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LONG RUN EFFECTS OF FREE SCHOOL CHOICE: COLLEGE ATTAINMENT, EMPLOYMENT, EARNINGS, AND SOCIAL OUTCOMES AT ADULTHOOD

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ABSTRACT

Research in economics of education about the effectiveness of educational programs and interventions have centered primarily on standardized test scores as a measure of success. However, since the ultimate goal of education is to improve lifetime well-being, attention shifted recently to long term consequences at adulthood, for example post-secondary schooling. However, the type of educational interventions studied is still limited and much remained to be unraveled. In this paper I study the long term consequences of free school choice by taking advantage of an experiment conducted two decades ago in the city of Tel Aviv, Israel. This school choice program was very effective in improving high school attainment and cognitive achievements six years later (Lavy 2010) and now I examine whether these effects persist beyond high school. The results indicate that treated students experience significant gains in post-secondary enrollment and in completed years of education and also have higher earnings at age 30. These significant positive treatment effects reflect mainly an increase in academic education, through increased enrollment in three-years academic colleges but not in research universities, and some shift away from vocational education at adulthood. Additional gains are reductions in eligibility and recipiency of disability welfare allowances.

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1. Introduction

Research in economics of education about the effectiveness of educational programs and interventions have centered on the evaluation of impact on short-term outcomes, primarily standardized test scores, as a measure of success. However, understanding that the ultimate goal of education is to improve lifetime well-being, attention shifted recently to long term consequences at adulthood with an initial focus on post-secondary attainment in light of the increasing economic returns to higher education (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 early adulthood earnings; Dustmann et al (2012) examined the effect of high school quality on completed schooling and labor-market outcomes; Dynarski et al (2013) examined the effect of smaller classes in primary school on college entry, college choice, and degree completion; Deming, Cohodes, Jennings, and Jencks (2013) studied the impact of accountability pressure in high schools on postsecondary attainment and earnings; Chetty et al (2014) examined the earnings consequences of primary and middle school teachers' 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 determining which interventions are more effective in achieving better long-term outcomes, but the scope of educational interventions studied is still limited and much remained to be unraveled. In this paper I study the long term consequences of free school choice offered to primary school students in the junction of transition to secondary schools. The main question that 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. To address this key issue, I take advantage of a school choice experiment which was conducted two decades ago in the city of Tel Aviv, Israel. In Lavy (2010) I analyzed the short and medium-term effects on cognitive outcomes and schooling attainment during middle and high school. With the passage of time, this previous research endeavor now presents an unusual opportunity to evaluate

whether free school choice among public schools have a lasting and long-term impact on social and economic outcomes. This paper provides the first evidence of links between school choice and students' employment and earnings, and social outcomes at adulthood. I examine the impact on various types of post-secondary schooling that vary by quality, and on employment, earnings, and dependency on welfare transfers at about age thirty. Therefore, this paper presents a wide characterization of effect of school choice among public schools at adulthood.

The evidence I present here shows that the school choice experiment had increased a wide range of post secondary schooling measures. Two decades after students made their school choice at the end of primary school, treated students are 4.7 percentage points more likely to enroll in post secondary schooling, and to complete almost an additional quarter year of college schooling in comparison to students in the control group. These gains reflect a 15 percent increase relative pre-program means and they are similar in magnitude to the gains due to the program at the end of high school in matricualtion outocmes (Lavy 2010). The increase in post-secondary schooling reflects mainly an increase in academic education, through increased enrollment in academic and teachers' colleges but without any gain in enrollment in research universities. This is not a surprising result since those affected by the program are marginal students from low socio-economic families who would not enroll in any academic post secondary schooling if not for the school choice program. I also find a shift away from vocational education (one year study program leading to a technicain diploma and two year colleges granting practical engineering defrees). It is important to note that these results are of general equilibrium 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. Naturally, this concern will have to be addressed in a context of a nationwide school choice program. Alongside these academic improvements, I also estimate an increase of 5-7 percentage points in average annual earnings among treated students at ages 28-30 relative to the respective control group mean. These gains are most likely explained by the improvement caused by the choice program in high school academic outcomes (matriculation composite score, matricualtion diploma, number of matricualtion subjects at honor level) and in post secondary schooling attainment,

both of which are highly correlated with labor market earnings. Finally, I also estimate that school choice led to a decline in eligibility and recipiency of disability welfare transfer from the governemnt. However, I do not find any systematic effect on marriage and parenthood rates.

The lessons learned from this analysis are easily transferable and applicable 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 and developing countries. Another important advantage of the evidence presented in this paper is that school choice is an intervention or a policy change that can be directly manipulated by public policy, while most recent related studies provided evidence on the effect on long-term outcomes of measures not easily measured and manipulated by policy such as school's or teachers' quality.

There is little causal evidence on the long term effect of school choice even though it is still a very controversial policy. The earlier studies on short term effects of school choice, for example Rouse (1998) and Cullen, Jacob and Levitt (2006), were followed recently by few studies that look at the effect of school choice on long term outcomes, in particular on misbehavior and crime (Deming 2011), and on post-secondary schooling attainment (Demming et al 2013, Chingos and Peterson 2013, Wondratschek et al 2013). Demming et al (2013) study the impact of the public school choice lottery in Charlotte-Mecklenburg schools and find a significant overall increase in college attainment among lottery winners who attend their first-choice school. 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 enrollment of the voucher offer, but they estimate large significant impacts for African-American students and smaller but statistically insignificant impacts for Hispanic students. Wondratschek et al (2014) 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 zero effects on university education, employment, criminal activity and health at age 25. Deming et al (2014) study the impact of a public school choice lottery in Charlotte-Mecklenburg schools on college enrollment and degree completion. Their findings suggest a

significant overall increase in college attainment among lottery winners who attend their first choice school.

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

2. Background

The results 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 6.5 percentage points (a 32 percent decline), increased the matriculation rate by 8 percentage points (a 25 percent increase), and increased the average score in the *Bagrut* exams by almost 7 points (a 12 percent increase). It also improved the quality of schooling as the number of *Bagrut* credit units increased by 2 (a 13 percent increase), the number of credits in science subjects increased by half a unit (a 33 percent increase), and the number of *Bagrut* subjects studied at honor level increased by 0.3 (a 20 percent increase). I summarize below the main feature of the Tel Aviv school choice program and then turn to examine whether these gains were translated to economically meaningful improvements of adulthood outcomes.

The Tel-Aviv school-choice program

In May 1994, the Israeli Ministry of Education approved TASCP (Tel Aviv School Choice Program) as a two-year experiment to be implemented in the city's 9th district (see Map 1 in online appendix). It was the first choice program in the country since the 1968 education reform that enacted compulsory integration in grades 7–9.[1] 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

[1] 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 who offered only the higher grades (10th-12th).

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 percent of the districts' pupils) and a few more of the districts' pupils (5 percent) were enrolled in charter schools outside the district. The graduates of the other seven secular primary schools were assigned to one of the three secondary schools within district 9.[2]

In May 1994 the education board of Tel-Aviv announced that as of September 1994 this system would be replaced by free choice for the incoming 7th graders in the district, while older cohorts would continue with the old system. The structure of choice was as follows. At the end of sixth grade each student was asked to rank his preferences among the five schools in his choice set, which consisted of the district's three secondary schools and two out of district schools (in districts 1-5 which were the same schools to which students were bused before the program). The choice set varied among students in accordance with the primary school they attended. In the event of excess demand for a particular school, students were assigned to schools in a manner that maintained a socio-economic balance matching the respective makeup of the city.[3] The city opened information centers and ran workshops to parents and pupils and high schools conducted open days in order to provide additional information for the incoming 7th grade cohort about the choice program.

City reports indicate that in the program's first year, 90 percent of students received their first choice and others their second. In the second year the respective first choice rate was even higher[4], and since 2003 excess demand was resolved by lottery. Another relevant factor was an expansion of the supply of middle school classes as four high schools, two in district 9 and two in the city's northern districts, who had only the higher grades (10th-12th), were expanded at the commencement of the reform to include also the middle school grades. Despite these changes, 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 enrollment). Enrollment 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

[2] These schools were located on the same campus but they were very different in terms of their curriculum of studies and programs offered to students. For example, one included low and high tech vocational schooling.
[3] Siblings in the same school and school capacity were also used as criteria to balance enrollment.
[4] The Tel-Aviv Education Authority (1999).

Tel Aviv. Schools who enjoyed expanded enrollment gained more resources as their budget was determined according to enrollment.

The choice program was accompanied by a decision that the city's post-primary schools would have a six grade structure that includes the middle (7th-9th) and higher grades (10th-12th) as part of the same school. This allowed the city in practice to cancel the admission process at the end of the 9th grade and to introduce the concept of 'persistence' whereby students automatically enrolled into the 10th grade in the same school in which they completed their middle school education. This important component of the reorganization of the school system in Tel Aviv very much limited the ability of schools to select students to their higher grades based on academic performance. The explicit default became that pupils could progress through their secondary education in the same school that they chose in the 7th grade. To prevent any student having this default option, a school had to gain an explicit approval of a special city committee that granted it only in cases of pupils with severe behavioral 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 percent before the choice program to about 15 percent following it.

In 1996 the experiment was expanded to district 8, in 1998 to district 7, and in the following year to the rest of the city (Tel-Aviv Education Authority, 2001).

The Israeli high school system

When entering high school (10^{th} grade), students choose whether to enroll in the academic or non-academic track. Students enrolled in the academic track obtain a matriculation certificate (*Bagrut*) if they pass a series of national exams in core and elective subjects taken between 10^{th} and 12^{th} 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 percent of all high school seniors received a *Bagrut* in the 1999 and 2000 cohorts (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 available in administrative files for the sample of participants in the treatment and control group of the experiment. The students in the sample were in high school between 1999 and 2001, and in 2011 they are adults aged 28-30. 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 its protected research lab. The underlying data sources include: (1) the population registry data, which contain information on personal status, number of children and their birth dates; (2) NII records of post-secondary enrollment from 2000 through 2011 based on annual reports submitted to the NII every fall term by all of Israel's post-secondary education institutions;[5] (3) Israel Tax Authority information on income and earnings of employee and self-employed individuals; (4) NII records on welfare allowances, unemployment benefits and disability compensations; and (5) NII records on physical/mental and work accidents disability.

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.[6] This information comes from administrative records of the Ministry of Education on the universe of Israeli primary schools during the 1992–1994 school years. In addition to 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

[5] The NII which is responsible for the mandatory health insurance tax in Israel tracks post-secondary enrollment because students pay a lower health insurance tax rate. Post-secondary 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 2001–2011 enrollment status of students in our study.

[6] 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. *Bagurt* eligibility is a prerequisite for admission to high education in Israel and the average 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 desired academic programs such as medicine, engineering, and computer science.

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 NII research lab at NII headquarters in Jerusalem.

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), over 50 colleges that confer academic undergraduate degrees (some of these also give master's degrees), and dozens of teachers' colleges that confer bachelor of education.[7] All universities require a *Bagrut* for enrollment. Most academic colleges and teachers' colleges also require a *Bagrut*, though some look at specific *Bagrut* 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 enrollment rates for the cohort of graduating seniors in 1995 (through 2003) was 55.4 percent, of which 27.6 percent were enrolled in universities, 8.5 percent in academic colleges, 7 percent in teachers' colleges, and the rest in non-academic institutions.[8] However, because the treated population is from a low socio-economic background with relatively higher enrollment rates at the lower end of quality of post-secondary schooling, we include more detailed decomposition of these type of schooling institutions such as teachers' colleges, practical engineering schools and other non-academic one or two year colleges.

The post-secondary education outcome variables of interest here are indicators of ever having enrolled in a post-secondary institution of a type described above, as of the 2010–2011 school year, and the number of years of schooling completed in these institutions by this date. We measure these two outcomes for our 1992-1994 7th grade (first grade in middle school) cohorts. The youngest cohort (1994) in our sample is 29-30 years old in 2010-2011. Even after accounting for compulsory military service[9], we expect that most students who enrolled in post-secondary education, including those who

[7] A 1991 reform sharply increased the supply of post-secondary schooling in Israel by creating publicly funded regional and professional colleges.

[8] 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).

[9] 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.

continued schooling beyond undergraduate studies, to have graduated by the 2010–2011 academic year. We therefore present evidence both for enrollment and for completed years of post-secondary education.

Definitions of Outcomes in Adulthood

In this subsection, I describe the outcomes in adulthood for students in the analysis sample:

Labor Market Outcomes:

Earnings: Individual earnings data come from the Israel Tax Authority (ITA). Filing tax forms in Israel is compulsory only for individuals with non-zero self-employment earnings but ITA has information on annual gross earnings from salaried and non-salaried employment and they transfer this information annually to NII, including number of months of work in a given year. Using the data for 2009-2011, I compute for each year the annual and monthly average income (from salaried and self-employment). Individuals with positive non-zero months of work and zero or missing value for earnings are coded as having zero earnings. 14.1% of individuals have 0 wage earnings at age 28-30 in our basic sample of 13,142 individuals and 16.6% have zero earnings in this sample. To account for earnings data outliers I dropped from the sample all observations with monthly wage higher than 30,000 NIS per month. Overall, we dropped 43 observations from the 2009 sample, 74 from the 2010 sample, and 116 from the 2011 sample.

Employment: An indicator with value 1 for individuals with non-zero number months of work in a given year, 0 otherwise.

Eligible for Unemployment Benefit: An indicator with value 1 if received in a given year any amount of unemployment benefits.

Unemployment Benefit Compensation: Annual benefits of unemployment compensation.

Education

Post-secondary Attendance: An indicator for being enrolled for at least one year in any form of post-secondary institutions. Completed post-secondary years of schooling is defined as the number of years of attendance of post-secondary schooling during the period 2000-2011.

University Attendance: An indicator for being enrolled for at least one year in university schooling and completed years of university schooling is the respective number of years of attendance.

College Attendance: An indicator for being enrolled for at least one year in academic (3 years) college and completed years of college schooling is the respective number of years of attendance.

Teachers colleges attendance, Practical engineering and other vocational schooling Attendance and Any other non-academic post-secondary schooling are defined accordingly.

Disability and Welfare

Eligibility for Disability Compensation: An indicator of whether an individual received in any year during 2010-2012 medical or mental disability compensation from NII.

Disability Compensation: Total annual medical or mental disability compensation.

Eligibility for Work Accident Related Disability Compensation: An indicator of whether an individual received in any year during 2010-2012 work accident related disability compensation from NII.

Disability Due to Work Related Accident Compensation: Total annual disability compensation.

4. Identification and Estimation

In previous work (Lavy 2010) I used difference in differences (DID) and regression discontinuity (RD) 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). Three alternative comparison groups were used in the DID estimation and two comparison groups were used in the RD analysis and they yielded almost identical evidence regarding the impact of the choice program.[10] I therefore use the same identification methods in order to estimate the effect of school choice on long term adulthood outcomes. However, I will combine all three comparison groups into one sample in the DID estimation and the two comparison groups in the RD estimation in order to increase efficiency in the estimation. Results based on using each of these comparison groups separately are much in line with the evidence that I present in this paper and are available from the author upon request. I summarize below briefly each of these five comparison groups while more details about each of them is provided in Lavy (2010).

[10] 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).

The first DID comparison group, based on the gradual implementation of the program, includes school districts in Tel Aviv that were enrolled immediately following the two year experiment. Since all the schools in districts 1-5 were included in the choice sets of students in district 9, only districts 6-8 could serve as a comparison group. Districts 6 and 8 are adjacent to district 9 but their sample of students is too small and therefore I consider district 7 as well to be part of the potential comparison group (see Map 1 in online appendix). All these three districts are part of South Tel Aviv, geographically adjacent or near district 9, and their population is much more similar to that of district 9 than to that of Northern Tel Aviv (Lavy 2010).

The second comparison group includes two adjacent Cities located east of Tel-Aviv, both are part of the Dan metropolitan area whose core region includes five major cities, including Tel Aviv. District 9 includes the city's southeastern neighborhoods and is tangent to two of the neighboring 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 the school choice reform of Tel-Aviv.[11] 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 adjacent to Tel-Aviv (south) and it is very close to district 9 and I use it as the third comparison group. It is more similar to district 9 in its characteristics than the GR group.[12]

Therefore, the first identification approach that I apply in this paper is based on a contrast between district 9 and a comparison group that includes districts 6-8, RG and Holon, before and after the program was implemented. I use data on pre- and post-program cohorts (panel data) in a

[11] Givataim, Ramat-Gan and Holon 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.

[12] The fact that two alternative sets of DID estimates, one that is based on a comparison group that has much better characteristics and outcomes (GR or Holon) than the treated group and a second that is based on a comparison group that has marginally worse characteristics and outcomes (districts 6-8), 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.

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 immediately prior cohort that I use as a control group might be affected through spillover effects at the school level. As these students 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. A useful way to check that the results are not biased by such spillover effects is to test if there are significant treatment effects when using two previous cohorts for estimating DID models. Such falsification tests are also useful to test for the effect of omitted time varying factors. I therefore exploit the presence of multiple control groups formed by successive cohorts not exposed to the choice program (the 1992 and 1993 6th grade cohorts) to conduct falsification tests for spillover effects and for spurious treatment effects.[13]

Using Adjacent Neighborhoods as a Comparison Group

I also check the robustness of the evidence by using a regression discontinuity design that limits the sample, in a manner similar to Black (1999), 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 (kind of property) taxes which are determined by the central government. But presumably, there might still be some differences, such as the political affiliation of the mayor for example. The concern remains then is 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 symmetric band around the municipal border, 250 or 500 meters on each side. Contrary to the imbalances between district 9 and GR, this RD sample yield perfectly balanced treatment and control groups. In

[13] See Rosenbaum (1987).

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 ban yields similar results.

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:

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

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 RD sample (using +/-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:

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

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, treatment status. 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.

Descriptive Statistics

Table 1 presents detailed descriptive statistics of the outcome variables in 2011 for the 1992-1994 cohorts, by treatment and control group and by pre- and post-reform cohorts. Post-secondary enrollment rates statistics are presented in panel A. The overall ever enrolled rate in any post-secondary schooling in the treatment group for the post-treatment cohort (1994) is 45.3 percent, of which 15.6 percentage points is in one of the seven universities, 23.7 percentage points is in an academic college, 10.9 percentage points is in a teachers' college, 1.5 percentage points in practical

engineering vocational school and 1.3 percentage points in any other post-secondary schooling.[14][15] Summary statistics on completed years of schooling are presented in panel B. The average number of post-secondary years of schooling completed until the school year 2010-2011 in our treated sample (1994 cohort) is 1.55, of which 0.6 are in university schooling, 0.7 are in college education, 0.2 are in teachers' colleges, 0.02 in practical engineering colleges, and 0.05 in other post-secondary schooling.

Summary statistics for the labor market outcomes and welfare related indicators are presented in panel C of Table 1. Focusing on the treated group in year 2011, we note that 85.2 percent of the individuals in the sample were employed, earning annually about NIS 75,000 ($20,500), 6.5 percent were unemployed during the year. The respective mean earnings for the full cohort are slightly lower, NIS 70,300, and so is the cohort wide mean unemployment, 6.2 percent. The descriptive statistics for 2009-2010 are presented Table A2 in the online appendix and in Tables A3-A4 for the RD sample.

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 by 6.5 percent (30 percent decline) and increased the matriculation rate by 8 percentage points (25 percent improvement). These very large effects were accompanied by improvement in the quality of schooling. Average number of *Bagrut* credits increased by two units relative to pre-program mean of 12 units and the average score in all the *Bagrut* exams was up by 6.5 points, about 10 percent improvement. Other dimensions of quality improvement are the increase in number of *Bagrut* credits in science subjects (half a unit, equivalent to 50 percent improvement) and the increase in *Bagrut* subject studies at honor level (up by a fourth relative to a mean of one such subject). These results are presented for convenience in Appendix Table A5. The estimates based on the RD sample are presented in Table A6.

[14] Note that very few students ever enroll in more than one type of post-school educational institution.
[15] The means for the whole cohort (82,500 students) are 58.5 percent for any post-secondary enrollment, of which 24.0 is in universities, 24.0 in an academic colleges, 13.0 in teachers' colleges or practical-engineering school and the rest in other post-secondary schooling.

In Lavy (2010) I also provided evidence about potential mechanisms of the effect of school choice on the short and medium term academic outcomes. This analysis show that school choice improved the learning and social environment in school. For example, as a result of the program teacher–student relationships and students' social acclimation and satisfaction at school improved and the level of violence, bulling and classroom disruptions declined. The higher satisfaction of students at school is most likely also a result of the better match between students and schools, an improvement facilitated by the school choice program. An indirect evidence of the improved matching is the fact that a large proportion of district 9 students who had the longest travel distance from home to schools in districts 1-4, opted out as well and chose out of district schools. This is evidence of willingness of students to enroll in a in what is from their point of view a better school even at the expense of longer travel time and higher cost. Also shown in Lavy (2010) is that competition among schools intensified following school choice and that it led to improved school quality. For example, two schools that experienced sharp decline in enrollment were closed while others expanded. Improved quality was facilitated by the flexibility that schools enjoyed with respect to curriculum and programs, for example some schools introduced unique new programs, for example enrollment in university courses. However, more conclusive evidence about whether free school choice improves real human capital accumulation and well-being can only be based on longer term effects, in particular on post-secondary enrollment and completed years of schooling, employment, earnings, welfare dependency and other social outcomes, which we study next.

Effect on Post-Secondary Schooling Attainment

Table 2 examines the impact of free school choice on post-secondary schooling attainments. The first row presents DID estimated effect on enrollment in any type of post-secondary schooling (column 2) and on the respective completed years of schooling (column 5). Standard errors appear below each estimate in brackets and are clustered by secondary school. Overall, I find positive and significant impacts of school choice on post-secondary schooling enrollment, an increase of 4.7 percentage points relative to a pre-program mean of 41.7 percent in treated schools and 52.9 percent in the control group. The effect on completed years of schooling (column 5) is 0.229 (SD=0.079). Relative to the pre-choice treatment group mean this is a 15 percent gain.

Naturally it is interesting and important to know what types of post-secondary schooling are affected. Since the treated population is from low socio-economic background with relative low enrollment at the higher quality end of academic institutions, we expect the effect to be low on university schooling and higher on colleges and non-academic post-secondary institutions. In rows 2-6 of Table 2 we present the estimates for five different type of academic post-secondary schooling (university, academic colleges, teachers' colleges) and non-academic post-secondary schooling (vocational schooling and non-academic colleges such as religious seminaries which I denote as 'other'). The effect on university schooling enrollment is practically zero (0.002, SD=0.013) and so is the effect on university years of schooling. The effect on academic colleges' enrollment is however up by 4.3 percent, significantly different from zero (t=2.7), and completed years of this type of schooling increased also, by almost a fifth year (0.186, SD=0.046).[16] Enrollment in teachers' colleges also increased, by 2.7 percent and significantly different from zero, but the respective increase in years of schooling which is positive is imprecisely measured. Enrollment in vocational education (two years colleges that confer practical engineering degrees) declined by 1.5 percentage points and this change, which is significantly different from zero, represents almost a 100 percent decline relative to the treatment group mean before the program started. This change led to fall in years of vocational schooling by 0.025 relative to a pre-program mean of 0.042.

In columns 3 and 6, I present results from a controlled experiment that can be viewed as placebo experiment based on a contrast between the two untreated cohorts of 1992 and 1993. The estimated effects on enrollment (column 3) are mostly positive (except for the effect on university enrollment which is negative) and all estimates are small and are not statistically different from zero. The estimated effects on completed years of schooling (column 6) are all negative but two estimates (of the effect on university schooling and other schooling) which are positive but are small and not statistically different from zero. Noting the placebo effect on any-post secondary enrollment (0.020) and its standard errors (0.018), it can be argued that the respective treatment effect (0.047) is consistent with a linear trend specific to the treatment group. The other evidence presented in Table 2

[16] 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

suggest that this is not the case because this placebo effect is associated with zero increase (-0.006) change in post-high school years of schooling and because the placebo effect on enrollment on academic college is practically zero (0.011) relative to the respective treatment effect (0.043).

The evidence presented above clearly demonstrates that the gain in post-secondary schooling is concentrated in higher academic education with some shift away from non-academic post-secondary schooling. However, we also note that there is no gain in enrollment and years of schooling in university education and the gain is at the low end of academic schooling in Israel. The academic and teachers' colleges are of course less prestigious than universities and their admission requirements are less demanding in terms of *Bagrut* outcomes. This pattern is perhaps expected because the treated population is mostly from a disadvantaged segment of the Israeli population and their enrollment and years of study in university schooling 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 schooling, which can also explain why the treatment effect is concentrated at the lower end of the quality distribution of academic education in Israel.

In order to check the robustness of the evidence presented above, I use the RD design described in the previous section. The RD sample includes observations within a 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 Tables A3 and A4 in the online appendix. It is important to note that in this sample, the treated group is from much higher socio-economic status and it resembles very much the control group. For example, the mean of father's and mother's years of schooling in the RD treated sample in the 1993 cohort is 11.43 and 11.58, respectively, while in the rest of district 9 the respective means are less than 10. A similar pattern is observed for the post-secondary educational outcomes, where in the RD treated sample the mean enrollment in university and academic colleges schooling in the post treatment cohort (1993) is 23.6 and 24.1 percent, respectively, while the respective means in the rest of district 9 are 15.4 and 16.2 percent, respectively. The much higher socio-economic status of treated students in the RD sample in comparison to the rest of district 9 suggests that we should find a higher impact in university

students affected was very small relative to the overall enrollment in academic colleges in the whole country.

schooling than what we estimated based on district's 9 full sample. Indeed this is the case as seen in Table 3 where we report the impact estimates of school choice on post-secondary schooling based on the RD sample. The remarkable difference relative to the evidence presented in Table 2 is the impact on university enrollment (0.033) with no impact on academic college enrollment (0.008), and effect on completed years of university schooling (0.219) while no effect on academic colleges years of schooling (0.047) in comparison to the evidence based on the full sample. Also noticeable in Table 3 is that there is no effect on teachers' colleges relative to the respective estimate in Table 2 which is positive and significant. As we will see below, the gain in university schooling will be rewarded with higher increase in annual earnings relative to the gain in annual earnings experienced by those who improved only academic college education. I think that this pattern of differences in the effect of school choice on post-secondary schooling by socio-economic status of students supports the credibility of the identification strategy and the interpretation of the evidence presented in this paper.

Effect on Employment and Earnings

Table 4 presents evidence on the effect of the school choice program on employment and monthly earnings in 2009, 2010 and 2011 and on the probability of receiving unemployment benefits during the three years period 2010-2012. The average employment rate in the treatment group in 2009, 2010, and 2011 was 85.4, 87.2 and 88.3, respectively. The treatment effect on employment in 2009 and 2010 is negative, suggesting that the treated group had a 3 percent lower employment rate due to the program. In 2011 however this negative effect is reduced to 1.3 percent and it is no longer significantly different from zero. It could be that this negative employment effect reflects a higher rate of being still in post-secondary schooling among treated students. However, the evidence does not support this explanation as the proportion of students in the 1994 cohort who are not yet employed and are still in school is the same in both groups: in the treatment group in 2011 it is 1.4 percent and in the control group it is 1.3 percent. The respective means in the 1992-93 cohorts are 0.7 percent and 0.8 percent.

The proportion of individuals in our sample that were recipients of unemployment benefits is 7.1 percent in 2009, 6.4 percent in 2010 and 5.9 percent in 2011 (second row in Table 4). The national average unemployment rate in 2010 and 2011 for the 25-34 age group was 7.4 and 6.8 percent,

respectively. The estimated treatment effect on the probability of receiving unemployment benefits in these three years is 0.000 (SD=0.006), 0.015 (SD=0.012) and 0.010 (SD=0.083), respectively. Although these estimates are positive, they are all not significantly different from zero. In the third row of Table 4 we also present the effect of the choice program on total unemployment insurance benefits received during the 2009-2011 period. The 1992-1993 cohorts in treated schools mean in 2009 was 670 shekels ($181), in 2010 739 shekels ($200) and in 2011 667 shekels ($180). The estimated treatment effect in 2009 is 36 shekels, in 2010 84 shekels and in 2011 205 shekels. The first two estimates are not statistically different from zero while the 2011 estimate is marginally significant (t=1.64).

We turn next to discuss estimates of the effect of the school choice program on annual earnings at age 28-30. The average annual earnings for the 1992-1993 cohorts in treated schools in 2009 was 65,196 shekels ($17,620 based on 3.7 exchange rate), in 2010 it was somewhat higher, 71,111 shekels ($19,216) and in 2011 even higher, 79,098 shekels ($21,377). The estimated effect of the school choice program on annual earnings is 3,461 shekels ($935) in 2009, 4,509 shekels ($1,218) in 2010 and 5,770 shekels ($1,559) in 2011. These gains are significantly different from zero and reflect a 5 percent increase in 2009, a 6 percent increase in 2010 and a 7 percent increase in 2011, relative to the mean for the 1992-1993 cohorts in treated schools in each of these years. These are sizeable effects and we should expect the earnings gain to increase over time because the earning profile of the more educated is steeper than that of the less educated, especially when the marginal gain is in terms of college schooling.[17]

In Table 5 we present the estimated effect on labor market outcomes based on the RD sample. The effect on earnings has the same pattern as in Table 4 but the estimates are much larger. For example, the annual earnings gain in 2011 due to the school choice program is 10,287 Shekels, 12 percent relative to total annual earnings in that year. The larger gain estimated based on the RD sample reflects to some extent the higher rate of return for university schooling in Israel relative the

[17] We also estimated earnings equation models using a combined 2009-2011 sample where we stacked the data together for these three years. The estimated effect of school choice on earnings from this sample is close to the average of the three year specific effect presented in Table 4. I prefer to present in the paper the year specific

rate of return to academic colleges schooling. This result is consistent with Caplan et al (2009) who report that in many fields of study, academic college graduates in their first jobs are paid on average 20 percent to 30 percent less than university graduates.

Note that if we assume that all the 7 percent average increase in annual earnings in 2011 (based on the DID estimates using the full sample and presented in Table 4) is due to the 0.229 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).[18] However, as shown above, treated students experienced a range of improvements in educational outcomes that are likely to be rewarded in the labor market independently of the return to post-secondary years of schooling. Particularly important is the matriculation rate which increased by the program by 6-7 percent and earns a return of about 13 percent independently of the return to years of schooling.[19] In addition, the quality improvements in the matriculation study program and diploma (for example, the average score, number of credit units and credits in honor and science subjects) could also be rewarded in the labor market beyond the return to years of schooling.[20] The interesting question therefore is could the gains (increases in academic colleges entry and completed years of schooling as well as earnings at adulthood) experienced by students due to the access to free school choice 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 equal role in this regard? It should be noted however that we can address this question but we cannot decompose the effect on earnings of *Bagrut*

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estimates because they show the changes over time in the treatment effect and because the estimated effect on other outcomes are mostly based on data for a specific year.

[18] It should be noted that the sample used in this analysis includes individuals with zero earning and 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 4 did not reveal any effect on employment.

[19] For example, Angrist and Lavy (2009) estimate that *Bagrut* holders earn 13 percent more than other individuals with exactly 12 years of schooling.

[20] Caplan et al (2009) demonstrate that earnings in Israel is 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.

outcomes to the component that operates through its effect on post-secondary schooling and the part that reflects a direct independent effect unrelated to post-secondary schooling.

We first approach this question by estimating 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. These results are presented in Table 6 panel A. For each of the years 2009-2011 we report estimates when only one of the high school outcomes is included in the regressions (columns 2, 5, 8) and also estimates when all outcomes are included jointly (columns 3, 6, 9). When included one at a time, estimates of all outcomes are positive and very precisely measured. When all four are included jointly only some remain significant as expected due to high collinearity among these variables. For example, in the 2011 regression the estimated coefficient of the average score and of the number of science credit units are significant.

I repeated this type of analysis but now added to the regressions the post-secondary enrollment and completed years outcomes, one at a time and also jointly. Here as well each of these outcomes had a positive and significant effect on earnings (Panel B in Table 6, columns 2, 5 and 8) and when included jointly all estimates were still positive but few were not significant (columns 3, 6 and 9).

In columns 4, 7 and 10 of Table 6, I report estimates where all high school and post-secondary education outcomes were included jointly in the regression. Note that all high school outcomes except high school matriculation indicator are positive and significant and the two post-secondary enrollment indicators are positive and significant. The conclusion from these results is that indeed high school and post-secondary schooling outcomes are correlated with earnings.

However, a more firm conclusion about how much of the program effect is due to improvement in these outcomes can be drawn by examining whether the estimated effect of school choice on post-secondary attainment and earnings disappears when we control for *Bagrut* outcomes. This is an informal test of whether school choice affects post-secondary attainment and earnings at adulthood through any channel other than these *Bagrut* outcomes. Of course such evidence could be only suggestive because the high school outcomes are endogenous and are likely correlated with the error term in the long term outcomes regressions. In panel A of Table 7 we 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 each at a time and then all four jointly. We report results based on the full sample and also based on the RD sample. For ease of comparison we present in column 1 the original treatment effect estimates from Tables 2-5. The effect of school choice on enrollment in any post-secondary schooling is 0.047. The inclusion of each of the high-school outcomes as an additional control in the DID regression shrinks to zero the treatment effect estimate except when number of *Bagrut* science credit units are included and the treatment estimate falls only by half (column 5). When all four high school outcomes are included, the treatment effect estimates is practically zero (column 6). A similar pattern is seen in the second row of Table 7, regarding the sensitivity of the treatment effect of school choice on completed years of post-secondary schooling to controlling for each or all of the four high-school achievement outcomes. For example, adding the average matriculation score to the regression changes the school choice effect from 0.229 to 0.008. Again, the treatment estimate is less sensitive to adding the number of science credit units, being lowered in this case by more than a half to 0.106.

The respective RD sample results are aligned with the evidence based on the full sample. These results are presented in rows 3-4 of Table 7 and for the sake of space we will not discuss them in detail here. Clearly we can conclude that the gains in post-secondary education enrollment and completed schooling caused by the school choice program are mediated through the improvement in high school education quality as measured by the *Bagrut* quantity and quality measures.

In panel B of Table 7 I present the evidence with regard to annual earnings, again based on the full and RD samples. Clearly, the treatment effect of school choice falls each time we add one of the high school *Bagrut* achievement measures: by 67 percent when the average matriculation score is added, by 49 percent when the average matriculation score is added, by 69 percent when number of credits is added and by 34 in the case of science credit units. When all four are added, the original treatment estimate, 5,770 shekels, falls to 1,314 shekels, a 77 percent decline and it is no longer significantly different from zero. The evidence from the RD sample is very much in line with this pattern. Again we can clearly claim that the gain in annual earnings caused by the school choice

program is mediated through improvements in high school quality caused by allowing students at the end of primary school to choose their preferred institute for their next six years of schooling.

Effect on Disability Status and Government Disability Transfers

Table 8 presents the estimated effect of school choice on physical-mental disability eligibility rates and on the average disability welfare transfer. The level of such transfer is an increasing function of the disability rate which is determined individually for each claimant by an NII committee that includes a doctor and a social worker. The proportion of individuals in the 1992-93 treated sample who received physical-mental disability welfare allowance in 2011 is 3.1 percent (column 1 in Table 8). This rate among the 1994 treated sample is much lower, 1.8 percent (panel C in Table 1). The mean monthly disability allowance is 880 shekels for the 1992-93 students in treated schools (column 1 in Table 8) and 490 Shekels for the 1994 cohort treated students (panel C in Table 1). I also examine eligibility and disability allowance for disability cases caused by accident at work. The proportion of individuals in the 1992-93 treated sample who received such work related disability welfare allowance in 2011 is 0.5 (column 1 in Table 8). This rate among the 1994 treated sample is again lower, 0/.3 percent (panel C in Table 1). The mean monthly of this type of disability allowance is 118 shekels for the 1992-93 students in treated schools (column 1 in Table 8) and 33 Shekels for the 1994 cohort treated students (panel C in Table 1).

In columns 2, 5, and 7 of Table 8 we report the DID estimates of the effect of school choice on each of these four outcomes. Focusing on results for 2011, it is evident that school choice reduced the proportion of disable individuals who are eligible for disability allowance by 2.4 percent and the mean disability allowance by 637 shekels. These treatment effects are precisely measured, being statistically different from zero. Note that all the controlled (placebo) experiment estimates (column 3) are very small and not statistically different from zero. Results for 2009 and 2010 are very similar to those of 2011 and follow the same pattern. The estimated effect of school choice on disability eligibility and allowance due to work accidents are also negative but small and imprecisely measured.

It is interesting to note that in the RD sample, the estimated effect on these four outcomes are much smaller than those based on the full sample of district 9, and most of them are not statistically different from zero. These results are presented in online appendix Table A6. Note that as expected

given the much higher socio-economic status of this sample, the means of all these outcomes presented in columns 1, 3, 5 of Table A6 are much lower as well relative to the respective means from the full sample.

Several potential mechanisms can account for the effect of the school choice program on disability status and eligibility for related welfare allowances. First, gaining disability status is often related to manipulative behavior of individuals Vis a Vis NII staff. For example, individuals who want to avoid the military draft may opt to gain NII disability status ('back' problems is a well-known excuse for this purpose). If school choice increased the draft (by increasing schooling quality for example), it could indirectly have reduced the motivation to gain disability status. A more direct channel is through the effect of school choice on education and on the opportunity cost of time. Eligibility to NII disability allowance involves restriction on employment and earnings and the school choice makes these restrictions more costly to individuals, lowering the financial gain from disability status. Better education may also reduce the likelihood of attempting to game the NII system and achieve erroneously disability status and welfare allowances.

The above estimated effects on disability related outcomes are large, but they are in line with evidence of other related studies. Evidence from the US suggests large effects of economic opportunities on disability program-enrollment. For example, Black, Kermit, and Sanders (2002) examined the impact of the coal boom of the 1970's and the coal bust of the 1980's on disability program participation and report evidence that suggest that as the value of labor-market participation increases, disability program participation falls. The elasticity of payments with respect to local earnings is between –0.3 and –0.7 depending on the disability program and it is higher for permanent than for transitory economic shocks.

Effect on marriage and children

I found no evidence that school choice had beneficial impacts on marriage and fertility outcomes. I examined the effect of choice on marital status in 2011, having children by 2011, and number of children in 2011. All these estimates were very small, close to zero, and not statistically significant. I obtained similar results when using the RD simple. These results are not presented here and are available from the author.

6. 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 of interest to society is the impact of such interventions on long-run life outcomes. 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 achieving better 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 and students' educational attainment. The empirical evidence from this study contributes to the building of 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 have many similar features with related 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 other educational settings throughout the developed countries of the world.

The school choice program studied here had positive longer term outcomes at adulthood. The evidence clearly suggests that allowing children to choose freely at age 13 which secondary school they wish to attend, not only improved sharply their high school outcomes six years later, but it also impacted positively their path to post-secondary schooling, increased meaningfully their earnings about a decade and a half later and reduced their dependency on the public welfare system. 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 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 became available, other school choice programs were introduced in the country, for example in Jerusalem in 2006 and more recently in many cities in Israel.[21]

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[21] The Ministries of Education and of Finance in Israel introduced recently large school choice program that include about 15 of 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. I this regard it is important to note that the creation of academic colleges that started in the mid 1990's have 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 - Post-Secondary, Employment and Earnings in 2011

	Treated Schools		Control Schools	
High School Cohort	1992-1993	1994	1992-1993	1994
Dependent variable	(1)	(2)	(3)	(4)
A. Enrollment in Post High School Education in 2011				
Any post secondary education (1 = Yes, 0 = No)	0.417	0.453	0.527	0.531
	(0.493)	(0.498)	(0.499)	(0.499)
University (1 = Yes, 0 = No)	0.177	0.156	0.238	0.227
	(0.382)	(0.363)	(0.426)	(0.419)
Academic College (1 = Yes, 0 = No)	0.192	0.237	0.263	0.266
	(0.394)	(0.426)	(0.440)	(0.442)
Teachers' College (1 = Yes, 0 = No)	0.087	0.109	0.062	0.056
	(0.283)	(0.312)	(0.241)	(0.230)
Practical Engineering School (1 = Yes, 0 = No)	0.024	0.015	0.033	0.043
	(0.152)	(0.123)	(0.178)	(0.203)
Other (1 = Yes, 0 = No)	0.015	0.013	0.020	0.019
	(0.120)	(0.112)	(0.139)	(0.137)
B. Post High School Years of Schooling in 2011				
Any post secondary education	1.562	1.555	2.126	1.960
	(2.243)	(2.096)	(2.444)	(2.232)
University	0.696	0.609	0.990	0.911
	(1.728)	(1.581)	(2.026)	(1.879)
Academic College	0.529	0.678	0.812	0.773
	(1.273)	(1.427)	(1.569)	(1.477)
Teachers' College	0.214	0.202	0.172	0.131
	(0.842)	(0.685)	(0.778)	(0.646)
Practical Engineering School	0.042	0.019	0.055	0.064
	(0.309)	(0.162)	(0.345)	(0.328)
Other	0.081	0.047	0.098	0.081
	(0.447)	(0.352)	(0.556)	(0.488)
C. Employment Outcomes in 2011				
Employed (1 = Yes, 0 = No)	0.861	0.852	0.856	0.855
	(0.346)	(0.356)	(0.351)	(0.352)
Average Annual Earnings (NIS)	79,098	74,952	85,709	76,918
	(66,015)	(62,569)	(72,919)	(66,622)
Received Unemployment Insurance Benefits (1 = Yes, 0 = No)	0.059	0.065	0.069	0.066
	(0.235)	(0.246)	(0.254)	(0.248)
Total Unemployment Insurance Benefits Received (NIS)	667	775	810	728
	(3,238)	(3,557)	(3,607)	(3,282)
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.031	0.018	0.019	0.030
	(0.172)	(0.132)	(0.136)	(0.169)
Total Disability Insurance Benefits Received in (NIS)	880	490	546	746
	(5,419)	(4,216)	(4,333)	(4,502)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.005	0.003	0.003	0.001
	(0.071)	(0.050)	(0.056)	(0.037)
Total Work Disability Insurance Benefits Received (NIS)	118	33	77	42
	(2,222)	(665)	(2,240)	(1,494)
Number of Observations	1,567	788	8,070	4,393

Notes: The table reports means and standard deviations for different post-secondary education and employment variables for the year of 2011. 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 2011 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 2011 in each type of the post-secondary institutions listed in panel A. Panel C reports the mean of an employment indicator, annual earnings and disability related variables in 2011.

Table 2: Differences-in-Differences Estimates of the Effect of School Choice on Post-Secondary Schooling

	Enrollment in Post High School Education			Post High School Years of Schooling		
	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment
	(1)	(2)	(3)	(4)	(5)	(6)
Any Post-Secondary education	0.417	0.047	0.020	1.562	0.229	-0.006
	(0.493)	(0.021)	(0.018)	(2.243)	(0.079)	(0.090)
University	0.177	0.002	-0.021	0.696	0.056	0.016
	(0.382)	(0.013)	(0.010)	(1.728)	(0.055)	(0.047)
Academic College	0.192	0.043	0.011	0.529	0.186	-0.005
	(0.394)	(0.016)	(0.018)	(1.273)	(0.046)	(0.058)
Teachers' College	0.087	0.027	0.005	0.214	0.022	-0.023
	(0.283)	(0.013)	(0.011)	(0.842)	(0.030)	(0.031)
Practical Engineering School	0.024	-0.015	0.004	0.042	-0.025	-0.000
	(0.152)	(0.006)	(0.005)	(0.309)	(0.011)	(0.013)
Other	0.015	-0.000	0.003	0.081	-0.010	0.007
	(0.120)	(0.005)	(0.005)	(0.447)	(0.015)	(0.020)
Number of Observations	1,567	14,818	9,637	1,567	14,818	9,637

Notes: This table presents the differences-in-differences estimates of the effect of the School Choice program on post-secondary schooling. Columns 1-3 measure enrollment into different types of post-secondary institutions, while columns 4-6 measure completed years of post-secondary education by institution type. The results are for the year of 2011. The variable "Any Post-Secondary Education" refers to all different post-secondary institutions. Columns 1 and 4 represent the mean and standard deviation for the 1992-1993 (untreated) cohorts in the treated schools. Columns 2 and 5 report the differences-in-differences estimates for each of the dependent variables. Columns 3 and 6 report the differences-in-differences estimates of a controlled experiment where the 1992-1993 cohorts are intentionally altered in the regression to receive treatment. Standard errors are clustered at the school level.

Table 3: Regression Discontinuity Sample: Differences-in-Differences Estimates of the Effect of School Choice on Post Secondary Schooling

	Enrollment in Post High School Education			Post High School Years of Schooling		
	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment
	(1)	(2)	(3)	(4)	(5)	(6)
Any Post secondary education	0.510	0.035	0.048	1.934	0.239	0.121
	(0.500)	(0.035)	(0.047)	(2.344)	(0.136)	(0.177)
University	0.236	0.033	-0.027	0.921	0.219	-0.023
	(0.425)	(0.031)	(0.036)	(1.913)	(0.138)	(0.149)
Academic College	0.241	0.008	0.104	0.675	0.047	0.206
	(0.428)	(0.031)	(0.038)	(1.403)	(0.104)	(0.132)
Teachers' College	0.105	0.006	-0.022	0.262	-0.049	-0.133
	(0.306)	(0.018)	(0.019)	(0.890)	(0.049)	(0.065)
Semiengineering School	0.009	-0.005	0.036	0.014	-0.018	0.068
	(0.093)	(0.013)	(0.008)	(0.156)	(0.022)	(0.017)
Other	0.019	0.008	-0.018	0.061	0.039	0.004
	(0.137)	(0.011)	(0.017)	(0.390)	(0.037)	(0.051)
Number of Observations	573	2,341	1,469	573	2,341	1,469

Notes: This table presents the differences-in-differences estimates of the effect of the School Choice program on post-secondary schooling for the regression discontinuity sample described in the paper. Columns 1-3 measure enrollment into different types of post-secondary institutions, while columns 4-6 measure completed years of post-secondary education by institution type. The results are for the year of 2011. The variable "Any Post-Secondary Education" refers to all different post-secondary institutions. Columns 1 and 4 represent the mean and standard deviation for the 1992-1993 (untreated) cohorts in the treated schools. Columns 2 and 5 report the differences-in-differences estimates for each of the dependent variables. Columns 3 and 6 report the differences-in-differences estimates of a controlled experiment where the 1992-1993 cohorts are intentionally altered in the regression to receive treatment. Standard errors are clustered at the school level.

Table 4: Differences-in-Differences Estimates of The Effect of School Choice on Employment and Income

	2011 Outcomes			2010 Outcomes			2009 Outcomes		
	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Employment Indicator (1 = Yes, 0 = No)	0.861	-0.013	-0.009	0.865	-0.035	-0.030	0.869	-0.031	-0.012
	(0.346)	(0.014)	(0.011)	(0.342)	(0.014)	(0.013)	(0.338)	(0.013)	(0.013)
Received Unemployment Insurance Benefits Indicator (1 = Yes, 0 = No)	0.059	0.0100	-0.022	0.064	0.015	0.0002	0.071	0.000	0.010
	(0.235)	(0.008)	(0.009)	(0.245)	(0.012)	(0.014)	(0.258)	(0.006)	(0.007)
Total Annual Earnings (NIS)	79,098	5,770	2,243	71,111	4,509	1,981	65,196	3,461	1,274
	(66,016)	(2,322)	(2,222)	(60,027)	(2,262)	(2,737)	(55,281)	(2,281)	(3,082)
Total Unemployment Insurance Benefits Received (NIS)	667	205	-362	739	84	2	670	36	92
	(3,238)	(125)	(113)	(3,383)	(143)	(183)	(2,859)	(68)	(86)
Number of Observations	1,567	14,818	9,637	1,567	14,818	9,637	1,567	14,818	9,637

Notes : This table presents the differences-in-differences estimates of the effect of the School Choice program on different employment and earnings outcomes. Columns 1-3 report results for 2011, columns 4-6 report results for 2010, and columns 7-9 report results for 2009. The variable "Employment Indicator" equals 1 if an individual has any work record for the given year and 0 otherwise. The variable "Received Unemployment Insurance Benefits Indicator" equals 1 if an individual has any record indicating that he received any amount of unemployment benefits in the given year, and 0 otherwise. The variable "Total Unemployment Insurance Benefits Received" describes the total NIS amount of unemployment benefits the individual received in the given year. Total Annual Earnings measure the total NIS amount of earnings the individual received in the given year. Columns 1,4, and 7 report the mean and standard deviation for the 1992-1993 (untreated) cohorts in the treated schools. Columns 2,5, and 8 report the differences-in-differences estimates for each of the dependent variables listed above. Columns 3,6, and 9 report the differences-in-differences estimates of a controlled experiment where the 1992-1993 cohorts are intentionally altered in the regression to receive treatment. Standard errors are clustered at the school level.

Table 5: Regression Discontinuity Sample: Differences-in-Differences Estimates of the Effect of School Choice on Employment and Income

	2011 Outcomes			2010 Outcomes			2009 Outcomes		
	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Employment Indicator (1 = Yes, 0 = No)	0.880	-0.048	0.043	0.873	-0.057	-0.020	0.883	-0.042	0.006
	(0.326)	(0.027)	(0.027)	(0.334)	(0.027)	(0.032)	(0.322)	(0.033)	(0.035)
Received Unemployment Insurance Benefits Indicator (1 = Yes, 0 = No)	0.065	-0.0246	-0.036	0.066	0.009	0.0315	0.070	0.012	0.037
	(0.246)	(0.017)	(0.015)	(0.249)	(0.018)	(0.022)	(0.255)	(0.016)	(0.020)
Total Annual Earnings (NIS)	88,040	10,287	262	77,669	8,101	653	70,658	3,829	-3,439
	(70,114)	(4,407)	(5,662)	(64,423)	(4,411)	(6,081)	(58,589)	(4,626)	(5,979)
Total Unemployment Insurance Benefits Received (NIS)	802	101	-818	723	200	348	714	131	155
	(3,642)	(235)	(180)	(3,275)	(209)	(282)	(3,122)	(198)	(268)
Number of Observations	573	2,341	1,469	573	2,341	1,469	573	2,341	1,469

Notes : This table presents the differences-in-differences estimates of the effect of the School Choice program on different employment and income outcomes for the regression discontinuity sample described in the paper.Columns 1-3 report results for 2011, columns 4-6 report results for 2010, and columns 7-9 report results for 2009. The variable "Employment Indicator" equals 1 if an individual has any work record for the given year, and 0 otherwise. The variable "Received Unemployment Insurance Benefits Indicator" equals 1 if an individual has any record indicating that he received any amount of unemployment benefits in the given year, and 0 otherwise. The variable "Total Unemployment Insurance Benefits Received" describes the total NIS amount of unemployment benefits the individual received in the given year. Total Annual Earnings measures the total NIS amount of earnings the individual received in the given year. Columns 1,4, and 7 report the mean and standard deviations for the 1992-1993 (untreated) cohorts in the treated schools. Columns 2,5, and 8 report the differences-in-differences estimates for each of the dependent variables listed above. Columns 3,6, and 9 report the differences-in-differences estimates of a controlled experiment where the 1992-1993 cohorts are intentionally altered in the regression to receive treatment. Standard errors are clustered at the school level.

Table 6: OLS Relationships between High School Matriculation Outcomes, College Schooling, and Earnings at Adulthood

		2011 Annual Earnings			2010 Annual Earnings			2009 Annual Earnings		
	1992-1993 Cohorts in Treated Schools	Separate Estimate	Joint Estimate Panel A	Joint Estimate Panels A + B	Separate Estimate	Joint Estimate Panel A	Joint Estimate Panels A + B	Separate Estimate	Joint Estimate Panel A	Joint Estimate Panels A + B
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. High School Matriculation Outcomes										
Average Matriculation Score	60.158	576	164	115	468	3,761	105	382	113	93
	(35.511)	(22)	(31)	(30)	(18)	(1443)	(28)	(16)	(25)	(25)
Received High School Matriculation (1 = Yes, 0 = No)	0.444	34,043	5,551	611	27,169	3,761	627	21,727	2,292	239
	(0.497)	(1,360)	(1,668)	(1,723)	(1,188)	(1,443)	(1,471)	(1,066)	(1,302)	(1,328)
Number of Credit Units in Matriculation Exams	15.858	1,833	829	753	1,495	739	693	1,219	627	599
	(10.937)	(62)	(100)	(101)	(52)	(95)	(98)	(48)	(85)	(87)
Number of Science Credit Units	1.552	4,411	2,425	1,906	3,396	1,738	1,482	2,716	1,366	1,233
	(3.349)	(188)	(193)	(205)	(177)	(182)	(187)	(163)	(168)	(180)
B. Post Secondary Schooling										
Enrollment in Any Post Secondary Schooling (1 = Yes, 0 = No)	0.417	35,140	13,035	5,287	26,078	10,468	3,444	20,054	9,945	3,958
	(0.493)	(1,300)	(2,941)	(2,878)	(1,162)	(2,712)	(2,702)	(1,084)	(2,521)	(2,543)
Completed Years of Any Post Secondary Schhooling	1.562	7,336	563	-187	5,202	-305	-967	3,867	-969	-1,545
	(2.243)	(294)	(810)	(807)	(263)	(731)	(737)	(236)	(744)	(739)
Enrollment in University or Academic College (1 = Yes, 0 = No)	0.334	37,192	14,678	13,028	27,999	13,315	11,722	21,577	9,643	8,281
	(0.472)	(1,313)	(3,076)	(2,968)	(1,191)	(2,839)	(2,785)	(1,080)	(2,730)	(2,690)
Completed Years of University or Academic College	1.225	7,721	2,552	682	5,560	1,846	334	4,205	1,857	591
	(2.064)	(307)	(841)	(842)	(282)	(783)	(778)	(242)	(796)	(792)
Number of Observations	1,567	14,688	14,688	14,688	14,725	14,725	14,725	14,752	14,752	14,752

Notes : This table presents OLS relationships between high school matriculation outcomes, college schooling, and earnings at adulthood. Column 1 reports means and standard deviations for the 1992-1993 (untreated) cohorts in treated schools. Columns 2,5, and 8 represent the OLS estimates of a regression where the dependent variable is the annual wage for year 2009-2011 and the independent variables include the same control variables as in the main model presented in tables 2-5, in addition to the outcome variable described in the table. Columns 3,6, and 9 report the OLS estimate when all the variables that appear in Panel A\B are controlled for in the wage regression in addition to the rest of the control variables of the model. Columns 4,7, and 10 report the OLS estimate from a wage regression where all the explanatory variables in the table are controlled simultaneously. Standard errors are clustered at the school level.

Table 7: Estimated Treatment Effect of the School Choice Program when Adding High School Educational Outcomes to the DID Regression

	Added Control Variables					
	Original Estimate/ No Added Variables	Average Matriculation Score	Received High School Matriculation	Number of Credit Units in Matriculation Exams	Number of Science Credit Units	All High School Outcome Variables
	(1)	(2)	(3)	(4)	(5)	(6)
A. Post-Secondary Educational Outcomes						
Full Sample (N = 14,818)						
Enrollment in Any Post Secondary Schooling (1 = Yes, 0 = No)	0.047	-0.003	0.006	-0.003	0.026	-0.010
	(0.021)	(0.017)	(0.019)	(0.018)	(0.020)	(0.017)
Completed Years of Any Post Secondary Schhooling	0.229	0.008	0.044	0.012	0.106	-0.027
	(0.079)	(0.064)	(0.078)	(0.069)	(0.071)	(0.065)
Regression Discontinuity Sample (N = 2,341)						
Enrollment in Any Post Secondary Schooling (1 = Yes, 0 = No)	0.035	-0.032	-0.012	-0.022	0.004	-0.036
	(0.035)	(0.031)	(0.035)	(0.029)	(0.033)	(0.031)
Completed Years of Any Post Secondary Schhooling	0.239	-0.063	0.030	-0.014	0.052	-0.100
	(0.136)	(0.122)	(0.146)	(0.115)	(0.128)	(0.127)
B. Earnings at Adulthood						
Full Sample (N = 14,688)						
Total Annual Earnings in 2011 (NIS)	5,770	1,894	2,950	1,801	3,782	1,314
	(2,322)	(2,398)	(2,281)	(2,392)	(2,252)	(2,309)
Regression Discontinuity Sample (N = 2,314)						
Total Annual Earnings in 2011 (NIS)	10,287	5,730	7,392	6,316	7,678	5,220
	(4,407)	(4,240)	(4,189)	(4,062)	(4,269)	(4,108)

Notes : This table assesses the sensitivity of the treatment effects presented in tables 2-5 when adding high school educational outcomes as control in the DID regressions. Column 1 reports the estimated treatment effects from tables 2-5 for each specific sample and dependent variable. Columns 2-5 present the estimated treatement effects when the high school educational outcome variable mentioned in the column header is added to the DID regression estimated in table 2 or 3. Column 6 represents the estimated treatement effect when all the four high school educational outcomes are added together to the DID regression estimated in table 2 or 3. Standard errors are clustered at the school level.

Table 8: Differences-in-Differences Estimates of the Effect of School Choice on Disability Rate

	2011 Outcomes			2010 Outcomes			2009 Outcomes		
	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment
	(1)	(2)	(3)	(4)	(5)	(6)	(6)	(7)	(8)
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.031	-0.024	-0.002	0.027	-0.021	-0.002	0.024	-0.026	0.001
	(0.172)	(0.007)	(0.007)	(0.163)	(0.007)	(0.007)	(0.152)	(0.006)	(0.006)
Total Disability Insurance Benefits Received (NIS)	880	-637	-29	787	-592	9	652	-608	23
	(5,419)	(181)	(222)	(4,977)	(185)	(208)	(4,464)	(160)	(182)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.005	-0.001	0.001	0.005	-0.002	-0.001	0.004	-0.002	-0.000
	(0.071)	(0.003)	(0.002)	(0.071)	(0.003)	(0.002)	(0.067)	(0.003)	(0.002)
Total Work Disability Insurance Benefits Received (NIS)	118	-64	76	96	-46	14	84	-47	32
	(2,222)	(69)	(77)	(2,073)	(61)	(75)	(2,061)	(59)	(75)
Number of Observations	1,567	14,818	9,637	1,567	14,818	9,637	1,567	14,818	9,637

Notes : This table presents the differences-in-differences estimates of the effect of the School Choice program on different disability rate outcomes. Columns 1-3 report results for 2011, columns 4-6 report results for 2010, and columns 6-8 report results for 2009. The variable "Received Disability Insurance Benefits" receives the value of 1 is the individual received any amount of diability benefits from the National Insurance Institution of Insrael (NII), 0 otherwise. The variable "Total Disability Insurance Benefits Received" describes the total NIS amount of disability insurance benefits the individual received in a given year. The variable "Received Work Disability Insurance Benefits" receives the value of 1 is the individual received any amount of work diability benefits from the National Insurance Institution of Insrael (NII), 0 otherwise. The variable "Total Work Disability Insurance Benefits Received" describes the total NIS amount of work disability insurance benefits the individual received in a given year. Columns 1,4, and 7 report the mean and standard deviation for the 1992-1993 (untreated) cohorts in the treated schools. Columns 2,5, and 8 report the differences-in-differences estimates for each of the dependent variables listed above. Columns 3,6, and 9 report the differences-in-differences estimates of a controlled experiment where the 1992-1993 cohorts are intentionally altered in the regression to receive treatment. Standard errors are clustered at the school level.

Table A1: Student Mean Characteristics and Outcomes by Location: District 9, Tel Aviv, Districts 6-8, Givataim and Ramat-Gan, and Holon, 1994

	Sample					Mean Equality T-stats			
	Tel Aviv Districts			Givataim & Ramat Gan	Holon	t-value: (2) vs. (1)	t-value: (2) vs. (3)	t-value: (2) vs. (4)	t-value: (2) vs. (5)
	All	9	6-8						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Students' characteristics:									
Fathers' years of schooling	12.049	10.336	9.755	12.097	11.635	-3.070	-1.173	-4.704	-3.554
Mothers' years of schooling	12.127	10.566	10.128	12.364	11.880	-3.058	-0.802	-4.660	-3.521
Number of siblings	1.948	2.145	2.278	1.910	2.121	1.627	0.425	2.286	0.227
Gender (male=1)	0.490	0.475	0.538	0.526	0.495	-0.855	2.364	-2.212	-0.787
Immigration status	0.054	0.052	0.090	0.077	0.083	-0.209	1.253	-1.580	-1.633
Country of origin - Israel	0.630	0.659	0.530	0.609	0.542	1.253	-3.270	1.711	3.491
Country of origin - Asia/Africa	0.157	0.196	0.219	0.194	0.231	2.140	0.823	0.073	-1.632
Country of origin - Europe/America	0.096	0.047	0.042	0.103	0.084	-3.580	-0.329	-3.861	-2.489
Number of students	2,809	791	576	1,842	2,426	3,600	1,367	2,633	3,217

Notes: The samples include only students in secular state schools. Religious Jewish schools and Arab schools are excluded.

Table A2: Descriptive Statistics for 2009 and 2010

	Treated Schools		Control Schools	
High School Cohort	1992-1993	1994	1992-1993	1994
Dependent variable	(1)	(2)	(3)	(4)
A. Employment Outcomes in 2010				
Employed (1 = Yes, 0 = No)	0.865	0.840	0.841	0.846
	(0.342)	(0.367)	(0.366)	(0.361)
Total Annual Earnings (NIS)	71,111	65,098	75,363	65,751
	(60,027)	(58,371)	(66,350)	(59,088)
Received Unemployment Insurance Benefits (1 = Yes, 0 = No)	0.064	0.076	0.062	0.056
	(0.245)	(0.265)	(0.241)	(0.231)
Total Unemployment Insurance Benefits Received (NIS)	739	666	740	562
	(3,383)	(3,000)	(3,593)	(2,805)
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.027	0.018	0.018	0.028
	(0.163)	(0.132)	(0.132)	(0.166)
Total Disability Insurance Benefits Received (NIS)	787	432	494	685
	(4,977)	(3,673)	(3,978)	(4,220)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.005	0.001	0.003	0.001
	(0.071)	(0.036)	(0.057)	(0.037)
Total Work Disability Insurance Benefits Received (NIS)	96	19	68	27
	(2,073)	(531)	(2,085)	(1,226)
Number of Observations	1,567	788	8,070	4,393
B. Employment Outcomes in 2009				
Employed (1 = Yes, 0 = No)	0.869	0.841	0.846	0.844
	(0.338)	(0.366)	(0.361)	(0.363)
Total Annual Earnings (NIS)	65,196	56,451	68,861	57,002
	(55,281)	(51,367)	(60,291)	(51,304)
Received Unemployment Insurance Benefits (1 = Yes, 0 = No)	0.071	0.063	0.071	0.061
	(0.258)	(0.244)	(0.257)	(0.239)
Total Unemployment Insurance Benefits Received (NIS)	670	391	704	373
	(2,859)	(1,652)	(3,111)	(1,722)
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.024	0.009	0.016	0.025
	(0.152)	(0.094)	(0.126)	(0.156)
Total Disability Insurance Benefits Received (NIS)	652	291	426	614
	(4,464)	(3,270)	(3,578)	(3,960)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.004	0.003	0.002	0.002
	(0.067)	(0.050)	(0.047)	(0.043)
Total Work Disability Insurance Benefits Received (NIS)	84	20	52	29
	(2,061)	(514)	(1,550)	(984)
Number of Observations	1,567	788	8,070	4,393

Notes: The table reports means and standard deviations for different employment and income variables for the years 2009-2010. Each column represents these statistics for a different group as described in each column's headline. Panel A represents 2010 outcomes, while panel B represents 2009 outcomes. For more information regarding the variables, see table 1.

Table A3: Regression Discontinuity Sample: Descriptive Statistics, Post-Secondary, Employment and Earnings in 2011

	Treated Schools		Control Schools	
High School Cohort	1992-1993	1994	1992-1993	1994
Dependent variable	(1)	(2)	(3)	(4)
A. Enrollment in Post High School Education				
Any post secondary education	0.510	0.512	0.609	0.600
	(0.500)	(0.501)	(0.488)	(0.490)
University	0.236	0.209	0.300	0.271
	(0.425)	(0.407)	(0.459)	(0.445)
Academic College	0.241	0.276	0.294	0.324
	(0.428)	(0.448)	(0.456)	(0.468)
Teachers' College	0.105	0.096	0.073	0.057
	(0.306)	(0.295)	(0.260)	(0.232)
Semiengineering School	0.009	0.017	0.021	0.034
	(0.093)	(0.131)	(0.144)	(0.182)
Other	0.019	0.009	0.029	0.009
	(0.137)	(0.093)	(0.168)	(0.097)
B. Post High School Years of Schooling				
Any post secondary education	1.934	1.820	2.551	2.362
	(2.344)	(2.166)	(2.512)	(2.355)
University	0.921	0.799	1.250	1.076
	(1.913)	(1.746)	(2.206)	(1.978)
Academic College	0.675	0.817	0.903	1.008
	(1.403)	(1.509)	(1.617)	(1.669)
Teachers' College	0.262	0.157	0.203	0.133
	(0.890)	(0.565)	(0.841)	(0.662)
Semiengineering School	0.014	0.020	0.039	0.063
	(0.156)	(0.161)	(0.287)	(0.362)
Other	0.061	0.026	0.156	0.083
	(0.390)	(0.269)	(0.729)	(0.538)
C. Employment Outcomes in 2011				
Employed (1 = Yes, 0 = No)	0.880	0.858	0.853	0.879
	(0.326)	(0.350)	(0.355)	(0.327)
Total Annual Earnings (NIS)	88,040	83,382	89,896	77,889
	(70,114)	(70,639)	(77,487)	(62,803)
Received Unemployment Insurance Benefits (1 = Yes, 0 = No)	0.065	0.061	0.061	0.080
	(0.246)	(0.240)	(0.240)	(0.271)
Total Unemployment Insurance Benefits Received (NIS)	802	888	849	818
	(3,642)	(3,870)	(3,828)	(3,350)
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.014	0.020	0.019	0.023
	(0.117)	(0.141)	(0.137)	(0.149)
Total Disability Insurance Benefits Received (NIS)	390	594	509	563
	(3,724)	(4,927)	(3,948)	(3,807)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.006	0.005	0.002	0.000
	(0.080)	(0.073)	(0.048)	(0.000)
Total Work Disability Insurance Benefits Received (NIS)	125	80	93	0
	(1,567)	(1,105)	(1,932)	(00)
Number of Observations	573	344	896	528

Notes: The table reports means and standard deviations for different post-secondary education and employment variables for the year of 2011. Each column represents these statistics for a different group as described in each column's headline. Unlike table 1, the sample of this table is restricted to the regression discontinuity sample mentioned in the text. Panel A is comprised of binary variables indicating whether the individual is enrolled or not to 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 2011 in each type of the post-secondary institutions described in panel A. Panel C reports different employment and income variables for the individual in the year 2011.

Table A4: Regression Discontinuity Sample: Descriptive Statistics for 2009 and 2010

	Treated Schools		Control Schools	
High School Cohort	1992-1993	1994	1992-1993	1994
Dependent variable	(1)	(2)	(3)	(4)
A. Employment Outcomes in 2010				
Employed (1 = Yes, 0 = No)	0.873	0.858	0.833	0.872
	(0.334)	(0.350)	(0.373)	(0.334)
Total Annual Earnings (NIS)	77,669	70,078	79,631	65,875
	(64,423)	(63,769)	(70,337)	(56,375)
Received Unemployment Insurance Benefits (1 = Yes, 0 = No)	0.066	0.087	0.045	0.061
	(0.249)	(0.283)	(0.207)	(0.239)
Total Unemployment Insurance Benefits Received (NIS)	723	761	658	564
	(3,275)	(3,203)	(3,668)	(2,954)
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.010	0.026	0.018	0.017
	(0.102)	(0.160)	(0.133)	(0.130)
Total Disability Insurance Benefits Received (NIS)	315	716	439	459
	(3,445)	(4,969)	(3,472)	(3,522)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.006	0.005	0.002	0.000
	(0.080)	(0.073)	(0.048)	(0.000)
Total Work Disability Insurance Benefits Received (NIS)	57	78	58	0
	(984)	(1,080)	(1,205)	(00)
Number of Observations	1,567	788	8,070	4,393
B. Employment Outcomes in 2009				
Employed (1 = Yes, 0 = No)	0.883	0.854	0.849	0.860
	(0.322)	(0.354)	(0.358)	(0.347)
Total Annual Earnings (NIS)	70,658	60,936	69,156	56,501
	(58,589)	(56,754)	(61,391)	(50,173)
Received Unemployment Insurance Benefits (1 = Yes, 0 = No)	0.070	0.076	0.061	0.055
	(0.255)	(0.265)	(0.240)	(0.228)
Total Unemployment Insurance Benefits Received (NIS)	714	479	708	385
	(3,122)	(1,861)	(3,180)	(1,868)
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.009	0.017	0.017	0.017
	(0.093)	(0.131)	(0.128)	(0.130)
Total Disability Insurance Benefits Received (NIS)	271	582	417	442
	(3,225)	(4,679)	(3,348)	(3,394)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.006	0.005	0.002	0.000
	(0.080)	(0.073)	(0.048)	(0.000)
Total Work Disability Insurance Benefits Received (NIS)	26	75	65	0
	(414)	(1,040)	(1,355)	(00)
Number of Observations	573	344	896	528

Notes: The table reports means and standard deviations for different employment and income variables for the years 2009-2010. Each column represents these statistics for a different group as described in each column's headline. Unlike table A3, the sample of this table is restricted to the regression discontinuity sample mentioned in the text. Panel A represents 2010 outcomes, while panel B represents 2009 outcomes. For more information regarding the variables, see table 1.

Table A5: Simple, Controlled and Difference-in-Differences Estimated Effects of School Choice: Tel Aviv District 9 vs.Tel Aviv Districts 6 - 8, Givataim, Ramat-Gan, and Holon. Estimated for Full Sample

	Tel Aviv District 9 vs. Tel Aviv Districts 6 - 8, Givataim, Ramat-Gan, and Holon				
				DID	
	1992	1993	1994	True	False
				92/3 vs. 94	92 vs. 93
Dependent variable	(1)	(2)	(3)	(4)	(5)
Drop out					
Control group mean	0.128	0.123	0.133		
Treatment-control	0.052	0.070	-0.006	-0.073	0.013
difference	(0.028)	(0.024)	(0.023)	(0.017)	(0.021)
Treatment-control	0.039	0.050	-0.026	-0.068	0.010
controlled difference.	(0.023)	(0.021)	(0.018)	(0.016)	(0.020)
Eligible for Bagrut					
Control group mean	0.504	0.542	0.528		
Treatment-control	-0.067	-0.097	-0.008	0.081	-0.025
difference	(0.063)	(0.043)	(0.044)	(0.023)	(0.026)
Treatment-control	-0.045	-0.047	0.039	0.085	-0.016
controlled difference.	(0.054)	(0.029)	(0.031)	(0.022)	(0.025)
Average score					
Control group mean	68.068	69.401	69.251		
Treatment-control	-8.225	-9.325	-2.553	6.883	-0.654
difference	(3.771)	(3.078)	(3.305)	(1.541)	(1.860)
Treatment-control	-6.546	-6.034	0.836	6.881	-0.089
controlled difference.	(3.058)	(2.320)	(2.194)	(1.464)	(1.792)
Number of Bagrut credits					
Control group mean	18.526	18.936	18.621		
Treatment-control	-2.855	-3.130	-1.047	2.113	-0.075
difference	(1.103)	(1.022)	(0.973)	(0.494)	(0.582)
Treatment-control	-2.220	-1.970	0.130	2.232	0.124
controlled difference.	(0.856)	(0.738)	(0.716)	(0.476)	(0.560)
Number of science credits					
Control group mean	2.603	2.427	2.414		
Treatment-control	-1.021	-0.935	-0.685	0.331	0.132
difference	(0.385)	(0.253)	(0.276)	(0.161)	(0.184)
Treatment-control	-0.820	-0.537	-0.231	0.445	0.194
controlled difference.	(0.305)	(0.156)	(0.171)	(0.157)	(0.178)
Number of honor-level subjects					
Control group mean	2.029	2.093	2.057		
Treatment-control	-0.531	-0.542	-0.282	0.275	0.020
difference	(0.160)	(0.151)	(0.143)	(0.069)	(0.078)
Treatment-control	-0.436	-0.358	-0.089	0.305	0.050
controlled difference.	(0.123)	(0.106)	(0.098)	(0.066)	(0.074)
Number of students	4,798	4,924	5,202	14,924	9,722
Control group	4,037	4,095	4,411	12,543	8,132
Treatment group	761	829	791	2,381	1,590

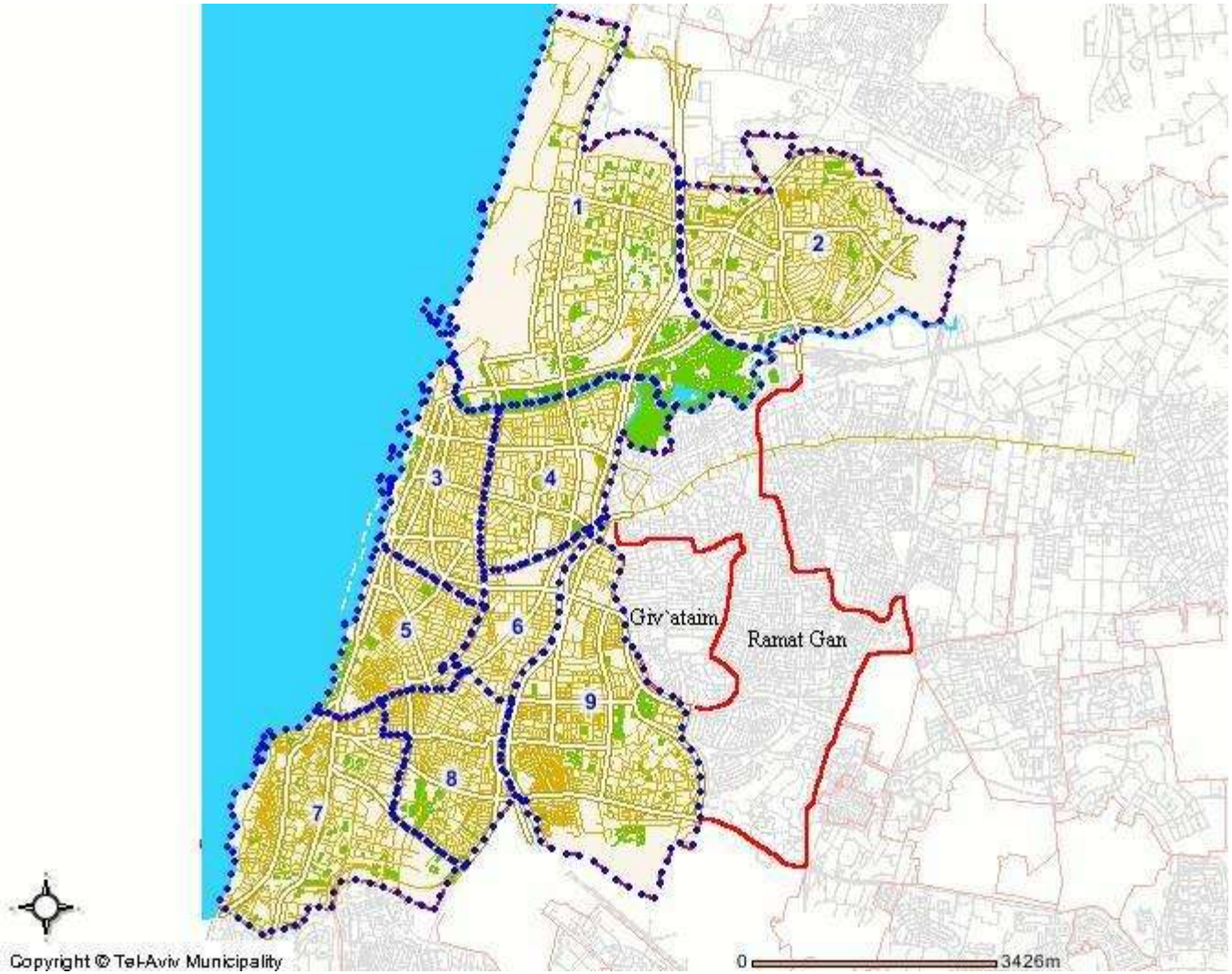
Notes: Standard errors in parentheses are adjusted for (primary) school level clustering in the level estimates for each year. In the difference in difference specifications, the outcome in question is regressed on treatment interacted with a dummy for 1994 (this is the coefficient reported), and other controls: year and primary school fixed effects, levels of maternal and paternal education, number of siblings, gender, immigrant status, and ethnicity. The sample is limited to schools that appear both before and after treatment in each of the difference in difference sub-samples.

Table A6: Regression Discontinuity Sample: Differences-in-Differences Estimates of the Effect of School Choice on Disability Rate

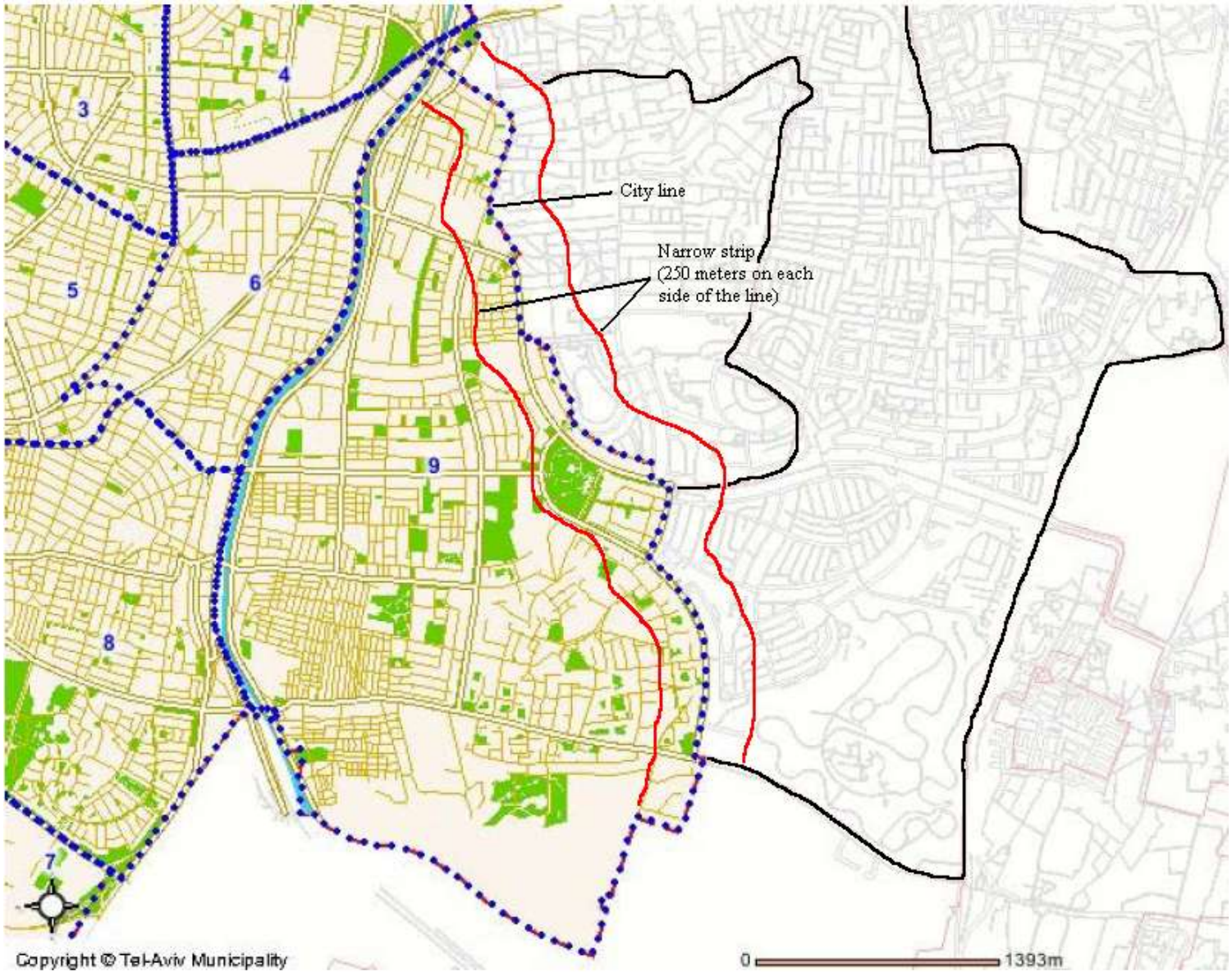
	2009 Outcomes			2010 Outcomes			2011 Outcomes		
	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment	1992-1993 Cohorts in Treated Schools	Estimate	Controlled Experiment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.009	0.004	0.014	0.010	0.015	0.021	0.014	0.000	0.014
	(0.093)	(0.008)	(0.007)	(0.102)	(0.011)	(0.008)	(0.117)	(0.011)	(0.010)
Total Disability Insurance Benefits Received (NIS)	271	123	476	315	277	611	390	15	554
	(3,225)	(212)	(163)	(3,445)	(264)	(200)	(3,724)	(274)	(238)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.006	0.000	0.015	0.006	0.001	0.016	0.006	0.000	0.017
	(0.080)	(0.007)	(0.006)	(0.080)	(0.005)	(0.005)	(0.080)	(0.006)	(0.007)
Total Work Disability Insurance Benefits Received (NIS)	125	41	396	57	69	211	26	108	178
	(1,567)	(149)	(164)	(984)	(87)	(91)	(414)	(75)	(94)
Number of Observations	573	2,341	1,469	573	2,341	1,469	573	2,341	1,469

Notes : This table presents the differences-in-differences estimates of the effect of the School Choice program on different disability rate outcomes for the regression discontinuity sample described in the paper. Columns 1-3 report results for 2011, columns 4-6 report results for 2010, and columns 7-9 report results for 2009. The variable "Received Disability Insurance Benefits" receives the value of 1 is the individual received any amount of diability benefits from the National Insurance Institution of Insrael (NII), 0 otherwise. The variable "Total Disability Insurance Benefits Received" describes the total NIS amount of disability insurance benefits the individual received in a given year. The variable "Received Work Disability Insurance Benefits" receives the value of 1 is the individual received any amount of work diability benefits from the National Insurance Institution of Insrael (NII), 0 otherwise. The variable "Total Work Disability Insurance Benefits Received" describes the total NIS amount of work disability insurance benefits the individual received in a given year. Columns 1,4, and 7 report the mean and standard deviation for the 1992-1993 (untreated) cohorts in the treated schools. Columns 2,5, and 8 report the differences-in-differences estimates for each of the dependent variables listed above. Columns 3,6, and 9 report the differences-in-differences estimates of a controlled experiment where the 1992-1993 cohorts are intentionally altered in the regression to receive treatment. Standard errors are clustered at the school level.

Map 1 – Tel Aviv City, School Districts 1-9, and the cities Giv`ataim and Ramat Gan



Map 2 – Tel Aviv School District 9 and Tangent Neighborhoods of Giv`ataim and Ramat Gan.



Note: The red lines approximately draw the band.