How Responsive is Investment in Schooling to Changes in Returns? Evidence from an Unusual Pay Reform in Israel's Kibbutzim*

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Abstract

This paper uses a novel dataset to test the important theoretical prediction that the level of investment in schooling is increasing in the rate of return to education. We exploit a unique episode where different Israeli kibbutzim shifted from equal sharing to productivity-based wages in different years, resulting in sharp increases in the return to education. We use a difference-in-differences approach comparing educational outcomes of high school students in kibbutzim that reformed early (the treatment group) and late (the control group), before and after the early reforms (but before the late reforms). The treatment group is shown to be identical to the control group in observable characteristics and pre-reform mean outcomes. We find that students in kibbutzim that reformed early increased their investment in education, as reflected by outcomes such as whether they graduated high school and their average matriculation scores. This effect is stronger for males, and is mainly driven by students whose parents have lower levels of education. It is also stronger for students in kibbutzim that reformed to a greater degree. We use various falsification tests to support our identification strategy and to show that our results are not driven by other factors such as differential time trends or differential exit rates. Our findings support the prediction that education is highly responsive to changes in returns, especially for students from weaker backgrounds.

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I. Introduction

Economic models of optimal human capital investment (Ben Porath 1967, Becker, 1967) suggest that the level of investment in schooling is expected to increase in the perceived rate of return to education. The basic premise is that future labor market return in the form of earnings is a main motivation for investment in schooling, and the higher is this market rate of return the higher is the optimal level of investment. However, despite its centrality in modern labor economics, this fundamental assumption has hardly been tested empirically, both because variation across individuals in the rate of return to schooling is rarely observed and because sharp changes in this return rarely occur.[1] We use a unique episode where some kibbutzim (plural of kibbutz) in Israel, after decades of having wages independent of an individual's human capital, set wages to reflect the market rate of return to schooling. The change from equal sharing to pay-for-productivity introduced a dramatic increase in the returns to schooling for kibbutz members. We rely on this sharp change to test whether and to what extent this increase in returns induced high school students to invest more in their education, as reflected in their academic achievements.

We use administrative records collected by the Israeli Ministry of Education for six consecutive cohorts (from 1995 to 2000) of 10th grade students. The data contain detailed demographic information on each student as well as his home address. We use the latter to determine who lives in a kibbutz and when a student's kibbutz implemented the pay reform. A main outcome of interest we examine is students' achievement in the matriculation exams at the end of high school. Passing all the matriculation exams successfully and getting a matriculation diploma (equivalent to a baccalaureate diploma in most of the European countries) is a major milestone in education in Israel as it is the ticket to post-secondary schooling; it yields a substantial earning premium in the general Israeli labor market. Other outcomes of interest are whether the student graduated high school, the average score in the matriculation exams, and whether her diploma meets the university entrance requirements.

[1] There is, however, literature on the relationship between the recent skill-biased technological change and the rise in wage inequality in the US (e.g. Autor, Katz and Krueger 1999, Card and DiNardo 2002). On the long term trends in inequality and the returns to education, see Goldin and Katz (2008).

Our identification strategy relies on the fact that the pay reform was not implemented in all kibbutzim in the same year.[2] We use a difference-in-differences approach comparing educational outcomes of high school students in kibbutzim that reformed early (the "treatment group") and late (the "control group"), before and after the early reforms (but before the late reforms). We show evidence that students in the treatment and control groups are nearly identical in all observable background characteristics on which we have information, such as parental schooling and number of siblings, as well as very similar in all their pre-reform schooling outcomes.

Specifically, as our first difference, we compare students in kibbutzim that reformed early (1998-2000) with students in kibbutzim that reformed late (2003-2004). As our second difference, we compare students who were affected by the early pay reforms (were in high school after the early reform but before the late reform began) with students who were unaffected by the early reforms (were in high school before the early reforms). The difference in this difference between kibbutzim that reformed early vs. late can be interpreted as the causal effect of the reform, under the assumption that in the absence of the reform, the increase in achievements would not have been systematically different in treatment and control groups.

We find that students in kibbutzim that reformed early increased their investment in education as reflected by their educational outcomes. The mean score in the matriculation exam (*Bagrut*) increased by 3.55 points relative a pre-reform mean of 70.6, or by 0.17 standard deviations of the test score distribution. The matriculation rate increased by 4.9 percentage points and the university qualified *Bagrut* rate increased by 6 percentage points, which is almost 12 percent of the pre-reform university qualified *Bagrut* rate in the control group. The pay reform even increased the high school completion rate by a significant 3.3 percentage points, from a baseline that was already over 95 percent for the pre-reform cohorts. We further show that the effect is larger for students in kibbutzim that reformed to a larger degree. To translate our coefficients into quasi elasticities, we calculate that the pay reform increased the returns to a year of schooling for the average student by about 7 percentage points. Our estimates thus imply

[2] Kibbutzim that reformed had different experiences from kibbutzim that did not (Abramitzky 2008).

that a 1 percentage point increase in the returns to a year of schooling increases graduation rates by 0.43 (3/7) percent, improves the proportion of students graduating with a university qualified matriculation by 1.7 percent (12/7) and improves mean exam scores by 0.51 points (3.55/7).

This positive effect of the reform on education outcomes is mainly driven by students whose parents have lower levels of education. This finding is opposite to that of a recent cleanly-identified paper by Jensen (2010), which found in the context of an experiment in the Dominican Republic that students with more educated parents respond more to changes in the perceived returns to schooling. One explanation for this difference could be that parental schooling often proxies for family wealth and richer people are less financially constrained when making education choices. In our context, this channel plays a lesser role because parents' pay pre-reform was unlinked to education and productivity. The difference we find more likely reflects inherent differences in responsiveness to the change in the returns to schooling brought by the reforms.

Furthermore, unlike other papers that find that females tend to be more responsive to changes in schooling incentives (e.g. Schultz 2004, Angrist and Lavy 2009), we find a larger effect for males. This difference could stem from the fact that, unlike other studies, we study a pay reform that affects future earnings, not a program that directly rewards high performance in school. Females could be less responsive if, for example, they anticipate lower wages or if they expect to not be the main earner in their household.

Finally, we exploit the variation in the intensity of the pay reform. Specifically, some kibbutzim introduced a full pay reform and moved to a pay system that reflected market forces. Other kibbutzim initially introduced only a partial pay reform that was partly based on market forces, but which combined a more progressive tax with a wide safety net for weak members. Many partially reformed kibbutzim eventually fully reformed, and many did so during our period of study. We find that the effect of the pay reform was much stronger for students who lived in a fully reformed kibbutz throughout their three years of high school. This "intensity of treatment" effect is shown again to be larger for students whose parents have lower levels of education, and larger for males.

Our findings also shed light on the responsiveness of labor supply and educational choices to changes in the income tax, because the pay reform we study essentially reflects a sharp reduction in the income tax and our setting allows us to estimate the educational responses to this change. While it is well known that changes in taxes affect labor supply decisions in the short run[3], as pointed out by Saez, Slemrod and Giertz (2009), less is known about how such changes affect labor supply decisions in the long run, because it is difficult to identify empirically how such tax changes will affect educational choices.[4]

The identifying assumption in our empirical strategy is that the exact year in which a kibbutz reformed is unrelated to the potential outcomes of its high school students. This assumption implies that older cohorts of early- and late-reforming kibbutzim should have had similar high school outcomes on average, which we indeed find to be the case. We use two additional falsification tests to support our identification strategy. First, we show that there were no pre-reform differences between treatment and control kibbutzim in time trends of educational matriculation outcomes. Second, high exit rates during high school that are differential between treatment and control kibbutzim could be a threat to identification if, for example, students who leave experience a decline in their academic performance because they need to adjust to their new high school, or if their parents moved in order for them to attend a different school. We show that exit rates were low and unrelated to the implementation of the pay reform.

The rest of the paper is structured as follows. The next section provides a short review of the existing literature that is most closely related to our paper. Section III presents a brief background of kibbutzim and the pay reform, and of the Israeli high school system, and also describes the data. Section IV discusses the empirical framework and identification strategy. Section V presents the estimation results, section VI puts into perspective the magnitudes of the effects, and section VII concludes.

II. Literature Review

Despite an important body of theoretical literature, the empirical literature that tests the relationship between the rate of return to education and investment in schooling

[3] See also Chetty et al. (forthcoming) on how the effects of taxes on labor supply are shaped by interactions between adjustment costs for workers and hours constraints set by firms.

[4] Note that even conceptually it is not obvious that higher returns (lower marginal tax rates) will lead to more education/labor supply, as there are both income and substitution effects associated with decreased tax rates.

is relatively small. Freeman (1976) studied changes in college enrollments in the US and found that they were responsive to changes in rates of return to schooling. Kane (1994) finds evidence that whites' college enrollment rates in the 1980's in the US were responsive to the increase in returns to education, as measured by the ratio of college graduates' to high school graduates' mean earnings (the college premium). For blacks, however, there is little evidence of such a relationship. This conclusion is based on the trends in enrollment rates not being different by gender for blacks even though there were large gender differences in the trend in the college earnings differential. However, Kane (1994) also reports that students seem to be more responsive to increases in direct costs than they are to the present value of future earnings differentials. However, as noted by the author, the limitation of these findings is that they are primarily based on a coincidence of time series, namely the similar timing of a rise in returns to education and a rise in college entry. Therefore a causal interpretation of the association between returns and college enrollment is not possible in this case.

Several studies estimate the perceived rate of return to schooling, as distinct from the actual rate of return estimated in this paper, and some then assess its effect on schooling. Betts (1996) and Smith and Powell (1990) have attempted to estimate the perceived returns among high school and college students. The latter study finds that students accurately estimate the earnings of college educated workers; however, men on average expect their own earnings to be above the mean for such workers, a finding the authors attribute to an optimism bias in perceived ability. Dominitz and Manski (1996) approach the measurement of perceived returns directly by asking high school and college students to estimate what their future earnings would be in hypothetical situations of different levels of education. They find that men accurately estimate the earnings of other men with different levels of education, while women tend to overestimate the earnings of other women. Avery and Kane (2004) also ask about own expected earnings in assessing the perceived returns to college education among high school students in the Boston area. They find that students overestimate the returns to education. Jensen (2010) uses survey data from the Dominican Republic reports the opposite findings. Specifically, students under-predict their returns to education, and students who were better informed (experimentally) of the higher estimated returns were significantly less likely to drop out of school in subsequent years.

Other somewhat related research explores the relationship between the business cycle and investment in schooling. Specifically, Sakellaris and Spilimbergo (2000) focus on foreign students coming to US universities and find that for OECD countries enrollment is countercyclical, whereas it is pro-cyclical for non-OECD countries.

Our paper is also related to a literature on the achievements of high school students in kibbutzim relative to students in cities. This literature focuses on the pre-reform period. Trumper (1997) compares samples of kibbutz and city students and finds that while in junior high school (grades 8 and 9) city students were better achievers than kibbutz students, this difference became insignificant in senior high school (grades 10 and 11). Gilboa (2004) uses data from psychometric test results during the period 1992-1996 and compares achievements of kibbutz and city children. He finds no differences in the average grade of children whose parents have more than 12 years of schooling. However, among children of less educated parents, kibbutz children had higher average grades.

III. Background and Data

a. The pay reform and the rate of return to schooling in kibbutzim

i. Brief description of kibbutzim and the nature of the pay reform

Kibbutzim are voluntary communities that have provided their members with a high degree of income equality for almost a century.[5] Traditionally, all kibbutzim were based on full income sharing between members. Specifically, each member of a kibbutz was paid an equal wage, regardless of her contribution to the community. Kibbutz members who worked outside their kibbutz brought their salaries in, and these were split equally among members. This meant that monetary returns to ability and effort were

[5] For a history of kibbutzim, see Near (1992, 1997). For an overview of the economics of kibbutzim, see Abramitzky (2011).

close to zero. Specifically, there were no earnings-related returns to schooling in the kibbutz, as members earned the same regardless of their education levels.[6]

The episode that we study is a unique pay reform that kibbutzim in Israel adopted beginning in 1998. During the following years, many kibbutzim shifted from equal sharing by introducing compensation schemes based on members' productivity, which created a link between productivity and earnings in kibbutzim for the first time. These pay reforms were a response to changing external pressures and circumstances facing kibbutzim. Some contributing factors were a decline in world prices of agricultural goods, bad financial management, and a high-tech boom during the mid-1990s, which increased members' outside options considerably. But perhaps the biggest problem was that many kibbutzim had borrowed heavily in the 1980s, both to expand their housing stock and to expand their industry. In the early 1980s, it was easy and cheap for kibbutzim to borrow money because inflation in Israel was very high and loans were often not indexed to inflation. However, the Israeli government eventually decided to take action to halt the inflation and, as part of the stabilization program, raised interest rates dramatically. Kibbutzim, like many other businesses in Israel, found themselves with huge debts they could not repay. Eventually, some of the loans were erased and others were rescheduled, but living standards in many kibbutzim still fell substantially, many members left during the late 1980s and early 1990s, and talk about a major reform of kibbutz life started.

In reformed kibbutzim, members' wages reflected market wages so that members were allowed to keep a substantial fraction of their earnings for themselves. For members who worked outside their kibbutzim, market wages were the wages they received from their employers (prior to the reform, these wages were brought by members to their kibbutz and were split equally among all members). For members who worked inside, market wages were based on the wages of non-kibbutz workers of similar occupation, education, skills, and experience. A kibbutz 'tax' was deducted from members' gross

[6] Kibbutz scholars and observers have often felt, as predicted by economic theory, that under the traditional kibbutz system, kibbutz-raised children often lacked ambition and a sense of personal achievement. Bettelheim (1969) concluded that "they will not be leaders or philosophers, will not achieve anything in science or art." This quote was also cited in Gavron (2000).

wages to guarantee older members and very low wage earners in the kibbutz a safety net (i.e. a minimum wage).

ii. How the reform changed the return to schooling

To gain a sense of how big the reform was in terms of an increase in the return to education, note that pre-reform the monetary return to education was zero and post reform the return to education became similar to the rest of Israel, which is estimated by various studies at about 8% per year of schooling. In actuality, while the reform resulted in a big increase in the return to schooling, it likely increased the return to education by less than this amount for several reasons. First, monetary rewards are not the only reason people acquire education.[7] Non-monetary incentives such as prestige and care about the collective had always played a role in the pre-reform period. Second, the exit option meant that the return to education was higher than zero pre reform and some members might have acquired education to improve their wages upon exit.[8] If a high school student knew for sure that he was going to leave in the future, his perceived return to schooling was high even pre reform, and the pay reform did not change his perceived return to schooling. On the other hand, a high school student who planned on staying faced no monetary returns to schooling and the full pay reform increased his return by 8 percentage points. For an average high school student who had not yet decided whether to stay, the reform increased the perceived returns by less than the full 8%, to about 6.4%.[9] Third, for kibbutzim that only reformed partially, the post reform return are smaller so that their pay reform increased the return by a lower amount.

[7] See Oreopoulus and Salvanes (2009) for a recent paper that makes this point convincingly.

[8] As noted, a kibbutz-born individual could always choose to leave her kibbutz and earn the market rate of return on her education outside. At the same time, a range of mechanisms was in place to limit the attractiveness of this option (for example, bequests were not allowed and members could not take their share of the assets of the kibbutz with them). However, note that Israel is a small country, meaning the outside market return to education was the same for members of all kibbutzim, specifically both in kibbutzim that reformed early and later. Moreover, we show in a later section that exit rates during the period we study were relatively low and nearly identical in kibbutzim that reformed early and late.

[9] To gain a sense of the increase in return for such an average student, we note that in the decade prior to the reform, about 20% of members left their kibbutz, implying that the perceived return for such a student pre reform was 1.6% so that the reform increased their perceived returns by 6.4%. To see this, assume a high school student that plan to exit the kibbutz in the future with probability 20%. The pre-reform returns for such student is 0.2*(ReturnOutside)+0.8*(ReturnInside)=0.2(ReturnOutside)+0.8*0=0.2*8%=1.6%. That is, the pay reform increases the return to schooling for such student by 6.4%.

An alternative way to interpret the reform is in terms of a decline in the income tax rate. Before the reform, income in kibbutzim was 100% taxed. Post reform, the tax rate in kibbutzim became more similar to the Israeli tax rates. Specifically, kibbutz members faced a progressive tax system, with marginal tax rates ranging from 20 to 50%.

To illustrate how the reform increased returns to schooling, we collected data on the earnings and education of all working members in one particular kibbutz that is currently reforming its pay system from equal sharing to a full pay reform. Table 1 illustrates that while before the reform members of all education levels earned the same wage, post reform more educated members earned higher wages in this kibbutz. In addition, we collected data on post reform wages of all working members in another fully reformed kibbutz (one of our treatment kibbutzim). Pooling observations from these two fully reformed kibbutzim, Table 2 illustrates the large returns to schooling after the pay reform. Specifically, we run a regression of ln(wages) post reform on education level, with and without controlling for a member's age and age squared. A bachelor degree is associated with 30% increase in wage relative to high school education. A master degree is associated with a 45% premium, and doctoral degree with a 63% premium. Stating these different degrees in terms of years of schooling, we find that an extra year of schooling is associated with 8% higher wages, which is the same as the returns outside of kibbutzim.

To gain a sense about the differences and similarities in returns to education between kibbutz members and city residents, Klinov and Palgi (2006) used survey data from 2004 (Labor Force Survey) and 2005 (kibbutzim survey[10]) to estimate Mincerian wage equations for a sample of wage earners in Israeli cities and for a sample of kibbutz members.[11] They find that the average return to increasing education from primary schooling only to a master degree is very similar in both samples, 59.4 percent in the city and 63 percent in kibbutzim. However there are large differences between the two

[10] This is a Survey of Public Opinion in kibbutzim to estimate Mincerian wage equations for a sample of wage earners in Israeli cities and for a sample of kibbutz members. The survey was conducted in 2005 by Michal Palgi and Eliat Orchan of the Institute for Research of the Kibbutz at Haifa University.

[11] The kibbutzim sample included equal pay non-reformed (28%), partial differential pay (27%) and full differential pay kibbutzim (45%). The survey includes 818 adult (age 21+) members but the wage data is available only for 625 individuals, mostly from reformed kibbutzim. The survey provides information on demographics, education and monthly (grouped) earnings. The mean monthly earnings in both samples were very similar, 6,568 Shekels in the kibbutzim and 6,248 Shekels in the CBS sample.

samples in the return to different levels of schooling. Completed high school relative to primary schooling yields a return of 23 percent in the city and only 13 percent in the kibbutz, but the return to college education relative to high school completion is 24 percent in kibbutzim and only 17 in cities. Similarly, masters and Ph.D. degrees (relative to college education) yield a 27 percent return in kibbutzim versus 19 percent in cities. The authors suggest that this pattern could be a result of the limited alternatives in the kibbutz for the less-educated members, who are therefore not optimally matched to an occupation and industry. In contrast, more educated members tend to work outside the kibbutz, where more options are available, and therefore are allocated more optimally to occupations and industries. We note that, if anything, this higher return to post high school education of kibbutz members is an additional incentive to invest more in human capital while in high school in order to increase the chances of getting into and succeeding in college, a hypothesis we test in this paper.

iii. Why compare kibbutzim that reformed in different years

Kibbutzim that even today are based on equal sharing and never reformed differ from those that did in that they had different experiences in the decade leading to the reform period. Specifically, kibbutzim that reformed experienced a deeper financial crisis and higher exit rates in the decade leading to the reform (Abramitzky 2008). Kibbutzim that did not reform are therefore not the most natural comparison group for those that did.

Kibbutzim that implemented the same pay reform a few years apart from each other are more likely similar to each other. While it is difficult to know exactly why some kibbutzim reformed in 1998 and others in 2003 this likely reflects differences in the degree of intra-kibbutz opposition to the reform by the objecting minority (often older members), and is unlikely to be related to the outcomes we study in this paper.[12] Indeed,

[12] Since the pay reform was such a fundamental change in the key defining principle of kibbutzim, implementing it in a kibbutz required the overwhelming support of a large majority of the kibbutz's members. This was not an easy process, because such a reform was against the original ideology of the kibbutz and many members, especially the older cohorts, had lived their entire lives under full equal sharing and strenuously resisted the change. Older members were also potentially the biggest losers from linking salaries to productivity because they no longer worked, but had supported the earlier generation by working during the previous decades of equal sharing. Some members even took their kibbutz to court, claiming that a shift from equal sharing meant reneging on the original contract they entered with kibbutzim years earlier. Younger members were the ones typically leading the reform. They claimed that

we later show that students in kibbutzim that reformed earlier were indistinguishable from those in kibbutzim that reformed later in both their background characteristics and their pre-reform educational outcomes.

iv. How salient was the reform

Evidence suggests the move from equal sharing to differential pay signaled strongly to young adults in the kibbutzim an increase in the financial rewards to human capital. First, this pay reform was a dramatic change in the returns to skill. Whereas before the reform wages were equal for all members of a kibbutz, the reform introduced huge productivity-related wage differences within a kibbutz for the first time. This increase in the return to skills was noticeable within a family, as students' parents experienced a decrease or increase in their earnings depending on their skills (Table 1 illustrates this point). This change was also noticeable in kibbutzim as a whole. A survey of 3000 kibbutz members conducted by Pilat institute in 2004 reveals huge wage differences by occupation and education. For example, a director of a kibbutz sector (e.g. agriculture sector or industry sector) started to earn close to 30,000 New Israeli Shekels (NIS) (about $8000 per month), and members in leading positions such as the main secretary (chairman) and the treasurer of the kibbutz earned over 15,000 NIS (about $4000). Over 80% of members holding such positions have academic degrees. In contrast, a member working as a menial laborer in the kitchen or in the laundry, none of whom had an academic education, earned less than 4,000 NIS (about $1000). A more recent survey in 2009 that included 180 kibbutzim that reformed their pay structure reveals large pay gaps within kibbutzim. The survey looked only within kibbutzim; it provided data on the monthly wages of 120 different occupations. The highest gross monthly income recorded in the survey was 17,500 NIS ($4,600) and the lowest 4,100 NIS ($1,080). This range suggests large income inequality, which is most likely much

times had changed and a reform that linked salaries to productivity was necessary for the kibbutzim to remain viable communities. The eventual reforms almost always protected older members who didn't work by providing them a safety net.

higher once the wages of the members employed outside the kibbutz are taken into account.[13]

Second, the pay reform was highly noticeable by members. The pay reforms in kibbutzim have been the most discussed topic in kibbutzim since reforms started. The new productivity-based sharing rules were hotly debated and voted on by members in kibbutzim. Booklets elaborating on the reforms were distributed to all members. There were even some cases of kibbutz members suing their kibbutz because of the pay reform. The pay reform also received a lot of attention in the media both in Israel and abroad. Naturally, high school students in kibbutzim observed the heated discussions over the pay reform and saw their parents' wages increase or decrease substantially depending on their education and skills. Thus they must have been aware both that their kibbutz had instituted a pay reform and of its practical implications.

We note that even though people in kibbutzim grew up in communities where the link between schooling and earnings was essentially non-existent, they were in a very good position to understand their rate of return to schooling. Kibbutzim are typically located close to cities, kibbutz-born children interact with non-members in their high schools, they very often have family outside of the kibbutz, and in general, unlike American communes, kibbutzim are not isolated from the Israeli society as a whole and they are well aware of their outside options (Abramitzky 2011). Moreover, with the implementation of the reforms, kibbutz members received detailed information about the new sharing rule and how earnings were now going to be linked to productivity and reflect market forces.

v. Did late reformers observe early reformers and anticipate they would reform too

While the pay reform was implemented starting at a particular date and created a sharp change in the wage structure, we cannot rule out that members in kibbutzim that reformed later observed the reforms in other kibbutzim, and anticipated that at some later date their kibbutz would reform too. However, three relevant things are worth noting. First, conceptually, we note that even if anticipation effects were present, such effects

[13] This information is provided in a haaretz article in 17/09/2009. [www.haaretz.co.il/hasite/objects/pages/PrintArticle.jhtml?itemNo=1115205]

make it more difficult for us to find an impact of the reform, because it would imply that students in the control group too perceived some possible increase in the returns to education and increased their investment in schooling accordingly. Second, this is one reason we choose as a control group kibbutzim that reformed at least four years after the treatment kibbutzim reformed, making such anticipation effects less likely and less prominent if they exist. Third, empirically, we don't find evidence for such an effect, in the sense that educational outcomes in control kibbutzim are similar for the earlier and later cohorts.

b. The Israeli high school system

Israeli high school students are enrolled either in an academic track leading to a matriculation certificate (*Bagrut*) or in an alternative track leading only to a high school diploma. The *Bagrut* is completed by passing a series of national exams in core and elective subjects taken by the students between 10th and 12th grade. Thus, *Bagrut* certificates are typically obtained at the end of senior year (twelfth grade) or later.

Students choose to be tested at various proficiency levels, with each test awarding one to five credit units per subject, depending on difficulty. Some subjects are mandatory and many must be taken for at least three units. Advanced level subjects are those subjects taken at a level of 4 or 5 credit units. A minimum of 20 credit units is required to qualify for a *Bagrut* certificate, though some university study programs require more, and students must also satisfy distribution requirements. About 52 percent of all high school seniors received a matriculation certificate in the 1999 and 2000 cohorts (Israel Ministry of Education 2001). Roughly 60 percent of those who took at least one *Bagrut* subject test ended up receiving a *Bagrut* certificate.

The matriculation certificate is a prerequisite for university admission and receiving it is one of the most economically important educational milestones. Similar high school matriculation exams are found in many countries and in some states in the United States. Examples include the French Baccalaureate, the German Certificate of Maturity, the Italian Diploma di Maturità, and the New York State Regents examinations.

Although the *Bagrut* is an Israeli institution, it can be understood in the American vernacular as a "college-bound" indicator. Most of the Israeli students who fail to complete a *Bagrut* still finish their secondary schooling. Nevertheless, postsecondary schooling options for high school graduates without a *Bagrut* are limited[14]; very few will obtain further schooling. Even institutions that are not otherwise very selective, such as teachers' colleges and two-year professional programs for nursing, optometry, and computer programming, favor applicants with a *Bagrut* certificate. Consistent with this, regression evidence from the Israeli census suggests that the economic returns to a *Bagrut* are high. While there is no experimental evidence for the earnings consequence of *Bagrut* certificate, Abramitzky (2009) shows that having at least a high school diploma (which means completing 12 years of schooling but not achieving a Bagrut certificate) is associated with 36% higher earnings.[15]

We note that kibbutz children typically go to regional high schools, where they are mixed with children from moshavim (small villages not based on equality and some of whose inhabitants are farmers) and other kibbutzim.

c. Data

The empirical analysis is based on a sample that includes high school students who live in kibbutzim at the start of 10th grade, the information on whom is drawn from several administrative data files obtained from the Ministry of Education in Israel. We use administrative records collected by the Israel Ministry of Education for six consecutive cohorts (from 1995 to 2000) of 10th grade students. The data are based on annual reports submitted by school authorities to the Ministry of Education at the beginning of the school year. Each record contains an individual identifier, a school and class identifier, and detailed demographic information on the student: date of birth, gender, parental education, number of siblings, year of immigration (where relevant),

[14] In the US context, Hoxby (2000) shows that people who invest in education at a more selective college earn back their investment several times over during their career. Dale and Krueger (2001) find that students who attended more selective colleges do not earn more than other students. However, the average tuition charged by the school is significantly related to the students' subsequent earnings. They also find a substantial internal rate of return from attending a more costly college and that the payoff to attending an elite college appears to be greater for students from more disadvantaged family backgrounds.

[15] In another context, a recent quasi-experimental study of exit exams in Texas suggests that those who pass these exams go on to get more postsecondary schooling than they otherwise would have (Francisco Martorell, 2005).

ethnicity and, importantly, the student's home address, which allow us to determine who lives in a kibbutz.

We also use the home addresses of students to identify which kibbutz they live in, which allows us to link these student-level data with additional data collected by the Institute for Research of the Kibbutz and the Cooperative Idea (Getz 1998-2004) on the date at which each kibbutz reformed. We can thus classify students as belonging to kibbutzim that reformed early, or kibbutzim that reformed late, and by the degree of their kibbutz's reform. We use 10th grade to define the base population because it is the first year of high school and the last year of compulsory schooling. Therefore, for every 10th grade cohort at the year of the reform or following it, any change in enrollment or outcomes should be treated as endogenous.

We link the students' files with administrative records on schooling outcomes. We focus on the following matriculation outcomes that are available for all the years: whether the student graduated high school, whether the student received a matriculation certificate, whether the student received a matriculation certificate that meets university entrance requirements (at least 4 credits in English and another subject at a level of 4 or 5 credits, in addition to being awarded the certificate), and the average score in the matriculation exams. Roughly, 6 percent of the students in the sample did not take the matriculation exams. These students get zero values in the average score. The other three matriculation outcomes that we use, matriculation status, matriculation status that meets university entrance requirements, and the high school completion indicator, do not require such imputation; a zero value that students get for these other outcomes is real and not an imputed measure of their achievements.

IV. Identification Strategy and Estimation

We take advantage of the different timing of the reforms in different kibbutzim to construct an appropriate control group in order to estimate the causal effect of the pay reform on human capital investment. We use a difference-in-differences approach comparing educational outcomes of high school students in kibbutzim that reformed early (treatment group) vs. late (control group), before and after the early reforms (but before the late reforms). As our first difference, we compare students in kibbutzim that reformed early (1998-2000) as the "treatment group", with students in kibbutzim that reformed late (2003-2004) as the "control group". As our second difference, we compare students who were in high school when the early reform started but before the late reform began (10^{th} grade students in 1999 and 2000), with those who were in high school before the early reforms (10^{th} grade students in 1995 and 1996). Figure 1 illustrates our identification strategy, and the time line of the early and late reforms, and of the affected and unaffected cohorts.

The identifying assumption in this strategy is that the exact timing of the reform is unrelated to potential outcomes of high school students. This assumption implies that older cohorts of early and late reformed kibbutzim should have had similar high school outcomes on average. Specifically, since kibbutzim started to reform their pay systems in 1998, all children who graduated from high school in 1997 or before could not have been affected by the reforms because they left high school before the pay reforms began. For younger children, the exposure is an increasing function of their date of birth. Hence, the effect of the pay reform should be close to 0 for cohorts of children who graduated in 1998 and increasing for younger cohorts. Therefore, the basic idea behind the identification strategy is to compare the difference in high school outcomes between potentially affected and unaffected cohorts in a kibbutz that reformed early and the respective difference in a kibbutz that reformed late. The difference in these differences can be interpreted as the causal effect of the reform, under the assumption that in the absence of the reform, the increase in achievements would not have been systematically different in students from early- and late-reforming kibbutzim.

In the next section we provide two related pieces of evidence in support of this assumption. First, we show that students in the treatment and control groups are practically indistinguishable in terms of their mean background characteristics. Very important in this regard is the similarity in the level of education of children's parents, which implies that the students in kibbutzim that reformed early vs. later were similar in their academic potential. Similarly, we show that students in the two groups are similar in their pre-reform mean schooling outcomes. This evidence implies that the first difference is close to zero. Second, we show that the treatment and control kibbutzim were on the same time trend of educational matriculation outcomes between 1992 (the first year with available *Bagrut* data) and 1998, when the first wave of reforms started.

We next discuss the estimation framework. Consider first the difference between the mean matriculation outcomes of a young cohort exposed to the reform and that of an older cohort not exposed to the reform. If the pay reform led to an increase in high school achievements, the difference between young and old cohorts in the affected kibbutzim relative to non-affected kibbutzim can be modeled as in the following simple difference-in-differences regression:

$$Y_{ikc} = \alpha_c + \beta_1 (EarlyReform)_k + \beta_2 (AffectedCohort_c EarlyReform_k) + \varepsilon_{ikc} \qquad (1)$$

where Y_{ikc} is the achievement outcome of student *i* in kibbutz *k* in cohort *c*, α_c are cohort dummies (for students starting high school in 1995, 1996, 1999 and 2000), $(EarlyReform_k)$ denotes whether the student belonged to a kibbutz that implemented the reform early, and $(AffectedCohort_c EarlyReform_k)$ is the interaction of interest, namely whether the student belonged to the affected (young) cohort and lived in a kibbutz that reformed early. Standard errors are adjusted for clustering at the kibbutz level.

In addition to the simple difference-in-differences regressions, we also run "controlled" specifications where we include kibbutz fixed effects, cohort fixed effects, and a vector of the student's background characteristics. We therefore estimate the following model:

$$Y_{ikc} = \gamma_k + \alpha_c + \beta_1 (AffectedCohort_c EarlyReform_k) + \beta_2 X_{ikc} + \varepsilon_{ikc} \qquad (2)$$

where γ_k are kibbutz fixed effects, X_{ikc} are student *i*'s characteristics: gender, father's and mother's education, number of siblings, a set of ethnic dummies (originate from Africa/Asia, Europe/America, the former Soviet Union (FSU), Ethiopia and other countries), and the rest of the variables are as in equation (1). Note that once we include kibbutz fixed effects in the model, we have to drop the $(EarlyReform_k)$ term. We note further that when comparing treated and control students, the kibbutz fixed effects essentially also capture school fixed effects because almost all students from the same kibbutz attend the same high school. We thus practically not only compare treated and control students in the same kibbutz, but also in the same school. We also note that kibbutz fixed effects provide an alternative to kibbutz-level clustering. By absorbing the kibbutz-level variation in the outcome variable, they may lead to a gain in precision; clustering standard errors makes very little difference.

An implication of the identification assumption can be tested because in none of the kibbutzim were individuals aged 18 or older in 1999-2000 exposed to the reform. The increase in high school matriculation achievements between cohorts in this age group should not differ systematically across kibbutzim that adopted the reforms earlier and those that adopted them later. We explore this control experiment and contrast the outcomes of two pre-reform cohorts, the 10th graders in 1995 and 1996 against the 10th graders in 1997. These are the three cohorts that started high school in the three years that preceded the first round of the pay reform. For our identification strategy to be convincing these difference-in-differences estimates should be close to zero.

a. Are the control and treatment groups observationally equivalent?

In this subsection we test directly whether the students in the treatment and control groups are statistically indistinguishable in terms of their observed characteristics. To address this issue, we check whether the treatment status (early reformed kibbutzim) is correlated with students' background variables like parental education, family size, and proportion of new immigrants. We perform these tests for two pre-reform cohorts (10th graders in 1995 and 1996), both separately and jointly, and for the post-treatment cohort of 10th graders in 1999 and 2000. For the pre-treatment cohorts we also check whether their academic high school matriculation outcomes are similar.

Table 3 presents the sample of kibbutzim and students by year of reform and by cohort. In the period 1998-2000, 74 kibbutzim reformed while 33 reformed in the period 2003-2004. The sample of students includes the cohorts of 10th graders in 1995-1996 as pre treatment and of 1999-2000 as post treatment. The pre-treatment sample includes 1,701 students while the post-treatment sample includes 1,648 students. We have also experimented with a larger control group by including in it 13 kibbutzim that reformed in 2005, but the results were unchanged and we therefore do not report them here.

Panel A of Table 4 provides evidence on these balancing tests and presents the mean student characteristics for each cohort by treatment status. In columns 1-3, we present the means for the 1995-1996 pre-reform cohorts and in columns 4-6 the means for the 1999-2000 post-reform cohorts.[16] The treatment and control sample means for the pre-reform and post-reform cohorts are presented in columns 1-2 and columns 4-5 respectively. The within-cohort treatment-control differences are presented in columns 3 and 6, using the following balancing regressions:

$$X_{ik} = \alpha + \beta(EarlyReform_k) + u_{ik} \tag{3a}$$

and:

$$Y_{ik} = \alpha + \beta(EarlyReform_k) + u_{ik} \tag{3b}$$

where $(EarlyReform_k)$ again denotes whether the student belonged to a kibbutz that implemented the reform early. Standard errors are adjusted for clustering at the kibbutz level. Below we also discuss comparisons across cohorts within treatment and control groups (results are available from the authors).

Table 4 shows that student background characteristics are very similar in the treatment and control groups, both for pre and post cohorts. For example, focusing first on the pre-reform cohorts, we see that levels of parental education are very similar in control and treatment, with just over 13 years of schooling for both mother and father. The differences in parental education presented in column 3 are -0.292 (s.e. 0.174) for mother's years of schooling and -0.328 (s.e. 0.264) for father's years of schooling. The two respective differences for the post reform cohorts are -0.140 (s.e. 0.229) and -0.523 (s.e. 0.419). Note that these differences are not statistically different from zero and equally importantly they are very small relative to the respective means. The similar

[16] In Table 3, we used the 1995 and 1996 cohorts together as the pre-treatment baseline but balancing tests based on each of these cohorts separately are not different from when pooling them in one sample.

levels of parental education in the treatment and control groups can also imply that students in the two groups had similar academic potential, both before and after the pay reform.

Similarly small and non-significant differences are also seen in all the other background characteristics. Out of the 16 estimated differences in background characteristics, the only one that is significant (at the 10% level of significance) is the difference in proportion of students of European/American ethnic origin in the post-reform sample. Note this lone significant control-treatment difference is unlikely to reflect a consistent pattern because statistically we expect one significant difference at the 10% level even in the absence of a true difference between the samples, and additionally the sign of this difference is positive while the respective difference for the pre-reform cohort is negative. We therefore view the results presented in Table 4 as an indication of near perfect balancing, meaning that, within cohorts, the treatment and control group are indistinguishable in their observables.

The above close similarity in background characteristics is also reflected in similarly small and insignificant differences in mean outcomes of the control and treatment groups in 1995/1996, presented in Panel B of Table 4. Recall that these means are pre-reform outcomes and therefore they imply an equal baseline for both groups. For example, the mean high school completion rate in 1995/96 is 95.1% in the treatment group and 96.7% in the control group, and the difference between the two is 1.6 percentage points and not statistically different from zero (s.e. 1.1%). The mean *Bagrut* (Israeli matriculation exam) completion rates are 54.9 and 56.9 and in the control group and treatment group, respectively, and the small difference (2.0) is again not significant (s.e. 3.6). A similar pattern exists for the other outcomes. These unconditional simple mean differences being close to zero is a good indication of a compelling natural experiment.

Table 4 also allows us to compare the cohorts within the control and treatment groups over time. The most notable change is in parental schooling, which is higher for the 1999/2000 cohort. For example, mean father's years of schooling increased in the control group from 13.6 in 1995/1996 to 14.1 in 1999/2000. Similarly, mother's years of schooling in the control group increased by about 0.4, from 13.7 percent to 14.1 percent.

However, the treatment-control differences remain balanced because equal changes occurred in both groups. It is also worth noting that the magnitude of the changes is small relative to the magnitude of the independent variable. In any case, in the outcome regressions we include specifications that control for the student background covariates (to allow a reduction in the variance of the error term) and we also include kibbutz fixed effects.

b. Did the control and treatment kibbutzim experience different exit rates?

We note that exit is not necessarily a threat for identification, even if the reform changes the probability of exit from the kibbutz. To see this, recall that post reform (in a fully reformed kibbutz) the return to schooling facing a kibbutz member is the same (8%) regardless of whether the student plans to stay or leave (because returns inside and outside the kibbutz become the same). That is, the change in return to schooling is the same regardless of whether the student is more or less likely to exit post reform.

At the same time, high exit rates during high school that are differential between treatment and control kibbutzim could be a threat to identification if, for example, students who leave suffer academically because they need to adjust to their new high school, or if their parents moved to allow them to attend a different school. We address this concern by checking whether the likelihood that a student leaves a kibbutz (by moving to a non-kibbutz community) is associated with the timing of the reform in his kibbutz. We identify students who exit using changes in their home address over time. We define a student as exiting if he lived in his kibbutz at the start of the 10th grade and lived outside it at the end of his 12th grade. E_{ik} is therefore an indicator that is equal to 1 if a student left the kibbutz before completing 12th grade, and zero otherwise, and we use it as the dependent variable in following balancing equation:

$$E_{ik} = \alpha + \beta(EarlyReform_k) + u_{ik} \quad (4)$$

We estimate this equation for three different samples, two pre-treatment (1995-96 and 1997-98) and one post treatment sample (1999-2000). Standard errors are adjusted for clustering at the kibbutz level.

Table 5 shows that the likelihood that a student leaves his/her kibbutz is relatively low and unrelated to the implementation of the pay reform. The exit rate from kibbutzim of the cohort of 10^{th} graders in 1994/5-95/6 was 4.2 percent in the control group and 5.6 percent in the treatment group. The difference between these two rates is 0.015 (s.e. 0.016). We note that the same patterns remain for the post-reform cohort of 10^{th} graders in 1999-2000: the respective exit rates were 3.8 and 5.2 percent and the difference is 0.014 (s.e. 0.011). Clearly, the difference in the differences is zero. Exit rates also remained the same over time in both the treatment and control groups. In the control group the exit rates declined from 4.2 in 1995-1996 to 3.8 in 1999-2000, practically no change. In the treatment group the exit rates declined from 5.6 in the pre-reform periods to 5.2 in 1999-2000 post reform years. These results show that the small and insignificant control-treatment differences within each period were paralleled by constancy in the exit rate in each of the two groups. The similarity in exit rates between treatment and control groups and over time within these groups suggests no association between the timing of the reform and the level and changes in the exit rates from early- and late-reforming kibbutzim. This suggests that the likelihood that a student leaves his/her kibbutz is unrelated to the implementation of the pay reform.[17]

c. Did the control and treatment kibbutzim experience different pre-reform time trends?

We use pre-reform data on the kibbutz mean matriculation rate and *Bagrut* mean test score (two representative outcomes; the evidence for the other outcomes is identical) from 1993 to 1998 to estimate differential time trends for treatment and control kibbutzim. The unit of observation in this analysis is the kibbutz. We employ two methods for this estimation. First, we estimate the following constant linear time trend

[17] We note that students who exit are still included in the sample because excluding them would imply a sample selection criterion based on an endogenous variable.

model while allowing for an interaction of the constant linear trend with the treatment indicator:

$$Y_{kc} = \gamma_k + \beta_1 c + \beta_2 (c \times EarlyReform_k) + \nu_{kc} \tag{5}$$

where c is the cohort (year the student started high school) so that β_1 measures the constant linear time trend and β_2 measures the mean treatment-control difference in this time trend. We also include specifications with the main effect for the treatment group (the indicator of early reform) instead of kibbutz fixed effects. Second, we estimate a model where we replace the linear time trend variable with a series of year dummies and include in the regression an interaction of each of these cohort dummies with the treatment indicator. Specifically, we estimate the following equation:

$$Y_{kc} = \gamma_k + \alpha_c + \beta_c (EarlyReform_k) + \nu_{kc} \tag{6}$$

For each cohort c, β_c measures the mean treatment-control difference in the outcome. The estimates from both models suggest that there is a time trend in the educational outcomes used, but this trend is identical for treatment and control kibbutzim. These results are presented in Table 6. Panel A presents the estimates of the linear trend model. The mean trend is an annual increase of 0.026 in the matriculation rate and a 1.225 points annual increase in test scores. The estimated coefficient on the interaction of this trend with the treatment indicator is practically zero in both cases. Moreover, the estimated coefficient of the treatment indicator main effect is zero in both cases, again confirming the balancing tests' results on pre-reform outcomes presented in Table 4.

Panel B presents the estimates of the year dummies model. The evidence presented in panel B is fully consistent with the linear trend model. The interaction terms of the treatment indicator with the year dummies are all small and not significantly different from zero; we also note that some are positive and others are negative, lacking any consistent pattern. This conclusion is supported by the fact that we cannot reject the hypothesis that all the interaction terms are jointly equal to zero. These F tests are reported in the bottom row of the table for the regressions reported in column 2 and column 4. We are therefore confident that there were no pre-1998 existing differential time trends in early- and late-reforming kibbutzim that could confound the estimated treatment effects that we present below.

V. The Effect of the Reform on Educational Outcomes

a. Basic Results

Panel A of Table 7 reports simple mean outcome differences between treatment and control for pre-reform (unaffected) cohorts (1995-1996) and for post-reform (affected) cohorts (1999-2000) of 10th graders. These results are presented in the first two rows of the table while in the third row we present simple difference in differences estimates with no additional controls. In the fourth row we present the difference in differences estimates which are based on regressions that also include individual characteristics and kibbutz fixed effects. Each cell in the table shows the estimated coefficient on the affected cohort in treated kibbutzim.

Table 7 shows a positive coefficient of interest for all schooling outcomes. That is, relative to control kibbutzim, there was a larger improvement in schooling outcomes following the reform in treated kibbutzim. Two things are worth noting before we describe the results. First, the pay reform increased the high school completion rate despite the fact that this rate was already over 95 percent for the pre-reform cohorts in the sample, implying limited scope for improvement. The estimated treatment effect on the high school completion rate is 0.033, amounting to a 3 percent improvement. Second, the simple and controlled difference in differences estimates of this treatment effect are similar, which is a result of the near perfect balancing between treatment and control in observables characteristics and in pre-reform outcomes.

Turning to the estimated treatment effect on the other outcomes, the mean exam score is up by 3.55 points relative a pre-treatment mean of 70.6, or 0.17 standard deviations of the test score distribution. The matriculation rate is up by 4.9 percentage points and the university qualified *Bagrut* rate is up by 6 percentage points, which amounts to almost 12 percent of the pre-reform university qualified *Bagrut* rate in the control group. The improvement in the university qualified *Bagrut* rate could be driven by two particular improvements. The first is an increase in the proportion of students who enroll in and pass the English matriculation program at more than a basic level. The second is an increase in the proportion of students who have passed the matriculation program in at least one advanced placement subject. These two criteria are an admission requirement for all universities and most colleges Israel. The improvement we observe likely reflects a higher intention to enroll in post secondary schooling. Finally, we note that the estimated gain from the pay reform is due to a positive treatment-control difference in outcome means in the post-reform period, coupled with a negligible or zero respective difference in the pre-reform period.

To translate these coefficients into quasi elasticities, recall that the pay reform increased the returns to a year of schooling of the average student by about 7 percentage points. Our estimates thus imply that a 1 percentage point increase in the returns to a year of schooling increases high school completion rates by 0.43 (3/7) percent, improves the proportion of students graduating with a university qualified matriculation by 1.7 percent (12/7) and improves mean scores by 0.51 points (3.55/7).

An implication of the identification assumption that the exact timing of the reform was unrelated to students' potential outcomes can be tested because individuals aged 18 or older in 1998 were not exposed to the program. The increase in high school matriculation achievements between cohorts in this age group should not differ systematically across kibbutzim that adopted the reforms earlier and those that adopted them later. In Panel B of Table 7, we present this control experiment. We contrast the outcomes of two pre- reform cohorts, the 10th graders in 1995-1996 and the 10th graders in 1997-1998. These placebo estimated difference in differences are very different from the treatment estimates presented in Panel A of Table 7, and they are very close to zero. For example, the placebo estimate of the effect on average *Bagrut* score is 0.304 (s.e. 1.544) and the estimates on the two *Bagrut* diploma outcomes are actually negative, though not significantly different from zero. We also conduct a placebo test contrasting the outcomes of the 10th graders in 1995 against the 10th graders in 1996 and find similar results, i.e. no effect; results (not presented) are available from the authors. These results provide additional suggestive evidence that the difference in differences estimates based on comparing the before and after cohorts are not driven by inappropriate identification assumptions.

b. Allowing for Heterogeneous Effects

i. Heterogeneous effect by social bakground

First, we look at whether the pay reform affected students with different social backgrounds differently. On the one hand, we expect students from lower social backgrounds to be more affected by the change in return if they are less likely to have inherent motivation to invest in schooling and will only do so when given external incentives, or because their parents suddenly experienced a decline in their earnings because they were less educated. On the other hand, students whose parents are more educated might receive more help at home or elsewhere, thus be in a better position to improve their schooling when given the incentives. We stratify the sample by parental schooling, splitting the sample into two groups as follows: students whose mothers have 13 or more years of schooling (50% percent of students) and the rest. Similarly, we stratify the sample by the father's years of schooling and find similar results.

The heterogeneous estimates by parental schooling presented in Panels A and B of Table 8 suggest that almost all the mean effects are coming from the sample of students for whom parents' schooling is below the median. Therefore these estimated treatment effects are much larger than the basic results presented in Table 7, and their percentage increases are also larger because the counterfactual means for these students are much lower than the mean of the overall sample. Based on partitioning the sample by mother's schooling, the estimated effect on high school completion is 0.049 (s.e. 0.024) for the students from low education families and 0.014 (s.e. 0.019) for pupils from high education families.[18] The two respective difference in differences estimates for the average score are 6.175 (s.e. 2.553) and 0.329 (s.e. 2.114), for the *Bagrut* rate they are 0.116 (s.e. 0.053) and -0.031 (s.e. 0.047), and for the university qualified *Bagrut* rate they are 0.100 (s.e. 0.053) and 0.002 (s.e. 0.048).

[18] We note that this higher effect on high school completion likely also reflect the "ceiling effect" on this outcome. That is, high school completions was already very high pre-treatment (95%) suggesting a very limited role for improvement. Because students from low education families had a somewhat lower pre-treatment high school completion rate, the treatment had more scope to improve their outcomes.

The results stratified by father's schooling are very similar and the implied percentage increases in the low education group are again large, except for the effect on high school completion, which in this case does not differ between the two groups. Specifically, the estimated effect on high school completion is 0.033 (s.e. 0.027) for the students from low education families and 0.031 (s.e. 0.017) for pupils from high education families. Overall, as can be seen in the top panels of Table 8, the estimates of the effect on the other outcomes are large and significant for the low education families but small and insignificant for students from high education families.

This result by parents' education level is exactly the opposite of Jensen's (2010) result that, in the context of an experiment in the Dominican Republic, students with more educated parents respond more to changes in the perceived returns to schooling. One explanation for this difference could be that parental schooling often proxies for family wealth, and richer people are less financially constrained when making education choices. In our context, this channel plays a lesser role because parents' pay was unlinked to education and productivity. The difference we find more likely reflects inherent differences in responsiveness to the change in the returns to schooling brought by the reforms. Alternatively, it could be that students from weaker backgrounds in our context were more affected by the reform because their parents experienced a decline in earnings.

Another possibility for why we find little effect for students from high social background is that pre reform high-ability members invested more in education because there was low opportunity cost in terms of forgone earnings, and they preferred to be in school or university than to be working in an unskilled job. We note that a low opportunity cost is an inherent feature of high income tax rates.

ii. Heterogeneous effect by gender

Next we allow for heterogeneity by gender. Male and female students have been shown to respond differently to incentives (e.g. Schultz 2004, Angrist and Lavy 2009), with females typically responding more to incentives. However, our estimates, presented in Panel C Table 8, suggest a stronger effect on males than on females, although the standard errors of the estimates are not precise enough to reject no gender differences. The estimated effect on high school completion is 0.052 (s.e. 0.023) for males and 0.011

(s.e. 0.019) for females, almost significantly different from each other. The two respective estimates for the average test scores are 4.820 and 2.549 and similar relative gaps are evident for the two *Bagrut* diploma related outcomes.

Our findings that boys are more affected by the pay reform, in particular in the school completion outcome, stand in contrast to Schultz (2004), who found that girls' school completion responded more to the incentives introduced by Progresa in Mexico. Our findings are also different from Angrist and Lavy (2009), who found that girls' *Bagrut* diploma attainment was affected by conditional bonus payments, whereas boys did not react to this monetary incentive. In these papers, girls responded more to an increase in incentives designed to directly increase educational outcomes. In our context, the pay reform does not increase such short run incentives to perform better in school. In contrast, the pay reform we study operates through affecting the future rewards in the labor market. It is possible that females perceive a lower return to education in the labor market, expect lower wages on average, or do not expect to become the main earner, for example because they plan to play a bigger role in raising children. Indeed, in regressions we run using the 1998-2000 Israeli labor force surveys and matching occupations to their mean earnings using income surveys, we find that females (both in kibbutzim and outside them) are substantially more likely to work in lower paying occupations; they sort into occupations and industries that pay around 20% less on average (regression results are available from the authors upon request).

c. Allowing for differential effect by "intensity" of reform

The pay reform was not identical across kibbutz and some kibbutzim reformed to a greater degree than others. Specifically, some kibbutzim introduced a full pay reform and moved to a "safety net" model that reflected market forces (and only a small additional safety net to low-earners). Other kibbutzim introduced only a partial pay reform and moved to a "combined" (*meshulav*) model that was still based on market forces, but combined them with a more progressive tax and wider safety net to members. However, many kibbutzim that initially introduced a partial reform eventually implemented a full differential pay reform

In this section we take advantage of the variation over time in the degree of pay reform, which is present because some kibbutzim changed immediately from an equal sharing system to a full differential pay system, while others introduced a partial differential pay system initially but later changed it to a complete differential pay structure.[19] We can exploit these changes to define treatment intensity because some of these kibbutzim made the change within the period of treatment. Specifically, of the 37 kibbutzim in this group 17 introduced a full pay reform and 20 a partial reform, and of the latter group only 6 changed to a full reform within the treatment period. Of the 14 kibbutzim that reformed in 1999, 7 introduced a full pay reform and 7 a partial reform; of the latter group 6 kibbutzim changed to full reform by 2002. Of the 22 kibbutzim that reformed in 2000, 13 introduced a full pay reform and 9 a partial reform; of the latter group 4 kibbutzim changed to full reform by 2002.

We therefore measure intensity of the pay reform by counting the number of years each student's kibbutz operated under a system of full differential pay while he was of high school age. We therefore define the following four treatment groups: the first includes students whose kibbutzim introduced a partial differential pay system initially and changed it only after they graduated from high school (3 years of partial reform); The second includes students whose first two years of high school were under partial differential pay system and whose last year was under full differential pay system (1 year of full reform); The third group includes students who were exposed to two years of a full differential pay system during high school (2 years of full reform), and the fourth group includes students who spent their entire high school under full differential pay system (3 years of full reform). The first group account for thirty percent of the treated sample, the second 11 percent, the third 20 percent , and the fourth 39 percent. We then estimated the following controlled difference in differences model while including in the estimated model four treatment groups according to the value of the intensity measure:

$$Y_{ijkc} = \gamma_k + \alpha_c + \beta_j (AffectedCohort_c) + \delta X_{ikc} + \varepsilon_{ijkc} \qquad (7)$$

[19] We note that no kibbutz moved from a full to partial pay system.

where j is treatment intensity as defined above. β_j are the coefficients of interest on the dummy variables indicating whether the student was treated with each of the four treatment intensities defined above.

The group with zero intensity of full pay had the lowest estimated effects, while the highest estimated effects are for the group with highest intensity of treatment. These results are presented in Table 9. The first panel presents the estimates with four intensity levels used as treatment measures. In panel B we use only two treatment groups, students exposed throughout high school (three years) to a partial pay reform versus students exposed to a full differential pay reform throughout their high school. Therefore panel B is based on a sample that excludes the two other treatment groups. The estimated effects of the lowest level of reform intensity on all four outcomes are very small and not significantly different from zero. On the other hand, the effect of being under a full differential pay system for 2 or 3 years has large and significant effect on all four outcomes. For example, the effect of three years in high school under a full differential pay system causes an 8.1 percentage point increase in the matriculation rate and 9.4 percentage point increase in the university qualified matriculation rate.

The results presented in panel B are very similar to the results in panel A and they reveal sharply the differences in estimated treatment effect of the full differential pay versus the zero estimated effect of the three years of partial differential pay system. Note that the size of the effect of the former is much larger than the average effect we presented in Table 7 for all outcomes besides high-school completion. For example, consider the treatment effect of the full differential pay system on the university qualified matriculation rate. This treatment effect is a 10.3 percentage points increase (up from average effect of 6 percentage points) in the university qualified matriculation rate, which amounts to a 20 percent increase relative to the counterfactual. The mean exam score is up by 4.43, or 0.21 standard deviations of the test score distribution.

Overall, the evidence reported in Table 9 suggests the magnitude of the treatment effect increases with years of exposure to a system of full differential pay. Especially important is the much larger estimated effect of treatment based on three years of exposure relative to the effect of only one year of exposure, because it is based on a comparison of the same type of treatment but with different "intensities". It is very unlikely that selection could explain this difference, and so this evidence strengthens the causal interpretation of our findings.

We next allow for heterogeneity of the effect by parental education and intensity of reform. Consistent with the evidence presented in this section and the previous one, the evidence presented in Table 10 suggests that the treatment effect is the largest for students who were exposed to a full differential pay system throughout their high schools and whose parents have lower levels of education. The treatment effect of the full differential pay system for students from lower social backgrounds is a 4.4 percentage point increase in high school completion rates, an 8.255 point increase in mean exam score, a 19.6 percentage point increase in the matriculation rate, and a 16.8 percentage point increase in the university qualified matriculation rate.

Similarly, Table 11 suggests that the treatment effect is not only larger for students who were exposed to a full differential pay system throughout their high schools, but it is the largest for *boys* who were fully exposed. The treatment effect of the full differential pay system for boys is a 4.2 percentage point increase (0.8 percentage points for girls) in high school completion rates, a 6.017 point increase (2.832 for girls) in mean exam score, a 10 percentage point increase (3.5 for girls) in the matriculation rate, and a 9.6 percentage point increase (4.8 for girls) in the university qualified matriculation rate.

VI. Putting the magnitude of the effects into perspective

To put the magnitude of the effect we find on the matriculation rate (one of our outcomes) into perspective, it is useful to compare it with the effects of various forms of high school intervention that had the direct objective of raising the matriculation rate and were implemented in Israel around the same academic year, 2000-2001.[20] The first is a remedial education program that provided individualized instruction to high school students in preparation for the matriculation exams (Lavy and Schlosser, 2005). The second is a student matriculation awards program that provided monetary bonuses to students who earned matriculation certificates (Angrist and Lavy, 2009), the third is a

[20] We note that those interventions were local and did not affect students in our sample.

teacher-bonus program that paid math, English and Hebrew teachers bonuses on the basis of their students' performance on matriculation exams (Lavy, 2009), and the fourth is a school choice program that allowed students in Tel Aviv to freely choose their secondary school in 7th grade (Lavy, 2010).

The effect of pay reform we study here was larger than the effect of these other programs, except for the remedial program. Specifically, the remedial program produced a gain similar to that of the pay reform we study here: an increase of 12 percentage points in the matriculation rate of participants in comparison to a gain of 10 percentage points caused by a full differential pay reform. Note however, that the remedial program was targeted to students with low probability of passing some of the *Bagrut* exams while the pay reform affected students with a much higher Bagrut passing rate. The students' bonus program increased the matriculation rate by 6 percentage points, though the gain was mainly among girls. The teacher bonus program increased the matriculation rate by 3.3 percent, while the school choice program led to a 6.2 percentage point increase in the matriculation rate.

VII. Conclusions

In this paper we use a natural experiment to test whether and to what extent investment in education is responsive to changes in the returns to education. This is, to the best of our knowledge, the first study that uses non-experimental data with an actual change in the rate of return to schooling to study the impact of an increase in the benefit from schooling on human capital investment. It allows an interesting comparison of our evidence with results from recent experiments that elicit potential schooling decisions of young adults as a reaction to possible rates of return to schooling (most notably, Jensen, 2010), and from similar studies based on correlating subjective expected rates of return to schooling with school enrollment decisions. For example, Attansio and Kaufmann (2010) find that youths' expectations about rates of return to schooling matter for high school and college attendance decisions.

The natural experiment that we exploit in this study is the reforms instituted by many Israeli kibbutzim in the late 1990s and early 2000s. These reforms took many kibbutzim from paying all their members the same wage regardless of their human capital and the jobs they performed, to paying wages based on the market rate of return to schooling. These reforms caused a sharp and salient increase in the returns to education for kibbutz members. We rely on this sharp change to test whether and to what extent an increase in returns induces high school students to invest more in their education, as reflected in their academic achievements.

We use a difference-in-differences approach and take advantage of the fact that different kibbutzim reformed at different times for our identification. Specifically, we compare educational outcomes of high school students in kibbutzim that reformed early (the treatment group) and late (the control group), before and after the early reforms (but before the late reforms). The treatment group is shown to be nearly identical to the control group in observable characteristics and pre-reform mean outcomes. We find students are indeed responsive to changes in returns to education: when their kibbutzim reformed, they considerably improved their educational outcomes such as whether they graduated and their average matriculation exam scores. Males reacted more strongly than females, and students with less educated parents reacted much more strongly than those with more educated parents. Students who spent their entire three years of high school in a kibbutz that reformed to a greater extent improved their educational outcomes more.

Our findings have important implications beyond the Israeli context. First, they shed light on the educational responses that could result from a decrease in the income tax rate, thus are informative on the long-run labor supply responses to tax changes. Second, they shed light on the educational responses expected when the return to education increases. For example, such changes might be occurring in many countries as technology-oriented growth increases the return to skills.[21] The transition from a centrally planned to a market economy in the former Soviet republics is another important historical episode that resulted in an increase in the rate of return to schooling. Brainerd (1998) shows that the transition to a market economy has produced a substantial and rapid change in the wage structure in Russia, the overall wage inequality nearly doubled from 1991 to 1994 and the returns to education have increased considerably. Svejnar (1999) for transitional central and east European economies. While our results are silent on the general equilibrium

[21] See, for example, the discussion in Autor, Katz and Krueger (1999), Card and Dinardo (2002), and Golding and Katz (2008).

effects that could result from such huge economy-wide changes, they shed light on the likely impact these changes had on human capital investment in the former Soviet nations. Third, our findings may suggest the likely human capital consequences in developing countries that liberalized their labor markets, for example Vietnam in the mid 1980's[22], and as a result experienced increases in the rate of return to schooling. Finally, our findings may improve our understanding of the large human capital gap between first and second generation immigrants in developed countries.[23] Our findings suggest that part of the higher education of immigrants' children is due to the higher rates of return to schooling they experience in their host countries relative to the returns in their home country.

[22] See Moock, Patrinos and Venkataraman (1998).
[23] See Dustmann and Theodoropoulos (2010) and Aydemir and Sweetman (2006).

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Table 1: Wage by education of all working members in one particular kibbutz pre and post reform

	Number of obs		Pre reform Mean/Median Wage	Post reform Mean Wage		Post reform Median Wage	
	all	no outliers	all	all	no outliers	all	no outliers
High school or less	44	37	8,661	7,980	9,331	6,929	8,000
College or other post-secondary	36	31	8,661	8,592	9,853	7,695	9,000
MA	20	19	8,661	10,060	10,536	9,750	10,500
Phd	2	2	8,661	10,881	10,881	10,881	10,881

Notes: Wages are measured in New Israeli 2010 Shekels per month. 1 US dollar is currently equal to approximately 3.6 shekels. Outliers are members with wages below 2000 shekels. We exclude them because we suspect they only work part time.

Table 2: Post reform wage by education of all working members in two kibbutzim

	(1)	(2)	(3)	(4)
Years of Schooling	.080	.083		
	(.021)	(.021)		
B.A or Other Post-Secondary			.318	.306
			(.088)	(.090)
Master			.443	.456
			(.135)	(.135)
Doctor			.584	.639
			(.283)	(.285)
Age and Age Squared	No	Yes	No	Yes
Kibbutz Fixed Effects	Yes	Yes	Yes	Yes

Notes: This tables presents results from OLS regressions where the dependent variable is the natural log of wages, run for members of two reformed kibbutzim. Wages are measured in New Israeli 2010 Shekels per month. 1 US dollar is currently equal to approximately 3.6 shekels. Outliers are members with wages below 2000 shekels. We exclude them because we suspect they only work part time.Years of schooling are calculated as 8 for elementary, 12 for high school, 14 for other post-secondary, 15 for B.A, 16 for an engineer, 17 for master and 20 for doctorate.

Table 3: Distribution of Kibbutzim, Schools and Students by Year of Reform and by 10th Grade Cohorts

	Year of Reform	
	1998-2000 (Treatment)	2003-2004 (Control)
	(1)	(2)
A. 10th Grade Students in 1995-1996		
Kibbutzim	74	33
Students	1,100	601
B. 10th Grade Students in 1999-2000		
Kibbutzim	74	33
Students	1,043	605

Notes : This table presents the number of kibbutzim and students in the treatment and control kibutzim and treated (10th grade in 1999-2000) and untreated (10th grade in 1995-96) cohorts.

Table 4: Balancing Tests of Students' Characteristics and Outcomes in Treatment and Control Kibbutzim

	10th Grade Students in 1995 and 1996			10th Grade Students in 1999 and 2000		
	Treatment	Control	Difference	Treatment	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
A. Student's Characteristics						
Male	0.495	0.507	-0.013	0.523	0.536	-0.012
	(0.500)	(0.500)	(0.027)	(0.500)	(0.499)	(0.023)
Father's Years of Schooling	13.26	13.59	-0.328	13.60	14.12	-0.523
	(2.776)	(2.841)	(0.264)	(2.525)	(2.973)	(0.419)
Mother's Years of Schooling	13.42	13.71	-0.292	13.94	14.08	-0.140
	2.47	2.44	(0.174)	2.23	2.25	(0.229)
Number of Siblings	2.56	2.65	-0.094	2.53	2.77	-0.239
	(1.357)	(1.358)	(0.199)	(1.249)	(1.581)	(0.280)
Ethnic Origin: Africa/Asia	0.105	0.103	0.001	0.091	0.079	0.012
	(0.306)	(0.304)	(0.016)	(0.288)	(0.270)	(0.021)
Ethnic Origin: Europe/America	0.346	0.379	-0.033	0.360	0.306	0.054
	(0.476)	(0.486)	(0.035)	(0.480)	(0.461)	(0.033)
Immigrants from non-FSU countries	0.016	0.015	0.001	0.013	0.013	0.000
	(0.127)	(0.122)	(0.006)	(0.115)	(0.114)	(0.006)
Immigrants from FSU countries	0.013	0.017	-0.004	0.031	0.023	0.008
	(0.112)	(0.128)	(0.007)	(0.173)	(0.150)	(0.009)
B. High School Outcomes						
High School Completion	0.951	0.967	-0.016	-	-	-
	(0.216)	(0.180)	(0.011)			
Mean Matriculation Score	70.62	72.48	-1.862	-	-	-
	(23.250)	(21.039)	(1.309)			
Matriculation Certification	0.549	0.569	-0.020	-	-	-
	(0.498)	(0.496)	(0.036)			
University Qualified Matriculation	0.516	0.536	-0.019	-	-	-
	(0.500)	(0.499)	(0.035)			
Observations	1,100	601	-	1,043	605	-

Notes: Columns 1, 2, 4 and 5 present means and standard deviations (in parentheses) of characteristics and outcomes of students in treatment and control kibbutzim for affected (1999-2000) and unaffected (1995-1996) cohorts of 10th graders. Columns 3 and 6 present the differences between treatment and control kibbutzim from regression equation 3. Standard errors of these differences clustered at the kibbutz level are given in parentheses. Treatment kibbutzim are those that reformed in 1998-2000. Control kibbutzim are those that reformed in 2003-2004.

Table 5: Treatment-Control and Between-Cohort Differences in Students' Exit Rates From Their Kibbutzim

	Treatment	Control	Difference
	(1)	(2)	(3)
10th Grade Students in 1995-1996	0.056	0.042	0.015
	(0.231)	(0.200)	(0.016)
10th Grade Students in 1999-2000	0.052	0.038	0.014
	(0.222)	(0.191)	(0.011)
Difference	-0.005	-0.004	-
	(0.010)	(0.016)	

Notes: This table presents exit rates from their kibbutzim of three cohorts of students in treatment and control kibbutzim. Columns 1 and 2 show means and standard deviations (in parentheses) of exit rates for the different groups of students. Column 3 shows differences between the groups and standard errors of the differences clustered at the kibbutz level (in parentheses), estimated from equation (4) in the text. Exit is defined as living in the kibbutz at the start of 10th grade, and living outside the kibbutz by the end of 12th grade. Treatment kibbutzim are those that reformed in 1998-2000. Control kibbutzim are those that reformed in 2003-2004.

Table 6: Treatment-Control Differences in Pre-Reform Time Trends in Schooling Outcomes, 10th Grade Students in 1993-1998

	Matriculation Certification		Mean Matriculation Score	
	(1)	(2)	(3)	(4)
A. Linear Trend Model				
Time Trend	0.025	0.026	1.225	1.287
	(0.011)	(0.010)	(0.478)	(0.451)
Treatment X Time Trend	-0.008	-0.006	-0.267	-0.361
	(0.013)	(0.012)	(0.580)	(0.547)
Treatment	0.005	-	0.681	-
	(0.050)		(2.270)	
B. Cohort Dummies Model				
Treatment X 1994	-0.022	-0.005	2.178	2.329
	(0.076)	(0.070)	(3.481)	(3.295)
Treatment X 1995	-0.011	0.003	-1.716	-1.782
	(0.075)	(0.070)	(3.446)	(3.255)
Treatment X 1996	-0.030	-0.008	0.403	0.024
	(0.075)	(0.070)	(3.446)	(3.255)
Treatment X 1997	0.036	0.051	1.765	0.816
	(0.075)	(0.070)	(3.449)	(3.259)
Treatment X 1998	-0.087	-0.074	-2.019	-1.962
	(0.075)	(0.069)	(3.416)	(3.221)
Treatment	-0.002	-	-0.358	-
			(2.424)	
	$F(5, 488) = 0.66$		$F(5, 488) = 0.48$	
	$\text{Prob} > F = 0.6516$		$\text{Prob} > F = 0.7897$	

Notes: This table presents the results from OLS regressions run at the kibbutz level predicting the proportion of students who received matriculation certificates (columns 1 and 2) or the mean scores in the matriculation exams (columns 3 and 4) for the cohorts of 10th graders from 1993 to 1998 (pre reform). In the regressions in Panel A, outcomes are allowed to vary according to a linear time (cohort) trend that differs in treatment and control kibbutzim, as described in regression equation 5. In the regressions in Panel B, the difference between treatment and control kibbutzim is allowed to vary freely for each cohort of students. Cohort dummies are included in the Panel B regressions but their coefficients are not reported. Estimates in columns 2 and 4 include kibbutz fixed effects. Standard errors are presented in parentheses. The number of observarions in each regression is 605. The F statistics at the bottom of the table test whether all the interaction terms in Panel B between treatment kibbutzim and the cohorts are jointly zero.

Table 7: Cross-Section Treatment-Control Differences and Difference-in-Differences Estimates

	High School Completion	Mean Matriculation Score	Matriculation Certification	University Qualified Matriculation
	(1)	(2)	(3)	(4)
A. Experiment of Interest, 10th Grade Students in 1995-1996 and 1999-2000				
Cross-Section Regressions				
Treatment-Control Difference, 1995-1996	-0.015	-1.554	-0.010	-0.008
	(0.010)	(1.091)	(0.025)	(0.025)
Treatment-Control Difference, 1999-2000	0.020	2.200	0.032	0.041
	(0.011)	(1.187)	(0.024)	(0.025)
Difference in Differences Regressions				
Simple Difference in Differences	0.033	3.112	0.029	0.040
	(0.016)	(1.517)	(0.035)	(0.035)
Controlled Difference in Differences	0.033	3.546	0.049	0.060
	(0.015)	(1.604)	(0.035)	(0.035)
B. Control Experiment, 10th Grade Students in 1995-1996 and 1997-1998				
Difference in Differences Regressions				
Simple Difference in Differences	0.011	0.213	-0.016	-0.025
	(0.015)	(1.527)	(0.036)	(0.036)
Controlled Difference in Differences	0.011	0.304	-0.013	-0.027
	(0.015)	(1.544)	(0.035)	(0.035)

Notes: The first half of Panel A presents the coefficients of interest in single difference regressions comparing outcomes of students of the same cohort between treatment kibbutzim (reformed in 1998-2000) and control kibbutzim (reformed in 2003-04). The dependent variable in column 1 is whether the student completed high school; in column 2 it is her mean score in the matriculation exams; in column 3 it is whether she received a matriculation certificate; in column 4 it is whether she received a matriculation certificate that satisfies the requirements for university study.

The second half of Panel A presents the coefficients of interest in difference-in-differences regressions comparing students in treatment and control kibbutzim who are treated (10th grade in 1999-2000) and untreated (10th grade in 1995-96). The single difference and simple difference-in-differences regressions include cohort dummies and standard errors (in parentheses) are clustered at the kibbutz level. The controlled difference-in-differences estimation (equation 2 in the text) include cohort dummies, kibbutz fixed effects, and the demographic controls gender, father's and mother's education, number of siblings, a set of ethnic dummies (origin from Africa/Asia, Europe/America, immigrants from FSU, Ethiopia and other countries). Panel B presents difference-in-differences regressions parallel to those in Panel A, but that compare two untreated cohorts. For the controlled difference-in-differences regressions, robust standard errors are presented in parentheses.

Table 8: Controlled Differences in Differences Estimates in Sub-Samples by Gender and Parental Education

	High School Completion	Mean Matriculation Score	Matriculation Certification	University Qualified Matriculation
	(1)	(2)	(3)	(4)
Experiment of Interest, 10th Grade Students in 1995-1996 and 1999-2000				
A. Sample Stratification By Mother's Education				
Low	0.049	6.175	0.116	0.100
	(0.024)	(2.553)	(0.053)	(0.053)
High	0.014	0.329	-0.031	0.002
	(0.019)	(2.114)	(0.047)	(0.048)
B. Sample Stratification By Father's Education				
Low	0.033	5.879	0.093	0.086
	(0.027)	(2.781)	(0.055)	(0.055)
High	0.031	1.701	0.010	0.034
	(0.017)	(1.924)	(0.046)	(0.047)
C. Sample Stratification By Gender				
Male	0.052	4.820	0.060	0.056
	(0.023)	(2.505)	(0.051)	(0.051)
Female	0.011	2.549	0.027	0.034
	(0.019)	(2.037)	(0.049)	(0.049)

Notes : This table presents the coefficients of interest in difference-in-differences regressions (equation 2 in the text) comparing students in treatment (reformed 1998-2000) and control (reformed 2003-04) kibbutzim who are treated (10th grade in 1999-2000) and untreated (10th grade in 1995-96), stratified by background characteristics. The dependent variable in column 1 is whether the student completed high school; in column 2 it is her mean score in the matriculation exams; in column 3 it is whether she received a matriculation certificate; in column 4 it is whether she received a matriculation certificate that satisfies the requirements for university study. All regressions include cohort dummies, kibbutz fixed effects, and the demographic controls gender, father's and mother's education, number of siblings, a set of ethnic dummies (origin from Africa/Asia, Europe/America, immigrants from FSU, Ethiopia and other countries). Robust standard errors are presented in parentheses.

Table 9: Controlled Difference in Differences Estimates by Level of Intensity of Exposure to Full Differential Pay

	High School Completion	Mean Matriculation Score	Matriculation Certification	University Qualified Matriculation
	(1)	(2)	(3)	(4)
A. Intensity of Exposure				
Three Years of Full Reform	0.029	4.288	0.082	0.100
(N=405)	(0.019)	(2.055)	(0.043)	(0.043)
Two Years of Full Reform	0.054	5.621	0.031	0.083
(N=211)	(0.018)	(1.925)	(0.047)	(0.047)
One Year of Full Reform	0.053	3.744	0.009	-0.020
(N=114)	(0.024)	(2.485)	(0.058)	(0.059)
Three Years of Partial Reform	0.016	1.239	0.036	0.025
(N=313)	(0.020)	(2.202)	(0.045)	(0.045)
B. Intensity of Exposure: partial versus full				
Three Years of Full Reform	0.030	4.431	0.084	0.103
(N=405)	(0.019)	(2.064)	(0.045)	(0.043)
Three Years of Partial Reform	0.015	1.285	0.035	0.026
(N=313)	(0.021)	(2.221)	(0.045)	(0.045)

Notes : This table presents the results of difference-in-differences regressions comparing students in treatment (reformed 1998-2000) and control (reformed 2003-04) kibbutzim who are treated (10th grade in 1999-2000) and untreated (10th grade in 1995-96), where the treatment effect varies by the number of years the student spent in high school under a full relative to partial differential pay system (versions of equation (7) in the text). The value of N for each intensity of treatment is the number of students who faced that intensity of treatment. The Panel A regressions interact dummies for the number of years each treated student spent in high school under a full differential pay system with the treatment cohort dummy.

Panel B regressions duplicate panel A regressions, but omit students who spent some high school years under a partial differential pay system and some under a full. In each case, estimation includes cohort dummies, kibbutz fixed effects, and the demographic controls gender, father's and mother's education, number of siblings, a set of origin dummies (Africa/Asia, Europe/America, immigrants from FSU, Ethiopia and other countries). Robust standard errors are presented in parentheses.

Table 10: Controlled Difference in Differences Estimates by Level of Intensity of Exposure to Full Differential Pay, Sub-Samples by Parental Education

	High School Completion	Mean Matriculation Score	Matriculation Certification	University Qualified Matriculation
	(1)	(2)	(3)	(4)
A. Sample Stratification by Mother's Education				
Low				
Three Years of Full Reform	0.044	8.255	0.196	0.168
	(0.033)	(3.421)	(0.067)	(0.068)
Three Years of Partial Reform	0.026	2.792	0.109	0.085
	(0.033)	(3.612)	(0.069)	(0.069)
High				
Three Years of Full Reform	0.008	-0.011	-0.034	0.023
	(0.024)	(2.624)	(0.058)	(0.059)
Three Years of Partial Reform	0.006	-0.246	-0.047	-0.036
	(0.027)	(2.899)	(0.063)	(0.064)
B. Sample Stratification by Father's Education				
Low				
Three Years of Full Reform	0.027	9.547	0.205	0.190
	(0.035)	(3.591)	(0.069)	(0.069)
Three Years of Partial Reform	0.025	0.996	-0.015	-0.035
	(0.036)	(3.990)	(0.072)	(0.071)
High				
Three Years of Full Reform	0.026	-0.207	-0.006	0.035
	(0.024)	(2.508)	(0.057)	(0.058)
Three Years of Partial Reform	0.016	2.964	0.091	0.096
	(0.024)	(2.576)	(0.061)	(0.062)

Notes: This table presents the results of difference-in-differences regressions comparing students in treatment (reformed 1998-2000) and control (reformed 2003-04) kibbutzim who are treated (10th grade in 1999-2000) and untreated (10th grade in 1995-96), where the treatment effect varies by the number of years the student spent in high school under a full relative to partial differential pay system (versions of equation (7) in the text), stratified by mother's (Panel A) or father's (Panel B) education. The regressions omit students who spent some high school years under a partial differential pay system and some under a full. They interact dummies for the number of years each treated student spent in high school under a full differential pay system with the treatment cohort dummy.

Estimation includes cohort dummies, kibbutz fixed effects, and the demographic controls gender, father's and mother's education, number of siblings, a set of origin dummies (Africa/Asia, Europe/America, immigrants from FSU, Ethiopia and other countries). Robust standard errors are presented in parentheses.

Table 11: Controlled Difference in Differences Estimates by Level of Intensity of Exposure to Full Differential Pay, Sub-Samples by Gender

	High School Completion	Mean Matriculation Score	Matriculation Certification	University Qualified Matriculation
	(1)	(2)	(3)	(4)
Male				
Three Years of Full Reform	0.042	6.017	0.100	0.096
	(0.030)	(3.211)	(0.063)	(0.063)
Three Years of Partial Reform	0.018	1.085	0.028	0.007
	(0.034)	(3.460)	(0.067)	(0.067)
Female				
Three Years of Full Reform	0.008	2.832	0.035	0.048
	(0.026)	(2.710)	(0.062)	(0.063)
Three Years of Partial Reform	0.017	2.201	0.045	0.037
	(0.021)	(2.702)	(0.064)	(0.064)

Notes : This table presents the results of difference-in-differences regressions comparing students in treatment (reformed 1998-2000) and control (reformed 2003-04) kibbutzim who are treated (10th grade in 1999-2000) and untreated (10th grade in 1995-96), where the treatment effect varies by the number of years the student spent in high school under a full relative to partial differential pay system (versions of equation (7) in the text), stratified by gender. The regressions omit students who spent some high school years under a partial differential pay system and some under a full. They interact dummies for the number of years each treated student spent in high school under a full differential pay system with the treatment cohort dummy.
Estimation includes cohort dummies, kibbutz fixed effects, and the demographic controls gender, father's and mother's education, number of siblings, a set of origin dummies (Africa/Asia, Europe/America, immigrants from FSU, Ethiopia and other countries). Robust standard errors are presented in parentheses.