The Long-Term Consequences of Free School Choice*

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Abstract

I study the long-term consequences of what amounted to an effective free school choice program which two decades ago targeted disadvantaged students in Israel. I show that the program led to significant gains in post-secondary education, through increased enrolment in academic and teachers' colleges but without any increase in enrolment in research universities. Free school choice increased also earnings at adulthood of treated students. Male students had much larger improvements in college schooling and labour market outcomes. Female students, however, experienced higher increases in marriage and fertility rates, which most likely interfered with their schooling and labour market outcomes.

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1. Introduction

The evaluation of educational programs and interventions has focused on short-term outcomes, primarily standardized test scores, as a measure of success. However, understanding that the purpose of education is to improve lifetime well-being, attention has shifted recently to long term consequences at adulthood. In light of the increasing economic returns to higher education, the initial focus has been on post-secondary attainment (Heckman and LaFontaine 2010; Acemoglu and Autor 2010). Garces et al (2002), Ludwig and Miller (2007) and Deming (2009) studied the long-term benefits of Head Start; Schweinhart et al (2005) examined the long term effect of the Perry Preschool program; Chetty et al (2011) studied the effect of kindergarten classroom on earnings in early adulthood; Dustmann et al (2012) examined the effect of high school quality on completed schooling and labour-market outcomes; Dynarski et al (2013) examined the effect of smaller classes in primary school on college entry, college choice, and degree completion; Deming et al (2013) studied the impact of accountability pressure in high schools on post-secondary attainment and earnings; Chetty et al (2014) examined the earnings consequences of primary and middle school teacher quality; and Deming, Billings, and Rockoff (2014) studied the impact of the end of race-based busing on college attainment and young adult crime.

The common goal of these studies is to determine which interventions are more effective in improving long-term outcomes, but the scope of educational interventions studied is still limited, and much remains to be unravelled. In this paper I study the long-term consequences of free school choice offered to primary school students at the point of transition to secondary schools. The main question I address here is whether the effects of free school choice persist beyond attainment and test scores in high school, and lead to long-term enhancements to human capital and well-being. I do so by studying a school choice experiment which was conducted two and a half decades ago in the city of Tel-Aviv, Israel. The program had positive short and medium-term effects on cognitive outcomes and schooling attainment during middle and high school and improved students' social skills (Lavy 2010). In this paper I study whether the free school choice among public schools had long-term effects on social and economic outcomes. This paper provides the first evidence of links between school choice, students' employment and earnings, and social outcomes at adulthood. I examine the impact on various types of post-secondary schooling that vary by quality, on employment, and on earnings at 11-13 years after high school graduation, almost twenty years after students were able to exercise free school choice. I also examine several potential mechanisms for these effects.

I observe students' outcomes every year from high school graduation (2000-2001), until age 30-32 (in 2014). Thus, I can estimate the treatment effects for every year in the period, and trace the dynamic evolution of the program effect. The evidence shows that the school choice increased post-secondary schooling. Treated students are 4.6 percentage points more likely to enrol in post-secondary schooling, and complete almost an additional a fifth year of college in comparison to students in the control group. These effect sizes reflect an 11% increase relative to pre-program means and are similar in magnitude to the effect of the program on end of high school matriculation outcomes. The increase in post-secondary schooling reflects mainly an increase in academic (as opposed to vocational) education, through increased enrolment in academic and teachers' colleges, but without any increase in enrolment in research universities. This is not a surprising result, since those affected by the program are marginal students from low socio-economic backgrounds who would probably not enrol in any academic post-secondary schooling if not for the school choice program.

It is important to note that these results are general equilibrium in nature, because those affected by the experiment are a very small proportion of their cohort and therefore the expansion in post-secondary schooling in the treated sample is not at the expense of others who could have been 'crowded out' by the new demand for higher education. Furthermore, these effects occurred during a period of expansion of the supply of academic colleges in Israel. Otherwise, the concern of general equilibrium effect will have to be addressed in a context of at scale implementation of such school choice program.

Alongside these gains in post-secondary schooling, average annual earnings among treated students 11-13 years after high school graduation increased by 6%-7% relative to the control group mean. These gains are due to improvements in high school outcomes (matriculation composite score, matriculation diploma, number of matriculation subjects at honour level) and in post-secondary schooling attainment, both of which are highly correlated with labour market earnings.

Finally, I find that the school choice program did not affect the marriage rate and age of marriage but it delayed by half a year the age of having the first child. However, this finding disguises large heterogeneity in the effect on marital outcomes by gender, with significant effects on females and no effect on males.

Exploring potential mechanisms for these effects, I find no evidence of sorting of high achieving students to high achieving out of districts schools, namely the choice program did not cause a "cream skimming" effect in the form of higher enrolment rate (take-up rate) of strong students relative to weak students in high achieving schools. However, district 9 students were able through school choice to improve on average the quality of their peers and the quality of the secondary school they attended. This was achieved mainly by better student-school matching and also by a gain in school quality. The larger was the latter, the bigger was the short and long-term effect of the choice program.

The lessons learned from this analysis are relevant to other educational settings in developed countries. Both the high school system in Israel and its high-stakes exit exams are very similar to those in other countries. Importantly, variants of the school choice program studied here have been implemented in recent years in developed countries.[1] The Tel-Aviv reform structure bears also great resemblance to many recent programs in US schooling districts where, like in Tel-Aviv, free choice has replaced zoning and cross-district busing. In the US free choice was introduced under court order

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[1] For example, school choice in Sweden and in particular in Metropolitan Stockholm provides since the mid 1990's relatively disadvantaged children with better educational opportunities, as students can choose a school within or outside their respective catchment area (Böhlmark, Holmlund and Lindahl, 2015, Wondratschek et al (2014)). In the UK, a national program of parental choice of schools established, particularly by the Education Reform Act 1988, and through subsequent laws and amending legislation. Consequently, all public and state schools are 'choice schools', and all families have the right to choose any school, within or outside their local education authority and schools cannot anymore refuse entry to any student until reaching enrollment capacity (Fitz and Taylor, 2003). Both of these programs have many features that are similar to those of the Tel-Aviv program.

that terminated race-based busing while in Tel-Aviv the choice reform was an initiative of the city school authorities and parents' organizations.[2]

Another important advantage of the evidence presented in this paper is that school choice can be directly implemented as a public policy, while most recent studies of longer-term outcomes cover measures that are not as easily influenced by policy, such as school or teacher quality.

There is little causal evidence on the long-term effect of school choice even though it is a controversial policy. The earlier studies examined the short term effects of voucher programs, for example Rouse (1998) and Cullen, Jacob and Levitt (2006). More recently, Chingos and Peterson (2013) report on an experiment that offered a private-school voucher to low-income families. Overall, this study reports no significant effects on college enrolment of the voucher offer, but they estimate large significant impacts for African-American students and smaller but not statistically significant impacts for Hispanic students. In recent years, the focus shifted to short and long term effects of open enrolment-free school choice programs, in particular on misbehaviour and crime and on post-secondary schooling attainment. Demming (2011) examines the public school choice lotteries in Charlotte-Mecklenburg school district and find that seven years after random assignment, lottery winners had significant reduced crime rates (less serious crimes and fewer days incarcerated). Demming et al (2013) study the same program in Charlotte-Mecklenburg and find a significant overall increase in college attainment among lottery winners who attend their first-choice school. Wondratschek et al (2013) study the short and long-term effect of Sweden's 1992 school choice reform, and find it had very small positive effects on marks at the end of compulsory schooling, but it had no effects on university education, employment, criminal activity and health at age 25.

The remainder of the paper is as follows. In Section 2 I describe the Tel-Aviv school choice program, in section 3 I present the data and in section 4 I outline the identification and econometric model. In Section 5 I present the results and in section 6 some conclusions.

[2] An example is the North Carolina Charlotte-Mecklenburg School Board vote to terminate school bussing and move forward with districtwide open enrollment for the 2002–2003 school year (see details Deming et al 2014).

2. Background

The analysis of the short and medium-term effects, presented in Lavy (2010), indicated that the Tel-Aviv school choice program reduced the dropout rate from 7th to 12th grade by 7.3-8.4 percentage points (a 32% decline), increased the matriculation rate by 8 percentage points (a 25% increase), and increased the average score in the *Bagrut* exams by almost 7 points (a 12% increase). It also improved the quality of schooling as the number of *Bagrut* credit units increased by 2 (a 11% increase), the number of credits in science subjects increased by one third of a unit (a 13% increase), and the number of *Bagrut* subjects studied at honour level increased by 0.3 (a 13% increase). I summarize below the main features of the Tel-Aviv school choice program and then examine whether these gains were translated into economically meaningful improvements in adulthood.

2.1 The Tel-Aviv School-Choice Program

In May 1994, the Israeli Ministry of Education approved a two-year trial of the Tel-Aviv School Choice Program (TASCP) in the 9th district (see Map 1 in online appendix). It was the first school choice program in the country since the 1968 education reform that enacted compulsory integration in grades 7–9.[3] TASCP was a response to parents' dissatisfaction with students' outcomes and with the rigid lack of school choice. Its objectives were to give disadvantaged students access to better schools, facilitate a better match between students and schools, and motivate school productivity improvements through competition. The 9th schooling district included 16 public primary schools - 12 secular and 4 religious. Until 1994 the graduates of five of these secular primary schools were bused to one of 5 secondary schools in districts 1-5 in north Tel-Aviv (about 36% of the districts' pupils) and a few more of the districts' pupils (5%) were enrolled in charter schools outside

[3] The 1968 reform established a three-tier structure of schooling: primary (grades 1–6), middle (7–9), and high school (10–12). The reform established neighborhood school zoning as the basis of primary enrollment and of the integration and busing of students out of their neighborhoods in middle school. In Tel-Aviv, most middle schools were part of six-year high schools and there were several high schools that only offered the higher grades (10th-12th).

the district. The graduates of the seven other secular primary schools were assigned to one of the three secondary schools within district 9.[4]

In May 1994 the Tel-Aviv Education Board announced that as of September 1994 this system would be replaced by free choice for the incoming 7th graders, while older cohorts in the district would continue with the old system. The program's main features were: (a) Allowing students to choose their middle-high school from a given set of five schools that included three out of districts schools (in districts 1-5 and these were the same schools to which students were bused before the program) and two within district schools. The choice set varied by primary school and students were asked to rank their preferences among the five schools in their choice set. The city opened information centres and ran workshops for parents and pupils, and high schools held open days to provide information about the choice program for the incoming 7th grade cohort. (b) In the event of excess demand for a particular school, students were assigned to schools by a committee of the city school authority, in a manner that maintained a socioeconomic balance, matching the respective makeup of the city. Siblings in the same school and school capacity were also used as criteria to balance enrolment. (d) Students were guaranteed that at least one friend from their list of nominated friends will attend the same school. (e) Schools were forbidden from any form of discrimination in accepting students – neither geographic nor merit based. (f) The choice program was accompanied by a decision that all the city's post-primary schools would be six grade structures that included the middle (7th–9th) and higher grades (10th–12th) as part of the same school. Most of the city's post-primary schools were already structured this way and only four schools had to be expanded to include middle-school grades. (g) Each school was given additional autonomy, and task forces were set up to assist schools develop their identity and improve their administrative and pedagogical processes. (h) Schools who enjoyed expanded enrolment gained more resources as school budget was based on enrolment.

Prior to the implementation of the program the following steps were taken.[5] School administrators were engaged and briefed by the municipality's education administration. Parents and

[4] These schools were located on the same campus but they were very different in terms of their curriculum and programs offered to students. For example, one included low and high-tech vocational schooling.

students were given information on the program and the choices available to them in a series of open meetings, written material that was distributed and 'open days' that each school held for prospective students. An information centre was set up to answer questions and provide additional information through meetings and written material. A major step in the preparation process was the opening of a new school in district 9, the second school in this district. It opened in the 1993 school year, and so we can observe it for only one academic year before the program. A third school was also opened the following year but it had low enrolment from the beginning and it was closed few years later.

According to city council protocols dating from 1995[6] and also based on other secondary sources, over 90% of students received their first choice, the rest getting their second choice.[7] Interestingly, both the protocols and the secondary sources reveal that in the aggregate, this control system was used to increase the number of students studying in Northern schools - by 15%, relative to the number that would have studied there had there been no control of student choice. Levy et al (1998) provide details on the number of students who were denied their first choice in in-district schools, based on administrative records that I do not have access to. These numbers demonstrate that almost all students who did not get their first choice were those that chose the new school in district 9. The Municipality employed its 'control and regulation systems' only for these students who ranked that school as their first choice. Some of these adjustments were later overturned in an appeals process, though many of the appeals were for students who had in fact received their first choice – possibly indicating that some students used this process to 'change their minds'.

An important element in the new program was the expansion of the supply of middle school classes: four high schools, two in district 9 and two in the city's northern districts, which had previously only offered the higher grades (10th-12th), were expanded to offer middle school grades at

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[5] Shapira, Shavit and Heiman (1994) for the Tel-Aviv municipality, and Heiman, Shapira and Bar-Shalev (1997).

[6] Tel-Aviv council protocol dated March 19th, 1995. These figures are met with some skepticism from a vocal opponent of the program in the city council.

[7] The Tel-Aviv Educational Authority (1999).

the commencement of the reform. In a later section of the paper I discuss why this change in the structure of secondary schools in Tel-Aviv does not confound my estimates of the effect of the school choice program. Beyond the fact that over two thirds of the secondary schools in the city had already a six-grade structure, I also highlight and discuss the fact that two of the three control groups that I use for identification, implemented a similar change in their secondary schools' structure around the same time that it was done in Tel-Aviv.

Over time the choice program led to the expansion of some high schools and to the contraction of others (one school was even closed due to declining enrolment). Enrolment in the city's schools was also affected by the stricter enforcement of the Ministry's rule that pupils were not allowed to attend schools outside of Tel-Aviv. Because school budgets were determined according to enrolment, schools that expanded enrolment gained more resources.

The change to a six-grade structure that includes the middle (7th-9th) and higher grades (10th-12th) as part of the same school allowed the city to cancel the admission process at the end of the 9th grade and to introduce the concept of 'persistence', whereby students automatically enrolled in the 10th grade in the school in which they completed their middle school education. This important component of the reorganization of the school system in Tel-Aviv strongly limited the ability of schools to select students into their higher grades based on academic performance. The explicit default became that pupils could remain in the school they chose in the 7th grade for the duration of their secondary education. To overcome this default option, a school had to gain explicit approval from a special city committee: consent was only given in cases where pupils displayed severe behavioural problems and never on the grounds of poor academic performance. This policy change most likely explains a large part of the dramatic decline in the pupil transfer rate in 9th grade, from about 50% before the choice program to about 15% following it.

The same school choice program was rolled in gradually in the other school districts of Tel-Aviv. First it was expanded in 1996 to district 8, then in 1998 to district 7, and in 1999 to the rest of the city (Tel-Aviv Educational Authority, 2001).

The school choice program in Tel-Aviv was implemented relatively earlier than most choice programs in other countries but it shared many similar elements. Importantly, like the Charlotte-

Mecklenburg school choice program of 2002 and other similar programs in the US, it replaced a regime of school bussing that served socio-economic integration in schools. For the first time in Israel children had a free choice among public schools and could opt out of neighbourhood schools.[8]

2.2 The Israeli High School Matriculation System

When entering high school (10th grade), students choose whether to enrol in the academic or non-academic track. Students enrolled in the academic track receive a matriculation certificate (*Bagrut*) if they pass a series of national exams in core and elective subjects taken between 10th and 12th grade. Students choose to be tested at various proficiency levels, with each test awarding one to five credit units per subject, depending on difficulty. Advanced level subjects are those subjects taken at a level of four or five credit units; a minimum of 20 credit units is required to qualify for a *Bagrut* certificate. About 52% of all high school seniors received a *Bagrut* in the 1999 and 2000 cohorts

[8] The Charlotte-Mecklenburg, North Carolina, school district (CMS) includes Mecklenburg County, which includes both the inner city areas of Charlotte and its suburbs. Following a 1971 Supreme Court ruling (Swann v. Charlotte-Mecklenburg Board of Education) and for over 30 years, CMS schools bused students across the district to achieve racial desegregation. In 2001 this busing plan was terminated when the CMS School Board voted to move forward with districtwide open enrollment for the 2002–2003 school year (see details in Hastings, Kane, and Staiger 2008 and Deming et al 2014). Similar termination of school busing and the introduction of free choice among public schools were adopted over the years in many other school districts in the US. The CMS and the Tel-Aviv choice programs share other similar features. For example, the submission of a list of school choices for each child, listed in order of preference, extensive information campaign to encourage parents to submit choice forms, preparation of comprehensive booklets with information about each school, informing parents that school choice forms were required to receive a school assignment in the subsequent year, guaranteeing to most students first choice. However, when demand exceeded supply, admission was allocated in CMS by lottery while in Tel-Aviv such circumstances were resolved by an assignment committee as explained above. In both programs siblings of currently enrolled children received guaranteed access, free transportation was provided, and the school authority expanded capacity at schools where it anticipated high demand in an attempt to give everyone his first choice.

(Israel Ministry of Education, 2001). The *Bagrut* is a prerequisite for university admission and receiving it is an economically important educational milestone. For more details on the Israeli high school system, see Abramitzky and Lavy (2014).

3. The Data

In this study I use data from administrative files for students in primary schools that were enrolled into the school choice program, pre (sixth graders in 1992 and 1993) and post (sixth graders in 1994) treatment cohorts, and similarly for the control schools, respective pre and post cohorts. The students in the sample completed high school between 1999 and 2001, and in 2013 they are adults, age 29-31. I use several panel datasets available from Israel's National Insurance Institute (NII). The NII is responsible for social security and mandatory health insurance in Israel. NII allows restricted access to this data in their protected research lab.

The underlying data sources include: (1) the population registry data (a national ID number that appears also in all the data sets described below and enable matching and merging the files at the individual level), which contains information on marital status, number of children and their birth dates; (2) NII records of postsecondary enrolment from 2000 through 2013 based on annual reports submitted to NII by all post-secondary education institutions, from which we calculated the number of years of post-secondary schooling[9]; (3) Israel Tax Authority information on income and earnings of employees and self-employed individuals for 2000-2014; (4) NII records on unemployment benefits, marriage and fertility for the period 2009-2012.

The NII matched and merged these files using the individual level national ID number. The matching is perfect without loss of observations. NII than linked this merged file to students' background data that I used in Lavy (2009) to study the effect of the teachers' incentive experiment

[9] The NII, which is responsible for the mandatory health insurance tax in Israel, tracks postsecondary enrollment because students pay a lower health insurance tax rate. Postsecondary schools are therefore required to send a list of enrolled students to the NII every year. For the purposes of our project, the NII Research and Planning Division constructed an extract containing the 2001–2013 enrollment status of students in our study.

on high school academic outcomes. This information comes from administrative records of the Ministry of Education on the universe of Israeli primary schools during the 1997-2002 school years. In addition to an individual identifier, and a school and class identifier, it also included the following family-background variables: parental schooling, number of siblings, country of birth, date of immigration if born outside of Israel, ethnicity and a variety of high school and high school achievement measures. This file also included a treatment indicator, school ID and cohort of study. I had restricted access to this data in the NII research lab at the NII headquarters in Jerusalem.

3.1 The Post High School Academic Schooling System in Israel

The post high school academic schooling system in Israel includes seven universities (one of which confers only graduate and PhD degrees), and over 50 colleges that confer academic undergraduate degrees (some of these also give master's degrees).[10] All universities require a *bagrut* diploma for enrolment. Most academic colleges also require a *bagrut*, though some look at specific *bagrut* diploma components without requiring full certification. For a given field of study, it is typically more difficult to be admitted to a university than to a college. The national university enrolment rates for the cohort of graduating seniors in 1995 (through 2003) was 27.6%and the rate for academic colleges was 8.5%.[11]

The post-high school outcome variables of interest here are indicators of ever having enrolled in a university and in an academic college as of the 2013 school year, and the number of years of schooling completed in these two types of academic institutions by this date. We measure these two outcomes for our 1999-2001 12th grade students. Even after accounting for compulsory military

[10] A 1991 reform sharply increased the supply of postsecondary schooling in Israel by creating publicly funded regional and professional colleges.

[11] These data are from the Israel Central Bureau of Statistics, Report on Post-Secondary Schooling of High School Graduates in 1989–1995 (available at: http://www.cbs.gov.il/publications/h_education02/h_education_h.htm).

service[12], we expect that most students who enrolled in academic post-high school education, including those who continued beyond the undergraduate level, to have graduated by the 2013 academic year. Figures 1-3 that present evidence about the evolution in post-secondary education, show very small changes in ever enrolled rates and years of schooling beyond the age of 30. The similarity in magnitude of the rates in the treatment and the control group during the most recent years suggest as well that there is no understatement or "catch-up" elements in measuring the effects of the program on post-secondary education. Data published by the Israeli Central Bureau of Statistics' show similar evidence. For example, in 2016, among bachelor degree students (the relevant degree level given our population of study) about 13% of all students were ages 30 and over (Central Bureau of Statistics, 2020). Therefore, a possibility of effects beyond 2013 is less likely.

3.2 Definitions of Outcomes in Adulthood

In this subsection, I describe the outcomes in adulthood for students in the sample. To account for age differences of the different cohorts included in the sample, the post-secondary schooling outcomes are also adjusted for years since graduating high school.

Labour Market Outcome. *Earnings*: Individual earnings data comes from the Israel Tax Authority (ITA). Only individuals with non-zero self-employment earnings are required to file tax returns in Israel, but the ITA has information on annual gross earnings from salaried and non-salaried employment, and they transfer this information, including the number of months of work in a given year, annually to the NII.

The NII produces an annual series of total annual earnings from salaries and self-employment and I used this variable for 2000-2014. Following NII practice, individuals with a positive (non-zero) number of months of work and zero or missing value for earnings are assigned zero earnings. 14.1% of individuals have zero earnings at age 30-32 in our basic sample of 13,142. To account for earnings

[12] Boys serve for three years and girls for two (longer if they take a commission). Ultra-orthodox Jews are exempt from military service as long as they are enrolled in seminary (Yeshiva); orthodox Jewish girls are exempt upon request; Arabs are exempt, though some volunteer.

data-outliers I dropped from the sample all observations that are six or more standard deviations away from the mean. Very few observations are dropped from the sample in each of the years and the results are not qualitatively affected by this sample selection procedure. To account for age differences of the different cohorts in the sample, the employment and earnings outcomes are adjusted for years since graduating high school.

The same earnings data is also available for the parents of the students in our sample, for the years 2000-2002 and 2008-2012. I compute the average earnings of each parent and of the household for 2000-2002 and use it as an additional control in a robustness check of the evidence presented in this paper. These data were not available for the analysis of the effect of the program on short-term outcomes. *Employment*: An indicator with value 1 for individuals with non-zero number months of work in a given year, 0 otherwise.

Education. Here as well I measure the outcomes by adjusting for years since graduating high school. *University schooling*: is an indicator for being enrolled for at least one year in university schooling and years of university schooling is the number of years of attendance during the period 2000-2013. *Academic college schooling:* is an indicator for being enrolled for at least one year in any academic college and years of college schooling is the number of years of attendance.

Personal Status Outcomes: The data on marital status and having children is available only for 2011. Therefore, for these outcomes, we can adjust for years since graduating high school based on information about date of marriage and children's birth dates.

Marriage: is an indicator for being married. *Children*: is an indicator for having at least one child. *Number of Children*: is the number of children.

The NII linked these data to students' background data that I used in Lavy (2010) to study the effect of the choice program on high school academic outcomes.[13] This information comes from

[13] As high school outcomes I used an indicator of dropping out before completing twelfth grade, an indicator for matriculation (*Bagrut*) eligibility, credit-weighted average score on the matriculation exams, number of matriculation credits, number of matriculation credits in science subjects and number of matriculation subjects at honors level. *Bagrut* eligibility is a prerequisite for admission to higher education in Israel and the average

administrative records of the Ministry of Education on the universe of Israeli primary schools during the 1992–1994 school years. In addition to an individual identifier, and a school and class identifier, it also included the following family-background variables: parental schooling, number of siblings, country of birth, date of immigration if born outside of Israel, ethnicity and a variety of high school and *Bagrut* high school achievement measures. This file also included a treatment indicator, school ID and cohort of study. I had restricted access to this data in the NII research lab at the NII headquarters in Jerusalem.

4. Identification and Estimation

In previous work (Lavy 2010) I used difference in differences (DID) and geographical discontinuity (GD) in program placement as two alternative methodologies to estimate the effect of the school choice program on short term outcomes (dropout rate) and on medium term outcomes (success at the end of high school, six years after the school choice decision, in high stakes exams). Using a DID methodology, I relied on three alternative comparison groups which all yielded almost identical evidence regarding the impact of the choice program.[14] I therefore use this same identification method to estimate the effect of school choice on long term adulthood outcomes, while combining all three comparison groups into one in order to increase efficiency in the estimation. However, results based on using each of these comparison groups separately are in line with the evidence obtained from pooling all three control groups into one large sample. I discuss these results later in the paper and present them in an online appendix table.

The GD approach yielded evidence about the effect of the choice program on high school outcomes that is consistent with the evidence based on the DID estimation (Lavy, 2010). I therefore

score on the matriculation exams, number of matriculation credits in science subjects and number of matriculation subjects at honors level are used to screen and select students for prestigious universities and sought-after academic programs such as medicine, engineering, and computer science.

[14] In Table A1 in the online appendix I present the mean demographic characteristics of the students in the treatment group and in each of the three alternative control groups used in the DID estimation. This table is a replication of Table 1 in Lavy (2010).

use the GD identification method in this paper as well. I summarize briefly below the comparison group used here in the DID estimation and the comparison group used in the GD estimation. More detail about each of them is provided in Lavy (2010).

The comparison group in the DID estimation includes three different control groups. The rational of the first is the gradual implementation of the program and therefore it includes school districts 6-8 in Tel-Aviv that were enrolled immediately following the two-year experiment in district 9.[15] All three districts are part of South Tel-Aviv, geographically adjacent to or near district 9 (see Map 1 in online appendix), and their population is much more similar to that of district 9 than to that of the other school districts in Central and Northern Tel-Aviv (Lavy 2010).

The second comparison group includes two adjacent cities east of Tel-Aviv, which are part of the Dan metropolitan area.[16] District 9 includes the city's south-eastern neighbourhoods and is tangential to two of the neighbouring cities: Givataim and Ramat-Gan (referred to as GR, see Map 1 in online appendix). GR have independent and separate education systems and therefore were not part of Tel-Aviv's school choice reform.[17] GR students are very different in mean characteristics from district 9 students (Lavy 2010). However, these differences are very stable as they are similar in 1992 and 1993 as well. The solution, therefore, to the pre-program imbalances is to use data on pre and post program cohorts (panel data) in a difference-in-differences framework that removes time invariant heterogeneity across treated and control groups.

Holon is another city in the Dan Metropolitan area which is adjacent to Tel-Aviv (south) and it is very close to district 9. It has three additional attractive features as potential comparison group; First, its high school enrolment system before the inception of the TASCP was based on zoning and it

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[15] Note that since all the schools in districts 1-5 were included in the choice sets of students in district 9, they cannot be included in the comparison group regardless of the date at which they implemented the choice program.

[16] The Dan metropolitan area includes ten cities, including Tel-Aviv.

[17] The Givataim and Ramat-Gan high school enrollment system before the inception of the TASCP was based on zoning and it has not changed since, nor have these cities undergone any other major educational reform since 1994.

has not changed since; Second, it changed its secondary school structure to a six-grades structure in 1995' Third, it is more similar to district 9 in its characteristics than the GR group. I therefore use it as a third control group.[18]

Therefore, the main identification approach that I apply in this paper is based on a contrast between district 9 and a comparison group that includes these three control groups together, before and after the program was implemented. I use data on pre- and post-program cohorts (panel data) in a difference-in-differences framework that removes any remaining time invariant heterogeneity across treated and control groups. Since this DID estimation compares two consecutive cohorts, and since the program was implemented immediately after it was announced, it is reasonable to assume that the remaining differences were constant within this narrow time range.

A concern with this DID approach, however, is that the cohort immediately prior to the treated cohort that I use as a control group might be affected through spillover effects at the school level. As students of this cohort will be attending the same schools as the treated students, peer effects or competitive effects on school productivity might impact the untreated students as well. As a robustness check, I re-estimated the econometric models based on using as control group the cohort that started middle school two years prior to the implementation of the program. In this case, it is expected that spillover effects will be smaller. I used a similar approach in Lavy (2010)) and the results indicated no spillover effect. I replicated this analysis with the longer-term outcomes (post-secondary schooling and labour market outcomes) and obtained similar evidence of no spillover effects.

[18] The fact that three alternative sets of DID estimates, one that is based on a comparison group that has much better characteristics and outcomes (GR) than the treated group, a second that is based on a comparison group that has marginally worse characteristics and outcomes (districts 6-8), and a third that has relatively similar characteristics (Holon), yield exactly the same results is reassuring, given the possibility that the DID estimates are biased because of regression to the mean or due to differential time trends in unobserved heterogeneity between treatment and control.

As noted above I also use a geographical discontinuity in program placement as an alternative identification strategy. Following Black (1999), I limit the sample to observations within a narrow band around the municipal border between district 9 and GR (see Map 2 in online appendix). As shown in Lavy (2010), the physical and other characteristics of the communities within this strip (for example, type and average size of homes) are identical, as are zoning laws and municipal (type of property) taxes, which are determined by the central government. Presumably there might still be some differences, such as the political affiliation of the mayor, for example. The concern remains then that such remaining differences may confound the effect of the program. As mentioned above, the use of data on pre- and post-program cohorts in a DID framework will remove such time invariant heterogeneity across treatment and control groups. I define this sample based on drawing a symmetric band around the municipal border, 250 or 500 meters on each side. Contrary to the imbalances between district 9 and GR, this GD sample yields better balanced treatment and control groups. In the analysis of the long-term outcomes I will use the +/-500 meters' band, again in order to have a larger sample for estimation, but it should be noted that the +/-250 meters' band yields similar results.

4.1 Estimation

I first present a controlled comparison of treated and untreated students using samples of pre and post treatment cohorts based on the following regression:

(1) $$Y_{ijt} = X_{ijt}\beta + Z_j\, d + U_{ijt}$$

where Y_{ijt} is the ith student's outcome in school j and year t; X_{ijt} is a vector of the same student's characteristics; Z_j is the treatment indicator (which equals 1 for district 9 students) and d is the treatment effect. As noted above, I will first estimate the equation using as a comparison group a sample that includes Tel-Aviv district 6-8 students, and GR and Holon students, and then I will also exploit the GD sample (using the +/-500 meters' sample).

In addition, I use the before-and-after cross section data as stacked panel data that permits regression analysis with controls for primary-school fixed effects. Therefore, I will estimate stacked models using three years of cross-section data combined. The treatment indicator Z_{jt} is now defined as the interaction between a dummy for the year 1994 and the district 9 indicator, as follows:

(2) $Y_{ijt} = \mu_j + \pi_t + X_{ijt}\beta + Z_{jt}d + \varepsilon_{ijt}$

where μ_j is the primary school fixed effect and π_t is a year (i.e., 1992, 1993 and 1994) fixed effect. Apart from providing a check on the precision of the 1992-1993 vs. 1994 contrast in treatment effects, the introduction of school (fixed) effects also provides an alternative approach to the clustering problem. The validity of this control, however, depends on the validity of an additive conditional mean function as a specification for potential outcomes in the absence of treatment.

4.2 Descriptive Statistics

Table 1 presents detailed descriptive statistics of the outcome variables for 11 years since high school graduation for the 1992-1994 cohorts, by treatment and control group and by pre- and post-reform cohorts. Post-secondary enrolment statistics are presented in panel A. The enrolment rate in university schooling in the treatment group for the pre-treatment cohorts (1992 and 1993) is 17.2% and for the control group it is 23.1%. The difference is -0.058 (se=0.012), and is statistically different from zero. The respective enrolment rates in academic colleges are 20.0% and 26.3%; the difference is -0.063 (se=0.012) and is statistically different from zero.[19][20] The respective means and estimated differences for the post-treatment cohort are presented in column 4-6. Note that the mean difference in university enrolment did not change much while the mean difference in the academic college enrolment rate declined to -0.028 from -0.063. The difference between these two differences (which is a simple uncontrolled difference in difference estimate), 0.035, will be shown to be very close to the controlled difference in difference estimate that I will present in the next section.

Summary statistics on completed years of schooling are presented in panel B. The average number of years of university completed eleven years after high school graduation in the pre-reform cohorts of the treatment group is 0.683 and in the control group it is 0.974. The difference is -0.290 (se=0.056). The respective means for years of college education are 0.555 and 0.822 and the

[19] Note that very few students ever enroll in more than one type of post-secondary educational institution.

[20] The respective means for the whole cohort (82,500 students) are 24.0 in universities and 24.0% in academic colleges.

difference is -0.267 (se=0.043). This evidence suggests, as expected, that the treatment-control imbalance at baseline in both types of post-secondary education is in favour of the control group.

A similar treatment-control comparison based on the post treatment cohort (1994) reveals that the gap in university attendance remained unchanged while the gap in academic colleges' attendance was almost completely eliminated, and the remaining difference became statistically negligible. The implied simple difference in differences estimate for college is 0.161 years and for university it is -0.040. These dynamic changes suggest that the program led to improvement in college-going rates and years of college completed without an effect on university outcomes. These estimates are similar to what I will present in the next section based on controlled difference in differences estimation.

Summary statistics for the labour market outcomes are presented in panel C of Table 1. Eleven years after high school graduation, 86.8 and 84.1% of the individuals in the pre-treatment cohorts in the treatment and control group, respectively, were employed and the difference between the two was 2.7% (se=0.010). The respective rates in the post treatment period are 84.1, 84.4 and -0.003 (se=0.014). Average annual earnings at baseline[21] was lower in the treated group by about NIS 3,000 ($750), a gap consistent with post-secondary education treatment-control differences.

The summary indicators in panel D suggest that just over half of the treated sample is married by 2011; in the pre-treatment control group this rate is 2 percentage points lower. Age of marriage in the treated group is 25.6, about a third of a year younger than in the control group and a similar gap is observed in age of having the first child.

In panel E I report statistics on parental income in 2000-2002. This information, which was not available when studying the short and medium-term effects of the school choice program, reveals gaps in favour of the control group. This is of course consistent with the other imbalances seen in Table 1: father's income is higher in the pre-program control cohorts by NIS 27,175 and in the post-program by NIS 24,093 and these two differences are not statistically different from each other. The respective differences between average mother's earnings are NIS -10,904 and NIS -7,974. These

[21] The mean earnings in this sample, NIS 70,639, is identical to mean earnings in the whole cohort, NIS 70,300.

stable differences will be shown not to affect the treatment effect estimates of school choices when added as controls in the difference-in-differences regressions.

4.3 Evidence on Pre-Trends

To check the assumption of equality of pre-time trend in the treatment and control group one needs data on outcomes for several years before the year treatment started. However, this data is not available because the Ministry of Education in Israel prepared digitized information on high school outcomes at the school and student level only since 1992. Therefore, I cannot estimate standard pre-program trends for the treatment and control groups. Nonetheless, I present here evidence on equality of treatment-control pre-trends based on an alternative approach, using other data that is available to me. While this data does not include variables that would make an exact test of a pre-trend possible, the following analysis provide evidence that still alleviates to some extent the concerns about pre-trends. The data I use here is on the background characteristics and high-school outcomes of each cohort after 1986. I use this data, to follow the same approach used in a recent publication (Lavy, February 2020) and examine whether there is systematic improvement in year-fixed effects for schools attended by district 9 students before and after the 1994 reform. Using this data, I compare students at schools attended by district 9 students, to all of GR and Holon students.

Online Appendix Table A2, presents the results of this analysis. Each column includes the results for a single outcome or characteristics: columns 1 through 5 include the five high school outcomes, and columns 6,7, and 8 include the three background characteristics. All eight variables are standardized to have mean zero and a unit standard deviation.

Panel A presents the estimated fixed effect of each cohort (Yj), where cohort is the year of attending 7th grade. The dummy variable of the 1987 cohort is dropped, serving as a benchmark. Panel B includes a single fixed effect (D) for the six Tel-Aviv schools which had a high share of district 9 students in the 92-94 cohorts. Since data on the school discrete of each school is available only from 1992 on, D is equal to one for students from these six schools regardless of their school district.[22]

Finally, Panel C includes an interaction between Yj and D. Each of these seven dummy variable indicators (one for each cohort) measures by how much the mean of characteristics and outcomes of students in schools with district 9 students differ from the respective means of other schools. Overall, there appears to be some variation between the cohorts in mean characteristics and outcomes but without any obvious time trend. Secondly, there are no significant differences in the cohort dummies between schools, with and without enrolment of students from district 9. These results suggest that the assumption of equality of pre-time trend in the treatment and control group is very plausible.

5. Empirical Evidence

The school choice program had positive and significant short and medium-term effects on students' high school completion rate and on academic achievements during high school. Across identification methods and comparison groups, the results consistently suggest school choice significantly reduced the drop-out rate by 8.4% (35% decline) and increased the matriculation rate by 6.1 percentage points (25% improvement). These results are presented in Table A3 in the online appendix. These very large effects were accompanied by an improvement in the quality of schooling. The average number of *Bagrut* credits increased by two units relative to the pre-program mean of 12 units and the average score in all of the *Bagrut* exams was up by 6.6 points, about 10% improvement. Other dimensions of quality improvement are the increase in number of *Bagrut* credits in science subjects and the increase in *Bagrut* honour level studies (up by a quarter relative to a mean of one such subject). These estimates are presented in Table A3.

[22] To check how sensitive are the results to this data limitation, I conducted a similar exercise while including in the sample only the students that enrolled in district's 9 high schools. In these schools, enrollment was almost exclusively by students who reside in district 9. The results based on this sample are qualitatively the same.

The estimates based on the GD sample are presented in Table A4. They show similar positive effects of the school choice program on high school outcomes. However, conclusions about whether free school choice improves real human capital accumulation and well-being can only be based on longer term effects, in particular outcomes such as post-secondary enrolment and completed years of tertiary education, employment, earnings, welfare dependency and other social outcomes to which we turn next.

5.1 Effect on Post-Secondary Schooling Attainment

I first present graphs illustrating the effect of the school choice program on post-secondary education. I focus on the two sub-sectors of academic post-secondary education in Israel. The first includes the seven research universities in Israel that confer BA, MA and PhD degrees. These universities require a matriculation diploma for admission, including an intermediate or advanced matriculation unit in English[23] and at least one matriculation subject at an advanced level. About 35% of all students are enrolled in one of the seven universities. The second sub-sector is made up of more than 50 academic colleges that mostly confer a BA degree and predominantly offer social sciences, business and law degrees.

Figure 1 presents the dynamic of the treatment effect on academic college enrolment (vertical axis) starting from the first year after high school graduation until 12 years later (horizontal axis). We note again that during the first few years after high school graduation almost all boys (for three years) and most girls (for two years) are still in military service and therefore the treatment effect is not informative because it is based on a limited sample of those not drafted to service. The treatment effect is positive and statistically significant from year four after graduating high school, reaching a high of 4.8% and declining slightly towards the end of the period to 4%. The respective mean enrolment rate for the treated group increases gradually from year one and is highest at 21% twelve years later. The effect size here is therefore a 20% increase. The effect on completed years of

[23] To qualify for a matriculation-diploma a basic study program in English is sufficient, but university admission requires a higher level.

academic college (presented in Figure 1A) increases continuously until 12 years after graduating high school, reaching a peak at 0.19 years and levelling thereafter. The mean of completed years of academic college in the treatment group is 0.6 and therefore the effect size is a 30% increase.

Figures 2 and 2A present the estimated effects on university enrolment and attainment and the pattern revealed in these figures is very different, as the effect is practically zero. The treated group mean of university enrolment rate is 0.18 and the mean years of university is 0.71 years, and both of these outcomes are not changed due to the program.

In Figures 3-3A I replicated this graphical analysis for any type of post-secondary education (including teachers' colleges and non-academic education) and the results are very similar to those presented in Figures 1-1A. Overall post-secondary enrolment increased by almost 5 percentage points and total education increased by a fifth of a year.[24]

Table 2 presents detailed estimates from regressions of the effect of free school choice on post-secondary education attainment when outcomes are measured twelve years after high school graduation. The first-row presents DID estimated effect on enrolment in any type of post-secondary education (column 2) and on the respective completed years of education (column 4). Standard errors appear below each estimate in brackets and are clustered by secondary school. School choice enrolment in any post-secondary education increased by 4.6 percentage points relative to a pre-program mean of 42.5% in treated schools and 52.9% in the control group. The effect on completed years of education (column 4) is 0.187 (SE=0.089). Relative to the pre-choice treatment group mean (1.648) this is a 15% gain.

It is interesting and important to know what types of post-secondary education are affected. Since the treated population is from a low socio-economic background with relatively low enrolment at the higher quality end of academic institutions, we expect the effect to be low on university education and higher on colleges and non-academic post-secondary institutions. In the second row I

[24] It is important to note here that the expansion in enrollment in academic colleges due to the school choice program could not have been at the expense of other students because the choice program and the number of students affected was very small relative to the overall enrollment in academic colleges in the whole country.

present the estimated effect on university education and in the third row the effect on education in academic colleges. The effect on university enrolment is practically zero (0.006, SE=0.014) and so is the effect on university years of education. The effect on academic college enrolment is however up by 4 percentage points, significantly different from zero (t=2.2), and completed years of this type of education increased by almost a fifth of a year (0.171, SE=0.050). The gain in academic college education is almost equal to the overall gain in any type of post-secondary education, indicating that the effect on any other type of education is very small or zero.[25] [26]

The evidence presented above clearly demonstrates that the gain in post-secondary education is concentrated in the lower end of academic education in Israel. The academic colleges are less prestigious than universities and their admission requirements are less demanding in terms of *Bagrut* results. This pattern is perhaps expected because the treated population is mostly from a disadvantaged segment of the Israeli population and their enrolment and years of study in university is much lower than the overall mean in the country. In addition, we can safely claim that the affected students are at the margin of being admitted to post-secondary education, which can also explain why the treatment effect is concentrated at the lower end of the quality distribution of academic education in Israel.

The long-term data that I use in this paper includes information on parental income during the years of the experiment. It allows me to assess how sensitive the post-secondary treatment estimates are to adding controls for family earnings. The estimates presented in Table A6 in the online appendix

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[25] Indeed, enrollment in teachers' colleges also increased, by 2.7% and significantly different from zero, but the respective increase in years of this type of education is positive but small and imprecisely measured. Enrollment and years of schooling in vocational education (two-year colleges that confer practical engineering degrees) declined by 1.5 percentage points and by 0.025 years, very small and imprecisely measured changes. These results are not presented in the paper and are available from the author.

[26] In online appendix Table A5, I present results of placebo effects based on a contrast between the two untreated cohorts of 1992 and 1993. Overall, these controlled experiment estimates are not significantly different from zero, similar to respective placebo regression estimates with high school outcomes as the dependent variables (Lavy 2010).

show that adding to the difference-in-differences regression a control for family income (average in 2000-2002) does not change at all the point estimates relative to those presented in Table 2. For example, the estimated effect on college enrolment in Table A6 is 0.041 and on college years of education it is 0.170, almost identical to the respective estimates in Table 2. This is a remarkable result given that the treatment and control samples are not balanced in family income, but they are equally imbalanced in this dimension, as in others, for the pre- and post-treatment cohorts.

To assess how sensitive are the results to pooling the three comparison groups as one control group, the online appendix Table A7 presents estimates based on using as control each of the three comparison groups separately. The results are similar to those estimated using the combined control groups. For example, the estimated effect on college enrolment in Table 2 is 4 percentage points. This estimate is 3.1 percentage points when using only GR as a control group, 4.1 percentage points when using Holon as a control group, and 4.8 percentage points when using Tel-Aviv's districts 6-8. Note that the average of these three estimates in exactly 4 percentage points, identical to the treatment effect when using the three control groups jointly. Naturally, when using each of the control group separately the sample is smaller and therefore the estimates are less precise than when pooling all three control groups together. To further illustrate that the estimates are similar, I computed the ratio of the treatment effect estimated from each control group separately, to the treatment effect estimated while using all control groups. These ratios are presented in columns 4, 6, and 8 of Table A7. These ratios are close to one for almost all outcomes, especially for outcomes for which the treatment estimates are statistically significant when using the combined control groups.

Evidence Based on the Geographic Discontinuity Sample: To further check the robustness of the evidence presented above, I use the GD design described in the previous section. The GD sample includes observations within a relatively narrow band around the municipal border between district 9 and GR and the descriptive statistics of the control and treatment group in this sample are presented in Table 3. It is important to note that in this sample, the treated group is from a much higher socio-economic status and it resembles more closely the control group. For example, the mean of fathers' and mothers' years of education in the GD treated sample in the 1993 cohort is 11.43 and 11.58, respectively, while in the rest of district 9 these two respective means are less than ten. A similar pattern is observed for the post-secondary educational outcomes presented in Table 3, where in the GD treated sample the mean enrolment in university and academic college education in the post treatment cohort (1994) is 22.6 and 24.7%, respectively, while the respective means in the rest of district 9 are 17.9 and 20.9%. The much higher socio-economic status of treated students in the GD sample in comparison to the rest of district 9 suggests that we might expect a higher effect of school choice on university schooling than what we estimated based on district 9's full sample.

Indeed, this is the case, as shown in Table 4 where I report estimates derived from the GD sample. The effect on university enrolment is 0.051 and the effect on academic college enrolment is -0.011. The effect on years of university education is 0.260 and the effect on academic college years of education is 0.017. This pattern is strikingly different from the evidence that is based on all of district 9. However, these estimates are less precisely measured than the respective estimates in Table 2, most likely because of the much smaller sample size. As we will see below, the gain in university education will be rewarded with a higher increase in annual earnings relative to the gain in annual earnings experienced by those who improved only academic college education.

5.2 Effect on Employment and Earnings

We start here as well with a graphical presentation of the impact of the school choice program on employment and earnings. Again, we measure for each individual these two outcomes based on number of years since graduating high school. The employment and earnings data are available until year 2014, so thirteen years is the longest period after graduating high school for which we examine the effect of the program. Figure 4 presents the yearly estimates on employment. As noted earlier, the estimates for the first three years are not meaningful because most of the students in our sample were still in military service. In the fourth year after high school graduation, about 85% of the individuals in the sample were employed (according to our definition of employment, which is being employed at least for one month during the year and had positive earnings). For almost all years the estimates are measured imprecisely: for 5-7 years after high school graduation the estimates are positive and thereafter they are negative, but in most years they are not statistically different from zero, especially in 12 and 13 years after high school graduation. Similar evidence is obtained for the outcome that measures the number of months per year of being gainfully employed. These imprecise and inconclusive employment dynamics imply that they do not play an important role in determining the change in earnings due to the school choice program.

We next turn to the time series of estimated effects on annual earnings, which are presented in Figure 5. The treatment effect estimates on earnings are positive throughout the period after high school graduation. They increase over time monotonically with the exception of a spike in treatment effect nine years after high school graduation. Similarly, average annual earnings in the sample also increase monotonically until the end of the period, from NIS 40,000 (about $10,000) five years after high school graduation to just over NIS 80,000 13 years after high school graduation. The treatment effect on earnings in the last two periods of analysis is just below NIS 5,500 a year and it is significantly different from zero at the 10% level of significance in both periods. It is interesting to note that the effect on earnings is not negative even in the period when post-secondary enrolment rate is higher among treated students.

The pattern discussed above is different from that reported in Lavy (2016), where teacher pay for performance increased university education while lowering employment and earnings during the three to five years when students are studying. This contrasting pattern is not surprising for several reasons. First, it is typical that students in academic colleges in Israel work part time or full time, while university studies have a more demanding academic schedule and requirements that correspond to full time attendance, making it more difficult to combine work with study. Secondly, the treated students in academic colleges are usually from a lower socio-economic background in comparison to students in universities, and therefore they can rely less on parental support. Thirdly, scholarships based on academic merit and on low family income are available for university students but not for students in academic colleges.

Table 5 presents evidence about the effect of the school choice program on employment, number of moths worked in a year, and monthly earnings, for 11 to 13 years after high school graduation (columns 2, 4, 6) and based on stacking the three periods in one sample (columns 8). I also present treatment effect on percentile rank of earnings and on log earnings (practically dropping from the sample observations with zero earnings). The average employment rate in the treatment group in these three periods is 87.0, 85.2 and 85.4, respectively. The respective treatment effect on employment is negative, though it is small and in the last two periods it is practically almost zero. The estimated effect on months worked per year has the same pattern. It is important to note that the negative though small employment effect does not reflect a higher rate of individuals still studying among treated students; the proportion of students in the 1994 cohort who are not yet employed and are still studying is the same in both groups: in the treatment group in 2012 it is 1.4% and in the control group it is 1.3%. The respective means in the 1992-93 cohorts are 0.7% and 0.8%.

The average annual earnings for the 1992-1993 cohorts in treated schools 11 years after high school graduation is NIS 74,709 ($17,620), 12 years after high school graduation it is NIS 78,313 ($19,216) and 13 years after high school graduation it is NIS 81,230 ($21,377). The estimated effect of the school choice program on annual earnings is NIS 3,368 ($935) 11 years after high school graduation, NIS 5,544 12 years after and NIS 5,662 13 years after. The last two estimates are significantly different from zero at the 5% level of significance. I also estimated an earnings effect using a combined 11 to 13 years after high school graduation earnings, stacking the data together for these three periods. This estimate is NIS 4,763 (column 8 of Table 5), close to the average of the estimates for 11, 12, and 13 years after high school graduation.

The estimated effect on percentile rank and on log earnings are positive and they are particularly precise and statistically significant on the latter.

The lower part of online appendix Table A7 present estimates of effect on labour market outcomes based on using each of the three comparison groups separately as control. The results are not different from those presented in Table 5. A notable exception is the labour market outcomes estimates obtained when using only Tel-Aviv's districts 6-8 as a lone control group. In the combined control group sample these estimates are large and positive for annual earnings, and small and insignificant on employment (Table 5). When using only Tel-Aviv's districts 6-8 as a control group, the effect on earnings becomes insignificant, and the effect on labour supply becomes more negative. It could be that this small and insignificant effect results from noisier earnings data in districts 6-8 which are populated by many immigrants and illegal foreign workers and also large number of minority Arab population. These groups have a tradition of less precise earnings records at the tax authorities. Nevertheless, it is important to note that the estimates obtained when GR or Holon are used as a control group are very similar to each other.

In Table A8 in the online appendix I present the treatment estimates on earnings and employment when controls are added for parental or family earnings. The three columns correspond to estimates for 11, 12 and 13 years after high school graduation. These estimates are very similar to those presented in Table 5, implying that adding a control for parental earnings does not affect at all these treatment estimates.

In Table 6, I present the estimated effect on labour market outcomes based on the GD sample. The effect on earnings has the same pattern as in Table 5 but the estimates are larger. Focusing on the stacked data estimates in column 8, the annual earnings' gain during 11 to 13 years since high school graduation is NIS 7,613: a 9% increase relative to total annual earnings in this period. This large effect is partly due to the higher increase in years of post-secondary education and to the higher rate of return for university education in Israel relative to the rate of return to academic college education.

Caplan et al (2009) report that in many fields of study, academic college graduates in their first jobs earn on average 20 to 30% less than university graduates. However, the effect on employment is negative and larger than what is observed in the full sample. This is a puzzling pattern that is resolved when I estimated all treatment effects by gender which show that all of the negative effect on employment is on women, and that most of the positive effect on earnings is due to men. Consistent with these results is the positive effect on women's marriage rate and fertility without a corresponding effect on men. I discuss these results in more detail in later sections where I report results by gender, and the marriage and fertility effect of school choice.

It is useful to compare the evidence presented above to evidence presented in Wondratschek, Edmark and Frolich (2013) which is the only other school choice study that examined long term effect on several outcomes, including college education and employment. It evaluates the effects of a large-scale school choice reform in Sweden implemented in 1992 that significantly increased the amount of choice in compulsory education, among the already existing public schools and also among private schools that were mostly opened later. The results of this study reveal very small effects on the short-term outcomes (test scores and grades) and zero effects on longer-term outcomes (employment, higher education, criminal activity and health). As an explanation for these findings the authors suggest that previously existing Tiebout choice (i.e. moving homes) may already have provided sufficient choice options for those families who wanted to choose. Another explanation could be that the long term outcomes are measured at a relatively young age: crime and health at age 22 and university degree and employment at age 25. The long term effects that I present in this paper are measured at a much older age. Actually, when estimated at age 22-25 they are also very small and imprecise.

Comparing the Effect on Earnings to Related Evidence

This is the first study to provide evidence of the effect of school choice on students' earnings at adulthood. However, it is still useful to compare our results to the impact of other childhood and education interventions on earnings at adulthood. Andersson et al. (2013) estimated that living during teenage hood in public or voucher housing increased females' earnings by 18%-21%. Each additional year of public or voucher-supported housing increases earnings by 7% for females. For males each year of public housing participation as a teenager increases adult earnings by 5% with no corresponding effect of voucher housing. Chetty et al. (2011) have shown that having a kindergarten teacher with more than ten years of experience increased students' average annual earnings at ages 25 to 27 by 6.9% ($1,093) between 2005 and 2007. Similarly, an improvement in class quality increased average annual income earned between ages 25 and 27. Johnson et al. (forthcoming QJE) show that for children from low-income families, increasing per-pupil spending by 10% in all 12 school-age years increased adult hourly wages by 13%.

Schweinhart et al. analyse the long-term effect of the High/Scope Perry Preschool experiment and find that students in treatment had significantly higher median annual earnings than the no-program group: 20% higher at age 27 and by 36% higher at age 40. Finally, Chetty, Hendren and Katz (2016) find that moving to a lower-poverty neighbourhood (MTO) significantly improves college attendance rates – by 2.5% – and earnings by 31%, for children who were young (below age 13) when their families moved. Clearly our estimated effects on earnings are not unusually high relative to estimates surveyed above. For example, the teachers' pay experiment raised college enrolment by 5%, twice that of the MTO effect, and increased earnings 10-12 years after high school graduation by 7%-9%, a quarter of the MTO effect.

5.3 Does the Change in School Structure Confound the Estimated Effect of School Choice?

As noted in Section 2, parallel to the school choice reform in District 9, the Tel-Aviv school authority changed the structure of four high schools in the city, from three grades schools to six grades schools, by merging into high schools all middle schools. All of the city's other post-primary schools were already structured this way. It is however important to check whether the results I reported above are potentially confounded by the structural change in these four schools.[27] The evidence presented in this section reassure that this is very unlikely.

First, the education system in Holon, one of the three control groups that I use in the paper, has also moved to a six grade structure around the same time that Tel-Aviv did. Holon made the change in 1995 and Tel Aviv in 1994.[28] This implies that Holon students were exposed to the new six grades school structure from 8th grade while the treated students in Tel Aviv were exposed to the same change from 7th grade. Thus Holon students had five years of exposure to the new school

[27] The school structure reform that incorporated the three middle school grades in high schools was part of a national program that was implemented gradually in all school districts in the country. This reform kept key features of middle schools unchanged which likely reduced the scope for potential confounding effects. For example, middle schools remained an independent entity, having an autonomous principle, unchanged educational curriculum, and a separate budget. Its teachers continued to get their education in teachers' colleges (while high school teachers had to obtain university degrees), they remained organized in the same separate union, working under a separate collective bargaining agreement with wages lower than those of high school teachers.

[28] Evidence regarding the year of reform in Holon is available from websites of schools that existed at the time. For example, in the front page of the Sharet secondary school link: https://www.holon.muni.il/Residents/Education/AlYesody/Lists/List/CustomDispForm.aspx?ID=32. Similarly, for Katzir secondary school: http://www.katzir.hs.holonedu.org.il/BRPortal/br/P102.jsp?ar. All high schools in the city made the change at the year.

structure, while district 9 students had six years of such exposure. This minor difference is unlikely to matter for the medium term effect at end of high school and for the longer term effect presented in this paper. Indeed, as shown above, the school choice effects on medium and long term outcomes estimated based on using Holon as a control group are identical the estimates when using GR as a control group.

Second, as noted in Section 2, school districts 6-8 in Tel-Aviv (one of three control groups I use for identification), implemented the change in school structure at the same time that district 9 did. As shown above, the estimated effect on high school and post-secondary schooling based on using districts 6-8 as a control group are similar to those obtained when using GR (or Holon) as a control group. The only difference is the estimated effect on earnings, which I discuss in section 5.2.

5.4 Effect of School Choice by Gender

Previous research on the effectiveness of schooling interventions has shown differences in the responsiveness of boys relative to girls (for example, Angrist and Lavy 2009). To test for this possibility, I stratify the sample based on the gender of students and present in Table 7 the effect of school choice on high school outcomes, for boys and girls separately.[29] Free school choice improved all five outcomes for boys and girls except the dropout rate of girls, which declined by only 2.1 percentage points, with a t statistic of just 1.5. However, it is noticeable that girls have higher means in all six high school outcomes and girls' dropout rate is much lower than that of boys, 10.3% versus 26.6% among boys in the sample. However, all of the point estimates suggest that the effects are larger among boys except for the effect on the matriculation rate. The gender differences are statistically significant for the following outcomes: dropout rate, average score, number of credits, number of subjects studied at honour classes.

In Table 8 I report the estimated effects of school choice on post-secondary schooling outcomes by gender. Boys gained a 9.3 percentage increase in enrolment and a third of a year of post-secondary schooling, most of it in academic colleges but some in university. The girls' gain is limited

[29] These results are not reported in Lavy (2010).

to academic colleges, but the estimated effect is much smaller than that for boys. Table 9 shows that these gender differences are also reflected in the labor market. Focusing on the estimates based on the stacked regressions that pool data for the period 11-13 years after high school graduation, the effect on boys' earnings is NIS 6,807 (se=3,615), an 8% increase, while the earnings effect for girls is NIS 1,785 (se=2,136), which is not statistically different from zero. However, the girls' labor market experience includes also a 3.1% decline in employment rate and a two-thirds of a month decline in months of work in a given year. Both of these effects are precisely measured. The decline in months of work among treated girls accounts for a negative NIS 4,931 (= [(68,347/9.288) x 0.671)]) effect, which wipes out the expected positive effect of the increase in college education on earnings. On the other hand, the effect on boys' employment is practically zero. What can explain the different pattern of the labour market effects by gender, in particular the negative effect on girls' employment? In section 5.4 below I present the effect of school choice on marriage and fertility outcomes, and use them to provide a partial answer to this puzzle.

5.5 Effect on Marriage and Children

In Table 10, I report results regarding the program's effect on marriage and fertility. The marriage and fertility data are available up to 2011. However, I can still compute these outcomes by years after high school graduation because the dates of marriage and of birth of children are available in the data. Fifty-six percent of the treated sample are married after ten years following high school graduation. The treatment effect on being married at this date is 2.8% but it is not precisely measured. The age of marriage effect is similar (0.125, se=0.151). The estimated effects on the indicator of having children and on the number of children are positive, but they are not measured precisely. However, the age of having the first child is delayed by half a year and this effect is precisely measured with SE=0.174.

In columns 4 and 6 I present the respective effects on boys and girls and here meaningful differences are revealed. School choice has a negative though imprecise effect on boys' marriage rate and it has a large positive effect on girls' marriage rate, which is 8.1% higher relative to a mean of 62.9%. Age of first marriage of girls is unchanged while it is delayed by a third of a year for boys.

The probability of having children by age 28 is increased for girls by 8.8%, relative to a mean of 51.8%, while the effect for boys is negative. The effect on number of children is positive for girls, up by 0.158 children, while for boys it is negative, -0.102 children. All the estimated effects on girls are statistically significant and so are some of the estimated effects on boys. The higher marriage and fertility rates among women in the treated sample could be related to the heterogeneity by gender in the treatment effect on labour market outcomes that we reported in the previous section. The lower treatment effect on employment and earnings of women is in contrast to the similar of even higher treatment effect among on women on post-secondary schooling outcomes relative to untreated women in the sample.

First point to note in this analysis is the evidence in Table 2, which show that the effect of the school choice program on various high-school outcomes is higher for girls by gender. Unlike the results documented in Deming et al. (2014), the initial gender gap in achievement is much smaller and hardly significant, with both genders displaying positive, significant treatment effects. In fact, the particularly important effect on eligibility for matriculation is even higher among girls. The lower effects for girls in these groups could be attributed to higher outcomes prior to the reform. The first major gap between effects for boys and girls appears in post-secondary schooling. In contrast to Deming et al. (2014), boys exhibit a positive (albeit imprecise) effect in university enrolment and years of schooling, girls exhibit no such rise. However, this gap does not appear for college outcomes– where both genders have almost identical treatment estimates. Therefore, at this stage the gap appears, if at all, only among those in the higher end of the education distribution. These effects speak of the potential for heterogeneous treatment effects.

The gender gap in the treatment effect on labour market outcomes is indeed striking. Women display negative labour supply effects, and insignificant effects on wages, whereas men have positive and significant effects on wages, and zero effects on labour supply. The divergence in annual earnings can be partially explained by the reduction in labour supply – a reduction of 0.671 months of work is worth 3,821 NIS given the mean income of woman. Without this ‘lost’ income, women would have enjoyed a 5,606 NIS treatment effect, 17% less than men, but much more than zero. The table below show a similar average reduction in wages among married women:

Labour Market Statistics, Means and Standard Deviations Among Women

	Labour supply (months)	Annual income (NIS)
Unmarried	9.039	76,586
	(4.431)	(65,984)
Married	8.891	72,561
	(4.410)	(63,022)
Average effect of marriage	-0.148	-4,025

The reduction in labour supply corresponds directly to an *increase* in marriage and fertility rates, of 8-9%. This is of similar magnitude to the reduction in labour supply, 7.22% in months worked, and 3.5% in employment. Therefore, it is likely that most of the statistically significant gender differences in the effect of school choice on labour market outcomes can be attributed to a rise in marriage and fertility rate among women, which itself can result from an improvement in educational attainment (specifically improved *bagrut* eligibility, a prime educational outcome in Israeli society). It is relevant to note in this regard that in Israel most women and men are at least two to three years older than American and European counterparts when making decisions about college schooling. This age difference is due to the compulsory military service in Israel, three years for men and two years for women, commencing at end of high school. Therefore, any gender-specific attenuation of treatment effects through marriage, at the same stage in the post-secondary schooling process, could be larger for Israelis.

6. Evidence on Mechanisms

In Lavy (2010) I provided evidence about the mechanisms of the effect of school choice on the short and medium-term academic outcomes. Some of it suggested that the program led to better match between students and schools. In this paragraph I present additional evidence about the role of this mechanism. First, after the free choice program, the majority of students in district 9 chose a different school than the one they otherwise would have been assigned to under the pre choice assignment rules. Second, after the introduction of free school choice there was a dramatic decline in students' mobility between schools at each potential junction between grades, from 7th to 12th grade. Third, a large proportion of district 9 students who had the longest travel distance from home to schools in districts 1-4, chose also an out of district school, which suggest willingness of students to 'pay' in terms of longer travel time in order to enrol in what they perceive to be a better school.

Also shown in Lavy (2010) is that competition among schools intensified following school choice and that it led to improved school quality. In this section I provide additional evidence that school quality was an important channel of the effect of school choice. Using pre-program cohorts' data, I measure two (related) aspects of school quality (similar to the approach used in Deming 2014). The first is the quality of peers, measured by peers' mean background characteristics. The second is school average academic achievements. Naturally, school choice might depend on these two aspects of school quality. Therefore, a starting point in this analysis is to examine whether high performing students opted out to better schools more than others. Figure 6 presents the school enrolment pattern of district 9 students before the school choice reform (1992-93) and after the reform (1994), for out-of-district schools and for in-district schools. School attendance of district 9 students in out-of-district schools hardly changed. However, there is a very significant shift of students from an existing (prior to the program) school in district 9 to a district 9 school that prior to the program had only 10^{th}-12^{th} grades and when the program started it expanded its grade coverage to include also grades 7^{th}– 9^{th}. Figure 7 shows that the proportion of district 9 students in each of the out-of-district schools also remained unchanged. The evidence in these two figures suggest that if sorting played a role in the program's effects, it was mostly through matching of students to schools that are more effective for them, rather than through an increase in enrolment of students in better schools.[30]

[30] A 1997 report (Levy, Levy-Keren and Liberman,1997) prepared for the Tel-Aviv municipality school authority, offers more evidence for the lack of significant systematic sorting in the first year of the program. This evidence was based on standardized tests in mathematics and in Hebrew that all Tel-Aviv students took in the year following the implementation of the school choice program. Regretfully, I was not able to gain access

These results and the evidence presented in Figures 6-7 suggest that an aggregate *improvement* in the learning environment is not the result of a large-scale exodus of students to better schools in North Tel-Aviv. This implies that there was no "cream skimming" as a result of the implementation of the school choice program. To check more formally the choice patterns of 1994 district 9 students, I first calculated the mean of school quality and of peer characteristics in the 1992-3 cohort. I then computed the weighted mean of these averages, first using as weights the proportion of district 9 students who attended each school during the 1992-3 cohorts, and secondly using as weights the proportion in each school of district 9 students in the 1994 cohort. I than standardize these variables to have a zero mean and a unit standard deviation (Z-score). The sample includes 14 schools which I use in a second step to measure the degree to which the students of the 1994 cohort improved their schooling environment through choice. I first stacked the 1992-93 and 1994 means of all variables and then used this sample to estimate regression models where the dependent variable is one

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to this data, and what appears in the report has been anonymized. Instead I present here the researchers' relevant results verbatim (except for translating them from the original Hebrew). The table below show the means for students from district 9 that attended in district schools (denoted by South in the table) and the means of students that attended out of district schools (denoted by North in the table). The differences between these means are very small, in math 2.9 points (0.25 sd) and in reading 2 points (0.16 sd).

Table X (Originally table 16 in Levy, Levy-Keren and Liberman (1997))

Subject	School District	Number students	Test score average	Standard deviation
Math	North	246	69.7	11.6
	South	403	66.8	11.6
Reading Comprehension	North	248	69.9	12.3
	South	407	67.9	12.3

This study also provides evidence on lack of systematic selection between students within district 9, and concluding that the Tel-Aviv choice program did not cause an imbalance in the proportion of high ability students between the those who enrolled in in and out of district schools. The same conclusion was drawn by the city education authorities as stated in the city council records (city council protocol from March 5th, 1995).

of these weighted means and the right-hand side variable is an indicator for the 1994 cohort. The stacked data on peer quality for each school includes the means of all peer characteristics and the stacked data on school quality includes the school means of *bagrut* outcomes. The estimated coefficient of the 1994 cohort indicator captures the average change in peer quality or in school quality, achieved through school choice. I than tested whether the pattern of choice in 1994 was different from the pattern of choice in the two previous years. I assumed throughout this analysis that students based their decision on observed characteristics of schools. The results are presented in following table.

Comparison of High School Peer Quality and School Mean *Bagrut* Outcomes Given School Choice Patterns of District 9 Students in Cohorts Before and After the Choice Reform

	Peer quality (mean Z-Score)	School quality (mean Z-Score)
1994 Dummy	0.397	0.373
(Clustered S.E.)	(0.215)	(0.310)
(Robust S.E.)	[0.227]	[0.236]

Column 1 presents the results from a regression of the mean of peer characteristics as the dependent variable. Columns 2 presents estimates from a similar regression, where the mean of school quality (z-score) is the dependent variable. Both estimates are positive and almost identical. The estimate in the first implies that district 9 students improved their peer quality by 0.397 sd. The estimate in the second column suggest district 9 students experienced a 0.373 sd increase in their school quality. The gain in peer quality is statistically significant but the gain in school quality is not (when standard errors are clustered by school).

Based on these results I explore further the effect of school quality as a mechanism for the effect of school choice. I estimate a student level outcome regression using a sample that includes only pre-program cohorts and include in the regression student's characteristics and school fixed effects. I then extract the school fixed effects, viewing them as a measure of school quality. These school fixed effects capture for each school the common factor of students' mean *bagrut* exams tests scores net of the contribution of all other covariates. I than standardize the schools' fixed effects to a zero mean and unit standard deviation Z-score. In a second step I add this school quality measure as a determinant in a regression of treated students' (the post reform cohort) outcomes in the difference in differences regressions, both as a main effect and as an interaction with the treatment indicator. In other words, the estimated parameter of these interaction term represents the degree to which school quality mediate the effect of school choice on treated students.

Beyond the possibility that there might be other mechanisms at play (for example, better student-school match which I discussed above), I also note here as a word of caution that the school quality variable is not completely exogenous because students choose their preferred secondary school. However, the analysis above has shown that there was not much selection in the choice between within and out of district schools and that the quality of peers, both based on mean characteristics and outcomes, did not vary with school quality. Therefore, we can proceed with this analysis even though identification of the effect of school quality is not 'safe proof'. Another reason not to be too concerned about the endogeneity of schooling quality is that the overall effect of the program from the model that includes an interaction effect between school choice and school quality, evaluated at the mean of schools' quality, is very similar to the estimated treatment effects presented in Tables 2 and 5.

I present in Table 11 the estimates of the main effect of school choice and of its interaction terms with school quality. Panel A presents results for high school outcomes, panel B for post-secondary schooling and panel C for labour market outcomes. In columns 1, 3, and 5 I present the school choice main effect and in columns 2, 4, and 6 the estimate of the interaction between school choice and school quality. First, I note that the estimates of the main effect of school choice are positive for all outcomes and in almost all cases they are statistically different from zero. The exceptions are those variables for which the effect was also not significant when the interaction term with school quality was not included. Secondly, the estimated interaction term between school choice and school quality is consistently positive, suggesting that the effect of school choice increases with school quality though with varying effect size. Third, the effect of school choice rises in most cases with school quality, indicating that the positive effect of school choice is driven partly by the good schools in the sample. For example, the effect of school choice on the average score, when evaluated at about 0.5 standard deviation above the mean of school quality, is 4.35 points and it jumps to about ten points when evaluated at the point when school quality. On the other hand, moving down away from the mean of school quality reduces quickly the positive effect of school choice.

6.1 What Explains the Increase in Earnings?

Note that if we assume that all of the 6% average increase in annual earnings 11-13 years after high school graduation (based on the DID estimates using the full sample and stacked data) is due to the 0.2 increase in years of schooling, this would imply a much higher rate of return to a year of schooling estimated in recent studies in Israel (Frisch and Moalem 1999, Frisch 2007).[31] However, as shown above, treated students experienced a range of improvements in educational outcomes that are likely to be rewarded in the labour market independently of the return to post-secondary years of schooling. Particularly important is the matriculation rate, which increased by 6%-7% and earns a return of about 13% independently of the return to years of schooling.[32] In addition, the quality improvements in the matriculation study program and diploma (for example, the average score, number of credit units and credits in honour and science subjects) could also be rewarded in the labour market beyond the return to years of schooling.[33]

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[31] It should be noted that the sample used in this analysis includes individuals with zero earnings. Therefore, the estimated impact on earnings could also reflect an indirect effect through an effect on employment, while a classic Mincer rate of return to schooling regression does not include individuals with zero earnings. However, the evidence in Table 5 did not reveal any effect on employment.

[32] For example, Angrist and Lavy (2009) suggest that *Bagrut* holders earn 13% more than other individuals with exactly 12 years of schooling but this estimate does not account for selection by ability in obtaining matriculation.

[33] Caplan et al (2009) demonstrate that earnings in Israel are highly positively correlated with the quality of post-secondary schooling (colleges versus universities and higher versus lower quality universities). For example, this study shows that earnings are much higher for graduates of Tel-Aviv, Jerusalem and the Technion Universities relative to graduates from the other four universities in the country. Admission to the top universities is of course positively correlated with the high school matriculation outcomes.

The program also led to improvements in some non-cognitive behavioural outcomes, for example it reduced the level of violence and classroom disruption, improved teacher–student relationships and increased students' social acclimation and satisfaction at school. These effects suggest that the program improved students' social kills, and recent evidence suggests that the labour market increasingly rewards such skills.[34]

The interesting question therefore is whether the gains experienced by students due to the access to free school choice (particularly the increase in academic college entry, completed years of education and the higher earnings at adulthood) could be predicted by the short- and medium-term positive effects of school choice on *Bagrut* outcomes? That is, are the effects measured at the time of the experiment predictive of the program's long-term effects? Do *Bagrut* outcomes that measure quality of study program play an equal role in this regard? For example, a matriculation certificate is a prerequisite in virtually all post-secondary programs in Israel. In addition, a matriculation certificate is a requirement in many early-career jobs which do not require a college degree. Thus, the matriculation channel of the program effect on labour market is twofold. Intuitively, in order to decompose the effect, we need to know the causal effect of matriculation on post-secondary outcomes, the labour market returns to a matriculation certificate, and the labour market returns to the different types of post-secondary schooling. Unfortunately, our research design does not allow getting unbiased estimates for these returns.[35] Therefore, given the threat of endogeneity, I can only provide some suggestive and correlative evidence on the importance of each mechanism. I use the logic of the Sobel-Goodman mediation test to examine whether getting a matriculation certificate is an important pathway for the influence of the program on long term post-secondary and labour market outcomes.[36] If this is indeed the case, we should observe a reduction in the coefficient of the program when adding high school matriculation outcomes as a control in the regression. A similar logic follows when

[34] See for example Deming (2015).

[35] I am also unaware of any reliable studies on the causal direct effect of receiving a matriculation degree on labor market outcomes in Israel.

[36] See Baron-Kenny (1986) for a canonical reference of the Sobel-Goodman test.

estimating the mediation effects of post-secondary education on labour outcomes. A recent application of this procedure is demonstrated in Satyanath, Voigtlaender and Voth (2017).

Following this approach, I first estimate OLS regressions of annual earnings on the various high school educational outcomes, while including in the regression controls for student's parental and demographic characteristics. In these regressions I use a sample that includes only the control group sample from the two pre-reform years. These results are presented in Table A9 in the online appendix. Using data for 13 years after high school graduation, I report in panel A estimates from regressions when only one of the high school outcomes is included in the regressions (columns 2) and also estimates when all outcomes are included jointly in the regression (columns 3). When included one at a time, estimates of all high school outcomes are positive and very precisely measured. When all four are included jointly, all their estimates remain positive and statistically significant. These results are consistent with evidence reported in Lavy, Ebenstein and Roth (2014) and Ebenstein, Lavy and Roth (2016) who use random shocks to performance in matriculation exams to identify the reduced form effects of these high school outcomes on earnings at adulthood, and find strong and significant positive effects.

In panel B column 2, I report estimates from regressions of the enrolment and completed years of post-secondary schooling on earnings 13 years after high school graduation, first when including only one of these outcomes at a time (column 2), and secondly where all four are included jointly (column 3). Here as well each of these outcomes have positive and significant association with earnings. In column 4, I report the estimates from a regression where all high school and post-secondary education outcomes are included jointly. Note that six of the eight outcomes still have positive and significant estimated coefficients and that three of the four high school outcomes are among them, even though the regression includes the post-secondary schooling variables. I view this as evidence that the high school outcomes that measure quality of education have an effect on earnings in addition to their effect on post-secondary schooling. A second conclusion from Table A9 is that high school and post-secondary schooling outcomes are indeed correlated with earnings.

Another way to check whether the program's effect on earnings stems from improvement in high school outcomes is to examine whether the estimated effect on post-secondary attainment and earnings shrinks or even disappears when the *Bagrut* outcomes are added as controls. Of course, such evidence could be only suggestive because the high school outcomes are endogenous and are probably correlated with the error term in the regressions of long-term outcomes. In Table A10 I present estimates of the coefficient of the school choice treatment effect in a DID regression that includes also the high-school outcomes as explanatory variables – first including one at a time and secondly all four jointly. For ease of comparison, I present in column 1 the original treatment effect estimates from Tables 2 and 6. The effect of school choice on enrolment in any post-secondary schooling is 0.040. The inclusion of each of the high-school outcomes as an additional control shrinks the treatment effect two thirds towards zero, and to zero when all four high school outcomes are included.

A similar pattern is seen in the second row of Table 7, when the long-term outcome is completed years of college schooling. However, the most striking result is the sensitivity of the treatment effect on earnings to adding the high school outcomes as controls: the average matriculation score and the number of matriculation credits reduce the treatment effect from NIS 4,763 to just over 600, an 87% decline. Similar results are obtained when the GD sample is used.

To conclude, the analysis in this section suggests that there is evidence that the long-run effect of the program is driven both by a direct effect of the program on human capital and an indirect effect of the program through earning a matriculation certificate and acceptance into post-secondary institutions. We caution once more that this analysis is only suggestive as the high-schools outcomes and the post-secondary controls are endogenous.

7. Conclusions

The vast majority of published research on the impact of school interventions has examined their effects on short-run outcomes, primarily by looking at their impact on standardized test scores. While important, a possibly deeper question is the impact of such interventions on life outcomes in the long-run. This is a critical question because the ultimate goal of education is to improve lifetime well-being. Therefore, gaining new insights about which interventions are more effective in improving long-term outcomes will make a potent contribution to the design and implementation of new interventions, better resource allocation and the efficiency of the education sector.

Recent research has begun to look at this issue, but much work remains to be done, particularly with regard to the long-term effects of interventions explicitly targeting improvement in the general quality of education and students' educational attainment. The empirical evidence from this study contributes to a more complete picture of the long term returns to various educational interventions. This effort should enable teachers, institutions and governments to make more informed decisions as to which educational programs constitute the most beneficial use of limited school resources. The high school system in Israel and its high-stakes exit exams are very similar to those in other countries, and the school choice program studied in this paper shares many features with programs implemented in recent years in the US and in European and other OECD countries. As a result, the lessons learned from this research are transferable and applicable to the education system in other developed countries.

The school choice program studied here had positive longer-term outcomes at adulthood. The evidence clearly suggests that allowing children to choose their secondary school freely at age 12, not only improved sharply their high school outcomes six years later, but it also impacted positively their path to post-secondary schooling and increased meaningfully their earnings about a decade and a half later. It is important to note also that these gains were not at the expense of other students in the receiving schools, because this later group was already exposed to a similar proportion of incoming students as a result of the bussing program that preceded the school choice program. If such a peer effect was operational it should have been positive, because the opting out to schools outside the own school district was voluntary in the choice program while being compulsory during the bussing program. In earlier work I have shown that the improved outcomes during middle school and high school were facilitated by a better student-school match, by more competition among schools and by higher schooling quality. These results are important because the school choice experiment targeted a disadvantaged population in some of the more deprived parts of Tel-Aviv. Since evidence about the

Tel-Aviv school choice program has become available, other school choice programs were introduced, for example in Jerusalem in 2006 and more recently in many cities in Israel.[37]

As a final remark, it is important to note that the Tel-Aviv school choice program is very similar to school-choice programs implemented in Europe, for example in the Netherlands, Belgium and Sweden, and in the US where free school choice programs have replaced, under court order, zoning and cross-district busing. But these programs, in Israel and elsewhere, are very different in its fundamentals from charter schools and school vouchers programs in the US that are evaluated in the literature. For example, US voucher programs are targeted to students from low income families and charter schools are not 'regular' public schools as they are exempted from many public education rules and regulations. The Tel-Aviv choice program highlights a compromise struck by the municipality between choice (among public schools) and regulation, which perhaps explains why the results presented in this paper are different from evidence in the US, and thus offering a path for other attempts at adjusting school choice programs.

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[37] The Ministries of Education and of Finance in Israel recently introduced a large school choice program in the largest cities in the country. As this recent expansion is reaching a nationwide scale, issues of general equilibrium effects become important, in particular with regard to whether the higher education system in Israel can accommodate the expected increased demand for post-secondary schooling. In this regard it is important to note that the creation of academic colleges that started in the mid 1990's has gained momentum and in the following two decades more such colleges were opened all over the country. This large supply expansion and the existing excess capacity in most of these colleges will be able to accommodate the increase in demand for higher education due to a country wide school choice program, without repercussions for the existing demand.

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Table 1: Descriptive Statistics and Pre and Post Treatment-Contorl Comparison of Means of Post-Secondary Schooling Outcomes , Employment Earnings, and Personal Status Outcomes (11 Years Since High School Graduation)

	Pre: 92 and 93 cohorts			Post: 94 cohort		
	Treated schools mean	Non treated Schools mean	Mean Difference (Standart error)	Treated schools mean	Non treated Schools mean	Mean Difference (Standart error)
	(1)	(2)	(3)	(4)	(5)	(6)
A. Enrollment						
University	0.172	0.231	-0.058	0.158	0.231	-0.073
	(0.378)	(0.421)	(0.012)	(0.365)	(0.421)	(0.016)
Academic College	0.200	0.263	-0.063	0.254	0.282	-0.028
	(0.400)	(0.440)	(0.012)	(0.436)	(0.450)	(0.017)
B. Years of Schooling						
University	0.683	0.974	-0.290	0.646	0.975	-0.330
	(1.728)	(2.044)	(0.056)	(1.688)	(2.019)	(0.077)
Academic College	0.555	0.822	-0.267	0.759	0.865	-0.106
	(1.311)	(1.595)	(0.043)	(1.559)	(1.594)	(0.062)
C. Labor Market Outcomes						
Employed (1 = Yes, 0 = No)	0.868	0.841	0.028	0.841	0.844	-0.003
	(0.338)	(0.366)	(0.010)	(0.366)	(0.363)	(0.014)
Months Worked	9.284	9.021	0.264	9.004	9.081	-0.077
	(4.369)	(4.572)	(0.126)	(4.534)	(4.524)	(0.176)
Average Annual Earnings (NIS)	70,639	73,588	-2,949	73,091	75,518	-2,427
	(58,957)	(64,070)	(1,759)	(64,950)	(67,281)	(2,608)
D. Personal Status Outcomes						
Married (1 = Yes, 0 = No)	0.525	0.505	0.020	0.457	0.393	0.064
	(0.500)	(0.500)	(0.014)	(0.498)	(0.489)	(0.019)
Age of first marriage	25.563	25.871	-0.308	24.953	25.260	-0.308
	(3.014)	(2.901)	(0.108)	(2.522)	(2.574)	(0.145)
Children (1 = Yes, 0 = No)	0.447	0.408	0.039	0.372	0.281	0.091
	(0.497)	(0.491)	(0.014)	(0.484)	(0.449)	(0.018)
Age of first child	26.592	26.989	-0.397	25.962	25.962	0.000
	(2.939)	(2.900)	(0.122)	(2.589)	(2.649)	(0.173)
Number of children	0.796	0.692	0.104	0.612	0.443	0.169
	(1.070)	(1.025)	(0.029)	(0.976)	(0.841)	(0.034)
E. Parental Earnings						
Average Father's Earnings in 2000-2002	93,374	120,550	-27,175	98,227	122,320	-24,093
	(114,484)	(154,358)	(4,229)	(100,779)	(158,485)	(6,043)
Average Mother's Earnings in 2000-2002	48,131	59,035	-10,904	53,899	61,873	-7,974
	(60,738)	(71,551)	(1,952)	(84,227)	(73,365)	(2,946)
Average Family Earnings in 2000-2002	141,820	179,988	-38,168	153,039	184,665	-31,626
	(137,004)	(184,831)	(5,080)	(139,228)	(186,745)	(7,256)
Number of Observations	1,519	8,902		779	4,255	

Notes: The table reports means and standard deviations for different post-secondary education and employment variables for 11 years after high school graduation. Each column represents these statistics for a different group as described in each column's headline. Panel A is comprised of binary variables indicating whether the individual was ever enrolled 11 years after high school graduation in a specific type of post-secondary institution. The categories are not mutually exclusive and overlapping is possible. Panel B reports the number of years of education an individual has attained by 11 years after high school graduation in each type of the post-secondary institutions listed in panel A. Panel C reports the mean of an employment indicator, annual earnings and the number of months worked 11 years after high school graduation.

Table 2: Effect of School Choice on Post-Secondary Schooling, 12 Years Since High School Graduation

	Enrollment		**Years of Schooling**	
	Mean, 1992-1993 Cohorts in Treated Schools	Treatment	Mean, 1992-1993 Cohorts in Treated Schools	Treatment
	(1)	(2)	(3)	(4)
A. Any Post Secondary Schooling	0.425	0.046	1.648	0.187
	(0.494)	(0.022)	(2.363)	(0.089)
B. University Schooling	0.179	0.006	0.717	0.050
	(0.383)	(0.014)	(1.801)	(0.063)
C. College Schooling	0.209	0.040	0.589	0.171
	(0.407)	(0.018)	(1.361)	(0.050)
Number of Observations	1,539	15,669	1,539	15,669

Notes: This table presents the differences-in-differences estimates of the effect of the School Choice program on post-secondary schooling. Columns 1-2 measure enrollment into different types of post-secondary institutions, while columns 3-4 measure completed years of post-secondary education by institution type. The results are for 12 years after high school graduation. The variable "Any Post-Secondary Education" refers to all different post-secondary institutions. Columns 1 and 3 .represent the mean and standard deviation for the 1992-1993 (untreated) cohorts in the treated schools. Columns 2 and 4 report the differences-in-differences estimates for each of the dependent variables. Standard errors are clustered at the school

Table 3: Geography Discontinuity Descriptive Statistics and Pre and Post Treatment-Contorl Comparison of Means of Post-Secondary Schooling Outcomes , Employment Earnings, and Personal Status Outcomes (11 Years Since High School Graduation)

	Pre: 92 and 93 cohorts			Post: 94 cohort		
	Treated schools mean	Non treated Schools mean	Mean Difference (Standart error)	Treated schools mean	Non treated Schools mean	Mean Difference (Standart error)
	(1)	(2)	(3)	(4)	(5)	(6)
A. Enrollment						
University	0.226	0.297	-0.071	0.209	0.272	-0.063
	(0.419)	(0.457)	(0.025)	(0.407)	(0.446)	(0.031)
Academic College	0.247	0.293	-0.046	0.288	0.339	-0.051
	(0.432)	(0.455)	(0.025)	(0.454)	(0.474)	(0.033)
B. Years of Schooling						
University	0.869	1.257	-0.387	0.850	1.162	-0.312
	(1.864)	(2.244)	(0.117)	(1.870)	(2.146)	(0.145)
Academic College	0.705	0.939	-0.234	0.885	1.084	-0.199
	(1.459)	(1.688)	(0.089)	(1.621)	(1.748)	(0.121)
C. Labor Market Outcomes						
Employed (1 = Yes, 0 = No)	0.890	0.830	0.060	0.856	0.864	-0.008
	(0.314)	(0.376)	(0.020)	(0.352)	(0.343)	(0.025)
Average Annual Earnings (NIS)	77,144	78,738	-1593.381	80,634	76,893	3740.997
	(59,438)	(70,772)	(3,688)	(71,412)	(70,312)	(5,055)
Months worked	9.473	8.938	0.535	8.874	9.212	-0.338
	(4.315)	(4.687)	(0.252)	(4.587)	(4.297)	(0.316)
Number of Observations	535	834		340	463	
D. Personal Status outcomes						
Married (1 = Yes, 0 = No)	0.546	0.489	0.057	0.453	0.352	0.101
	0.499	0.501	0.039	0.499	0.478	0.035
Age of first marriage	25.894	26.689	-0.795	24.955	25.528	-0.573
	2.947	2.395	0.290	2.429	2.366	0.269
Children (1 = Yes, 0 = No)	0.403	0.308	0.095	0.335	0.222	0.113
	(0.491)	(0.462)	(0.037)	(0.473)	(0.416)	(0.032)
Age of first child	26.917	27.959	-1.042	24.955	25.528	-0.573
	2.889	2.375	0.340	2.429	2.366	0.269
Number of children	(0.713)	(0.486)	(0.227)	(0.535)	(0.333)	(0.203)
	1.063	0.794	0.072	0.926	0.710	0.058
E. Parental Earnings						
Average Father's Earnings in 2000-2002	112,853	139,061	-26,208	107,873	138,760	-30,887
	(140,079)	(153,169)	(8,343)	(112,571)	(151,137)	(9,971)
Average Mother's Earnings in 2000-2002	56,552	71,761	-15,210	61,268	76,347	-15,079
	(70,152)	(76,009)	(4,093)	(110,611)	(93,145)	(7,237)
Average Family Earnings in 2000-2002	169,279	211,501	-42,222	169,992	215,898	-45,906
	(162,594)	(181,331)	(9,827)	(163,890)	(198,632)	(13,600)
Number of Observations	1,519	8,902		779	4,255	

Notes : The table reports means and standard deviations for different post-secondary education and employment variables for 11 years after high school graduation for the geography discontinuity sample described in the paper. Each column represents these statistics for a different group as described in each column's headline. Panel A is comprised of binary variables indicating whether the individual was ever enrolled until 11 years after high school graduation in a specific type of post-secondary institution. The categories are not mutually exclusive and overlapping is possible. Panel B reports the number of years of education an individual has attained 11 years after high school graduation in each type of the post-secondary institutions listed in panel A. Panel C reports the mean of an employment indicator, annual earnings and the number of months worked 11 years after high school graduation.

Table 4: Geography Discontinuity Estimates of the Effect of School Choice on Post-Secondary Schooling, 10-12 Years Since High School Graduation

	Enrollment Post High School Education		Post High School Years of Schooling	
	Mean of 1992-1993 Cohorts in Treated Schools	Estimate	Mean of 1992-1993 Cohorts in Treated Schools	Estimate
	(1)	(2)	(3)	(4)
A. Any Post Secondary Schooling				
Ten years	0.503	0.044	1.845	0.265
	(0.500)	(0.037)	(2.258)	(0.145)
Eleven years	0.509	0.045	1.945	0.247
	(0.500)	(0.037)	(2.363)	(0.155)
Twelve years	0.512	0.042	2.004	0.209
	(0.500)	(0.039)	(2.453)	(0.165)
B. University Schooling				
Ten years	0.232	0.048	0.879	0.259
	(0.423)	(0.032)	(1.836)	(0.148)
Eleven years	0.236	0.046	0.908	0.258
	(0.425)	(0.032)	(1.897)	(0.157)
Twelve years	0.235	0.051	0.913	0.260
	(0.424)	(0.031)	(1.941)	(0.158)
C. College Schooling				
Ten years	0.234	-0.006	0.647	0.049
	(0.424)	(0.030)	(1.363)	(0.098)
Eleven years	0.247	-0.004	0.705	0.043
	(0.432)	(0.030)	(1.455)	(0.102)
Twelve years	0.257	-0.011	0.749	0.017
	(0.437)	(0.030)	(1.515)	(0.102)
Number of Observations	547	2,206	547	2,206

Notes : This table presents the differences-in-differences estimates of the effect of the School Choice program on post-secondary schooling for the geography discontinuity sample described in the paper. Columns 1-2 measure enrollment into different types of post-secondary institutions, while columns 3-4 measure completed years of post-secondary education by institution type. The results are for 10-12 years after high school graduation. The variable "Any Post-Secondary Education" refers to all different post-secondary institutions. Columns 1 and 3 represent the mean and standard deviation for the 1992-1993 (untreated) cohorts in the treated schools. Columns 2 and 4 report the differences-in-differences estimates for each of the dependent variables. Standard errors are clustered at the school level.

Table 5: Effects of School Choice on Employment and Income By Years Since High School Graduation

	11 Years		12 Years		13 Years		Stacked Regression 11-13 Years	
	Mean, 1992-1993 Cohorts in Treated Schools	Estimate	Mean, 1992-1993 Cohorts in Treated Schools	Estimate	Mean, 1992-1993 Cohorts in Treated Schools	Estimate	Mean, 1992-1993 Cohorts in Treated Schools	Estimate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employment Indicator (1 = Yes, 0 =	0.87	-0.031	0.852	-0.012	0.854	-0.09	0.858	-0.015
	(0.337)	(0.015)	(0.355)	(0.014)	(0.353)	(0.012)	(0.349)	(0.011)
Total Annual Earnings (2009 NIS)	74709	3368	78313	5544	81230	5632	78188	4763
	(64,595)	(2,285)	(67,5210	(2,500)	(70,432)	(2,668)	(67,808)	(2,282)
Months Worked	9.31	-0.31	9.251	-0.313	9.17	-0.154	9.228	-0.241
	(4.354)	(0.198)	(4.453)	(0.155)	(4.456)	(0.150)	(4.433)	(0.146)
Percentile Rank	41.358	1.405	40.695	1.51	40.266	1.969	40.822	1.621
	(30.582)	(1.171)	(30.597)	(1.106)	(30.531)	(1.103)	(30.630)	(1.022)
Number of Observations	1537	15634	1532	15616	1527	15578	4668	47276
Log(Earnings)	11.035	0.086	11.119	0.043	11.15	0.09	11.102	0.068
	(0.988)	(0.037)	(0.935)	(0.032)	(0.938)	(0.037)	(0.958)	(0.028)
Number of Observations	1339	13282	1313	13229	1307	13198	3959	39709

Notes: Columns 1-2 report results for 11 years after high school graduation, columns 3-4 for 12 years, and columns 5-6 for 13 years.
Columns 1,3, 5 and 7 report the mean and standard deviation for the 1992-1993 (untreated) cohorts in the treated schools.
Columns 2, 4, 6 and 8 report the differences-in-differences estimates for each of the dependent variables listed above.
Standard errors are clustered at the school level.

Table 6: Goegraphy Discontinuity Estimates of the Effect of School Choice on Employment and Income By Years Since High School Graduation

	11 Years		12 Years		13 Years		Stacked Regression 11-13 years	
	Mean of 1992-1993 Cohorts in Treated Schools	Estimate	Mean of 1992-1993 Cohorts in Treated Schools	Estimate	Mean 1992-1993 Cohorts in Treated Schools	Estimate	Mean 1992-1993 Cohorts in Treated Schools	Estimate
	(1)	(2)	(4)	(5)	(7)	(8)	(7)	(8)
Employment Indicator (1 = Yes, 0 = No)	0.892	-0.065	0.867	-0.066	0.865	-0.042	0.872	-0.058
	(0.311)	(0.027)	(0.340)	(0.026)	(0.343)	(0.023)	(0.335)	(0.023)
Total Annual Earnings (2009 NIS)	83,397	10,099	87,782	9,044	92,623	6,005	87,810	7,613
	(67,627)	(4,341)	(71,565)	(5,281)	(76,376)	(5,882)	(72,250)	(4,859)
Months worked	9.720	-0.678	9.497	-0.752	9.538	-0.758	9.534	-0.747
	(4.061)	(0.347)	(4.298)	(0.273)	(4.286)	(0.256)	(4.256)	(0.262)
Number of Observations	546	2,204	546	2,204	539	2,184	1,668	6,659

Notes: This table presents the differences-in-differences estimates of the effect of the School Choice program on different employment and earnings outcomes for the geography discontinuity sample described in the paper. Columns 1-2 report results for 11 years after high school graduation, columns 4-5 report results for 12 years after high school graduation and columns 5-6 report results for 13. The variable "Employment Indicator" equals 1 if an individual has any work record for the given year and 0 otherwise. Columns 1,3, 5 and 7 report the mean and standard deviation for the 1992-1993 (untreated) cohorts in the treated schools. Columns 2, 4, 6 and 8 report the differences-in-differences estimates for each of the dependent variables listed above. Standard errors are clustered at the school level.

Table 7: Effect of School Choice on High School Outcomes By Gender

	Boys		Girls	
	Mean, 1992-1993 Cohorts in Treated Schools	Treatment	Mean, 1992-1993 Cohorts in Treated Schools	Treatment
	(1)	(2)	(5)	(6)
Drop out	0.266	-0.101	0.103	-0.021
	(0.442)	(0.027)	(0.304)	(0.014)
Eligible for Bagrut	0.377	0.071	0.513	0.090
	(0.485)	(0.024)	(0.500)	(0.029)
Average score	53.499	8.856	66.823	3.968
	(37.166)	(1.784)	(32.578)	(1.643)
Number of science credits	1.414	0.466	1.709	0.333
	(3.101)	(0.199)	(3.606)	(0.201)
Number of Credit Units in Matriculation Exams	14.427	2.913	17.312	1.167
	(11.641)	(0.590)	(10.000)	(0.541)
Number of honor-level subjects	1.390	0.386	1.698	0.182
	(1.540)	(0.085)	(1.346)	(0.068)
Number of Observations	801	8,093	741	7,601

Notes : This table presents the differences-in-differences estimates of the effect of the School Choice program on high school outcomes for boys and girls separately. Columns 1 and 3 represent the mean and standard deviation for the 1992-1993 (untreated) cohorts in the treated schools. Columns 2 and 4 report the differences-in-differences estimates for each of the dependent variables. Standard errors are clustered at the school level.

Table 8: Effect of School Choice on Post-Secondary Schooling, 12 Years Since High School Graduation

	Boys				Girls			
	Enrollment		Years of Schooling		Enrollment		Years of Schooling	
	Mean, 1992-1993 Cohorts in Treated Schools	Estimate	Mean, 1992-1993 Cohorts in Treated Schools	Estimate	Mean, 1992-1993 Cohorts in Treated Schools	Estimate	Mean, 1992-1993 Cohorts in Treated Schools	Estimate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Any Post Secondary Schooling	0.372	0.093	1.447	0.331	0.481	-0.003	1.864	0.029
	(0.484)	(0.031)	(2.323)	(0.120)	(0.500)	(0.029)	(2.389)	(0.136)
B. University Schooling	0.143	0.030	0.605	0.113	0.218	-0.021	0.838	-0.015
	(0.350)	(0.023)	(1.751)	(0.105)	(0.413)	(0.022)	(1.848)	(0.108)
C. College Schooling	0.191	0.039	0.561	0.197	0.222	0.039	0.619	0.133
	(0.393)	(0.024)	(1.340)	(0.076)	(0.416)	(0.022)	(1.383)	(0.066)
Number of Observations	798	8,064	798	8,064	741	7,605	741	7,605

Notes: This table presents the differences-in-differences estimates of the effect of the School Choice program on post-secondary schooling for boys and girls separately. Columns 1-2 and 5-6 measure enrollment into different types of post-secondary institutions, while columns 3-4 and 7-8 measure completed years of post-secondary education by institution type. The variable "Any Post-Secondary Education" refers to all different post-secondary institutions. Columns 1, 3, 5 and 7 represent the mean and standard deviation for the 1992-1993 (untreated) cohorts in the treated schools. Columns 2, 4, 6 and 8 report the differences-in-differences estimates for each of the dependent variables. Standard errors are clustered at the school level.

Table 9: Effect of School Choice on Employment and Income, By Gender and Years Since High School Graduation

	11 Years		**12 Years**		**13 Years**		**Stacked Regression 11-13 Years**	
	mean, 1992-1993 Cohorts in Treated Schools	Estimate	mean, 1992-1993 Cohorts in Treated Schools	Estimate	mean, 1992-1993 Cohorts in Treated Schools	Estimate	1992-1993 Cohorts in Treated Schools	Estimate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Boys								
Employment Indicator (1 = Yes, 0 = No)	0.853	-0.010	0.836	-0.002	0.837	0.009	0.842	-0.000
	(0.354)	(0.023)	(0.371)	(0.023)	(0.370)	(0.021)	(0.365)	(0.020)
Total Annual Earnings (2009 NIS)	82,746	4,705	86,638	7,949	91,653	8,790	87,400	6,807
	(73,242)	(3,696)	(76,652)	(3,898)	(79,139)	(4,256)	(76,785)	(3,615)
Months Worked	9.205	0.171	9.173	0.011	9.153	0.335	9.172	0.187
	(4.542)	(0.290)	(4.627)	(0.247)	(4.639)	(0.258)	(4.601)	(0.244)
Number of Observations	797	8040	792	8028	789	7994	2411	24222
B. Girls								
Employment Indicator (1 = Yes, 0 = No)	0.888	-0.052	0.870	-0.020	0.873	-0.029	0.875	-0.031
	(0.316)	(0.018)	(0.336)	(0.020)	(0.334)	(0.019)	(0.331)	(0.014)
Total Annual Earnings (2009 NIS)	66,053	1,218	69,403	2,252	70,087	1,504	68,347	1,785
	(52,438)	(2,891)	(54,794)	(2,340)	(57,743)	(2,408)	(55,007)	(2,136)
Months Worked	9.423	-0.827	9.335	-0.612	9.188	-0.659	9.288	-0.671
	(4.143)	(0.260)	(4.261)	(0.222)	(4.255)	(0.212)	(4.245)	(0.175)
Number of Observations	738	7,584	1,532	14,605	1,527	14,569	2,257	23,054

Notes: This table presents the differences-in-differences estimates of the effect of the School Choice program on different employment and earnings outcomes for boys and girls separately. Columns 1-2 report results for 11 years after high school graduation, columns3-4 report results for 12 years after high school graduation and columns 5-6 report results for 13. The variable "Employment Indicator" equals 1 if an individual has any work record for the given year and 0 otherwise. Columns 1,3, 5 and 7 report the mean and standard deviation for the 1992-1993 (untreated) cohorts in the treated schools. Columns 2, 4, 6 and 8 report the differences-in-differences estimates for each of the dependent variables listed above. Standard errors are clustered at the school level.

Table 10: Effect of School Choice on Marriage and Fertility, 10 Years After High School

	Entire Sample		Boys		Girls	
	mean, 1992-1993 Cohorts in Treated Schools	Estimate	mean, 1992-1993 Cohorts in Treated Schools	Estimate	mean, 1992-1993 Cohorts in Treated Schools	Estimate
	(1)	(2)	(3)	(4)	(5)	(6)
Children (1 = Yes, 0 = No)	0.433	0.031	0.355	-0.021	0.518	0.088
	(0.496)	(0.021)	(0.479)	(0.026)	(0.500)	(0.030)
Number of children	0.794	0.021	0.614	-0.102	0.972	0.158
	(1.069)	(0.045)	(0.966)	(0.051)	(1.136)	(0.065)
Age of first child	26.590	0.513	27.316	0.460	26.067	0.522
	(2.941)	(0.174)	(2.530)	(0.255)	(3.103)	(0.241)
Married (1 = Yes, 0 = No)	0.562	0.028	0.499	-0.022	0.629	0.081
	(0.496)	(0.021)	(0.500)	(0.030)	(0.483)	(0.025)
Age of first marriage	25.567	0.125	26.561	0.345	24.752	0.004
	(3.014)	(0.151)	(2.537)	(0.172)	(3.134)	(0.232)
Number of Observations	1,542	15,708	801	8,098	741	7,610

Notes: This table presents the differences-in-differences estimates of the effect of the School Choice program on different personal outcomes. Columns 1-2 report results for the entire sample while columns 3-4 and columns 5-6 report the results for boys and girls respectivly.Columns 1,3, and 5 report the mean and standard deviation for the 1992-1993 (untreated) cohorts in the treated schools. Columns 2,4, and 6 report the differences-in-differences estimates for each of the dependent variables listed above. Standard errors are clustered at the school level.

Table 11: Estimates of Treatment Effect Heterogeneity by School Quality

A. High School Outcomes

	Main treatment effect (1)	Treat X School quality (2)
Drop out	-0.029	-0.127
	(0.018)	(0.034)
Eligible for Matriculation Diploma	0.082	0.092
	(0.024)	(0.034)
Average Score	4.35	11.973
	(1.608)	(3.127)
Matriculation Units	1.762	3.22
	(0.509)	(0.919)
Science Matriculation Units	0.587	0.265
	(0.161)	(0.269)
Number Honor-level Subjects	0.273	0.36
	(0.068)	(0.121)

B. Post-Secondary Outcomes

	Main treatment effect (3)	Treat X School quality (4)
Any Post-Secondary, Enrollment	0.056	0.101
	(0.026)	(0.047)
Any Post-Secondary, Years	0.203	0.526
	(0.107)	(0.237)
University, Enrollment	0.018	0.033
	(0.014)	(0.033)
University, Years	0.085	0.079
	(0.079)	(0.19)
College, Enrollment	0.041	0.057
	(0.016)	(0.028)
College, Years	0.18	0.351
	(0.069)	(0.108)

C. Labor Market Outcomes

	Main treatment effect (5)	Treat X School quality (6)
Employment Indicator	-0.011	0.037
	(0.013)	(0.032)
Total annual Earnings	5691	10881
	(2911)	(6455)
Percentile Rank	2.195	5.371
	(1.164)	(2.34)
Months Worked	-0.132	0.479
	(0.165)	(0.384)

Note: Standard errors are presented in parentheses and are clustered by school.

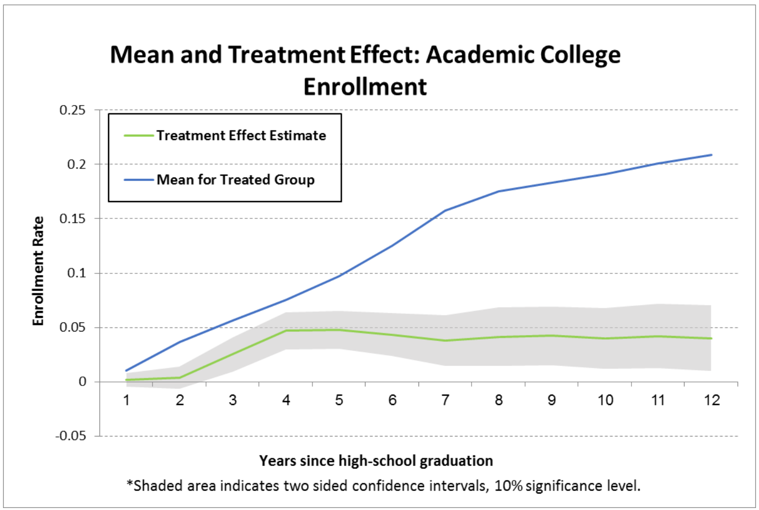


Figure 1

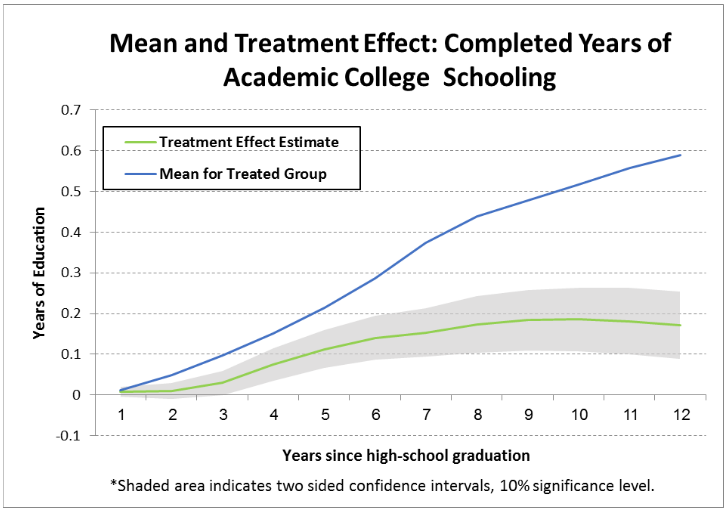


Figure 1A

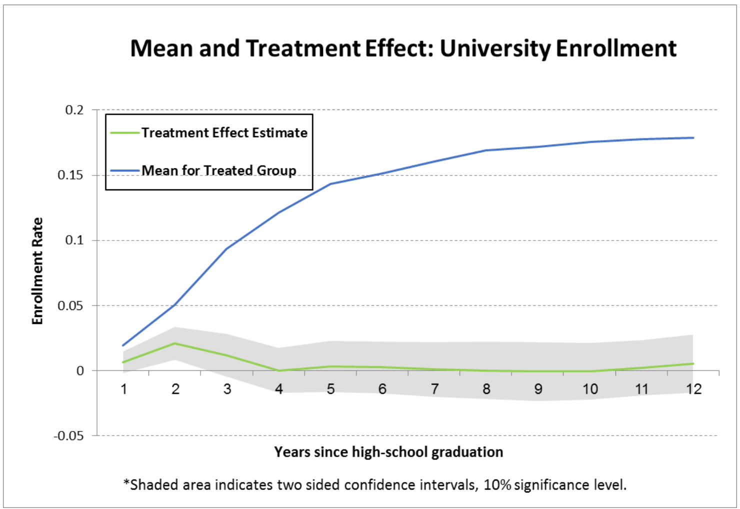


Figure 2

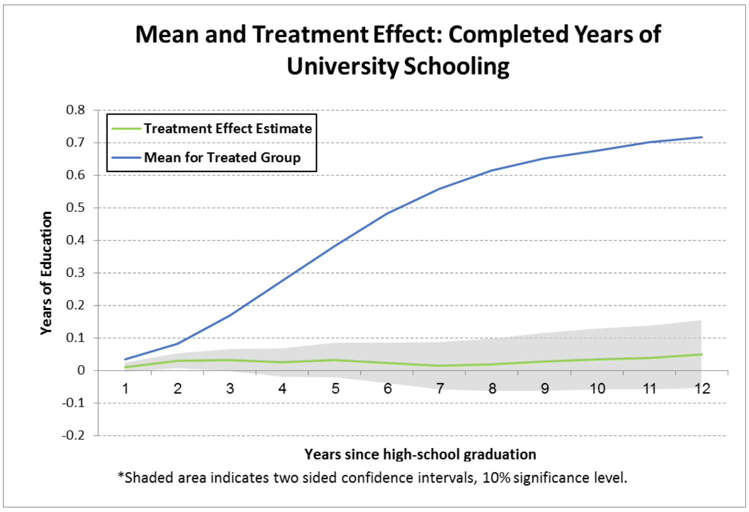


Figure 2A

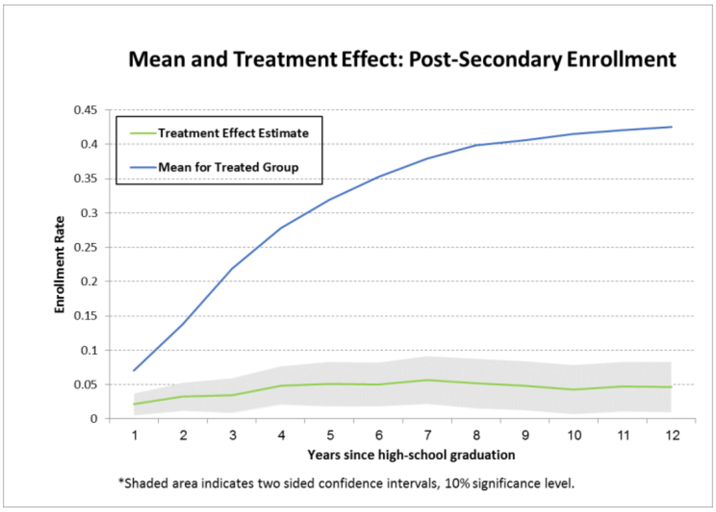


Figure 3

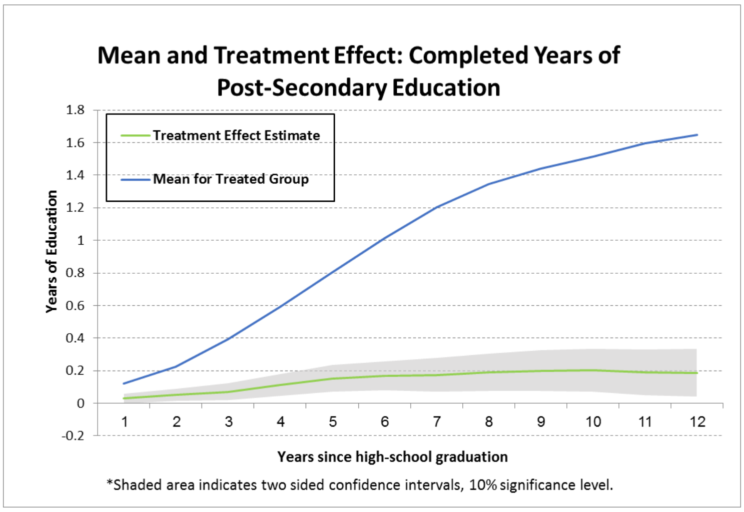
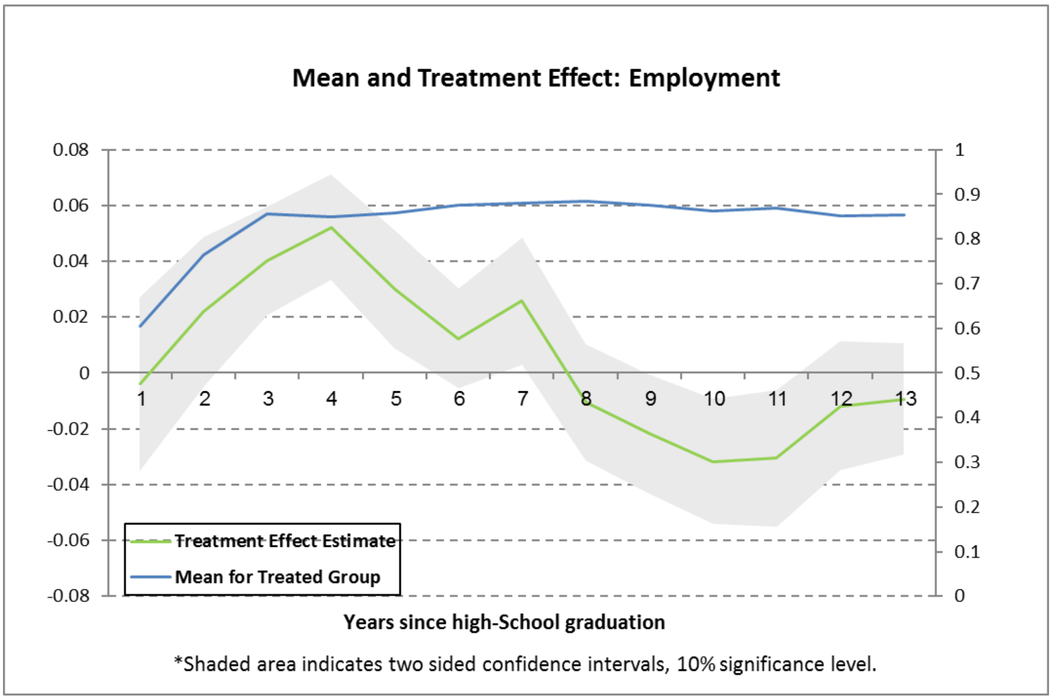


Figure 3A



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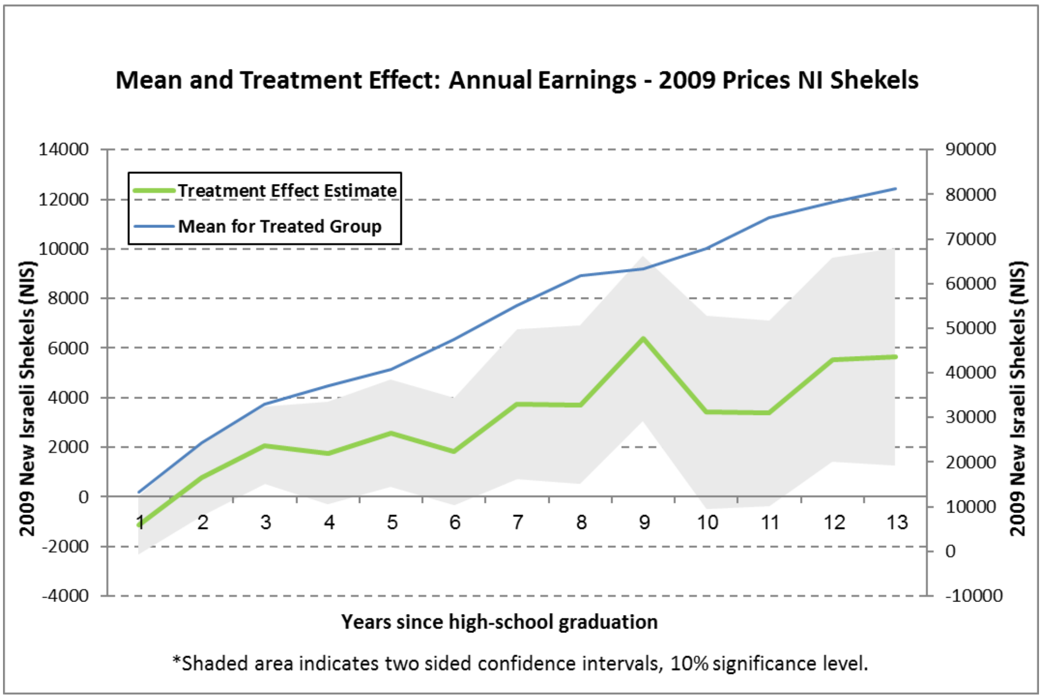

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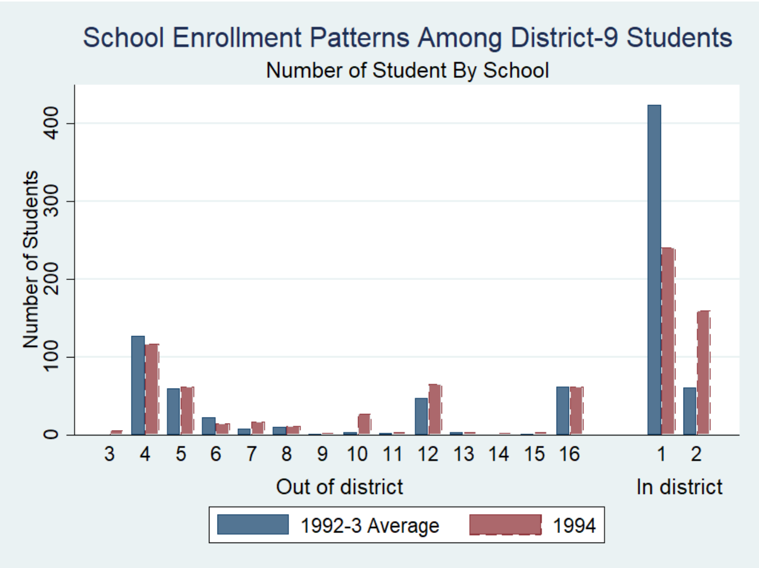

Figure 6

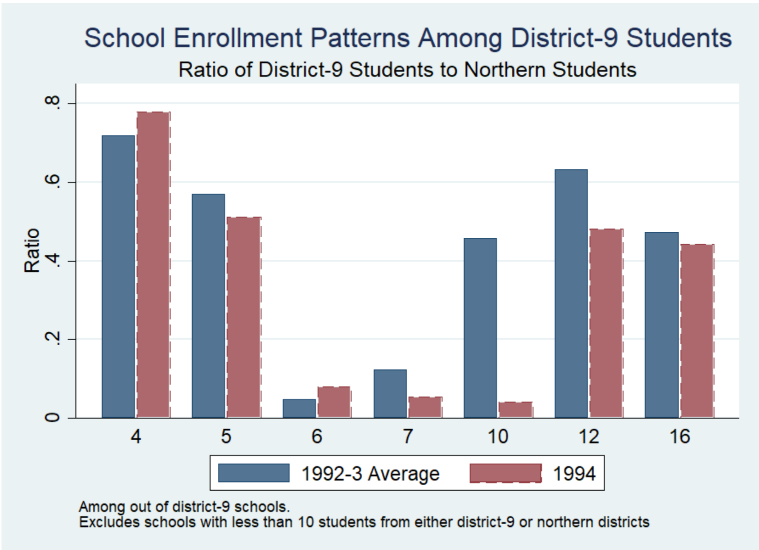

Figure 7