ijt} is the proportion of education attainment of the 18 or 19 years old children of mother i born in locality j in time t.

In Table 7 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 affected mothers from cohorts age 9-13, 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 7. Gaining access to schooling in 1964 of Arab mothers caused an increase in schooling of their oldest children. This gain is reflected in increased attainment in primary and secondary schooling and also in getting a "Bagrut" diploma. The positive effect does not reach the level of college degree completion. The effect on children completion of secondary schooling and on receiving a matriculation diploma are substantial: the first outcome increases by 11.3 percent and the second by 15.2 percent. The probability of completing primary schooling increased by 5.5 percent, just marginally lower than the effect of the policy change on the probability that mothers completed primary schooling (0.055).

In Table 8 we present the OLS and 2SLS estimates of the effect of mother schooling on children educational outcomes. In the first two columns we present estimates based on using the 14-18 cohorts as a control group. In the third and forth columns we extend the control group to include also the older unaffected cohorts that were at age 19-23 in 1964 and allow a differential pre-1964 trend for treatment and control observations.

Focusing on the first two columns shows that the OLS estimates are positive and are precisely measured with t-values over 4 except for the estimated effect on college degrees. The 2SLS estimates are much higher than the OLS estimates suggesting that the latter are biased downward by a large factor.[7] For example, the OLS estimate of the effect on completing at least secondary schooling is 0.024 while the 2SLS estimate is 0.129 and a similar gap exists between the two respective estimates for the effect on obtaining at least a matriculation diploma.

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[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).

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 the probabilities that the mother and the child completed primary school. The estimated effect on the mothers' probability is 0.061 (from Table A2). The probability for children is 0.04 (0.055 divided by 1.4 which the number of children in age 18-19 per mother). The ratio is 0.73, twice 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.

7. Conclusions (to be added)

- Effect of natural experiment on female schooling is very large in comparison to other studies that used exogenous variation in schooling, e.g Angrist and Krueger (1991) quarter of birth effect, Duflo (2000) school construction effect in Indonesia and Oreopulos (2007) compulsory schooling law effect in England.
- Effect on fertility is very large as well. It implies that the increase in Arab women schooling in Israel over the last fifty years explains two thirds of the decline over the same period in the fertility of this population group.
- First paper to estimate these effects on a population based sample.
- The Arab population during the period of study is well representative of a developing country population.
- Effect on a population that is mostly Moslem has important generalisation.
- First paper to demonstrate a large causal effect of education that 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 demographics such as marriage, age of marriage and divorce.
- Education increase could have impacted Arab's women fertility through other channels: knowledge and ability to process information regarding fertility options, healthy pregnancy behaviors, and contraception options. Education may also improved the wife's bargaining power inside the marriage though this may be less likely in Moslem families in Israel.

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Appendix: The Arab Population in Israel[i]

1. Background and Political and Demographic Developments

Following the 1948 War of Independence and the establishment of the State of Israel from parts of what had been the British Mandate of Palestine, the Arab population in this area experienced extreme demographic, social, and economic change. While exact figures are not available, it has been estimated that in 1947 Mandate Palestine had an Arab population of roughly 1.2 million, between 750,000 and 900,000 of whom resided in the territory that later became Israel. During 1948, most of these Palestinian Arabs fled or were expelled from their homes, so that only approximately 156,000 remained in Israel immediately following the War of Independence. Moreover, it has been suggested that the more educated, urban professional and business classes were less likely to have remained in their homes, so that what was left of the Arab population in Israel was quite poor, uneducated, and agrarian; the remnants of the Arab economy in Israel lacked a significant manufacturing and trade infrastructure (Goldscheider 1996). Immediately following Statehood, the depleted population of Arabs comprised roughly 19 per cent of the population of Israel, with a Jewish population comprising the rest (Friedlander and Goldscheider 1979; Lewin-Epstein and Semyonov 1993). Since 1948, the proportion of Arabs in the population of Israel has remained fairly steady; the higher natural growth rate of the Arabs has been roughly counterbalanced by heavy Jewish immigration. In 2001, Muslims comprised roughly 82 per cent of the total Arab population of Israel, while the Christian and Druze populations comprised about 9 per cent each. Differential population growth has been primarily a function of heterogeneous fertility patterns, with Muslim and Druze women maintaining levels of total fertility of between four and five, following reductions in fertility from strikingly higher levels, while Christian total fertility fell to roughly 2.6 during the period 1995-2000 (Goldscheider 1996; Friedlander 2002).

Despite abolition of the military administration in 1966, and the granting to Arabs of the legal right of free movement, internal migration among Arabs has been quite limited in scope. Consequently, there is extreme residential segregation in Israel between Jews and Arabs, explained by historical circum-stances during the Mandate period, later military rule, and more recent social exclusion and financial limitations. The great majority of Arabs in Israel reside in villages and towns in which they are the sole inhabitants, and there are few 'mixed' localities in the country that contain both significant Jewish and Arab populations. Also, the regional residential concentration of Arabs is high: nearly half of all Arabs live in the more peripheral, northern region of the country, whereas only about 10 per cent of Jews live there. The majority of Arab men today commute to work in Jewish areas (Lewin- Epstein and Semyonov 1993). For Arab women, the situation is perhaps even more extreme. Because they are limited by cultural norms to working in their own communities, their job prospects are largely determined by local opportunities. Only recently have Jewish controlled companies established small factories in Arab villages in order to employ local women at low wages (Shavit 1990). The small and slowly growing proportion of Arab women in the labor force are generally better educated than their male counterparts and work almost exclusively in the Arab public sector, in educational and similar institutions.

During the first decades of Statehood, the Arab educational system expanded rapidly, partly in response to larger birth cohorts, and partly as a result of new laws providing for compulsory primary education and free secondary education. This expansion increased the labor-market demand for educated Arabs, particularly for teachers and educational administrators (State of Israel 1955). In recent years, the expansion has slowed significantly. This change, together with an increasing number of Arab university graduates in need of employment, has resulted in a growing mismatch between educational attainment and occupational rewards (Shavit 1990; Lewin-Epstein and Semyonov 1993).

The Arab Educational System

During the British Mandate educational opportunities in the Arab communities were limited. Illiteracy rates for Arab adult men and women were over 70 and 90 per cent, respectively, in 1931, while the corresponding rates for Jews were only 6 and 22 per cent (Metzer 1998). The quality of education in the Arab sector of Palestine under the British Mandate was very low. There were no compulsory schooling laws, and in more than a third of the Arab communities there were no primary schools. Most of these schools had multi grade classes with only one or two classes and children remained in school only until 4th grade. Only in the very large villages and in cities there were complete schools that offered all grades. Secondary schools existed only in the large cities and in 1955 there were only 6 such schools (The Arabs in Israel, State of Israel, 1955). Kindergarten, special education schools and extra-curricular activities were completely missing in the Arab sector in Palestine. During and after the 1948 war many teachers left the Arab villages and as a result the education system was shaken and impoverished from its human capital. There was therefore a need for a restructuring and building it (Kopelevitch, 1973). As an immediate solution to the scarcity of teachers, many unqualified staff, some even with only primary education schooling, were recruited. Needless to say that many of the new and old teachers lacked any pedagogical training. Two additional factors contributed to the lack of teachers during the first decade and a half since 1948. The first is the Military Government restriction on travel that made it difficult for teachers to commute to schools. The second is the prohibition due to security consideration that did not allow some of the veteran teachers to continue working in schools because they were viewed as a security treat (Zameret, 2003).

In the first two decades following Statehood, shortages of qualified teachers, suitable textbooks, and adequate physical facilities affected the quality of Jewish schools and severely hampered the spread of education in the Arab sector, but these conditions began to improve, especially in the 1970s.

Although publicly funded education is available to the entire population, including Arabs, the educational systems of the Jewish majority and the Arab minority are almost entirely separate. Within the Israel Ministry of Education, distinct administrative bodies operate the Jewish and Arab systems. With few exceptions, Arab students do not study with Jewish students in the same school. Moreover, in the large majority of cases, Arab teachers work in the Arab schools and Jewish teachers work in the Jewish schools. In primary and secondary schools, instruction in the Arab sector is in Arabic, with Hebrew taught as a second language, whereas Hebrew is the language of instruction in the Jewish system. The only educational level where integration occurs is the post-secondary level, which includes universities and colleges. Another important distinction between Jewish and Arab schools is in the distribution of students between academic and vocational 'tracks'. Secondary education in Israel comprises two tracks-an academic track, the aim of which is to prepare students to pass the matriculation exams and continue to post-secondary studies, and a vocational track. The proportion of Jewish children who study in the academic track has traditionally been smaller than the corresponding proportion among Arab children. Shavit (1990), Klinov (1996), and Mazawi (1998) discuss possible explanations for the limited development of the vocational track in the Arab sector. It is well documented that the chances of attaining a matriculation diploma are significantly higher among those in the academic track than among those in the vocational track (e.g., Shavit 1990; Friedlander et al. 2002a). In fact, many vocational secondary schools do not prepare their pupils for the matriculation exams. Therefore, all other things being equal, those population groups with higher proportions of high-schoolers enrolled in the academic track (Arabs) would tend to have higher proportions earning matriculation diplomas, which thereafter open doors to post-secondary education. In fact, despite their greater likelihood of studying in the academic track, Arab students generally have lower overall rates of matriculation and post-secondary school enrolment than do their Jewish counterparts. Most primary and secondary school students in the Jewish sector attend State-owned schools, although there has been a recent trend toward establishing some semi-private schools, which are supported in part by the State or the municipal government or both. In the Arab sector too, some schools, particularly at the secondary level, are privately owned and operated by various Christian denominations. Having developed from a history of Christian education and community organization going back to the Ottoman period, these Christian private schools are generally considered to be of better quality than State-run Arab schools, and are preferred by those who can afford to attend them (Al-Haj 1995; Kraus et al. 1998). Private Christian schools are selective in their admissions policy, and generally give preference to Christian applicants over Muslim applicants. Moreover, Arab private schools are generally located in larger urban areas, which have higher concentrations of Christians than do smaller localities. During the late 1970s, roughly three-quarters of students in private Arab primary schools were Christian, while only about one-quarter were Muslims (Bashi et al. 1981). An exceedingly important development in Arab education in Israel was the introduction of co-educational schooling. Driven largely by practical considerations and a severe shortage of female Arab teachers, rather than an ideological position, the decision by the government of Israel soon after Statehood to make Arab education co-educational had a huge impact on the educational level of Arab girls and women (Al-Haj 1995).

[i] This section draws heavily on material presented in Okun and Friedlander (2005).

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

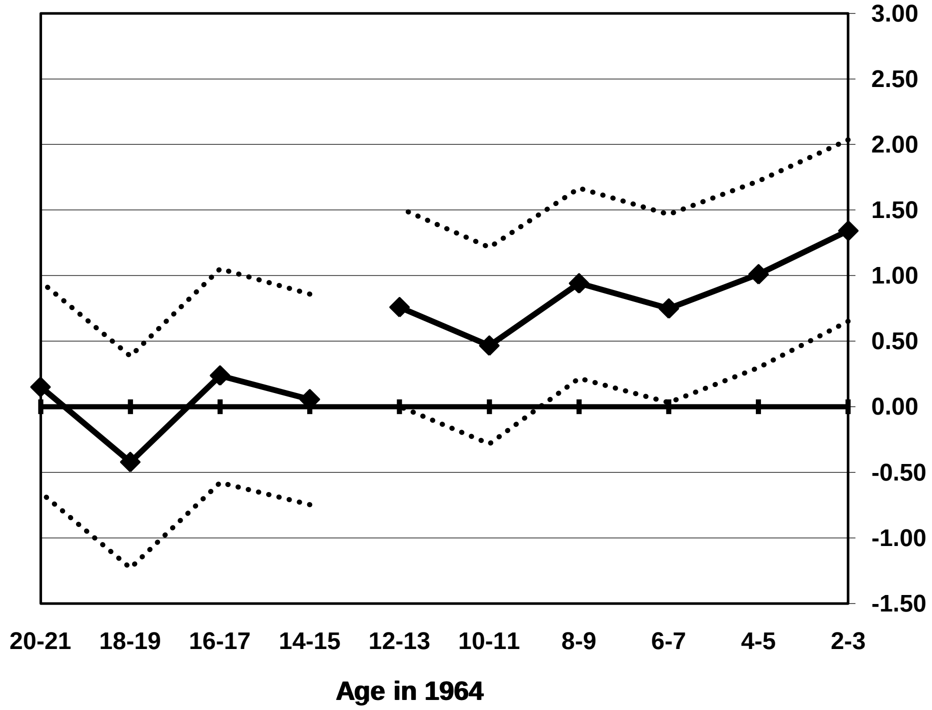

Figure 2: Coefficients of the interaction of age in 1964 and access to schooling in the education equation using control group II

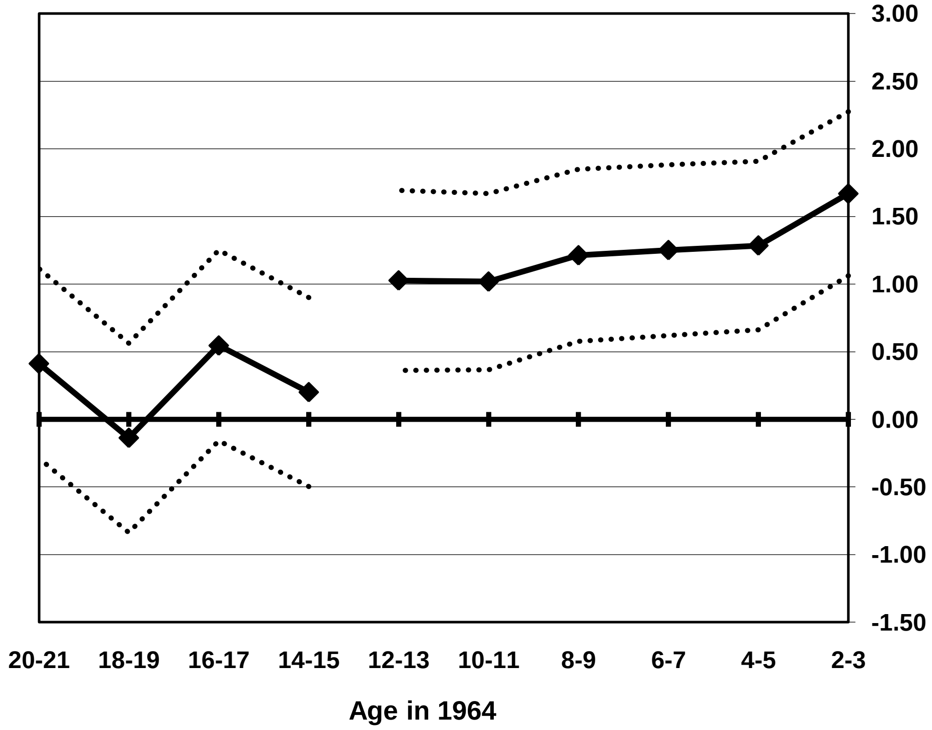

Table 1: Descriptive Statistics, 1983 Census Data

	Treatment		Control I		Control II	
	Age in 1964		Age in 1964		Age in 1964	
	14-18	19-23	14-18	19-23	14-18	19-23
	(1)	(2)	(3)	(4)	(5)	(6)
A: Women						
Years of Schooling	4.322	2.886	7.434	5.920	6.789	5.720
	(4.071)	(3.585)	(4.174)	(4.210)	(4.476)	(4.221)
Fertility	5.552	6.767	4.228	5.206	4.532	5.228
	(3.031)	(3.491)	(2.511)	(3.212)	(2.852)	(3.177)
Labour Force Participation	0.115	0.085	0.207	0.211	0.218	0.217
	(0.320)	(0.279)	(0.406)	(0.408)	(0.413)	(0.413)
Marriage	0.909	0.941	0.898	0.895	0.920	0.906
	(0.288)	(0.236)	(0.303)	(0.307)	(0.271)	(0.293)
Age of Marriage	19.96	20.50	20.97	21.17	20.60	20.99
	(3.710)	(4.454)	(3.884)	(4.295)	(3.949)	(4.365)
Divorce	0.004	0.014	0.020	0.008	0.021	0.017
	(0.063)	(0.117)	(0.140)	(0.088)	(0.145)	(0.130)
B: Spouse						
Years of Schooling	7.242	6.097	8.933	7.772	8.058	7.279
	(3.718)	(3.732)	(3.892)	(3.925)	(4.181)	(4.058)
Labour Force Participation	0.915	0.843	0.918	0.933	0.899	0.910
	(0.279)	(0.364)	(0.275)	(0.250)	(0.302)	(0.286)
Ln (Monthly Earnings)	9.837	9.790	9.884	9.834	9.857	9.865
	(0.556)	(0.608)	(0.761)	(0.704)	(0.700)	(0.640)

Notes: Standard deviations are presented in parenthesis. The fertility measure is a woman's total number of live births until 1983 (the Census year). Log monthly earnings is measured in 1983 prices in Israely shekels. Control also the mixed localities with Jews and Arabs residents. Control I includes 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 1948. Control group II includes control group I and also the Arab popuilation residing in mixed localities with Jews and Arabs residents.

Table 2: Estimated Effect of Female Education on Fertility: First Stage, Reduced Form, OLS and 2SLS Estimates, 1983 Census Data, Control Group I

	Years of Schooling			Fertility			Fertility	
	Fist Stage			Reduced Form			OLS	2SLS
Age in 1964	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2-3 (N=2,130)	1.572	1.290	1.360	-1.233	-1.069	-1.051	-0.066	-0.772
	(0.330)	(0.309)	(0.305)	(0.197)	(0.191)	(0.189)	(0.013)	(0.210)
4-5 (N=2,000)	1.113	0.888	0.988	-1.156	-1.021	-1.094	-0.052	-1.107
	(0.371)	(0.347)	(0.345)	(0.220)	(0.211)	(0.211)	(0.014)	(0.423)
6-7 (N=1,936)	0.889	0.604	0.771	-0.915	-0.756	-0.827	-0.054	-1.072
	(0.377)	(0.354)	(0.350)	(0.234)	(0.225)	(0.223)	(0.015)	(0.540)
8-9 (N=1,869)	1.034	0.856	0.953	-0.702	-0.571	-0.633	-0.073	-0.665
	(0.387)	(0.363)	(0.357)	(0.252)	(0.241)	(0.239)	(0.016)	(0.331)
10-11 (N=1,750)	0.788	0.346	0.444	-0.389	-0.166	-0.180	-0.036	-0.405
	(0.419)	(0.390)	(0.384)	(0.272)	(0.262)	(0.260)	(0.016)	(0.660)
12-13 (N=1,706)	0.882	0.634	0.608	-0.352	-0.228	-0.276	-0.073	-0.453
	(0.428)	(0.396)	(0.389)	(0.287)	(0.275)	(0.271)	(0.017)	(0.501)
Control Variables								
Individual characteristics	no	yes	yes	no	yes	yes	yes	yes
Locality fixed effects	no	no	yes	no	no	yes	yes	yes

Notes: Standard errors are presented in parenthesis. The control group includes age cohorts 14-18 in 1964 . Individual characteristics include religious (Moslem or Christian) and age dummies. Control I includes localities that were under the Military Government with same travel and other restrictions as treated localities but they had primary and high schools by 1948.

Table 3: Estimated Effect of Female Education on Fertility: First Stage, Reduced Form, OLS and 2SLS Estimates, 1983 Census Data, Control Group II

	Years of Schooling			Fertility			Fertility	
	Fist Stage			Reduced Form			OLS	2SLS
Age in 1964	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2-3 (N=2,628)	1.194	1.101	1.390	-0.984	-0.898	-1.002	-0.080	-0.721
	(0.297)	(0.275)	(0.269)	(0.177)	(0.170)	(0.168)	(0.012)	(0.172)
4-5 (N=2,459)	0.813	0.718	0.970	-0.919	-0.858	-0.991	-0.067	-1.021
	(0.329)	(0.305)	(0.299)	(0.196)	(0.187)	(0.185)	(0.013)	(0.348)
6-7 (N=2,388)	0.790	0.662	1.015	-0.737	-0.658	-0.845	-0.072	-0.832
	(0.340)	(0.317)	(0.309)	(0.209)	(0.200)	(0.196)	(0.013)	(0.299)
8-9 (N=2,320)	0.772	0.750	1.012	-0.355	-0.313	-0.428	-0.083	-0.423
	(0.344)	(0.319)	(0.310)	(0.223)	(0.212)	(0.208)	(0.014)	(0.227)
10-11 (N=2,209)	0.985	0.571	0.694	-0.359	-0.146	-0.194	-0.056	-0.279
	(0.371)	(0.342)	(0.331)	(0.238)	(0.228)	(0.222)	(0.014)	(0.333)
12-13 (N=2,121)	0.459	0.361	0.564	0.004	0.051	-0.133	-0.081	-0.236
	(0.386)	(0.353)	(0.344)	(0.255)	(0.243)	(0.237)	(0.015)	(0.424)
Control Variables								
Individual characteristics	no	yes	yes	no	yes	yes	yes	yes
Locality fixed effects	no	no	yes	no	no	yes	yes	yes

Notes: Standard errors are presented in parenthesis. The control group includes age cohorts 14-18 in 1964. The individual characteristics include religious dummy (Moslem or Christian) and age dummies. Control group II includes control group I and also the Arab popuilation residing in mixed localities with Jews and Arabs residents.

Table 4: Simple Difference in Differences Estimates of the Effect of Access to Schooling on Female Education and Fertility of Affected Cohorts

(1995 Census Data)

	Years of Schooling			Fertility		
	Treatment	Control	Difference	Treatment	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment of Interest						
Cohorts age 4 to 8 in 1964	8.172	9.200	-1.028	4.111	3.367	0.744
	(0.086)	(0.116)	(0.144)	(0.052)	(0.070)	(0.087)
Cohorts age 14 to 18 in 1964	4.549	7.143	-2.594	5.683	4.317	1.366
	(0.134)	(0.170)	(0.216)	(0.094)	(0.120)	(0.153)
Difference	3.624	2.057	1.566	-1.572	-0.950	-0.622
	(0.151)	(0.209)	(0.254)	(0.102)	(0.124)	(0.164)
Panel B: Experiment of Interest						
Cohorts Age 9 to 13 in 1964	6.162	8.123	-1.961	5.043	3.956	1.087
	(0.101)	(0.135)	(0.168)	(0.069)	(0.092)	(0.115)
Cohorts age 14 to 18 in 1964	4.549	7.143	-2.594	5.683	4.317	1.366
	(0.134)	(0.170)	(0.216)	(0.094)	(0.120)	(0.153)
Difference	1.613	0.980	0.633	-0.640	-0.361	-0.279
	(0.160)	(0.227)	(0.271)	(0.119)	(0.139)	(0.188)
Panel C: Control Experiment						
Cohorts age 14 to 18 in 1964	4.549	7.143	-2.594	5.683	4.317	1.366
	(0.134)	(0.170)	(0.216)	(0.094)	(0.120)	(0.153)
Cohorts age 19 to 23 in 1964	3.194	6.027	-2.833	6.421	4.775	1.646
	(0.129)	(0.171)	(0.215)	(0.105)	(0.139)	(0.174)
Difference	1.355	1.116	0.239	-0.738	-0.458	-0.279
	(0.175)	(0.268)	(0.307)	(0.145)	(0.174)	(0.231)

Notes: Standard errors are presented in parenthesis. The control group is control group II. Fertility is measured by the woman's number of children by 1995.

Table 5: Estimated Effect of Female Education on Fertility: First Stage, Reduced Form, OLS and 2SLS Estimates

(1995 Census Data)

	Years of Schooling			Fertility			Fertility	
	First Stage			Reduced Form			OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Experiment of Interest								
Cohorts age 4-8 and 14-18	1.566	1.485	1.434	-0.622	-0.586	-0.615	-0.050	-0.429
in 1964	(0.254)	(0.239)	(0.232)	(0.164)	(0.159)	(0.157)	(0.010)	(0.125)
Observations: 4,885								
Panel B: Experiment of Interest								
Cohorts age 9-13 and 14-18	0.633	0.726	0.723	-0.279	-0.324	-0.371	-0.062	-0.513
in 1964	(0.271)	(0.252)	(0.246)	(0.188)	(0.182)	(0.178)	(0.011)	(0.288)
Observations: 4,124								
Panel C: Control Experiment								
Cohorts age 14-18 and 19-23	0.239	0.136	0.078	-0.279	-0.224	-0.167	-	-
in 1964	(0.307)	(0.279)	(0.274)	(0.231)	(0.221)	(0.217)		
Observations: 3,053								
Control Variables								
Individual characteristics	no	yes	yes	no	yes	yes	yes	yes
Locality fixed effects	no	no	yes	no	no	yes	yes	yes

Notes: Notes: Standard errors are presented in parenthesis. The individual characteristics include religious dummy (Moslem or Christian).

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

(1995 Census Data)

	Experiment of Interest			
	Cohorts age 4-8 and 14 -18 in 1964		Cohorts age 9 -13 and 14 -18 in 1964	
	OLS	2SLS	OLS	2SLS
Panel A: Own Outcomes				
Labour Force Participation	0.038	-0.022	0.034	-0.019
	(0.001)	(0.018)	(0.001)	(0.038)
Marriage	0.005	0.005	0.004	-0.006
	(0.001)	(0.012)	(0.001)	(0.025)
Age of Marriage	0.198	0.297	0.165	0.030
	(0.023)	(0.256)	(0.026)	(0.703)
Divorce	0.000	0.011	-0.000	0.021
	(0.001)	(0.006)	(0.001)	(0.015)
Panel B: Spouse Outcomes				
Years of Schooling	0.454	0.428	0.405	0.180
	(0.014)	(0.136)	(0.015)	(0.335)
Labour Force Participation	0.012	0.013	0.014	0.047
	(0.002)	(0.015)	(0.002)	(0.040)
Ln (Monthly Earnings)	0.036	0.032	0.029	0.066
	(0.003)	(0.022)	(0.003)	(0.038)

Notes: Standard errors are presented in parenthesis.

Table 7: Estimated Effect of Access to Schooling on Education of Children of Age 18-19 in 1995

	Primary School			Secondary School		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment of Interest						
Cohorts age 9 to 13 and 14 to 18 in 1964	0.058	0.058	0.055	0.124	0.127	0.113
	(0.027)	(0.027)	(0.027)	(0.048)	(0.047)	(0.047)
Observations: 1,743						
Panel B: Control Experiment						
Cohorts age 14 to 18 and 19 to 23 in 1964	0.024	0.024	0.037	-0.098	-0.098	-0.065
	(0.034)	(0.034)	(0.034)	(0.058)	(0.058)	(0.058)
Observations: 1,213						
	Matriculation Diploma			College Degree		
	(7)	(8)	(9)	(10)	(11)	(12)
Panel C: Experiment of Interest						
Cohorts age 9 to 13 and 14 to 18 in 1964	0.157	0.160	0.152	0.007	0.008	0.003
	(0.050)	(0.049)	(0.048)	(0.016)	(0.016)	(0.016)
Observations: 1,743						
Panel D: Control Experiment						
Cohorts age 14 to 18 and 19 to 23 in 1964	-0.108	-0.108	-0.083	0.017	0.017	0.021
	(0.060)	(0.060)	(0.060)	(0.019)	(0.019)	(0.020)
Observations: 1,213						
Control Variables						
Individual Characteristics	no	yes	yes	no	yes	yes
Locality Fixed Effects	no	no	yes	no	no	yes

Notes: Standard errors are presented in parenthesis.

Table 8: OLS and 2SLS Estimates of the Effect of Mother Education on Schooling Attainment of Children of Age 18-19 in 1995

	Experiment of Interest			
	Cohorts age 9-13 and 14 -18 in 1964		Cohorts age 9 -13 and 14 -23 in 1964	
	OLS	2SLS	OLS	2SLS
Primary School	0.009	0.063	0.008	0.058
	(0.002)	(0.037)	(0.002)	(0.036)
Secondary School	0.024	0.129	0.023	0.116
	(0.003)	(0.067)	(0.003)	(0.063)
Matriculation Diploma	0.025	0.173	0.025	0.164
	(0.003)	(0.080)	(0.003)	(0.076)
Colledge Degree	0.001	0.003	0.002	0.001
	(0.001)	(0.018)	(0.001)	(0.018)

Notes: Standard errors are presented in parenthesis.

Table A1: Descriptive Statistics, 1995 Census Data

	Treatment		Control	
	Age in 1964		Age in 1964	
	14-18	19-23	14-18	19-23
	(1)	(2)	(3)	(4)
A: Women				
Years of Schooling	4.527	3.292	7.136	6.071
	(4.065)	(3.654)	(4.657)	(4.418)
Fertility	5.683	6.421	4.317	4.775
	(3.113)	(3.221)	(2.820)	(3.030)
Labour Force Participation	0.125	0.097	0.299	0.253
	(0.331)	(0.297)	(0.458)	(0.435)
Marriage	0.937	0.947	0.922	0.918
	(0.243)	(0.224)	(0.269)	(0.275)
Age of Marriage	21.24	19.61	21.84	20.33
	(6.230)	(8.053)	(6.279)	(8.366)
Divorce	0.011	0.008	0.044	0.030
	(0.104)	(0.088)	(0.205)	(0.171)
B: Spouse				
Years of Schooling	6.667	6.104	7.814	7.191
	(3.754)	(4.055)	(4.341)	(4.163)
Labour Force Participation	0.721	0.615	0.764	0.677
	(0.449)	(0.487)	(0.425)	(0.468)
Ln (Monthly Earnings)	8.154	8.133	8.266	8.177
	(0.525)	(0.531)	(0.598)	(0.644)

Notes: Standard deviations are presented in parenthesis. The fertility measure is a woman's total number of live births until 1995 (the Census year). Log monthly earnings is measured in 1995 prices in Israely shekels.

Table A2: Estimated Effect of Access to Schooling on Female Educational Attainment

	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
5-8 Years of Schooling	0.200	0.103	0.040
	(0.021)	(0.026)	(0.031)
Primary School	0.164	0.061	-0.005
	(0.026)	(0.028)	(0.031)
9-10 Years of Schooling	0.055	0.002	-0.028
	(0.026)	(0.025)	(0.026)
Secondary School	0.029	0.009	-0.035
	(0.024)	(0.021)	(0.021)
Matriculation Diploma	0.033	0.010	-0.045
	(0.022)	(0.019)	(0.018)
Colledge Degree	0.033	0.005	-0.019
	(0.017)	(0.016)	(0.015)

Notes: Standard errors are presented in parenthesis.

Table A3: Estimated Effect of Access to Schooling On Male Own Education

	Outcome: Years of Schooling		
	(1)	(2)	(3)
Panel A: Experiment of Interest			
Cohorts age 4 to 8 and 14 to 18 in 1964	1.024	0.905	0.711
	(0.250)	(0.246)	(0.240)
Observations: 4,665			
Panel B: Experiment of Interest			
Cohorts age 9 to 13 and 14 to 18 in 1964	0.421	0.494	0.382
	(0.269)	(0.264)	(0.258)
Observations: 3,969			
Panel C: Control Experiment			
Cohorts age 14 to 18 and 19 to 23 in 1964	0.012	-0.001	0.033
	(0.299)	(0.292)	(0.284)
Observations: 2,980			
Control Variables			
Individual Characteristics	no	yes	yes
Locality Fixed Effects	no	no	yes

Notes: Notes: Standard errors are presented in parenthesis.

Table A4: Effect of Male Education on Own Labour Force Participation, Log of Mounthly Earnings, Marriage, Age of Marriage, Divorce and Spouse's Outcomes

	Sample			
	Experiment: Cohorts age 4 -8 and 14-18 in 1964		Experiment: Cohorts age 9 -13 and 14 -18 in 1964	
	OLS	2SLS	OLS	2SLS
Panel A: Own Outcomes				
Labour Force Participation	0.017	0.015	0.018	0.010
	(0.001)	(0.023)	(0.001)	(0.039)
Ln (Monthly Earnings)	0.050	-0.018	0.052	-0.029
	(0.002)	(0.046)	(0.002)	(0.069)
Marriage	-0.001	0.004	0.000	0.006
	(0.001)	(0.012)	(0.001)	(0.016)
Age of Marriage	0.362	1.312	0.354	1.522
	(0.019)	(0.443)	(0.021)	(1.122)
Divorce	0.000	0.005	0.001	0.022
	(0.000)	(0.007)	(0.000)	(0.013)
Panel B: Spouse Outcomes				
Years of Schooling	0.449	1.729	0.457	1.736
	(0.012)	(0.471)	(0.013)	(0.745)
Fertility	-0.113	-0.889	-0.102	-1.170
	(0.007)	(0.310)	(0.008)	(0.796)
Labour Force Participation	0.030	0.101	0.027	0.069
	(0.002)	(0.039)	(0.002)	(0.054)

Notes: Standard errors are presented in parenthesis.

Table: Distribution of Localities By Treatment and Control

Localities Under Military Government Without Secondary Schools Untill 1964		Localities Under Military Government With Secondary Schools Untill 1964	Localities Not Under Military Government
ABU SINAN	KISRA-SUMEI	KAFAR YASIF	ABU GHOSH
AR'ARA	MA'ALOT-TARSHIHA	NAZARETH	AKKO
ARRABE	MAGHAR	RAME	DALIYAT AL-KARMEL
BAQA AL-GHARBIYYE	MAJD AL-KRUM	TAYIBE	FUREIDIS
BASMAT TAB'UN	MAZRA'A	TIRE	HAIFA
BEIT JANN	MESHHED		ISFIYA
BI'INA	MI'ELYA		JISR AZ-ZARQA
BIR EL-MAKSUR	MUAWIYYE		LOD
BU'EINE-NUJEIDAT	MUQEIBLE		RAMLA
DABURIYYA	MUSHAYRIFA		TEL AVIV - YAFO
DEIR AL-ASAD	MUSMUS		
DEIR HANNA	NAHEF		
EILABUN	PEQI'IN (BUQEI'A)		
EIN MAHEL	QALANSAWE		
FASSUTA	REINE		
HURFEISH	SAJUR		
I'BILLIN	SAKHNIN		
IKSAL	SHA'AB		
ILUT	SHEFAR'AM		
JALJULYE	SHIBLI		
JATT	TAMRA		
JISH (GUSH HALAV)	TUBA-ZANGARIYYE		
JUDEIDE-MAKER	TUR'AN		
KABUL	UMM AL-GHANAM		
KAFAR KAMA	UMM AL-FAHM		
KAFAR KANNA	YAFI		
KAFAR MANDA	ZALAFA		
KAFAR QARA	ZARZIR		
KAFAR QASEM	ZEMER		
KAOKAB ABU AL-HIJA			

Notes:

אזורי הממשל הצבאי