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HOW RESPONSIVE IS INVESTMENT IN SCHOOLING TO CHANGES IN REDISTRIBUTIVE POLICIES AND IN RETURNS?

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This paper uses an unusual pay reform to test the responsiveness of investment in schooling to changes in redistribution schemes that increase the rate of return to education. We exploit an episode where different Israeli kibbutzim shifted from equal sharing to productivity-based wages in different years and find that students in kibbutzim that reformed earlier invested more in high school education and, in the long run, also in post-secondary schooling. We further show that the effect is mainly driven by students in kibbutzim that reformed to a larger degree. Our findings support the prediction that education is highly responsive to changes in the redistribution policy.

KEYWORDS: Schooling, redistributive policies, pay reform, return to education.

1. INTRODUCTION

WE STUDY A UNIQUE EPISODE where some kibbutzim (plural of kibbutz) changed their decades-long policy of setting wages independent of an individual's human capital to setting wages to reflect the market rate of return. This sharp change in the redistributive policy from equal sharing to pay-for-productivity introduced a dramatic increase in the returns to schooling for kibbutz members. We test whether and to what extent this policy change induced high school students to invest more in their education, as reflected by their academic achievements during high school and in adulthood.

We use administrative records collected by the Israeli Ministry of Education for six consecutive cohorts (from 1995 to 2000) of 10th grade students, following them to graduation, combined with National Social Security Administra-

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tion data on completed years of higher education when individuals in our sample were 28 to 33 years old. One important outcome we examine is whether the student passed all the matriculation exams successfully and got a matriculation diploma (equivalent to a baccalaureate diploma in most European countries), which is necessary for post-high school education in Israel and yields a substantial earning premium in the general Israeli labor market. Other outcomes of interest are whether the student graduated high school, her average score in the matriculation exams, and whether her diploma meets the university entrance requirements. We then study whether, in the long run, these students enrolled in post-high school education of various types and how many years of schooling they completed.

Our identification strategy relies on the fact that the pay reform was not implemented in all kibbutzim in the same year. We use a difference-in-differences approach, comparing educational outcomes of high school students in kibbutzim that reformed early (1998–2000) and late (2003–2004), before and after the early reforms. We show evidence that students in early-reforming (the treatment group) and late-reforming kibbutzim (control group) are very similar in their observable background characteristics and in their pre-reform schooling outcomes.

Overall, we find that students in kibbutzim that reformed early experienced an improvement in high school outcomes such as graduation rate and mean score in matriculation exams (*Bagrut*). We further show that the effect is mainly driven by students in kibbutzim that reformed to a larger degree, and appears to be largely driven by males and by the subgroups of students who have less educated parents, although these differences are not statistically significant. In the long term, we show that students in kibbutzim that reformed early tended to shift away from universities, toward academic colleges and teachers' colleges.

This paper contributes to two strands of the literature. From a public economics perspective, this paper sheds light on the extent to which redistributive policy influences long run labor supply, as mediated through educational choices. While it is well known that changes in taxes affect labor supply decisions in the short run (Saez, Slemrod, and Giertz (2009), Chetty, Friedman, Olsen, and Pistaferri (2014)), much 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 affect educational choices. This paper fills this gap by studying how responsive educational choices are to tax changes.

From a labor economics perspective, economic models of optimal human capital investment (Becker (1967), Ben-Porath (1967), Weiss (1995)) suggest that the level of investment in schooling is expected to increase in the perceived rate of return to education.[2] However, despite its centrality in modern labor

[2]Note simple models of investment in education, such as presented in Eaton and Rosen (1980), show that, when the only cost of education is the opportunity cost of foregone earnings, a pro-

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.[3]

While the pay reform in kibbutzim sharply increased the returns to schooling, it could influence schooling outcomes through two other channels. The first channel is the reduction in social incentives for encouraging education that had been used under equal sharing (pre reform). Under equal sharing, the kibbutz provided members with various services and communal organization, and members might have felt indebted to their kibbutz and invested in education for the common cause. Such social norms would be reduced following the pay reform. The second channel is the changes in income levels of parents, which might affect education decisions through liquidity constraints, because children's education is a normal good, or through the concavity of utility in income assuming some intergenerational transfers. Our paper cannot fully disentangle these mechanisms, but we provide suggestive evidence that the returns to the education channel operated above and beyond the social incentives channels, and that the income channel played only a limited role.

The rest of the paper is structured as follows. The next section presents a brief background of kibbutzim and the pay reform, and of the Israeli high school system. Section 3 describes the data and discusses the empirical framework and identification strategy. Section 4 presents the results on the effect of the reform on high school outcomes, Section 5 presents the results on the long term effect on post-high school education, and Section 6 concludes.

2. BRIEF BACKGROUND

The Pay Reform in Kibbutzim

Kibbutzim are voluntary communities that have provided their members with a high degree of income equality for almost a century.[4] 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

portional change in the income tax rate does not affect private incentives to invest in education. However, because education inevitably involves effort costs and likely other costs besides, theory predicts that the change in income tax rates that we study will affect investment in education.

[3]Freeman (1976) and Kane (1994) found a positive response of schooling investments to increased returns. However, the limitation of these studies 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 difficult to establish. Several studies estimated the perceived rate of return to schooling, and then assessed its effect on schooling (Betts (1996)). Jensen (2010) found that students who were better informed (experimentally) of higher returns were significantly less likely to drop out of school in subsequent years. Attanasio and Kaufman (2009) found that college attendance decisions depend on expected returns to college.

[4]For a history of kibbutzim, see Near (1992, 1997), Abramitzky (2011).

brought their salaries in, and these were split equally among members. This meant that monetary returns to ability and effort were close to zero.

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. Perhaps the biggest problem was the 1985 stabilization program in Israel following a few years of high inflation, which raised interest rates dramatically and left many kibbutzim with huge debts they could not repay. As a result, living standards in many kibbutzim fell substantially, members left in large numbers during the late 1980s and early 1990s, and talk about a major reform of kibbutz life began.

In reformed kibbutzim, members' wages reflected market wages. Members who worked outside their kibbutzim (about a quarter of all members) largely kept the wages they received from their employers. Members who worked inside received wages based on the wages of non-kibbutz workers of similar occupation, education, skills, and experience. A kibbutz 'tax' was deducted from these gross wages to guarantee older members and very low wage earners in the kibbutz a safety net (i.e., a minimum wage).[5] The pay reform was essentially a sharp decrease in the income tax rate. Before the reform, the income tax rate in kibbutzim was 100%. Post reform, the tax rates 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%.[6]

The pay reform was also highly salient. The move from equal sharing to differential pay strongly signaled an increase in the financial rewards for human capital to young adults. This increase in the return to skills was noticeable within a family, as students' parents experienced a decrease or increase in their

[5]Traditionally, kibbutzim paid income taxes to the government based on members' average income.

[6]Data we collected on two particular kibbutzim that are currently reforming their pay systems, presented in Table S.I in the Supplemental Material (Abramitzky and Lavy (2014)), illustrate that, before the reform, members of all education levels earned the same wage, but post reform, more educated members earned higher wages in these kibbutzim. Pooling observations from these two fully reformed kibbutzim, Table S.II in the Supplemental Material documents the large returns to schooling after the pay reform, around 8% per year of schooling, which is the same as the returns for the country as a whole (see Klinov and Palgi (2006), Frisch (2007), Frisch and Moalem (1999)). Members' exit option and non-monetary returns to education (prestige, social norms) likely cause us to overstate the increase in returns due to the pay reforms. These are discussed in the working paper version.

earnings depending on their skills. 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. The productivity-based sharing rules were hotly debated by members in the kibbutzim, and the reforms also received a lot of attention in the media both in Israel and abroad.

The Israeli High School System

When entering high school (10th grade), students choose whether to enroll in the academic or non-academic track. Students enrolled in the academic track obtain a matriculation certificate (*Bagrut*) if they pass a series of national exams in core and elective subjects taken between 10th and 12th grade. Students choose to be tested at various proficiency levels, with each test awarding one to five credit units per subject, depending on difficulty. Advanced level subjects are those subjects taken at a level of four or five credit units; a minimum of 20 credit units is required to qualify for a *Bagrut* certificate. About 52 percent of all high school seniors received a *Bagrut* in the 1999 and 2000 cohorts (Israel Ministry of Education (2001)). The *Bagrut* is a prerequisite for university admission and receiving it is an economically important educational milestone. For more details on the Israeli high school system, see Angrist and Lavy (2009) and Trumper (1997).

3. DATA AND ESTIMATION

The empirical analysis is based on a sample that includes students who live in kibbutzim at the start of 10th grade, and on information drawn from several administrative data files obtained from the Ministry of Education in Israel. We obtained data for six consecutive cohorts (from 1995 to 2000) of 10th grade students. Each record contains an individual, a school and a class identifier, student date of birth, gender, parental education, number of siblings, year of immigration, ethnicity, and schooling outcomes (graduating high school, receiving a *Bagrut*, receiving a *Bagrut* that meets university entrance requirements,[7] and the average score in the matriculation exams). We 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. Table S.III in the Supplemental Material presents the number of kibbutzim that reformed and the number of students by year of reform. We also use data on post-high school educational outcomes that we obtained from the National Insurance Institute of Israel. We describe these data in Section 5.

[7] A *Bagrut* that meets university entrance requirements is one that contains at least 4 credits in English and another subject at a level of 4 or 5 credits.

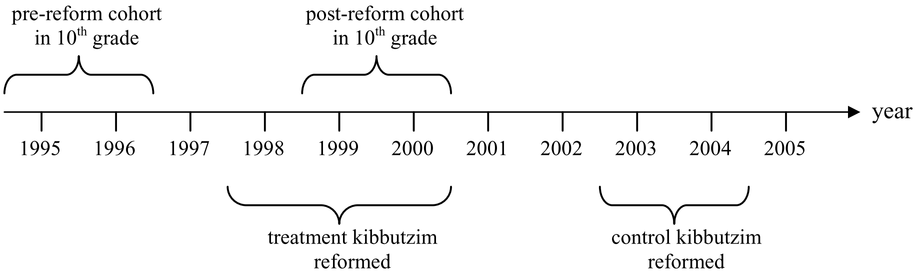

FIGURE 1.—Timeline of the pay reform and the selection of treatment and control groups.

We use a difference-in-differences (DID) approach comparing educational outcomes of high school students in kibbutzim that reformed early (1998–2000, treatment group) versus late (2003–2004, control group), before and after the early reforms (but before the late reforms).[8] These timings are illustrated in Figure 1.

In Section 4.3, we take a more continuous treatment approach by exploiting the time-varying "intensity" of the reform. The difference between pre- and post-reform cohorts in the treatment kibbutzim relative to control kibbutzim can be modeled as in the following simple DID regression:

$$\begin{aligned} Y_{ikc} = {} & \alpha_c + \beta_1(\mathit{TreatmentKibbutz}_k) \\ & + \beta_2(\mathit{PostCohort}_c \mathit{TreatmentKibbutz}_k) + \varepsilon_{ikc}, \end{aligned} \tag{1}$$

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), $(\mathit{TreatmentKibbutz}_k)$ denotes whether the student belonged to a kibbutz that implemented the reform early, and $(\mathit{PostCohort}_c \mathit{TreatmentKibbutz}_k)$ is the interaction of interest, namely, whether the student belonged to the affected (post-reform) cohort and lived in a kibbutz that reformed early. Standard errors are adjusted for clustering at the kibbutz level. We also estimate a specification that includes kibbutz fixed effects, cohort fixed effects, and a vector of the student's background characteristics, as follows:

$$Y_{ikc} = \gamma_k + \alpha_c + \beta_1(\mathit{PostCohort}_c \mathit{TreatmentKibbutz}_k) + \beta_2 X_{ikc} + \varepsilon_{ikc}, \tag{2}$$

where γ_k are kibbutz fixed effects, X_{ikc} are student i's characteristics, and the rest of the variables are as in equation (1). Note that when comparing pre- and post-reform students, the kibbutz fixed effects essentially also capture school

[8]We exclude kibbutzim that reformed in 2001–2002 to avoid anticipation effects (see discussion in Section 3).

fixed effects because almost all students from the same kibbutz attend the same high school. Note also that kibbutz children typically go to high schools located outside of kibbutzim, together with children from other kibbutzim and from villages. This means that the effects we document are due to the behavioral responses of students rather than changes in the quality of the educational system.

The identifying assumption of our strategy is that the exact timing of the reform is unrelated to potential outcomes of students. This assumption implies that older cohorts of early- and late-reforming (treatment and control) kibbutzim should have had similar high school outcomes, on average. We next provide evidence in support of the research strategy and this identification assumption.

Are the Control and Treatment Groups Observationally Equivalent?

Here we test directly whether the students in the treatment and control groups (comprising 74 and 33 kibbutzim, respectively; see Table S.III in the Supplemental Material) are statistically indistinguishable in terms of their observed characteristics for two pre-reform cohorts (10th graders in 1995 and 1996, 1,701 students in total), both separately and jointly, and for the post-reform cohorts (10th graders in 1999 and 2000, 1,648 students). For the pre-reform cohorts, we also check whether their academic high school matriculation outcomes are similar. Panel A of Table I shows that student background characteristics are very similar in the treatment and control groups, for both the pre and post cohorts. Out of the 32 estimated differences in background characteristics, the only ones that are significant (at the 10% level of significance) are the difference in mother's years of schooling in the pre-reform sample (t-statistic equal to 1.68) and the difference in proportion of students of European/American ethnic origin in the post-reform sample (t-statistic equal to 1.64). We note, however, that in the post cohort, the control variables are jointly marginally significant, as the F-statistic value is 1.8 and the p-value is 0.0853. These statistics for the pre and post samples are reported at the bottom of panel A in Table I.[9]

[9]In Tables S.IV–S.XII in the Supplemental Material, we present additional evidence on the well-balanced comparison between the treatment and control group. This includes balancing for each cohort separately and comparison based on including all background characteristics jointly in the regression and F-tests for the joint significance (Table S.IV). We also compare treatment and control separately for kibbutzim that implemented full reform (Tables S.V and S.VI) and those that implemented a partial reform (Tables S.VII and S.VIII). We also compare kibbutzim that reformed fully to kibbutzim that reformed partially (Tables S.IX and S.X). In Table S.XI, we present balancing tests of characteristics of students who faced different intensities of reform.

TABLE I

BALANCING TESTS OF STUDENTS' CHARACTERISTICS AND OUTCOMES IN TREATMENT AND CONTROL KIBBUTZIM[a]

	10th Grade Students in 1995 and 1996			10th Grade Students in 1999 and 2000		
	Treatment (1)	Control (2)	Difference (3)	Treatment (4)	Control (5)	Difference (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
	(2.471)	(2.439)	(0.174)	(2.226)	(2.248)	(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)
F-statistic			1.04			1.80
p-value			0.4136			0.0853
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)			

(*Continues*)

Similarly small and insignificant differences in pre-reform mean outcomes of the control and treatment groups in 1995/1996 are presented in panel B of Table I. We conclude that students in the treatment and control groups are similar in their mean background characteristics and pre-reform mean schooling outcomes.

TABLE I—*Continued*

	10th Grade Students in 1995 and 1996			10th Grade Students in 1999 and 2000		
	Treatment (1)	Control (2)	Difference (3)	Treatment (4)	Control (5)	Difference (6)
C. Entry and Exit						
Exit	0.056	0.042	0.015	0.052	0.038	0.014
	(0.231)	(0.200)	(0.016)	(0.222)	(0.191)	(0.011)
Entry	0.029	0.042	−0.013	0.012	0.012	−0.000
	(0.168)	(0.200)	(0.016)	(0.107)	(0.107)	(0.007)
Kibbutzim	74	33		74	33	
Students	1,100	601	–	1,043	605	–

[a]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. Standard errors of these differences are clustered at the kibbutz level and are presented in parentheses. Treatment kibbutzim are those that reformed in 1998–2000. Control kibbutzim are those that reformed in 2003–2004. All the estimated coefficients are based on a regression of the characteristic\outcome as a dependent variable, and the treatment indicator is the explanatory variable. The F-statistics reported at the bottom of panel A test whether the estimated coefficients of all characteristics are jointly zero in a regression where treatment is the dependent variable and all the student's characteristics are included jointly as regressors. Panel C, first row, reports mean exit rates in treatment and control kibbutzim and the differences between them. The exit indicator is equal to 1 if a student left the kibbutz between 10th and 12th grade. Panel C, second row, reports mean entry rates in treatment and control kibbutzim and the differences between them. The entry indicator is equal to 1 if a student lived in a different locality while in 9th grade.

Were the Control and Treatment Kibbutzim on Different Pre-Reform Time Trends?

We use pre-reform data from 1993 to 1998 to estimate differential time trends in outcomes for treatment and control kibbutzim. The unit of observation in this analysis is a kibbutz-year. First, we estimate a constant linear time trend model while allowing for an interaction of the constant linear trend with the treatment indicator. We also include specifications with the main effect for the treatment group 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.

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 II for the mean matriculation rate and *Bagrut* mean test score (two representative outcomes; the evidence for the other outcomes is identical). The mean trend is an annual increase of 0.025 in the matriculation rate and a 1.225 point 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 not statistically different from zero in

TABLE II

TREATMENT-CONTROL DIFFERENCES IN PRE-REFORM TIME TRENDS IN SCHOOLING OUTCOMES, 10TH GRADE STUDENTS IN 1993–1998[a]

	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 × 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 × 1994	−0.022	−0.005	2.178	2.329
	(0.076)	(0.070)	(3.481)	(3.295)
Treatment × 1995	−0.011	0.003	−1.716	−1.782
	(0.075)	(0.070)	(3.446)	(3.255)
Treatment × 1996	−0.030	−0.008	0.403	0.024
	(0.075)	(0.070)	(3.446)	(3.255)
Treatment × 1997	0.036	0.051	1.765	0.816
	(0.075)	(0.070)	(3.449)	(3.259)
Treatment × 1998	−0.087	−0.074	−2.019	−1.962
	(0.075)	(0.069)	(3.416)	(3.221)
Treatment	−0.002		−0.358	
	(0.053)		(2.424)	
Kibbutz fixed effects	No	Yes	No	Yes
F-statistic	0.58	0.66	0.50	0.48
p-value	0.7125	0.6516	0.7773	0.7897

[a]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' results reported in panel A, outcomes are allowed to vary according to a linear time (cohort) trend that differs in treatment and control kibbutzim. 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. The number of observations in each regression is 766. The F-statistics at the bottom of the table test whether all the interaction terms in panel B between treatment kibbutzim and the cohorts dummy variables are jointly zero. Standard errors clustered at the kibbutz level are presented in parentheses.

both cases, again confirming the balancing tests' results on pre-reform outcomes presented in Table I. The results when we add kibbutz fixed effects to the regressions (presented in columns 2 and 4 of the table) are very similar.

The evidence presented in the cohort dummies model 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, based on the F-tests presented in the table, we cannot reject the hypothesis that all the interaction terms are jointly equal to zero. We conclude that both groups were on a similar time trend of educational matriculation outcomes in the six years prior to the reform.

Did the Control and Treatment Kibbutzim Experience Different Exit or Entry Rates?

We address this concern by checking whether the likelihood that a student leaves or enters a kibbutz is associated with the timing of the reform in his kibbutz. 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 the 12th grade.[10] We define a student as entering if he did not live in his kibbutz at the start of the 10th grade and lived in by the end of the 12th grade. We estimate whether there is such a differential exit or entry rate for the pre-treatment (1995–1996) and post-treatment sample (1999–2000). Panel C of Table I shows that the likelihood that a student leaves or enters his kibbutz is relatively low and unrelated to the implementation of the pay reform: the DID in exit and entry rates are essentially zero, and exit and entry rates remained the same over time in both the treatment and control groups.[11]

Are Kibbutzim That Never Reformed an Appropriate Comparison Group?

Kibbutzim that never reformed differ from those that did in that they had different experiences in the decade leading to the reform period (Abramitzky (2008)). Specifically, kibbutzim that reformed experienced a deeper financial crisis and higher exit rates in the decade leading to the reform. Subsequently, kibbutzim that never reformed formed the "egalitarian/communal wave" (*zerem shitufi*) that revived the traditional egalitarian norms by instilling communal and equality norms in members, opposed the reforms in other kibbutzim, and proudly became "the only kibbutzim like in the good old days." These kibbutzim have often become even more successful economically and socially. There are thus reasons to believe kibbutzim that did not reform strengthened their group identity and social norms, which may have improved educational outcomes through a different channel. Empirically,

[10] Note that students are included in the sample based on their location at the start of 10th grade, so students who exit a kibbutz during high school are included, whereas those who enter are not.

[11] A similar analysis of entry and exit rates for the 1997–1998 cohorts yields comparable results, which are available from the authors.

in Tables S.XII–S.XIV in the Supplemental Material, we present versions of these three tests that compare treatment kibbutzim with kibbutzim that never reformed. We show a large and significant difference in exit rates of the post-reform cohort, and significant differences in some of the students' observable characteristics. These results suggest students in kibbutzim that never reformed differ from the early reformers in ways that make them an inappropriate comparison group. We also show in Table S.XV in the Supplemental Material that kibbutzim that reformed in 2001–2002 had a larger and significant exit rate among the 1999 and 2000 10th grade cohorts, which suggests that it is preferable not to use these kibbutzim as a control group.

Were There Anticipation Effects?

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, any anticipation effects that were present make it more difficult for us to find an impact of the reform. Second, our choice of kibbutzim that reformed at least four years after the treatment kibbutzim reformed as our control group makes such anticipation effects less likely and less prominent if they exist. Third, empirically, we do not find evidence of anticipation effects, in the sense that educational outcomes in control kibbutzim are similar for the earlier and later cohorts (see footnote 16, where we present and discuss this evidence).

4. THE EFFECT OF THE REFORM ON HIGH SCHOOL EDUCATIONAL OUTCOMES

4.1. *Basic Results*

This section shows the basic results without taking the intensity of the reforms into account. Panel A, first row, of Table III reports simple DID estimates with no additional controls (equation (1)), and in the second row we present the DID estimates which are based on regressions that also include individual characteristics and kibbutz fixed effects (equation (2)). Each cell in the table shows the estimated coefficient on the post cohort in treated kibbutzim. We find a positive coefficient of interest for all schooling outcomes, although some of the estimates are not precisely measured. The simple and controlled DID treatment effect estimates are similar, which is a result of the similarity between treatment and control groups in observables characteristics and in pre-reform outcomes.

Focusing the discussion on the controlled difference-in-differences estimation, the high school completion rate is up by 3.3 percent (se $= 0.017$), an im-

TABLE III

DIFFERENCE-IN-DIFFERENCES ESTIMATES[a]

	No Control for Other Social Reforms					Control for Other Social Reforms				
	High School Completion (1)	Mean Matriculation Score (2)	Matriculation Certification (3)	University Qualified Matriculation (4)	Outcome Index (5)	High School Completion (6)	Mean Matriculation Score (7)	Matriculation Certification (8)	University Qualified Matriculation (9)	Outcome Index (10)
A. Experiment of Interest, 10th Grade Students in 1995–1996 and 1999–2000										
Difference-in-Differences Regressions										
i. Full Sample										
Simple difference-in-differences	0.033	3.112	0.029	0.040	0.101	–	–	–	–	–
	(0.016)	(1.517)	(0.035)	(0.035)	(0.072)	–	–	–	–	–
Controlled difference-in-differences	0.033	3.546	0.049	0.060	0.141	0.048	4.501	0.076	0.082	0.206
	(0.017)	(1.605)	(0.035)	(0.036)	(0.071)	(0.020)	(1.985)	(0.042)	(0.043)	(0.087)
Probit controlled difference-in-differences,	0.034		0.054	0.066		0.049		0.088	0.095	
marginal effects	(0.014)		(0.037)	(0.039)		(0.015)		(0.047)	(0.047)	
ii. Sample Stratification by Mother's Education										
Low	0.049	6.175	0.116	0.100	0.265	0.071	6.539	0.093	0.073	0.237
	(0.028)	(2.556)	(0.053)	(0.052)	(0.110)	(0.036)	(3.717)	(0.072)	(0.066)	(0.147)
High	0.014	0.329	−0.031	0.002	−0.015	0.030	1.804	0.022	0.049	0.101
	(0.019)	(2.050)	(0.046)	(0.046)	(0.092)	(0.021)	(2.279)	(0.054)	(0.054)	(0.112)
F-statistic	1.244	3.329	4.259	2.116	3.764	1.025	1.207	0.566	0.074	0.493
p-value	0.267	0.071	0.041	0.149	0.055	0.314	0.274	0.453	0.787	0.484

(*Continues*)

TABLE III—*Continued*

	No Control for Other Social Reforms					Control for Other Social Reforms				
	High School Completion (1)	Mean Matriculation Score (2)	Matriculation Certification (3)	University Qualified Matriculation (4)	Outcome Index (5)	High School Completion (6)	Mean Matriculation Score (7)	Matriculation Certification (8)	University Qualified Matriculation (9)	Outcome Index (10)
iii. Sample Stratification by Gender										
Male	0.051	4.816	0.060	0.055	0.166	0.072	5.590	0.082	0.073	0.226
	(0.026)	(2.697)	(0.053)	(0.050)	(0.111)	(0.032)	(3.215)	(0.063)	(0.062)	(0.133)
Female	0.012	2.582	0.025	0.031	0.068	0.023	3.904	0.050	0.051	0.124
	(0.020)	(2.140)	(0.048)	(0.051)	(0.103)	(0.022)	(2.319)	(0.059)	(0.062)	(0.127)
F-statistic	1.567	0.412	0.234	0.116	0.393	1.802	0.210	0.136	0.062	0.299
p-value	0.213	0.522	0.630	0.734	0.532	0.182	0.647	0.713	0.803	0.586
B. Placebo Experiment										
Simple difference-in-differences	0.011	0.213	−0.016	−0.025	−0.030	–	–	–	–	–
	(0.015)	(1.527)	(0.036)	(0.036)	(0.076)	–	–	–	–	–
Controlled difference-in-differences	0.011	0.377	−0.011	−0.025	−0.025	0.011	0.035	0.003	−0.014	−0.001
	(0.017)	(1.580)	(0.035)	(0.036)	(0.076)	(0.017)	(1.633)	(0.034)	(0.035)	(0.074)

[a]The dependent variable in column 1 is an indicator of whether the student completed high school; in column 2 it is her mean score in the matriculation exams; in column 3 it is an indicator of whether she received a matriculation certificate; in column 4 it is an indicator of whether she received a matriculation certificate that satisfies the requirements for university study; the dependent variable in column 5 is an outcome index that receives the following values: 0 if the student drops out of school, 1 if the student graduates without receiving matriculation certification, 2 if the student receives a matriculation certification, and 3 if the student receives a matriculation certification that is university qualified. Panel A presents the estimated 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 1995–1996). The simple difference-in-differences regressions include only cohort dummies. The controlled difference-in-differences regressions include cohort dummies, kibbutz fixed effects, and the following student's 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). In the regressions' results reported in columns 6–10, we also include four indicators of other social reforms. The Probit Controlled difference-in-differences regressions (presented in row 3) report the marginal effects calculated at the sample mean. In these regressions, we include the same controls as in the controlled difference-in-differences regression. We run the Probit regressions only for the binary outcome variables. The lower sections of panel A stratify the sample by high and low maternal education (threshold defined to be the median maternal education) and by gender. The specifications in these regressions are identical to the controlled difference-in-differences regressions. F-statistic and p-value are reported for the hypothesis that all the coefficients of both groups are equal. Panel B presents placebo difference-in-differences regressions parallel to those in panel A, comparing two untreated cohorts, 10th grade students in 1995–1996 and 10th grade students in 1997–1998. Standard errors clustered at the kibbutz level and are presented in parentheses.

pressive gain given that the pre-treatment mean was 0.951 and the fact that this rate cannot exceed 1. The mean exam score is up by 3.55 points relative to 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 (although the coefficient is not statistically significant) and the university qualified *Bagrut* rate is up by 6 percentage points (marginally statistically significant), 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 pass the matriculation program in at least one advanced placement subject. These two criteria are an admission requirement for all universities and most colleges in Israel. The improvement we observe likely reflects a higher intention to enroll in post-secondary schooling. Finally, it is worth noting that in Table S.XVI, we present the cross-section regressions for the pre- and post-reform period, which show that most of the difference-in-differences estimates reflect post-treatment differences in favor of the treatment group.

We also explore an alternative dependent variable that combines the information of three of our outcomes, an index variable that is 0 for high school dropouts, 1 for high school graduation, 2 for matriculation certification, and 3 for university-qualified matriculation. Imposing cardinality, we estimate an OLS regression with this outcome index as the dependent variable. The estimated difference-in-differences effects on this index are presented in column 5 of Table III. The controlled difference-in-differences estimate is 0.141 (se = 0.071).

We also use a probit specification instead of the linear probability models to estimate equation (2). These estimates are presented in the third row of panel A in Table III. The implied respective estimated marginal effects are similar to those reported in the second row of this table, for example, the probit marginal effect on high school completion rate 0.034 (se = 0.014) versus 0.033 (se = 0.016) in the OLS regression.

In panel B of Table III, we present a placebo 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 DID are very different from the treatment estimates presented in panel A of Table III, and they are very close to zero. For example, the placebo estimate of the effect on average *Bagrut* score is 0.377 (se = 1.580), and the estimates on the two *Bagrut* diploma outcomes and on the outcome index 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, that is, no effect.

4.2. *Controlling for Other Reforms*

One potential concern is that the pay reforms affected schooling outcomes by changing social incentives more broadly. In fact, the 1990s saw a number of other reforms in kibbutzim that are likely to have changed social incentives to invest in schooling without changing the financial returns to education. If our estimates of the effects of the pay reform are insensitive to the inclusion of controls for these social reforms, and the estimated effects of these reforms are small, this will suggest the social incentives channel is unlikely to be a major driver of our estimated effect of the pay reform. We collected information on the precise years in which four relevant reforms were implemented: the introduction of user fees for (i) meals in the common dining room, (ii) electricity at home, (iii) personal laundry, and (iv) private health insurance.

Controlling for these social reforms does not alter the estimated effect of the pay reform. These results are presented in columns 6–10 of Table III. Overall, the estimates from these specifications are marginally larger and more precisely estimated than the estimates reported in the same row in columns 1–5. We first note that all five outcome treatment estimates are statistically different from zero, two at the 10 percent level of significance and 3 at the five percent level of significance. Focusing on the controlled difference-in-differences estimates in the second row of the panel, the effect on school completion rate is now 0.048 (se = 0.020), on the average score 4.5 points (se = 1.985), on the university matriculation rate it is 0.082 percent (se = 0.043), and on outcome index it is 0.206 (se = 0.087).

The probit regressions estimates from regressions that control for the other social reforms are very similar to the OLS regressions. For example, the probit estimate on the high school completion rate is 0.049 versus 0.048 in the OLS regression. Finally, we also note that none of the estimated effect of these four other social reforms is significant (see estimates in Table S.XVII in the Supplemental Material).[12]

4.3. *Allowing for Differential Effect by "Intensity" of Reform*

The pay reform was not identical across kibbutzim. Specifically, some kibbutzim introduced a full pay reform, moving to a "safety net" model that reflected market forces. Other kibbutzim introduced only a partial pay reform, moving to a "combined" model (*meshulav*) that was still based on market

[12]In Table S.XVIIa in the Supplemental Material, we present the cross-section regressions that include controls for the four social reforms and that correspond to the above difference-in-differences estimates. Again, one can see that these estimates are very similar to the cross-section estimates without the controls for the four social reforms, and that the difference-in-differences estimates reflect mainly an increase in outcomes in the post-treatment period with statistically zero treatment-control at baseline.

forces, but combined them with a more progressive tax and wider safety net for members.[13] 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 to a complete differential pay structure. We can exploit these changes to define treatment intensity because some of these kibbutzim made the second change within the period of treatment.[14] 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 define four treatment groups, ranging from three years of full reform to zero years of full reform (three years of partial reform).[15]

The group with zero intensity of full pay has the lowest estimated effects, while the highest estimated effects are for the group with the highest intensity of treatment, although the differences between the coefficients are only statistically significant when controls for the other reforms are included. These results are presented in Table IV, columns 1–5 (without controlling for other social reforms) and columns 6–10 (with controls for other four social reforms). 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 two or three years has large and significant effect on all four outcomes (although, again, the differences between the coefficients are not statistically significant). For example, three years in high school under a full differential pay system causes an 8.2 percentage point increase in the matriculation rate and a 10.0 percentage point increase in the university qualified matriculation rate. When controlling

[13]We could not obtain information on kibbutz tax schemes so cannot quantify the partial and full pay reform.

[14]Specifically, of the 37 kibbutzim that reformed in 1998, 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 (before 2003). 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.

[15]We perform balancing tests similar to those presented in Table III, and the results suggest that the students in these four treatment groups are statistically indistinguishable from the students of the control group in their observed characteristics (see Tables S.IX–S.XI in the Supplemental Material).

TABLE IV

CONTROLLED DIFFERENCE-IN-DIFFERENCES ESTIMATES BY LEVEL OF INTENSITY OF EXPOSURE TO FULL DIFFERENTIAL PAY[a]

	No Control for Other Social Reforms					Control for Other Social Reforms				
	High School Completion	Mean Matriculation Score	Matriculation Certification	University Qualified Matriculation	Outcome Index	High School Completion	Mean Matriculation Score	Matriculation Certification	University Qualified Matriculation	Outcome Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Intensity of Exposure										
Three years of full reform	0.029	4.288	0.082	0.100	0.212	0.056	5.984	0.147	0.146	0.348
($N = 405$)	(0.022)	(2.105)	(0.049)	(0.049)	(0.101)	(0.026)	(2.748)	(0.058)	(0.057)	(0.117)
Two years of full reform	0.054	5.621	0.031	0.083	0.167	0.067	6.548	0.058	0.105	0.230
($N = 211$)	(0.019)	(2.098)	(0.049)	(0.053)	(0.108)	(0.021)	(2.310)	(0.053)	(0.055)	(0.116)
One year of full reform	0.053	3.744	0.009	−0.020	0.042	0.067	4.766	0.035	0.004	0.106
($N = 114$)	(0.020)	(2.479)	(0.054)	(0.048)	(0.100)	(0.023)	(2.652)	(0.055)	(0.054)	(0.108)
Three years of partial reform	0.016	1.239	0.036	0.025	0.077	0.025	2.130	0.060	0.049	0.134
($N = 313$)	(0.023)	(2.259)	(0.048)	(0.051)	(0.099)	(0.025)	(2.474)	(0.051)	(0.054)	(0.106)
F-statistic	2.379	2.065	0.801	1.539	1.298	3.084	2.239	1.723	2.369	2.383
p-value	0.056	0.091	0.527	0.196	0.276	0.019	0.070	0.150	0.057	0.056

(*Continues*)

TABLE IV—*Continued*

	No Control for Other Social Reforms					Control for Other Social Reforms				
	High School Completion (1)	Mean Matriculation Score (2)	Matriculation Certification (3)	University Qualified Matriculation (4)	Outcome Index (5)	High School Completion (6)	Mean Matriculation Score (7)	Matriculation Certification (8)	University Qualified Matriculation (9)	Outcome Index (10)
B. Intensity of Exposure: Partial versus Full										
Three years of full reform	0.030	4.431	0.084	0.103	0.216	0.062	5.882	0.188	0.189	0.440
($N = 405$)	(0.022)	(2.120)	(0.049)	(0.050)	(0.102)	(0.029)	(3.158)	(0.063)	(0.061)	(0.124)
Three years of partial reform	0.015	1.285	0.035	0.026	0.077	0.024	2.039	0.076	0.063	0.163
($N = 313$)	(0.023)	(2.286)	(0.048)	(0.051)	(0.100)	(0.027)	(2.601)	(0.053)	(0.055)	(0.109)
F-statistic	0.943	2.185	1.505	2.141	2.301	2.292	1.734	4.540	4.900	6.298
p-value	0.393	0.118	0.227	0.123	0.105	0.106	0.181	0.013	0.009	0.003

[a]This table presents the results of difference-in-differences regressions comparing students in treatment (reformed 1998–2000) and control (reformed 2003–2004) kibbutzim who are treated (10th grade in 1999–2000) and untreated (10th grade in 1995–1996), 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. The value of N for each intensity of treatment is the number of students who faced that intensity of treatment. 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. The outcome index receives the following values: 0 if the student drops out of school, 1 if the student graduates without receiving matriculation certification, 2 if the student receives a matriculation certification, and 3 if the student receives a matriculation certification that is university qualified. 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 following 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). Clustered standard errors at the kibbutz level are presented in parentheses. F-statistic and p-value are reported for the hypothesis that all the coefficients of treatment's intensity are zero.

for the other social reforms, the two respective estimates are higher, 14.7 and 14.6 percent, both precisely measured.[16]

The results presented in panel B are very similar to the results in panel A, and they highlight the difference in estimated treatment effect of three years of full differential pay versus three years of partial differential pay. Overall, the evidence reported in Table IV 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 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 duration.

We also explore an alternative specification that exploits information on students in all kibbutzim that reformed between 1998 and 2004, and assign separate treatment dummy indicators for students in cohorts that spent 1, 2, and 3 years of their high school in a reformed kibbutz. We then regress the outcome variable on these three indicators of length of exposure to treatment, a full set of year of reform dummies, a full set of cohort dummies, controls for the other four social reforms, and all other students' control variables. Like the difference-in-differences specification, the treatment variable is identified by (reform year) $*$ cohort interactions, but now exploits all possible variation. The results of this estimation are presented in Table S.XVIII in the Supplemental Material, and the estimates are similar to our benchmark difference-in-differences specification and sample, which are reported in Table IV, columns 6–10.

4.4. *Allowing for Heterogeneous Effects: Heterogeneous Effect by Social Background*

First, we look at whether the pay reform, full or partial, affected students with different social backgrounds differently. On the one hand, assuming utility is concave in income, we expect students from lower social classes, who will face a decrease in parental income and are expected to have lower personal income on average, to be more affected by the decrease in the income tax because a future dollar increase in earnings is more valuable for them. Moreover, we expect students from lower social backgrounds to be more affected by the

[16]In the end of Section 3, we argued against the likelihood of an anticipation effect, namely, the possibility 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. To further support this claim, in Table S.XVIIb, we present results from an extended version of our intensity of reform regressions, where we allow the effect of the reform to differ for students who spent 1, 2, or 3 years of high school in a reformed kibbutz, and also for students whose kibbutzim reformed in the year after they finished high school. If anticipation effects were important, we would expect this last group to also show improved schooling outcomes. In fact, the estimated coefficients for this group are small and insignificantly different from zero for all four dependent variables.

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. On the other hand, students whose parents are more educated might receive more help at home or elsewhere, because their parents are more able to help them or pay for private tutoring, and thus be in a better position to improve their schooling when given the incentives. We stratify 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 other students. Alternatively, we stratify by the father's years of schooling and find similar results.[17]

The heterogeneous estimates by parental schooling presented in panel C of Table III suggest that the total effect on educational outcomes is largely driven by students who have less educated parents (although the differences are not statistically significant). That is, these estimated treatment effects for these students appear larger than the basic controlled difference-in-differences results presented in Table III, and their percentage increases are also larger because their counterfactual means are much lower than the mean of the overall sample.

Next, we allow for heterogeneity of the effect by parental education and intensity of reform simultaneously. Consistent with the evidence presented in this section and the previous one, Table S.XIX in the Supplemental Material suggests that the total effect on educational outcomes seems to be largely driven by students who were exposed to a full differential pay system throughout their high schools and whose parents have lower levels of education. These results by parents' education level are the opposite of Jensen's (2010). We note that the less educated parents in the kibbutz are, on average, more educated than the more educated parents in the Dominican Republic, meaning that financial constraints are likely to be less important in our context. We again note that this finding that students whose parents are less educated respond more rules out a possible income effect whereby we would expect more educated people who gained from the reform to respond more because they could invest more in their children's education. However, less educated parents experienced a decline in their income following the pay reform. This change may have triggered children, potentially with encouragement from their parents, to invest more in schooling in order to offset the lower well-being associated with lower relative income at adulthood, as suggested by Luttmer (2005).

Our result that children from low educated families respond more strongly to the reduction in the income tax rate could reflect a higher rate of return to schooling perceived by this group. A growing body of evidence suggests that,

[17]We also ran balancing tests like those reported in Table III for these subsamples. The results suggest that the treated and the respective control group have very similar characteristics, regardless of whether we stratify the sample by father's or by mother's schooling.

indeed, the rate of return to schooling is higher among individuals who are more credit constrained, have greater immediate need to work, or have greater distaste for school (Card (1995, 1999, 2001)). Brenner and Rubinstein (2011) showed evidence of higher returns to schooling for individuals in poor families in the United States.

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 being more responsive. However, our estimates stratified by gender, presented in panel D of Table III, 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. For example, the estimated effect on high school completion is 0.051 (se $= 0.026$) for males and 0.012 (se $= 0.020$) for females.

Finally, Table S.XX in the Supplemental Material suggests that the treatment effect is not only larger for students who were exposed to a full differential pay system throughout their high school years, but it is the largest for *boys* who were fully exposed (although we note again that differences between boys and girls are not statistically significant). 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 6.0 point increase (2.8 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.

Our findings that there are no significant differences between the effect of the pay reforms on boys versus the effect on girls, 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 those of Angrist and Lavy (2009), who found that girls' *Bagrut* diploma attainment is affected by conditional bonus payments, whereas boys do not react to this monetary incentive. In these papers, girls respond 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 to work in lower paying jobs on average, perhaps because they do not expect to become the main earner (e.g., 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).

5. THE EFFECT OF THE REFORM ON POST-HIGH SCHOOL EDUCATIONAL OUTCOMES

This section discusses estimates of the effects of the pay reform in kibbutzim on college enrollment and completed years of schooling. In assessing this exercise, we should note that, unlike high school outcomes, post-secondary schooling could be affected by the pay reform through two channels. The first channel operates through the effect of the improved high school outcomes and the higher educational aspirations while in high school. The second channel is an additional effect where individuals may respond as adults to the higher rate of return to schooling, regardless of their attainment in high school. The treatment group is exposed to both effects, while the control group is exposed only to the second because their kibbutzim reformed after they completed high school. In this paper, we cannot cleanly distinguish between these two potential channels because the effect of an increase at adulthood in the rate of return to schooling on the decision to pursue higher education could be different for individuals in treated and control kibbutzim. If these two effects are similar, then the estimates reported below capture mainly the first channel of effect on post-high school education.

The post-high school academic schooling system in Israel includes seven universities (one of which confers only graduate and Ph.D. degrees), over 45 colleges that confer academic undergraduate degrees (some of these also give Master's degrees), and dozens of teachers' colleges that confer Bachelor of Education degrees.[18] All universities require a *Bagrut* for enrollment. Most academic colleges and teachers' colleges also require a *Bagrut*, though some look at specific *Bagrut* components without requiring full certification. For a given field of study, it is typically more difficult to be admitted to a university than to a college. The national enrollment rates for the cohort of graduating seniors in 1995 (through 2003) was 55.4 percent, of which 27.6 percent were enrolled in universities, 8.5 percent in academic colleges, 7 percent in teachers' colleges, and the rest in non-academic institutions.[19]

The post-high school outcome variables of interest here are indicators of ever having enrolled in a post-high school institution of a type described above, as of the 2010–2011 school year, and the number of years of schooling completed in these institutions by this date. We measure these two outcomes for our 1995–2000 high school graduating cohorts. The youngest cohorts (1999 and 2000) in our sample are 28–29 years old in 2010–2011. Even after accounting

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

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

for compulsory military service,[20] we expect that most students who enrolled in post-high school education, including those who continued schooling beyond undergraduate studies, to have graduated by the 2010–2011 academic year. We therefore present evidence both for enrollment and for completed years of post-high school education.

Our information on post-secondary enrollment comes from administrative records provided by Israel's National Insurance Institute (NII). The NII is responsible for social security and mandatory health insurance in Israel; it tracks post-secondary enrollment because students pay a lower health insurance tax rate. Post-secondary schools are therefore required to send a list of enrolled students to the NII every year. For the purposes of our project, the NII Research and Planning Division constructed an extract containing the 2001–2011 enrollment status and number of years of post-secondary schooling of students in our study. This file was merged with the other information in our sample and we used it for analysis at the protected research lab with restricted access at NII headquarters in Jerusalem.

We coded three indicators for enrollment in post-high school education. The first indicator identifies if the person ever enrolled in one of the seven universities (at any time from 2001–2011); the second identifies if she ever enrolled in one of the certified academic colleges; and the third identifies if she ever enrolled in a teachers' college. The overall ever enrolled rate in any post-secondary schooling in our sample is 69 percent, of which 31 percentage points is in one of the seven universities, 32 percent is in an academic college, and 2.3 percentage points is in a teachers' college.[21] The average number of post-high school years of schooling completed until the school year 2010–2011 in our sample is 2.7, of which 1.21 are in university schooling, 1.25 are in college education, and 0.05 are in teachers' colleges. Table S.XXI presents more detailed descriptive statistics of these variables by treatment and control groups and by pre- and post-reform cohorts.

Table V presents results from our basic sample and specification of the controlled difference-in-differences without (panel A) and with (panel B) controlling for the other social reforms. Specifically, the sample includes students of cohorts 1995, 1996, 1999, and 2000 from kibbutzim that reformed in 1998–2000 and 2003–2004. Overall, when not controlling for other social reforms, the results suggest the reform increased post-high school enrollment by 4.3% points, although the coefficient is not statistically significant. Interestingly, while the reform did not positively affect (in fact, insignificantly negatively affected) university enrollment (column 2), it increased academic college enrollment by 7%

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

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

TABLE V

THE EFFECT OF THE PAY REFORM ON POST-SECONDARY SCHOOLING[a]

	Enrollment in Post-High School Education				Post-High School Years of Schooling			
	All	University	Academic Colleges	Teachers' Colleges	All	University	Academic Colleges	Teachers' Colleges
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. No Control for Other Social Reforms								
Difference-in-Differences Regressions								
i. Full Sample								
Controlled difference-in-differences	0.043	−0.031	0.070	0.019	0.054	−0.152	0.174	0.048
	(0.036)	(0.030)	(0.038)	(0.010)	(0.167)	(0.137)	(0.119)	(0.033)
ii. Sample Stratification by Gender								
Male	0.068	0.031	0.080	0.016	0.260	−0.034	0.267	0.039
	(0.053)	(0.046)	(0.049)	(0.008)	(0.233)	(0.186)	(0.140)	(0.026)
Female	0.026	−0.097	0.078	0.023	−0.152	−0.285	0.124	0.052
	(0.048)	(0.048)	(0.061)	(0.022)	(0.244)	(0.193)	(0.185)	(0.060)
F-statistic	0.406	3.298	0.000	0.084	1.776	1.029	0.464	0.043
p-value	0.525	0.072	0.984	0.772	0.186	0.313	0.497	0.836

(*Continues*)

TABLE V—*Continued*

	Enrollment in Post-High School Education				Post-High School Years of Schooling			
	All (1)	University (2)	Teachers'Colleges (3)	Academic Colleges (4)	All (5)	University (6)	Academic Colleges (7)	Teachers'Colleges (8)
B. Control for Other Social Reforms								
Difference-in-Differences Regressions								
i. Full Sample								
Controlled difference-in-differences	0.048	−0.025	0.094	0.010	0.019	−0.161	0.219	0.018
	(0.046)	(0.035)	(0.047)	(0.011)	(0.204)	(0.148)	(0.140)	(0.042)
ii. Sample Stratification by Gender								
Male	0.098	0.042	0.123	0.017	0.279	0.004	0.304	0.033
	(0.072)	(0.055)	(0.061)	(0.008)	(0.288)	(0.223)	(0.187)	(0.036)
Female	0.003	−0.087	0.062	0.009	−0.270	−0.301	0.121	0.001
	(0.052)	(0.052)	(0.069)	(0.023)	(0.278)	(0.213)	(0.198)	(0.071)
F-statistic	1.430	2.699	0.484	0.089	2.573	1.019	0.544	0.192
p-value	0.234	0.103	0.488	0.766	0.112	0.315	0.462	0.663

[a]This table presents the results of Post-Secondary Schooling Outcomes difference-in-differences regressions comparing students in treatment (reformed 1998–2000) and control (reformed 2003–2004) kibbutzim who are treated (10th grade in 1999–2000) and untreated (10th grade in 1995–1996). The regressions' results presented in panel A are based on the full sample (i), and on subsamples of boys and girls (ii). The estimates in panel B are based on difference-in-differences regressions with added controls of indicators of implementation of four other social reforms. Here as well, we present results for the full sample and results by gender. The dependent variables in columns 1–4 are dummy variables that receive the value 1 if the student was enrolled in a given type of post-secondary schooling and 0 otherwise. The dependent variables in columns 5–8 are variables that measure the number of post-high school years of schooling in any given type of post-secondary schooling. Standard errors are clustered at the kibbutz level and are presented in parentheses.

points (column 3), which reflects a 22% increase relative to the baseline of the treatment group, and increased teachers' college enrollment by 1.9% points, an over 100% increase. In columns 5–8, we present the estimated effects on completed years of post-high school education by the various categories of higher education. The evidence here shows the same pattern as the effects on enrollment: an average increase of 0.174 years of academic college schooling and a 0.048 increase in years of teachers' college education, though the latter effect is not significantly different from zero, and a negative but insignificant effect on university schooling.[22]

Patterns are similar when controlling for the four other major reforms implemented in the kibbutzim since the early 1990s (panel B), though the estimated effect on academic college enrollment and college completed years is marginally higher and the estimated effect on teachers' colleges enrollment is lower and less precisely measured.[23]

There are several possible reasons for why the reform increased enrollment and attainment in colleges but not in universities. First, we showed that the effect on high school outcomes was largely driven by the subgroup of students whose parents were less educated, and such students are more likely to enroll in colleges, where admission requirements tend to be less strict than at universities.[24] Second, the number of academic colleges expanded dramatically since the mid 1990s, making them more accessible and less costly than university education, since these colleges are located in all regions of the country. The proximity of many kibbutzim to these new colleges made it possible for kibbutz members to enroll in higher education without having to move to a big city, where the universities are located. Third, the decline in university enrollment may reflect a shift in preferences of kibbutz students among different tracks

[22]We also estimated the effect of the pay reform on post-high school education within 14 years of being in 10th grade, namely, when most of the treated cohorts reached age 30. The results when we impose this restriction in calculating the higher education outcomes are presented in Table S.XXII in the Supplemental Material, and they are similar to those reported in Table V in the paper. For example, the effect on academic college enrollment is 6.8 percent and on academic colleges' years of schooling it is 0.168, both estimates similar to the respective estimates reported in Table V.

[23]We also estimated these models by including in the sample the cohorts of 1997 and 1998, and the results are very similar to those reported in Table V. We also estimated the models of the effect of measures of reform intensity on post-high school schooling outcomes, and the results suggest as well a positive effect on college education. However, the distinction between and interpretation of the estimated effects by intensity of treatment are less clear in this case because it has been more than a decade since the end of high school, and some time since even the control kibbutzim reformed.

[24]We also estimated the effect of the pay reform separately for students of low and high parental education. The results obtained from the sample of students with low parental schooling indicate mainly an increase in enrollment and years of schooling in academic teachers' colleges. This is not surprising because the enrollment of students from low education families at universities was lower before the reform started because universities typically have higher admission requirements.

of higher education following the pay reform. For example, kibbutz members may now find university education, especially in the humanities and social sciences, to be less attractive and less "practical" in terms of financial rewards in the "new" kibbutz in comparison with law, economics, and business education, which are now available in almost all the academic colleges. Such a shift in preferences may have been more relevant to women, who tended to enroll in larger proportions in humanities at universities, and now may be shifting to more financially rewarding subjects.

Consistent with this idea, in panel C of Table V, we present the estimates obtained from separate samples of boys and girls. These panels suggest that the reform induced a shift of girls away from university enrollment and toward colleges. Regrettably, our data do not allow a more rigorous examination of this conjecture. However, we note that the net effect on girls is close to zero, consistent with our findings in the previous section of no effect of the pay reform on girls' high school outcomes. In the sample of boys, on the other hand, the effect is positive both on university and on academic college enrollment, though only the latter is significant in the specification without controls for the other social reforms and both are significant when these controls are included. The effect on boys' academic college years of schooling is quite large, over a quarter of a year of schooling, which is about a 28 percent increase.

Another way for measuring the effect on post-secondary schooling is to create three cumulative educational outcome measures. The first is an indicator of being ever enrolled in "at least teacher colleges," which gets value 1 if a student enrolled in university, academic college, or teachers' college. The second, "at least academic colleges," is an indicator of enrollment in university or college, and the third indicator is "university." Similarly, we create three variables measuring completed years of schooling that correspond to each of these enrollment indicators. We report the results of the estimated effect of the pay reform on these outcomes in Table S.XXIII in the Supplemental Material. The results are as expected given the evidence in Table V. In the full sample, there is a positive effect on enrollment in "at least academic colleges" and also in "at least teachers' colleges" (and these estimates are significant in specification with controls for other social reforms). For boys, these estimates seem larger and more precisely measured. The estimated effects on years of schooling of university and academic colleges or of university, academic, and teachers' colleges for boys are positive and large (about 0.3 year of at least academic college schooling), but their respective t-values are only 1.4–1.5 (in specification with controls for other social reforms).

6. CONCLUSIONS AND IMPLICATIONS

In this paper, we use a natural experiment to estimate the responsiveness of investment in education to changes in redistributive policy that change the returns to education. This is, to the best of our knowledge, one of the first studies[25] that use 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. We find students are indeed responsive to changes in the redistributive policies: when their kibbutzim reformed, they considerably improved their educational outcomes such as whether they graduated and their average matriculation exam scores. Students who spent their entire three years of high school in a kibbutz that reformed to a greater extent improved their educational outcomes more. Males seem to have reacted more strongly than females, and students with less educated parents appear to have reacted more strongly than those with more educated parents, although these differences between subgroups are not statistically significant.

The pay reform increased the returns to schooling, which encouraged students to invest more in education. However, the pay reform could have influenced schooling outcomes through two other channels: first, via the reduction in social incentives for encouraging education that had been used under equal sharing (pre reform), and second, via the changes in income levels of parents, which might affect education decisions through liquidity constraints, because children's education is a normal good, or through the concavity of utility in income assuming some intergenerational transfers. Our paper cannot fully disentangle these mechanisms, but our findings provide suggestive evidence that the returns to the education channel operated above and beyond the social incentives channels, and that the income channel played only a limited role. Specifically, when we control for other reforms in the kibbutzim that arguably changed social incentives without altering the returns to education, the effect of the pay reform is largely unaffected. Moreover, if liquidity constraints or the normality of children's education were important, then we would expect students whose parents experienced declines in their income (less educated parents) to reduce investment in schooling. Instead, we find the *improvement* in schooling outcomes is largely driven by students whose parents have low education. Parental income effects that operate through the concavity of utility also seem unlikely the main drivers of the results because they should be small on average within a kibbutz, as some parents face income increases and others decreases, yet the overall effect of the reform is positive on average (even if not always statistically significant). We further show that, for many students, the increase in high school outcomes translated into increased enrollment in academic and teachers' colleges and practical engineering schools (possibly at the expense of University enrollment).

While an important advantage of our setting is the high internal validity of the estimates, we believe that our findings also have implications beyond the Israeli context. First, they shed light on the educational responses that could

[25] Foster and Rosenzweig (1996) showed that the green revolution in India increased returns to primary schooling and resulted in increases in private investments in schooling.

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 returns to education increase. For example, such changes might be occurring in many countries as technology-oriented growth increases the return to skills.[26] While the pay reform in kibbutzim is likely larger than many other policy changes aiming to reduce the income tax rates or increase the rates of return to education, the kibbutz serves as a microcosm for learning about other important episodes with similarly large reforms. Examples of such episodes include the transitions of central and eastern European countries from centrally planned to market economies after the fall of the Iron Curtain (see Brainerd (1998)), the abolition of village collectives in China in the 1980s, and Vietnam's labor market liberalization in the mid 1980s (see Moock, Patrinos, and Venkataraman (1998) and Svejnar (1999)). Finally, our findings may improve our understanding of the large human capital gap between first and second generation immigrants in developed countries (see Aydemir and Sweetman (2006)). Our findings suggest that part of the higher education of immigrants' children from some countries could be due to the higher rates of return to schooling they experience in their host countries relative to the returns in their home countries.

Our findings also contribute to the literature on the increase in earnings inequality in the United States and many other developed countries over the past decades, which perhaps is one of the most important aggregate phenomena in labor markets since WWII (known as "Skill Biased Technological Change"). A large body of research focuses on the implications of technological advancement for the demand for skill (see Katz and Autor (1999), Autor and Katz (1999), and recent updates of this survey, e.g., Autor, Katz, and Kearney (2008)), yet no attention is given to estimating the impact of the returns to education on the supply of educated workers. This is a key factor for understanding the longer-run consequences of changes in the demand structure in the era of "Skill Biased Technological Change." To the best of our knowledge, this paper is the first to tackle this question. Estimating the supply elasticity requires an external source of variation in the returns to education, solely driven by demand factors, and independent of preexisting stocks and current flows of skilled labor. This might explain the lack of credible empirical research on this front. The experience of the Israeli economy during the 1980s–1990s in general, and the kibbutzim communities in particular, provides a unique setting for estimating the causal impact of the returns to education on school choices and the supply of educated workers.

[26]See, for example, the discussion in Autor, Katz, and Krueger (1998), Card and DiNardo (2002), and Goldin and Katz (2008).

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