Israel Economic Review Vol. 7 No. 1 (2009), 73–112

THE COMPULSORY EDUCATION LAW IN ISRAEL AND LIQUIDITY CONSTRAINTS

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Abstract

Israel's Compulsory Education Law was amended several times in the 1970s, changes that were reflected in an increase in the number of years of both compulsory education and free education. This process caused a significant reduction in the school dropout rate among students from ninth to twelfth grades, principally among Sephardic (Oriental) Jews (Jews of Asian or North-African origin) and the non-Jewish population. The fact that compulsory schooling and free schooling were dealt with separately during these years makes it possible to examine what effective constraint deters some individuals from continuing their studies. To examine this question, I compared the "compulsory effect" to the "free effect" on the duration of studies and the return to education derived from each of these amendments. It was found that the extra years of compulsory schooling caused a steep fall in the dropout rate in the ninth and tenth grades that had been included in the provisions of the Law, and a moderate decline in dropout rates in the following grades (eleventh and twelfth), which were not compulsory or free at that time. The introduction of free education in the eleventh and twelfth grades also contributed to reducing dropout rates, mostly among female students. It was also found that the return in terms of salary derived from the added years of compulsory education did not differ from the return derived from the added years of free education, and that they both resembled the return to the years of high-school education estimated using the OLS method. These findings indicate that a liquidity constraint deters some individuals from making an optimal investment in their human capital. According to the findings, this constraint is not only due to the need to pay tuition, but also apparently derives from the family decision-making framework, which does not necessarily take into account the benefit that the individual will derive from acquiring additional education.

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MA thesis in economics at the Hebrew University of Jerusalem under the guidance of Prof. Victor Lavy.

I thank Victor Lavy for his generous guidance and help in this paper. I thank Noam Zussman, Shai Tsur, and the participants in the seminar of the Bank of Israel Research Department for their useful comments. I thank Michal Saft and Helena Vilensky for their help in obtaining the necessary documents.

The research was conducted in the research room of the Israel Central Bureau of Statistics, based on the files prepared by the Census and Demography Department. I thank Sigalit Shmueli, Anat Katz, and David Gordon for preparing the file and for their help at every stage.

1. INTRODUCTION

Extensive economic literature exists dealing with the question of the contribution of education to various outputs at the individual and social levels—for example, in the areas of salary, health, and crime. The goal of these studies is to examine whether there is benefit in studying, or whether a high level of formal education is nothing other than one of the characteristics of an economically advantaged population, correlated with high salary for that reason. Most studies show that formal education does indeed contribute to an individual's earnings, although the estimated rate of the contribution varies greatly from study to study.

This finding raises two important questions for discussion regarding economic theory and the policies derived from it. If formal education indeed contributes substantially to an individual's earning ability, why do some individuals choose not to acquire such education? In particular, does the choice stem from an individual's optimal choice, or from other reasons? The second question follows: whether the state should encourage the acquiring of education, and in what manner.

The conventional assertion holds that the decision not to continue studying, despite the high return to education, is non-optimal. This assertion relies on the finding that people from an economically disadvantaged background tend to acquire little education, indicating that they are faced with a constraint that deters them from acquiring the education that is optimal for them. The literature distinguishes between two types of constraint:

1) A short-term liquidity constraint, meaning a financial constraint in acquiring education, whether in the simple sense of paying educational expenses, or in the broader sense of alternative cost and the need to contribute to the family income.

2) A long-term liquidity constraint, meaning a cognitive constraint in acquiring education stemming from the child's educational deprivation at a young age. This constraint may be due to a budgetary constraint of the parents during the child's entire life, or even from the parents' inability to provide the child with a supportive educational environment. Subject to this constraint, an individual may optimally decide the extent of his education at the point in time relevant to his decision-making, but his decision stems from a non-optimal starting point.

By analyzing the effect of changes in the Compulsory Education Law, this paper will attempt to answer the question of what constraint prevents individuals from increasing the number of years that they study.

During the 1970s, the Compulsory Education Law was extended a number of times. The law was extended in two ways: education in 9th and 10th grades became both compulsory and free, while education in 11th and 12th grades became free, but not compulsory. The fact that compulsory education and free education were implemented separately during these years makes it possible to answer the above question with the help of a comparison between the "compulsory effect" and the "free effect" on both dropout rates and the return to education.

The monetary constraint stemming from the need to pay tuition should manifest itself in an active response to both the introduction of compulsory-free years of education and the introduction of free years of education, and in a high return to both of these.[1] This is because in both cases, the monetary constraint was removed. This finding leads to the conclusion that the optimal policy is subsidizing education. On the other hand, a monetary constraint stemming from the need to contribute to the family income will be reflected in a sluggish response to the introduction of free education and an active response to the introduction of compulsory education, as well as in a high return to both of them. This is because the introduction of free education does not remove the monetary constraint; only compulsory studies can lead to the continuing of education. In this case, a high return indicates that the optimal choice from the individual's perspective is the continuation of studies, but the optimal choice from the family perspective is the termination of studies..[2] This finding leads to the conclusion that subsidizing education is not enough; strengthening compulsion is the optimal measure that will prevent people from being caught in the poverty trap. A long-term constraint, i.e., a constraint on the student in developing his capabilities, will be reflected in a slack response to the introduction of free education and an active response to the introduction of compulsory studies, but the return to the introduction of compulsory education will be lower than that on adding years of free education. This is because compulsory education forces students to study even if this is not their optimal choice. In the latter case, it appears that it is preferable to allocate resources to developing the students' abilities at an early age, when their cognitive abilities are determined.[3]

This paper is constructed as follows: the second section presents a review of the literature; the third section describes the history of the Compulsory Education Law in Israel; the fourth section presents the data and the research method; the fifth section presents the results of the empirical research; and the sixth section summarizes the findings.

2. A REVIEW OF THE LITERATURE

Economic research, both empirical and theoretical, into the introduction of a system of compulsory and/or free education deals with two principal questions. The first is estimation of the causal relationship between formal education and earnings; the second is the existence of credit constraints and their effect on the optimal decision on investment in human capital.

[1] The interpretation given in this paper to the comparison of the return to compulsory studies with the return to free studies is not unequivocal, because these were introduced at different times and for different grades. Nevertheless, in both cases the direction of the bias is expected to be in favor of the return to the added free studies, and the interpretation of the findings (see below) of a high return to the introduction of compulsory studies is therefore also strengthened.

[2] Note that other explanations, such as underestimation and lack of certainty with respect to the expected return, shortsightedness, or irrational behavior for other reasons, are also consistent with this finding. Findings in support of this explanation are presented below.

[3] The consequence of this policy is valid only in the narrow sense of benefit in the labor market. There may be other reasons that justify larger budget allocations for education that make a smaller contribution to the labor market, and likewise forcing individuals to study more than they would choose to from the wage aspect.

a. The return to education

The question of how formal education affects salary has been discussed extensively in the literature. These studies use compulsory education legislation as a means of estimating the causal effect of education (usually measured by years of schooling) on the logarithm of the salary, which is commonly regarded as the "return to education." This assumes that the investment needed for acquiring education is solely in terms of time and loss of alternative income, and the consequent percentage of additional salary in the future income profile is in effect the return on that investment. Using this method, it is possible to compare the return on the investment in education with the return on any other investment. These studies are of decisive importance for public policy. For example, if the marginal return to education is greater than the marginal return on capital, it is possible that allocating resources to investment in the economy is inefficient. The conventional explanation for this lack of efficiency is an assumption that the capital market is imperfect (Galor and Zeira, 1993), and that some individuals therefore face liquidity constraints rendering them unable to acquire the optimal education.

It should be noted that there are a number of problems with the assumptions underlying this interpretation of the correlation between education and salary:

(a) The quality of the education is not taken into account, although it is reasonable to assume that the return to education depends on it, and it is doubtful whether everyone is able to benefit from the same educational quality.

(b) Additional costs of education (tuition, textbooks, effort, etc.) are ignored. This renders the comparison with the return on capital incomplete.

(c) It is likely that investment in human capital has positive external effects that are greater than those of investment in physical capital.

(d) It is difficult to estimate the causal effect between education and salary, because of the correlation between the unobserved "abilities" (intelligence, motivation, a supportive environment) and education and salary. Simple estimation of the return to education therefore actually also reflects the return to those abilities.

The first problems have been relatively neglected in research, while the latter problem has been more widely addressed. Many studies have attempted to isolate the causal effect of education on salary. Such studies can be classified into two main groups. The first consists of studies that attempted to control for the unobserved abilities. For example, Griliches (1977) added IQ and family background as control variables; Ashenfelter and Kruger (1994) examined the return to education for identical twins. These studies and many others found that the return to education, net of the ability variables, is not significantly different from the estimated return in simple salary regression. Even when a significant difference was found, it was relatively small.

The second group of studies attempted to find some exogenous event that should have affected the supply of education, and used this event as an instrumental variable in order to isolate the causal effect of additional education on salary. For example, Card (1995) used proximity to a college as an instrumental variable: he asserts that localities far away from a college generate a constraint that deprives some young people of access to higher education, while proximity to a college removes this constraint. Angrist and Kruger (1996) showed that season of birth is related to educational attainment; the law requires studying until age sixteen, and students born near the end of the year must therefore register for an additional year of studies. These studies and many others, which used the supply side method in various countries, found that the return to education was much higher than, and sometimes even double, that estimated by the OLS method.[4] This result contradicts the initial logic according to which the estimate should be lower. On second thought, however, it is clear that the effect estimated in these studies is the return to the addition of a particular year of education, in high school or university, not to an average year of education, which is what the OLS method estimates. Hungerford and Solon (1987) estimated the marginal effect of each year of education using a dummy variable for each year of education. They found that the end of an educational year accompanied by the receiving of a diploma (the end of 8th grade, 12th grade, and a bachelors degree) yields a premium beyond the average return per year of education. Another explanation of the high return is that the return is estimated for those individuals who were affected by the same exogenous change. In many cases, this population is unrepresentative of the general population; these estimates therefore reflect a causal effect, but this effect is purely local. It is therefore difficult to draw conclusions from it about the entire population.

A study of the compulsory education law in England (Oreopoulos, 2006) discusses this point. It distinguishes between average treatment effect (ATE) and local average treatment effect (LATE). The former is the average treatment effect on the population as a whole (which cannot be known, because the change in the law does not affect the entire population), while the latter is the average treatment effect on the treatment group (which is what we are actually estimating). It examines the effect in England on education and salary of raising the legal age for compulsory education from 14 to 15. The law was amended in 1947, and the change led to a substantial fall in dropout rates at age 15 (from 60 percent to 10 percent). The author estimates the return to education with the help of this change, and finds that the estimated return resembles that found in similar studies in which the treatment group was much smaller, amounting to a few percent of the population. The study therefore concludes that the high return is not due to the fact that the law affects a small population that is significantly different from the average, and that conclusions can be drawn about the population as a whole from studies of this type.

Two studies conducted in Israel used changes in the Compulsory Education Law as an instrumental variable to estimate the effect of education on various outputs. The first (Reid, 2005) examines the effect of non-Jewish women's education on their participation in the labor market, their age of marriage, and the number of children they have. It uses the two main amendments to the law—one in 1969, when 9th and 10th grades were added to the compulsory-free education framework, and one in 1978, when 11th and 12th grades were added to the free education framework. Among other things, it was found that extending the Compulsory Education Law caused a rise in the rate of participation and the marriage age, and a drop in the number of children of Druze and Muslim women. The second study (Frish, 2006) estimated the return to education in Israel in different ways, one of which was

[4] Card (Card, 2000) presents a comprehensive review of the theory and findings according to the important studies on the subject of the instrumental variables of the supply side in the 1990s.

through the 1978 amendment to the Compulsory Education Law. Like similar studies around the world, both these studies focused on the use of the law as a means of estimating another factor, not for detailed testing of the effect of the law. They did not distinguish between introduction of compulsory education and introduction of free education, and did not use the identification of the timing and the groups to which the law applies. The main argument underlying this approach by the researchers was that even if the increase in the extent of education was not due to the law, the fact that it results from an event exogenous to the individual that does not directly affect salary justifies its use as an instrumental variable.

b. Credit constraints

Heckman (2002) distinguishes between two types of credit constraints. The first, which he calls a "short-term credit constraint," concerns the monetary constraint, as mentioned above: due to the imperfection of the capital market, economically disadvantaged populations are unable to obtain a loan for optimal investment in their human capital. The second type, which he calls a "long-term credit constraint," concerns a constraint on full development of the child's cognitive and other capabilities during his childhood. These capabilities are a precondition for acquiring higher education, and therefore constitute an effective constraint for certain population groups. The study found that after controlling for the long-term constraints, the financial constraint itself plays a very small role in acquiring education. The importance of distinguishing between the two types of constraint is due to the policy measures derived from them: the short-term constraint can be handled relatively simply through a system of public loans, or through free education. If, however, the constraint results from inadequate investment by the parents in their child's human capital, most resources should be allocated to compensation for the inadequate investment during the early education stages.

A study conducted in Israel (Friedman, 2007) attempts to deal with this question. The study estimated the probability of completing a BA degree with respect to the family's economic status and the student's previous educational achievement. The study found that only 20 percent of the difference in the proportion of those completing a BA between economically advantaged and disadvantaged populations resulted from the short-term constraints; the remaining 80 percent was due to the gap in previous educational achievement, i.e., to the long-term constraint.

Another study dealing with this question (Harmon, Hogan, and Walker, 2002) examined the degree of dispersion of the return to education over time. The researchers assert that if the reason why not everyone keeps on studying lies in gaps between individuals' capabilities, then the dispersion of the returns should increase over time as the number of educated people rises. The study found that there is no increase in the dispersion of the returns over time, and the researchers therefore concluded that the rise in educational rates is due to an easing of the short-term liquidity constraint facing individuals.

A study by Friedman (2006) examined the alternatives to the Compulsory-Free Education Law. Compulsory education effectively raises the level of the human capital of economically disadvantaged groups, thereby benefiting both individuals and society, but it also makes at least some of these groups study more than their optimum would dictate. On the other hand, free education is less effective in expanding human capital, but enables everyone desiring an education to study, without forcing excessive investment on some individuals. The study included simulations, which showed that the educational budget up to a given level should be used exclusively for compulsory education, while above that level free education should be expanded more rapidly than compulsory education. This study is theoretical, while the current study adds an empirical analysis to the picture, and makes it possible to quantify both the effectiveness of the free and compulsory aspects of the Compulsory Education Law in raising the educational level, and the private return resulting from this higher level.

3. THE COMPULSORY EDUCATION LAW IN ISRAEL

The Compulsory Education Law[5] was first enacted in 1949. This law stated that every child was required to acquire nine years of schooling (one year of kindergarten and eight years of elementary school). The obligation to obey the Law applied to the child himself, his parents, and his local authorities. The Law also proposed punitive measures for parents violating the law (a fine, or even imprisonment). In addition, the Law stated that a student to whom the law applies was entitled to free education. While the Law was important in principle, in practice it affected mostly the non-Jewish population, since almost the entire Jewish population was already studying until at least 8th grade before the Law was enacted.

During the 1950s and 1960s, following the influx of immigrants from Asia and North-Africa (henceforth, Sephardic Jews), which brought to Israel a poor and uneducated population, it became necessary to extend the law in order to raise the educational level of young people and provide equal opportunities for the disadvantaged. Differential tuition was introduced in the 1960s, and the Compulsory Education Law was amended in 1969.[6] The amendment increased the number of years of compulsory schooling to eleven, two more years than in the original law, making 9th and 10th grades compulsory and free. The law was not implemented immediately, and a gradual implementation over four years was permitted. The method of gradually implementing the law was not explicitly stated in the law's provisions. In the first stage, the law was gradually applied to students in 9th grade in 1970–73 according to lists of localities published at the beginning of each year, and was partially applied to students in 10th grade in 1974–75. Implementation of the law was discontinued, however, because of budgetary problems, creating a situation in which education was compulsory up to 9th grade in some areas of Israel, and up to 10th grade in other areas.

It is difficult to find data proving that the Compulsory Education Law was enforced. It is clear that parents were not imprisoned, and that the extent of fines was limited. At the same time, in the search for material for this study I came across many documents in the State Archives indicating that an effort was made to enforce the law, including letters

[5] Compulsory Education Law (1949).
[6] Compulsory Education Law (Amendment 5) (1969).

appointing high school officers, referral of social workers to the families of students, and letters from parents and managers to the Director General of the Ministry of Education asking that a particular student be exempted from compulsory studies. In any case, even if enforcement was only partial, it is reasonable to assume a certain degree of obedience to the law, leading to fewer dropouts.

The Compulsory Education Law was amended again in 1978.[7] The law restated the requirement for studies through 10th grade, and extended the right to free studies to 11th and 12th grades. This time, it was decided to implement the law immediately and fully in the 1979 school year. In practice, in addition to the right to free education, the law also caused the introduction of compulsory studies in 10th grade in those areas in which it had not yet been implemented. Table 1 displays the level of tuition in 1976 and the table of discounts used as part of the differential tuition system. Before high school studies were made free, annual tuition was IL 3,500. In practice, however, there were twenty levels of discount, ranging from complete exemption for those with per capita income of less than IL

Diagram A: How the Compulsory-Free Education Law Was Implemented

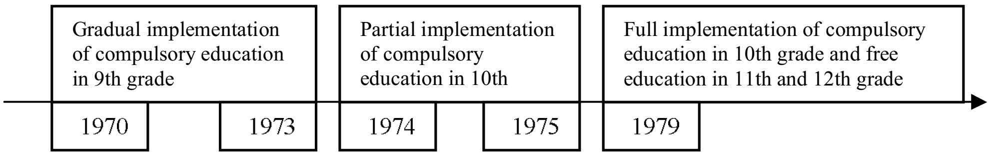


Table 1
Tuition and Per Capita Family Income

Tuition in 1976					
Grade	Full tuition (in Israeli lira)		Grade	Full tuition (in Israeli lira)	
9th	3,060		11th	3,420	
10th	3,230		12th-14th	3,580	
Ratio of 12th grade tuition to per capita family income					
Per capita income (in Israeli lira)	Rate of exemption (%)	Ratio of tuition to per capita income (%)	Per capita income (in Israeli lira)	Rate of exemption (%)	Ratio of tuition to per capita income (%)
425	100	0	569	50	27
438	95	3	591	45	28
450	90	7	613	40	30
467	85	10	634	35	31
475	80	13	656	30	32
488	75	15	678	25	34
500	70	18	700	20	35
513	65	21	750	15	35
525	60	23	850	10	34
547	55	25	1,125	5	29

Source: General Manager circular 35\1 (August 31, 1975)

[7] Compulsory Education Law (Amendment 115) (1978).

425 per month to a 5 percent discount for those with monthly incomes of IL 850–IL 1,125. It can be seen that tuition constituted 25–35 percent of per capita income for those with incomes higher than IL 500.

4. DATA AND METHODOLOGY

a. Data

The study uses Israeli census data from 1983 and 1995 (demographic version) compiled by the Israel Central Bureau of Statistics. In the census, a sample of 20 percent of the population answered a complete questionnaire, which is therefore more reliably representative and includes many observations, thereby facilitating complex statistical analyses. In the first stage, I examined the effect of the law on education using two different econometric strategies: for the first strategy, in Section 5a(1), I used the 1995 census, and for the second, in Section 5a(2), I used the 1983 census,. In the second stage, I used the 1995 census to examine the effect of the law on salary (Section 5b).

The 1995 census includes diverse individual data; the relevant data for this study are income, education, and general characteristics, such as age, gender, and ethnic origin. The importance of using the 1995 census lies in the ability to identify the cohorts benefiting from the law some twenty years after its implementation, which makes it possible to estimate its effect on the individuals in the labor market. At the same time, since the census was conducted in 1995, it does not include information about the family background of the individuals at the time of the inquiry (such as the education and income of the parents and the number of siblings). Similarly, a treatment group cannot be distinguished from a control group, because the individual's residence in the 1970s (the years during which the law was implemented gradually) is not available.

It should be noted that the identification of the cohorts in the census is by age, which does not necessarily indicate the date they started any particular grade. For example, a child born in 1954 did not benefit from the law according to this system of identification, because he reached age 15 before 1970 (when compulsory education was implemented for 9th grade). It is possible, however, that he began 1st grade a year earlier or later than the rest of his age group, and it is possible that he repeated or skipped a grade. In such cases, the age group does not indicate the date of reaching 9th grade, which could lead to under-estimation of the law's effect. In 1995, for example, the proportion of 15-year-olds reporting nine years of schooling in the census (9th grade) was only 60 percent. Twenty percent reached only 8th grade, and 10 percent reached 10th grade.

Table 2 displays descriptive statistics of the 1995 census data. The sample includes individuals aged 24–60 born or raised in Israel (who immigrated to Israel by age 12 at the latest). Groups defined were Jews of European or American origin (immigrants from Europe, the US, and Canada), Jews of Asian or African origin (immigrants from Western

Table 2
Descriptive Statistics – 1995 Census

	Entire sample	Men	Women	Europe-America	Asia-Africa	Non-Jewish
Years of education	11.7 (3.8)	11.8 (3.6)	11.6 (4.0)	13.7 (3.1)	11.5 (3.0)	8.9 (4.5)
Years of education – restricted to 8–12	10.7 (1.5)	10.7 (1.5)	10.8 (1.5)	11.2 (1.2)	10.8 (1.5)	10.0 (1.7)
Dummy for completing 9 or more years of education	0.82 (0.4)	0.83 (0.4)	0.81 (0.4)	0.95 (0.2)	0.84 (0.4)	0.55 (0.5)
Dummy for completing 10 or more years of education	0.78 (0.4)	0.79 (0.4)	0.77 (0.4)	0.94 (0.2)	0.82 (0.4)	0.44 (0.5)
Dummy for completing 11 or more years of education	0.70 (0.5)	0.70 (0.5)	0.70 (0.5)	0.89 (0.3)	0.70 (0.5)	0.37 (0.5)
Dummy for completing 12 or more years of education	0.63 (0.5)	0.62 (0.5)	0.64 (0.5)	0.83 (0.4)	0.60 (0.5)	0.34 (0.5)
Dummy for earning matriculation certificates	0.45 (0.5)	0.44 (0.5)	0.47 (0.5)	0.68 (0.5)	0.36 (0.5)	0.25 (0.4)
Years of vocational school education	1.2 (1.7)	1.4 (1.8)	1.0 (1.6)	1.2 (1.8)	1.6 (1.9)	0.3 (0.9)
Age	38.0 (9.6)	37.8 (9.6)	38.1 (9.6)	40.9 (9.7)	37.7 (9.0)	36.7 (9.7)
Dummy for Gender (male=1)	0.48 (0.5)			0.48 (0.5)	0.48 (0.5)	0.49 (0.5)
Number of persons in household	4.3 (2.0)	4.3 (2.0)	4.3 (2.0)	3.8 (1.6)	4.2 (1.7)	5.5 (2.4)
Monthly salary (NIS)	4,898 (4,277)	5,915 (4,963)	3,606 (2,697)	6,257 (5,406)	4,434 (3,523)	3,321 (2,328)
Hourly wage (NIS)	30 (35)	32 (39)	28 (29)	37 (37)	27 (30)	24 (41)
Employed	0.64 (0.5)	0.74 (0.4)	0.55 (0.5)	0.74 (0.4)	0.68 (0.5)	0.43 (0.5)
Dummy for European-American origin	0.27 (0.4)	0.27 (0.4)	0.27 (0.4)			
Dummy for Asian-African origin	0.41 (0.5)	0.41 (0.5)	0.41 (0.5)			
Dummy for Non-Jews	0.22 (0.4)	0.22 (0.4)	0.22 (0.4)			
Dummy for Muslims	0.17 (0.4)	0.18 (0.4)	0.17 (0.4)			0.79 (0.4)
Dummy for Christians	0.03 (0.2)	0.03 (0.2)	0.03 (0.2)			0.13 (0.3)
Dummy for Druze	0.02 (0.1)	0.02 (0.1)	0.02 (0.1)			0.08 (0.3)
Number of observations	297,346	143,472	153,874	79,186	121,908	65,270

The numbers in the table are averages. The numbers in parentheses are the standard deviations of the variables. The sample was restricted to ages 24–60.

Source: 1995 Israeli census data.

Asia and North Africa), and non-Jews.[8] The other population groups (immigrants from South America, Central and East Asia and others) were omitted from the sample. In cases in which the number of years of an individual's education was used, the sample was restricted to those with 8–12 years of schooling, the relevant years for the amendments to the law. This definition stemmed from the difficulty in evaluating the effect of the law by comparing the number of years of education of people of different ages, before and after the law, since in the census year not everyone had finished their education. This restriction reduced the sample by 38 percent. It can be seen in the table that the average number of years of schooling for the sample as a whole was one year more than the average according to this definition. The gap between the two definitions was 2.5 years of schooling among those of European-American origin, 0.7 years of schooling among those of African-Asian origin, and negative 1.1 years of schooling among non-Jews.

The 1983 census includes information about the residence of individuals in 1978. This makes it possible to define treatment and control groups, based on the partial implementation of compulsory education in 10th grade in 1974–75. The law was implemented in about 500 localities, of which 20 were non-Jewish, out of about 1,000 localities in Israel, of which about 120 were non-Jewish. Among the cities included were Eilat, Ashdod, Ashkelon, Dimona, Tiberias, Jerusalem, Akko, Afula, Safad, and Kiryat Gat.[9] Cross-referencing this list of localities with the names of the localities in the census file produced 87 localities in which the law was implemented and which were sampled in the census. The sample was restricted to Jewish localities, due to the paucity of observations in Arab localities.

Table 3 displays descriptive statistics from the 1983 census figures, including a comparison of a group of localities in which the law was implemented (hereafter, the treatment group) with localities in which it was not (hereafter, the control group). Certain differences between the level of education in the treatment group and that of the control group, which favored the control group, can be distinguished, particularly in the proportion of those completing 12 years of schooling (a gap of 5 percentage points). There is also a wide gap between the two groups in the proportion of Sephardic Jews: 54 percent in the treatment group, compared with 41 percent in the control group. At the same time, no significant difference was found between the two groups in the size of the community or in family income. Among those of European-American origin, family income in the treatment group was 5 percent higher than in the control group, while among those of African-Asian origin, family income was 2 percent lower in the treatment group than in the control group.

The individuals who benefited from the law were aged 23–28 in the year of the census, too young for examining their individual total years of schooling or income. All that can be checked is the completion of high school education. Furthermore, like the 1995 census, this census also lacks family background data.

[8] In part of the paper, distinctions between non-Jews are also made, i.e., between Christians, Muslims, and Druze.

[9] The Publications Anthology, Government Publications, July 19, 1973.

Table 3
Descriptive Statistics – 1983 Census

	Treatment Group			Control Group		
	Entire Sample	Asia-Africa	Europe-America	Entire Sample	Asia-Africa	Europe-America
Years of education	11.4 (3.2)	11.1 (2.6)	13.0 (3.1)	11.5 (3.4)	11.2 (2.6)	13.2 (2.8)
Years of education – restricted to those with 8-12 years of education	10.7 (1.5)	10.6 (1.5)	11.0 (1.3)	10.7 (1.5)	10.6 (1.5)	11.3 (1.2)
Dummy for completing 9 or more years of education	0.82 (0.4)	0.83 (0.4)	0.94 (0.2)	0.83 (0.4)	0.84 (0.4)	0.96 (0.2)
Dummy for completing 10 or more years of education	0.78 (0.4)	0.78 (0.4)	0.92 (0.3)	0.79 (0.4)	0.80 (0.4)	0.95 (0.2)
Dummy for completing 11 or more years of education	0.66 (0.5)	0.63 (0.5)	0.83 (0.4)	0.69 (0.5)	0.65 (0.5)	0.88 (0.3)
Dummy for completing 12 or more years of education	0.56 (0.5)	0.50 (0.5)	0.75 (0.4)	0.61 (0.5)	0.53 (0.5)	0.82 (0.4)
Dummy for earning matriculation certificates	0.19 (0.4)	0.15 (0.4)	0.26 (0.4)	0.21 (0.4)	0.17 (0.4)	0.27 (0.4)
Age	26.3 (4.6)	26.2 (4.5)	27.4 (4.6)	26.4 (4.6)	26.2 (4.5)	27.4 (4.6)
Dummy for Gender (male=1)	0.50 (0.5)	0.50 (0.5)	0.52 (0.5)	0.50 (0.5)	0.50 (0.5)	0.50 (0.5)
Number of residents in residential community	148,933 (165,300)	119,902 (154,257)	160,846 (164,550)	142,964 (109,942)	134,859 (106,831)	149,517 (110,103)
Monthly family income (in Israeli shekels)	14,795 (19,830)	15,174 (15,893)	17,365 (22,339)	15,297 (21,333)	14.802 (17,733)	18,194 (27,515)
Dummy for Asian-African origin	0.54 (0.5)			0.41 (0.5)		
Dummy for European-American origin	0.22 (0.4)			0.33 (0.5)		
Number of observations	54,385	29,249	12,082	146,160	59,549	47,800

The numbers in the table are averages. The numbers in parentheses are the standard deviations of the variables. The sample was restricted to ages 19–34.

b. Methodology

The purpose of the empirical analysis is to estimate the effect of the three amendments to the law on the duration of the education and the salary of students who benefited from these amendments. In order to demonstrate that the law did have an effect, it is not sufficient to show that the duration of schooling increased following the change, because it is possible that it would have increased even without the law. In order to demonstrate that the longer duration of schooling was due to the extension of the law, I therefore adopted two econometric strategies: (i) defining trend variables designed to reflect the rising trend over time without the law, and a comparison of the duration of studies before and after implementation of the law, given this trend (Section 5a(1)); and (ii) defining a treatment group and a control group, based on the partial introduction of compulsory education in 10th grade, and a comparison of the duration of schooling before and after the law was implemented in each of the groups (Section 5a(2)).

(i) According to the first strategy, since the three changes took place one after another, it was difficult both to examine the effect of each one separately, and to control for the trend. A model was therefore estimated that includes the entire period of the sample and estimates the cumulative effect of the three changes. The gradual implementation of the various amendments provides two estimation possibilities stemming from two different assumptions about the character and duration of the law's effect on the level of education. The two possibilities are displayed in Figure 1. The first assumes that the effect of the law is immediate, while the second assumes that it is gradual.

According to the first possibility, shown in Figure 1A, implementation was immediate, and the difference between the periods preceding and following implementation is therefore a constant that can be estimated through a dummy variable, as proposed in equation (1):

$$(1) \quad Educ = \beta_1 T + \beta_2 T^2 + \beta_3 Law70 + \beta_4 Law74 + \beta_5 Law79 + \beta_6 X \ ,$$

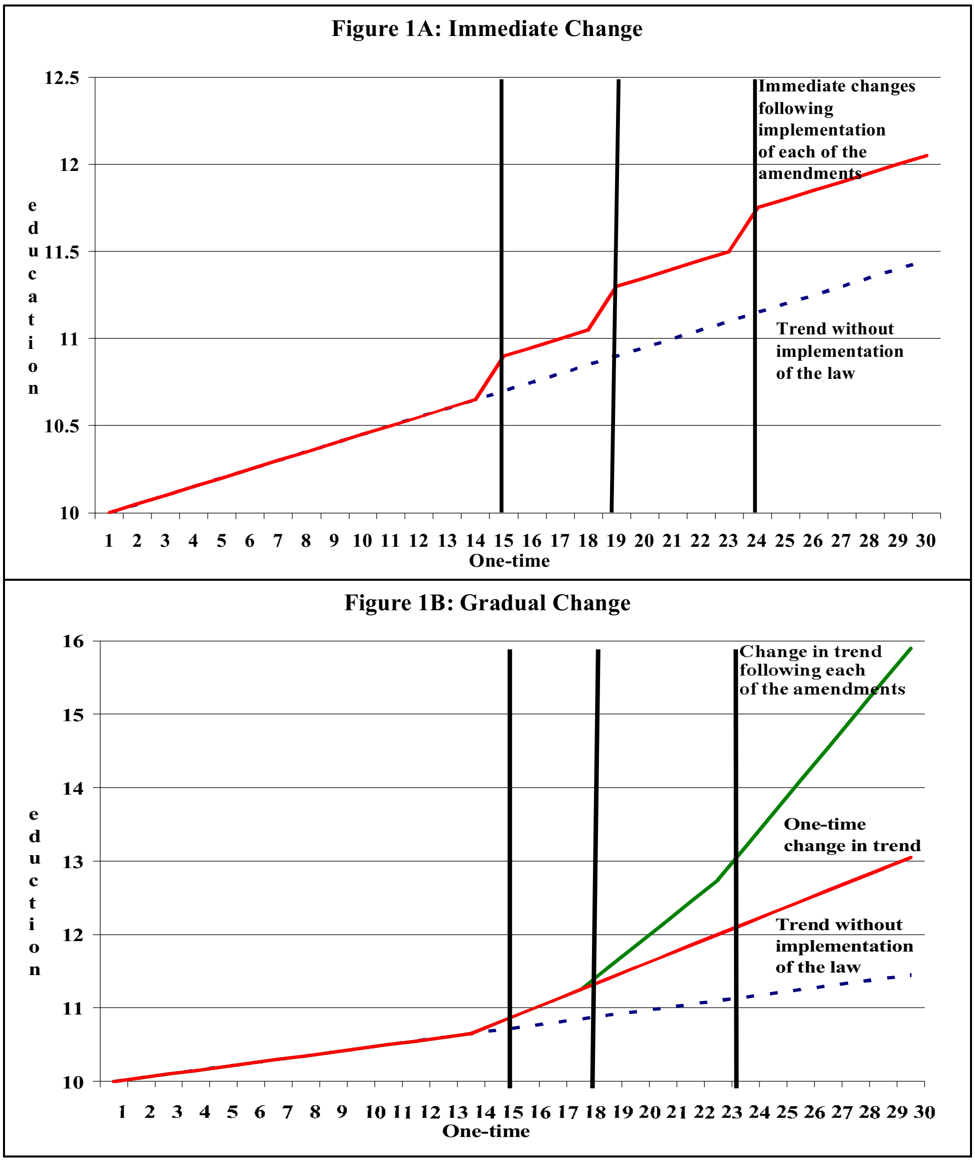
where *Educ* is the duration of studies (the number of years of schooling or a dummy variable for the end of a given school year), and T and T^2 are the time trend variables, which are actually the cohort to which individual *i* belongs. These variables should not have any direct effect on the duration of studies, because the length of studies being examined is up to 12 years of schooling. Their function is to reflect the individual's age cohorts, and to control for the rising trend over time in the average duration of studies. *Law*70, *Law*74, and *Law*79 are three dummy variables for each of the amendments to the law. For example, *Law*70 is 0 for all the individuals who reached age 15 before 1970, who were not required to complete 9th grade, and 1 for those who reached age 15 in 1970 or later and were required to complete 9th grade. A positive coefficient for each of these variables reflects an increase in the average level of education, beyond the time trend, resulting from implementation of the law. X is a vector of additional control variables (gender and ethnic origin). As noted in the data section, important control variables, such as the education and income of the parents, were unavailable.

The second possibility, shown in Figure 1B, which is slightly more reasonable with respect to how the law was implemented, is that the change in the level of education was gradual, not immediate; a change in the trend occurred, not a one-time jump, as reflected in equation (2):

$$(2) \quad Educ = \beta_1 T + \beta_2 T^2 + \beta_3 Law70*T + \beta_4 Law74*T + \beta_5 Law79*T + \beta_6 X \ .$$

Equation (2) is the same as Equation (1), except for one difference: the dummy variables for the various amendments, *Law*70, *Law*74, and *Law*79, are multiplied by T, the time trend variable. A positive coefficient for each of these variables reflects a rise in the time trend of the level of education, compared with the previous trend. Note that in this estimation, even if the coefficients of the two later amendments (β_4 and β_5) are not significantly positive (the curve representing the one-time change in trend in Figure 1B), this does not mean that these amendments had no effect. It is possible that the change in trend caused by the previous amendment faded, to be replaced by an effect of similar magnitude caused by the new amendment.

Figure 1
Alternative Effects of the Law (Illustration)



(ii) According to the second strategy, the effect of the law can be identified through the fact that the law was implemented in 10th grade for only some of the students in 1974–75. This makes it possible to examine the effect of the law on the "treated" group using the difference in differences method. In this method, the differences between the change in the treatment group following implementation of the law and the change in the control group are compared:

(3) $Educ = \delta_1 Law74 + \delta_2 Group + \delta_3 Law74xGroup + \delta_4 X$,

where *Law*74 is again a dummy variable that receives the value 0 for the age cohort that did not benefit from the law and the value 1 for the age cohort that did benefit from it. *Group* is a dummy variable for people living in a community in which the law was implemented in 1974–75, and *Law*74x*Group* is a variable for the interaction between the dummy variable for the period when the law was implemented and the dummy variable for the localities in which it was implemented. If the time trend is identical in the two groups of localities (as will be shown below), this variable reflects the causal effect of the law on the treatment group.

In 1979, implementation of free education for 11th and 12th grade was accompanied by implementation of compulsory education in 10th grade in the other areas of Israel (the localities for which $Group = 0$). By estimation similar to that of equation (3), but for the 1979 amendment, the effect of compulsory education can be isolated from the effect of the free education law:

(4) $Educ = \delta_1 Law79 + \delta_2 Group + \delta_3 Law79xGroup + \delta_4 X$.

In this equation, the coefficient of *Law*79 reflects the effect of the law on the control group (the combined effect of compulsory and free education) while the coefficient of the interaction variable reflects the added effect on the treatment group. Of course, it is expected that this coefficient will be negative, reflecting the effect of implementing compulsory education, which does not apply to the treatment group in this case. The sum of the two coefficients is the effect of the law on the treatment group, and reflects the effect of free education. It should be kept in mind that in this inquiry, the time trend cannot be deducted, because all of the population benefits from some treatment.

Finally, since the hypothesis that implementation of the various amendments had a significant exogenous effect on the duration of studies has been strengthened, this exogenous effect can be used to estimate the return to education from each of the amendments, i.e., the TSLS method can be used to estimate the causal effect of education on the logarithm of salary by using the various amendments as instrumental variables (Section 5b). The resulting estimate for the return can be compared with the return estimated by the OLS method in order to obtain an indication of the gap between the estimates, to compare the estimated effect following the additional compulsory education with that of the estimated effect following the additional free education, and to examine the assertion that implementing compulsory education detracts from the individual's optimal choice.

5. FINDINGS

a. Estimation of the law’s effect

This section presents findings on the magnitude of the effect of the Compulsory Education Law on the duration of studies, particularly on the number of high-school years; on the probability of completing each of 9th, 10th, 11th, and 12th grades; on the probability of a student being eligible for a matriculation certificate; and on the duration of studies at a vocational high schools. Sub-section (1) estimates the law’s effect by controlling for the time trend, while sub-section (2) estimates the law’s effect by comparing a treatment group to a control group.

(1) The first method—controlling for the time trend

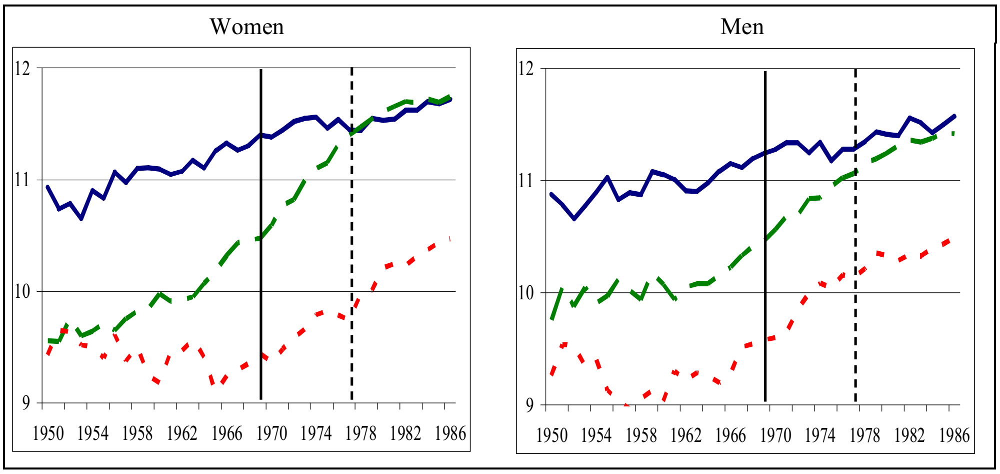
During the 1970s, a number of measures aimed at extending the duration of studies among economically disadvantaged populations in Israel were taken. As explained above, these measures can be divided into three phases: 1970–73 (extension of compulsory education to 9th grade), 1974–75 (partial extension of compulsory education to 10th grade), and 1979 (extension of compulsory education to all of 10th grade and extension of free education to 11th and 12th grades).

Figure 2 presents the average number of years of schooling according to age cohorts in different population groups.[10] The first year in which compulsory education was implemented is marked on a continuous vertical line, and the first year in which free education was implemented is marked on a broken vertical line. During the period in which the law was implemented, a rising trend is indeed evident in the average number of years of schooling in all population groups. Among Sephardic Jewish men, the average rate of increase in 1969–82 was 0.9 years of schooling, compared with 0.3 years of schooling in the 13 preceding years. Among non-Jewish men, the rate of increase during this period was 0.8 years of schooling, compared with 0.5 years of schooling in the corresponding earlier period. Among Jews of European or American origin, the increase was only 0.3 years, but the average numbers of years of schooling in this group was high and near the maximum even before the law was implemented.[11] An increase of 1.2 years of study was recorded among Sephardic Jewish women, and a 0.8-year increase in schooling among non-Jewish women. The increase was also small among women of European-American origin – only 0.2 years of schooling.

[10] 1995 census figures. The sample is restricted to those with 8-12 years of schooling, the relevant years for the law.

[11] Because the number of years of schooling is bounded above, as the average number of years of schooling approaches the upper bound, any small increase is more significant.

Figure 2
Average Number of Years of Schooling, by Age Cohorts

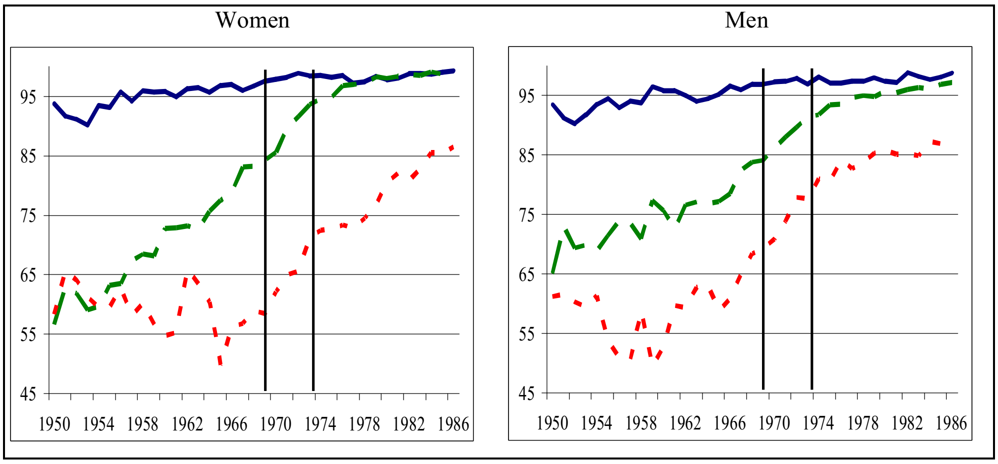


The horizontal axis is the year in which the age cohort became 15 years old. Only those with 8-12 years of education were included in the calculation of the average.
Europe America Asia-Africa Non-Jews

Despite this evidence of an increase in education during the years of the reform, it is difficult, based on these figures, to substantiate the claim that the increase in the number of years of schooling was due to implementation of the law. In order to focus the inquiry on the added years of education relevant to the law, Figure 3 displays the proportion of those completing 8th grade who also completed 9th grade.[12] It can be seen that in the years in which 9th grade was added to the compulsory education framework (between the vertical lines), the number of those completing 9th grade rose significantly in all population groups (except for Jews of European-American origin, among whom the dropout rate in 9th grade was marginal throughout the entire period). At the same time, in most of the groups, it is difficult to establish that the change in trend occurred during the years in which the law was implemented.

[12] This figure is suitable for examining only those affected immediately by the law.

Figure 3
Proportion of Those Completing 9th Grade, by Age Cohorts

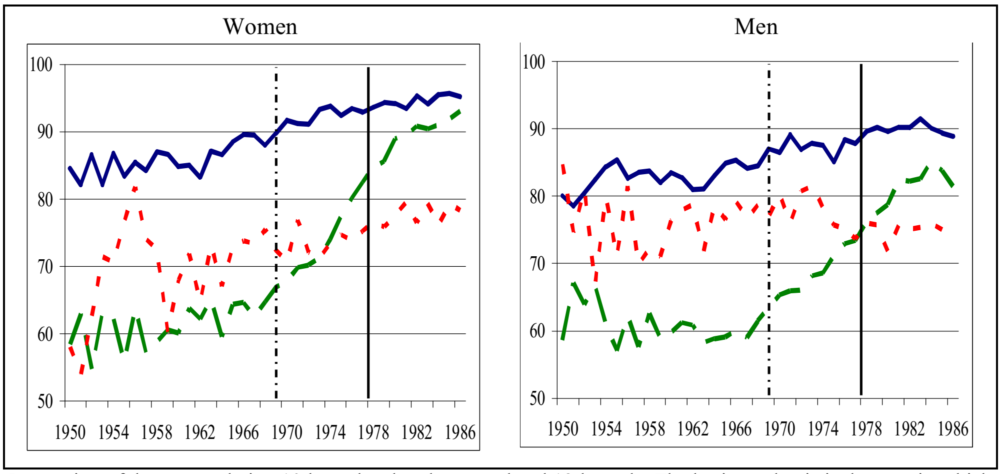


Proportion of those completing 8th grade who also completed 9th grade. The horizontal axis is the year in which the individual became 15 years old.
Europe America Asia-Africa Non-Jews

Figure 4 displays the proportion of those completing 10th grade who also completed 12th grade.[13] Here various processes can be distinguished in each group. Among Jews of European or American origin, a moderate rise in the proportions of those studying in these grades during the entire period, apparently unconnected to implementation of the law, was evident. Among Sephardic Jews, the proportions of those studying were stable until the end of the 1960s. During the period of the reform, these rates rose significantly, both when compulsory 9th and 10th grade was implemented (starting from the dotted line) and when free education was instituted for 11th and 12th grade (starting with the unbroken line). This finding indicates that the contribution for this population of the addition of 9th and 10th grade to compulsory schooling extended beyond these grades; it also caused an increase in education in the higher grades. It appears that the change in the trend was not solely a result of making 11th and 12th grade free; it also resulted from making those grades more accessible to a population that had previously dropped out at an earlier stage. Among the non-Jewish population, the opposite occurred—stability (and even a certain decline among men) in the rate of participation in 11th and 12th grades before the introduction of free education. During the implementation of free education, the rate of participation responded only moderately, with an increase among women and stability among men. This indicates that the population that began 9th grade when it became compulsory, and to a lesser extent 10th grade, tended not to finish high school; they completed only compulsory studies. Only when these grades became free did a small proportion of this population continue studying until the end of high school.

[13] A similar picture emerges from the proportion of those completing 11th grade.

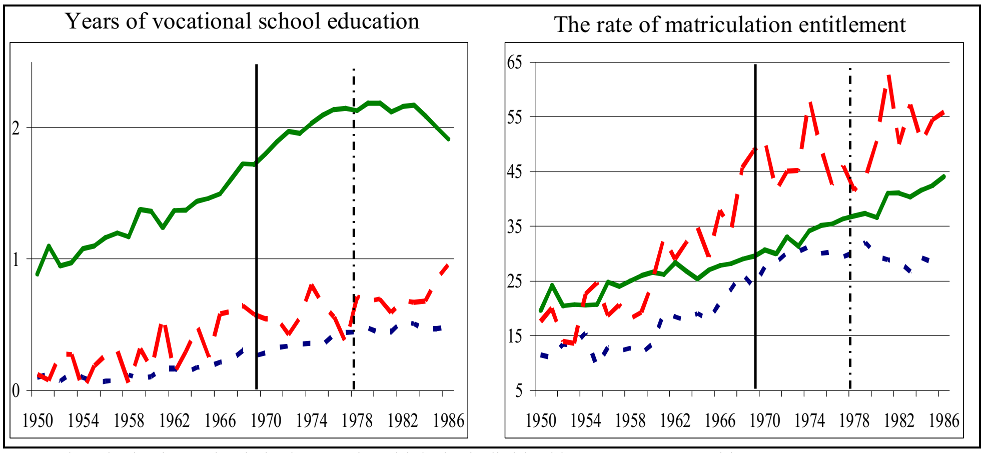
Figure 4
The Proportion of Those Completing 12th Grade, by Age Cohorts



Proportion of those completing 10th grade who also completed 12th grade. The horizontal axis is the year in which the individual became 16 years old.
Europe America Asia-Africa Non-Jews

Another interesting aspect is the effect of the reform on the probability of receiving a matriculation certificate. Beyond the importance of completing 12 years of schooling, a matriculation certificate makes it possible to continue studying in an academic framework. A rise in the proportion of those eligible for a matriculation certificate can therefore reflect a significant improvement in equal opportunity in a society. If making education compulsory and/or free indeed does make it more accessible, we would expect the proportion of those receiving matriculation certificates to also rise following implementation of the reform. As can be seen in Figure 5, among Sephardic Jews, who apparently were most affected by the reform, the trend did not change during the relevant years. On the other hand, among the non-Jews, it appears that the reform led to some change in the trend and a rise in the proportion of those obtaining matriculation certificates. The proportion of those eligible for matriculation certificates among Christians even exceeded that among Sephardic Jews during the period when the law was implemented (among both women and men). As can be seen in Figure 5, education in vocational schools reflects a mirror image of this finding: while the average duration of schooling among Sephardic Jews studying in vocational schools grew significantly, the increase was relatively marginal in the non-Jewish groups. This finding, combined with the other findings in this chapter, shows both aspects of the effect of vocational school education: expansion of access to vocational school education for the Jewish population (both in the period preceding the Compulsory Education Law and when implementation of the law began) led on the one hand to acceleration in the rise of the duration of education among Sephardic Jews, and on the other hand to a slowing of the increase in the proportion of those eligible for matriculation certificates (compared with non-Jews).

Figure 5
The rate of Matriculation entitlement and years of Vocational School Education, by Age Cohorts



Men only. The horizontal axis is the year in which the individual became 15 years old.
Asia-Africa Christians Muslims

Table 4 displays the results of regression for estimating the effect of the three phases in implementing the Compulsory Education Law on the number of years of schooling. The dependent variable is the number of years of education.[14] The independent variables are two trend variables that control for the time trend; a dummy variable for gender; three dummy variables for receiving the "treatment:" (1) those born in 1955 or later, who benefited from the addition of 9th grade to the compulsory education, starting in 1970; (2) those born in 1958 or later, who benefited from the addition of 10th grade to the compulsory education, starting in 1974; and (3) those born in 1963 or later, who benefited from free high school education, starting in 1979; and three variables for the interaction between the "treatment" variables and gender, which test whether the effect of the law was different for women and men. Every column is an estimation for a different population group: Jews of European- or American origin, Jews of Asian or African origin, and non-Jews.

The table shows that the average number of years of secondary schooling rose following the implementation of the various amendments in every population group other than Jews of European or American origin. A significant rising trend can be seen, which among Sephardic Jews moderates marginally over time, and which strengthens over time among non-Jews. This trend is negligible among Jews of European and American origin, and particularly significant among non-Jews. The effect of gender varies from one group and another, and for various specifications of the dependent variable. The overall effect of the law among Sephardic Jews was 0.46 years of schooling, with the increase resulting from implementation of the three amendments, particularly the laws on compulsory schooling.

[14] 1995 census figures. The sample was restricted to those completing 8–12 years of study, the relevant years for the law.

Table 4
The Effect of the Reform on the Number of Years of Schooling[a]

	Europe-America (1)	Asia-Africa (2)	Non-Jews (3)	Christians (4)	Muslims (5)	Druze (6)
Dummy for Gender (Male = 1)	0.03 (0.05)	0.52*** (0.05)	-0.53*** (0.12)	-0.52*** (0.20)	-0.46*** (0.15)	-0.28 (0.49)
Time trend	3.34*** (0.46)	6.33*** (0.41)	-4.83*** (0.89)	0.09 (1.56)	-5.28*** (1.19)	-9.71** (3.78)
Time trend squared	-0.64 (1.58)	-2.85** (1.11)	16.08*** (1.92)	7.46 (4.56)	16.37*** (2.37)	34.93*** (7.37)
Time trend X dummy for gender	-1.31** (0.65)	-4.21*** (0.57)	5.26*** (1.15)	4.42* (2.26)	4.05*** (1.47)	14.19*** (4.44)
Time trend squared X dummy for gender	1.06 (2.17)	5.97*** (1.56)	-11.73*** (2.59)	-6.50 (6.56)	-9.49*** (3.09)	-36.13*** (9.15)
Dummy for 1970 amendment	0.04 (0.04)	0.19*** (0.03)	0.07 (0.06)	0.21* (0.13)	0.08 (0.07)	-0.08 (0.22)
Dummy for 1974 amendment	-0.06 (0.04)	0.27*** (0.03)	0.16*** (0.05)	0.39*** (0.12)	0.19*** (0.06)	-0.04 (0.18)
Dummy for 1979 amendment	-0.12** (0.05)	0.13*** (0.03)	0.16*** (0.05)	0.18 (0.14)	0.18*** (0.06)	0.13 (0.16)
Dummy for 1970 amendment X dummy for gender	0.07 (0.06)	0.03 (0.04)	0.16* (0.08)	-0.16 (0.19)	0.27*** (0.10)	-0.07 (0.27)
Dummy for 1974 amendment X dummy for gender	-0.08 (0.06)	-0.11*** (0.04)	0.12 (0.07)	-0.21 (0.19)	0.08 (0.08)	0.52** (0.23)
Dummy for 1979 amendment X dummy for gender	0.15** (0.07)	-0.04 (0.04)	-0.10 (0.07)	-0.13 (0.20)	-0.14* (0.08)	0.20 (0.22)
Total effect of the law on women	**-0.14* (0.08)**	**0.58*** (0.05)**	**0.39*** (0.09)**	**0.79*** (0.22)**	**0.45*** (0.10)**	**0.01 (0.29)**
Total effect of the law on men	**0.00 (0.08)**	**0.46*** (0.05)**	**0.57*** (0.08)**	**0.28 (0.22)**	**0.65*** (0.09)**	**0.66** (0.24)**
The constant	10.71*** (0.04)	9.29*** (0.04)	9.69*** (0.09)	9.53*** (0.13)	9.68*** (0.13)	9.24*** (0.43)
Number of observations	34,408	87,388	37,413	5,108	28,952	3,316
R^2	0.04	0.17	0.06	0.15	0.06	0.21

[a] 1995 Census figures. The dependent variable is the number of years of education. The sample is restricted to those with 8-12 years of education. The numbers in parentheses are the standard errors.

* Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

The effect was greater among Sephardic Jewish women, totaling 0.58 years of schooling. Among non-Jewish men, the effect of the law totaled 0.57 years of schooling, again, due mostly to implementation of the Compulsory Education Law, while among non-Jewish women, the effect totaled 0.39 years, most of which was due to implementation of free education. In a separate estimation for the three non-Jewish subgroups, it can be seen that the effect of the law on Christians was similar to its effect on Jews of Asian or African origin, and even stronger among Christian women. The effect of the law on Druze and

Muslims was similar to that estimated for non-Jews in general (because most of the people in this group are Muslims). Note that the number of observations of the Christian and Druze sub-groups is relatively low.

In order to augment the validity of this inquiry, which relies on valid control for the time trend, additional specifications for the trend, which were not presented here, were assessed—a purely linear trend, a third degree polynomial, and a logarithmic trend. The results were preserved in all of them.

This method of estimation assumes that the effect of the law was immediate. In practice, as explained above, compulsory education was instituted gradually. I therefore also estimated an alternative model, in which the law causes a rising trend in the extent of education, making the rise in the extent of education gradual, not immediate. The results of this estimation, listed in Appendix A, match most of the findings in Table 2, but the interpretation of the coefficients is slightly more questionable. Nor does it appear that this estimation is preferable to the preceding estimation (the addition to R^2 and the F statistic is very negligible, compared with what was obtained in Table 2).

Table 5 displays the results of regression for estimating the effect of the three phases in implementation of the Compulsory Education Law on the probability of completing 9th through 12th grade, the probability of becoming eligible for matriculation, and the number of years of education at a vocational school. Table 5A displays the estimation for Jews of Asian-African origin, and Table 5B displays the estimation for the non-Jewish population.[15] The table shows that the reform led to a rise in the proportion of those completing studies at each of the high school education levels, from 9th through 12th grades, and in the proportion of those eligible for a matriculation certificate. It also emerges that each of the amendments affected chiefly the dropout rates from the relevant level of studies (for example, the addition of compulsory 9th grade studies led mainly to a rise in the proportion of those completing 9th grade, as explained below). A comparison between the non-Jewish group and Sephardic Jews shows that the addition of years to compulsory education affected both non-Jews and Sephardic Jews, while the addition of years to free education affected mainly Sephardic Jews. Furthermore, in continuation of the followings from Figure 5, there was a steep rise among Sephardic Jews in the number of those studying at vocational schools, and a small increase in eligibility for a matriculation certificate, while development among non-Jews was in the opposite direction. In addition, the effect of the law on the probability of completing at least eight years of schooling and at least fifteen years of schooling (not displayed in the table) was examined, and it was indeed found that the law had no effect on the proportion of those completing these levels, which were not included in the law.

Making 9th grade compulsory caused a significant drop in the dropout rate for 9th through 12th grade among both Sephardic Jews and non-Jews. Among Sephardic Jews, the dropout rate in 9th grade fell by 2.8 percentage point, reflecting a 15 percent decrease in the

[15] 1995 census figures. In order to avoid complicating the reading, there is no differentiation between women and men.

Table 5
The Effect of the Reform on the Proportion of Those Completing Secondary School[a]
Table 5A: Jews of Asian-African Origin

	9th Grade (1)	10th Grade (2)	11th Grade (3)	12th Grade (4)	Matriculation (5)	Vocational School Studies (6)
Dummy for Gender (Male = 1)	-0.40**	-1.08***	-3.04***	-5.85***	-5.80***	0.36***
	(0.19)	(0.21)	(0.25)	(0.26)	(0.27)	(0.01)
Time trend	276.94***	259.64***	212.29***	123.06***	57.69***	8.29***
	(7.29)	(7.47)	(7.79)	(7.92)	(7.70)	(0.29)
Time trend squared	-365.12***	-319.54***	-170.02***	18.46	76.87***	-13.46***
	(14.55)	(15.50)	(17.89)	(19.88)	(22.40)	(0.88)
Dummy for 1970 amendment	2.81***	2.64***	2.64***	3.70***	-0.09	0.16***
	(0.45)	(0.48)	(0.57)	(0.59)	(0.56)	(0.02)
Dummy for 1974 amendment	1.82***	2.44***	4.33***	5.09***	1.88***	0.11***
	(0.37)	(0.41)	(0.52)	(0.57)	(0.56)	(0.02)
Dummy for 1979 amendment	-0.62*	0.01	3.30***	4.89***	0.65	0.04
	(0.32)	(0.38)	(0.50)	(0.59)	(0.67)	(0.03)
Effect of the law	**4.02*****	**5.09*****	**10.27*****	**13.68*****	**2.45****	**0.32*****
	(0.59)	(0.65)	(0.81)	(0.92)	(1.02)	(0.04)
The constant	40.41***	38.62***	27.75***	26.84***	19.42***	0.19***
	(0.78)	(0.79)	(0.79)	(0.77)	(0.69)	(0.03)
Number of observations	121,908	121,908	121,908	121,908	121,908	121,908
R^2	0.13	0.12	0.13	0.13	0.04	0.05

Table 5B: Non-Jews

	9th Grade (1)	10th Grade (2)	11th Grade (3)	12th Grade (4)	Matriculation (5)	Vocational School Studies (6)
Dummy for Gender (Male = 1)	13.69***	12.45***	10.58***	9.47***	6.75***	0.20***
	(0.36)	(0.37)	(0.36)	(0.36)	(0.33)	(0.01)
Time trend	140.10***	101.36***	87.65***	79.70***	86.68***	0.79***
	(9.05)	(8.91)	(8.58)	(8.35)	(7.71)	(0.16)
Time trend squared	-3.56	44.86*	43.91*	36.08	-51.97**	0.37
	(25.02)	(26.39)	(26.43)	(26.18)	(24.50)	(0.54)
Dummy for 1970 amendment	6.60***	3.97***	3.59***	3.34***	2.96***	0.00
	(0.89)	(0.86)	(0.83)	(0.81)	(0.76)	(0.02)
Dummy for 1974 amendment	4.88***	4.19***	2.57***	2.45***	1.38*	0.05***
	(0.84)	(0.84)	(0.82)	(0.80)	(0.75)	(0.02)
Dummy for 1979 amendment	1.25	1.50*	1.47*	1.30	-0.06	0.01
	(0.81)	(0.85)	(0.86)	(0.85)	(0.80)	(0.02)
Effect of the law	**12.73*****	**9.66*****	**7.63*****	**7.09*****	**4.27*****	**0.6****
	(1.29)	**(1.35)**	**(1.35)**	**(1.33)**	**(1.25)**	**(0.03)**
The constant	6.20***	4.67***	3.38***	2.86***	1.60***	-0.07***
	(0.77)	(0.73)	(0.68)	(0.65)	(0.59)	(0.01)
Number of observations	65,270	65,270	65,270	65,270	65,270	65,270
R^2	0.16	0.12	0.09	0.08	0.04	0.03

a. 1995 census figures. The dependent variable is a dummy variable for completion of the number of years of education listed at the top of each column. The regression is linear, and the standard errors are therefore adjusted for heteroskedasticity. The coefficients were multiplied by 100.

* Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

number of dropouts as a whole,[16] while the 3.7 percentage point decrease in the 12th grade dropout rate reflected a 10 percent decrease in the number of dropouts as a whole. This decrease is significant, given the fact that the law did not apply to these grades. Among the non-Jewish population, the reduction in the number of dropouts ranged from 10 percent of the total number of 9th grade dropouts to 5 percent of the total number of 12th grade dropouts. Similarly, making 10th grade compulsory lowered the dropout rate in 9th through 12th grades, with the maximum decrease in the dropout rate being in 10th grade.

The introduction of free education in 11th and 12th grades lowered the dropout rate, beyond the effect of the trend and the previous amendments, by 3.3 percentage point in 11th grade and 4.9 percentage point in 12th grade among Sephardic Jews – a total of 13 percent of the total number of dropouts in these grades. Among non-Jews, the decline in dropout rates following the free education amendment was very small and not significant. The last two columns in Table 3 display the effect of the three amendments on eligibility for a matriculation certificate and on education at vocational schools. As shown in Figure 5, these amendments caused a rise in eligibility for matriculation certificates mostly among non-Jews. Among Sephardic Jews there was a steep rise in vocational school education - half of the rise in the total number of years of schooling.

The findings in Tables 4 and 5 are consistent with the conclusions from the above graphs. They show that the amendments in the Compulsory Education Law had a profound effect among both Sephardic Jews and the non-Jewish population. While men, particularly non-Jewish men, were affected principally by the introduction of compulsory education, women were affected by the introduction of both compulsory and free education. These findings may indicate that the monetary constraint (the short-term liquidity constraint) was more effective among Sephardic Jews (especially Sephardic Jewish women). The very removal of this constraint, whether by compulsory education or by free education, caused a significant rise in the number of years of education. Among men, on the other hand, particularly non-Jewish men, it appears that the monetary constraint was not the only constraint, and the introduction of free education therefore had less effect.

(2) The second method—comparison of the treatment group and the control group

The Compulsory Education Law was partially implemented for 10th grade in 1974–75. At the beginning of every year, a list of localities in which studies were compulsory was published (the treatment group). In the other localities the law was implemented only in 1979, and they serve as a control group for neutralizing the effect of the time trend. Figure 6A displays the proportion of those completing 10th grade in the treatment group and in the control group over time.[17] Figure 6B displays the same figure after controlling for the effect of origin and gender.[18] The vertical lines stand for the period in which the law applied only to the treatment group.

[16] The ratio of the decrease in the number of dropouts to the total number of dropouts was calculated by dividing the regression coefficient by the percentage of dropouts without the law. This calculation is a better reflection of the law's effectiveness than the regression coefficient itself.

[17] 1983 census figures.

[18] The residual from regression in which the dependent variable is a dummy variable for completing 10th grade, and the independent variables are dummy variables for origin and gender.

Figure 6A: The Proportion of Those Completing 10th Grade, by Age Cohorts

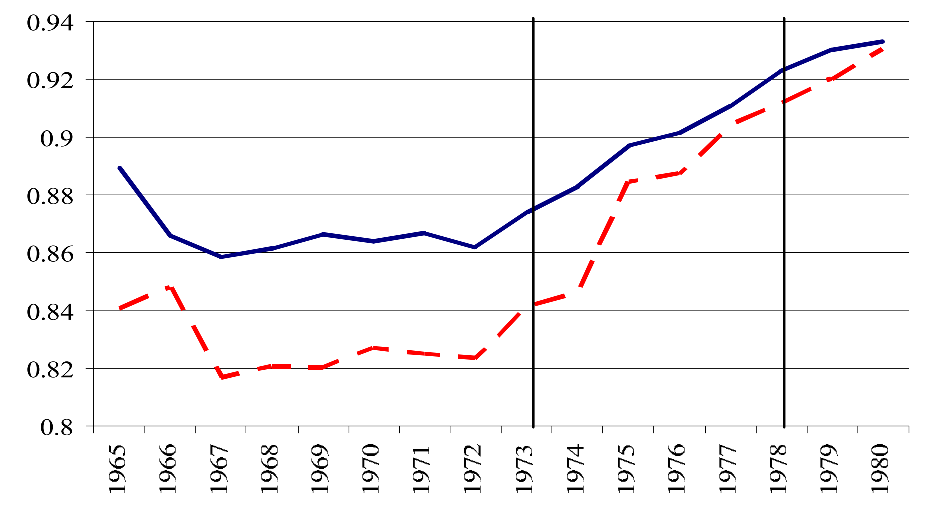
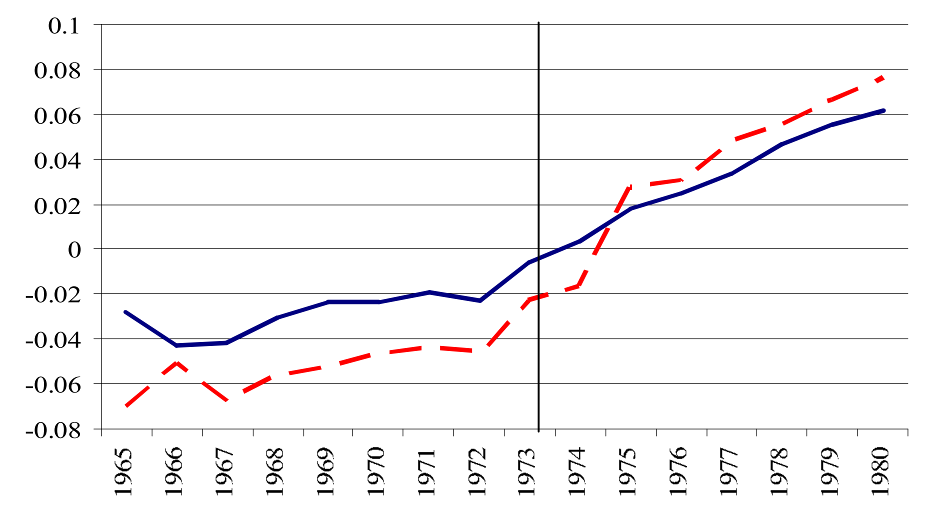


Figure 6B: The Proportion of Those Completing 10th Grade, by Age Cohorts (After controlling for the Effect of Origin and Gender[a])



The horizontal axis is the year in which the individual became 16 years old.
The control group ——— The treatment group – – –
a. The regression residual after controlling for gender and origin

According to Figure 6A, before the law was implemented, there was a wide gap between the groups in the proportion of those completing 10th grade. This gap was mostly explained by the weight of Sephardic Jews in the treatment group (Table 3). Indeed, according to Figure 6B, after deducting the effect of origin, the gaps are greatly reduced. Despite the gap between the groups, a similar development trend over time can be distinguished: stability up until 1972, and a moderate rise in 1973. This rise could be due to the implementation of compulsory education in 9th grade in 1970–73, which, as noted, led to a higher rate of study at all educational levels. After the law was implemented in 1974, the moderate rising trend in the control group continued, while there was a steep rise in the treatment group, causing the gap between the groups to close almost completely. Figure 6B shows a similar picture: the gap narrows, and even reverses. This finding supports the hypothesis that the increase in the number of years of schooling during this period was due to implementation of the law.

Table 6
The Effect of the 10th Grade Compulsory Education Law on the Probability of Completing 10th Grade[a]

	1 year before and 1 year after	The entire sample	
Dummy for Asian-African origin	-0.12** (0.004)	-0.14*** (0.002)	-0.14*** (0.002)
Dummy for gender (female = 1)	0.04*** (0.005)	0.03*** (0.002)	0.03*** (0.002)
Time trend			0.0002 (0.0009)
Time trend squared			0.0004*** (0.00004)
Dummy for treatment group	-0.02** (0.008)	-0.02*** (0.003)	-0.02*** (0.003)
Dummy for years of treatment	0.02*** (0.006)	0.06*** (0.002)	0.005 (0.003)
Dummy for the effect of the law	**0.026** (0.012)**	**0.033*** (0.004)**	**0.032*** (0.004)**
Constant	0.91*** (0.005)	0.91*** (0.002)	0.89*** (0.004)
Rate at which participation expanded[b]	**0.17**	**0.25**	**0.21**
Number of observations	16,340	127,813	127,813
R^2	0.039	0.055	0.057

[a] 1983 census figures. The numbers below the coefficients are the standard errors adjusted for heteroskedasticity.
[b] The rate of increase in those completing 10th grade following the law, divided by the proportion of dropouts in the population without the law.
* Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Table 6 displays an estimate, obtained by using the difference in differences method, of the effect of implementing the Compulsory Education Law in 10th grade on the probability of completing 10th grade.[19] The independent variables are: (1) dummy variables that control for origin and gender; (2) time trend variables; (3) a dummy variable for the treatment group, i.e., the group of localities that benefited from the amendment; this variable controls for the initial gaps between the two groups; (4) a dummy variable for years of treatment, i.e.,

[19] 1983 census figures.

the period following implementation of the law; this variable controls for the time trend common to both groups; (5) a dummy variable for the law's effect, i.e., a variable for the interaction between the treatment group and the years of treatment, reflecting the unique time trend for the treatment group, this trend being explained by the law. The first column in the table was restricted to individuals turning 16 one year before the law was implemented (who did not benefit from it) or a year after the law was implemented (who did benefit from it). The following columns use the complete sample of age 19 through age 34 (eight age cohorts before the law was implemented, and eight after its implementation).

The table shows that the law caused a 3 percentage point rise in the proportion of those completing 10th grade, amounting to 20 percent of the dropouts without the law,[20] slightly higher than the increase estimated under the first method. The proportion of those completing 10th grade was only 2 percentage points lower in the treatment group than the proportion in the control group. Most of the differences in the proportion of those finishing were due to origin and gender. These results persist in both the restricted sample and in the general sample. In addition, in contrast to the findings in the preceding section, the effect of the law was similar for both women and men, and for both Sephardic and Ashkenazi Jews.

Estimation of the difference in differences assumes that the differences between the treatment group and the control group are random, or at least that the time trends for the two groups are identical. The control group can therefore be used as an alternative universe for assessing the situation without the law. In Table 3 in the data section, the data show differences between the groups in average education, average income, and especially in the proportion of Sephardic Jews. This indicates that the socioeconomic status of the treatment group was lower. The differences are apparently due to a selective choice of localities, giving rise to concern that the estimated treatment effect is a result of a different in trend between the two groups, not the law. On the other hand, according to Figure 5, it appears that there was no difference in trend between the groups. In order to test the differences in trends statistically, the model in Table 6 was estimated for a "placebo" treatment: instead of the 1974–75 treatment years, four fictitious treatment years were chosen, 1969–72, and with the help of the sample of the year before and a year after, the hypothesis of a change in favor of the treatment group was tested (identical regression to that in the first column of Table 6). The results of the estimation appear in Appendix B. In all of them, the estimate of the effect of the fictitious treatment's effect was negligible and insignificant, a finding that substantiates the hypothesis that the control group can be used for comparison.

In 1979 free education for the whole country (in 11th and 12th grades) and compulsory education (in areas where it had not been implemented in 1974, i.e., the control group in Table 6) were put into effect simultaneously. In order to distinguish between the effect of compulsory education and that of free education, I estimated the law's effect on both the treatment group and the control group. The effect of the law on the treatment group reflects the influence of free education, because compulsory education had already been in effect there for a number of years. The effect of the law on the control group reflects the

[20] The last row in the table displays the proportion of the decline in the number of dropouts: the rate of increase in those completing 10th grade following the law, divided by the proportion of dropouts in the population without the law.

combined influence of compulsory and free education. The gap between the two reflects the influence of compulsory education, beyond that of free education. Table 7 displays the effect of the 1979 amendment on the probability of completing 12th grade.[21] The sample was restricted to one year before the amendment was implemented and one year after it. The independent variables are a dummy variable for Jews of Asian-African origin, a dummy variable for gender, a dummy variable for the treatment group (which appeared above), a dummy variable for the year in which the amendment took effect (reflecting the influence of the amendment on the control group (the combined effect of compulsory and free education), and a variable for the interaction between the treatment group and the year in which the law was implemented. This variable, the gap between the effect on the treatment group and the effect on the control group, reflects the effect of compulsory education. The larger negative magnitude that this variable has, the greater the proportion of the combined effect that can be attributed to the effect of compulsory education.

Table 7
The Effect of the Free Education Law on the Probability of Completing 12th Grade[a]

		Sephardic Jews		
	The entire sample	Total	Men	Women
Dummy for Asian-African origin	-0.19*** (0.006)			
Dummy for gender (female = 1)	0.07*** (0.006)	0.10*** (0.009)		
Dummy for treatment group	-0.03*** (0.011)	-0.02 (0.015)	-0.01 (0.021)	-0.03 (0.021)
Dummy for the effect of the law	**0.041*** (0.007)**	**0.074*** (0.011)**	**0.066*** (0.016)**	**0.081*** (0.016)**
Dummy for the gap between the effect of compulsory education and the effect of free education	**-0.00 (0.015)**	**-0.02 (0.020)**	**-0.03 (0.029)**	**-0.00 (0.028)**
Constant	0.814*** (0.007)	0.60*** (0.010)	0.60*** (0.012)	0.69*** (0.011)
Rate at which participation expanded[b]	**0.13**	**0.20**	**0.09**	**0.24**
Number of observations	16,917	9,681	4,890	4,791
R^2	0.057	0.009	0.004	0.009

a. 1983 census figures. The numbers below the coefficients are the standard errors adjusted for heteroskedasticity.
b. The rate of increase in those completing 12th grade following the law, divided by the proportion of dropouts in the population without the law.
* Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

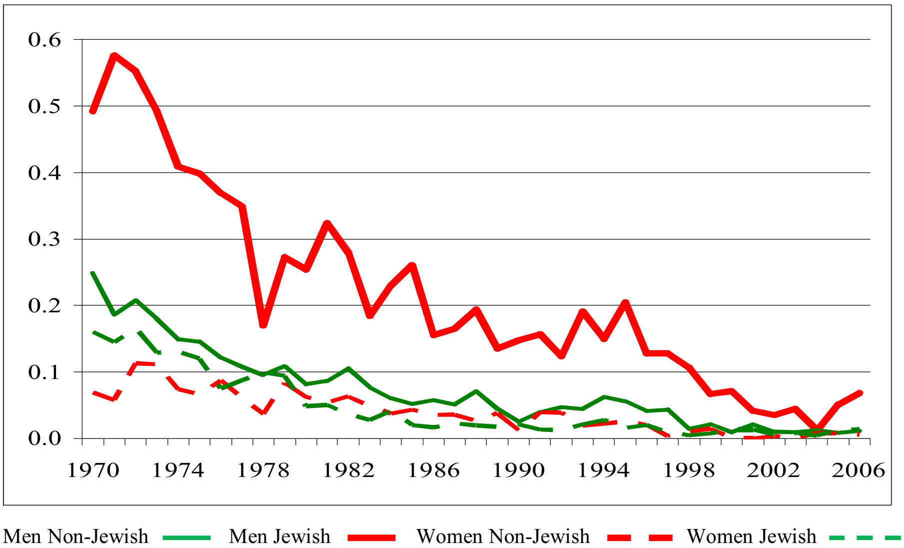
The table shows that the probability of completing 12th grade in the treatment group did not differ significantly from the probability in the control group. It therefore follows that the decline in dropping out from 12th grade in this year was due solely to the introduction of free education. This finding is no surprise, because by 1979, the dropout rates for 10th

[21] 1983 census figures.

grade had stabilized at a very low level. The law caused a 4.1 percentage point decrease in dropping out from the 12th grade, amounting to 13 percent of all dropouts without the law. Among Sephardic Jewish women, the effect was stronger, totaling 24 percent of the dropouts, while the effect among Sephardic Jewish men was a more limited 9 percent. In comparison with the estimated effect of compulsory education, free education was less effective among men.

A summary of the findings in Section 5a shows that implementation of both free and compulsory education had an effect on the duration of studies in the educational stages relevant to the law, and that the effect of introducing compulsory education was stronger than that of implementing free education, particularly among non-Jews and men. These findings indicate the existence of liquidity constraint on investment in human capital. It appears that the financial constraint, reflecting difficulty in financing studies, is effective, and its removal lowered the dropout rates significantly. At the same time, the effect of compulsory education, beyond that of free education, shows that for some individuals, a further constraint exists deterring them from studying, even after free education was put into effect. A certain indication of the nature of this constraint can be seen in the difference between men and women and between non-Jews and Sephardic Jews in the effect of free education. This could imply a greater alternative cost of acquiring education for men, especially non-Jews. Support for this assertion comes from an examination of the proportion of 17 year-old working full-time (Figure 7).[22] In 1970, 50 percent of the non-

Figure 7
Proportions of 17-year-olds Working Full-Time



Source: Labor Force Surveys for 1970-2009.

[22] Source: Labor Force surveys for 1970-2006.

Jewish 17 year-old boys worked full-time, compared with only 25 percent among Jewish 17 year-old boys. The proportion of working 17-year-old girls was 15 percent among Jews and only 7 percent among non-Jews.

b. The return to education

The preceding section studied the effect of the three phases of the law on the dropout rate and the number of years of education. It found that in addition to the financial constraint, an additional constraint existed that deters some individuals from acquiring education. This section will attempt to examine the nature of this constraint by comparing the return to education derived from the introduction of compulsory education with the return to education derived from the introduction of free education. If this constraint was of the type defined in the literature as a long-term constraint, the return to the amendment forcing individuals to acquire education that is non-optimal for them should be lower than the return to the amendment enabling all those wishing to acquire free education to do so. On the other hand, if a short-term liquidity constraint is involved, resulting, for example, from decision-making in a family framework, rather than a personal one, it is likely that adding compulsory studies that are non-optimal for the family, but optimal for the child, will also yield a high return.

After it was found that these amendments did affect the duration of studies, they could then be used as instrumental variables for the purpose of estimating the return to education derived from the law. The first condition for the validity of an instrumental variable is that it must have a sufficiently strong exogenous effect on the endogenous variable (the number of years of education). It is clear that in this case, the law had an exogenous effect on the number of years of study, but there is room to distinguish between the effect of introducing free education and that of introducing compulsory education. In both cases, the estimate of the return reflects a causal effect, but in each case, the return will be for a population of a different type. A comparison between the two estimates obtained will answer the study's question. The second condition for the validity of an instrumental variable is the absence of any direct effect on the dependent variable (salary). This assumption cannot be tested directly, but if there is a diminishing marginal productivity on human capital it might not hold if the law had a macro effect; a rise in the average number of years of education could lead to a drop in the return to education, thereby leading to underestimation of the private return.

Table 8 displays the estimated return to education in two stages using the three phases of the law as instrumental variables. The estimate is calculated with the help of 1995 census figures, the dependent variable is the logarithm of salary,[23] and the independent variable is the number of years of education, which was restricted, as in the rest of the study, to those with 8-12 years of schooling. The left panel (columns 1-4) displays the return derived from

[23] In the census, a question was presented about salary in September 1995, and about the number of working days in this month. A question was also presented about the number of monthly working hours in an average month. The return estimated through the three variables: total salary, salary per working day, and hourly wage –both by OLS and TSLS – is very similar (see Appendix C). The total salary gave more consistent results for various specifications, and is therefore used in this study.

Table 8
The Return to Education Derived from the Amendments to the Compulsory Education Law[a]

	Return to the Free Years				Return to the Compulsory Years			
	First Stage		Second Stage		First Stage		Second Stage	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Asia-Africa	Non-Jewish	Asia-Africa	Non-Jewish	Asia-Africa	Non-Jewish	Asia-Africa	Non-Jewish
Dummy for 1970 amendment	-0.5 (0.38)	-1.2 (0.81)	-0.1 (0.22)	-0.1 (0.42)	0.21*** (0.02)	0.15*** (0.04)		
Dummy for 1970 amendment X time trend	2.7 (1.77)	5.5 (3.72)	0.4 (1.02)	0.4 (1.93)				
Dummy for 1974 amendment	1.93*** (0.40)	3.20*** (0.84)	0.1 (0.25)	0.0 (0.49)	0.22*** (0.02)	0.21*** (0.04)		
Dummy for 1974 amendment X time trend	-7.41*** (1.79)	-12.98*** (3.74)	-0.6 (1.08)	0.0 (2.13)				
Dummy for 1979 amendment	0.18*** (0.03)	0.18*** (0.04)			0.12*** (0.02)	0.10*** (0.03)	0.0 (0.01)	0.0 (0.02)
Dummy for gender (Male = 1)	-0.13*** (0.01)	0.12*** (0.02)	0.63*** (0.02)	0.61*** (0.05)	-0.13*** (0.01)	0.12*** (0.02)	0.62*** (0.01)	0.60*** (0.02)
Time trend	1.72*** (0.65)	-5.44*** (1.27)	0.2 (0.41)	1.33* (0.69)	4.23*** (0.29)	-1.57*** (0.56)	0.52** (0.23)	1.10*** (0.31)
Time trend squared	10.58*** (2.61)	26.25*** (5.23)	-4.70*** (1.56)	-6.46*** (3.11)	-0.050 (0.78)	9.22*** (1.28)	-5.40*** (0.43)	-5.38*** (0.66)
Education			**0.15**** **(0.08)**	**0.10** **(0.09)**			**0.14***** **(0.03)**	**0.08**** **(0.06)**
Constant	9.73*** (0.04)	9.45*** (0.07)	6.30*** (0.75)	6.51*** (0.85)	9.62*** (0.03)	9.28*** (0.06)	6.46*** (0.28)	6.72*** (0.37)
F test for I.V.	47.3	19.6			198.2	39.1		
Number of observations	87,388	37,413	57,214	17,394	87,388	37,413	57,214	17,394
R^2	0.16	0.06	0.23	0.12	0.16	0.06	0.23	0.13

a. 1995 census figures. Estimates are made using the TSLS method. The dependent variable is the logarithm of the salary. The endogenous variable is the number of years of education. The sample is restricted to those with 8-12 years of education. The numbers in parentheses below the coefficients are the standard errors.

* Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

the implementation of the free education amendment in 1979. The right panel (columns 5-8) displays the return derived from implementation of compulsory schooling in 1970–74. In both panels, the first two columns present the first stage—the effect of the law on the number of years of education: the first column for Jews of Asian-African origin, and the second column for non-Jews. The next two columns display the second stage—estimating the return to education for the same regression as in the first stage.

In the right panel, the instrumental variables are the dummy variables for the 1970 and 1974 amendments (the two compulsory education amendments), and the control variables are the two trend variables – the dummy variable for men and the dummy variable for the 1979 free education amendment.[24] As can be seen from the table, the influence of the instrumental variables according to the F test was sufficiently significant to use them as instrumental variables for both Sephardic Jews and non-Jews. The estimated return caused by the law was 14 percent for Sephardic Jews and 8 percent for non-Jews.

In the left panel, the instrumental variable is the dummy variable for the 1979 (free education) amendment, and the control variables are the two trend variables – a dummy variable for men and four dummy variables for the 1970–74 compulsory education amendments: two for the level and two for the trend.[25] As can be seen in the table, the effect of the instrumental variables according to the F test was sufficiently significant to use them as instrumental variables, although they were substantially less significant than the compulsory education amendments. The estimated return resulting from the law was 15 percent for Sephardic Jews and 10 percent for non-Jews.

The gap between the return derived from the compulsory education amendments and the return derived from the free education amendment was small and insignificant, and the hypothesis that the compulsory education amendment forces unproductive schooling without a return in the labor market on some students can therefore be rejected. In terms of the findings, this means that a short-term constraint of the type proposed was what deterred some individuals from studying, not a long-term constraint. This was due to the fact that the decision on investment in human capital was taken for reasons other than maximizing salary, such as the need to help support the family, an absence of information about the benefit of schooling, dislike of studying as a result of low achievement, or even a strong individual discount rate.

Another interesting comparison is the return obtained using the OLS method, displayed in Table 9. The sample population is of age 30–40 in the 1995 census (the relevant age cohorts for the period of the law). Column 1 is for the entire sample, while the other columns are for those with 8-12 years of schooling. The average return to education, as estimated by the OLS method, was 9 percent. This figure, however, cannot be compared with the return estimated using the IV method, because the instrumental variable led to the addition of 9th to 12th grade, in which the return was higher than average. The return for these years under the OLS method averaged 13 percent for the entire population and 17 percent among Jewish Sephardic women (the group most affected by the amendment).

[24] This was done so that the instrumental variables would not also reflect the effect of free education. Another alternative tested was restricting the sample to the period preceding the free education law, and the results were similar.

[25] This was done so that the instrumental variables would not also reflect the effect of compulsory education. In contrast to the preceding table, it was not possible to both restrict the sample to the period after compulsory education was instituted and to control for the trend. An alternative model of the year before the amendment and the year after it was therefore estimated, without the effect of the trend, and the results were similar.

Figure 8 displays the marginal return to the educational years estimated under the OLS method by including a dummy variable for each level of education in the regression.[26]

Table 9
Return to Education – OLS[a]

	The entire sample (1)	Those with 8-12 Years of Education: The entire sample (2)	Women – Asia-Africa (3)	Women – Non-Jewish (4)	Men – Asia-Africa (5)	Men – Non-Jews (6)
Education	0.09***	0.13***	0.17***	0.09***	0.10***	0.04***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)
Age	0.06*	0.0	0.20**	-0.2	0.0	0.1
	(0.03)	(0.04)	(0.08)	(0.25)	(0.07)	(0.09)
Age squared	0.0	0.0	-0.0**	0.0	0.0	0.0
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Dummy for gender	0.55***	0.58***				
(Male = 1)	(0.01)	(0.01)				
Constant	5.34***	5.42***	1.8	9.65**	6.59***	6.35***
	(0.55)	(0.71)	(1.48)	(4.38)	(1.16)	(1.48)
Number of observations	57,686	32,516	8,565	916	10,293	5,229
R^2	0.230	0.200	0.070	0.070	0.050	0.020

a. 1995 census figures. The numbers under the coefficients are the standard errors.

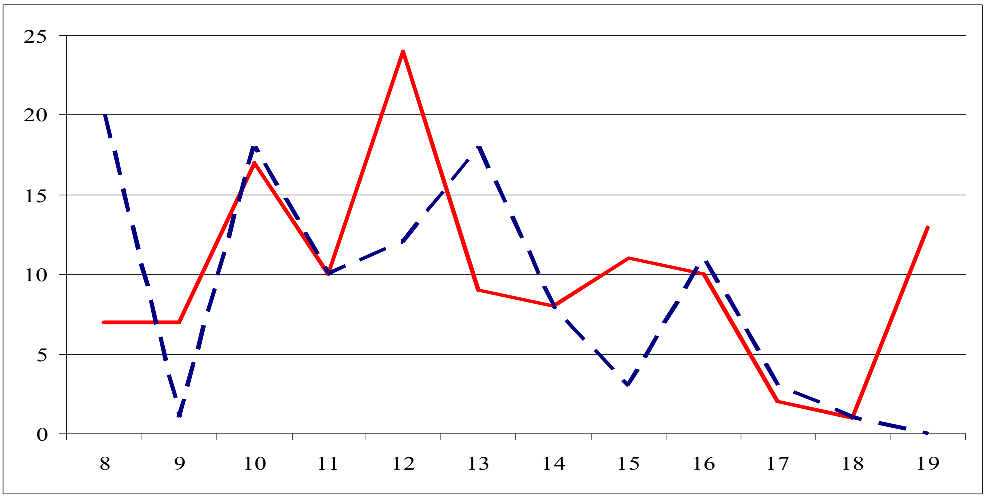
* Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

It can be seen that in general, the marginal return drops off slightly, reflecting a diminishing marginal return on human capital. At the same time, fluctuation is considerable, indicating the importance of education as a signal in the labor market. Among men, the highest return was from completing 8th grade (grammar school), from the high school years, and from 16 years of education (BA degree). Other years gave a lower return. Among women, the highest return was 24 percent for 12 years of schooling (completing high school), while the return for the 13th year of education was only 9 percent.

The amendments led to the addition of the 9th through 12th grade educational stages in various population groups (women, men, Sephardic Jews, and non-Jews). It is therefore difficult to find the specific marginal return from the OLS estimate, which is comparable to that obtained from TSLS. The relevant range varies from 10 percent to 20 percent. The average return for Sephardic Jews is 14 percent, similar to that estimated for this group using TSLS. The average return for non-Jews was 7 percent, lower than that estimated using TSLS. It can therefore be stated that the estimated return using the OLS method does not have an upward bias, despite the omission of ability variables, which should be correlated with both education and salary.

[26] For the complete estimate, see Appendix D.

Figure 8
The Marginal Return to Education – OLS



The horizontal axis is the number of years of education. The vertical axis is the marginal return in percentages.
Women Men

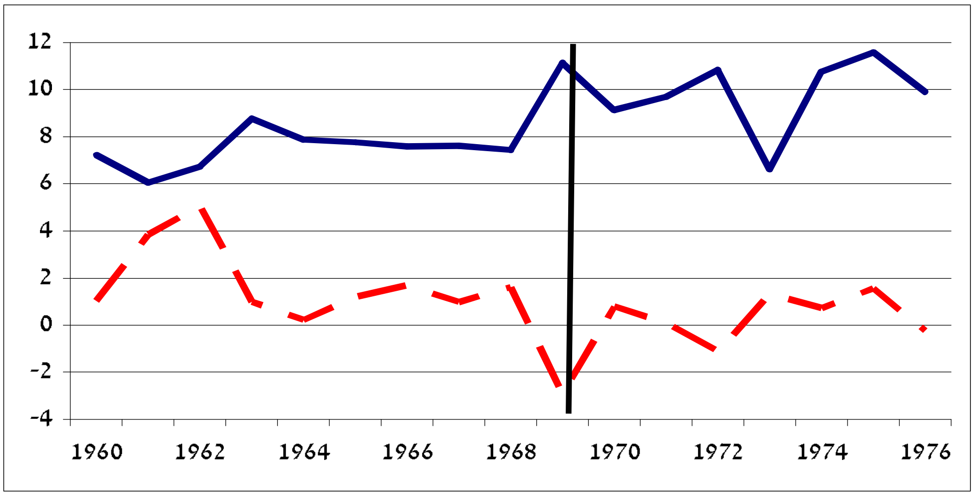
c. Heteroskedasticity in Return to Education

More evidence of the existence of liquidity constraints is the relatively constant level of heteroskedasticity in the salary equation over time. If the variance in the level of education is due to heterogeneity in individuals' abilities, rather than their liquidity constraints, it should be expected that as the level of education rises over time, the variance in the abilities of educated people will rise, and the variance in the abilities of uneducated people will drop (particularly if it stems from implementation of the Compulsory Education Law). This follows the shifting of those with lesser abilities from the uneducated to the educated group. This process should be reflected in an increase in heteroskedasticity in estimating the return to education over time.

In order to test this phenomenon, the return to education was estimated over time using income surveys for the years 1991–2006. this was done to facilitate estimation of the return to education for every cohort (year of birth), while controlling for the age variable, which provides an indication of an individual's experience. After the salary regression was estimated, the correlation between the variance of the error term and the independent variables from the salary regression was estimated. The positive correlation between the variance of the error term and education may indicate that the variance in the abilities of educated people is greater than the variance in the abilities of those without education.

Figure 9 displays the return to education and heteroskedasticity among Jews of Asian-African origin over time. The year in which the cohort became 15 years old appears on the horizontal axis, and the vertical axis represents the year in which compulsory education was instituted for 9th grade. It can be seen that no change in heteroskedasticity took place over time, especially not after the compulsory education amendment was implemented. It can be concluded that no significant change occurred in the distribution of the return following the extension of education, and that the gaps in education were therefore not due to gaps in the ability to benefit from it.

Figure 9
The Return to Education and Heteroskedasticity over Time



The sample was restricted to Jews of Sephardic origin only. The horizontal axis is the year in which the age group became 15 years old.
Return to education ▬▬ Heteroskedasticity ▬ ▬
The return to education is estimated with the help of income surveys for 1991-2006. The dependent variable is the logarithm of the hourly wage, and the independent variable is the number of years of education (8-12). The control variables are age, age squared, and gender.
Heteroskedasticity is estimated through regression in which the dependent variable is the variance of the error from the salary regression, and the independent variables are education, age, age squared, and gender. The estimate displayed is the estimate of the effect of education.

6. SUMMARY

This study examined the effect of the amendments to the Compulsory Education Law on education and salary. The aim was to describe the type of constraints facing individuals making decisions about the extent of the investment in their human capital, while distinguishing between two main types of constraints: a short-term liquidity constraint, consisting of difficulty in financing studies or a need to help support the family, and a long-term liquidity constraint, consisting of the child's educational deprivation at a young age, which prevents him from deriving benefit from continuation of his studies. In order to test this, an empirical comparison was conducted between the effect of the introduction of compulsory education and the effect of the introduction of free education in Israel. The law underwent a number of changes during the 1970s: the legislature added 9th and 10th grade to the compulsory-free educational framework in 1970–75, and added 11th and 12th grade to the free educational framework in 1979.

It was found that the Compulsory Education Law caused a significant rise in the proportion of those completing 9th grade through 12th grade. It was found that this rise was due to both the introduction of compulsory education in 9th and 10th grades and the introduction of free education in 11th and 12th grades. At the same time, it was found that introducing compulsory education had a greater effect, reflected not only in the compulsory grades themselves, but also in 11th and 12th grades, in which education was neither compulsory nor free at the time. It was also found that the return to education derived from these changes amounted to 14 percent. This return, which resulted from the implementation of both compulsory and free education, was similar to the return estimated through simple salary regression for of high school education.

These findings indicate the existence of a short-term liquidity constraint on investment in human capital. It appears that the financial constraint, reflected in difficulty in paying for studies, is effective, since its removal significantly reduced the dropout rate. At the same time, in addition to the financial constraint, another existing constraint deterred some individuals from studying, even after free education was introduced. Since the return to education from the addition of compulsory years of education is no lower than the return to the addition of free educational years, it can be concluded that the decision not to study is not optimal in salary terms, and is not due to the long-term liquidity constraint. It can therefore be hypothesized that the decision not to study is made not by the child, but by the family, for whom the optimal choice is to put the child to work. Support for this hypothesis is obtained from the high rates of child employment among males, particularly non-Jewish males, before the law was implemented. These groups responded more to the introduction of compulsory education, and less to the introduction of free education. Together with the decline in dropout rates, child employment rates fell steeply in these groups.

These findings lead to the conclusion that implementing compulsory-free education is preferable to implementing only free education. The decision in the context of the 2008 state budget to add 11th and 12th grades to the years of compulsory education is therefore justified. Furthermore, if the liquidity constraint also exists at higher educational levels (which it is unfeasible to make compulsory), alternative ways should be found to encourage the acquisition of higher education among economically disadvantaged groups, in addition to subsidizing tuition. For example, the Shochat Committee, which conducted an inquiry into the higher educational system in Israel, recommended a system of loans for payment of tuition, while subsidizing the risk element of the loan - a system that provides a solution to the financial constraint – combined with an additional system of monetary aid for students from particularly disadvantaged backgrounds.

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Appendix A: The Effect of the Reform on the Trend of the Numbers of Years of Education

	Europe-America (1)	Asia-Africa (2)	Non-Jews (3)	Christians (4)	Muslims (5)	Druze (6)
Dummy for gender (Male = 1)	0.03 (0.05)	0.53*** (0.05)	-0.55*** (0.11)	-0.48** (0.20)	-0.48*** (0.15)	-0.35 (0.47)
Time trend	3.12*** (0.53)	7.43*** (0.44)	-3.93*** (0.86)	1.37 (1.68)	-4.22*** (1.13)	-9.62*** (3.61)
Time trend squared	0.45 (2.08)	-7.33*** (1.40)	12.19*** (2.23)	2.15 (5.91)	11.82*** (2.60)	33.99*** (7.98)
Time trend X dummy for gender	-1.15 (0.74)	-4.40*** (0.61)	5.55*** (1.13)	3.41 (2.43)	4.43*** (1.42)	15.33*** (4.29)
Time trend squared X dummy for gender	0.36 (2.87)	6.75*** (1.97)	-12.54*** (3.08)	-1.78 (8.51)	-10.70 (3.53)	-40.47*** (10.32)
Dummy for 1970 amendment	0.11 (0.20)	0.99*** (0.13)	0.49* (0.30)	1.13* (0.63)	0.53 (0.35)	-0.27 (1.04)
Dummy for 1974 amendment	-0.30 (0.19)	1.04*** (0.11)	0.67*** (0.23)	1.49*** (0.54)	0.81*** (0.26)	-0.06 (0.79)
Dummy for 1979 amendment	-0.39** (0.18)	0.28*** (0.11)	0.55*** (0.18)	0.40 (0.49)	0.61*** (0.20)	0.60 (0.58)
Dummy for 1970 amendment X dummy for gender	0.36 (0.28)	0.12 (0.18)	0.71* (0.40)	-0.93 (0.94)	1.21** (0.47)	-0.22 (1.30)
Dummy for 1974 amendment X dummy for gender	-0.33 (0.27)	-0.48*** (0.16)	0.36 (0.32)	-0.88 (0.81)	0.15 (0.36)	2.22** (1.02)
Dummy for 1979 amendment X dummy for gender	0.50* (0.25)	-0.10 (0.16)	-0.51** (0.25)	-0.40 (0.71)	-0.65** (0.28)	0.43 (0.78)
Constant	10.71*** (0.04)	9.23*** (0.04)	9.65*** (0.09)	9.47*** (0.13)	9.63*** (0.12)	9.25*** (0.42)
Number of observations	34,408	87,388	37,413	5,108	28,952	3,316
R^2	0.04	0.17	0.06	0.14	0.06	0.21

Source: 1995 census figures. The numbers below the coefficients are the standard errors.

* Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Appendix B: The Placebo Effect on the Probability of Completing 10th Grade

	1969	1970	1971	1972
Dummy for those of Asian-African origin	-0.22***	-0.19***	-0.20***	0.18***
	(0.006)	(0.006)	(0.006)	(0.006)
Dummy for gender (Female = 1)	0.01*	0.02***	0.01**	0.02***
	(0.005)	(0.005)	(0.005)	(0.005)
Dummy for treatment group	-0.02*	-0.02**	-0.02**	-0.02**
	(0.010)	(0.009)	(0.009)	(0.009)
Dummy for treatment years	0.025***	0.01	0.01	0.01
	(0.006)	(0.006)	(0.006)	(0.006)
Dummy for the effect of the law	**0.00**	**0.00**	**0.006**	**0.00**
	(0.013)	**(0.013)**	**(0.013)**	**(0.013)**
Constant	0.93***	0.93***	0.94***	0.93***
	(0.005)	(0.005)	(0.005)	(0.005)
Rate at which participation expanded[1]	**0.00**	**0.00**	**0.03**	**0.00**
Number of observations	16,195	16,579	16,438	16,299
R^2	0.097	0.073	0.080	0.059

Source: 1995 census figures. The numbers below the coefficients are the standard errors.

* Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Appendix C: Return to Education Using the OLS and TSLS Methods According to Different Income Variables[a]

	Open Squares (OLS)			Open Squares in 2 Stages (TSLS)		
	Logarithm of Total Salary (1)	Logarithm of Daily Wage (2)	Logarithm of Hourly Wage (3)	Logarithm of Total Salary (4)	Logarithm of Daily Wage (5)	Logarithm of Hourly Wage (6)
Education	0.13***	0.12***	0.12***	0.14***	0.13***	0.15***
	(0.00)	(0.00)	(0.00)	(0.03)	(0.03)	(0.04)
Age	0.0	0.0	-0.1			
	(0.04)	(0.04)	(0.06)			
Age squared	0.0	0.0	0.0**			
	(0.00)	(0.00)	(0.00)			
Dummy for gender	0.58***	0.54***	0.18***	0.62***	0.58***	0.25***
(Male = 1)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Time trend				0.55**	0.49**	0.1
				(0.21)	(0.22)	(0.31)
Time trend squared				-5.52***	-5.04***	-5.00***
				(0.32)	(0.32)	(0.49)
Constant	5.42***	2.18***	1.91*	6.46***	3.23***	1.37***
	(0.71)	(0.72)	(1.09)	(0.28)	(0.29)	(0.42)
Number of observations	32,516	28,992	31,635	57,214	51,144	55,733
R^2	0.200	0.190	0.030	0.230	0.230	0.040

Source: 1995 census figures. The numbers below the coefficients are the standard errors.

* Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

a. In the first and fourth columns, the dependent variable is the logarithm of the total salary. In the second and fifth columns, the dependent variable is the daily wage. In the third and sixth column, the dependent variable is the hourly wage. The sample is restricted to Jews of Asian-African origin with 8–12 years of education.

Appendix D: The Marginal Return to Education – OLS

		Women			Men		
	The entire	The entire sample	Africa-Asia	Non-Jews	The entire sample	Africa-Asia	Non-Jews
Age	0.06**	0.1	0.14**	-0.1	0.0	0.0	0.1
	(0.03)	(0.05)	(0.07)	(0.17)	(0.04)	(0.06)	(0.08)
Age squared	0.0	0.0	-0.00*	0.0	0.0	0.0	0.0
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Dummy for gender (Male = 1)	0.57***						
	(0.01)						
Dummy for completion:							
8 years of study	0.17***	0.1	0.0	0.1	0.20***	0.12**	0.17***
	(0.02)	(0.05)	(0.08)	(0.07)	(0.02)	(0.05)	(0.02)
9 years of study	0.19***	0.14***	0.21***	0.1	0.21***	0.24***	0.14***
	(0.02)	(0.05)	(0.08)	(0.08)	(0.02)	(0.05)	(0.03)
10 years of study	0.36***	0.31***	0.29***	0.26***	0.39***	0.30***	0.26***
	(0.02)	(0.04)	(0.07)	(0.08)	(0.02)	(0.04)	(0.03)
11 years of study	0.46***	0.41***	0.44***	0.2	0.49***	0.39***	0.26***
	(0.02)	(0.04)	(0.07)	(0.10)	(0.02)	(0.04)	(0.03)
12 years of study	0.63***	0.65***	0.66***	0.42***	0.61***	0.51***	0.32***
	(0.02)	(0.04)	(0.07)	(0.06)	(0.02)	(0.04)	(0.02)
13 years of study	0.77***	0.74***	0.73***	0.42***	0.79***	0.70***	0.41***
	(0.02)	(0.04)	(0.07)	(0.09)	(0.02)	(0.05)	(0.05)
14 years of study	0.85***	0.82***	0.84***	0.53***	0.87***	0.76***	0.35***
	(0.02)	(0.04)	(0.07)	(0.07)	(0.02)	(0.05)	(0.04)
15 years of study	0.92***	0.93***	0.97***	0.91***	0.90***	0.77***	0.53***
	(0.02)	(0.04)	(0.07)	(0.06)	(0.02)	(0.05)	(0.03)
16 years of study	1.02***	1.03***	1.07***	0.80***	1.01***	0.93***	0.41***
	(0.02)	(0.04)	(0.07)	(0.07)	(0.02)	(0.05)	(0.03)
17 years of study	1.05***	1.05***	1.12***	0.91***	1.04***	0.96***	0.51***
	(0.02)	(0.04)	(0.08)	(0.11)	(0.03)	(0.05)	(0.05)
18 years of study	1.05***	1.06***	1.08***	0.81***	1.05***	0.90***	0.56***
	(0.02)	(0.04)	(0.08)	(0.12)	(0.03)	(0.06)	(0.05)
19 years of study	1.11***	1.19***	1.25***	1.09***	1.05***	1.02***	0.72***
	(0.03)	(0.05)	(0.09)	(0.18)	(0.03)	(0.07)	(0.06)
20 years of study	1.04***	1.07***	1.01***	1.05***	1.03***	0.92***	0.61***
	(0.03)	(0.06)	(0.10)	(0.23)	(0.04)	(0.09)	(0.08)
21 years of study	0.95***	1.08***	0.92***	0.82*	0.88***	0.65***	0.1
	(0.05)	(0.09)	(0.19)	(0.45)	(0.06)	(0.13)	(0.19)
22 years of study	0.96***	0.98***	0.81***	0.0	0.95***	1.11***	0.55***
	(0.05)	(0.08)	(0.20)	(0.00)	(0.06)	(0.15)	(0.13)
23 years of study	0.90***	0.89***	1.95***	0.0	0.90***	0.94***	0.62***
	(0.11)	(0.21)	(0.64)	(0.00)	(0.12)	(0.34)	(0.21)
24 years of study	0.88***	1.21***	1.0	0.0	0.66***	0.3	-0.1
	(0.11)	(0.18)	(0.64)	(0.00)	(0.14)	(0.29)	(0.28)
Constant	5.64***	5.59***	4.30***	7.75**	6.50***	6.71***	6.52***
	(0.55)	(0.85)	(1.15)	(3.01)	(0.71)	(1.01)	(1.31)
Number of observations	57,686	24,990	13,182	1,940	32,696	14,338	7,971
R^2	0.25	0.15	0.15	0.20	0.18	0.13	0.08

Source: 1995 census figures. The numbers below the coefficients are the standard errors.
* Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.