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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

The long term effect of teachers' pay for performance is of particular interest, as critics of these schemes claim that they encourage teaching to the test or orchestrated cheating by teachers and schools. In this paper, I address these concerns by examining the effect of teachers' pay for performance 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 base this study on an experiment conducted a decade and a half ago in Israel and present evidence that the pay for performance scheme increased a wide range of long run human capital measures. Treated students are 4.3 percentage points more likely to enroll in a university and to complete an additional 0.17 years of university schooling, a 60 percent increase relative to the control group mean. These gains are mediated by overall improvements in the high school matriculation outcomes due to the teachers' intervention at 12th grade. The pay scheme led also to a significant 7 percent increase in annual earnings, to a 2 percent reduction in claims for unemployment benefits, and a 1 percent decline in eligibility for the government disability payment.

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1. Introduction

It is important examining the long term effect of teachers' pay for performance schemes because skeptics often claim that they only improve students' test scores by encouraging 'teaching to the test' practices or orchestrated cheating by teachers and schools. Specifically, it is claimed there is no actual improvement in human capital because teachers do not respond to pay incentives by promoting broad human capital acquisition. Instead, they focus on improving students' test taking ability; on testing preparation sessions at the expense of teaching material not included in the exam; on strategies of answering exam questions such as 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 this key issue, I examine the effect of high school teachers' pay for performance on students' 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 take advantage of a teachers' pay for performance experiment which I conducted almost a decade and half ago in Israel (Lavy, 2009) in which I analyzed the short-term effects of this experiment on students' cognitive schooling outcomes. This earlier research now presents an unusual opportunity to evaluate whether an intervention that offered performance-based bonuses to teachers for students' test achievements has had a long-term impact on adult well-being.

This paper provides the first evidence of links between teachers' pay for performance during high school and students' schooling and labor market outcomes during their late 20's and early 30's. I examine the impact on various long-term outcomes, including post-secondary educational attainment and quality, employment, earnings, and different dimensions of social behavior (e.g., eligibility for and amount of welfare allowances, marriage, and fertility). Some of these outcomes, for example marriage and fertility, can also be viewed as potential mechanisms for the effect of the intervention on employment and earnings.

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

The evidence I present in this paper shows that the pay for performance scheme improved a wide range of long-run human capital measures. A decade after the end of the intervention, treated students are 4.3 percentage points more likely to enroll in a university and to complete an additional 0.17 years of university schooling, a 60 percent increase relative to the control group mean. The road to higher university enrollment and completed years of schooling was paved by the overall improvement in high school matriculation outcomes due to the teachers' intervention. The higher passing rate and average score in the Math and English matriculation exams, which were directly targeted by the intervention, led also to improved average matriculation outcomes, such as earning a matriculation diploma and the overall composite matriculation score. These two outcomes determine admission to post-secondary schooling and to various academic programs, especially at universities. For example, obtaining a *Bagrut* diploma is a pre-requisite for admission to universities, and enrollment in selective study programs such as medicine, engineering, and computer science is based on the *Bagrut* composite score, and on completing Math and English courses at an advanced level. The teachers' bonuses program increased the *Bagrut* composite score by 2.9 points and the *Bagrut* graduation rate by 5.5 percentage points. Other dimensions of the matriculation study program that signal quality of schooling were also improved: in particular the number of science credit units increased by 26 percent and the number of subjects studied at advanced level increased by 5 percent. These improvements, along with the increase in university schooling, led to a large increase in earnings at age 28-30: a decade after the experiment ended, treated students experienced a 7 percent increase in earnings relative to the control group. I also estimate that the teachers' program led to a reduction of 2 percent (a 33 percent decline) in eligibility for unemployment benefits, and a 1 percent decline in eligibility for government disability transfers (a 50 percent decline). I did not find, however, any effect of the intervention on marriage and parenthood rates.

The lessons from this analysis have meaningful external validity and can be easily transferred and applied to other educational settings in developed countries. Both the high school system in Israel and its high-stakes exit exams are very similar to those in other countries. Importantly, variants of the teachers' pay for performance intervention studied here have been implemented in recent years in developed and developing countries. Therefore, this study contributes to the accumulation of empirical evidence that provides a more complete picture of the returns to educational interventions, which can improve the effectiveness of spending on education. Another important advantage of the evidence presented here is that teachers' pay for performance is an intervention that can be directly manipulated by public policy, which is preferable to evidence based on measures such as school or teacher quality, which are not easily measured or manipulated by policy.

This paper contributes to a recent 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 targeted schooling programs aimed at improving the quality of education in addition to increasing attainment. Most of these studies have centered on the evaluation of short-term outcomes, primarily standardized test scores, as a measure of success. Nevertheless, an equally persistent question is the extent to which educational interventions lead to long-term improvements in well-being – measures assessed not by attainment on tests but attainment in life. Puzzling and conflicting results from several evaluations make this a highly salient issue. Two small-scale, intensive preschool experiments produced large effects on contemporaneous test scores that quickly faded (Schweinhart et al., 2005; and Anderson, 2008). Non-experimental evaluations of Head Start, a preschool program for poor children, revealed a similar pattern, with test-score effects gone by middle school. But in each of these studies, treatment effects re-emerged in adulthood 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 childhood investments on postsecondary attainment (Krueger and Whitmore 2001, Dynarski et al., 2011). Very recently, Chetty et al. (2014) examined the long-term effect of value-added measures of teacher 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. In contrast, Dustmann et al. (2012) found that attending a better school in Germany had no effect on school attainment or labor-market outcomes. However, there are no studies that focus on the long-term effect of teachers' performance pay programs, even though there is much uncertainty about the long term gains from such programs.

Determining which interventions achieve better long-term outcomes is critical for improving the efficiency of education and school resource allocation.

The remainder of my paper is as follows. Section 2 describes the Pay for Performance 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 increasingly popular. 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 the notion that incentive pay may motivate teachers to improve their performance (Lazear (2000, 2001), Lavy (2002, 2007), Neal (2011), Duflo et al. (2012)). Opponents of teacher incentive programs argue that schools may respond to test score-based incentives in perverse ways such as cheating in grading and teaching to the test (Glewwe et al (2010), Neal (2011)) so that any performance gains are short-lived and there is no long-term accumulation of human capital. Even though there is some evidence that performance pay for teachers has significant short-term learning benefits, their critique is focused on harmful long-term effects, although the evidence is scanty. The evidence presented in this paper fills this gap with results on a wide array of lifetime outcomes.

The Teachers' Incentive Experiment

In early December 2000, the Ministry of Education unveiled a new teachers' bonus experiment in 49 Israeli high schools that I designed and helped to implement. The main feature of the program was an individual performance bonus paid to teachers on the basis of their own students' achievements. The experiment included all English, Hebrew, Arabic, and Mathematics teachers who taught classes in grades 10 through 12 leading to 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.

Schools eligible for the program met two criteria: 1) they had 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 on the matriculation rate. 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 assigned at random. Nevertheless, the design of the program enables the implementation of a randomized trial identification strategy which I outline below. The results of the short term effect, presented in Lavy (2009), suggested that teachers' 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 tests and grading external to schools, accounted for more than half of the increase in the unconditional outcomes in math and somewhat less in English. These improvements appear to stem 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 suggests that incentives have induced improved effort and pedagogy is important in the context of the recent 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' pay for performance schemes improve real human capital accumulation can only be based on long-term effects, in particular on completed post-secondary schooling, employment, wages, and welfare dependency, which I intend to investigate in this proposal.

Identification and econometric model

The program rules had 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. The data given to administrators were culled from a preliminary and incomplete file of matriculation status. For many students, matriculation status was erroneous as it was based on missing or incorrect information. The Ministry later corrected this preliminary file, as it does every year.[2] As a result, the matriculation rates used for assignment to the program were inaccurate in a majority of schools. 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.

This measurement error could be useful for identification of the program's 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 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 line. 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^*. 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 are perfectly balanced in student and school characteristics. The following model is used as the basis for regression estimates using the RT sample:

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

[2] There are many requirements to complete the matriculation process that tend to vary by school type and level of proficiency in each subject. The verification of information between administration and schools is a lengthy process. The first version of the matriculation data becomes available in October and is finalized in December.

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.

3. Data

In this study, I use data from administrative files for the sample of participants in the treatment and control group of the experiment. The students in the sample were in high school between 2001 and 2002, and in 2011 they are adults aged 28-29. I use several panel datasets available from Israel's National Insurance Institute (NII). The NII is responsible for social security and mandatory health insurance in Israel. NII allows restricted access to this data in their protected research lab. The underlying data sources include: (1) the population registry data, which contain information on personal status, number of children and their birth dates; (2) NII records of post-secondary enrollment from 2001 through 2011, based on annual reports submitted to NII every fall term by all of Israel's post-secondary education institutions. Based on this data on annual enrollment, I computed number of post-secondary years of schooling[3]; (3) Israel Tax Authority information on income and earnings of employee and self-employed individuals; (4) NII records on welfare allowances, unemployment benefits and disability compensations, and (5) NII records on physical and mental disability. The NII linked these data to students' background data (see Table A1 for descriptive statistics) that I used in Lavy (2009) to study the effect of the teachers' incentive experiment on high-school academic outcomes. This information comes from administrative records of the Ministry of Education on the

[3] The NII, which is responsible for the mandatory health insurance tax in Israel, tracks post-secondary schooling enrollment because students pay a lower health insurance tax rate. Post-secondary schools are therefore required to send a list of enrolled students to the NII every year. For the purposes of our project, the NII Research and Planning Division constructed an extract containing the 2001–2011 enrollment status of students in our study.

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

The post high-school academic schooling system in Israel

The post high-school academic schooling system in Israel includes seven universities (one of which confers only graduate and PhD degrees), over 50 colleges that confer academic undergraduate degrees (some of these also give masters degrees), and dozens of teachers' colleges that confer bachelor of education qualifications.[4] All universities require a *Bagrut* diploma for enrollment. Most academic and teachers' 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 enrollment rates for the cohort of graduating seniors in 1995 (through 2003) was 55.4 percent, of which 27.6 percent were enrolled in universities, 8.5 percent in academic colleges, 7 percent in teachers' colleges, and the rest in non-academic institutions.[5] However, because the treated population is from a low socio-economic background with relatively higher enrollment rates in lower quality post-secondary schooling, we include more detailed decomposition of these type of schooling institutions such as teachers' colleges, practical semi-engineering schools and other non-academic one or two-year colleges.

The post-high school outcome variables of interest here are indicators of ever having enrolled in a post-high school institution of a type described above, as of the 2010–2011 school years, and the number of years of schooling completed in these institutions by this date. I measure these two outcomes for the 2000 and 2001 12th grade students. The youngest cohort (2001) in the sample is 29-30 years old in 2010-2011. Even after accounting for compulsory military service[6]**,** I expect most students enrolled in post-high school

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

education, including those who continued schooling beyond undergraduate studies, to have graduated by the 2010–2011 academic year. I therefore present evidence both for enrollment and for completed years of post-high school education.

Definitions of Outcomes in Adulthood

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

Labor Market Outcomes:

Earnings: Individual earnings data come from the Israel Tax Authority (ITA). Filing tax forms in Israel is compulsory only for individuals with non-zero self-employment earnings, but ITA has information on annual gross earnings from salaried and non-salaried employment and they transfer this information annually to NII, including number of months of work in a given year. Using the data for 2009-2011, I compute for each year the annual income (from salaried and self-employment).

Individuals with positive non-zero months of work and zero or missing value for earnings are coded as having zero earnings. 12.3% of individuals have 0 wage earnings at age 28-30 in our full sample. Since employment and earning are endogenous outcomes it is not appropriate to exclude individuals with zero or missing value for earnings from the sample but this choice does not affect the results. To account for earnings data outliers I dropped from the sample all observations with monthly wage higher than 30,000 NIS per month. Overall, very few observations were dropped from the 2009, 2010 or the 2011 samples. The results are not changed when we keep these outliers observations in the analysis sample or when we cap the earnings at this cutoff.

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

Eligible for Unemployment Benefit: An indicator with value one if received unemployment benefits in a given year, zero otherwise.

Unemployment Benefit Compensation: Total annual benefits (ILS) of unemployment compensation.

Education

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

University Attendance: an indicator for being enrolled for at least one year in university schooling. Completed years of university schooling is the number of years of attendance.

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

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

Disability and Welfare

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

Disability Compensation: Total annual benefits (ILS) of disability compensation.

Descriptive Statistics

Table 1 presents detailed summary descriptive statistics of the outcome variables for 2011 by treatment and control group and by pre- and post-reform cohorts for the sample that includes the first three quartiles (3Q) of the ability distribution of students. Table 2 presents the respective summary statistics for the full sample (4Q, all four quartiles). Attention is focused on the 3Q sample because these are the students that were mostly affected by the teachers' bonuses program (Lavy 2009). Post-secondary enrollment statistics are presented in panel A, in columns 1-2 for the students in treated schools who graduated from high-school in 2000 (pre-treatment) and 2001 (post-treatment). Columns 3-4 present the respective statistics for students in control schools. Focusing for illustration on treated students (2001 cohort), the overall ever enrolled rate in any post-secondary schooling in the treatment group for the post-treatment cohort (2001) in the 3Q sample is 50.0 percent, of which 11.2 percentage points is in one of the seven universities, 26.1 percent is in an academic college, 10.4 percentage points is in a teachers' college, 11.9 percentage points in practical engineering colleges and 3.3 percentage points in any other post-secondary schooling.[7] Summary statistics

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

on completed years of schooling are presented in panel B. The average number of post-high school years of schooling completed up to the school year 2010-2011 in the treated sample (2001 cohort) is 1.53, of which 0.33 are in university schooling, 0.68 are in college education, 0.2 are in teachers' colleges, 0.21 in practical-engineering schools, and 0.11 in other post-secondary schooling.

The descriptive statistics presented in Table 2 show clearly that in the 4Q sample, the mean over all enrollment in post-secondary schooling and in university schooling are higher than in the full sample, and so are the various means of completed years of schooling. This is expected as the students in the fourth quartile have the highest mean high-school outcomes. For example, the mean university enrollment in the full sample of the 2001 treated group is 17.8 percent versus only 11.2 in the 3Q sample. The respective means of completed years of university schooling in the two samples are 0.626 and 0.332.

Summary statistics for labor market outcomes and welfare related indicators in 2011 are presented in panel C of Table 1 for the 3Q sample and in Table 2 for the full sample (respective data for 2009 and 2010 are presented in Table A2 and Table A3). Focusing again on the treated group in year 2011, we note that 82.7 percent of the 2000 cohort in treated schools in the 3Q sample were employed, earning annually ILS 61,854 ($15,500) on average, and 6.7 percent were unemployed during the year. In the full sample, the respective means are 84.0, 6.7 and ILS 65,015. Regarding the welfare status of individuals in our sample, 1.6 percent and 1.0 percent of the 2000 and 2001 cohorts in treated schools in the 3Q sample, respectively, received physical/mental disability payment, and the respective rates for work related disability are much lower, 0.5 and 0.3 percent.

4. Empirical Evidence

Effect on Post-Secondary Schooling Attainment

The program had positive and significant short-term effects on high-school English and Math outcomes at end of high school for students in the first three quartiles of the ability distribution, but had small and insignificant effects on students in the fourth quartile of the ability distribution (Lavy 2009). I therefore will focus the analysis on evidence based on the 3Q sample and compare the results to those based on the 4Q sample.

Since the program increased participation rate, average score, and passing rate in the math and English matriculation exams, we should expect some positive effect on overall summary measures (outcomes) of the matriculation exams such as obtaining a matriculation diploma, total number of matriculation credits, and the average score in the all matriculation exams. Evidence on improvement in these summary achievement measures should lead to an increase in post-secondary enrollment and attainment because they are used as admission criteria for various academic institutions and programs. One would also expect to see some shift away from post-secondary schooling where the admission process places less emphasis on having a passing score in the English and Math matriculation exams, and on obtaining a matriculation certificate. Such post-secondary institutions include one-year and two-year vocational colleges and some of the three-year academic colleges. These achievements are pre-requisites for admission in all seven universities.

Table A4 presents results for the short-run impact of the pay for performance experiment on the average matriculation score, on obtaining a matriculation diploma, and other related outcomes on the end of high-school outcomes. In the 3Q sample, the average matriculation score is up by 2.9 points (SE=1.017), and the matriculation rate went up by 5.5 percentage points, which amounts to a 13 percent improvement – a relatively large gain. The average number of credit units increased by 0.669, the number of science credits increased by one third of a unit, and the number of subjects studied at the most advanced level (5 credits) increased by almost one-tenth. The respective estimated effects based on the full sample are similar to those based on the 3Q sample with one exception, the effect on the matriculation rate is half of the effect in 3Q sample (0.026) and it is not significantly different from zero.

Table 3 presents results for the long-run impact of the pay for performance experiment on post-secondary schooling attainment. Evidence based on the 3Q sample is presented on columns 1-2 and 5-6. Evidence based on the full sample is presented in columns 3-4 and 7-8. I distinguish three types of post-secondary education. 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 (while a basic study program in English is sufficient to qualify for a matriculation diploma), and at least one matriculation subject studied at an advanced level. About 35% of all students are enrolled in one of the seven universities. The second type of post-secondary schooling includes over 50 academic colleges that confer only a BA degree and offer mostly social sciences, business, and law degrees. The third group includes teachers' colleges. The fourth group includes two-year practical engineering colleges and the last group includes all other non-academic institutions.

Enrollment in university schooling increased by 4.6 percent in the 3Q sample and this effect is precisely measured (SE=0.010). This gain, relative to the control group mean of 10.2 percent, is a 45 percent increase. The increase in enrolment led to a 0.183 increase in completed years of university schooling, reflecting a dramatic 63 percent increase relative to the baseline mean of 0.291 years of university schooling. The relative gain in university enrollment and in the respective completed years of schooling are of similar magnitude, both being very large relative to the impact of any other educational intervention or policy change, for example in comparison to the gain due to an increase in compulsory schooling.

The results based on the full sample are similar to the results based on the 3Q sample. University enrollment increased by 5.5 percent and completed years of university schooling increased by 0.25 years. The relative magnitude of these gains is also similar to those based on the 3Q sample. However, the effect on academic-college enrollment and years of schooling estimated with the full sample are strikingly different than those obtained using the 3Q sample. While the effect on enrollment in academic colleges of students in the 3Q sample is zero, the effect in the full sample is negative, large and almost completely offsets the increase in university enrollment in the 3Q sample. This results means that the teachers' PFP program expanded the extensive margin of post-secondary schooling of students who are up to the 75th percentile of the ability distribution, while in the 4Q sample I find a compositional shift: an increase in university enrollment and in years of schooling that are almost fully offset by a reduction in enrollment and years of schooling in academic college education. This pattern obviously reflects the pattern of offsetting effect (an increase in university schooling and a decline in other forms of post-secondary schooling) of the program on students in the fourth quartile of ability.

Effect on Earnings and Employment

I next present estimates of the effect of the program on annual earnings at around age 28-30. These results are presented in Table 4. The average annual earnings in the control group in 2009 was 46,945 shekels ($12,688 based on an exchange rate of 3.7 Israeli Shekels to one US Dollar); in 2010 it was higher, 51,558 shekels ($13,935) and in 2011 even higher, 61,854 shekels ($16,718).[8] Note that the employment rate did not change during these three years so the increase from year to year in average earnings are due to an increase in wages. The estimated effect of the pay for performance program on annual earnings is 4,405 shekels ($1,590) in 2009, 4,869 shekels ($1,988) in 2010 and 4,442 shekels ($1,200) in 2011. These gains are significantly different from zero and relative to the control group mean they reflect a 9.3 percent increase in 2009, a 9.4 percent increase in 2010, and a 7.2 percent increase in 2011. Results based on the full sample are qualitatively the same as those based on the 3Q sample.

Note that the gain in annual earnings is relative to a small increase in mean years of schooling (a 0.074 increase in years of schooling in the 3Q sample and close to zero respective gain in the Q4 sample), but a much larger increase in university schooling. Therefore, the increase in earnings perhaps reflects not only the increase in mean schooling, but also a change in the composition of post-secondary education, with a larger component of university schooling and a smaller component of academic college. The implied mechanism that explains part of the earnings gain is, therefore, a positive effect of the teachers' incentive experiment on the quality of post-secondary schooling. The better access to higher quality post-secondary education is mediated through the range of improvements in high-school educational outcomes, which allowed affected students to switch from academic colleges to universities, and perhaps also enroll in more selective programs that lead to a higher return to schooling. Another very realistic explanation for the large earning gain 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 under the program by 2-5 percent. There is a return in the labor market of about 13 per cent to those having a matriculation diploma, independently of the effect of a matriculation diploma on post-secondary schooling.[9] 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) could also be rewarded in the labor market beyond their effect on post-secondary schooling.[10]

[8] The mean earnings for the full 2001 cohort is slightly higher, ILS 70,300, and the 2001 cohort mean unemployment rate is 6.2 percent.

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

[10] 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

The interesting question that is raised therefore from these results is whether the positive effect of the teachers' program on earnings at adulthood is predicted by the short- and medium-term positive effects that the program had on *Bagrut* outcomes? That is, are the effects measured at the time of the experiment predictive of the program's long-term effects? Do *Bagrut* outcomes that measure quality of study program play equal role in this regard? It should be noted, however, that this question can be addressed but cannot decompose the effect on earnings of *Bagrut* outcomes to the component that operates through its effect on post-secondary schooling and the part that reflects a direct independent effect unrelated to post-secondary schooling.

I first approach this question by estimating OLS regressions of annual earnings on the various high-school *Bagrut* outcomes, while including in the regressions controls for student's parental and demographic characteristics. These results are presented in Table 5 panel A. For the year of 2011, I report estimates when only one of the high-school outcomes is included in the regression (column 2), and also estimates when all outcomes are included jointly (column 3). When included one at a time, estimates of all outcomes are positive and very precisely measured. When all four are included jointly, only some remain significant. For example, the estimates of the average score and of the number of science credit units are significant.

The conclusion from these results is that high-school outcomes are indeed correlated with earnings. However, a more firm conclusion about how much of the program's effect is due to improvement in these outcomes can be drawn by examining whether the estimated effect of school choice on post-secondary attainment and earnings disappears when we control for *Bagrut* outcomes. This is an informal test of whether school choice affects post-secondary attainment and earnings at adulthood through any channel other than these *Bagrut* outcomes. Of course, this analysis and its results are only suggestive because the *Bagrut* outcomes are endogenous and potentially correlated with the error term in the long term regression equations.

In panel A of Table 6, I present estimates of the coefficient of the teachers' incentives program treatment effect in a DID regression that includes also the high-school outcomes as explanatory variables, first each at a time and then all jointly. Results are reported based on the full and the 3Q samples. For ease of

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 comparison, column (1) presents the original treatment effect estimates from Tables 3-4. For example, the estimated effect of treatment on enrollment in university schooling in the 3Q sample is 0.046. The inclusion of each of the high-school outcomes as an additional control in the DID regression attenuates the treatment effect estimates towards zero. When all five high-school outcomes are added as controls in the regression, the estimated treatment effect drops to 0.029, a 37 percent decline. The respective decline in the full sample treatment estimate is 40 percent. The magnitude of the decline in the treatment effect estimate on completed years of university schooling is very similar, just below 40 percent in both samples.

In panel B of Table 6, I present similar evidence with regard to annual earnings, based on the 3Q and full samples. Again, the treatment effect of teachers' incentives clearly falls each time we add one of the high-school *Bagrut* achievement measures. For example, based on the 4Q sample, it falls by 35 percent when the average matriculation score is added, by 10 percent when the matriculation diploma is added, by 30 percent when number of credits is added, and by 29 when the number of *Bagrut* subjects studied at the most advanced level is added. When all five are added, the original treatment estimate, ILS 3,329, falls to ILS 1,817 (column 7, panel B), a 45 percent decline, and it is no longer significantly different from zero. The evidence from the 3Q sample is qualitatively similar, when all five outcomes are added to the DID regression, the effect of teachers' incentives on earnings declines by 40 percent, from ILS 4,442 to ILS 2,696, and the treatment estimate is no longer statistically significant.

In column 6, I present regressions where I control only for the outcomes of the math and English matriculation exams. These outcomes are the score in the English and math exams and the test taking rate in each of these two subjects. This is an interesting exercise because these four outcomes were directly targeted by the teachers' pay for performance experiment, especially in the 3Q sample. Focusing on the sensitivity of the effect of the program on earnings (panel B of Table 6), it is clear that adding these controls lowers the treatment estimate in the 3Q sample more than adding any of the four overall *Bagrut* outcomes. The estimates in the full sample reveal a somewhat different pattern, as adding the two directly affected math and English outcomes lowers more the earnings treatment estimated effect only in comparison to adding the number of matriculation credits or the number of advanced level matriculation subjects.

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correlated with the high school matriculation outcomes.

In column 9, I report treatment effect estimates when I also add university enrollment and completed years of schooling as controls. Adding these two additional controls lowers the earnings treatment effect further. The overall decline is now 56 percent in the full sample and 44 percent in the 3Q sample. These results clearly indicate that the gain in earnings in adulthood is mediated to large extent by the gains in the measured high-school and post-secondary schooling outcomes.

Table 4 presents also evidence on the effect of the teachers' incentive experiment on employment and annual earnings in 2009, 2010 and 2011, and on the probability and amount received of unemployment benefits during each of these three years.[11] The average employment rate is high, over 82 percent in all three years, both in the treatment and control groups group. For example, for the treatment group it is 84.4, 83.4 and 85.2 percent in 2009, 2010, and 2011, respectively. The treatment effect on employment in 2009 and 2010 is positive, in 2009 it is 0.029 and significant, in 2010 it is 0.020 but not precisely measured (SE=0.012). In 2011, however, this effect turns negative but it is not significantly different from zero. Based on these results, I conclude that there is no systematic effect of the teachers' incentive program on employment at about age 30.

The proportion in the sample that received unemployment benefits is 7.9 percent in 2009, 6.9 percent in 2010 and 6.7 in 2011 (fourth row in Table 4). These rates are very similar to the national average unemployment rate in 2010 and 2011 in the 25-34 age groups, which was 7.4 and 6.8 percent, respectively. The estimated treatment effect on the probability of receiving unemployment benefits in these three years is -0.011 (SE=0.009), 0.015 (SE=0.009) and -0.017 (SE=0.008), respectively. The effect in 2011 is negative and significantly different from zero. In the third row of Table, 4 I also present the effect of the teachers' incentive program on total unemployment insurance benefits received during these years, but in all three years the estimated effect is not significant. In 2009, the control group mean was 476 ILS ($120), in 2010, it was 676 ILS ($186) and in 2011, 679 ILS ($180). The estimated treatment effect in 2009 is 12 ILS, in 2010 it is 89 ILS and in 2011 it is -10 ILS. None of these estimates are statistically different from zero.

Effect on Disability Status and Allowance

[11] I also estimated the treatment effects using a sample where the data of all three years were stacked together. The results from this estimation are very similar to the evidence presented in Tables 3-4, but I prefer to present the year specific estimates because they show how the experimental effect changes with age.

Table 7 presents the estimated effect of the teachers' pay for performance program on physical-mental disability eligibility rates and on the average disability welfare transfer. The level of such transfer is an increasing function of the disability rate, which is determined individually for each claimant by an NII committee that includes a doctor and a social worker. The proportion of individuals in 2011 for the 3Q sample of the 2000 cohort in treated schools who received physical or mental disability allowance is 1.6 percent (column 1 in Table 7). The mean monthly disability allowance is 445 ILS.

In columns 2, 4, and 6 of Table 7, I report the DID estimates of the effect of teachers' incentives on each of these two outcomes. Focusing on results based on the 3Q sample suggests that this intervention reduced the proportion of disabled individuals who receive disability allowance by one percent and this estimate is statistically different from zero. It also lowered the mean disability allowance by 205 ILS, but this estimate is not precisely measured, with a t-value of 1.45. The respective estimates based on 2009 and 2010 data are also negative, but they are smaller and not statistically different from zero.

In panel B, I present the respective evidence based on the full sample (4Q). These results are similar to those in panel A but they represent marginally larger estimated effects and the two parameter estimates are statistically different from zero in the 2011 and 2010 data. The estimated effect on the eligibility for disability welfare allowance in 2011 is -1.2 (SE=0.4) percent, and on the average allowance it is -308 ILS (SE=115).

The estimated effects in both samples in 2011 are quite large, representing about a 50 percent reduction in disability eligibility rate and in the monthly payment received as a welfare transfer from the government. However, they are in line with evidence from studies that examined the impact of economic conditions and income on disability rates. Evidence from the US suggests large effects of economic opportunities on disability program-enrollment, for example, Black, Kermit, and Sanders (2002) examined the impact of the coal boom of the 1970's and the coal bust of the 1980's on disability program participation and found evidence to suggest that as the value of labor-market participation increases, disability program participation falls. The elasticity of payments with respect to local earnings is between –0.3 and –0.7, depending on the disability program, and it is higher for permanent than for transitory economic shocks. Several potential mechanisms can account for the large effect of the teachers' incentive program on disability status and eligibility for related welfare allowances. First, gaining disability status is often related to manipulative behavior of individuals' vis-à-vis NII staff. For example, individuals who want to avoid the military draft may opt to gain NII disability status ('back' problems is a well-known excuse for this purpose). If school choice increased the draft (by increasing schooling quality, for example, which may increase motivation for the draft, or by increasing the range of options during the military service), it could have indirectly reduced the motivation to seek disability status. A more direct channel is through the effect of school choice on education and on the opportunity cost of time. Eligibility for NII disability allowance involves restrictions on employment and earnings, and the incentive program makes these restrictions more costly to individuals, lowering the financial gain from disability status. This mechanism is in line with the finding of Black, Kermit, and Sanders (2002), suggesting that better labor-market opportunities reduce disability program participation.

I also examine eligibility and disability allowance for disability cases caused by an accident at work. The higher schooling attainment and quality could have reduced treated students' entry into the manual labor force which has a higher risk of accidents, and it could also reduce directly the probability of a work related accident by improving productivity and safer work practices. The proportion of individuals in the 2000 sample who received such work related disability welfare transfer in 2011 is 0.4 percent (column 1 in Table 7), and this rate was similar in 2010 and 2009. The mean monthly welfare allowance for this type of disability is 102 ILS for the 2000 students in treated schools (column 1 in Table 7). The teachers' incentive program lowered the work accident disability in the 3Q samples by 0.4 percent and this effect is statistically different from zero (SE=0.2). The estimated effect based on the 4Q sample is slightly lower (0.3) and less precisely measured (SE=0.3). The estimated effect on the average government monthly transfer for this allowance declined sharply as well, in the 3Q sample by 115 ILS (SE=44) and in the 4Q sample by 85 ILS (SE=30).

Effect on marriage and children

I found no evidence that the teachers' incentives program had any systematic impacts on marriage and fertility outcomes. I examined the effect of the program on marital status in 2011, having children by 2011, and number of children in 2011. All these estimates were very close to zero and not statistically different from zero. I obtained similar results based on the 3Q and 4Q samples. These results are not presented here and are available from the author.

5. Conclusions

In this paper, I study the long term effect of an experiment that paid teachers additional salary 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 follow students from high-school to adulthood to examine the impact of a teachers' pay for performance scheme on long-run life outcomes. Such an analysis can address the critical question of whether an education policy intervention can achieve the ultimate goal of improving lifetime well-being. This research has also the advantage of focusing on a particular intervention that targets improvement in teaching quality and therefore can provide clear policy guidance, relative to more generally defined measures such as teachers' value added measures.

The evidence presented above shows that more than a decade after the initial intervention occurred, treated individuals experienced sizable gains in schooling attainment and quality, large increases in annual earnings, some of which reflect a return to education quality beyond the return to years of schooling, and an economically meaningful reduction in dependency on welfare income, such as unemployment insurance, disability benefits and welfare transfers.

These results are relevant and important for education policy in developed and developing countries as merit pay and incentive based pay for teachers is being implemented or contemplated in many countries. In U.S. education policy, for example, merit pay reforms for teachers have recently re-emerged at the top of the education policy agenda. In his first major education policy speech, President Obama promoted merit pay for teachers, and in 2009 he allocated $4.4 billion in federal funds into the Race to the Top program to encourage States to implement performance pay system for teachers.[12] Another example comes from the UK; where from September 2014 teachers have their annual salary rises tied to performance. This replaces a system where almost all teachers automatically moved up a point on the pay scale every year and has been

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

hugely controversial: on March 26 2014, the National Union of Teachers struck in protest of the overall in-pay structures that are due to begin later in the year.[13]

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[13] The Economist, March 29 2014.

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Table 1: Three Quartiles Sample: Post-Secondary Schooling and Employment and Income Statistics in 2011

	Treated Schools		Control Schools	
High School Cohort	2000	2001	2000	2001
Dependent variable	(1)	(2)	(3)	(4)
A. Enrollment in Post High School Education				
Any post secondary education	0.490	0.500	0.459	0.480
	(0.500)	(0.500)	(0.498)	(0.500)
University	0.102	0.112	0.110	0.079
	(0.303)	(0.315)	(0.313)	(0.270)
Academic College	0.259	0.261	0.275	0.306
	(0.438)	(0.439)	(0.447)	(0.461)
Teachers' College	0.106	0.104	0.051	0.056
	(0.308)	(0.306)	(0.220)	(0.230)
Semiengineering School	0.128	0.119	0.055	0.062
	(0.334)	(0.323)	(0.229)	(0.240)
Other	0.034	0.033	0.064	0.055
	(0.180)	(0.179)	(0.245)	(0.228)
B. Post High School Years of Schooling				
Any post secondary education	1.600	1.524	1.470	1.427
	(2.034)	(1.898)	(1.982)	(1.864)
University	0.291	0.332	0.340	0.243
	(0.999)	(1.087)	(1.170)	(0.934)
Academic College	0.732	0.680	0.705	0.808
	(1.443)	(1.347)	(1.353)	(1.456)
Teachers' College	0.243	0.201	0.107	0.097
	(0.882)	(0.735)	(0.586)	(0.512)
Semiengineering School	0.218	0.205	0.109	0.117
	(0.639)	(0.619)	(0.513)	(0.511)
Other	0.116	0.106	0.209	0.162
	(0.548)	(0.496)	(0.700)	(0.611)
C. Employment Outcomes in 2011				
Employed (1 = Yes, 0 = No)	0.827	0.831	0.876	0.894
	(0.378)	(0.375)	(0.330)	(0.308)
Average Annual Earnings (NIS)	61,854	57,882	68,643	62,268
	(52,706)	(50,358)	(55,618)	(48,679)
Received Unemployment Insurance Benefits (1 = Yes, 0 = No)	0.067	0.062	0.078	0.093
	(0.250)	(0.242)	(0.269)	(0.291)
Total Unemployment Insurance Benefits Received (NIS)	679	573	765	719
	(3,101)	(2,669)	(3,227)	(2,757)
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.016	0.010	0.009	0.014
	(0.125)	(0.100)	(0.095)	(0.118)
Total Disability Insurance Benefits Received in (NIS)	445	281	297	347
	(3,680)	(3,018)	(3,283)	(3,242)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.005	0.003	0.002	0.002
	(0.069)	(0.054)	(0.048)	(0.046)
Total Work Disability Insurance Benefits Received (NIS)	102	29	21	32
	(1,731)	(585)	(621)	(695)
Number of Observations	2,023	2,064	1,725	1,637
Weighted Number of Observations	3,095	3,071	3,049	2,828

Notes: The table reports means and standard deviations for different post-secondary schooling and employment and income variables for the year of 2011. The sample consists of students who are in the lowest three quartiles of test grades. Each column represents these statistics for a different group as described in each column's headline. Panel A is comprised of binary variables indicating whether the individual is enrolled or not to a specific type of post-secondary institution. The categories are not mutually exclusive and overlapping is possible. Panel B reports the number of years of education an individual has attained by 2011 in each type of the post-secondary institutions described in panel A. Panel C reports different employment and income variables for the individual in the year 2011.

Table 2: Four Quartiles Sample: Post-Secondary Schooling and Employment and Income Statistics in 2011

	Treated Schools		Control Schools	
High School Cohort	2000	2001	2000	2001
Dependent variable	(1)	(2)	(3)	(4)
A. Enrollment in Post High School Education				
Any post secondary education	0.569	0.564	0.533	0.579
	(0.495)	(0.496)	(0.499)	(0.494)
University	0.201	0.178	0.184	0.146
	(0.401)	(0.383)	(0.388)	(0.353)
Academic College	0.260	0.267	0.292	0.359
	(0.438)	(0.443)	(0.455)	(0.480)
Teachers' College	0.119	0.112	0.052	0.057
	(0.324)	(0.316)	(0.222)	(0.232)
Semiengineering School	0.117	0.110	0.049	0.057
	(0.322)	(0.313)	(0.215)	(0.232)
Other	0.034	0.034	0.058	0.054
	(0.181)	(0.180)	(0.233)	(0.226)
B. Post High School Years of Schooling				
Any post secondary education	2.104	1.902	1.884	1.895
	(2.297)	(2.106)	(2.177)	(2.054)
University	0.742	0.626	0.665	0.483
	(1.712)	(1.548)	(1.621)	(1.345)
Academic College	0.743	0.710	0.795	1.021
	(1.451)	(1.379)	(1.443)	(1.608)
Teachers' College	0.294	0.246	0.119	0.113
	(0.984)	(0.850)	(0.636)	(0.573)
Semiengineering School	0.197	0.188	0.095	0.102
	(0.603)	(0.594)	(0.479)	(0.468)
Other	0.129	0.132	0.210	0.177
	(0.580)	(0.572)	(0.715)	(0.631)
C. Employment Outcomes in 2011				
Employed (1 = Yes, 0 = No)	0.840	0.836	0.866	0.888
	(0.367)	(0.371)	(0.340)	(0.316)
Average Annual Earnings (NIS)	65,015	60,125	71,393	64,687
	(56,493)	(52,823)	(58,909)	(52,547)
Received Unemployment Insurance Benefits (1 = Yes, 0 = No)	0.067	0.063	0.073	0.084
	(0.250)	(0.242)	(0.260)	(0.277)
Total Unemployment Insurance Benefits Received (NIS)	673	573	748	619
	(3,041)	(2,645)	(3,243)	(2,556)
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.017	0.011	0.008	0.015
	(0.128)	(0.104)	(0.088)	(0.121)
Total Disability Insurance Benefits Received in (NIS)	461	301	244	374
	(3,749)	(3,068)	(2,950)	(3,296)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.004	0.002	0.002	0.002
	(0.065)	(0.048)	(0.050)	(0.045)
Total Work Disability Insurance Benefits Received (NIS)	83	23	33	39
	(1,528)	(518)	(866)	(1,157)
Number of Observations	2,723	2,677	2,479	2,350
Number of Observations	4,202	3,916	4,017	3,977

Notes: The table reports means and standard deviations for different post-secondary schooling and employment and income variables for the year of 2011. The sample consists of all four test quartiles. Each column represents these statistics for a different group as described in each column's headline. Panel A is comprised of binary variables indicating whether the individual is enrolled or not to a specific type of post-secondary institution. The categories are not mutually exclusive and overlapping is possible. Panel B reports the number of years of education an individual has attained by 2011 in each type of the post-secondary institutions described in panel A. Panel C reports different employment and income variables for the individual in the year 2011.

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

Sample	Enrollment in Post-Secondary Schooling				Post-Secondary Years of Schooling			
	3 Quartiles		4 Quartiles		3 Quartiles		4 Quartiles	
	Mean 2000 Cohort in Treated Schools	Treatment Estimate	Mean 2000 Cohort in Treated Schools	Treatment Estimate	Mean 2000 Cohort in Treated Schools	Treatment Estimate	Mean 2000 Cohort in Treated Schools	Treatment Estimate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any Post secondary education	0.490	0.016	0.569	-0.003	1.600	0.074	2.104	0.034
	(0.500)	(0.021)	(0.495)	(0.018)	(2.034)	(0.059)	(2.297)	(0.070)
University	0.102	0.046	0.201	0.055	0.291	0.183	0.742	0.247
	(0.303)	(0.010)	(0.401)	(0.015)	(0.999)	(0.045)	(1.712)	(0.070)
Academic College	0.259	-0.003	0.260	-0.038	0.732	-0.075	0.743	-0.190
	(0.438)	(0.021)	(0.438)	(0.023)	(1.443)	(0.060)	(1.451)	(0.086)
Teachers' College	0.106	0.000	0.119	-0.004	0.243	-0.006	0.294	-0.014
	(0.308)	(0.014)	(0.324)	(0.011)	(0.882)	(0.033)	(0.984)	(0.023)
Semiengineering School	0.128	-0.008	0.117	-0.013	0.218	-0.007	0.197	-0.013
	(0.334)	(0.010)	(0.322)	(0.009)	(0.639)	(0.023)	(0.603)	(0.020)
Other	0.034	-0.005	0.034	-0.006	0.116	-0.020	0.129	0.004
	(0.180)	(0.007)	(0.181)	(0.007)	(0.548)	(0.016)	(0.580)	(0.015)
Number of Observations	2,023	7,449	2,723	10,229	2,023	7,449	2,723	10,229
Weighted Number of Observations	3,095	12,043	4,202	16,112	3,095	12,043	4,202	16,112

Notes : This table presents the differences-in-differences estimates of the effect of the Teachers' Bonuses program on post-secondary schooling for the three- and four-quartiles samples described in the paper. Columns 1-4 measure enrollment into different types of post-secondary institutions, while columns 5-8 measure completed years of post-secondary education by institution type. The results are for the year of 2011. The variable "Any Post-Secondary Education" comprises all of the different post-secondary institutions reported together. Columns 1,3,5, and 7 represent the means and standard deviations for the 2000 cohort in 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 estimates 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

	2011 Outcomes		2010 Outcomes		2009 Outcomes	
	Mean 2000 Cohort in Treated Schools	Treatment Estimate	Mean 2000 Cohort in Treated Schools	Treatment Estimate	Mean 2000 Cohort in Treated Schools	Treatment Estimate
	(1)	(2)	(3)	(4)	(5)	(6)
A. 3 Quartiles Sample						
Employment Indicator (1 = Yes, 0 = No)	0.827	-0.001	0.808	0.020	0.825	0.029
	(0.378)	(0.013)	(0.394)	(0.012)	(0.380)	(0.017)
Received Unemployment Insurance Benefits Indicator (1 = Yes, 0 = No)	0.067	-0.0174	0.069	0.015	0.079	-0.011
	(0.250)	(0.008)	(0.254)	(0.009)	(0.271)	(0.009)
Total Annual Earnings (NIS)	61,854	4,442	51,558	4,869	46,945	4,405
	(52,706)	(1,947)	(45,759)	(1,988)	(41,377)	(1,590)
Total Unemployment Insurance Benefits Received (NIS)	679	-10	676	89	476	12
	(3,101)	(110)	(3,038)	(105)	(1,891)	(49)
Number of Observations	2,023	7,449	2,023	7,449	2,023	7,449
Weighted Number of Observations	3,095	12,043	3,095	12,043	3,095	12,043
B. 4 Quartiles Sample						
Employment Indicator (1 = Yes, 0 = No)	0.840	-0.010	0.817	0.008	0.832	0.014
	(0.367)	(0.010)	(0.387)	(0.010)	(0.374)	(0.015)
Received Unemployment Insurance Benefits Indicator (1 = Yes, 0 = No)	0.067	-0.0132	0.069	0.006	0.073	-0.007
	(0.250)	(0.006)	(0.253)	(0.007)	(0.259)	(0.009)
Total Annual Earnings (NIS)	65,015	3,329	53,414	4,063	47,587	4,834
	(56,493)	(1,766)	(48,452)	(1,535)	(43,584)	(1,423)
Total Unemployment Insurance Benefits Received (NIS)	673	46	666	3	430	43
	(3,041)	(79)	(3,019)	(88)	(1,803)	(57)
Number of Observations	2,723	10,229	2,723	10,229	2,723	10,229
Weighted Number of Observations	4,202	16,112	4,202	16,112	4,202	16,112

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, respectively. Columns 1-2 report results for 2011, columns 3-4 report results for 2010, and columns 5-6 report results for 2009. The variable "Employment Indicator" receives the value of 1 if 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, and 5 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 their averages as a benchmark for the treatment effect. Columns 2,4, and 5 report the differences-in-differences estimates for each of the dependent variables listed above. Standard errors are clustered at the school year level.

Table 5: OLS Relationships between High School Matriculation Outcomes, College Schooling, and Earnings at Adulthood

	Mean 2000 Cohort in Treated Schools	2011 Annual Earnings: Separate Estimate	2011 Annual Earnings: Joint Estimate Panel A	2011 Annual Earnings: Joint Estimate Panels A + B
	(1)	(2)	(3)	(4)
A. High School Matriculation Outcomes				
Average Matriculation Score	73.305	373	200	169
	(23.153)	(46)	(46)	(45)
Received High School Matriculation (1 = Yes, 0 = No)	0.538	10,289	2,381	1,268
	(0.499)	(1,446)	(2,092)	(1,993)
Number of Credit Units in Matriculation Exams	22.374	1,006	586	572
	(10.232)	(213)	(322)	(335)
Number of Honor Level Subjects	2.523	4,779	967	-159
	(1.826)	(836)	(925)	(997)
B. Post Secondary Schooling				
Enrollment in Any Post Secondary Schooling (1 = Yes, 0 = No)	0.569	13,241	10,538	8,678
	(0.495)	(1,469)	(3,610)	(3,578)
Completed Years of Any Post Secondary Schhooling	2.104	3,150	-3,926	-689
	(2.297)	(350)	(3,167)	(877)
Enrollment in University or Academic College (1 = Yes, 0 = No)	0.410	11,947	-286	-4,038
	(0.492)	(1,628)	(868)	(3,155)
Completed Years of University or Academic College	1.485	3,300	2,845	2,731
	(2.103)	(429)	(927)	(933)
Number of Observations	2,723	10,229	10,229	10,229
Weighted Number of Observations	4,202	16,112	16,112	16,112

Notes : This table presents OLS relationships between high school matriculation outcomes, college schooling, and earnings at adulthood for the four-quartiles 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 for year 2011 and the independent variables include the same variables as reported in tables 3-4, in addition to the high-school outcome variables described in the table. Column 3 reports the OLS estimate when all the high-school outcomes variables that appear in Panel A\B are controlled for in the wage regression in addition to the rest of the control variables from tables 3-4. 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 6: Estimated Treatment Effect of the Teachers' Bonuses Program when Adding High School Educational Outcomes to the DID Regression

	Added Control Variables								
	Original Treatment Estimate/ No Added Variables	Average Matriculation Score	Received High School Matriculation	Number of Credit Units in Matriculation Exams	Number of Honor Level Subjects	Math and English Test Scores and Attendance	All High School Outcome Variables	University Enrollment & Completed Years of University Schooling	All Outcome Variables
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Post-Secondary Educational Outcomes									
4 Quartiles Sample (N = 10,229)									
University Enrollment (1 = Yes, 0 = No)	0.055	0.040	0.051	0.046	0.039	0.049	0.033	-	-
	(0.015)	(0.012)	(0.015)	(0.013)	(0.011)	(0.014)	(0.010)	-	-
Completed Years of University Schooling	0.247	0.187	0.237	0.215	0.189	0.225	0.158	-	-
	(0.070)	(0.057)	(0.068)	(0.060)	(0.053)	(0.066)	(0.049)	-	-
3 Quartiles Sample (N = 7,449)									
University Enrollment (1 = Yes, 0 = No)	0.046	0.037	0.040	0.040	0.035	0.038	0.029	-	-
	(0.010)	(0.008)	(0.009)	(0.009)	(0.008)	(0.009)	(0.008)	-	-
Completed Years of University Schooling	0.183	0.151	0.163	0.161	0.143	0.157	0.124	-	-
	(0.045)	(0.040)	(0.041)	(0.041)	(0.040)	(0.042)	(0.039)	-	-
B. Earnings at Adulthood									
4 Quartiles Sample (N = 10,201)									
Total Annual Earnings in 2011 (NIS)	3,329	2,173	3,012	2,343	2,419	2,803	1,817	2,571	1,469
	(1,766)	(1,788)	(1,722)	(1,881)	(1,857)	(1,815)	(1,883)	(1,753)	(1,879)
3 Quartiles Sample (N = 7,427)									
Total Annual Earnings in 2011 (NIS)	4,442	3,396	3,769	3,440	3,533	3,347	2,696	3,962	2,486
	(1,947)	(1,974)	(1,916)	(2,073)	(2,061)	(1,953)	(2,031)	(1,910)	(2,001)

Notes : This table demonstrates the sensitivity of the treatment effect as measured in tables 3 and 4 to high school educational outcomes. Column 1 reports the estimated treatment effects from tables 3 and 4 according to the relevant sample and dependent variable. Columns 2-6 present the estimated treatement effect when the high school educational outcome variable mentioned in the column header is added to the differences-in-differences regression estimated in table 3 or 4. Column 7 present the estimated treatement effect when all five high school educational outcomes are added to the differences-in-differences regression estimated in table 3 or 4 simultaneously. Column 8 adds university education to the wage regression, and column 9 includes both high-school and university variables. Standard errors are clustered at the school year level.

Table 7: Differences-in-Differences Estimates of the Effect of the Teachers' Bonuses Program on Disability Rate

	2011 Outcomes		2010 Outcomes		2009 Outcomes	
	Mean 2000 Cohort in Treated Schools	Treatment Estimate	Mean 2000 Cohort in Treated Schools	Treatment Estimate	Mean 2000 Cohort in Treated Schools	Treatment Estimate
	(1)	(2)	(3)	(4)	(5)	(6)
A. 3 Quartiles Sample						
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.016	-0.010	0.015	-0.004	0.014	-0.001
	(0.125)	(0.005)	(0.121)	(0.004)	(0.117)	(0.004)
Total Disability Insurance Benefits Received (NIS)	445	-205	397	-117	371	-74
	(3,680)	(141)	(3,409)	(120)	(3,222)	(107)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.005	-0.004	0.005	-0.007	0.005	-0.002
	(0.069)	(0.002)	(0.067)	(0.003)	(0.072)	(0.004)
Total Work Disability Insurance Benefits Received (NIS)	102	-115	77	-141	75	-92
	(1,731)	(44)	(1,522)	(47)	(1,407)	(42)
Number of Observations	2,023	7,449	2,023	7,449	2,023	7,449
Weighted Number of Observations	3,095	12,043	3,095	12,043	3,095	12,043
B. 4 Quartiles Sample						
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.017	-0.012	0.016	-0.007	0.015	-0.003
	(0.128)	(0.004)	(0.124)	(0.003)	(0.120)	(0.004)
Total Disability Insurance Benefits Received (NIS)	461	-308	408	-188	377	-128
	(3,749)	(115)	(3,460)	(99)	(3,252)	(90)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.004	-0.003	0.004	-0.006	0.005	-0.001
	(0.065)	(0.002)	(0.065)	(0.002)	(0.067)	(0.003)
Total Work Disability Insurance Benefits Received (NIS)	83	-85	64	-100	62	-62
	(1,528)	(30)	(1,349)	(34)	(1,256)	(29)
Number of Observations	2,723	10,229	2,723	10,229	2,723	10,229
Weighted Number of Observations	4,202	16,112	4,202	16,112	4,202	16,112

Notes : This table presents the differences-in-differences estimates of the effect of the Teachers' Bonuses program on different disability rate outcomes. Panel A and Panel B report the results for the three quartile and four quartile samples described in the paper, respectively. Columns 1-2 report results for 2011, columns 3-4 report results for 2010, and columns 5-6 report results for 2009. The variable "Received Disability Insurance Benefits" receives the value of 1 is the individual received any amount of diability benefits from the national insurance institution of Israel (NII), 0 otherwise. The variable "Total Disability Insurance Benefits Received" describes the total NIS amount of disability insurance benefits the individual received in the given year. Columns 1,3, and 5 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 2,4, and 6 report the differences-in-differences estimates for each of the dependent variables listed above. Standard errors are clustered at the school year level.

Table A1: Descriptive Statistics: Treated Schools Versus all Other Eligible Schools

	2000			2001		
	Treated schools	Non treated Schools	Difference	Treated schools	Non treated Schools	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
A. School characteristics						
Religious school*	0.199	0.269	-0.070	0.182	0.258	-0.076
			(0.084)			(0.080)
Arab school*	0.260	0.099	0.161	0.284	0.107	0.176
			(0.081)			(0.087)
Lagged "Bagrut" rate	0.369	0.620	-0.251	0.377	0.607	-0.230
			(0.027)			(0.035)
B. Student background						
Father education	9.062	11.386	-2.324	9.029	11.357	-2.329
			(0.639)			(0.582)
Mother education	8.817	11.486	-2.669	8.551	10.846	-2.295
			(0.709)			(0.751)
Number of siblings	3.463	2.580	0.883	3.472	2.481	0.991
			(0.422)			(0.425)
Gender (male=1)	0.495	0.466	0.028	0.508	0.492	0.016
			(0.032)			(0.029)
Immigrant	0.031	0.027	0.004	0.022	0.010	0.012
			(0.015)			(0.009)
Asia-Africa ethnicity	0.190	0.208	-0.018	0.170	0.190	-0.020
			(0.031)			(0.030)
C. Student lagged outcomes						
Math credits gained	0.290	0.499	-0.209	0.320	0.571	-0.251
			(0.132)			(0.130)
English credits gained	0.127	0.194	-0.067	0.116	0.183	-0.067
			(0.047)			(0.050)
Total credits attempted	4.292	5.283	-0.991	4.502	5.464	-0.962
			(0.320)			(0.341)
Total credits gained	3.388	4.591	-1.203	3.633	4.773	-1.140
			(0.301)			(0.303)
Average score	56.580	69.555	-12.974	58.381	69.699	-11.318
			(2.296)			(2.010)
# obs	6,250	5,931	12,181	6,084	5,820	11,904
# schools	48	50	98	48	50	98

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 A2: Three Quartiles Sample: Employment and Income Statistics for the years 2009-2010

	Treated Schools		Control Schools	
High School Cohort	2000	2001	2000	2001
Dependent variable	(1)	(2)	(3)	(4)
A. Employment Outcomes in 2010				
Employed (1 = Yes, 0 = No)	0.808	0.821	0.869	0.873
	(0.394)	(0.383)	(0.338)	(0.333)
Total Annual Earnings (NIS)	51,558	49,209	57,192	52,122
	(45,759)	(45,374)	(47,839)	(41,002)
Received Unemployment Insurance Benefits (1 = Yes, 0 = No)	0.069	0.059	0.082	0.053
	(0.254)	(0.235)	(0.274)	(0.223)
Total Unemployment Insurance Benefits Received (NIS)	676	413	715	341
	(3,038)	(2,048)	(2,926)	(1,743)
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.015	0.011	0.010	0.011
	(0.121)	(0.103)	(0.097)	(0.102)
Total Disability Insurance Benefits Received (NIS)	397	288	267	276
	(3,409)	(2,970)	(2,916)	(2,860)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.005	0.002	0.001	0.006
	(0.067)	(0.044)	(0.036)	(0.080)
Total Work Disability Insurance Benefits Received (NIS)	77	11	18	88
	(1,522)	(391)	(609)	(1,169)
B. Employment Outcomes in 2009				
Employed (1 = Yes, 0 = No)	0.825	0.812	0.879	0.862
	(0.380)	(0.391)	(0.327)	(0.345)
Total Annual Earnings (NIS)	46,945	43,315	51,683	45,212
	(41,377)	(40,678)	(43,261)	(35,670)
Received Unemployment Insurance Benefits (1 = Yes, 0 = No)	0.079	0.064	0.085	0.068
	(0.271)	(0.245)	(0.279)	(0.252)
Total Unemployment Insurance Benefits Received (NIS)	476	348	530	345
	(1,891)	(1,591)	(1,939)	(1,405)
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.014	0.010	0.010	0.008
	(0.117)	(0.102)	(0.099)	(0.090)
Total Disability Insurance Benefits Received (NIS)	371	266	233	230
	(3,222)	(2,778)	(2,460)	(2,595)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.005	0.003	0.004	0.002
	(0.072)	(0.051)	(0.065)	(0.046)
Total Work Disability Insurance Benefits Received (NIS)	75	15	18	20
	(1,407)	(322)	(562)	(498)
Number of Observations	2,023	2,064	1,725	1,637
Weighted Number of Observations	3,095	3,071	3,049	2,828

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

Table A3: Four Quartiles Sample: Employment and Income Statistics for the years 2009-2010

	Treated Schools		Control Schools	
High School Cohort	2000	2001	2000	2001
Dependent variable	(1)	(2)	(3)	(4)
A. Employment Outcomes in 2010				
Employed (1 = Yes, 0 = No)	0.817	0.821	0.854	0.861
	(0.387)	(0.383)	(0.353)	(0.346)
Total Annual Earnings (NIS)	53,414	50,081	59,052	52,799
	(48,452)	(46,829)	(51,752)	(43,624)
Received Unemployment Insurance Benefits (1 = Yes, 0 = No)	0.069	0.056	0.076	0.056
	(0.253)	(0.230)	(0.265)	(0.230)
Total Unemployment Insurance Benefits Received (NIS)	666	387	658	346
	(3,019)	(1,970)	(2,812)	(1,740)
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.016	0.011	0.008	0.011
	(0.124)	(0.105)	(0.090)	(0.106)
Total Disability Insurance Benefits Received (NIS)	408	306	222	279
	(3,460)	(3,031)	(2,636)	(2,784)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.004	0.002	0.002	0.006
	(0.065)	(0.039)	(0.047)	(0.074)
Total Work Disability Insurance Benefits Received (NIS)	64	9	33	72
	(1,349)	(346)	(856)	(1,025)
B. Employment Outcomes in 2009				
Employed (1 = Yes, 0 = No)	0.832	0.809	0.872	0.855
	(0.374)	(0.393)	(0.334)	(0.352)
Total Annual Earnings (NIS)	47,587	43,434	53,518	44,957
	(43,584)	(41,100)	(46,332)	(36,755)
Received Unemployment Insurance Benefits (1 = Yes, 0 = No)	0.073	0.063	0.079	0.066
	(0.259)	(0.242)	(0.269)	(0.249)
Total Unemployment Insurance Benefits Received (NIS)	430	350	489	333
	(1,803)	(1,604)	(1,859)	(1,427)
Received Disability Insurance Benefits (1 = Yes, 0 = No)	0.015	0.011	0.010	0.009
	(0.120)	(0.105)	(0.098)	(0.092)
Total Disability Insurance Benefits Received (NIS)	377	276	205	212
	(3,252)	(2,799)	(2,284)	(2,380)
Received Work Disability Insurance Benefits (1 = Yes, 0 = No)	0.005	0.002	0.004	0.002
	(0.067)	(0.045)	(0.067)	(0.045)
Total Work Disability Insurance Benefits Received (NIS)	62	12	29	18
	(1,256)	(285)	(760)	(455)
Number of Observations	2,723	2,677	2,479	2,350
Weighted Number of Observations	4,202	3,916	4,017	3,977

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

Table A4: Differences-in-Differences Estimates of the Effect of Teachers' Bonuses Program on High School Education Outcomes

Sample	3 Quartiles		4 Quartiles	
	Mean 2000 Cohort in Treated Schools	Treatment Estimate	Mean 2000 Cohort in Treated Schools	Treatment Estimate
	(1)	(2)	(3)	(4)
Average Matriculation Score	66.537	2.868	72.926	2.779
	(22.599)	(1.017)	(23.098)	(0.892)
Received High School Matriculation (1 = Yes, 0 = No)	0.423	0.055	0.532	0.026
	(0.494)	(0.023)	(0.499)	(0.020)
Number of Credit Units in Matriculation Exams	20.205	0.669	22.199	0.803
	(10.238)	(0.329)	(10.257)	(0.334)
Number of Science Credit Units in Matriculation Exams	1.340	0.343	2.339	0.589
	(2.910)	(0.154)	(3.813)	(0.181)
Number of Honor Level Subjects	2.034	0.092	2.491	0.128
	(1.631)	(0.065)	(1.821)	(0.062)
Number of Observations	3,072	11,921	4,162	16,031

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 for 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.