The Effect of Changes in the Skill Premium on College Degree Attainment and the Choice of Major *

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Abstract

We study the impact of financial incentives on higher education decisions and the choice of major. We rely on a reform whereby Israeli kibbutzim shifted from their traditional policy of equal sharing to productivity-based wages, using for identification the staggered implementation of this reform. In this setting of very low initial returns to education, we find that the dramatic increase in the rate of return and its sharp variation across fields of study led to a large increase in the probability of receiving a Bachelor's degree, especially in STEM fields of study that are expected to yield higher financial returns. men and women.

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1 Introduction

This paper brings new evidence on the causal link between financial returns to education and higher education decisions. A central prediction of optimal human capital investment models is that a higher rate of return to education would lead to higher investment in education (Becker, 1964; Ben-Porath, 1967; Weiss, 1995). However, this key prediction is challenging to test because sharp and exogenous changes in the rate of returns to education rarely occur.

We address this challenge by using a unique setting of a sharp increase from lack of financial incentives to the market rate of return to education and variation across fields of study. Starting in the late 1990s, kibbutzim (plural of kibbutz) egalitarian communities in Israel shifted away from their decades-long policy of equal sharing of incomes to productivity-based wages that reflect the market rate of return (See Abramitzky, 2008, 2011, 2018; Abramitzky and Lavy, 2014). This pay reform in kibbutzim increased the relative returns to schooling across majors. Before the reform, all college majors had the same monetary return, given the kibbutzim's equal sharing practice. After the reform, college majors have heterogeneous returns, with majors such as STEM yielding higher returns than humanities, as in the rest of Israel. For the first time, the pay reform introduced financial considerations to the choice of field of study, allowing us to examine how a college major's choice responds to a dramatic increase in the rate of return to certain areas of study like science and engineering. This unique transition from nearly zero financial returns to education to market rates of return provides an empirical advantage in studying the link between financial incentives and higher education decisions without resting on strong assumptions about expectations and preferences.

Using newly-available administrative data from Israel's Central Bureau of Economics Research on the field of study of adult kibbutz members, we test how this pay reform influenced kibbutz members' college attainment and the choice of major during college. We use the staggered implementation of the reform over the years to implement a difference-in-differences analysis, comparing the proportion of BA degree attainment and the field of study of adult kibbutz members in kibbutzim that implemented the pay reform early ("treatment group") to adults in kibbutzim that reformed at a later date ("control group"), before and after the early reforms. We provide evidence that these two groups were indistinguishable in their observable background characteristics and their pre-reform academic outcomes.

We also use an alternative identification strategy that uses a non-kibbutz control group based on the population of young adults in Tel-Aviv, perhaps the most competitive labor market in the country with a concentration of highly skilled workers. This comparison group was completely unexposed to changes in kibbutzim, allowing us to overcome remaining potential confounders such as different trends and anticipation effects that might have occurred across early and late reformers. This comparison group also had much better pre-reform outcomes, allowing us to document the convergence process relative to a population that lived

in a non-egalitarian market economy.

We provide evidence consistent with the optimal human capital investment models. We find that young adults respond to the change in returns to schooling by increasing their BA degree attainment rate and by choosing fields of studies in college and university that are expected to yield higher financial returns, mainly STEM subjects. These effects are most evident for those who had pre-determined pre-requisite high school achievements, i.e., those who excelled in high school and were in the best position to take advantage of the increase in the returns after they graduated from high school.

Next, we study the effect on men and women separately. We find that both men and women are highly responsive to changes in financial returns to schooling, with some gender differences across the field of study. The pay reform induced men to major in engineering, physics, and computer science. An important finding is that young adult women were responsive to changes in returns. The pay reform induced women to major in STEM fields traditionally dominated by women, such as biology, and in STEM majors commonly traditionally attended by men, such as computer science. This evidence contrasts with recent studies that show low responsiveness by women to the increase in the relative market prices of majors with high returns to skills during the 80's (Gemici and Wiswall, 2014; Zafar, 2013).

Our findings highlight one potential disadvantage of egalitarian societies. Such societies offer low returns to schooling that discourage investment in human capital. We compare young adults who live in the kibbutz with young adults who live in Tel-Aviv. We show that before the pay reform, the proportion of BA holders in Tel-Aviv was significantly larger than in the kibbutzim. Both men and women kibbutz members chose majors with lower returns relative to residents of Tel - Aviv. After the pay reform, kibbutz members closed much of this gap within only a few years, indicating an increase in returns to schooling can matter a great deal.

Our paper relates to past and more recent literature on college attainment and choice of college major. Most of the empirical evidence indicates that financial consideration modestly influences the major decision (Altonji et al., 2012; Arcidiacono, 2004; Beffy et al., 2012; Athreya and Eberly, 2021; Turner, 2004; Wiswall and Zafar, 2015).[1] We contribute to this literature in several ways. First, while the earlier literature relies on various assumptions[2] to deal with the gap between

[1]For example, Wiswall and Zafar (2015) based on lab experimental variation in information about the returns to schooling, and Beffy et al. (2012) based on variation in the returns to schooling induced by business cycle fluctuations, find that variation in the return to schooling plays a small role in the choice of field of study in university. This evidence could suggest that the elasticity of demand for schooling with respect to the skill premium is small (Altonji and Zimmerman, 2019; Heckman and LaFontaine, 2010). While descriptive studies (Long et al., 2015; Montmarquette et al., 2002) found positive elasticities of choice of major with respect to expected earnings, recent studies suggest no causal response in the choice of major to variation in returns across field of study. See Altonji et al. (2016), for an extensive survey of studies on the relationship between the rate of return to schooling and the choice of field of study.

[2]For example Freeman (1971) assumed that students expectations is myopic. Willis and Rosen (1979) relies on rational expectations.

realized and expected earnings, more recent literature combines choice data and elicited expectations data.[3] In our analysis, given the kibbutzim's equal sharing practice, there is no need to map between realized and expected earnings. Second, unlike in other contexts in which there is always a positive college premium, even before the increase in the rate of return to education, in our setting there were no financial incentives to acquire a BA degree before the reform, because those with a high school diploma would earn just as much as those with a BA degree. Thus, we can estimate the full potential increase in the proportion of BA holders that comes from introducing financial incentives. Third, credit constraints can play an important role in higher education decisions; See Lochner and Monge-Naranjo (2012) for a review of this literature. Unfortunately, available administrative data typically does not allow identifying those constraints and who is affected. However, university tuition in Israel is low (3500$ per academic year), roughly equal to the average monthly salary of a person holding a B.A. degree. Furthermore, we provide evidence that there is no significant difference in the socioeconomic status and outside options between the control and treatment groups. Therefore, there is less concern about bias due to borrowing limitations and differences between or within the control and treatment groups.

Finally, we want to specifically address the contributions of the current paper relative to the previous work by Abramitzky and Lavy (2014). The current paper studies the same pay reforms in kibbutzim, and their effect on human capital investments, but the focus and research questions we address are different. Abramitzky and Lavy (2014) focuses on high school matriculation test scores of children who were about ten years away from entering the labor market (three years to complete high school, three years of military service and at least three years of completing an academic degree). This paper focuses on a very different sample, adults in their 20s who are on the verge of joining the labor market. The human capital outcomes in the current paper are also different as they relate to the decision regarding the field of study in university. With the growing literature on the choice of field of study, the natural experiment in kibbutzim that we exploit adds important evidence to this area of research.

The rest of the paper is structured as follows. Section 2 briefly presents the

[3]There is also an extensive literature, past and more recent, that focuses on the role of perceived financial and non-financial returns on college enrollment decisions. These studies often use surveys to elicit students' beliefs about the benefits of university education and about intention to engage in university schooling. For example, Boneva and Rauh (2017) finds based on a sample of secondary school students that perceived pecuniary and non-pecuniary benefits explain a large share of the variation in intentions to enroll in university education. The perceived non-pecuniary factors have a larger effect than pecuniary returns, in particular expected job satisfaction, parental approval, and perceptions about social life after secondary school are most important. Other recent examples include Arcidiacono et al. (2012); Manski (2004); Zafar (2013). Providing evidence from another context, Delavande and Zafar (2019) investigate the determinants of students' university choice in Pakistan, with a focus on monetary returns, nonpecuniary factors enjoyed at school, and financial constraints. They estimate a life-cycle model of students' university choice and find that expected earnings play a small (though statistically significant) role. Instead, nonpecuniary outcomes, such as the school's ideology, are the major determinants.

background of the kibbutzim, their traditional lifestyle, the causes of privatization and the structure of the pay reform, and the Israeli higher education system. Section 3 describes the data and restrictions we impose to define the study sample. Section 4 presents the empirical framework and identification strategy. Section 5 presents preliminary graphical results comparing the outcomes of kibbutzim that reformed early with kibbutzim that reformed late. Section 6 presents the results on the reform's effect on college attainment and choice of major and placebo estimates, and Section 7 concludes.

2 Background

2.1 The Kibbutz, the Pay Reform and its Impact on the Return to Education

The traditional kibbutzim are collective communities in Israel that have provided their members with a high degree of income equality for almost a century. The kibbutz operates under the ideology that can be described by a slogan popularized by Karl Marx "From each according to his ability, to each according to his needs". All income went into a common pool and was used to run the kibbutz, make investments, and guarantee mutual aid and responsibility between members. kibbutz members receive the same budget, regardless of their job or position. The salary of members who worked outside the kibbutz also goes into the shared pool. Today, there are around 120,000 kibbutz members across 265 kibbutzim, and they account for about 2.5% of the Jewish population in Israel. Less than 20% of them still maintain equal sharing. For an overview of the history and economics of kibbutzim, see Abramitzky (2018).[4] Unlike members of many other communally based living arrangements, kibbutz members were never at the margin of society. They have always interacted with the rest of the population and played an important role in Israeli society. This contrasts with many other communes, whose members have often been more marginal and isolated from the outside world.

Starting in the late 1990s kibbutzim shifted away from pay based on equal sharing to 'market-based' differential wages. In this new system, compensation was based on members' productivity. These pay reforms responded to a major financial crisis in the late 1980s.[5] Like many other businesses in Israel, Kibbutzim found themselves with huge debts they could not repay.[6] Even though some of the

[4]See also Abramitzky (2008, 2011); Near (1992, 1997).

[5]Beyond the financial crisis, a decline in the world price of cotton, a major source of income for kibbutzim, was another blow. The capital-intensive nature of kibbutz agriculture meant the high interest rates now required to borrow and invest in capital equipment were even more damaging. Kibbutzim were not alone in this. Many Israeli businesses went bankrupt, and the cooperative moshav villages were severely impacted as well. Kibbutzim were also hit by the fallout from the financial crisis in other sectors of the economy. The shares of the major Israeli banks crashed, and kibbutzim that had invested in them faced large losses.

[6]In the decade before the financial crisis, kibbutzim borrowed on a large scale. They found it

loans were rescheduled or erased, living conditions still fell substantially in many kibbutzim, leading to a higher exit rate during the late 1980s and early 1990s and to a discussion about major reforms of traditional kibbutz lifestyle and ideology. Kibbutzim that were hitless by the crisis and remained richer were much more likely to maintain equal sharing (Abramitzky, 2008, 2018)

We find that the reform's timing was not related to the economic and financial strength of the kibbutz in the years before the reforms. This is additional important evidence that the pay change's timing was not endogenously related to factors that affect or are related to kibbutz members' schooling decisions.[7] The similarity we later demonstrate between the characteristics and pre-reform outcomes in these two groups is not surprising given that the timing of the pay reform was mostly determined by non-confounding factors such as the age distribution of kibbutz members, the leadership skills, organizational affiliation, and how long it took the kibbutz to reach a consensus for the reform (that required a super majority).

After the reform, members' wages reflected market wages. For members who held jobs outside their kibbutzim, their wages was simply the wage they received from their employers. For those who worked inside the kibbutz, market wages were calculated based on non-kibbutz workers' wages in similar occupations and with similar education, skill, and experience.

Each kibbutz member paid a 'tax' that was deducted from their gross wages. These funds were used to maintain a safety net that supported older members and those with very low wages (i.e., minimum wage). This meant that a member whose earnings were above the safety net amount would pay a "community tax" for the communal services she received and for the mutual aid and assistance. The member would keep the rest of her earnings for herself and her family.

The change was from a near-zero rate of return to a post-reform return similar to the rest of Israel, estimated by various studies at about 8% per year of schooling Frish (2007).[8]

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easy to raise capital by obtaining high-interest loans, which remained cheap to repay given inflation was running as high as 400 percent per annum. They borrowed to expand their industries; they borrowed to enlarge members' rooms and facilitate the move of children back into their parents' homes; they borrowed to improve their dining halls, swimming pools, and theaters. However, eventually the Israeli government decided to take action to slow the rampant inflation. It put in place a comprehensive stabilization program, which succeeded in bringing inflation under control. This made the high nominal interest rates faced by kibbutzim high in real terms too, and left many kibbutzim, like many other businesses in Israel, overwhelmed by debt.

[7]We ran a regression where the dependent variable is an indicator of early (=1) versus late (=0) reform against an index of the economic strength of the kibbutz in 1995, while controlling for the kibbutz characteristics (age, size, average household size, all measured pre-reform). The parameter estimate on economic strength of the kibbutz is 0.127 (se=0.104), indicating no meaningful or statistically significant correlation between these two variables. Data limitation permitted using a sample of 44 out of 62 kibbutzim that reformed early or late. However, we find the missing data is not selective with respect to the timing of reform; the regression coefficient of missing data indicator on time of reform is -0.062 (0.115).

[8]However, we are aware of the possibility that the return to schooling increased by less than 8% per year of schooling the option to leave the kibbutz at any time meant that the pre-reform

Once the reform took place, we find that there are no differences in the return to schooling for kibbutz members (who worked outside the kibbutz) and non-kibbutz members (see Table A1 for Mincerian earning regressions for the year 2010 for reformed kibbutzim).[9] For example, the return to a BA degree over high school dropout is 52 percent for non-kibbutz labor market participants and 55 percent for kibbutz members who work outside the kibbutz. This pattern holds for both men and women.

2.2 The Higher Education System in Israel

In Israel, most students begin their post-high school academic studies between 21 and 24 because of 2-3 years of compulsory military service after high school. The higher education system in Israel includes nine research universities that confer bachelor's, master and doctoral degrees in all field of study (one of them confers only graduate and PhD degrees), and 62 colleges that grant only bachelor's degrees (some of these also give master degrees).[10] These colleges are similar to four-year community colleges. All the universities and most colleges are publicly funded; a Bachelor degree costs approximately 3,500-4,500 US$ per year (10%-15% of the average annual wage). The research universities have higher admission requirements than colleges in terms of Israeli matriculation exams-bagrut diploma and a psychometric (similar to the SAT) admission test. To receive a completed bagrut, it is necessary to pass a series of national exams. These exams cover core and elective subjects. Most academic colleges also require a bagrut, though some look at specific bagrut diploma components without requiring full certification. It is typically more difficult for a given field of study to be admitted to a university than college.

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return to education was higher than zero, and some members might have acquired education to improve their wages upon exit or leverage that for (potentially non-monetary) benefits within the kibbutz. It is important to note that kibbutzim developed various mechanisms that limited the attractiveness of the exit option. For example, bequests were not allowed, and members could not take their share of the assets of the kibbutz with them. Moreover, Abramitzky and Lavy (2014) show that exit rates during the period we study were relatively low and nearly identical in kibbutzim that reformed early and late.

[9] Our paper estimates the effect of the pay reform on expected and not actual earnings because the latter is not available in the administrative data we use in the protected lab. In fact, the Israeli Tax Authority that provides the earnings data does not have earnings information on kibbutz members who work inside the kibbutz because the kibbutz pays taxes as an aggregate economic unit based on the sum of income of all its members and therefore it does not report to the tax authority individual level income. However, over a quarter of kibbutz members work outside the kibbutz and their employer does report to the tax authority their incomes and therefore they appear in our data.

[10] A 1991 reform sharply increased the supply of postsecondary schooling in Israel by creating publicly funded regional and professional colleges.

3 Data

Our datasets are derived from the Ministry of the Interior population registry and are made available to us at a protected research lab at the Israeli Central Bureau of Statistics (CBS). These datasets contain an individual identifier, gender, date of birth, number of siblings, country of birth, parent's country of birth, and year of immigration (if relevant).

We merge this data with information from several additional administrative data sources. First are the 1995 and 2008 censuses, from which we obtain the information on a current residence that allows us to identify those who lived in kibbutzim in the relevant years. Data from the Ministry of Education provides us with student-level information on parental schooling, information on high school attendance, year of graduation, years of schooling, matriculation eligibility, and matriculation exams test scores. The high school data is available only for cohorts that graduated high school from 1995 onwards. From the National Council for Higher Education, we obtained administrative data files containing information about all individuals who obtained a BA or higher academic degree from any postsecondary institution in the country, including the field of study (one or two majors) and year of graduation. This data is available for all cohorts that we examine in this study. The Institution for the Research of the Kibbutz and the Cooperative Idea, University of Haifa, publishes reports about the dates in which the pay reform started in each kibbutz. This data allows us to sort the kibbutzim into early and late reformers.

Our sample includes 32 kibbutzim that reformed early, in 1998, 1999, and 29 that reformed later, in 2004, 2005. All members of these kibbutzim that were age 22-27 in 1995, 1996 (pre-reform) or 2001, 2002 (post-reform) form our first sample. In the empirical strategy section, we will explain the rationale for these sample selection rules.

We focus our analysis on two college-related outcomes: obtaining a B.A. diploma and the field of study. Based on CBS categorization of the field of study, we group the BA degrees to humanities, social sciences, and sciences. This division is our main focus on assessing the effect of the return to schooling on the choice of field of study. However, we also look into a more detailed classification of the field of study within these categories. In particular, in social science, we examine whether there was a stronger effect on higher-return fields such as economics, business, and law. In sciences, we estimated specific treatment effects on the following aggregates: (1) biology, chemistry, pre-health sciences, (2) STEM (math, engineering, physics, computer science, statistics), (3) computer science (as its category) (4) engineering. These more detailed definitions of fields of study are particularly interesting for discussing results by gender.

We also use data from the office of the Chief Economist in the Israeli Ministry of Finance that rank all field of study by expected average earnings in the labor market for BA holders.[11] These means are computed based on the population

[11]The ranking is based on unconditional mean earnings across majors without any controls for

of employees in Israel in 2013. We use this ranking as an alternative dependent variable (to the division of degrees to the categories described in the previous paragraph), which allows us to examine whether the pay reform induced young adults in kibbutzim to choose majors with higher wages.[12]

4 Empirical Strategy

Our empirical strategy takes advantage of the staggered implementation of the reforms in different kibbutzim. We choose the kibbutzim that implemented the pay reform in 1998 and 1999 as the treatment group and the kibbutzim that adopted it in 2004 and 2005 as the control group.[13] To estimate the effect of the pay reform on university schooling attainment and the choice of field of study, we compare the treatment group to the control group before and after the early reform (but before the late reform). We cannot rule out anticipation of reform in kibbutzim that reformed later. However, anticipation effects would attenuate our results because it would imply that students in the control group perceived some possible increase in the returns to education and increased their investment in schooling accordingly.

Unlike secondary education, there is some variability in the year of graduation in tertiary education among students who belonged to the same birth cohort. The rationale of the study design is to maximize the number of years we follow individuals in the most relevant age group at the most relevant time, subject to data limitations. Our sample consists of two "disjoint sets", affected and unaffected cohorts. The affected cohort includes individuals aged 22-27 in 2001,2002 (three years after the early reform and three years before the late reform). The unaffected cohort includes individuals of the same age in 1995, 1996 (three years before the early reform).[14] We follow each cohort for four years. We follow the affected cohort for four years from the earliest year in which we can identify the impact of the reform (3 years after the early reform, given that completing a B.A. takes at least three years) until the late reform. We follow the unaffected cohort for four years until the early reform. Figure 2 illustrates the sample structure, the timeline of the early and late reforms and the follow-up periods Both the control and treatment groups are untreated in the earliest period, and in the late period, the treatment group is fully treated, and the control group is completely untreated. Given that

differences in observables.

[12]Data from the Israel Tax Authority includes yearly payroll data and the number of months worked during the relevant year. Unfortunately, this information cannot be used to evaluate the effect of the reform on the wages since the salary is the same for all members of a kibbutz before the reform, while after the pay reform, the report from the Israel Tax Authority includes earnings data only for members of kibbutzim that are employed outside the kibbutz.

[13]We do not use kibbutzim that did not reform as a control group for kibbutzim that did because they hit less by the crisis and remained richer were much more likely to maintain equal sharing.

[14]The sample include only individuals who lived in the kibbutz prior the reform. Data from the 1995 and the 2008 Censuses allowed us to identify new entrants who do not live in the kibbutz before the reform and omit them from the sample.

we follow each cohort for four years, 22-27 is the most relevant age range where most Israelis earn their BA degree (which typically takes three years).[15] Indeed, Figure 1 suggests that only about 10% manage to earn a BA degree before the age of 24, and only about 10-15% earn their BA degree after the age of 32.

Using the pre-reform and post-reform cohorts, we implement a difference-in-differences methodology. As the first difference ('after' treatment), we compare individuals aged 22-27 in 2001 and 2002 in kibbutzim that reformed early (1998, 1999) vs late (2003, 2004). As the second difference ('before' treatment), we compare individuals aged 22-27 in 1995 and 1996 in kibbutzim that reform early vs late.

We estimate the following regression equation:

$$Y_{ikc} = \alpha_c + \beta_1(EarlyRef_k) + \beta_2(AffectedCohort_c X EarlyRef_k) + \epsilon_{ikc} \qquad (1)$$

where Y_{ikc} is the BA degree attainment of student i in kibbutz k in cohort c. α_c are cohort fixed effects (for individuals age 22-27 in 1995, 1996, and 2001, 2002). ($EarlyRef_k$) denotes a kibbutz that implemented the reform early, and the interaction of interest ($AffectedCohort_c X EarlyRef_k$) is whether the individuals belonged to the affected (younger) cohort and were members of a kibbutz that reformed early. Standard errors are adjusted for clustering at the kibbutz level.

We also run "controlled" specifications where we add kibbutz fixed effects and a vector of the individual's background characteristics. We, therefore, estimate the following model:

$$Y_{ikc} = \gamma_k + \alpha_c + \beta_1(AffectedCohort_c X EarlyRef_k) + \beta_2 X_{ikc} + \epsilon_{ikc} \qquad (2)$$

where γ_k are kibbutz fixed effects, α_c are cohort fixed effects, and X_{ikc} are individual i's characteristics including gender, number of siblings, a set of ethnic dummies (originate from Africa/Asia, Europe/America, the former Soviet Union (FSU), Ethiopia and other countries). All other variables are same as in equation (1).

The identifying assumption in the difference-in-differences strategy is that the exact timing of the reform is unrelated to the potential of academic outcomes. This assumption implies that older cohorts of early and late reformed kibbutzim should have had similar average college/university schooling outcomes. Since the early pay reforms were in 1998, for all individuals who completed their military service and are in their 20's, the exposure is a decreasing function of their date of birth. Particularly, all individuals aged 30 years or older were less likely to be affected by the reforms because they had fewer years to benefit from this investment once the pay reforms began. Hence, the effect of the pay reform should be closer to 0 for cohorts that are 30 years or older around the date of the reform and increasing for younger cohorts. The appendix includes estimation results from a sample that includes somewhat older cohorts, but we cannot include much older

[15] Formally, by chossing this age range we maximize the area of a simple transformation that for each bin (age) of histogram (a) in Figure 1 calculate the sum of four consecutive bins. Each bin in the new form represents the probability to graduate in the next 4 years (follow up period).

cohorts for the practical reasons discussed earlier. Therefore, the identification strategy is based on comparing college outcomes between individuals in the most relevant age group at the most relevant time from kibbutzim that reformed early and comparison from respective groups in kibbutzim that reformed late. These comparisons yield a difference-in-differences estimate that can be interpreted as the causal effect of the reform, under the assumption that in the absence of the reform, the increase in college schooling would not have been systematically different for individuals from early- and late-reforming kibbutzim. We provide three related pieces of evidence in support of this assumption.

First, we show that individuals in the treatment and control groups are similar in terms of their mean background characteristics and their pre-reform mean college schooling outcomes. Here we test directly whether the individuals 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 individuals' pre-determined variables such as age and ethnicity. We perform these tests for pre-reform cohorts (individuals aged 22-27 in 1995, 1996) and post-reform cohorts (individuals aged 22-27 in 2001, 2002). We look at post-reform cohorts here to show that the types of people in early- and late-reformed kibbutzim look similar even after the early reform. For the pre-reform cohorts, we also check whether their college attainment outcomes are similar.

Table 1 provides evidence on the balancing tests and presents the mean individual and kibbutz level characteristics for the pre and post samples by treatment status. Columns 1, 2, and 3 present pre-reform means of the treatment and control groups and the difference between them, respectively. Columns 5, 6, and 7 present post-reform treatment and control group means and the difference between them, respectively. In panel A we show that student background characteristics are 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 number of siblings is very similar in control and treatment, with 2.7 children per family. The number of siblings presented in column 3 is -0.002 (p-val=0.983), and the respective difference for the post-reform cohorts presented in column 7 is 0.034 (p-val=0.736). These differences are not statistically different from zero and are very small relative to the respective means. The differences in proportion ethnicity Africa/Asia and ethnicity Europe/America are very small, -0.001 (p-val=0.972) and 0.016 (p-val=0.689) respectively; in the post-period they are -0.014 (p-val=0.436) and 0.049 (p-val=0.102) respectively. The similar proportion of these two important ethnic groups in the treatment and control groups suggest that students in the two groups had similar academic potential, both before and after the pay reform, because these two characteristics are strong predictors of socio-economic status. Similarly, small and non-significant differences are also seen in all the other background characteristics. We show balancing tests for kibbutz level characteristics (socioeconomic index, organizational affiliation, and population) in panel B. The estimates show that the only disparities seen are in terms of geography and they are small. The control group is closer to Tel Aviv (T-C=27 kilometers) while the

treatment group is more peripheral. Therefore, we view the results presented in Table 1 as an indication of good balancing, meaning that, within cohorts, the treatment and control groups are indistinguishable in their observables.

While Table 1 looks at pre-determined variables, Table 2 provides a first look at how the reforms changed the outcomes. This table shows no significant difference in the proportion of BA degree attainment between early- and late-reformed kibbutzim among pre-reform cohorts. In addition, there are no significant differences between the two groups in the proportion of BA degrees by field (humanities, social science and science studies). These suggest similarities between the early- and late-reformed kibbutzim in their pre-reform outcomes, implying that kibbutzim reformed late are a valid control group for kibbutzim that reformed early. The last four columns, Table 2, show post-reform outcomes and thus already show results rather than balancing. Overall, Table 1 and Table 2 show that while the pre-and post-reform cohorts have similar characteristics and similar pre-reform outcomes, after the reform, there is an increase in BA attainment, especially in the sciences, by members of early-reformed kibbutzim.

Next, we show in Table 3 that early- and late-reform kibbutzim were on the same time trend of educational college outcomes. The unit of observation in this analysis is a kibbutz-year. In Panel A, we estimate a linear time trend model, testing whether there is an interaction of the linear trend with an early reformed kibbutz (treatment). In Panel B, we estimate a model with a series of cohort dummies and include an interaction of each of these cohort dummies with the treatment indicator in the regression. The table suggests a secular positive time trend attainment of BA degrees, with a slope of 0.003 that is significant in both models. This positive trend is also seen in Figure 3, which suggests that the trend was positive. However, the interaction term between the trend slope and the treatment status (panel A) is small and not significantly different from zero, suggesting that the control and treatment groups were on the same time trend before the pay reform was implemented. The estimates from the specification that replaces the linear time trend with year dummies, presented in panel B, lead to the same conclusion of no pre-reform time trends.

5 Preliminary Results: Graphical Representation of the Evidence

Figures 4 and 5 illustrate the paper's main finding that the pay reform affected BA degree attainment. Figure 4 shows the proportion of individuals aged 22-27 who received a BA degree for two samples: kibbutzim that reformed early and kibbutzim that reformed late. The means for these samples are presented for 1990, 1995, 2001 and 2007. Comparing at first early and late reform kibbutzim before the pay reform took place, the rates of receiving a BA degree in 1990 and 1995 are similar. By 2001, the pay reform took place in the early-reformed kibbutzim group. Consistent with the increase in the return to schooling, by 2001, early-

reformed kibbutzim opened a gap of 4 percentage points in BA degree attainment. This gap is eliminated in 2007, once the reform took place in the late reformed kibbutzim.

Figure 5 further investigates these results by providing a graphical representation of the estimates of the leads and lags of the impact of the pay reform obtained via the estimation of the mean differences in the proportion receiving a BA degree between kibbutzim that reformed early (treatment group) and late (control group). The first red vertical line denotes the time of the early reform, and the second red line denotes the time of the late reform. The horizontal axis measures the years since the early reform. None of the coefficients in the years leading to the reform are significant, suggesting that the evolution of BA attainment was similar before the early implementation of the pay reform. Following the early reform, individuals in early-reformed kibbutzim open a gap relative to individuals in late-reformed kibbutzim, and this gap disappears after late reform.

6 Results

6.1 Pre and Post Cross Section Regressions

Table 4 shows that the pay reform induced kibbutz members to obtain more BA degrees, mainly in fields with higher expected earnings. Panel A reports treatment-control differences in outcomes before and after the reform. The pre-reform cross section regression estimates show no difference in BA degree attainment between individuals in early- and late-reformed kibbutzim (this difference is -0.005 with a standard error 0.011). Notably, there is no significant difference between individuals in early- and late-reformed kibbutzim in any field of study: namely -0.005 (se=0.005) in humanities, 0.006 (se=0.007) in social sciences, and -0.007 (se=0.007) in sciences. Moreover, within these fields, there are no significant differences across subfields (for example, 0.000 (se=0.003) in computer science and -0.002 (se=0.004) in engineering).

In contrast, the post-reform cross-section regression estimates show significant improvements in outcomes of individuals in early-reformed kibbutzim relative to those in late-reformed kibbutzim. Individuals from kibbutzim that reformed early had a higher overall BA attainment (difference of 0.029 with a standard error of 0.011), and opened a significant gap in sciences (estimate of 0.031 (se=0.007)). Each sub-field in sciences experienced a statistically significant expansion, for example, in computer science 0.014 (se=0.003) and in engineering 0.007 (se=0.004). In humanities and social sciences, in contrast, there remained no difference between early and late-reformed kibbutzim (-0.001 (se=0.005) in humanities and -0.002 (se=0.007) in social sciences).

6.2 Simple and Controlled Difference-in-Differences (DID) Regressions

Panel B of Table 4 presents simple, controlled difference-in-differences estimates. We find a positive effect of the pay reform on BA degree completion, especially in STEM subjects. Focusing on the controlled difference-in-differences estimation, the first column shows that the BA degree completion rate is up by 3.3 percentage points (se=0.016). Given that the post-reform control mean was 0.082, the pay reform increases the BA degree completion rate by 40%.[16] Column 2 shows that there is no effect in humanity majors and column 3 shows a very small and insignificant decrease of 0.9 percentage points (coefficient= -0.009, se=0.010) in social sciences majors. Column 5 shows that the BA degree completion rate in sciences is up by 3.8 percentage points (se=0.010), from a post-reform control mean of 0.022, meaning the increase in BA degree completion is driven by the sciences. The difference-in-differences treatment estimates within sciences suggest that the effect is present in a wide range of subjects, including biology and chemistry, computer science and engineering. Figure 7 presents the difference-in-differences estimates and confidence intervals by field of study, illustrating the effect on Science/STEM subjects and the lack of effect on other majors.

Panel B also of Table 4 also presents a set of results controlling for other reforms that took place in kibbutzim during the 1990s.[17] Controlling for other reforms allows us to rule out the effect of other mechanisms besides the pay reform that may have changed social norms in the kibbutzim. We found that our estimated effect of the pay reform is not affected by these other social reforms. These findings suggest that the pay reform affected above and beyond other reforms.

Our finding of over 3 percent points expansion in BA degree certification, mostly in STEM subjects, reflecting an expansion at the extensive and the intensive margin as well People who would otherwise have no BA might now complete a BA degree in STEM subjects, a pure extensive margin expansion. But another feasible scenario is that some have moved from no BA certification to BA certification in non-STEM subjects in parallel to people switching from BA in non-STEM to BA in STEM. A third possibility is a combination of the two above scenarios. We cannot distinguish clearly between these three possibilities.

The positive and significant treatment effect estimates are similar in the simple and controlled DID, resulting from the treatment-control similarity in background characteristics and pre-reform outcomes. Indeed, the estimates from the cross-section treatment-control comparison from the period after the early reform,

[16] A comparison of this estimated effect to the estimates of the effect of several high school interventions on post-secondary schooling suggests that an increase in the rate of return to schooling is a much more 'powerful' channel to induce individuals to invest in higher education. See evidence of effect of pay for performance incentives to teachers (Lavy, 2020), free school choice (Lavy, 2021) and remedial education (Lavy et al., 2021).

[17] During the 1990s, kibbutzim implemented additional social reforms such as paying for electricity, laundry, and dining. These reforms came out of a need to increase efficiency and reduce misuse. Members were required to pay for those services, but in return, the kibbutz increased the budget of its members.

presented in the previous section, are similar to the DID estimates.

6.3 Results Based on an Alternative Control Group

The interpretation of the results presented above as causal effects hinges on the evidence we presented that the timing of reform did not depend on potentially confounding factors, that early and late reformed kibbutzim had similar pre-reform trends in outcomes, that anticipation effect in late reform kibbutzim does not bias downward our treatment effect estimates, and that there were no unobserved factors that can explain our findings. In this section, we present evidence based on an alternative identification strategy. It is still a difference-in-differences estimation, but the control group is not derived from the staggered implementation of the pay reform. Rather, we use an alternative non-kibbutz control group based on the population of young adults in Tel-Aviv, perhaps the most competitive labor market in the country with a concentration of highly skilled workers. We chose this control group because it has higher pre-reform outcomes (see Panel A in Table 5 and Table A4 in the online appendix). So the estimates relative to this group represent the closure of pre-reform gaps, which to some extent can be viewed as convergence towards the mean outcomes of the general population in the competitive market economy. However, as can be seen from the estimates presented in columns 3-4 of Table 3, the time trend of Tel-Aviv is not different from that of early reformed kibbutzim.

Moreover, the evidence based on Tel-Aviv as a control group cannot be driven by an anticipation effect. In a forward-looking environment, young adults' choices in late reformed kibbutzim could start to shift in anticipation of the change, which would bias our estimates downwards. Such an anticipation effect is not a concern when using Tel-Aviv as a comparison group. Using Tel-Aviv as a control group also alleviates the concern that the timing of reform can cause selection bias.

Table 5 presents the estimates based on this alternative control group in the same format as Table 4. We get similar results in this alternative experiment even though this control group had much better pre-reform outcomes.[18] For example, the treatment estimate for getting a BA degree in any field is 0.035, and in any field, in science, it is 0.030. The respective estimates in Table 4 are 0.033 and 0.038. Like the evidence presented in Table 4, Table 5 also shows no effect on humanities and social sciences.

Therefore, unlike the first set of estimates based on late reforming kibbutzim as a control group, which captured only post-reform differences with complete pre-reform balancing, the results using Tel-Aviv as a control group reflect a partial narrowing of the pre-reform gap between treatment and control.[19] These divergent patterns in the difference-in-differences estimates indicate that our treatment

[18]The results on balancing between treatment and Tel-Aviv as a control are presented in online appendix table A3.

[19]The pre-reform difference in BA attainment between Tel-Aviv and early reform kibbutzim was 0.066 and in the post-reform period it declined to 0.029, the difference is exactly the simple difference-in-differences estimates of 0.037 (see estimates in column 1 of Table 5).

estimates are not driven by convergence to the mean following random shocks to outcomes in the treated kibbutzim.

6.4 Treatment Effect Estimates by Gender

In Tables 6 and 7, we present results by gender. Looking at evidence separately for men and women is important for several reasons. First, there is a large gender gap in earnings. We can examine how this earning inequality is related to differential responses by gender to changes in the financial return to schooling. Secondly, a growing literature suggests that women shy away or are deterred from occupations traditionally dominated by men, such as STEM fields (Arcidiacono et al., 2012; Bronson, 2014; Gemici and Wiswall, 2014; Kugler et al., 2021).

Table 6 shows that women, not just men, are highly responsive to changes in financial returns to schooling, with some gender differences across the field of study choice.[20] The estimated effect on BA attainment is 0.033 for men and 0.034 for women. The gain for men is against a control mean of 0.039 in the post-reform cohorts, and for women, it is against a mean of 0.132 in the post-reform cohort. Therefore, the treatment effect is much larger for men (more than doubling the rate) than for women (a 26 percent increase). The gain among men is mostly in science (0.024), mostly in STEM, with a small but statistically insignificant increase in humanities (and no change in social sciences). For women, the pattern is somewhat different: there is a 0.055 increase in science majors coupled with a decrease of 0.020 in social science, mainly in economics and law (and no change in humanities). Within science majors, the increase is concentrated in biology, chemistry and pre-medical studies, but it is also evident in expansion in STEM subjects, mainly computer science. There is no effect in engineering. Women respond to the pay reform beyond simply expanding university schooling at an extensive margin. Instead, they increase their choice of some subjects (those we show below to be more financially rewarding) at the expense of the field of studies with a lower monetary rate of return. The attendance effects are larger for men, but the choice of-major effects are larger for women. Figure 8 provides a graphical representation of treatment-control differences by fields of study, pre-and post-reform. It is interesting to note that an earlier paper Abramitzky and Lavy (2014) found that, during high school, female kibbutz students were not very responsive to the reform. In contrast, we find that young adult women were highly responsive to the increase in financial returns to schooling.

The evidence in Table A5 for men and women, obtained using Tel-Aviv as a control group, is very similar to the results obtained using the control group of late-reform kibbutzim presented in Table 6. The estimated effect on the attainment of BA degree among men is 0.030 (in comparison to 0.033 in Table 6), and it is 0.043 among women (in comparison to 0.034 in Table 6). Three things are worth noting.

[20] We note that the treatment-control samples by gender are also well balanced in terms of background characteristics and, for the pre-treatment cohort, also in terms of outcomes. These balancing tables are presented in online appendix Table A2.

First, the effect on women is now estimated more precisely. Second, the difference-in-differences for both men and women reflect a decline of an initial gap for each of the genders between Tel-Aviv and early reformed kibbutzim. This is the same pattern observed in for the whole sample (Table 5). Third, the estimates using Tel Aviv as a control group (Table A5) are consistent with the estimates using late-reformed kibbutzim as a control group (Table 6) in showing an increase among women in BA degrees in sciences, especially in biology and computer sciences, and zero effect in engineering.

6.5 Treatment Effect Estimates on Actual and Expected Wages

In Figure 6, we provide a graphical representation of the estimates of the leads and lags of the impact of the pay reform obtained via the estimation of the mean differences in earnings in 2014 between kibbutzim that reformed early (treatment group that includes kibbutzim that reformed in 1998 and 1999) and late (control group that includes kibbutzim that reformed in 2004 and 2005). The sample consists of individuals aged 22-27 each year, from five years before the early reform until ten years after it. We note that earnings data is available only to kibbutz members that work for an employer (private or public) outside the kibbutz. By 2014 about 46 percent of kibbutz employment is outside the kibbutz. Working outside the kibbutz is a choice that will likely lead to differences in characteristics between those working in the kibbutz and those outside the kibbutz. But in Figure 6, we do not compare these two groups within the kibbutz. Rather, we compare those that work outside the kibbutz in the treatment to the same group in the control kibbutzim. Importantly, as will be discussed below, the evolution of earnings of these two groups was similar before the early implementation of the pay reform. Admittedly, the characteristics of those working outside the kibbutz may have changed, but this is a result of the reform, and we do not view it as a selection bias. Rather it is an effect of the reform.

The first red vertical line in Figure 6 denotes the early reform time, and the second red line denotes the time of the late reform. The horizontal axis measures the years since the early reform. The pattern seen in Figure 6 on earnings is very similar to that in Figure 5 on BA attainment (although recall that these results on earnings are only based on kibbutz members who worked outside their kibbutz). None of the coefficients in the years leading to the reform are significant, suggesting that the evolution of earnings was similar before the early implementation of the pay reform. The pre-reform wage differences between the treatment and control group are small, and in all the lagged years and the year of the reform, they are not statistically different from zero. Following the early reform, individuals in early reformed kibbutzim open a gap relative to individuals in late-reformed kibbutzim. The gap is positive and statistically significant in the lead years 2-5, falls to zero after the late reform and remains low and not statistically significant until lead year ten. At its peak, the wage gap in 2014 is 2,000 NIS, about 12 percent of the control group's mean.

We next map fields of studies into expected earnings and show a positive ef-

fect of the reform on majors with higher expected earnings.[21,22] Table 7 shows that the pay reform effect is skewed towards BA degrees in fields with higher expected earnings. We define three different measures of expected wages. The first is actually expected wages in Israeli Shekels, the second is a dummy indicator for fields of study with above 75th percentile wages and the third a dummy indicator for fields of study with above-median wages. We present pre and post cross section regression estimates for each of these outcomes and simple and controlled difference-in-differences estimates. We show evidence based on the full sample and for men and women separately.

Focusing on the difference-in-differences estimates, the pay reform expanded BA degree attainment in fields of study in the top quartile of the wage distribution, both for men and women. The likelihood of obtaining an academic degree in fields with expected wages in the upper quartile of the wage distribution, presented in columns 4-6, increased by 2.0 percent points for men and by 1.1 percent points for women.

In columns 1-3, we present the estimates on expected wages as the dependent variable. The effect in the full sample suggests that the pay reform increased expected wages by 309.4 NIS a month, about $100. This gain accounts for about 4 percent of monthly expected earnings. The gain for men is 377.5 NIS, and for women, it is only marginally lower at 265.6 NIS. Because mean expected wages for women are lower,[23] this absolute increase in expected earnings translates to a higher proportional increase for women.[24]

In Table A6, we present similar evidence based on using Tel-Aviv as a control group. The results are very similar to those shown in Table 7. For example, the likelihood of obtaining an academic degree in fields with expected wages above the median of the wage distribution is 0.040 versus 0.032 when using later reformers as a control group. The two estimates are not statistically different. The effect

[21]Ideally, we would also like to test whether the increase in education attainment translates into differences in earnings for these same individuals. Unfortunately, the administrative data on earnings does not include information on earnings of kibbutz members who work inside their kibbutz.

[22]The large variation in earnings in Israel by post-secondary field of study are not unusual.Kirkeboen et al. (2016) examine the labor-market payoffs to post-secondary education in Norway, including field and institution of study, and show that different fields of study have substantially different labor-market payoffs, even after accounting for institution and peer quality. The payoffs they estimate to field of study are much larger than the effect on earnings of attending a more selective university.

[23]Our data on expected wage is not available by gender. However, related evidence from the Labor Force Survey 2017 suggest that a higher proportion of women work less than full time which lowers expected earnings.

[24]The predicted earning gains may overstate actual earnings gains, given that the predictions come from simple uncontrolled earning means by major and thus are not corrected for selection bias from higher ability students sorting into more lucrative fields. However, if this selection is similar for kibbutz and non-kibbutz individuals than the predicted wages are accurate predictions. We do not have any reason to believe the selection should be different for these two groups. We have shown in Table A1 in the online appendix that the rate of return to schooling, by different level of education, are practically identical for kibbutz and non-kibbutz individuals, which is an indication that selection based on unobservebles to different fields of study is not different.

on expected earnings is 402.0 NIS, higher but not significantly different from 309.4 NIS (Table 7).

6.6 Validation of the Causal Interpretation and Robustness Checks

Next, we show evidence of two sets of placebo treatment effects on pre-reform outcomes measured before the reform was implemented. First, since data on matriculation high school outcomes is only available for the post-reform cohort, we can only estimate treatment effect based on post-reform cross-section regression. This may be less of a limitation than initially perceived because we have shown that the pre-reform treatment-control differences are practically zero. We use four ends of high school outcomes: receiving a matriculation diploma, number of matriculation credit units, and matriculation units in English and math. These results are presented in Table 8 for the full sample (columns 1-3), for men (columns 4-6) and women (columns 7-9). All 12 controlled cross-section estimates presented in columns 3, 6, and 9 are small and not statistically different from zero.

Second, In Table 9, we present evidence from a placebo test. We use a difference-in-differences model to compare two older cohorts less likely to be affected by the reform, namely individuals aged 22-27 in 1989, 1990 and 1995, 1996. We note that the simple and the controlled difference-in-differences estimates are similar, again reaffirming that the control and treatment groups are balanced in characteristics even in older cohorts. This result suggests no differential trends in background characteristics of the treatment and the control groups, in line with the evidence we have shown in the previous section of no differential trends in outcomes. We also note that there are only small differences in the cohort leading to the reform. The differences on BA degree attainment in any field is 0.004 (se=0.014) in comparison to 0.033 (se=0.016) in Table 4 panel B. The estimate on BA degree attainment in science fields is 0.005 (se=0.009) in comparison to 0.038 (se=0.010) in Table 4 panel B. The two estimates in each pair are either marginally statistically different (first pair) or statistically different (second pair).

We perform another validation check by estimating the effect for two sub-samples, those in our sample who earned a matriculation diploma and those who did not. Since a matriculation diploma is a pre-requisite for admission to universities, we expect the effect presented in Table 4 to originate from the sub-sample of those who hold a matriculation diploma. We present these heterogeneity results in Table 10. The sample is split almost evenly between those who have and those who do not have a matriculation diploma. The estimates show that all the effect on university degree attainment comes from those who attained a matriculation diploma. For example, the effect on BA attainment in the sample of matriculation diploma holders is 0.057 (se=0.030) and it is only 0.019 (se=0.014) in the sample without a matriculation diploma. The difference between the two groups in effect on expected earnings is even more striking: 1078 NIS (se=397) versus 142 NIS (se=110). Similar evidence is obtained when using Tel-Aviv as a control group (presented in Table A7 in the online appendix).

Another informative robustness check originates from the fact that most of the

effect is on science majors. In Israel, admission is for a specific department, not for the university. Admission to science-related departments typically requires high-level math in high school. The high school matriculation program is offered at basic, intermediate, and advanced levels. The latter is a pre-requisite for admission to engineering and computer science programs at all universities and most colleges that offer these programs. Table 11 presents results for the two subsamples defined by math level in high school. We group the basic and intermediate math levels and keep students with advanced math in a second sample. As expected, Table 11 shows that most of the effect originates from students in the advanced math sample. For example, the effect on BA attainment in the advanced math sample is 0.182 (se=0.060), while it is only 0.016 (se=0.019) in the basic and intermediate math sample. The effect on BA attainment in science in the advanced math sample is 0.194 (se=0.048) versus 0.021 (se=0.012) in the basic and intermediate math sample. The effect on expected earnings in the two sub-samples is 3238 NIS (se=397) versus 257 NIS (se=202).[25] Similar evidence is obtained when using Tel-Aviv as a control group (presented in Table A8 in the online appendix).

The analysis so far was based on a sample that included individuals aged 22-27. For robustness, in Tables A9-A14 in the online appendix, we replicate all our results reported above for the sample of individuals aged 23-28. These tables present treatment effect estimates for the full sample and by gender, balancing tests for the full sample and gender. Overall, the treatment estimates obtained from this alternative age group are similar to those reported above based on the 22-27 age group.

7 Conclusion

This paper provides quasi-experimental evidence on the effect of changes in the skill premium on young adults' propensity to obtain a BA degree from a university and on their choice of field of study. Our empirical setting provides a compelling natural experiment with a large discrete increase in the financial return to schooling, from a very low rate of return to the level of the market-wide rate of 8-9 percent return to a year of schooling. Thus, our setting provides a rare opportunity to study individuals who grew up in a more egalitarian society than the US, and who suddenly faced an increase in the financial returns to schooling as young adults for the first time. Furthermore, this setting also allows us to study how women who grew up in an environment that strives for both income and gender equality respond to changes in the returns to schooling.

Our findings suggest that the response of skill investment to the returns to schooling may vary across societies. Altonji et al. (2008) summarize this evi-

[25]The evidence from these robustness checks also provide the useful insight that most of the reform compliers were not marginal in terms of ability as indicated by their very high achievements in high schools. We therefore can safely assume that for this compliers group, college completion rate and obtaining a BA degree is not much lower than college enrollment rate. The implication is that program affected equally college enrollment and college completion rates.

dence in the US context as "the anemic response of skill investment to skill premium growth" and concluded that "the earnings premium for skilled labor has increased dramatically in recent decades. Yet, Americans are not acquiring significantly greater skills in response to this change."[26] In the context of low initial returns, our findings show a large response to changes in the return to schooling, both in terms of attainment of BA university degrees and in terms of choice of field of study. The response is mainly driven by individuals who had the high school pre-requisites for admission to universities and STEM fields. Both men and women shifted their choice of field of study towards majors with higher expected earnings. The effect on earnings is not substantially different by gender, so we do not expect it to substantially change the expected gender gap in earnings. However, more work on the occupation choice after schooling is needed to assess the effect on the actual gender earning gap later in life.

In the context of people who grew up under equal sharing and who faced very low returns to education, we find that people are responsive in their choices of majors to changes in the return to schooling, and that women are not less responsive than men and may switch to typically men-dominated majors that are expected to yield higher earnings. This vigorous response can perhaps be explained by the starting point of zero financial return to schooling in the pre-reform period. During this period, most people who had the pre-requisites to be admitted to STEM subjects might have preferred to enroll in less financially rewarding majors or not go to college at all. However, once returns increased, members who had already satisfied the pre-requisites to enroll in STEM subjects, namely those who had studied math in high school at the highest level, enrolled in STEM subjects in large numbers. This could be one explanation for the difference between our findings and the 'anemic' response to the increase in the skill premium in the US, where most people with such potential had already been engaged in STEM majors even before the increase in skill premium.

A natural question that arises is the external validity of our findings. The context is surely different from a typical environment like the US due to the equal sharing and commune lifestyle that preceded the pay reform. Moreover, this structural change manifests itself as a sharp and large change in the return to schooling that is rarely observed in modern times. Nevertheless, we believe that our findings are informative given recent events such as the transition from centrally planned to market economies following the collapse of the Soviet Union (Brainerd, 1998),[27] the abolition of village collectives in China in the 1980s, the

[26] Altonji et al. (2012) show that the observed increase in schooling investment in recent decades is fully explained by the increase in parental education.

[27] Several studies document the increase in the return to schooling in Central and Eastern European (CEE) countries following the fall of the Iron Curtain. Fleisher et al. (2005), review this literature and conclude that returns to education increased markedly during the transition, both in CEE economies and in Russia. Orazem and Vodopivec (1997), compare the wages of different skill groups in Slovenia before and after the collapse of communism, and find that returns to schooling increased sharply during the early phases of the transition. Similar results are obtained by Münich et al. (2005), who study the case of the Czech Republic, Andrén et al. (2005), for Romania and Flabbi et al. (2008), for several CEE countries.

labor market liberalization in Vietnam in the 1980s (Moock et al., 2003; Svejnar, 1999), and the effect of skill biased technical change that sharply increased the skill premium in the United States and many other developed countries over the past decades (see the survey by Autor et al. (2008)).

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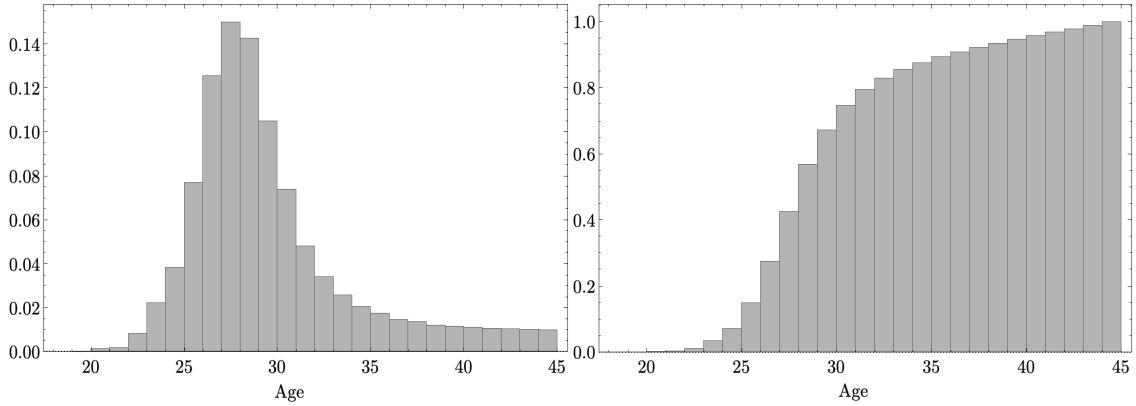
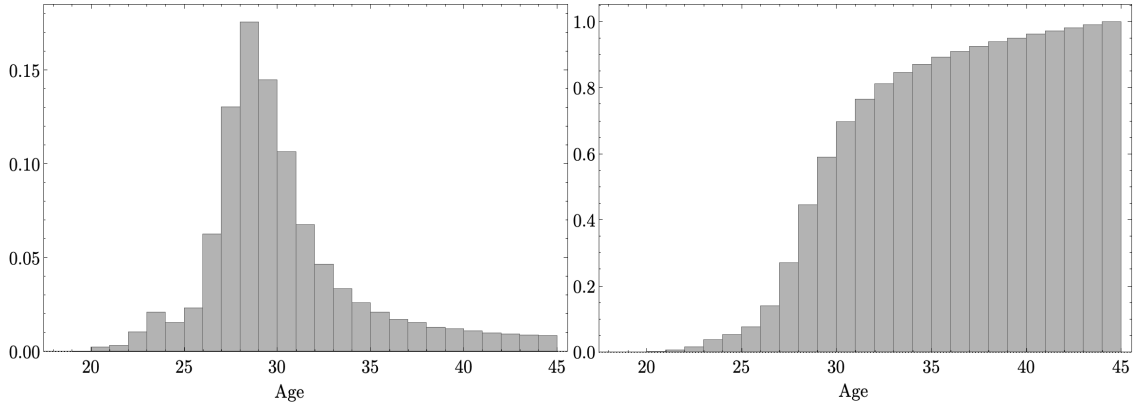
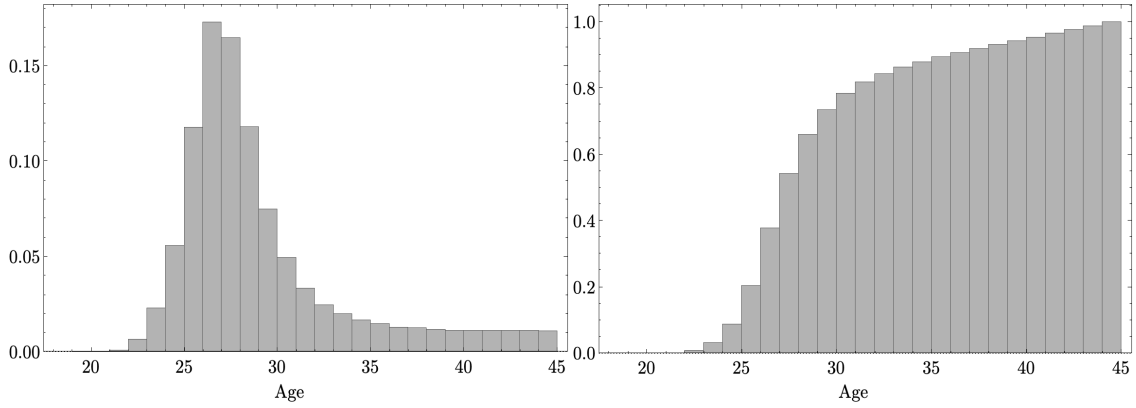


(a) Full Sample PDF (b) Full Sample CDF



(c) Male PDF (d) Male CDF



(e) Female PDF (f) Female CDF

Figure 1: Distribution of BA Attainment By Age at Graduation

Note–Figure 1 presents a PDF and CDF of the age at graduation for 1990-2015 Israeli Jews aged 18-45. Panel (a) & (b) present the distribution for the full sample (n=502,996), Panel (c) & (d) for Male (n=215,538) and Panel (e) & (f) for Female (n=287,458).
Source: Central Bureau of Statistics, Israel.

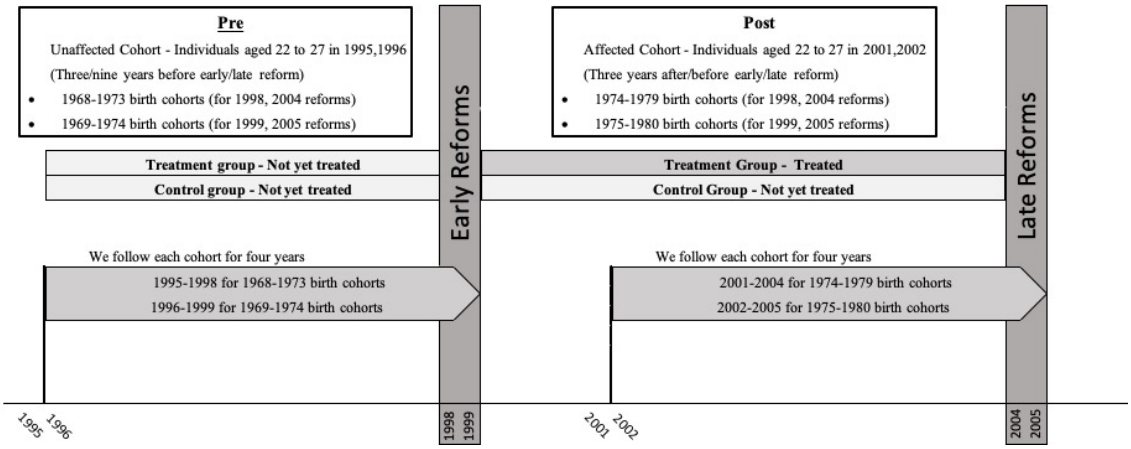


Figure 2: Timeline of the Pay Reform

Note–This figure presents the timeline of the pay reform for the selected treatment and control groups and the affected and unaffected cohorts relative to the time of the early and late reform. The treatment group includes kibbutzim that reformed early (1998, 1999) and the control group includes kibbutzim that reformed late (2004, 2005). The pre-reform cohort includes individuals who are aged 22-27 three years before the pay reform of the treatment group and nine years before the pay reform of the control group (aged 22-27 in 1995 and 1996 for the 1998, 2004 and 1999, 2005 reform, respectively). The post-reform cohort includes individuals who are aged 22-27 three years after the reform of the treatment group and three years before the reform of the control group (aged 22-27 in 2001 and 2002 for the 1998, 2004 and 1999, 2005 reform, respectively). In order to calculate the outcomes of higher education, we follow each cohort for four years. For the pre-reform cohort (aged 22-27 in 1995, 1996) we follow four years until 1998, 1999 (at the end of the followup period they were aged 26-31). For the post-reform cohort (aged 22-27 in 2001, 2002) we follow four years until 2004, 2005.

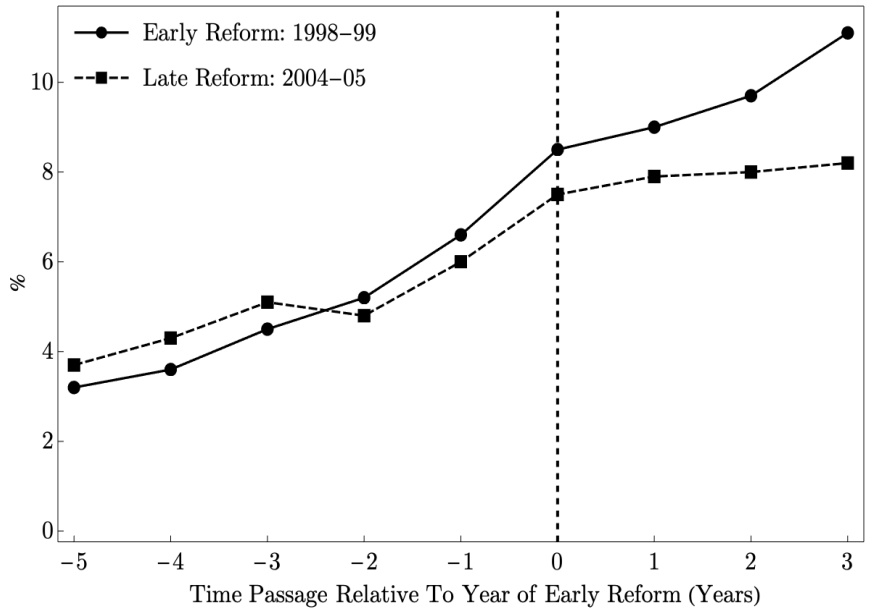


Figure 3: Pre- and Post-Reform Time Trend of BA Degree Attainment Rate. Treatment vs Control Group.

Note–BA degree attainment rate for 22-27-year-old. The vertical line represents the year of the early reform.

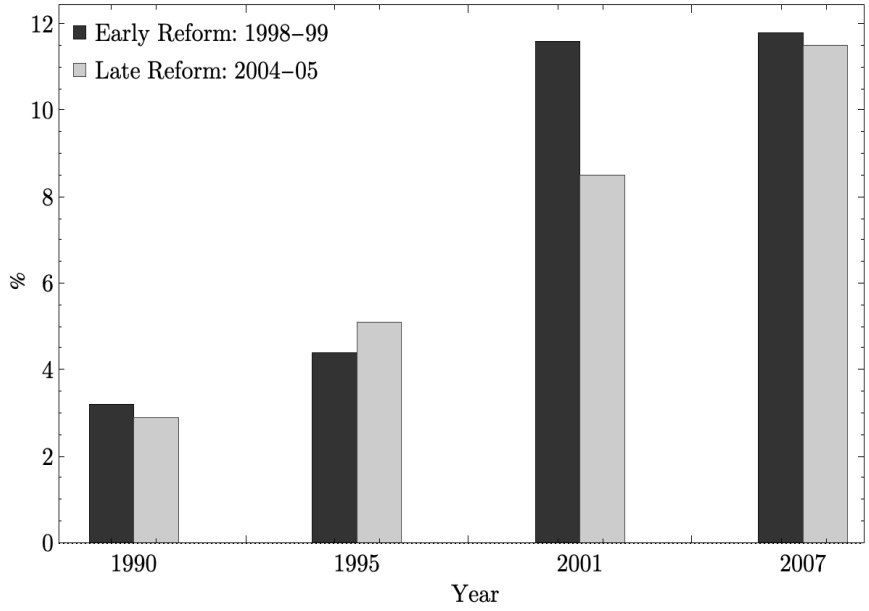


Figure 4: Proportion Receiving BA Degree

Note–This figure presents the proportion of BA degree recipients of individuals who were aged 22-27 in four different years (before the early reform: 1990, 1995, after the early reform and before the late reform: 2001, after the late reform: 2007) for treatment group (early reform: 1998, 1999) and control group (late reform: 2004, 2005). In order to calculate the proportion, we follow each cohort for four years until the age of 26-31 and examine how many received a BA degree during the follow-up period.

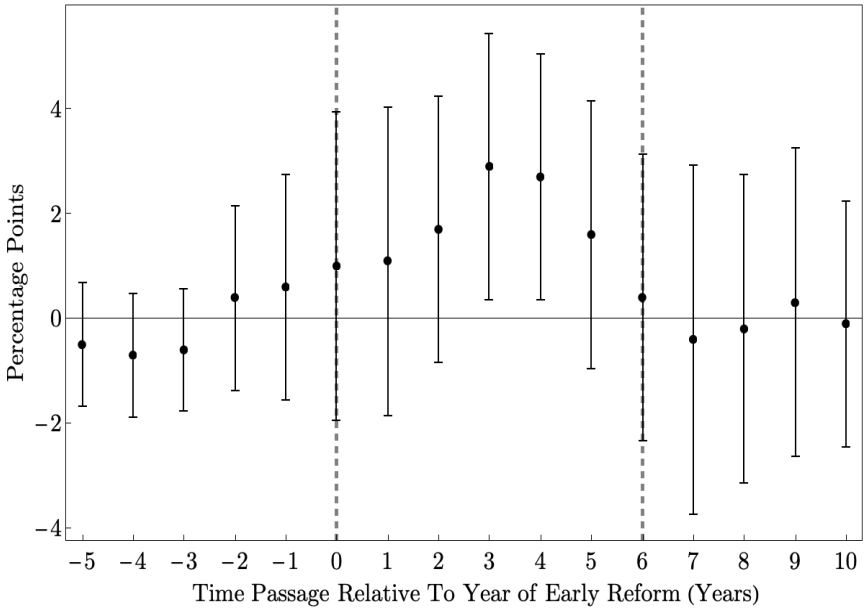


Figure 5: Treatment-Control Differences in Proportion Receiving BA Degree, By Years Since Reform

Note–This figure presents treatment (early reform: 1998, 1999) - control (late reform: 2004, 2005) differences in the proportion of BA degree recipients of individuals who were aged 22-27 in each year, from five years before the early reform until ten years after it. The vertical bands represent 95% confidence interval.

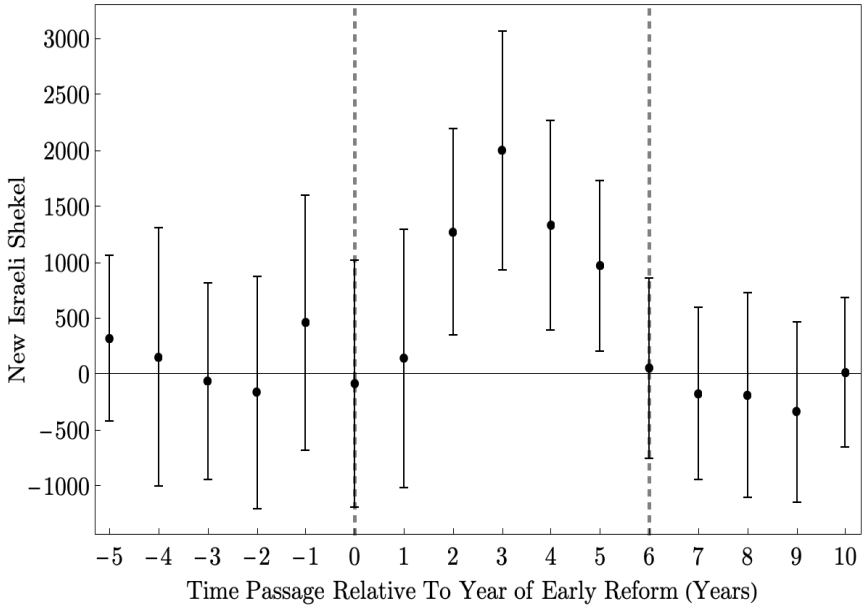
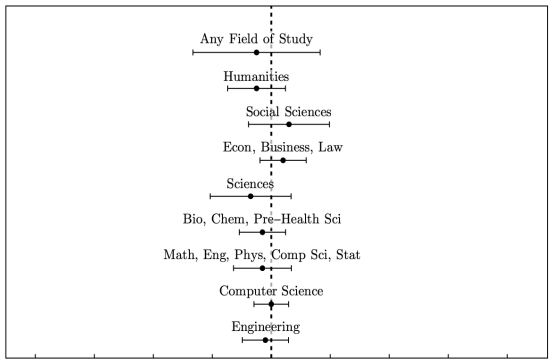


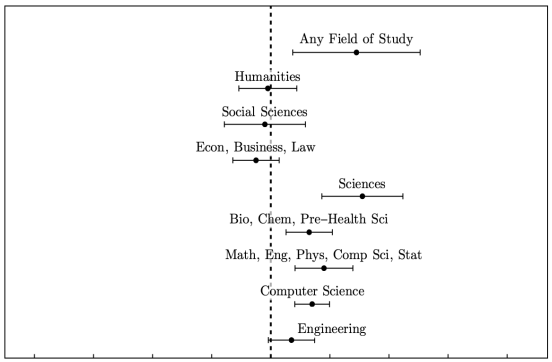
Figure 6: Treatment-Control Wage Differences at 2014, By Years Since Reform

Note–This figure presents treatment (early reform: 1998, 1999) - control (late reform: 2004, 2005) wage differences at 2014 of individuals who were aged 22-27 in each year, from five years before the early reform until ten years after it. The vertical bands represent 95% confidence interval.

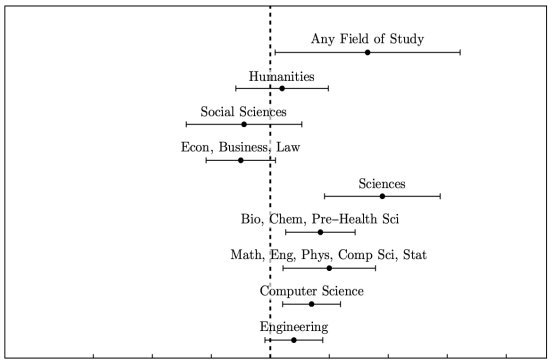
Late Reformed Kibbutzim as a Control Group



(a) Pre-Reform

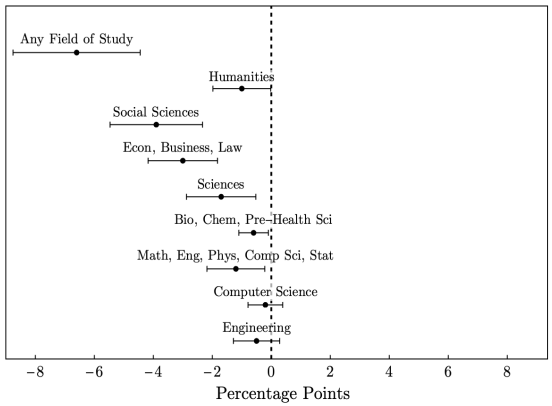


(b) Post-Reform

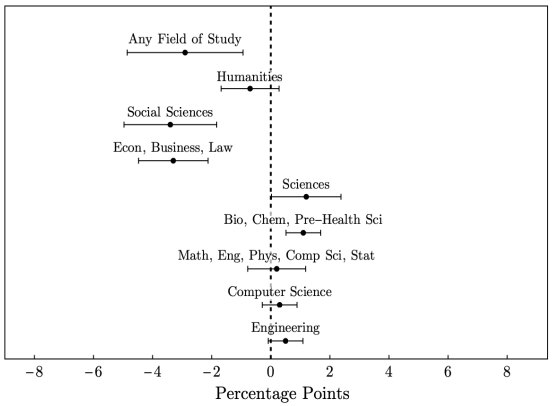


(c) Treatment Effect

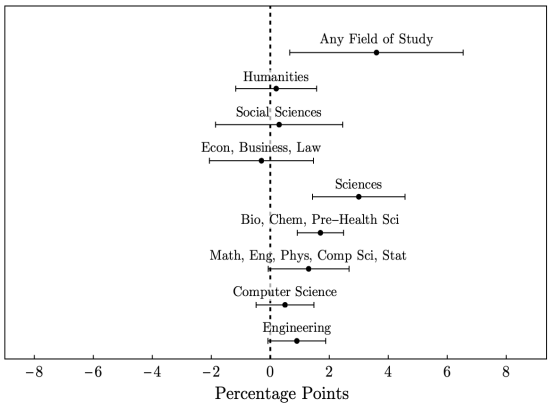
Tel-Aviv as a Control Group



(d) Pre-Reform



(e) Post-Reform



(f) Treatment Effect

Figure 7: Treatment-Control Differences by Fields of Study, Pre- and Post- Reform

Note–The points in this figure present treatment-control differences estimates shown in Tables 4 and 5. The horizontal bands represent 95% confidence interval.

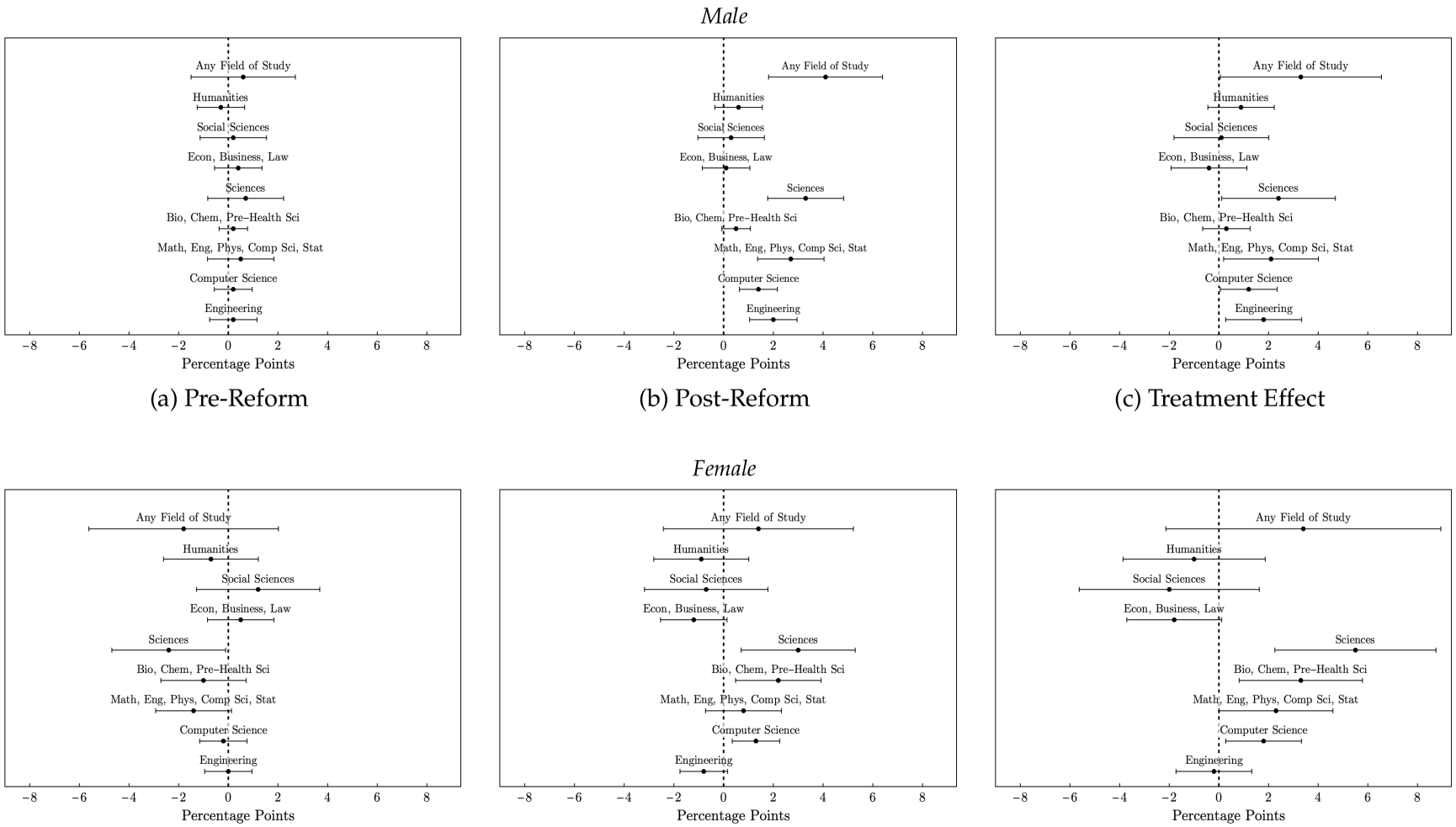


Figure 8: Treatment-Control Differences by Fields of Study and Gender, Pre- and Post- Reform

Note–The points in this figure present treatment-control differences estimates shown in Table 6 . The horizontal bands represent 95% confidence interval.

Table 1: Balancing Tests of Individuals' and Kibbutz Characteristics, by Treatment Group, Pre- and Post- Reform

	Pre-Reform Individuals Aged 22-27 in 1995, 1996				Post-Reform Individuals Aged 22-27 in 2001, 2002			
	Means		Balancing Tests (T-C)		Means		Balancing Tests (T-C)	
	Treatment	Control	Coeff	*p-val*	Treatment	Control	Coeff	*p-val*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Individuals' lavel								
Male	0.555	0.548	0.006	*0.764*	0.546	0.544	0.003	*0.896*
	[0.497]	[0.498]			[0.498]	[0.498]		
Age	24.366	24.410	-0.043	*0.508*	24.573	24.499	0.074	*0.317*
	[1.665]	[1.671]			[1.710]	[1.689]		
Number of Siblings	2.757	2.759	-0.002	*0.983*	2.645	2.611	0.034	*0.736*
	[1.291]	[1.290]			[1.170]	[1.029]		
Ethnic Origin: Africa/Asia	0.171	0.172	-0.001	*0.972*	0.093	0.107	-0.014	*0.436*
	[0.377]	[0.377]			[0.290]	[0.309]		
Ethnic Origin: Ethiopia	0.000	0.003	-0.003	*0.318*	0.000	0.006	-0.006	*0.133*
	[0.000]	[0.052]			[0.000]	[0.080]		
Ethnic Origin: FSU Countries	0.032	0.025	0.007	*0.484*	0.017	0.021	-0.005	*0.475*
	[0.176]	[0.155]			[0.128]	[0.145]		
Ethnic Origin: Europe/America	0.179	0.162	0.016	*0.689*	0.166	0.117	0.049	*0.102*
	[0.383]	[0.369]			[0.372]	[0.321]		
Ethnic Origin: Israel	0.562	0.553	0.009	*0.881*	0.654	0.662	-0.009	*0.830*
	[0.496]	[0.497]			[0.476]	[0.473]		
Ethnic Origin: Other	0.056	0.086	-0.030	*0.284*	0.071	0.086	-0.015	*0.495*
	[0.230]	[0.280]			[0.257]	[0.281]		
Panel B. Kibbutz lavel								
Population	761.52	704.02	57.50	*0.625*	764.29	719.54	44.74	*0.706*
	[331.2]	[312.5]			[312.4]	[321.1]		
Organization: Artzi	0.138	0.328	-0.190	*0.121*	0.124	0.308	-0.184	*0.122*
	[0.345]	[0.470]			[0.330]	[0.462]		
Organization: Takam	0.830	0.672	0.158	*0.202*	0.824	0.692	0.132	*0.281*
	[0.376]	[0.470]			[0.381]	[0.462]		
Distance From TLV (km)	107.27	79.45	27.82	*0.056*	111.22	80.84	30.37	*0.036*
	[45.94]	[47.26]			[42.81]	[47.92]		
Peripherality Index	-0.596	-0.089	-0.507	*0.056*	-0.662	-0.108	-0.553	*0.035*
	[0.756]	[0.921]			[0.671]	[0.941]		
Socioeconomic index	6.972	6.477	0.495	*0.183*	6.889	6.510	0.379	*0.287*
	[0.883]	[1.404]			[0.855]	[1.346]		
Observations	1035	1096	-	-	1025	1078	-	-
Kibbutzim	32	29	-	-	32	29	-	-

Note–This table presents means and means-difference of characteristics of individuals in treatment kibbutzim (reformed early 1998, 1999) and control kibbutzim (reformed late 2004, 2005) who are aged 22-27 at the beginning of the follow-up periods: pre-reform, 1995, 1996 (untreated) and post-reform, 2001, 2002 (treated). Columns 1-3 present pre-reform means of treatment and control groups and the difference between them, respectively. Columns 5-7 present post-reform means of treatment and control groups and the difference between them, respectively. All estimated coefficients are based on a regression of the characteristics as a dependent variable and the treatment indicator is the explanatory variable.
* Standard deviations presented in brackets. p-values in italics. Difference in means significant at ***1% **5% *10%.

Table 2: Outcomes Means and Treatment-Control Differences, Pre- and Post-Reform

	Pre-Reform Individuals Aged 22-27 in 1995, 1996				Post-Reform Individuals Aged 22-27 in 2001, 2002			
	Treatment	Control	Difference (T-C)		Treatment	Control	Difference (T-C)	
			Coeff	*p-val*			Coeff	*p-val*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any Field	0.041	0.046	-0.005	*0.405*	0.110	0.082	0.029	*0.025***
	[0.197]	[0.209]			[0.313]	[0.274]		
Humanities Any Field	0.013	0.017	-0.005	*0.318*	0.017	0.018	-0.001	*0.867*
	[0.111]	[0.131]			[0.128]	[0.132]		
Social Sciences Any Field	0.017	0.011	0.006	*0.230*	0.040	0.042	-0.002	*0.802*
	[0.131]	[0.104]			[0.196]	[0.200]		
Economics, Business, Law	0.008	0.004	0.004	*0.183*	0.011	0.016	-0.005	*0.211*
	[0.088]	[0.060]			[0.103]	[0.125]		
Sciences Any Field	0.011	0.017	-0.007	*0.134*	0.054	0.022	0.031	*0.000****
	[0.103]	[0.131]			[0.225]	[0.148]		
Bio, Chem, Pre-Health Sci	0.006	0.009	-0.003	*0.453*	0.020	0.006	0.013	*0.009****
	[0.076]	[0.095]			[0.138]	[0.080]		
Math, Eng, Phys, Comp Sci, Stat	0.005	0.008	-0.003	*0.318*	0.034	0.016	0.018	*0.024***
	[0.069]	[0.090]			[0.182]	[0.125]		
Computer Science	0.001	0.001	0.000	*0.318*	0.019	0.005	0.014	*0.000****
	[0.031]	[0.030]			[0.135]	[0.068]		
Engineering	0.002	0.004	-0.002	*0.318*	0.017	0.009	0.007	*0.243*
	[0.044]	[0.060]			[0.128]	[0.096]		
Observations	1035	1096	-	-	1025	1078	-	-
Kibbutzim	32	29	-	-	32	29	-	-

Note–This table presents means and means-difference of outcomes of individuals in treatment kibbutzim (reformed early 1998, 1999) and control kibbutzim (reformed late 2004, 2005) who are aged 22-27 at the beginning of the follow-up periods: pre-reform, 1995, 1996 (untreated) and post-reform, 2001, 2002 (treated). Columns 1-3 present pre-reform means of treatment and control groups and the difference between them, respectively. Columns 5-7 present post-reform means of treatment and control groups and the difference between them, respectively. The dependent variable is an indicator of whether the student completed a BA degree in the areas of study indicated by the outcome. All estimated coefficients are based on a regression of the outcomes as a dependent variable and the treatment indicator is the explanatory variable. Standard deviations presented in brackets. p-values in italics. Difference in means significant at ***1% **5% *10%.

Table 3: Treatment-Control Differences in Pre-Reform Time Trends in Academic Outcomes, 1989-1995

	BA Any Field			
	Control Group: Late Reform 04-05		Control Group: TLV	
	(1)	(2)	(3)	(4)
Pane A. Linear Trend Model				
Treatment	0.005	-	-0.031***	-
	(0.007)	-	(0.007)	-
Time Trend	0.003***	0.003***	0.004***	0.004***
	(0.001)	(0.001)	(0.000)	(0.000)
Treatment X Time Trend	0.000	0.000	-0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Panel B. Cohort Dummies Model				
Treatment	0.004	-	-0.031***	-
	(0.010)	-	(0.009)	-
Treatment X 1990	-0.001	0.000	-0.001	-0.001
	(0.013)	(0.013)	(0.013)	(0.013)
Treatment X 1991	0.010	0.010	0.010	0.010
	(0.013)	(0.013)	(0.013)	(0.013)
Treatment X 1992	0.010	0.010	0.005	0.006
	(0.013)	(0.013)	(0.013)	(0.013)
Treatment X 1993	-0.009	-0.008	-0.014	-0.013
	(0.013)	(0.013)	(0.013)	(0.013)
Treatment X 1994	-0.012	-0.011	-0.016	-0.016
	(0.013)	(0.013)	(0.012)	(0.012)
Treatment X 1995	-0.012	-0.012	-0.011	-0.011
	(0.013)	(0.013)	(0.012)	(0.012)
Kibbutz Fixed Effects	NO	YES	NO	YES

Note–This table presents results from OLS regressions where the dependent variable is an indicator of whether the student completed a BA degree. The sample includes individuals aged 22-27 in each year from 1989 to 1995. The treatment group consists of individuals who lived in kibbutzim that reformed in 1998, 1999. The control group consists of individuals who lived in Tel-Aviv. In the regression results reported in panel A, outcomes are allowed to vary according to a linear time (cohort) trend that differs in treatment and control groups. The regression in panel B includes cohort dummies.
* Standard errors clustered by kibbutz are presented in parentheses. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 4: Effect of Pay Reform on BA Degree Attainment, by Field of Study

(Sample: Individuals Aged 22-27 in 1995, 1996 and in 2001, 2002)

	BA Degree by Field of Study								
		Humanities	Social Sciences		Sciences				
	Any Field	Humanities Any Field	Social Sciences Any Field	Economics, Business, Law	Sciences Any Field	Bio, Chem, Pre-Health Sci	Math, Eng, Phys, Comp Sci, Stat	Computer Science	Engineering
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Cross Section Regressions									
Pre-Reform	-0.005	-0.005	0.006	0.004	-0.007	-0.003	-0.003	0.000	-0.002
	(0.011)	(0.005)	(0.007)	(0.004)	(0.007)	(0.004)	(0.005)	(0.003)	(0.004)
Mean of Dependent Var. (Control)	0.046	0.017	0.011	0.004	0.017	0.009	0.008	0.001	0.004
Post-Reform	0.029***	-0.001	-0.002	-0.005	0.031***	0.013***	0.018***	0.014***	0.007*
	(0.011)	(0.005)	(0.007)	(0.004)	(0.007)	(0.004)	(0.005)	(0.003)	(0.004)
Mean of Dependent Var. (Control)	0.082	0.018	0.042	0.016	0.022	0.006	0.016	0.005	0.009
Panel B. Difference-in-Differences									
Simple	0.034**	0.004	-0.008	-0.009	0.038***	0.016***	0.022***	0.014***	0.009*
	(0.016)	(0.008)	(0.010)	(0.006)	(0.010)	(0.006)	(0.008)	(0.005)	(0.005)
Controlled	0.033**	0.004	-0.009	-0.010*	0.038***	0.017***	0.020***	0.014***	0.008
	(0.016)	(0.008)	(0.010)	(0.006)	(0.010)	(0.006)	(0.008)	(0.005)	(0.005)
Controlled for Others Reforms	0.037**	0.007	-0.008	-0.007	0.039***	0.015**	0.023***	0.013**	0.014**
	(0.017)	(0.008)	(0.011)	(0.006)	(0.011)	(0.006)	(0.008)	(0.005)	(0.006)
Observations	4233	4233	4233	4233	4233	4233	4233	4233	4233

Note–This table presents the estimated coefficients of interest of difference-in-differences regressions, comparing cohorts of individuals aged 22-27 in pre/post reform period. The treatment and the control groups consist of individuals who lived in early (1998, 1999) and late (2004, 2005) reformed kibbutzim respectively (See Figure 1). The dependent variable is an indicator of whether the student completed a BA in the areas of study indicated by the outcome. 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 demographic controls: gender, number of siblings, a set of ethnic dummies (origin from Africa/Asia, Europe/America, immigrants from FSU, Ethiopia, Israel, and other countries).

* Standard errors clustered by kibbutz are presented in parentheses. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 5: Tel-Aviv as a Control Group, Effect of Pay Reform on BA Degree Attainment, by Field of Study

(Sample: Individuals Aged 22-27 in 1995 and in 2001)

	BA Degree by Field of Study								
		Humanities	Social Sciences		Sciences				
	Any Field	Humanities Any Field	Social Sciences Any Field	Economics, Business, Law	Sciences Any Field	Bio, Chem, Pre-Health Sci	Math, Eng, Phys, Comp Sci, Stat	Computer Science	Engineering
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Cross Section Regressions									
Pre-Reform	-0.066***	-0.010**	-0.039***	-0.031***	-0.017***	-0.005**	-0.011**	-0.002	-0.005
	(0.011)	(0.005)	(0.008)	(0.006)	(0.006)	(0.003)	(0.005)	(0.003)	(0.004)
Mean of Dependent Var. (Control)	0.110	0.025	0.059	0.042	0.026	0.007	0.018	0.004	0.008
Post-Reform	-0.029***	-0.007	-0.034***	-0.033***	0.012**	0.011***	0.002	0.003	0.005
	(0.010)	(0.005)	(0.008)	(0.006)	(0.006)	(0.003)	(0.005)	(0.003)	(0.003)
Mean of Dependent Var. (Control)	0.145	0.023	0.076	0.047	0.046	0.007	0.039	0.017	0.016
Panel B. Difference-in-Differences									
Simple	0.037**	0.004	0.004	-0.002	0.029***	0.016***	0.013*	0.005	0.009*
	(0.015)	(0.007)	(0.011)	(0.009)	(0.008)	(0.004)	(0.007)	(0.005)	(0.005)
Controlled	0.035**	0.002	0.003	-0.003	0.030***	0.017***	0.013*	0.005	0.009*
	(0.015)	(0.007)	(0.011)	(0.009)	(0.008)	(0.004)	(0.007)	(0.005)	(0.005)
Observations	91,660	91,660	91,660	91,660	91,660	91,660	91,660	91,660	91,660

Note–This table presents the estimated coefficients of interest of difference-in-differences regressions, comparing cohorts of individuals aged 22-27 in pre/post reform period (See Figure 1). Treatment group consists of kibbutzim that reformed in 1998, 1999. Control group consists of Individuals who lived in Tel-Aviv. The dependent variable is an indicator of whether the student completed a BA in the areas of study indicated by the outcome. 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 demographic controls: gender, number of siblings, a set of ethnic dummies (origin from Africa/Asia, Europe/America, immigrants from FSU, Ethiopia, Israel and other countries).

* Standard errors clustered by kibbutz are presented in parentheses. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 6: Effect of Pay Reform on BA Degree Attainment by Field of Study and Gender

(Sample: Individuals Aged 22-27 in 1995, 1996 and in 2001, 2002)

	BA by Field of Study								
		Humanities	Social Sciences		Sciences				
	Any Field	Humanities Any Field	Social Sciences Any Field	Economics, Business, Law	Sciences Any Field	Bio, Chem, Pre-Health Sci	Math, Eng, Phys, Comp Sci, Stat	Computer Science	Engineering
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Male									
Panel A. Cross Section Regressions									
Pre-Reform	0.006	-0.003	0.002	0.004	0.007	0.002	0.005	0.002	0.002
	(0.011)	(0.005)	(0.007)	(0.005)	(0.008)	(0.003)	(0.007)	(0.004)	(0.005)
Mean of Dependent Var. (Control)	0.018	0.010	0.005	0.003	0.003	0.002	0.002	0.000	0.002
Post-Reform	0.041***	0.006	0.003	0.001	0.033***	0.005	0.027***	0.014***	0.020***
	(0.011)	(0.005)	(0.007)	(0.005)	(0.008)	(0.003)	(0.007)	(0.004)	(0.005)
Mean of Dependent Var. (Control)	0.039	0.003	0.020	0.012	0.015	0.002	0.014	0.003	0.005
Panel B. Difference-in-Differences									
Simple	0.035**	0.009	0.001	-0.003	0.026**	0.004	0.022**	0.013**	0.018**
	(0.016)	(0.007)	(0.010)	(0.008)	(0.011)	(0.005)	(0.010)	(0.006)	(0.008)
Controlled	0.033**	0.009	0.001	-0.004	0.024**	0.003	0.021**	0.012**	0.018**
	(0.017)	(0.007)	(0.010)	(0.008)	(0.011)	(0.005)	(0.010)	(0.006)	(0.008)
Observations	2321	2321	2321	2321	2321	2321	2321	2321	2321
Female									
Panel C. Cross Section Regressions									
Pre-Reform	-0.018	-0.007	0.012	0.005	-0.024**	-0.009	-0.014*	-0.002	-0.006
	(0.020)	(0.010)	(0.013)	(0.007)	(0.012)	(0.009)	(0.008)	(0.005)	(0.005)
Mean of Dependent Var. (Control)	0.079	0.026	0.018	0.004	0.034	0.018	0.016	0.002	0.006
Post-Reform	0.014	-0.009	-0.007	-0.012*	0.030**	0.022**	0.008	0.013**	-0.008
	(0.020)	(0.010)	(0.013)	(0.007)	(0.012)	(0.009)	(0.008)	(0.005)	(0.005)
Mean of Dependent Var. (Control)	0.132	0.035	0.067	0.020	0.030	0.012	0.018	0.006	0.014
Panel D. Difference-in-Differences									
Simple	0.032	-0.002	-0.019	-0.016*	0.053***	0.032***	0.021*	0.015**	-0.002
	(0.028)	(0.015)	(0.019)	(0.009)	(0.017)	(0.012)	(0.011)	(0.007)	(0.008)
Controlled	0.034	-0.001	-0.02	-0.018*	0.055***	0.033***	0.023*	0.018**	-0.002
	(0.028)	(0.015)	(0.019)	(0.010)	(0.017)	(0.013)	(0.012)	(0.008)	(0.008)
Observations	1913	1913	1913	1913	1913	1913	1913	1913	1913

Note–This table presents the estimated coefficients of interest of difference-in-differences regressions, comparing individuals aged 22-27 in pre/post reform period (See Figure 1). The treatment group consists of kibbutzim that reformed in 1998, 1999. The control group consists of kibbutzim that reformed in 2004, 2005. The dependent variable is an indicator of whether the student completed a BA in the areas of study indicated by the outcome. The simple difference-in-differences regressions includes only cohort dummies. The controlled difference-in-differences regressions includes cohort dummies, kibbutz fixed effects and the following student demographic controls: number of siblings, a set of ethnic dummies (origin from Africa/Asia, Europe/America, immigrants from FSU, Ethiopia, Israel and other countries).

* Standard errors clustered by kibbutz are presented in parentheses. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 7: Effect of Pay Reform on Expected Wages, Full, Men and Female Samples

(Sample: Individuals Aged 22-27 in 1995, 1996 and in 2001, 2002)

	BA Degree by Expected Wages								
	Expected Wages			Field of Studies With Expected Wages Above 3rd Quartile			Field of Studies With Expected Wages Above Median		
	All	Male	Female	All	Male	Female	All	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Cross Section Regressions									
Pre-Reform	-0.697	74.53	-92.38	-0.002	0.005	-0.012	-0.003	0.004	-0.013
	(78.37)	(117)	(99.33)	(0.006)	(0.008)	(0.010)	(0.009)	(0.010)	(0.015)
Post-Reform	321.3***	474.3***	138.3	0.013**	0.026***	-0.002	0.029***	0.028***	0.030**
	(78.88)	(118.5)	(99.25)	(0.006)	(0.008)	(0.010)	(0.009)	(0.010)	(0.015)
Panel B. Difference-in-Differences									
Simple	322***	399.7**	230.7	0.015*	0.021*	0.010	0.032***	0.024*	0.043**
	(111.2)	(166.5)	(140.4)	(0.009)	(0.011)	(0.014)	(0.012)	(0.014)	(0.022)
Controlled	309.4***	377.5**	265.6*	0.015*	0.020*	0.011	0.032***	0.022	0.046**
	(112)	(170)	(143)	(0.009)	(0.011)	(0.014)	(0.013)	(0.014)	(0.022)
Observations	4233	2321	1912	4233	2321	1912	4233	2321	1912

Note–This table presents the estimated coefficients of interest of difference-in-differences regressions, comparing cohorts of individuals aged 22-27 in pre/post reform period (See Figure 1). Treatment group consists of kibbutzim that reformed in 1998, 1999. Control group consists of kibbutzim that reformed in 2004, 2005. In columns 1-3 the dependent variable is continuous and the measurement unit is New Israeli Shekels per month. 1 US dollar is currently equal to approximately 3.7 shekels. the dependent variable in columns 4-9 is an indicator of whether the student completed a BA in a field of study with expected wages between the different quartiles. 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 demographic controls: gender, number of siblings, a set of ethnic dummies (origin from Africa/Asia, Europe/America, immigrants from FSU, Ethiopia, Israel and other countries).

* Standard errors clustered by kibbutz are presented in parentheses. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 8: Placebo Experiments, Effects on Pre-Determined High School Matriculation Outcomes

(Sample: Individuals Aged 22-27 in 2001, 2002)

	Full Sample			Male			Female		
	Treatment Mean	Control Mean	Controlled Differences	Treatment Mean	Control Mean	Controlled Differences	Treatment Mean	Control Mean	Controlled Differences
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Matriculation Certificate	0.525	0.554	-0.030	0.486	0.502	-0.019	0.567	0.608	-0.042
	(0.500)	(0.497)	(0.028)	(0.500)	(0.501)	(0.039)	(0.496)	(0.489)	(0.032)
Matriculation Credit Units	20.57	20.93	-0.412	19.69	20.25	-0.663	21.50	21.63	-0.199
	(8.149)	(7.832)	(0.555)	(8.699)	(8.389)	(0.798)	(7.424)	(7.151)	(0.632)
Math Number of Credits	2.582	2.688	-0.109	2.583	2.773	-0.203	2.582	2.599	-0.011
	(1.696)	(1.697)	(0.094)	(1.767)	(1.756)	(0.123)	(1.620)	(1.631)	(0.118)
English Number of Credits	3.865	3.867	-0.019	3.782	3.810	-0.023	3.953	3.927	-0.010
	(1.409)	(1.435)	(0.090)	(1.459)	(1.511)	(0.129)	(1.350)	(1.350)	(0.112)
Observation	741	785	-	381	400	-	360	385	-

Note–This table presents means, means-difference and standard deviations (in parentheses) of outcomes of individuals who are aged 22-27 in 2001, 2002. Treatment group consists of kibbutzim that reformed in 1998, 1999. Control group consists of kibbutzim that reformed in 2004, 2005. The dependent variable in row I is whether the student received a matriculation certificate; in row II it is the number of credit unites of the matriculation certificate; in rows III and IV it is the number of matriculation units in English and mathematics subjects, respectively. The range of units in these subjects is 0-5.

* Standard errors clustered by kibbutz are presented in parentheses. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 9: Placebo Experiments, Using Older Unaffected Cohort in Difference-In-Differences Estimation

(Sample: Individuals Aged 22-27 in 1989, 1990 and in 1995, 1996)

	BA Degree by Field of Study								
		Humanities	Social Sciences		Sciences				
	Any Field	Humanities Any Field	Social Sciences Any Field	Economics, Business, Law	Sciences Any Field	Biol, Chem, Pre-Health Sci	Math, Eng, Phys, Comp Sci, Stat	Computer Science	Engineering
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Simple Difference-in-Differences	0.008	-0.013**	0.015*	0.009	0.006	0.006	-0.000	0.003	-0.004
	(0.014)	(0.007)	(0.009)	(0.006)	(0.009)	(0.006)	(0.006)	(0.003)	(0.005)
Controlled Difference-in-Differences	0.004	-0.012*	0.011	0.008	0.005	0.005	-0.000	0.003	-0.005
	(0.014)	(0.007)	(0.009)	(0.006)	(0.009)	(0.006)	(0.007)	(0.003)	(0.005)
Observations	3863	3863	3863	3863	3863	3863	3863	3863	3863

Note–This table presents difference-in-differences and controlled difference-in-differences coefficients of placebo experiment that compare cohorts of individuals aged 22-27 in two pre-reform periods. The treatment group consists of kibbutzim that reformed in 1998, 1999. The control group includes kibbutzim that reformed in 2004, 2005. The dependent variable is an indicator of whether the student completed a BA in the areas of study indicated by the outcome. 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 demographic controls: gender, number of siblings, a set of ethnic dummies (origin from Africa/Asia, Europe/America, immigrants from FSU, Ethiopia, Israel, and other countries).

* Standard errors clustered by kibbutz are presented in parentheses. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 10: Treatment-Control Differences, by Eligibility for High School Matriculation Certificate

(Sample: Individuals Aged 22-27 in 2001, 2002)

	Individuals With a Matriculation Certificate				Individuals Without a Matriculation Certificate			
	Treatment Mean	Control Mean	Treatment-Control Difference	Controlled Difference	Treatment Mean	Control Mean	Treatment-Control Difference	Controlled Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. BA Degree by Field of Study								
Any Field	0.216	0.163	0.053*	0.057*	0.057	0.037	0.02	0.019
	(0.412)	(0.370)	(0.029)	(0.030)	(0.232)	(0.189)	(0.015)	(0.014)
Humanities Any Field	0.028	0.039	-0.011	-0.011	0.009	0.006	0.003	0.004
	(0.166)	(0.194)	(0.013)	(0.013)	(0.092)	(0.075)	(0.006)	(0.005)
Social Sciences Any Field	0.069	0.08	-0.011	-0.009	0.031	0.02	0.011	0.009
	(0.254)	(0.272)	(0.017)	(0.017)	(0.174)	(0.140)	(0.011)	(0.012)
Economics, Business, Law	0.021	0.03	-0.009	-0.009	0.009	0.006	0.003	0.004
	(0.142)	(0.170)	(0.010)	(0.010)	(0.092)	(0.075)	(0.006)	(0.006)
Sciences Any Field	0.118	0.044	0.075***	0.077***	0.017	0.011	0.006	0.006
	(0.323)	(0.205)	(0.022)	(0.022)	(0.130)	(0.106)	(0.008)	(0.008)
Biology, Chemistry, Pre-Health Sci	0.036	0.009	0.027**	0.028***	0.011	0.006	0.006	0.006
	(0.187)	(0.096)	(0.010)	(0.011)	(0.106)	(0.075)	(0.006)	(0.007)
Math, Eng, Physics, Comp Sci, Stat	0.082	0.034	0.048**	0.048**	0.006	0.006	-0.000	-0.000
	(0.275)	(0.183)	(0.019)	(0.020)	(0.075)	(0.075)	(0.005)	(0.006)
Computer Science	0.044	0.011	0.032***	0.033***	0.003	0.000	0.003	0.003
	(0.205)	(0.107)	(0.011)	(0.011)	(0.053)	(0.000)	(0.003)	(0.003)
Engineering	0.041	0.021	0.02	0.022	0.003	0.003	0.000	0.000
	(0.199)	(0.143)	(0.015)	(0.015)	(0.053)	(0.053)	(0.004)	(0.004)
Panel B. BA Degree by Expected Wages								
Expected wage	8878	7834	1044.175**	1078.1***	6910	6767	142.5	142.6
	(5600.638)	(3829.801)	(395.873)	(397.072)	(1965.729)	(1529.918)	(116.385)	(110.218)
Above 75'th Percentile	0.075	0.030	0.045**	0.045**	0.006	0.003	0.003	0.003
	(0.263)	(0.170)	(0.018)	(0.019)	(0.075)	(0.053)	(0.005)	(0.005)
Above 50'th Percentile	0.108	0.064	0.044*	0.044*	0.014	0.011	0.003	0.003
	(0.311)	(0.246)	(0.022)	(0.022)	(0.119)	(0.106)	(0.007)	(0.007)
Observations	389	435	-	-	352	350	-	-

Note–This table presents means, means-difference and standard deviations (in parentheses) of outcomes of individuals who are aged 22-27 in 2001, 2002. The treatment group consists of kibbutzim that reformed in 1998, 1999. The control group consists of kibbutzim that reformed in 2004, 2005. In Panel A the dependent variable is an indicator of whether the student completed a BA in the areas of study indicated by the outcome. In Panel B the dependent variable is an indicator of whether the student completed a BA in a field of study with expected wages between the different quartiles. The outcome "Expected Wages" is continuous, and the measurement unit is New Israeli Shekels per month. 1 US dollar is currently equal to approximately 3.7 shekels. The simple difference regressions include only cohort dummies. The controlled difference regressions include cohort dummies, kibbutz fixed effects and the following student demographic controls: gender, number of siblings, a set of ethnic dummies (origin from Africa/Asia, Europe/America, immigrants from FSU, Ethiopia, Israel and other countries).

* Standard errors clustered by kibbutz are presented in parentheses. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 11: Treatment-Control Differences, by Level of High School Math Matriculation Study Program

(Sample: Individuals Aged 22-27 in 2001, 2002)

	Advanced				Basic and Intermediate			
	Treatment Mean	Control Mean	Treatment-Control Difference	Controlled Difference	Treatment Mean	Control Mean	Treatment-Control Difference	Controlled Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. BA Degree by Field of Study								
Any Field	0.364	0.180	0.184***	0.182***	0.110	0.096	0.014	0.016
	(0.484)	(0.386)	(0.060)	(0.060)	(0.314)	(0.295)	(0.020)	(0.019)
Humanities Any Field	0.023	0.004	-0.017	-0.018	0.018	0.022	-0.004	-0.003
	(0.150)	(0.197)	(0.026)	(0.025)	(0.135)	(0.147)	(0.009)	(0.009)
Social Sciences Any Field	0.080	0.080		0.005	0.048	0.005	-0.002	-0.002
	(0.272)	(0.273)	(0.033)	(0.032)	(0.213)	(0.217)	(0.011)	(0.012)
Economics, Business, Law	0.045	0.007	-0.025	-0.018	0.011	0.012	-0.001	-0.001
	(0.209)	(0.256)	(0.029)	(0.028)	(0.103)	(0.108)	(0.006)	(0.006)
Sciences Any Field	0.261	0.006	0.201***	0.194***	0.044	0.025	0.020	0.021*
	(0.442)	(0.239)	(0.047)	(0.048)	(0.206)	(0.156)	(0.012)	(0.012)
Biology, Chemistry, Pre-Health Sci	0.034	0.000	0.034*	0.032**	0.023	0.009	0.014**	0.015**
	(0.183)	(0.000)	(0.018)	(0.016)	(0.150)	(0.093)	(0.006)	(0.006)
Math, Eng, Physics, Comp Sci, Stat	0.227	0.060	0.167***	0.162***	0.021	0.016	0.005	0.006
	(0.421)	(0.239)	(0.047)	(0.047)	(0.145)	(0.126)	(0.009)	(0.009)
Computer Science	0.114	0.030	0.084**	0.081**	0.012	0.003	0.009*	0.010*
	(0.319)	(0.171)	(0.036)	(0.036)	(0.110)	(0.054)	(0.005)	(0.005)
Engineering	0.102	0.030	0.072*	0.079*	0.012	0.010	0.002	0.002
	(0.305)	(0.171)	(0.039)	(0.042)	(0.110)	(0.101)	(0.006)	(0.007)
Panel B. BA Degree by Expected Wages								
Expected wage	11826	8563	3263.432***	3238.032***	7421	7184	237.6	257.3
	(8099.873)	(5227.583)	(974.442)	(973.728)	(3285.824)	(2575.711)	(204.451)	(202.592)
Above 75'th Percentile	0.205	0.060	0.145***	0.137***	0.020	0.012	0.008	0.009
	(0.406)	(0.239)	(0.047)	(0.047)	(0.140)	(0.108)	(0.008)	(0.008)
Above 50'th Percentile	0.273	0.130	0.143**	0.144**	0.035	0.028	0.007	0.008
	(0.448)	(0.338)	(0.059)	(0.058)	(0.185)	(0.164)	(0.012)	(0.012)
Observations	88	100	-	-	652	684	-	-

Note–This table presents means, means-difference and standard deviations (in parentheses) of outcomes of individuals who are aged 22-27 in 2001, 2002. The treatment group consists of kibbutzim that reformed in 1998, 1999. The control group consists of kibbutzim that reformed in 2004, 2005. In Panel A the dependent variable is an indicator of whether the student completed a BA in the areas of study indicated by the outcome. In Panel B the dependent variable is an indicator of whether the student completed a BA in a field of study with expected wages between the different quartiles. The outcome "Expected Wages" is continuous, and the measurement unit is New Israeli Shekels per month. 1 US dollar is currently equal to approximately 3.7 shekels. The simple difference regressions include only cohort dummies. The controlled difference regressions include cohort dummies, kibbutz fixed effects and the following student demographic controls: gender, number of siblings, a set of ethnic dummies (origin from Africa/Asia, Europe/America, immigrants from FSU, Ethiopia, Israel and other countries).

* Standard errors clustered by kibbutz are presented in parentheses. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.*10%.