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TEACHERS' PAY FOR PERFORMANCE IN THE LONG-RUN:
EFFECTS ON STUDENTS' EDUCATIONAL AND LABOR MARKET OUTCOMES IN ADULTHOOD

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Teachers' Pay for Performance in the Long-Run: Effects on Students' Educational and Labor Market Outcomes in Adulthood
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ABSTRACT

This paper examines the dynamic effects of a teachers' pay for performance experiment on long-term outcomes at adulthood. The program led to a gradual increase in university education of the high school treated students, reaching a gain of 0.25 years of schooling at age 28-30. The effects on employment and earnings were initially negative, coinciding with a higher enrollment rate in university, but became positive and significant with time. These gains are largely mediated by the positive effect of the program on several high school outcomes, including quantity and quality gains in the high stake matriculation exams.

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1. Introduction

The long term effect of teachers' pay for performance (PFP) schemes is of particular interest because of skeptics' claims that they only improve student test scores by teaching to the test, or by encouraging teachers and schools to cheat. Skeptics claim there is no real increase in human capital because teachers do not respond to pay incentives by promoting broad human capital acquisition. They argue that teachers focus on improving students' test taking ability; on test preparation instead of teaching material not included in the exam; on exam strategies such as how to answer multiple choice questions, and on skills and actions that raise scores on the formulas used to reward teachers.[1] Concerns about narrowly targeted gains are heightened if those gains are focused on areas where labor market rewards are due to signaling rather than human capital acquisition.

To address these claims, I examine the effect of teachers' PFP on long term human capital outcomes, in particular attainment and quality of higher education, and labor market outcomes at adulthood, in particular employment and earnings. I use a teachers' PFP experiment which I conducted a decade and half ago in Israel. In Lavy (2009) I analyzed the short-term effects of this experiment on students' cognitive high school outcomes. Now, this earlier research presents an unusual opportunity to evaluate whether an intervention that offered teachers performance-based bonuses for student test achievements has had a lasting impact on adult well-being. This paper provides the first evidence of links between teachers' PFP during high school and students' schooling and labor market outcomes in their late 20s and early 30s. I measure post-secondary educational attainment, employment, earnings, eligibility for unemployment benefit and marriage and fertility. Some of these outcomes, for example the latter two, can also be viewed as potential mechanisms for the effect of the intervention on employment and earnings.

I observe these students' outcomes every year, from high school graduation (2000-2001) until age 30 (in 2012). Thus I can estimate the treatment effects for every year in the period, and trace the dynamic evolution of the program effect. Since a high proportion of the sample was in military

[1] See Jacob and Levitt (2003), Glewwe, Ilias and Kremer (2010), Neal (2011) and Muralidharan and Sundararaman (2011) for a discussion of this issue.

service for two (female) or three (male) years after high school[2], the estimates for these years (2000-2004) are not very informative, because they are based on a small and selective sample of those students not enlisted into military service. However, during 2005-2008 the treated group showed a higher enrollment rate in university schooling, and a corresponding lower employment rate and lower earnings. By the end of this period these negative effects were eliminated and the earnings effect turned positive, increasing in size and becoming significantly different from zero 9-12 years after high school graduation.

Just over a decade after the end of the intervention, treated students are 5 percentage points more likely to be enrolled in university education and to complete an additional 0.25 years of university schooling, a 35 percent increase relative to the control group mean. These gains are most likely explained by the improvements in high school *bagrut* outcomes facilitated by the teachers' PFP intervention. The higher passing rate and average score in the math and English matriculation exams (Lavy 2009), are also expressed in improvements in average matriculation outcomes, such as matriculation diploma certification (up by 3 percentage points) and the overall composite matriculation score (up by 2.9 points). These two outcomes determine admission to university and to selective study programs, such as medicine, engineering and computer science. Other dimensions of the matriculation study program that signal quality of schooling also improved, in particular the number of science credit units, which increased by 26 percent, and the number of subjects studied at the most advanced level, which increased by 5 percent.

These high school outcomes are also highly correlated with labor market outcomes at adulthood. These improvements, along with the increase in university schooling, led to a 1.2 percentage point gain in employment rate and to a 6-7 percent increase in earnings at age 28-30. The estimates suggest that the program did not have an effect on average marriage and parenthood rates. These average gains mask some heterogeneity by family income and gender. For example, children from families with above median income experience a higher increase in schooling but no effect on

[2] Israelis begin a period of compulsory military service after high-school graduation. Boys serve for three years and girls for two (longer if they take a commission). Ultra-orthodox Jews are exempt from military service as

employment, while children from families with below median income have lower gains in schooling and a large positive effect on employment. The effect on earnings is the same for both groups. Children from families with below median income also experienced a significant decline in marriage rate and a more modest decline in fertility.

The results of this analysis have meaningful external validity and are easily transferable and applicable to education in other 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 teachers' PFP intervention studied here have been implemented in recent years in developed and developing countries. This study contributes to the accumulation of empirical evidence about the returns to educational interventions, creating a useful guide for policy makers. Another important advantage of the evidence presented in this paper is that teachers' PFP is an intervention that can be directly implemented by public policy, whereas evidence based on parameters such as school or teacher quality are not so easily measured or rewarded by policy interventions.

This paper adds to a growing literature on the long term effects of educational programs. Earlier studies focused on the long-term effects of compulsory schooling laws on adult educational attainment (Angrist and Krueger, 1991) and on adult health (Lleras-Muney, 2005), for example. More recent studies have addressed schooling programs aimed at improving the quality of education in addition to increasing attainment. Most of these studies have asked whether the evaluation of short-term outcomes, primarily standardized test scores, are an effective measure of success. However, an equally relevant question is the extent to which educational interventions lead to long-term improvements in well-being – measures assessed not by attainment on tests but by attainment in life. Puzzling and conflicting results from several evaluations make this a highly salient issue. Three small-scale, intensive preschool experiments produced large effects on contemporaneous test scores that quickly faded (Schweinhart et al., 2005; Anderson, 2008). Non-experimental evaluations of Head Start, a preschool program for poor children, revealed a similar pattern, with test-score effects dissipating by middle school. But in each of these studies, treatment effects re-emerged in adulthood

long as they are enrolled in seminary (Yeshiva); orthodox Jewish girls are exempt upon request; Arabs are in the form of increased educational attainment, enhanced labor market attachment, and reduced crime (Deming, 2009; Garces et al., 2002; Ludwig and Miller, 2007). Other studies have shown evidence for the effect of investments in childhood on postsecondary attainment (Krueger and Whitmore 2001, Dynarski et al 2011). Very recently, Chetty et al (2011 and 2014) examined the longer-term effect of value-added measures of teachers' quality in a large urban school district in the United States, and reported significant effects on earnings at age 27 even though the effect on test scores had faded away much earlier. Dustmann et al (2012), however, found that attending a better school in Germany had no effect on school attainment or labor-market outcomes. Even though the ultimate goal of education is to improve lifetime well-being and there is much uncertainty about the long term gains from such programs, there have not been any studies that focus on the long term effect of teachers' PFP. Determining which interventions are more effective in improving long-term outcomes is critical for refining the effectiveness of education and school resource allocation.

The remainder of this paper is as follows. Section 2 describes the PFP experiment and the identification and econometric model. Section 3 describes the data and Section 4 presents the empirical findings. Section 5 concludes.

2. The Pay for Performance Experiment

Teacher incentive programs are increasing in popularity. Performance-related pay for teachers is being introduced in many countries, amidst much controversy and opposition from teachers and unions. The rationale for these programs is that incentive pay may motivate teachers to improve their performance (Lazear 2000 and 2001, Lavy 2002, 2007 and 2009, Neal 2011, Duflo et al. 2012). Opponents of teachers' incentive programs argue that schools may respond to test score-based incentives in perverse ways such as by cheating in grading and teaching to the test (Glewwe et al, 2010, Neal 2011), leading to short term gains in performance but not to the long-term accumulation of human capital. Even though there is some evidence that performance pay for teachers has significant short term benefits for student outcomes, their critique is focused on purported harmful long term

exempt, though some volunteer.

effects, for which there is scant evidence. The evidence in this paper cast doubts on these claims by presenting results on a wide array of lifetime outcomes.

The Teachers' Incentive Experiment

In December 2000, the Israeli Ministry of Education unveiled a new teachers' bonus experiment that I designed and helped to implement in 49 high schools. The main feature of the program was an individual performance bonus paid to teachers on the basis of their students' achievements. The experiment included all teachers of English, Hebrew, Arabic, and mathematics for grades 10 through 12, in advance of matriculation exams in these subjects in June 2001. The ranking was based on the difference between the actual outcome, and a value predicted on the basis of a regression that controlled for the students' study program and socioeconomic characteristics and a fixed school level effect. Separate regressions were used to compute the predicted pass rate and mean score, and each teacher was ranked twice – once for each outcome – using the size of the residual from the regressions. All teachers whose students' mean residual was positive in both outcomes were divided into four ranking groups, from first place to fourth. The first place award was $7,500; second place, $5,750; third place, $3,500; and fourth place, $1,750.

There were two criteria for school eligibility: 1) a recent history of relatively poor performance in the mathematics or English matriculation exams, and 2) the most recent school-level matriculation rate was equal to or lower than the national mean of 45 percent. Though 99 schools met the first criterion, only 49 met the second criterion. The program included 629 teachers. Nearly half of the teachers, 302 of them, won awards—94 in English, 124 in math, 67 in Hebrew and Arabic, and 17 in other subjects. Although the program was designed as an experiment, schools were not randomly assigned to it. Nevertheless, the design of the program enables the implementation of a randomized trial identification strategy, which I outline below. The short term impact, presented in Lavy (2009), was that teacher incentives increased students' achievements by increasing the test taking rate, as well as the conditional pass rate and test scores, in math and English exams. The improvement in these conditional outcomes, which were estimated based on external tests and grading, accounted for more than half of the increase in the unconditional outcomes in math and somewhat less in English. These improvements appear to result from changes in teaching methods, after-school teaching, and increased responsiveness to students' needs, and not from artificial inflation or manipulation in test scores. The evidence that incentives induced improved effort and pedagogy is an important counter to concern that incentives may have unintended effects, such as "teaching to the test" or cheating and manipulation of test scores, and that they do not generate real learning. However, more conclusive evidence about whether teachers' PFP schemes improve human capital accumulation can only be based on longer term outcomes, in particular the effect on completed post-secondary schooling, employment, wages, welfare dependency and crime, which I investigate here.

Identification and econometric model: measurement error in the assignment variable

The program rules limited assignment to schools with a 1999 matriculation rate equal to or lower than 45 percent (43 percent for religious and Arab schools). However, the matriculation rate used for assignment was an inaccurate measure of this variable. This measurement error could be useful for identification of the program effect. In particular, conditional on the true matriculation rate, program status may be virtually randomly assigned due to mistakes in the preliminary file. Most (80 percent) of the measurement errors were negative, 17 percent were positive, and the rest were free of error. Identification based on the random measurement error can be presented formally as follows:

Let $S = S^* + \varepsilon$ be the error-affected 1999 matriculation rate used for the assignment, where S^* represents the correct 1999 matriculation rate and ε the measurement error. T denotes participation status, with $T = 1$ for participants and $T = 0$ for non-participants. Since $T(S) = T(S^* + \varepsilon)$, once we control for S^*, assignment to treatment is random ("random assignment" to treatment, conditional on the true value of the matriculation rate). The presence of a measurement error creates a natural experiment, where treatment is assigned randomly, conditional on S^*, in a sub-sample of the 98 eligible schools. Eighteen of the eligible schools had a correct 1999 matriculation rate above the threshold. Thus, these schools were "erroneously" chosen for the program. For each of these schools, there is a school with an identical correct matriculation rate but with a draw from the (random) measurement error distribution which is not large (and negative) enough to drop it below the assignment threshold. Such pairing of schools amounts to non-parametrically matching schools on the basis of the value of S^*(see Figure 3 in Lavy 2009 for a graphical presentation of this matching). Therefore, the eighteen untreated schools may be used as a control group that reflects the counterfactual for identification of the effect of the program. The group of 18 treated and 18 control schools is perfectly balanced in student and school characteristics (see Table A1 in the appendix, which is reproduced from Lavy 2009 Table 3. In the next section I show that the treatment-control similarities observed in Table A1 are also evident when comparing the pre-program long-term outcomes). The following model is used as the basis for regression estimates using the NE sample:

$$Y_{ijt} = \alpha + X_{ijt}{}' \beta + Z_{jt}{}' \gamma \; + \delta\, T_{jt} + \Phi_j + \eta\, D_t + \varepsilon_{ijt}$$

where i indexes individuals; j indexes schools; t indexes years 2000 and 2001, and T is the assigned treatment status. X and Z are vectors of individual and school level covariates and D_t denotes year effects with a factor loading η. The treatment indicator T_{jt} is equal to the interaction between a dummy for treated schools and a dummy for the year 2001. The regressions will be estimated using pooled data from both years (the two adjacent cohorts of 2000 and 2001), stacked as school panel data with fixed school-level effects (Φ_j) included in the regression. The resulting estimates can be interpreted as an individual-weighted difference-in-differences procedure comparing treatment effects across years. The estimates are implicitly weighted by the number of students in each school. The introduction of school fixed effects controls for time-invariant omitted variables and also provides an alternative control for school-level clustering.

Identification based on a Regression Discontinuity model

To check the robustness of the results based on the NE sample, I use an additional alternative method, an RD design, to identify the effect of the teacher bonus program. Given that the rule governing selection to the program was simply based on a discontinuous function of a school observable, the probability of receiving treatment changes discontinuously as a function of this observable. The discontinuity in our case is a sharp decrease (to zero) in the probability of treatment beyond a 45 percent school matriculation rate for nonreligious Jewish schools and beyond 43 percent for Jewish religious schools and Arab schools. I exploit this sharp discontinuity to define a treatment sample that included schools that were just below (up to -5 percent) the threshold of selection to the program and a comparison group that included untreated schools that were just above (up to +5 percent) this threshold. The time series on school matriculation rates show that the rates fluctuate from year to year for reasons that transcend trends or changes in the composition of the student body. Some of these fluctuations are random. Therefore, marginal (in terms of distance from the threshold) participants may be similar to marginal nonparticipants. The degree of similarity depends on the width of the band around the threshold. Sample size considerations exclude the possibility of a bandwidth lower than 10 percent, and a wider band implies fluctuations of a magnitude that are not likely to be related to random changes. Therefore, a bandwidth of about 10 percent seems to be a reasonable choice in our case. The main drawback of this approach is that it produces an estimate from marginally (relative to the threshold) exposed schools only. However, this sample may be of particular interest because the threshold schools could be representative of the schools that such programs are most likely to target.

There are 13 untreated schools with matriculation rates in the 0.46–0.52 range and 14 treated schools in the 0.40–0.45 range. The 0.40–0.52 range may be too large, but I can control for the value of the assignment variable (the mean matriculation rate) in the analysis. Note, also, that there is some overlap between this sample and the natural experiment sample. Eleven of the 14 treated schools and 8 of the 11 control schools in the RD sample are also part of the natural experiment sample, leaving only six schools (3 control and 3 treated), which are included in the former but not in the latter. However, there are 17 schools in the NE sample (7 treated and 10 control) that are not included in the RD sample, which suggests that there is enough "informational value added" in each of the samples.

Table A2 in the appendix (reproduced from Lavy 2009 Table 6) is similar to Table A1, but for the RD sample. The treatment-control differences and standard errors in the student background variables (columns 3 and 6) reveal that the two groups are very similar in both years in all characteristics except the ethnicity variable in year 2000 and number of siblings in 2001. However, both estimated differences are only marginally significant. The third panel reveals some treatment-control differences: in math lagged credits and in the average score for the 2001 cohort, and in English lagged credits for the 2000 cohort. However, the control-treatment gaps in lagged credits are opposite in sign in math (negative) and English (positive) and in each subject they are significant for only one of the two cohorts. In the next section I show that the RD sample treatment-control similarities observed in Table A2 are also evident when comparing the pre-program long-term outcomes.

To interpret similarity or differences between the estimates of the two methods, it is important to note that the main conceptual difference between the NE and the RD methods is that the latter does not control for S* and, if there were no measurement errors, the RD design would have compared, in addition, pupils or schools with different S*. The two methods would therefore yield similar results if either S* is weakly related to the outcomes or if the variance of the measurement error is large relative to the variance of S* around the cut-off point (which means that those above and below the critical S have approximately the same S*). The first condition is met, as S* has very small positive correlations with the high school and post-secondary schooling and labor market outcomes. The second condition is not met because within the range (0.40-0.52) around the cutoff point of the assignment variable (S), the two relevant variances are very similar, 0.075 for the measurement error and 0.078 for S*.

3. Data

In this study I use data from the administrative files of the participants in the treatment and control groups. The students in the sample graduated from high school between 2000 and 2001, and in 2013 they are adults aged 30-31. I use several panel datasets from Israel's National Insurance Institute (NII). The NII is responsible for social security and mandatory health insurance in Israel. NII allows restricted access to this data in their protected research lab. The underlying data sources include: (1) the population registry data, which contains information on marital status, number of children and their birth dates; (2) NII records of post-secondary enrollment from 2000 through 2013, based on annual reports submitted to NII every fall term by all of Israel's post-secondary education institutions. Based on this annual enrollment data I computed the number of years of post-secondary schooling[3]; (3) Israel Tax Authority information on income and earnings of employees and self-employed

[3] 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

individuals for each year during 2000-2012. This file includes information both for the students and their fathers and mothers; (4) NII records on unemployment benefits for the period 2009-2012, and marriage and fertility information as of 2012. The NII linked these data to students' background data that I used in Lavy (2009). This information comes from administrative records of the Ministry of Education on the universe of Israeli primary schools during the 1997-2002 school years. In addition to individual identifier, and a school and class identifier, it also included the following family-background variables: parental schooling, number of siblings, country of birth, date of immigration if born outside of Israel, ethnicity and a variety of high school and high school achievement measures. This file also included a treatment indicator, school ID and cohort of study. I had restricted access to this data in the NII research lab at the NII headquarters in Jerusalem.

The NII data track all individuals in Israel and also those who left the country providing they continue to pay National Insurance Tax (similar to social security tax in the US). The basic sample of students from all 98 schools that were eligible to participate in the program included 25,588 students. Only 153 students from this sample (0.6 percent) were not found in the Population Registry at NII, 68 were from the control group (out of 12,500) and 85 from the treated group (out of 13,068). The proportion in the natural experiment and in the regression discontinuity sample are very similar to the proportion in the eligible sample. This means our long term analysis tracks 99.4 of the students to adulthood.

The post high school academic schooling system in Israel

The post high school academic schooling system in Israel includes seven universities (one of which confers only graduate and PhD degrees), and over 50 colleges that confer academic undergraduate degrees (some of these also give masters degrees)[4]. All universities require a *bagrut* diploma for enrollment. Most academic colleges also require a *bagrut*, though some look at specific *bagrut* diploma components without requiring full certification. For a given field of study, it is typically more difficult to be admitted to a university than to a college. The national university

to send a list of enrolled students to the NII every year. For the purposes of this project, the NII Research and Planning Division constructed an extract containing the 2001–2013 enrollment status of students in our study.

enrollment rates for the cohort of graduating seniors in 1995 (through 2003) was 27.6 percent and the rate for academic colleges was 8.5 percent.[5]

The post-high school outcome variables of interest here are indicators of ever having enrolled in a university or in an academic college as of the 2013 school year, and the number of years of schooling completed in these two types of academic institutions by this date. I measure these two outcomes for the 2000 and 2001 12th grade students. Even after accounting for compulsory military service[6], we expect that most students who enrolled in academic post-high school education, including those who undertook post-graduate studies, to have graduated by the 2013 academic year.

Definitions of Outcomes in Adulthood

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

Post-Secondary Academic Schooling

In the NII data, I observe two sets of post-secondary outcomes for each of the students in the sample. First, I observe year-by-year post-secondary schooling attainment, including the type of post-secondary schooling institution attended – if any – and the number of years of schooling completed in each type of institution.

Labor Market Outcomes

I observe year-by-year labor market outcomes from high school graduation to 2012, including employment status and annual earnings. Individual earnings data come from the Israel Tax Authority (ITA). Only individuals with non-zero self-employment income are required to file tax forms in Israel, but ITA has information on annual gross earnings from salaried and non-salaried employment and transfers this information annually to NII, including the number of months of work in a given year. NII produces an annual series of total annual earnings from salaried and self-employment. Following NII practice, individuals with positive (non-zero) number of months of work and zero or

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[4] A 1991 reform sharply increased the supply of post-secondary schooling in Israel by creating publicly funded regional and professional colleges.
[5] 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).

missing value for earnings are assigned zero earnings. 14.1% of individuals (students) have zero earnings at age 30-31 in our basic sample and 16.6% have zero earnings in this sample. To account for earnings data outliers I dropped from the sample all observations that are six or more standard deviations away from the mean. Very few observations are dropped from the sample in each of the years and the results are not qualitatively affected by this sample selection procedure. To account for age differences of the different cohorts included in the sample, the outcomes are adjusted for years since graduating high school. The same earnings data is also available for the parents of the students in our sample, for the years 2000-2002 and 2008-2012. I compute the average earnings of each parent and of the household for 2000-2002 and use it as an additional control in a robustness check of the evidence presented in this paper. These data were not available for the analysis of the effect of the program on short-term outcomes. I also use as additional outcomes the NII indicator of being *Eligible for Unemployment Benefit* and the annual amount of *Unemployment Benefit Compensation*.

Personal Status Outcomes

The Population Registry is available to us only for 2012. However, the dates of each marriage and birth event are reported in the data and therefore I can adjust the demographic outcomes for years since graduating high school. These outcomes include indictors for *Marriage Status* and for *Having Children.*

Descriptive Statistics

Table 1, columns 1-2, presents detailed summary descriptive statistics for the outcome variables for 2012 by treatment and control group, for the pre-program cohort who graduated high school in 2000 (pre-treatment) for the NE full sample and treatment-control balancing tests (column 3). In columns 4-6, I present the respective evidence for the first three quartiles sample (3Q) of the ability distribution of students. I present results based separately on the 3Q sample because these are the students whose math and English matriculation outcomes were most affected by the teachers' bonuses program (Lavy 2009). The balancing tests extend the analysis presented in Lavy (2009) for the similarity between the treatment and the control group in terms of family characteristics and pre-program high school outcomes. These results are reproduced here for convenience in Table A1 in the appendix. In this section I show that the treatment-control similarities observed in Table A1 are also evident when comparing the pre-program long-term outcomes presented in Table 1.

Statistics on post-secondary academic enrollment rates are presented in panel A, and on post-secondary completed years of schooling in panel B. The ever enrolled rate in university up to 2012 in the treatment group for the pre-treatment cohort (2000) in the full sample is 21.6 percent, and in the control group it is 19.1 percent. The treatment-control difference is 0.026 (se-0.046), not statistically different from zero. The respective enrollment rates in academic colleges are 14.5 and 14.3, practically equal for the two groups.[7] This perfect balancing is also evident when the treatment-control differences are measured based on the 3 quartile sample (columns 4-6): the university and academic-college enrollment rate differences between the two groups are 0.001 (se=0.034) and 0.009 (se=0.035). Note that the means of these outcomes show clearly that in the full sample the mean of enrollment rate in university is higher than in the 3 quartiles sample while the respective means in academic colleges in the two samples are identical. This is expected as the students in the fourth quartile have the highest mean high school outcomes.

Similar treatment-control similarities are observed with respect to completed years of university and academic college. This evidence is presented in panel B: all four differences (in the natural experiment full and three quartile samples) are small and not statistically different from zero. For example, the mean years of academic college in the full sample is 0.379 in the treatment sample and 0.365 in the control sample; the difference, 0.014, is not statistically different from zero. In summary, of the 8 schooling related difference estimates presented in panel A and B, none suggest any pre-program control-treatment gaps.

Summary statistics and balancing tests for the pre-program cohort labor market outcomes in 2012 are presented in panel C of Table 1. The employment rate in 2012 among the treatment and control groups is 84.0 and 84.6 percent, the difference (-.006) is small and not significant. Average annual earnings of these two groups are 67,214 New Israel Shekels (NIS), equivalent to $17,690, and 69,478 NIS ($18,300), respectively, and the difference is not statistically different from zero (-2,265, se=3945). The unemployment rate for these two groups is 8.0 percent and 8.7 percent (very similar to the national unemployment rate in 2012), and again the groups' difference is not statistically different from zero. Correspondingly there is no difference between the mean annual unemployment benefits received during 2012 for the two groups. The evidence from the 3 quartile sample, presented in columns 4-6, shows the same basic similarity in outcome means between the treatment and control groups. None of the eight differences presented in panel C of Table 1 are statistically different from zero.

Panel D presents the means and balancing tests for marriage and fertility outcomes. About 56 percent of the individuals in the sample are married by 2012 and the treatment-control difference (two percent) is not different from zero. 45 percent of the sample have children, suggesting that almost all who are married have children by 2012. However, there are no differences in these two outcomes between treatment and control group and in the mean number of children, which is just below one. The evidence about the means and balancing tests based on the three quartile sample are identical to those based on the full sample.

Panel E presents the means and balancing tests for father and mother average earnings in 2000-2002, the years that students in the sample were in high school. Note that this information became available only recently through the NII data, and I therefore add them now to the treatment-control balancing analysis. The treatment-control differences in these variables are positive in the full sample but they are relatively small and not significantly different from zero. In the three quartile sample the differences are even smaller, and in the case of maternal earnings the difference changes signs and is negative. Here as well, the small differences are not statistically different from zero. The evidence on balancing of parental earnings when using the treated cohort sample is very similar, without any noticeable treatment-control differences in the full or three quartile sample. These results are not presented in Table 1, which presents balancing tests for the pre-treatment cohort.

Table 2 presents the descriptive statistics and balancing tests based on the Regression Discontinuity (RD) sample. This sample is smaller than the NE sample, in particular the treated group is smaller by about 30 percent. Yet the means of the treatment and control groups are very similar to those based on the NE sample, and the respective balancing tests show a very similar pattern to that

[7] Note that very few students ever enroll in both university and academic colleges.

obtained from the NE sample. Namely, there are no detectable differences between treatment and control groups and all t-tests permit rejecting the hypothesis that the differences are different from zero. However, it is worth noting that the signs of some of the differences are opposite to the signs of the NE sample differences. Of particular interest and importance is the treatment-control earnings gap in the pre-program cohort. In the NE sample (panel C in Table 1) this gap is negative though small and not significant, while in the RD sample it is positive, though small and not significant (panel C in Table 2). I will return to this distinction when discussing the treatment estimates of the effect on earnings which, as will be shown, are very similar in the NE and RD samples.

4. Empirical Evidence

Effect on Post-Secondary Academic Schooling Attainment

The program had positive and significant short term effects on high school English and math outcomes at the end of high school (Lavy 2009). Since the program increased exam participation, the average score, and the passing rate in the math and English matriculation exams, we should expect also a positive effect on the overall summary outcomes of the matriculation exams. This includes the matriculation rate, total number of matriculation credits, and the average score in the all matriculation exams which could also have improved because of spillover effects of the program on other subjects' exam take up rate and test scores. Improvement in these summary achievement measures should lead to an increase in post-secondary academic schooling, because they are used as admission criteria for various academic institutions and study programs.

Table A3 presents results for the short-run impact of the PFP experiment summary measures on the matriculation exam program, including the average matriculation score, the matriculation rate and other related end of high school outcomes. In the full sample, the average matriculation score is up by 3.8 points (se=1.017), and the matriculation rate went up by 3.6 percentage points, which amounts to a 8 percent improvement. The average number of credit units increased by 0.8, the number of credits in science increased by 0.6 units (a 25% percent increase), and the number of subjects studied at the most advanced level (5 credits) increased by 0.1. The respective estimated effects based on the three quartile sample are similar to those based on the full sample with one exception: the effect on the matriculation rate is a 5.5 percentage point increase, larger than the effect estimated in the full sample. This difference is consistent with and can be explained by the evidence presented in Lavy (2009) that indicated that the effect on the math and English tests' passing rate was also marginally larger in the three quartile sample, an improvement that allowed marginally failing students to qualify for the matriculation diploma.

I make a graphical representation of the effect of the PFP program on post-secondary schooling, focusing on the two branches of academic post-secondary education in Israel. The first includes the seven research universities in Israel that confer BA, MA and PhD degrees. These schools require a matriculation diploma for admission, an intermediate or advanced matriculation study level in English (note that to qualify for a matriculation diploma a basic study program in English is sufficient) and at least one matriculation subject at an advanced level. About 35% of all students are enrolled in one of the seven universities. The second branch includes more than 50 academic colleges that mostly confer a BA degree and generally offer social sciences, business and law degrees.

In figure 1, we measure the treatment effect for each year since high school graduation and trace the dynamic pattern for university enrollment for the NE sample. To do so, we run a separate regression for each of the outcomes and for each of the years since high school graduation. We then plot the coefficients of these regressions around a 90% confidence interval. Note that both the ever-enrolled variable and the years of schooling are cumulative variables. Hence, we expected the effects to be either flat or increasing over time.

This treatment effect becomes positive from year three after graduating high school and it reaches its height, at 5 percentage points, from the eighth year after high school graduation, remaining flat afterwards.[8] This pattern likely reflects the fact that students who do not enroll in post-secondary schooling in the first eight years are very unlikely to return to school later in life. In contrast, the effect on years of schooling accumulates over time (Figure 1A). Although most of the increase happens in the first eight years, the effect seems to be increasing even after 12 years since graduation, reaching a peak of 0.25 years. The fact that the increase keeps accumulating even 12 years after high

[8] The emergence of treatment effect after three years is reasonable given that most of the female students are in military service for the two years following high school graduation and for boys this period is three years.

school graduation suggests that focusing on outcomes immediately after graduation may underestimate the long-term effects. Note that the effect on the intensive margin seems to operate beyond the increase in enrollment. Given a 5 percentage-point increase in enrollment and a typical duration of 3-4 years, we should expect schooling to increase by only 0.15-0.20 years. The fact that the effect on years of schooling is larger than 0.20 years suggests that the program induced treated students to stay longer and complete longer programs.

The effect size on enrollment and years of schooling can be assessed in comparison to the mean enrollment rate for the treated group, which increases gradually from year one and is highest at 20 percent thirteen years later. The mean of university years of schooling in the treatment group is 0.8.[9] Figures 2 and 2A present the estimated effects on academic college enrollment and attainment and the pattern revealed in these figures is very different, as the effect is negative and small, practically close to zero.

In Figures 3-3A and 4-4A I replicated this graphical analysis based on the RD sample and the results are identical to those obtained based on the NE sample. Figures 5-5A and 6-6A present the respective graphs for the NE 3Q sample and Figures 7-7A and 8-8A do the same for the RD 3Q sample. The evidence in these figures is practically the same as those obtained based on the full sample.

Table 3 presents the point estimates and their standard errors for the impact of the PFP experiment on university and academic colleges schooling attainment, as measured at the end of the period of study. The table presents results based on the NE sample (columns 1-4) and the RD sample (columns 5-8). Effects on enrollment are presented in columns 1-2 and 5-6. Evidence for years of schooling is presented in columns 3-4 and 7-8. The results based on the full sample are presented in panel A.

Enrollment in university increased by 4.8 percentage points and this effect is precisely measured (se=0.013). This gain, relative to the pre-program mean of students in treatment schools (21.6 percent), is a 22 percent increase. This increase in enrolment led to a 0.250 increase in

[9] The yearly university enrollment rate is highest at year 4-7 and then starts to decline until practically leveling at close to zero at year 13 (this result is not shown in the paper due to space limitations).

completed years of university schooling, reflecting a 30 percent increase relative to the baseline mean of 0.825 years of university schooling. The relative gain in university enrollment and in the respective completed years of schooling are of similar magnitude, both being large relative to the impact of some other educational interventions or policy change, for example in comparison to the gain due to an increase in compulsory schooling. The effect on academic-colleges enrollment and years of schooling is negative but close to zero and not very precisely measured. This pattern may suggest some compositional change in the academic schooling but the offsetting decline in academic college schooling is too small to be economically meaningful.

The estimates based on the RD sample are consistent with the estimates based on the NE sample. Enrollment in university education increased by 6 percentage points (se=0.014) and completed years of university schooling increased by 0.242 years (se=0.073). The relative magnitude of these gains is identical to those presented above based on the NE sample. Obtaining similar results based on two different identification strategies, randomized assignment to treatment based on a natural experiment and assignment to treatment based on a threshold of an observed criterion, lends credibility to the causal interpretation of the finding reported in panel A of Table 3. The comparability of the NE and RD evidence with respect to the treatment effect of the program on long term educational outcomes at adulthood is similar to the respective comparability between the results obtained from the NE and RD samples regarding the program's effect on high school educational outcomes.

In Table 4 I present uncontrolled simple differences in differences estimates, using only a post treatment dummy, a treated school dummy interacted with the post dummy and school fixed effects. The estimated treatment effects based on this limited control specification are positive and significant for all university attainment outcomes, both in the NE and RD samples. These estimates are very close to those of estimates based on the full specification presented in Table 3, confirming what is expected given the balancing tests presented in Tables 1-2 and in appendix Tables A1-A2. For example, the full sample simple difference in differences estimate on university enrollment is 0.039 (se=0.015) and the respective controlled difference in difference estimate is 0.048 (se=0.013). The two respective estimated effects on university years of schooling are 0.206 (0.072) and 0.250 (0,067).

The results based on the three quartile sample (presented in panel B) are similar to the results based on the full sample. Based on the NE sample, university enrollment increased by 5.2 percentage points and completed years of university schooling increased by 0.24 years. The estimated effects on academic-college enrollment and years of schooling are also negative but somewhat smaller than those estimated based on the full sample. The results based on the RD three quartile sample are fully consistent with the respective estimates based on the NE sample. Note also that a comparison between the uncontrolled and controlled differences in differences estimates based on the Q3 sample (lower panel of Tables 3-4) shows even smaller differences.

Effect on Employment and Earnings

We expect the increase the quality and the quantity of high school and post-secondary education to result in better labor market outcomes in adulthood. In figures 9-12, I repeat the year-by-year analysis, focusing on labor market outcomes. The figures show the estimated effects by years since graduation from high school. The employment and earnings data are available until year 2012, so eleven years is the longest period since graduating high school for which I examine the effect of the program. Overall I find an increasing pattern in both employment and earnings. The effects become significantly different from zero about 9-11 years after high school graduation. Perhaps reflecting the higher enrollment in post-secondary schooling, the effect on employment is initially negative and increases thereafter. The effects on earnings follow a similar pattern. As treated students spent more years on average in the schooling system and appear on average to start working later, we expect the effect on earnings to be initially negative and to increase as students complete their post-secondary schooling and accumulate labor market experience. Indeed, we find that the effects are initially negative and become significantly different from zero by the end of our sample period. In the following paragraph I describe these results in more detail.

Figures 9 and 9A present the yearly estimates on employment for the NE and RD samples respectively. As already noted, the estimates for the first three years are not meaningful because most of the students in our sample were still in military service. In the fourth year after high school graduation, about 70 percent of the individuals in the sample were employed (according to our definition of employment, which is employed for at least for one month during the year and had positive earnings). From year four until year eight following high school graduation, the treatment effect estimates on employment are negative. The largest effect is about -0.02 in the NE sample and -0.03 in the RD sample. The NE estimates are not precisely estimated: none of these negative estimates are different from zero. The RD estimates are more precisely measured and two of the four estimates are statistically different from zero. When stacking the data for these four years (from the fourth to the seventh year following high school graduation), both the NE estimates (-0.014 se=0.008) and the RD estimates (-0.026 se=0.010) are statistically different from zero.[10] From year seven following high school graduation the treatment effect on employment is positive, statistically significant in some years and marginally so in others, in both the NE and RD samples. The highest employment treatment effect estimate is about 3 percentage points. The average employment rate from the seventh year following high school graduation is about 87 percent and this rate is stable until the end of the period. The evidence based on the NE and RD 3 quartiles samples is very similar to the evidence from the full sample.

The year-by-year estimated treatment effects on annual earnings are presented in Figures 11-11A. These estimates are negative from the fourth to the seventh year since graduation and then they turn positive and remain so until the end of the period studied. The lowest estimate based on the NE sample is about IS -4,000 shekels relative to mean earnings of IS 25,000 in the same year. The estimates based on the RD sample are very similar. The period with negative earnings effect coincides with the years with negative employment effect and with the period when the treatment effect on university enrollment becomes positive and increasing. This inverse mirror image of the treatment effect on employment and on university enrollment explains the negative effect on earnings. The treatment effect on earnings turns positive and significant from year seven on but it fluctuates in size, not surprisingly because earnings is a noisier outcome than university enrollment. The evidence based on the RD sample and the full and three quartiles samples reveal similar patterns to those based on the NE full sample.

[10] However, it is likely that these estimates are biased downward because the employment measure I use does not distinguish between part time and full time employment and students usually have lower labor supply while

Table 5 columns 1-2 and 5-6, present the point estimates and their standard errors for the various labor market outcomes at the end point of the period we study. I also present in columns 3-4 and 7-8 estimates from stacked regressions where I pool the data from the last three years of the studied period, namely nine to eleven years after graduating high school. The stacked regression yields average treatment effect for this period and allows more precise estimation of the effect on labor market outcomes. Focusing on the stacked regression results, the teachers' incentive experiment increased employment of treated students by 1.3 percentage points. The relevant average employment rate is 83 percent. Based on the three quartiles sample the effect is higher, about a 2 percentage point higher employment rate, and more precisely measured (se=0.010). Similar estimates are obtained based on the RD sample. The estimated effect on eligibility for unemployment benefits is negative but small and not significantly different from zero. The average unemployment rate in the treated group before treatment is low, only 6.9 percent, perhaps the reason why there is no discernable effect on this outcome. This rate is very similar to the national unemployment rate in 2010-2012 (7.1 percent) for the closest age group (25-34).

In Table 6 I present the simple differences in differences estimates for the labor market outcomes. These estimates are very close to those of estimates based on the full specification presented in Table 5, again reaffirming our earlier conclusion that the treatment and control groups are well balanced in characteristics and also in terms of outcomes of the pre-treatment cohort. For example, the simple difference in differences estimate on earnings 11 years after school completion is 5,312 (se=2,022) and the respective controlled difference in difference estimate is 5851 (se=1993). The two respective estimated effects from the stacked regression for 9-11 years after school completion are 4,353 (1,773) and 4,714 (1,519).

The average annual earnings in the pre-treatment cohort in 2009-2011 NE sample is NIS 55,311 ($14,555 based on an exchange rate of 3.8 Israeli Shekels to one US Dollar). The average estimated effect of the PFP program on annual earnings for this three years' period is NIS 4,711 (se=1519) ($1240). The estimated effect based on the RD sample is similar, also amounting to a 9

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in school.

percent annual increase relative to the pre-treatment treatment group mean.[11] The effect on annual earnings based on the three quartile sample is lower, 7 percent in the NE sample and 6 percent in the RD sample. I note again that the similar earnings effect estimates from two different identification strategies lends support and credibility to the causal interpretation of these estimates. I further note that the pre-program treatment-control mean difference in earnings was small and not significant in both samples and it was of different signs, negative in the NE sample and positive in the RD sample, yet both samples yield the same positive earnings gain.

A natural question about the above estimated effect on earnings is whether it captures the permanent long term effect. First, note that I measure the effect on earnings at about age 30-31, when individuals already completed their post-secondary schooling. Second, based on a sample of older cohorts, I find that earnings at age 30-35 is a strong predictor of earnings at an older age. Yet, it is important to note that earnings have larger variation over time than other personal outcomes. To get a better indication about the permanency of the effect on earnings, I estimated the effect on the percentile rank of individuals in the respective distribution of their cohort. There is no direct evidence that suggests that rank forecast is more stable than earnings or log earnings. However, recent papers in the intergenerational mobility literature provide some indirect evidence that is relevant to this issue. These studies have shown that movements across ranks in the income distribution are uncorrelated with parental income conditional on rank at age 30; in contrast, movement in log earnings are correlated with parental income conditional on log income at age 30 — in particular, rich offspring have higher earnings growth, so that age 30 measurements are biased predictors of later-life earnings. However, the rank forecasts appear to be less biased. For example, Nybom and Stuhler (2016) show with data from Sweden that the relationship between a child's income rank and their parental income rank stabilizes by around age 30; in contrast, the relationship in log earnings is less stable. Chetty et al (2015) find a similar pattern in the US tax data, reporting that percentile ranks predict well where children of different economic backgrounds will fall in the income distribution later in life. Using

[11] The fact that the treatment effect on earnings stays positive over several consecutive years is perhaps an indication that this gain reflects real productivity differences and not signaling of the higher schooling outcomes that resulted from 'teaching to the test'.

instead log earnings leads to inferior predictions because of the growth path expansions at the top of the income distribution.

Table 7 presents estimates of the effect of the program on percentile rank of earnings, where the rank is computed separately for each cohort. The estimates are fully consistent with the estimated effects on earnings that are presented in Table 5. After nine to eleven years from high school graduation, the program moves treated individuals by 2 percentile ranks (column 4, first row) and this effect is relatively precisely measured. The rest of the estimates presented in the table suggest similar findings.[12]

How Robust Are the Results to Controlling for Parental or Family Earnings?

In this section I present a robustness check of the PFP program effect when I add father, mother, or family earnings as an additional control in the DID estimation of treatment effect. Each of these variables is the respective average earnings in 2000-2002, the years just before and during the program implementation. I prefer to use a three-year average of earnings instead of a specific year because this measure is more likely to be correlated with the permanent level of family resources. These results for university schooling based on the NE sample are presented in Tables A4. The respective estimates for the effect on employment and earnings are presented in Table A5.

In columns 1-3 of Table A4, I present the treatment estimates on university enrollment when a control for father's earnings (row 1) or mother's earnings (row 2) is added to the controlled DID regressions. The three columns correspond to estimates after 10, 11 and 12 years since high school graduation. These estimates of the treatment effect of the program are very similar to the estimates presented in Table 3. Columns 4-6 present the respective treatment estimates on years of schooling. Again these estimates are practically identical to those presented in Table 3.

In Table A5 I present the treatment estimates on earnings and employment when controls are added for parental or family earnings. The three columns correspond to estimates after 9, 10 and 11 years since high school graduation. These estimates of the treatment effect of the program are very similar to the estimates presented in Table 5. Columns 5-7 present the treatment estimates on employment. These estimates suggest somewhat larger effect on employment in comparison to the estimates presented in Table 5.

The obvious conclusion is that adding a control for parental earnings does not affect at all the treatment estimates of the effect of the PFP program on university schooling attainment and on labor market outcomes. This result is not unexpected given that the parental or family earnings controls are quite balanced between treatment and control, both in the pre and post-treatment cohorts (see estimates presented in panel E of Table 1).

Treatment Heterogeneity by Family Earnings and Gender

Next I estimate program treatment heterogeneity in university schooling, employment and earnings, by baseline family income. The possibility of a different program effect by family income has policy implications with respect to the targeted versus universal implementation of teachers' incentives programs, and for the external validity of our findings with respect to different socio-economic backgrounds of treated students. The sample is divided by the median of family income in 2000-2002. The estimates based on these two samples are presented in Table 8 for the NE sample. Columns 1-2 present the estimates for sub-samples by family income, panel A for the above median sample and panel B for the below median sample. The effect on university schooling is positive and significant for both groups but the effect in the high income sample is twice as large the effect in the lower income sample. The effect on earnings, however, is just about the same, though marginally higher for the lower income sample. The much larger effect on employment for the low family income group is what makes the income effect shortfall, due to the lower education gain of this group. The estimated increase in employment is 4.9 percentage points versus no employment effect at all for the higher family income sample. However, it is interesting to note that the increase in earnings for a unit gain in university schooling is the same for the high and low family income samples. The higher income sample had no gain in employment, therefore the earnings gain for every tenth of a year of schooling gain is 1492 NIS (4,447/2.98). The earnings gain due to the increase in employment for the sample of low family income is 3,445 NIS ((54,885/80.4) x 4.9)) and therefore the earnings gain per a

[12] The uncontrolled difference in differences estimates of the percentile rank regressions, not presented here for

tenth of a year increase in schooling is 1,483 NIS (1958/1.32), remarkably identical to 1,492 NIS, the figure for the high income sample. This means that the return to an increase in years of university schooling does not differ by family income, which implies that the increase in quality of schooling, both at high school and at university, is similar for both groups.

The estimated effects by gender are presented in columns 3-4, panel C for boys and panel D for girls. The effect on schooling is positive and significant for both genders, but it is higher for girls, a gain of a third of university year of schooling versus about half of that for boys. However, boys have a larger increase in earnings, by about 2,000 NIS a year, which is almost 30 percent of the total gain. Two explanations related to employment patterns can account for this difference. First, there is a positive effect of 1.7 percentage points on male employment (though this effect is not precisely measured) while the respective estimate for women is zero. This effect on male employment will account for a large part of the gender difference in earnings. Secondly, the lower gain in female earnings most likely reflects a much higher propensity among women to work part-time during this period in life. I cannot provide direct evidence on this second explanation because the data I use does not include information on hours of work. However, based on data from the 2012 Israeli Labor Force Survey, I estimated that in the age group 29-34 the rate of part time employment is 25 percent among women versus 8 percent among men.

Comparing the Effect on Earnings to Related Evidence

This is the first study to provide evidence on the effect of teachers' PFP on student earnings at adulthood. However, it is still useful to compare our results to the impact of other childhood and schooling interventions on earnings at adulthood. Andersson et al. (2013) estimated that the effects of living in public or voucher housing on later earnings are positive, substantial, and significant for non-Hispanic Black female teenagers, but living in public or voucher housing has no effect on the later earnings of non-Hispanic Black male teenagers. The point estimates suggest that females earn 21 percent more if they ever resided in voucher housing and 18 percent more if they ever resided in public housing. The corresponding estimates when treatment is measured as number of years indicate

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sake of space, are very similar to the control difference in differences estimates presented in Table7.

that each additional year of voucher-supported housing increases earnings by 7 percent for females, while each additional year of public housing also increases female earnings by 7 percent. The overall estimated treatment effects for males suggest that each year of public housing participation as a teenager increases adult earnings by 5 percent.

Gertler et al. (2014) report substantial effects on the earnings of participants in a randomized intervention conducted in 1986–1987 that gave psychosocial stimulation to growth-stunted Jamaican toddlers. The intervention consisted of weekly visits from community health workers over a 2-year period that taught parenting skills and encouraged mothers and children to interact in ways that develop cognitive and socioemotional skills. Twenty years later the intervention participants' earnings increased by 25 percent. Chetty et al. (2011) have shown that having a kindergarten teacher with more than ten years of experience increased students' average annual earnings at ages 25 to 27 by 6.9 percent ($1,093) between 2005 and 2007. Similarly, an improvement in class quality increased average annual income earned between ages 25 and 27. Johnson et al. (forthcoming QJE) show that for children from low-income families, increasing per-pupil spending by 10 percent in all 12 school-age years increased adult hourly wages by 13 percent. Schweinhart et al. analyze the long term effect of the High/Scope Perry Preschool experiment and find that students in treatment had significantly higher median annual earnings than the no-program group: 20 percent higher at age 27 and by 36 percent higher at age 40. Finally, Chetty, Hendren and Katz (2016) find that moving to a lower-poverty neighborhood (MTO) significantly improves college attendance rates – by 2.5 percent – and earnings by 31 percent, for children who were young (below age 13) when their families moved. Clearly our estimated effects on earnings are not unusually high relative to estimates surveyed above. For example, the teachers' pay experiment raised college enrollment by 5 percent, twice that of the MTO effect, and increased earnings 10-12 years after high school graduation by 7-9 percent, a fourth of the MTO effect.

Mechanisms for the Effect on Earnings

The direct effect of the program on high school outcomes, for example the increase in the average composite score in the matriculation exams and the increase in the matriculation diploma rate, could have caused the increase in earnings that we find. Lavy, Ebenstein and Roth (2014) and Ebenstein, Lavy and Roth (forthcoming) use random shocks to performance in matriculation exams to identify the reduced form effects of these high school outcomes on earnings at adulthood and find strong and significant positive effect. We still want, however, to understand the sources of the reduced form effect of 6 to 9 percentage points increase in earnings that we estimated in this paper. First, we should account for the contribution of the positive effect on employment to the increase in earnings. In the NE full sample, the employment gains accounts for 2 of the 9 percentage points increase and in the NE three quartile sample it accounts for 2 of the 7 percentage points increase in earnings. Similarly, in the RD three quartile sample it accounts for 1 of the 6 percentage-points increase in earnings. A second factor explaining earning growth is the increase in university years of schooling. Recent estimates of the rate of return to a year of university schooling in Israel range from 12 to 16 percent.[13] The lowest estimate (12%) implies that the 0.25 increase in years of university schooling contributed 3 percentage points to the gain in earnings. The highest estimate (16%) implies that the increase in university schooling accounts for 4 percentage points of the increase in earnings.

Another factor that accounts for part of the increase in earnings is the direct effect of the improved matriculation outcomes on earnings, independently of the effect they have on university years of schooling. Particularly important is the matriculation rate, which increased by 3.5-5 percentage points. The evidence suggests that having a matriculation diploma is rewarded in the labor market by a return beyond its effect on post-secondary schooling. For example, Angrist and Lavy (2009) estimate that *bagrut* holders earn 13 percent more than other individuals with exactly 12 years of schooling. Therefore, the matriculation rate accounts for almost 0.5 percentage points of the earnings gain in the NE full sample and 0.7 percentage points of the earnings gain in the NE three quartile sample. Similarly, the quality improvements in the matriculation study program (as reflected in the composite score, number of credit units and credits in honor and science subjects) are also

[13] Frish (2009) exploits changes in compulsory schooling laws and obtains IV estimates that are much larger than the OLS Mincerian estimates. Navon (2005) estimates that the return to an MA degree (two years of schooling) is 30 percent.

rewarded in the labor market beyond their effect on post-secondary schooling (Caplan et al (2009)).[14] The implied mechanism is that the improvements in high school educational outcomes that resulted from the PFP intervention gave students access to higher quality post-secondary education, mainly by facilitating enrollment in more selective programs that have a higher return to schooling. Examples of selective programs include computer science, engineering, and the top 2-3 law schools. We can partially assess this channel by checking the correlations between *bagrut* outcomes that measure quality and earnings. We estimate OLS regressions of annual earnings on the various high school *bagrut* outcomes, controlling for student's parental and demographic characteristics. We only use the sample of pre-program students in control schools, though the evidence is similar when using the sample of treated students. The results for the NE sample are presented in Table A6. Clearly each of the high school outcomes is highly and positively correlated with earnings at adulthood (panel A column 2). When all four high school outcomes are included jointly in the regression, they still have positive coefficients but only two of them are statistically significant: the average matriculation score and the number of honor level subjects (column 3). In panel B, I present similar evidence for the correlation between two post-secondary outcomes, enrollment in an academic college or university and the number of completed years of schooling in any of these institutions. Clearly both of these outcomes have a high correlation with earnings at adulthood (panel B column 2). When included jointly in the regression, they have still a positive coefficient but only the first two are statistically significant (panel B column 3). In column 4, I present the estimates from a regression when the high school and the post-secondary schooling outcomes are included jointly. All estimates are positive but the outcomes that are significant are the average matriculation score and the enrollment indicator in university and academic colleges. In Table A7, I present similar regressions for the two sub-samples by family income. The estimates in these two sub-samples are qualitatively similar.

Effect on marriage and children

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

I next examine teacher PFP treatment impacts on students' marriage and fertility outcomes in Table 9. I define these outcomes 11 years after graduating from high school because I only have data for these variables for 2012. Therefore, the two outcomes I examine are an indicator of being married and an indicator of having children, 11 years after high school graduation. About 58 percent of the students from the pre-treatment treated schools sample are married by 2011. The treatment effect on marriage rate based on the full NE sample is negative, but not significantly different from zero. This is evident in the full sample (NE sample) and in sub-samples by family income and by gender. Similarly, the estimated effect on having children, based on the full sample, is negative but very small and not statistically different from zero. In panel B I report results based on sub-samples by family income. For the sub-sample of low family income, the estimates on marriage and children are negative and the first effect is large and significant: a decline of 3.3 percentage points in the marriage rate and decline of 1.7 percentage points in the probability of having children 11 years after high school graduation, but the latter effect is not precisely measured. The two estimates for the high family income sample are small and not statistically significant. In panel C, I report the results by gender, and clearly the treatment effect on the two demographic outcomes is small and not distinguishable from zero. This implies that the teachers' PFP program reduced the marriage rate – probably by delaying it to a later age – among students from low income families, but not for others.

5. Conclusions

In this paper I study the long term effect of an experiment that paid teachers a bonus based on their students' performance in high stakes exams at the end of high school. All studies of teachers' incentive programs and the vast majority of published research on the impact of other school interventions has examined their effects on short-run outcomes, primarily by looking at their impact on standardized test scores. This study is the first to use a long horizon follow up, from high school to age 30, to examine impact of a teachers PFP scheme on long-run life outcomes. This analysis addresses the critical question of whether a public education intervention can achieve the ultimate goal of improving lifetime well-being. It also makes an important contribution to the growing literature on the long-term effects of education quality by providing evidence about an intervention that changes a specific input which can improve student achievement. Focusing on an intervention that can be expanded or implemented elsewhere, such as teachers' PFP, provides explicit guidance for educational policy making. This avenue of research is a natural follow up to recent studies that estimated a positive effect of teaching quality using teachers' fixed effect and value added models. However, explicit policy prescriptions for how to improve teaching quality do not follow immediately from this important evidence; the results presented in this paper help in this regard by unraveling 'wires' in the 'black box' of teachers quality.

This study shows that more than a decade after the initial intervention, treated individuals experienced sizable gains in schooling attainment and quality and large increases in annual earnings, some of which reflect a return to education quality beyond the return to years of schooling. These gains are very large relative to the cost of the program. The average cost of the program was $170 per student versus a gain of $1,000 in annual earnings starting at about age 28-30. A complete cost-benefit analysis should also take into account the forgone earnings during post-secondary education and tuition fees. However, given that individuals will benefit for many years from the increase in earnings, the present value of benefits clearly outweigh the cost, suggesting a high private rate of return. A social rate of return analysis of this project should take into account the cost of university schooling not recovered by tuition fees, and the additional tax revenue levied on higher earnings. Clearly, these adjustments will still yield a high social internal rate of return on this project.

Merit and incentive based pay for teachers is being contemplated or implemented in many countries, making the evidence in this paper relevant and important for education policy world wide. In U.S. education policy, for example, merit pay reforms for teachers have recently returned to the top of the policy agenda. In his first major education policy speech, President Obama promoted merit pay for teachers and in 2009 announced the Race to the Top, supported by $4.4 billion in federal funds, to encourage states to implement performance pay for teachers.[15] In a 2014 UK reform, teachers' annual salary increases have been tied to performance, replacing a system where almost all teachers automatically moved up a point on the pay scale every year. The move has been hugely controversial.

[15] Merit-Based Pay For Teachers | eduflow: https://eduflow.wordpress.com/2013/10/08/merit-based-pay-for-teachers.

For example, on March 26 2014 the National Union of Teachers struck in protest at the overhaul in pay structures that was due to begin later in the year.[16]

The intervention described in this paper targeted the period leading up to high-stake exams that play a key role in determining university and college admission. Since in this experiment the stakes were high both for students and teachers, it makes sense that the PFP intervention produced long-term results. However, if a similar program were introduced in a primary or middle school, the gains in test scores may not necessarily lead to similar long-term effects. Nevertheless, the evidence presented here is relevant for countries that use similar high stake high school exams for university admission.[17] Another point to note is that a PFP program when implemented at scale, for example nation-wide, will have general equilibrium implications. The scope of expansion of university enrollment estimated above will only be possible if the supply of post-secondary education can meet the increase in demand, as was the case in this study.

[16] The Economist, March 29 2014.

[17] High school exit exams play a similar role for university admission in many other countries. Those mentioned as examples in Wikepdia include the A level exams in the UK, the matriculation ('Bacalaureate') exams in Finland, Germany, Italy and Norway, the 'Matura exams' in Albania, Austria, Bosnia and Herzegovina, Bulgaria, Croatia, the Czech Republic, Hungary, Poland, Serbia, Slovakia, Slovenia, Switzerland, Ukraine, the Gaokao National Matriculation Examination in China and the General Exiting Exam (EGEL) in Mexico.

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Table 1: Post-Secondary Schooling, Employment and Income, and Demographic Statistics in 2012: The 2000 Cohort: The Natural Experiment Sample

	4 Quartiles Sample			3 Quartiles Sample		
Dependent variable	Treated schools (1)	Non treated Schools (2)	Difference (3)	Treated schools (4)	Non treated Schools (5)	Difference (6)
A. Enrollment in Post High School Education						
University	0.216	0.191	0.026	0.119	0.118	0.001
			(0.046)			(0.034)
Academic College	0.145	0.143	0.001	0.142	0.133	0.009
			(0.029)			(0.035)
B. Post High School Years of Schooling						
University	0.825	0.716	0.109	0.343	0.380	-0.037
			(0.211)			(0.122)
Academic College	0.379	0.365	0.014	0.372	0.328	0.044
			(0.080)			(0.093)
C. Employment Outcomes in 2012						
Employed (1 = Yes, 0 = No)	0.840	0.846	-0.006	0.837	0.860	-0.023
			(0.015)			(0.016)
Average Annual Earnings (NIS)	67,214	69,478	-2,265	63,456	66,565	-3,108
			(3,945)			(4,657)
Received Unemployment Insurance Benefits (1 = Yes, 0 = No)	0.080	0.087	-0.007	0.083	0.092	-0.009
			(0.011)			(0.014)
Total Unemployment Insurance Benefits Received (NIS)	911	941	-30	870	937	-67
			(138)			(158)
D. Demographic Outcomes						
Married	0.563	0.542	0.020	0.557	0.549	0.008
			(0.042)			(0.037)
Children (1 = Yes, 0 = No)	0.451	0.454	-0.003	0.455	0.468	-0.013
			(0.051)			(0.046)
Number of Children	0.836	0.792	0.044	0.845	0.834	0.011
			(0.127)			(0.125)
Age at Marriage	24.338	24.591	-0.253	24.314	24.467	-0.153
			(0.379)			(0.383)
Age at First Birth	25.302	25.467	-0.165	25.236	25.269	-0.033
			(0.357)			(0.353)
E. Parental Outcomes						
Average of Father Earnings in 2000-2002	102,212	96,212	6,001	86,975	85,776	1,199
			(16,693)			(12,666)
Average of Mother Earnings in 2000-2002	47,715	45,484	2,231	39,336	41,351	-2,014
			(7,798)			(6,096)
Number of Observations	2,424	2,703	5,127	1,704	2,020	3,724
Weighted Number of Observations	3,980	4,171	8,151	3,058	3,087	6,145

Notes: The table reports means and standard deviations for different post-secondary schooling, employment, income, and dempographic variables for the natural experiment sample described in the paper. Columns 1-3 report results for all four quartiles, and columns 4-6 report results for students who are in the lowest three quartiles of test grades. Panel A is comprised of binary variables indicating whether the individual has been enrolled or not to a specific type of post-secondary institution by 2012. The categories are not mutually exclusive and overlapping is possible. Panel B reports the number of years of education an individual has attained by 2012 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 2012. Panel D reports different demographic variables for the year 2012 in addition to the age at marriage and the age at first birth. Panel E reports different parental variables. Standard errors in parenthesis are adjusted for school level clustering. Number of observations does not apply to the the age at marriage and the age at first birth variables, as these are computed on a sub-sample of individuals that are married/have children.

Table 2: Post-Secondary Schooling, Employment and Income, and Demographic Statistics in 2012: The 2000 Cohort: The Regression Discontinuity Sample

Dependent variable	4 Quartiles Sample: Treated schools (1)	4 Quartiles Sample: Non treated Schools (2)	4 Quartiles Sample: Difference (3)	3 Quartiles Sample: Treated schools (4)	3 Quartiles Sample: Non treated Schools (5)	3 Quartiles Sample: Difference (6)
A. Enrollment in Post High School Education						
University	0.209	0.148	0.061	0.116	0.084	0.032
			(0.039)			(0.023)
Academic College	0.160	0.127	0.033	0.162	0.112	0.050
			(0.032)			(0.035)
B. Post High School Years of Schooling						
University	0.793	0.523	0.270	0.333	0.249	0.084
	(0.155)	(0.100)	(0.184)			(0.080)
Academic College	0.423	0.317	0.106	0.422	0.274	0.148
			(0.084)			(0.093)
C. Employment Outcomes in 2012						
Employed (1 = Yes, 0 = No)	0.843	0.844	-0.001	0.842	0.858	-0.016
			(0.017)			(0.019)
Average Annual Earnings (NIS)	70,091	67,389	2,701	66,275	64,249	2,026
			(3,841)			(4,744)
Received Unemployment Insurance Benefits (1 = Yes, 0 = No)	0.080	0.091	-0.011	0.086	0.100	-0.014
			(0.013)			(0.015)
Total Unemployment Insurance Benefits Received (NIS)	890	967	-77	889	1,026	-137
			(165)			(178)
D. Demographic Outcomes						
Married	0.548	0.558	-0.010	0.551	0.556	-0.005
			(0.050)			(0.047)
Children (1 = Yes, 0 = No)	0.427	0.487	-0.060	0.439	0.493	-0.054
			(0.053)			(0.053)
Number of Children	0.760	0.830	-0.070	0.789	0.864	-0.075
			(0.138)			(0.150)
Age at Marriage	24.586	24.495	0.090	24.521	24.349	0.172
			(0.409)			(0.415)
Age at First Birth	25.523	25.405	0.117	25.451	25.165	0.287
			(0.415)			(0.402)
E. Parental Outcomes						
Average of Father Earnings in 2000-2002	103,816	81,924	21,892	91,632	75,312	16,321
			(16,668)			(13,093)
Average of Mother Earnings in 2000-2002	49,082	39,383	9,699	43,912	36,496	7,416
			(6,945)			(5,935)
Number of Observations	1,697	2,471	4,168	1,257	1,844	3,101
Weighted Number of Observations	2,843	3,064	5,907	2,281	2,246	4,527

Notes: The table reports means and standard deviations for different post-secondary schooling, employment, income, and dempographic variables for the natural experiment sample described in the paper. Columns 1-3 report results for all four quartiles, and columns 4-6 report results for students who are in the lowest three quartiles of test grades. Panel A is comprised of binary variables indicating whether the individual has been enrolled or not to a specific type of post-secondary institution by 2012. The categories are not mutually exclusive and overlapping is possible. Panel B reports the number of years of education an individual has attained by 2012 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 2012. Panel D reports different demographic variables for the year 2012 in addition to the age at marriage and the age at first birth. Panel E reports different parental variables. Standard errors in parenthesis are adjusted for school level clustering. Number of observations does not apply to the the age at marriage and the age at first birth variables, as these are computed on a sub-sample of individuals that are married/have children.

Table 3: Differences-in-Differences Estimates of the Effect of Teachers' Bonuses Program on Post-Secondary Schooling

	The Natural Experiment Sample				**The Regression Discontinuity Sample Sample**			
	Enrollment in Post-Secondary Schooling		**Post-Secondary Years of Schooling**		**Enrollment in Post-Secondary Schooling**		**Post-Secondary Years of Schooling**	
	12 Years After High-School Graduation Outcomes		**12 Years After High-School Graduation Outcomes**		**12 Years After High-School Graduation Outcomes**		**12 Years After High-School Graduation Outcomes**	
	2000 Cohort in Treated Schools	Estimate	2000 Cohort in Treated Schools	Estimate	2000 Cohort in Treated Schools	Estimate	2000 Cohort in Treated Schools	Estimate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. 4 Quartiles Sample								
University	0.216	0.048	0.825	0.250	0.209	0.060	0.793	0.242
	(0.412)	(0.013)	(1.877)	(0.067)	(0.407)	(0.014)	(1.856)	(0.073)
Academic College	0.145	-0.026	0.379	-0.072	0.160	-0.017	0.423	-0.047
	(0.352)	(0.014)	(1.051)	(0.037)	(0.367)	(0.018)	(1.107)	(0.046)
Number of Observations	2,703	10,077	2,703	10,077	2,471	8,230	2,471	8,230
Weighted Number of Observations	4,171	15,903	4,171	15,903	3,064	11,561	3,064	11,561
B. 3 Quartiles Sample								
University	0.119	0.052	0.343	0.239	0.116	0.061	0.333	0.226
	(0.324)	(0.012)	(1.115)	(0.043)	(0.320)	(0.014)	(1.130)	(0.053)
Academic College	0.142	-0.011	0.372	-0.028	0.162	-0.014	0.422	-0.019
	(0.349)	(0.013)	(1.046)	(0.030)	(0.369)	(0.017)	(1.106)	(0.036)
Number of Observations	2,020	7,382	2,020	7,382	1,844	6,161	1,844	6,161
Weighted Number of Observations	7,382	11,952	7,382	11,952	2,246	8,801	2,246	8,801

Notes : This table presents the differences-in-differences estimates of the effect of the School Choice program on post-secondary schooling 12 years after high-school graduatoin. Panel A and Panel B report the results for the three quartile and four quartile samples described in the paper. Columns 1-4 report the results for the natural experiment sample described in the paper, and columns 5-8 report the results for the regression discontinuity sample described in the paper. Columns 1,3,5, and 7 represent the mean and standard error for the 2000 cohort in the treated schools. These cohorts did not receive the treatment so it is useful to compare their averages as a benchmark for the treatment effect. Columns 2,4,6, and 8 report the Differences-in-Differences estimate for each of the dependent variables. Standard errors are clustered at the school year level.

Table 4: Differences-in-Differences Estimates of the Effect of The Teachers' Bonuses Program on Employment and Income

	The Natural Experiment Sample				The Regression Discontinuity Sample			
	11 Years After High-School Graduation Outcomes		9-11 Years After High-School Graduation Outcomes Stacked Outcomes		11 Years After High-School Graduation Outcomes		9-11 Years After High-School Graduation Outcomes Stacked Outcomes	
	2000 Cohort in Treated Schools	Estimate	2000 Cohort in Treated Schools	Estimate	2000 Cohort in Treated Schools	Estimate	2000 Cohort in Treated Schools	Estimate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. 4 Quartiles Sample								
Employment Indicator (1 = Yes, 0 = No)	0.841	0.010	0.830	0.013	0.842	0.008	0.832	0.012
	(0.366)	(0.009)	(0.376)	(0.008)	(0.365)	(0.007)	(0.373)	(0.007)
Total Annual Earnings (NIS)	64,993	5,851	55,311	4,714	66,903	6,731	56,655	4,860
	(56,317)	(1,993)	(49,396)	(1,519)	(57,493)	(2,481)	(50,032)	(1,857)
Received Unemployment Insurance Benefits Indicator (1 = Yes, 0 = No)	0.068	0.000	0.070	-0.002	0.069	-0.004	0.071	-0.005
	(0.252)	(0.011)	(0.255)	(0.006)	(0.254)	(0.014)	(0.257)	(0.008)
Total Unemployment Insurance Benefits Received (NIS)	693	37	597	28	725	-112	602	-14
	(3,076)	(114)	(2,699)	(60)	(3,193)	(134)	(2,694)	(82)
Number of Observations	2,703	10,077	8,109	30,231	2,471	8,230	7,413	24,690
Weighted Number of Observations	4,171	15,903	12,513	47,709	3,064	11,561	9,192	34,683
B. 3 Quartiles Sample								
Employment Indicator (1 = Yes, 0 = No)	0.827	0.024	0.819	0.020	0.831	0.019	0.825	0.014
	(0.378)	(0.013)	(0.385)	(0.010)	(0.375)	(0.012)	(0.380)	(0.010)
Total Annual Earnings (NIS)	61,919	4,982	53,438	3,869	64,158	5,190	55,187	3,567
	(52,965)	(1,513)	(46,647)	(1,435)	(54,107)	(1,457)	(47,487)	(1,524)
Received Unemployment Insurance Benefits Indicator (1 = Yes, 0 = No)	0.068	0.003	0.073	-0.002	0.057	-0.003	0.057	-0.007
	(0.252)	(0.012)	(0.260)	(0.008)	(0.231)	(0.015)	(0.231)	(0.010)
Total Unemployment Insurance Benefits Received (NIS)	700	114	619	37	520	-73	429	-18
	(3,137)	(133)	(2,750)	(67)	(2,570)	(147)	(2,133)	(90)
Number of Observations	2,020	7,382	6,060	22,146	1,844	6,161	5,532	18,483
Weighted Number of Observations	3,087	11,952	9,261	35,856	2,246	8,801	6,738	26,403

Notes : This table presents the differences-in-differences estimates of the effect of theTeachers' Bonuses program on different employment and income outcomes. Panel A and Panel B report the results for the three quartile and four quartile samples described in the paper. Columns 1-4 report the results for the natural experiment sample described in the paper, and columns 5-8 report the results for the regression discontinuity sample described in the paper. Columns 1-2 and 5-6 report results for 11 years after high-school graduation, and columns 3-4 and 7-8 report results for the stacked outcomes of 9-11years after high-school graduation. The variable "Employment Indicator" receives the value of 1 is the individual has any work record for the given year and 0 otherwise. The variable "Received Unemployment Insurance Benefits Indicator" Receives the value of 1 if the 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. Average Annual Earnings measure the total NIS amount of earnings the individual received in the given year. Columns 1,3, 5, and 7 report the mean and standard error for the 2000 cohort in the treated schools. This cohort did not receive the treatment so it is useful to compare their averages as a benchmark for the treatment effect. Columns 2,4, 6, and 8 report the Differences-in-Differences estimate for each of the dependent variables listed above. Standard errors are clustered at the school year level.

Table 5: Differences-in-Differences Estimates of the Effect of The Teachers' Bonuses Program on Percentile Ranking of Income

	The Natural Experiment Sample				The Regression Discontinuity Sample			
	11 Years After High-School Graduation Outcomes		9-11 Years After High-School Graduation Outcomes Stacked Outcomes		11 Years After High-School Graduation Outcomes		9-11 Years After High-School Graduation Outcomes Stacked Outcomes	
	2000 Cohort in Treated Schools	Estimate	2000 Cohort in Treated Schools	Estimate	2000 Cohort in Treated Schools	Estimate	2000 Cohort in Treated Schools	Estimate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. 4 Quartiles Sample								
Total Annual Earnings (NIS)	48.777	2.638	48.3	2.041	49.814	3.018	49.0	1.972
	(30.337)	(0.863)	(30.3)	(0.794)	(30.547)	(1.019)	(30.5)	(0.899)
Number of Observations	2,703	10,077	8,109	30,231	2,471	8,230	7,413	24,690
Weighted Number of Observations	4,171	15,903	12,513	47,709	3,064	11,561	9,192	34,683
B. 3 Quartiles Sample								
Total Annual Earnings (NIS)	47.164	2.181	47.5	1.533	48.174	2.334	48.5	1.256
	(29.975)	(0.836)	(30.3)	(0.892)	(30.113)	(0.832)	(30.3)	(0.938)
Number of Observations	2,020	7,382	6,060	22,146	1,844	6,161	5,532	18,483
Weighted Number of Observations	3,087	11,952	9,261	35,856	2,246	8,801	6,738	26,403

Notes : This table presents the differences-in-differences estimates of the effect of theTeachers' Bonuses program on percentile ranking income outcomes. Percentile ranking of income are assigned within each cohort and are lagged to be age-adjusted. Panel A and Panel B report the results for the three quartile and four quartile samples described in the paper. Columns 1-4 report the results for the natural experiment sample described in the paper, and columns 5-8 report the results for the regression discontinuity sample described in the paper. Columns 1-2 and 5-6 report results for 11 years after high-school graduation, and columns 3-4 and 7-8 report results for the stacked outcomes of 9-11years after high-school graduation. Average Annual Earnings measure the total NIS amount of earnings the individual received in the given year. Columns 1,3, 5, and 7 report the mean and standard error for the 2000 cohort in the treated schools. This cohort did not receive the treatment so it is useful to compare their averages as a benchmark for the treatment effect. Columns 2,4, 6, and 8 report the Differences-in-Differences estimate for each of the dependent variables listed above. Standard errors are clustered at the school year level.

Table 6: Differences-in-Differences Estimates of the Effect of The Teachers' Bonuses Program by Family Income and Gender, 11 Years After High-School Graduation - The Natural Experiment Sample

	2000 Cohort in Treated Schools	Estimate	2000 Cohort in Treated Schools	Estimate
	(1)	(2)	(3)	(4)
	A. High Family Income		**C. Boys**	
University Enrollment	0.246	0.078	0.176	0.028
	(0.431)	(0.027)	(0.381)	(0.015)
University Years of Schooling	0.953	0.298	0.647	0.161
	(1.969)	(0.103)	(1.663)	(0.060)
Employment Indicator (1 = Yes, 0 = No)	0.868	0.002	0.860	0.017
	(0.339)	(0.012)	(0.348)	(0.015)
Total Annual Earnings (NIS)	69,678	4,477	73,557	6,778
	(57,684)	(1,930)	(59,466)	(3,416)
Number of Observations	1,481	5,891	1,353	4,953
	B. Low Family Income		**D. Girls**	
University Enrollment	0.127	0.027	0.209	0.066
	(0.333)	(0.014)	(0.407)	(0.028)
University Years of Schooling	0.451	0.132	0.804	0.333
	(1.423)	(0.041)	(1.852)	(0.117)
Employment Indicator (1 = Yes, 0 = No)	0.804	0.049	0.819	0.004
	(0.397)	(0.015)	(0.386)	(0.013)
Total Annual Earnings (NIS)	54,885	5,403	52,400	4,681
	(50,950)	(1,654)	(48,378)	(2,226)
Number of Observations	1,222	4,186	1,350	5,124

Notes: This table presents the differences-in-differences estimates of the effect of theTeachers' Bonuses program on employment, income, university enrollment and university years of schooling by family income and by gender, for the natural experiment sample described in the paper. Columns 1-2 report the results for the high family income sample and the low family income sample. Individuals included in the high family income sample are those who come from families in which the average household income in 2000-2002 is higher than the mean average household income in the same years. Columns 3-4 report the results for boys and for girls. The variable "Employment Indicator" receives the value of 1 is the individual has any work record for the given year and 0 otherwise. Columns 1 and 3 report the mean and standard error for the 2000 cohort in the treated schools. This cohort did not receive the treatment so it is useful to compare their averages as a benchmark for the treatment effect. Columns 2 and 4 report the Differences-in-Differences estimate for each of the dependent variables listed above. Standard errors are clustered at the school year level.

Table 7: OLS Relationships between High School Matriculation Outcomes, College Schooling, and Earnings at Adulthood: : The Natural Experiment Sample - Treated Schools

		Annual Earnings - 11 Years After High-School Graduation		
	2000 Cohort in Treated Schools	Separate Estimate	Joint Estimate Panel A\B	Joint Estimate Panels A + B
	(1)	(2)	(3)	(4)
A. High School Matriculation Outcomes				
Average Matriculation Score	73	321	221	205
	(23)	(62)	(65)	(67)
Received High School Matriculation (1 = Yes, 0 = No)	0.538	9,099	4,360	3,787
	(0.499)	(2,352)	(2,628)	(2,611)
Number of Credit Units in Matriculation Exams	22	600	41	86
	(10)	(131)	(159)	(155)
Number of Honor Level Subjects	2.517	3,579	1,744	1,040
	(1.820)	(916)	(1,211)	(1,222)
B. Post Secondary Schooling				
Enrollment in University or Academic College Throughout 11 Years After High-School Graduation (1 = Yes, 0 = No)	0.318	10,577	8,431	7,073
`	(0.466)	(2,419)	(3,462)	(3,212)
Completed Years of University or Academic College Throughout 11 Years After High-School Graduation	1.138	2,213	648	397
	(1.984)	(770)	(1,177)	(1,166)
Number of Observations	2,703	5,344	5,344	5,344
Number of Weighted Observations	4,171	7,995	7,995	7,995

Notes : This table presents OLS relationships between high school matriculation outcomes, college schooling, and earnings at adulthood for the natural experiment four-quartile treated schools sample described in the paper. Column 1 reports means and standard deviations for the 2000 cohort in treated schools. Column 2 represents the OLS estimate of a regression where the dependent variable is the annual wage 11 years after high-school graduation, and the independent variables include the same variables as reported in the paper in addition to the outcome variable described in the table. Column 3 reports 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 described in the paper. Column 4 reports 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 year level.

Table 8: Differences-in-Differences Estimates of the Effect of the Teachers' Bonuses Program on Demographic Outcomes 11 Years After High-School Graduation - The Natural Experiment Sample

	Married		**Children**	
	2000 Cohort in Treated Schools	Estimate	2000 Cohort in Treated Schools	Estimate
	(1)	(2)	(3)	(4)
A. Full Sample				
	0.584	-0.011	0.451	-0.003
	(0.493)	(0.013)	(0.498)	(0.011)
B. By Family Income				
High Family Income	0.554	-0.003	0.407	0.006
	(0.497)	(0.019)	(0.491)	(0.015)
Low Family Income	0.621	-0.033	0.506	-0.017
	(0.485)	(0.017)	(0.500)	(0.018)
C. By Gender				
Boys	0.493	-0.005	0.338	-0.003
	(0.500)	(0.020)	(0.473)	(0.018)
Girls	0.677	-0.007	0.569	-0.001
	(0.468)	(0.016)	(0.495)	(0.016)

Notes : This table presents the differences-in-differences estimates of the effect of the Teachers' Bonuses program on different demographic rate outcomes 11 years after high-school graduation for the natural experiment sample described in the paper. Panel A reports the results for the full sample, Panel B reports the results for the high and low family income samples, and Panel C reports the results by gender. Columns 1-2 report the results for the variable "Married"", which receives the value of 1 is the individual is married 11 years after graduation, 0 otherwise. Columns 3-4 report the results for the variable "Children", which receives the value of 1 is the individual has any children by 11 years after graduation, 0 otherwise. Columns 1 and 3 report the mean and standard error for the 2000 cohort in the treated schools. This cohort did not receive the treatment so it is useful to compare its' average as a benchmark for the treatment effect. Columns 2 and 4 report the Differences-in-Differences estimate for each of the dependent variables listed above. Standard errors are clustered at the school year level.

Table A1 - Treatment-Control Balancing Tests: The Natural Experiment Sample

	2000			2001		
	Treatment	Control	Difference	Treatment	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
			A. School characteristics			
Religious school	0.330	0.219	0.110	0.324	0.214	0.110
			(0.163)			(0.164)
Arab school	0.158	0.000	0.158	0.155	0.000	0.155
			(0.088)			(0.087)
Lagged "Bagrut" rate	0.467	0.509	-0.042	0.474	0.475	-0.001
			(0.032)			(0.053)
Two-years lagged Bagrut rate	0.490	0.519	-0.029	0.527	0.528	-0.002
			(0.049)			(0.034)
			B. Student background			
Father education	10.685	10.586	0.100	10.539	10.332	0.207
			(0.821)			(0.838)
Mother education	10.624	10.764	-0.140	10.519	10.539	-0.020
			(0.849)			(0.947)
Number of siblings	3.009	2.026	0.983	2.912	1.662	1.250
			(0.410)			(0.384)
Gender (male=1)	0.513	0.414	0.098	0.556	0.431	0.125
			(0.066)			(0.061)
Immigrant	0.016	0.029	-0.013	0.025	0.012	0.013
			(0.017)			(0.018)
Asia-Africa ethnicity	0.218	0.325	-0.107	0.235	0.276	-0.041
			(0.062)			(0.054)
			C. Student lagged outcomes			
Math credits gained	0.337	0.277	0.061	0.256	0.453	-0.197
			(0.172)			(0.118)
English credits gained	0.155	0.077	0.078	0.107	0.079	0.028
			(0.051)			(0.061)
Total credits attempted	5.251	4.594	0.657	5.322	5.342	-0.020
			(0.674)			(0.498)
Total credits gained	4.308	3.761	0.547	4.218	4.482	-0.264
			(0.601)			(0.393)
Average score	63.131	64.774	-1.643	62.121	67.710	-5.589
			(2.591)			(2.217)
# obs	2,654	2,369	5,023	2,598	2,236	4,834
# obs, weighted	4,095	3,818	7,913	3,812	3,679	7,491
# schools	18	18	36	18	18	36

Notes: Standard errors in parenthesis are adjusted for school level clustering.
* Observations were weighted with frequency weights in order to have similar number of students in control and treatment schools within each group of schools with close true matriculation rate.
* The schools status of nationality and religiosity does not change. Therefore, any change in the means across years reflects relative changes in the number of students in a cohort.
* This table is based on the math sample.

Table A2 - Treatment-Control Balancing Tests: The Regression Discontinuity Sample

	2000			2001		
	Treatment	Control	Difference	Treatment	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
			A. School characteristics			
Religious school	0.100	0.301	-0.201	0.095	0.290	-0.195
			(0.142)			(0.140)
Arab school	0.131	0.000	0.131	0.132	0.000	0.132
			(0.094)			(0.096)
Lagged "Bagrut" rate	0.448	0.495	-0.047	0.458	0.470	-0.012
			(0.017)			(0.041)
			B. Student background			
Father education	11.027	10.219	0.808	10.835	10.081	0.753
			(0.591)			(0.643)
Mother education	11.095	10.526	0.570	11.027	10.527	0.501
			(0.659)			(0.711)
Number of siblings	2.622	2.288	0.335	2.605	1.902	0.703
			(0.352)			(0.383)
Gender (male=1)	0.493	0.425	0.068	0.499	0.451	0.048
			(0.058)			(0.052)
Immigrant	0.014	0.045	-0.031	0.013	0.009	0.004
			(0.021)			(0.007)
Asia-Africa ethnicity	0.215	0.313	-0.097	0.214	0.273	-0.060
			(0.052)			(0.054)
			C. Student lagged outcomes			
Math credits gained	0.185	0.364	-0.180	0.185	0.452	-0.267
			(0.131)			(0.128)
English credits gained	0.207	0.053	0.155	0.183	0.101	0.083
			(0.061)			(0.088)
Total credits attempted	4.788	4.944	-0.156	5.064	5.346	-0.283
			(0.476)			(0.489)
Total credits gained	4.008	4.066	-0.058	4.188	4.394	-0.206
			(0.376)			(0.384)
Average score	61.671	64.548	-2.877	61.797	65.770	-3.973
			(2.932)			(1.973)
# obs	2,471	1,638	4,109	2,401	1,519	3,920
# schools	14	13	27	14	13	27

Notes: Standard errors in parenthesis are adjusted for school level clustering.
* The schools status of nationality and religiosity does not change. Any change in the means across years reflects relative changes in the number of students in a cohort.
* This table is based on the math sample.

Table A3: Differences-in-Differences Estimates of the Effect of Teachers' Bonuses Program on High School Education Outcomes

Sample	4 Quartiles		3 Quartiles	
	Mean 2000 Cohort in Treated Schools	Treatment Estimate	Mean 2000 Cohort in Treated Schools	Treatment Estimate
	(1)	(2)	(3)	(4)
Average Matriculation Score	72.926	2.779	66.537	2.868
	(23.098)	(0.892)	(22.599)	(1.017)
Received High School Matriculation (1 = Yes, 0 = No)	0.532	0.036	0.423	0.055
	(0.499)	(0.020)	(0.494)	(0.023)
Number of Credit Units in Matriculation Exams	22.199	0.803	20.205	0.669
	(10.257)	(0.334)	(10.238)	(0.329)
Number of Science Credit Units in Matriculation Exams	2.339	0.589	1.340	0.343
	(3.813)	(0.181)	(2.910)	(0.154)
Number of Honor Level Subjects	2.491	0.128	2.034	0.092
	(1.821)	(0.062)	(1.631)	(0.065)
Number of Observations	4,162	16,031	3,072	11,921

Notes : This table presents the differences-in-differences estimates of the effect of the Teachers' Bonuses program on high-school educational outcomes fo the three and four quartiles samples described in the paper. Columns 1 and 3 report the means and standard deviations for the 2000 cohort in the treated schools. This cohort did not receive the treatment so it is useful to compare its' average as a benchmark for the treatment effect. Columns 2 and 4 report the differences-in-differences estimates for each of the dependent variables. Standard errors are clustered at the school year level.

Table A4: Estimates of Parental Earnings Controls Added to the Basic University Enrollment and Years of Schooling Difference-in-Difference Model - The Natural Experiment Sample

	University Enrollment, Years After High School Graduation			**University Years of Schooling, Years After High School Graduation**		
	10	**11**	**12**	**10**	**11**	**12**
	Main Effect	Main Effect	Main Effect	Main Effect	Main Effect	Main Effect
	(1)	(2)	(3)	(4)	(5)	(6)
A. Main Effect						
Father's Average Earning 2000-2002	0.048	0.045	0.044	0.229	0.237	0.237
	(0.016)	(0.015)	(0.014)	(0.072)	(0.073)	(0.075)
Mother's Average Earning 2000-2002	0.048	0.044	0.042	0.228	0.234	0.233
	(0.013)	(0.012)	(0.012)	(0.058)	(0.059)	(0.061)
Household's Average Earning 2000-2002	0.048	0.044	0.042	0.220	0.227	0.225
	(0.015)	(0.014)	(0.013)	(0.066)	(0.067)	(0.069)

Notes: This table presents the differences-in-differences estimates of the effect of the Teachers' Bonuses program on post-secondary education outcomes for the natural experiment sample described in the paper, when controling for parental income. Panel A reports the estimates of the main treatment effect from the differences-in-differences model describes in Table 4, to which each of these parental income controls are separately added. Columns 1 and 5 report results for 10 years after high-school graduation, columns 2 and 6 report results for 11 years after high-school graduation, and columns 3 and 7 report results for 12 years after high-school graduation. Standard errors are clustered at the school year level.

Table A5: Estimates of Parental Earnings Controls Added to the Basic Earnings and Employment Difference-in-Difference Model - The Natural Experiment Sample

	Earnings, Years After High School Graduation				Employment, Years After High School Graduation		
	9	**10**	**11**	**9-11 Years After Graduation - Stacked**	**9**	**10**	**11**
	Main Effect	Main Effect	Main Effect	Main Effect	Main Effect	Main Effect	Main Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Main Effect							
Father's Average Earning 2000-2002	5,203	3,974	6,174	5,105	0.025	0.010	0.018
	(1,377)	(1,852)	(2,137)	(1,588)	(0.011)	(0.009)	(0.009)
Mother's Average Earning 2000-2002	4,837	2,970	5,676	4,532	0.025	0.001	0.012
	(1,289)	(1,965)	(2,027)	(1,558)	(0.011)	(0.009)	(0.009)
Household's Average Earning 2000-2002	5,220	3,706	6,055	4,983	0.026	0.009	0.017
	(1,415)	(1,846)	(2,187)	(1,627)	(0.011)	(0.009)	(0.009)

Notes: This table presents the differences-in-differences estimates of the effect of the Teachers' Bonuses program on earnings and employment outcomes, when controling for parental income. Panel A reports the estimates of the main treatment effect from the differences-in-differences model describes in Table 4, to which each of these parental income controls are separately added. Columns 1 and 5 report results for 9 years after high-school graduation, columns 2 and 6 report results for 10 years after high-school graduation, and columns 3 and 7 report results for 11 years after high-school graduation. Column 4 report results for the stacked earnings of 9-11 years after high-school graduation. The variable "Employment Indicator" receives the value of 1 is the individual has any work record for the given year and 0 otherwise. Standard errors are clustered at the school year level.

Table A6: OLS Relationships between High School Matriculation Outcomes, College Schooling, and Earnings at Adulthood by Family Income: The Natural Experiment Sample - Treated Schools

	High Family Income				Low Family Income			
		Annual Earnings - 11 Years After High-School Graduation				Annual Earnings - 11 Years After High-School Graduation		
	2000 Cohort in Treated Schools	Separate Estimate	Joint Estimate Panel A\B	Joint Estimate Panels A + B	2000 Cohort in Treated Schools	Separate Estimate	Joint Estimate Panel A\B	Joint Estimate Panels A + B
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. High School Matriculation Outcomes								
Average Matriculation Score	78	377	305	295	68	292	172	150
	(20)	(113)	(117)	(124)	(25)	(68)	(69)	(67)
Received High School Matriculation (1 = Yes, 0 = No)	0.608	6,911	2,751	2,348	0.452	11,199	5,608	4,887
	(0.488)	(2,702)	(3,466)	(3,560)	(0.498)	(2,792)	(3,065)	(2,883)
Number of Credit Units in Matriculation Exams	24	436	-104	-94	20	747	184	286
	(09)	(208)	(238)	(232)	(11)	(139)	(245)	(228)
Number of Honor Level Subjects	2.869	2,773	1,560	1,266	2.078	4,359	1,827	590
	(1.804)	(1,310)	(1,749)	(1,811)	(1.743)	(1,020)	(1,522)	(1,411)
B. Post Secondary Schooling								
Enrollment in University or Academic College Throughout 11 Years After High-School Graduation (1 = Yes, 0 = No)	0.409	6,861	7,267	6,429	0.204	15,926	8,126	6,182
`	(0.492)	(2,982)	(3,948)	(3,907)	(0.403)	(4,096)	(7,073)	(6,630)
Completed Years of University or Academic College Throughout 11 Years After High-School Graduation	1.506	1,164	-120	-477	0.677	4,140	2,476	2,323
	(2.186)	(1,064)	(1,569)	(1,595)	(1.582)	(1,087)	(1,856)	(1,823)
Number of Observations	1,481	2,958	2,958	2,958	1,222	2,386	2,386	2,386
Number of Weighted Observations	2,317	4,464	4,464	4,464	1,854	3,531	3,531	3,531

Notes : This table presents OLS relationships between high school matriculation outcomes, college schooling, and earnings at adulthood for the natural experiment four-quartile treated schools sample described in the paper by family income. Columns 1-4 reports the results for individuals who come from high income families. Individuals included in this sample are those who come from families in which the average household income in 2000-2002 is higher than the mean average household income in the same years. Columns 5-8 report the results for individuals who come from low income families. Individuals included in this sample are those who come from families in which the average household income in 2000-2002 is lower than the mean average household income in the same years. Columns 1 and 5 report means and standard deviations for the 2000 cohort in treated schools. Columns 2 and 6 represents the OLS estimate of a regression where the dependent variable is the annual wage 11 years after high-school graduation, and the independent variables include the same variables as reported in the paper in addition to the outcome variable described in the table. Columns 3 and 7 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 described in the paper. Columns 4 and 8 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 year level.

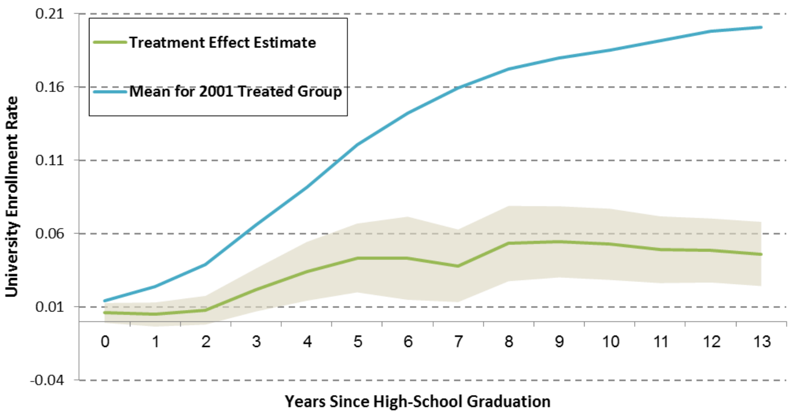


Figure1: Mean and Treatment Effect: University Enrollment (Full Sample, Natural Experiment Sample)

*Shaded area indicates two sided confidence intervals, 10% significance level.

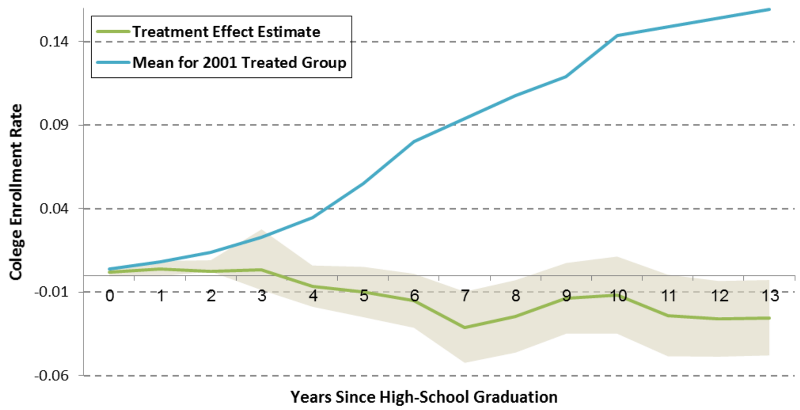


Figure 2: Mean and Treatment Effect: College Enrollment (Full Sample, Natural Experiment Sample)

*Shaded area indicates two sided used confidence intervals, 10% significance level.

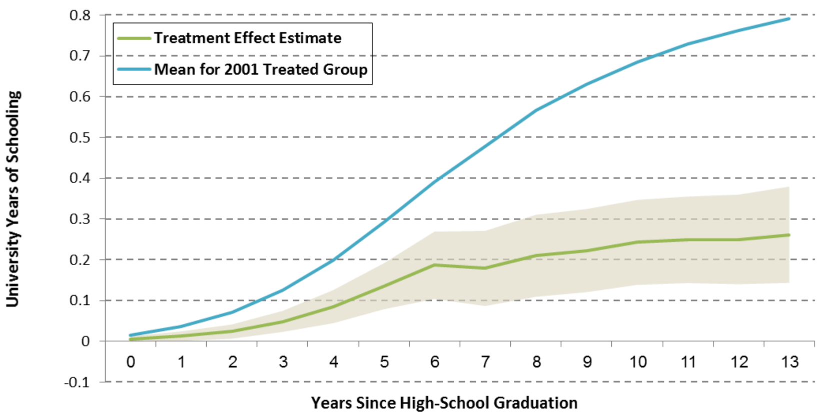


Figure 1A: Mean and Treatment Effect: University Years of Schooling (Full Sample, Natural Experiment Sample)

*Shaded area indicates two sided used confidence intervals, 10% significance level.

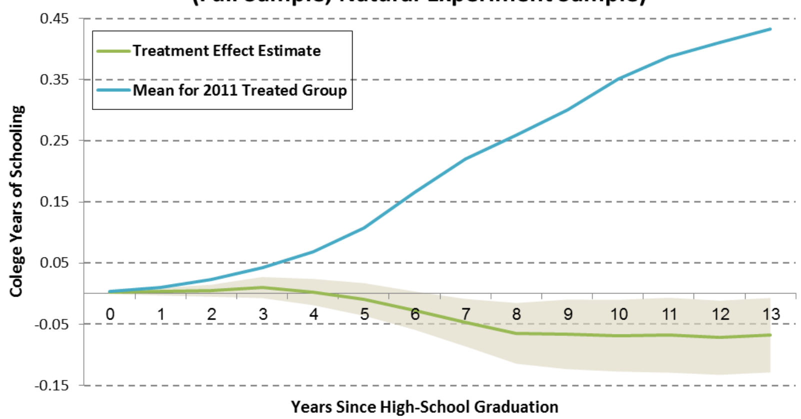
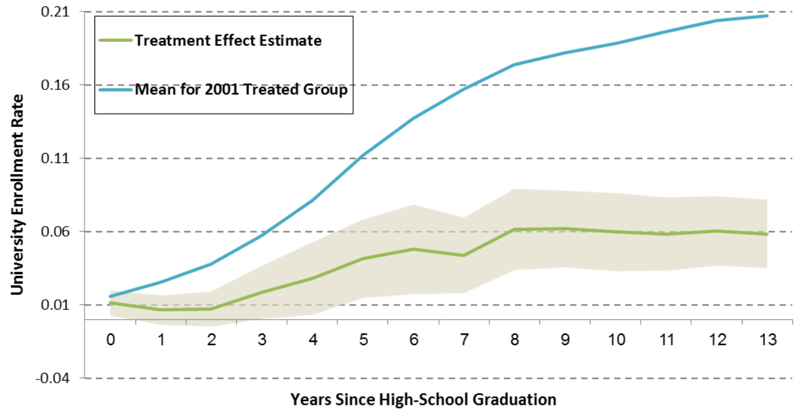


Figure 2A: Mean and Treatment Effect: College Years of Schooling (Full Sample, Natural Experiment Sample)

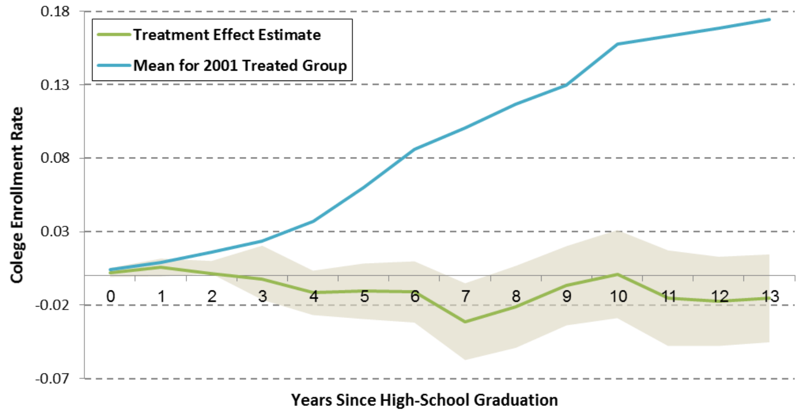
*Shaded area indicates two sided used confidence intervals, 10% significance level.

Figure 3: Mean and Treatment Effect: University Enrollment (Full Sample, Regression Discontinuity Sample)



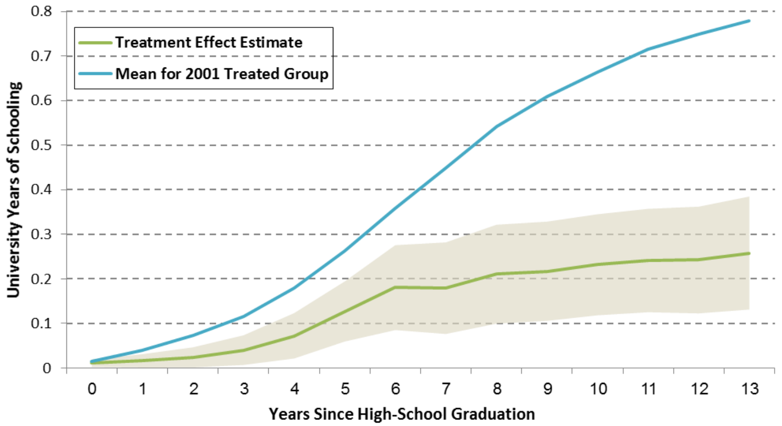
*Shaded area indicates two sided confidence intervals, 10% significance level.

Figure 4: Mean and Treatment Effect: College Enrollment (Full Sample, Regression Discontinuity Sample)



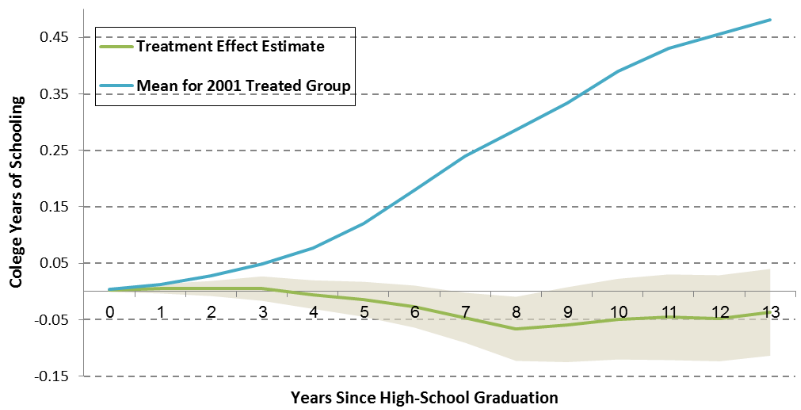
*Shaded area indicates two sided used confidence intervals, 10% significance level.

Figure 3A: Mean and Treatment Effect: University Years of Schooling (Full Sample, Regression Discontinuity Sample)



*Shaded area indicates two sided used confidence intervals, 10% significance level.

Figure 4A: Mean and Treatment Effect: College Years of Schooling (Full Sample, Regression Discontinuity Sample)



*Shaded area indicates two sided used confidence intervals, 10% significance level.

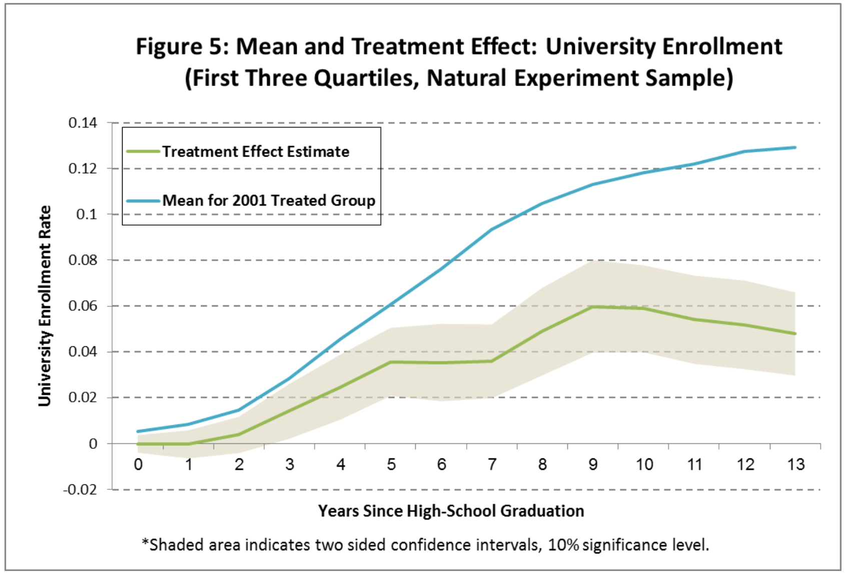


Figure 5: Mean and Treatment Effect: University Enrollment
(First Three Quartiles, Natural Experiment Sample)
Treatment Effect Estimate
Mean for 2001 Treated Group
University Enrollment Rate
0.14
0.12
0.1
0.08
0.06
0.04
0.02
0
-0.02
0 1 2 3 4 5 6 7 8 9 10 11 12 13
Years Since High-School Graduation
*Shaded area indicates two sided confidence intervals, 10% significance level.

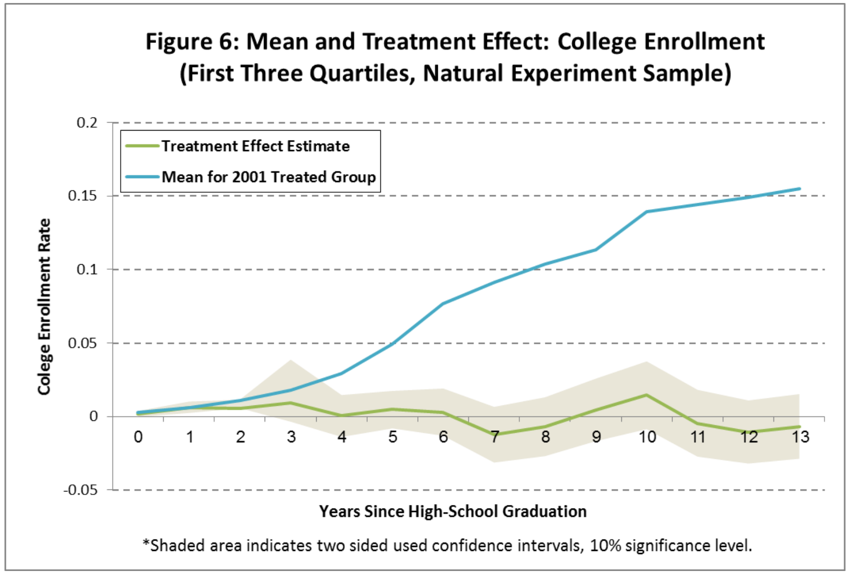


Figure 6: Mean and Treatment Effect: College Enrollment
(First Three Quartiles, Natural Experiment Sample)
Treatment Effect Estimate
Mean for 2001 Treated Group
Colege Enrollment Rate
0.2
0.15
0.1
0.05
0
-0.05
0 1 2 3 4 5 6 7 8 9 10 11 12 13
Years Since High-School Graduation
*Shaded area indicates two sided used confidence intervals, 10% significance level.

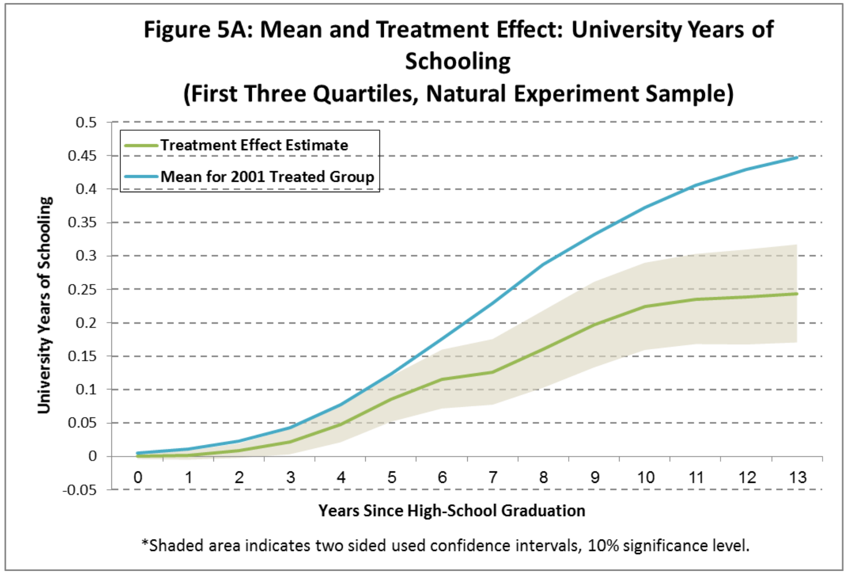


Figure 5A: Mean and Treatment Effect: University Years of Schooling
(First Three Quartiles, Natural Experiment Sample)
Treatment Effect Estimate
Mean for 2001 Treated Group
University Years of Schooling
0.5
0.45
0.4
0.35
0.3
0.25
0.2
0.15
0.1
0.05
0
-0.05
0 1 2 3 4 5 6 7 8 9 10 11 12 13
Years Since High-School Graduation
*Shaded area indicates two sided used confidence intervals, 10% significance level.

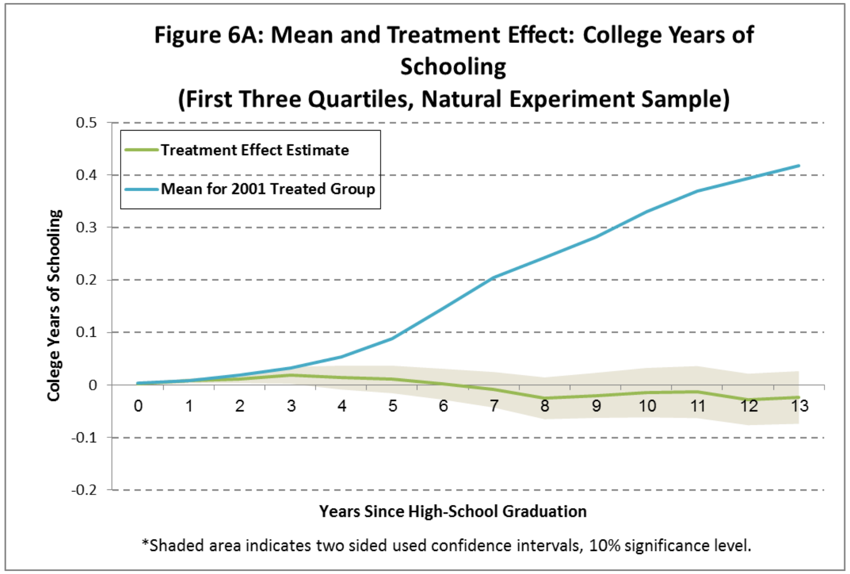
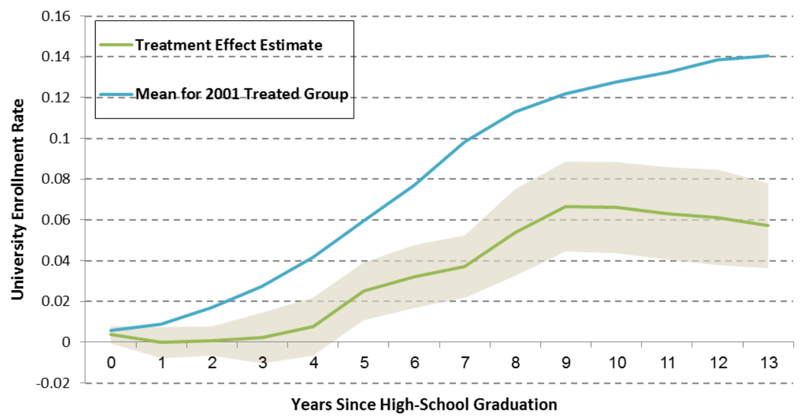


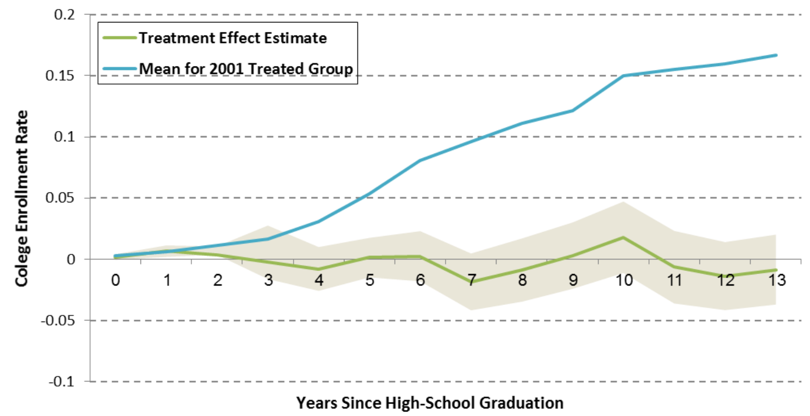
Figure 6A: Mean and Treatment Effect: College Years of Schooling
(First Three Quartiles, Natural Experiment Sample)
Treatment Effect Estimate
Mean for 2001 Treated Group
Colege Years of Schooling
0.5
0.4
0.3
0.2
0.1
0
-0.1
-0.2
0 1 2 3 4 5 6 7 8 9 10 11 12 13
Years Since High-School Graduation
*Shaded area indicates two sided used confidence intervals, 10% significance level.

Figure 7: Mean and Treatment Effect: University Enrollment (First Three Quartiles, Regression Discontinuity Sample)



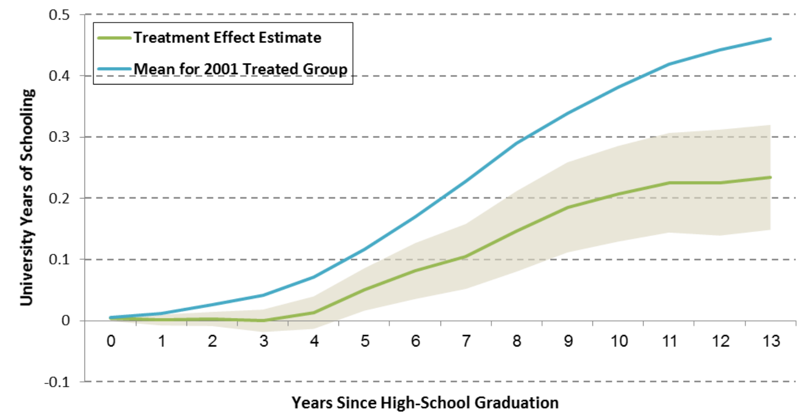
*Shaded area indicates two sided confidence intervals, 10% significance level.

Figure 8: Mean and Treatment Effect: College Enrollment (First Three Quartiles, Regression Discontinuity Sample)



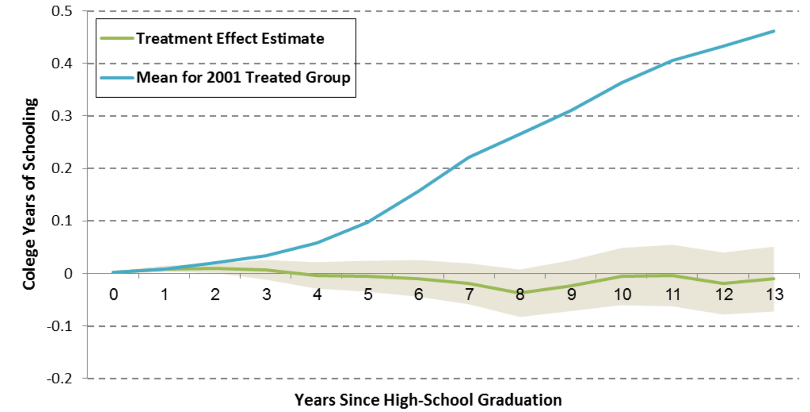
*Shaded area indicates two sided used confidence intervals, 10% significance level.

Figure 7A: Mean and Treatment Effect: University Years of Schooling (First Three Quartiles, Regression Discontinuity Sample)



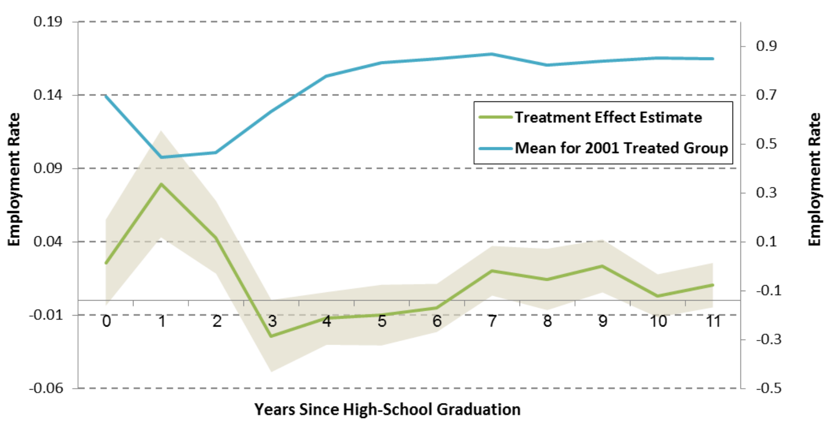
*Shaded area indicates two sided used confidence intervals, 10% significance level.

Figure 8A: Mean and Treatment Effect: College Years of Schooling (First Three Quartiles, Regression Discontinuity Sample)



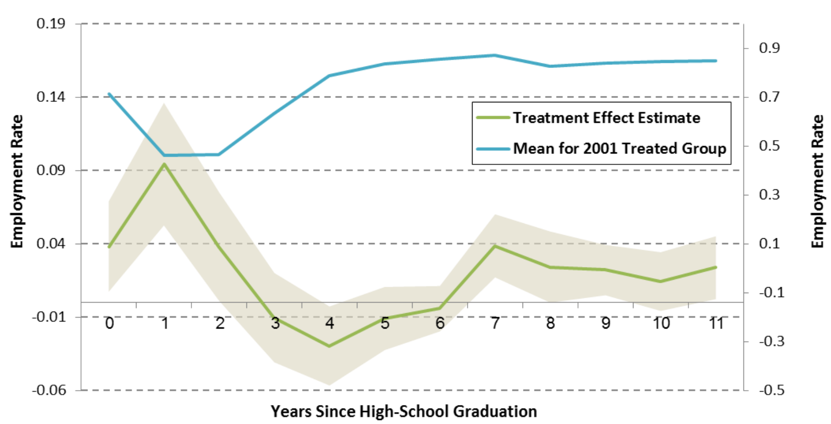
*Shaded area indicates two sided used confidence intervals, 10% significance level.

Figure 9: Mean and Treatment Effect: Employment Rate (Full Sample, Natural Experiment Sample)



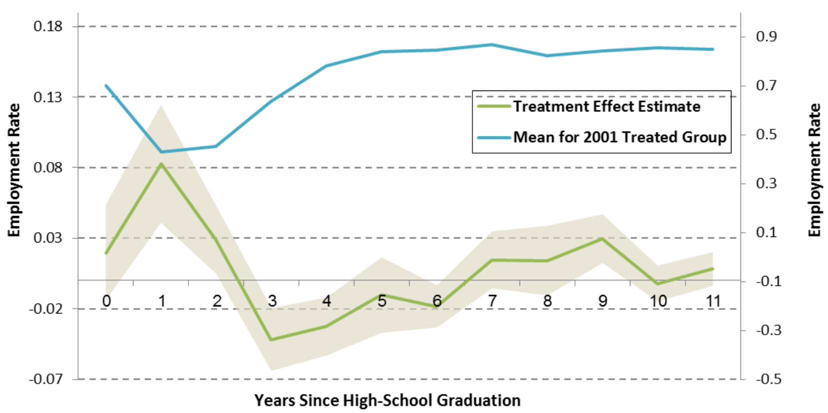
*Shaded area indicates two sided confidence intervals, 10% significance level.

Figure 10: Mean and Treatment Effect: Employment Rate (First Three Quartiles, Natural Experiment Sample)



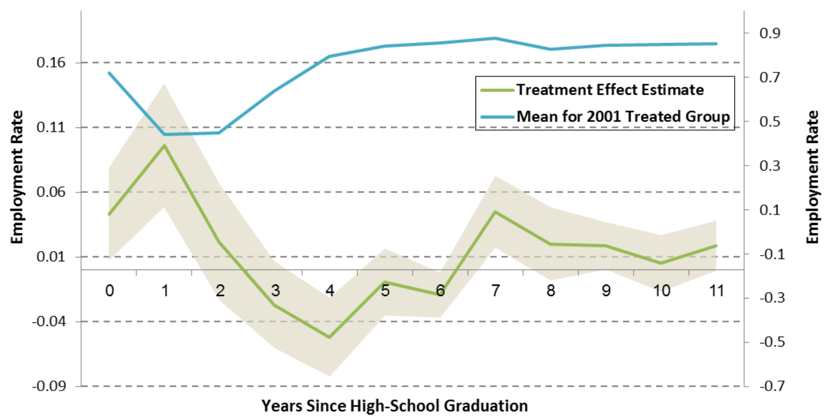
*Shaded area indicates two sided confidence intervals, 10% significance level.

Figure 9A: Mean and Treatment Effect: Employment Rate (Full Sample, Regression Discontinuity Sample)



*Shaded area indicates two sided confidence intervals, 10% significance level.

Figure 10A: Mean and Treatment Effect: Employment Rate (First Three Quartiles, Regression Discontinuity Sample)



*Shaded area indicates two sided confidence intervals, 10% significance level.

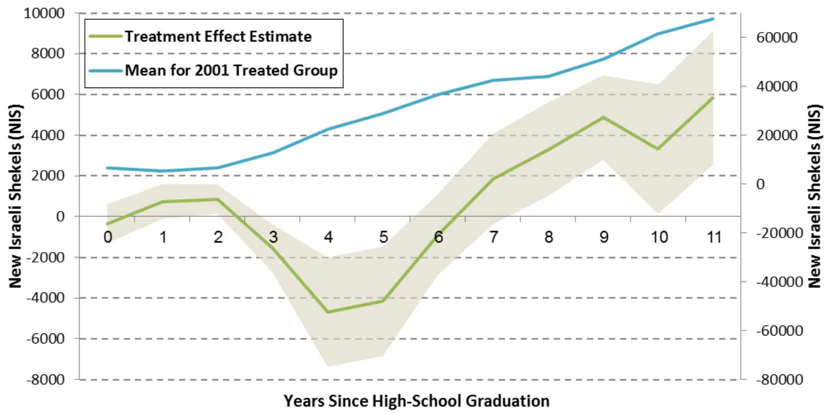


Figure 11: Mean and Treatment Effect: Annual Earnings - 2009 Prices NI Shekels (Full Sample, Natural Experiment Sample)

*Shaded area indicates two sided confidence intervals, 10% significance level.

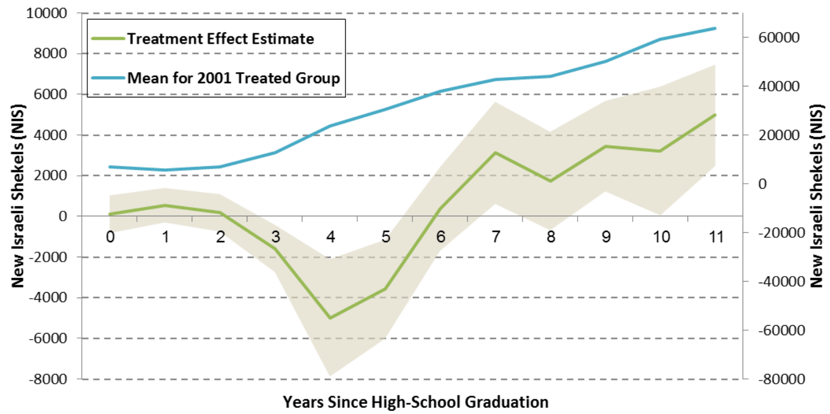


Figure 12: Mean and Treatment Effect: Annual Earnings - 2009 Prices NI Shekels (First Three Quartiles, Natural Experiment Sample)

*Shaded area indicates two sided confidence intervals, 10% significance level.

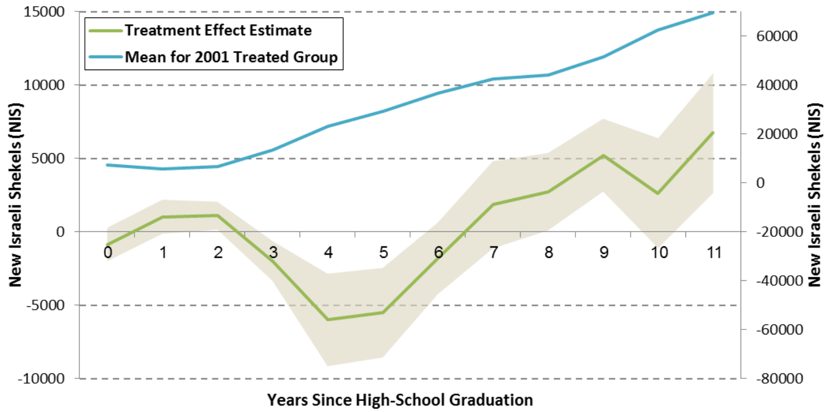


Figure 11A: Mean and Treatment Effect: Annual Earnings - 2009 Prices NI Shekels (Full Sample, Regression Discontinuity Sample)

*Shaded area indicates two sided confidence intervals, 10% significance level.

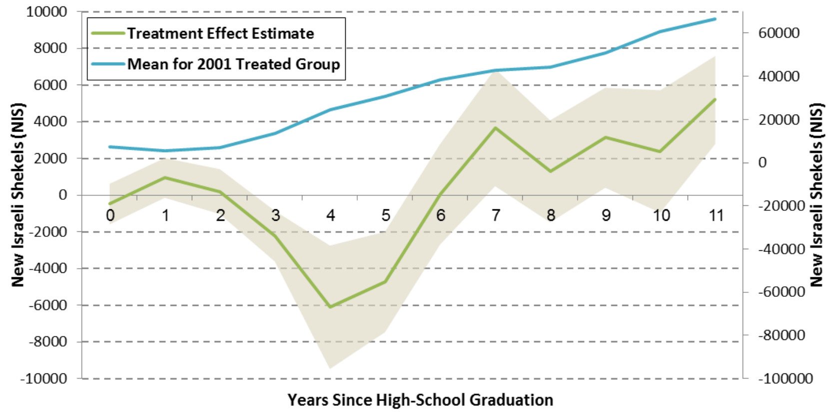


Figure 12A: Mean and Treatment Effect: Annual Earnings - 2009 Prices NI Shekels (First Three Quartiles, Regression Discontinuity Sample)

*Shaded area indicates two sided confidence intervals, 10% significance level.