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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. We use for identification the staggered implementation of this reform in different kibbutzim. 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 degree, especially in STEM fields of study that are expected to yield higher financial returns. For men this increase was largely in computer science and engineering, and for women in biology, chemistry and computer science. Our findings suggest that investment in higher education and the choice of major are responsive to increases in the return to education for both men and women.

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A data appendix is available at http://www.nber.org/data-appendix/w26420

1. Introduction

According to economic models of optimal human capital investment (Becker, 1964; Ben-Porath, 1967; Weiss, 1995), higher rate of return to education would lead to higher investment in education. However, two sets of recent studies yield evidence that is not consistent with this prediction. First, the dramatic increase in the earnings premium for skilled labor that took place over recent decades (Heckman et al., 2008) did not seem to induce Americans to acquire significantly greater skills (Altonji et al., 2012; Eberly and Athreya, 2019; Turner, 2004). Second, while descriptive studies (Long et al., 2015; Montmarquette et al., 2002) found positive elasticities of choice of major with respect to expected earnings,[1] recent studies suggest no causal response in the choice of major to variation in returns across field of study (Beffy et al., 2012; Wiswall and Zafar, 2015).[2] [3]

In this paper, we examine the effect of changes in the rate of return to education on college degree attainment and on field of study choice by exploiting a unique episode. Starting 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. The pay reform in kibbutzim

[1] 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, 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, 2017; Heckman and LaFontaine, 2010).

[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) and 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.

increased the average financial return to education from close to zero to about 8% per year of schooling on average, as well as the relative returns to schooling across majors. Our setting is unique because the pay reform introduced for the first time financial considerations to the choice of field of study. Before the reform, all college majors had the same (zero) monetary return given the equal sharing practice in kibbutzim. After the reform, college majors have heterogeneous returns, with majors such as STEM yielding higher return than humanities, as in the rest of Israel. Overall, this unique episode allows us to study the education decisions in reality setting of low financial incentives.

Our setting and research design also shed light on the broader debate over the advantages and disadvantages of more egalitarian vs. capitalist systems (see Abramitzky, 2018, for discussion of this debate and the kibbutzim as important social experiments in voluntary socialism). We study here one potential disadvantage of egalitarian societies, namely that they offer low returns to schooling and may discourage academic achievement. Specifically, our setting allows us to study young adults who grew up in a more egalitarian society than the US, and who suddenly faced an increase in the financial returns to schooling in their 20's. This setting also allows us to study how women who grew up in an egalitarian environment respond to changes in the returns to schooling, and whether their responses differ from those of men.

We use newly-available administrative data from Israel's Central Bureau of Economics Research on the field of study of adult kibbutz members to test how this pay reform influenced kibbutz members' college attainment and the choice of major during college. Kibbutzim that did not reform are not a plausible control group for kibbutzim that did because they tended to be financially stronger. Instead, we use kibbutzim that reformed later as a control group for kibbutzim that reformed earlier. The timing of the reform was not related to the economic and financial strength of the kibbutz in years prior to the reforms. We use difference-in-differences approaches, comparing the field of study of adult kibbutz members in kibbutzim that implemented the pay reform early to adults in kibbutzim that reformed at a later date, before and after the early reforms. We show evidence that these two groups (the treatment and control group) were indistinguishable in both their observable background characteristics and their

pre-reform academic outcomes. A similar identification strategy used by Abramitzky and Lavy (2014) showed that the increase in the returns to schooling induced high school students to improve their high school and matriculation achievements.

Unlike in the abovementioned evidence from the US and elsewhere, in the kibbutz context of low initial returns we find that young adults respond to the change in returns to schooling by increasing their rate of BA degree attainment and by choosing fields of studies in college and university that are expected to yield higher financial returns, mainly STEM subjects. As expected, these effects are most evident for individuals who had the pre-determined pre-requisite high school achievements. Men increase their academic degrees in engineering, physics, and computer science. Women also respond to the changes in returns, both by selecting fields that are traditionally dominated by women such as biology and by choosing fields that are commonly perceived to be attended by men such as computer science. This finding that women are also responsive to changes in returns is in contrast to recent studies that show low responsiveness by females 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). In the context of young adults who grew up in egalitarian communities, we show that before the pay reform men and women kibbutz members chose majors with lower return relative to others in Israel, and that after the reform they closed much of this gap.

Our findings are robust to using alternative identification strategies. For example, 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 get similar results in this different natural experiment even though this control group had much better pre-reform outcomes.

The rest of the paper is structured as follows. Section 2 briefly presents the background of the kibbutzim, their traditional lifestyle, the causes of privatization and the structure of the pay reform and of 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 effect of the

reform on college attainment and choice of major, as well as 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. Today, there are around 120,000 kibbutz members, and they account for about 2.5% of the Jewish population in Israel. Traditionally, all kibbutzim were based on equal wages for all their members, regardless of their economic or other contribution to the community. 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 lies in contrast to many other communes, whose members have often been more marginal and isolated from the outside world.

Kibbutz members who held a job outside the kibbutz shared their salaries equally with other members. This meant that the monetary return to schooling in the kibbutz was very low, close to zero.[5] In this paper, we study a pay reform that kibbutzim adopted beginning in the late 1990s, moving from pay based on equal sharing to 'market based' differential wages. In this new system, compensation was based on members' productivity. These pay reforms were a response to a major financial crisis in the late 1980s.[6] Kibbutzim, like many other businesses in Israel, found themselves

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

[5]Under the kibbutz system where members earned the same salary regardless of their education level, economic theory would predict that individuals will under invest in education. In the context of kibbutzim, it was noted that kibbutz-raised children often lacked ambition and a sense of personal achievement (Bettelheim, 1969; Gavron, 2000).

[6]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.

with huge debts they could not repay.[7] Even though some of the loans were rescheduled or erased, living conditions still fell substantially in many kibbutzim, leading to higher exit rate during the late 1980s and early 1990s, and to a discussion about major reforms of traditional kibbutz life style and ideology.

Kibbutzim started to shift away from equal sharing for the first time in their history. Kibbutzim that were hit less by the crisis and remained richer were much more likely to maintain equal sharing (Abramitzky, 2018, 2008). For that reason, we do not use kibbutzim that did not reform as a control group for kibbutzim that did. Instead, we use kibbutzim that reformed later as a control group for kibbutzim that reformed earlier. We find that the timing of the reform was not related to the economic and financial strength of the kibbutz in years prior to the reforms. This is additional important evidence that the timing of the pay change was not endogenously related to factors that affect or are related to schooling decisions of kibbutz members.[8] 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, and how long it took the kibbutz to reach a consensus for the reform (that required a super majority).

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.

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

[8]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).

Kibbutzim began encouraging members to seek high-paying jobs outside the kibbutz and to establish small businesses within the kibbutz. Outsiders were hired to replace the kibbutz workers in the less-skilled work they had left. To be sure, since the 1960s the kibbutz had tolerated some kibbutz members who were professionals such as teachers, doctors, professors, painters, and designers working outside the kibbutz, but "until now [the late 1990s] it had been seen as a deviation from the norm, tolerated in order to ensure the self-fulfillment and happiness of the individual or the welfare of the neighboring town" (Near, 1997, p. 353). In the period we study, about 25% of kibbutz members worked outside their kibbutz.

In reformed kibbutzim, 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 the wages of non-kibbutz workers in similar occupations and with similar education, skill, and experience.

Each kibbutz member paid a 'tax' that was deducted from her gross wages. These funds were used to maintained a safety net that supported older members and those with very low wage (i.e. a minimum wage). In practice, 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. An important point to emphasize is that throughout the period we study, from late 1990's to date, adults in kibbutzim made free educational choices.

Abramitzky and Lavy (2014) show that the move from equal wages to differential pay signaled strongly to members in kibbutzim an increase in the monetary rate of return to schooling by introducing productivity-related wage differences within a kibbutz for the first time. The change was from a near zero rate of return to a post-reform return similar to the rest of Israel, which is estimated by various studies at about 8% per year of schooling (Frish, 2007). However, we are aware of the possibility that the return to schooling increased by less than 8% per year of schooling for several reasons. First, monetary rewards are not the only reason people acquire education.[9]

[9]See Oreopoulos and Salvanes (2011) for a recent paper that makes this point convincingly.

Nonmonetary incentives such as prestige and care about the collective likely encouraged kibbutz members to pursue education in the pre-reform period. Peer pressure and collective bargaining over responsibilities within the community may also have played a role. Second, the option to leave the kibbutz at any time meant that the pre-reform 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.[10]

We find that, once the reform took place, 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).[11] 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.

Abramitzky and Lavy (2014) also show that the pay reform was highly salient to members as it has been the most discussed topic in kibbutzim since its start, being hotly debated and voted on by members in kibbutzim. Upon implementation, kibbutz members received detailed information about the new sharing rule and about the link between earnings and productivity and the role played in this regard by market forces. Booklets elaborating on the reforms were distributed to all members. Kibbutzim emphasized that each member was responsible for his own livelihood and the livelihood of his family. Another way to view the pay reform is through the lens of taxation, as it essentially reduced the income tax rate in kibbutzim from 100% to similar tax rates in Israel.

[10]However, 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.

[11]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.

2.2 The Higher Education System in Israel

In Israel, most students begin their academic studies between the ages of 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, master and doctoral degrees in all field of studies (one of them confers only graduate and PhD degrees), and 62 colleges that grant only bachelor degrees (some of these also give master degrees).[12] These colleges are similar to four-year community colleges. All the universities and most the colleges are publicly founded, Bachelor degree cost 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 both Israeli matriculation exams-*bagrut* diploma and psychometric (similar to the SAT) admission test. In order 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. For a given field of study, it is typically more difficult to be admitted to a university than to a college.

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 current residence that allows us to identify those who lived in kibbutzim at the relevant years. Data from the Ministry of Education provides us with student-level information on parental schooling, ethnicity and country of birth, as well as information on high

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

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 that obtained a BA or higher academic degree from any post-secondary institution in the country, including the institution, 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 was also used in Abramitzky and Lavy (2014) and it 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 in 2001, 2002 (post-reform) form our first sample. We will explain in the empirical strategy section 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 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 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, and 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 own category) (4) engineering. These more detailed definitions of fields of study are particularly interesting for the discussion of results by gender.

We also make use of data that we obtained 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.[13] These means are computed based on

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

the population 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.[14]

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, 1999 as the treatment group and the kibbutzim that adopted it in 2004, 2005 as the control group. 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 a 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 as well and increased their investment in schooling accordingly.

Our sample includes individuals aged 22-27 in 2001, 2002 (affected cohort) and in 1995, 1996 (unaffected cohort), who lived in a kibbutz before the reform. We follow each cohort for 4 years. The rationale for these samples is that 22-32 is the age range where the majority of Israeli earns their BA degree (which typically takes three years). 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. Figure 2 illustrates the timeline of the early and late reforms, and of the affected and unaffected cohorts.

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

differences in observables.

[14]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.

22-27 in 2001, 2002 in kibbutzim that reform early (1998, 1999) vs. late (2003, 2004). As the second difference ('before' treatment), we compare individuals aged 22-27 in 1995, 1996 in kibbutzim that reform early vs. late.

We estimate the following regression equation:

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

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). $(EarlyReform_k)$ denotes a kibbutz that implemented the reform early, and the interaction of interest $(AffectedCohort_c X EarlyReform_k)$ 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:

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

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 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 age 30 years or older were less likely to be affected by the reforms because they have fewer years left 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 for the practical reasons discussed earlier, we cannot include much older cohorts. Therefore, the identification strategy is based on a comparison of college outcomes between potentially affected and unaffected age groups (several cohorts) from kibbutzim that reformed early and a comparison from respective groups in kibbutzim that reformed late. These comparisons yield a difference in differences estimate 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 both 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 for the post-reform cohorts (individuals age 22-27 in 2001, 2002). We look at post-reform cohorts here just 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 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 means of treatment and control group and the difference between them respectively.

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 are very similar in control and treatment, with 2.7 children per family. The differences in number of siblings presented in column 3 are -0.002 (p-val=0.983) and the respective difference for the post reform cohorts presented in column 7 are 0.034 (p-val=0.736). Note that these differences are not statistically different from zero and they 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 therefore view the results presented in Table 1 as an indication of good balancing, meaning that, within cohorts, the treatment and control group 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 that among pre-reform cohorts, there is no significant difference in the proportion of BA degree attainment between early- and late-reformed kibbutzim. There are also 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, suggesting that kibbutzim that reformed late are a compelling 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 shows 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 being an early reformed kibbutz (treatment). In Panel B, we estimate a model with a series of cohort dummies and include in the regression an interaction of each of these cohort dummies with the treatment indicator. 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 seen also 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

Figure 4, 5 and 6 illustrate the main finding of the paper 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 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 also took place in late reformed kibbutzim.

Figure 5 and Figure 6 further investigate 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 proportion receiving a BA degree (Figure 5) and the earnings (Figure 6) 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 and earnings were 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 report treatment-control differences in outcomes before and after the reform. The estimates from the pre-reform cross section regression show no difference in BA degree attainment between individuals in early- and late-reformed kibbutzim (this difference is -0.005 with 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. 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 of the sub-fields 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 and 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%. 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.

Our finding of over 3 percent expansion in BA degree certification, mostly in STEM subjects, seems to mostly reflect an expansion at the extensive margin, though there could be intensive margin substitution at play 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. However, our findings (reported below) show that most of the effect is derived from those with a math prerequisite, suggesting that the first scenario is the most consistent

with our findings.

The positive and significant treatment effect estimates are similar in the simple and controlled DID, which is a result of 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, presented in the previous section, are similar to the DID estimates.

6.3 Treatment Effect Estimates by Gender

In Table 5, 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, and we can examine how this earning inequality is related to differential responses by gender to changes in the financial return to schooling. Secondly, there is a growing literature suggesting that women shy away or are deterred from occupations that are traditionally dominated by men, such as STEM fields (Arcidiacono et al., 2012; Bronson, 2015; Gemici and Wiswall, 2014; Kugler et al., 2017).

Examining differences by gender in kibbutzim is especially interesting. Gender equality has always been an important principle in kibbutzim. For example, the traditional communal sleeping arrangement in kibbutzim, whereby children lived in special residences outside of their parents' homes, at least in principle, promoted gender equality by freeing women from their traditional role in society of raising the children. At the outset of kibbutzim, women were often a small minority, responsible for child care, and generally employed in traditional "women's jobs" in services rather than in agriculture. However, they strove for gender equality and established early on their right to work in agricultural and defense jobs, and kibbutzim established communal responsibility for child care. In light of this strive of kibbutzim for gender equality, we might expect women in kibbutzim to be as responsive to the changes in financial returns to schooling.

Gender equality in kibbutzim was more important in theory than in practice, however, and the nannies in kibbutzim were all women. Similarly, other occupations in kibbutzim followed the stereotypical gender divide: women were more often in

charge of cooking, doing the laundry, and educating the children; and men worked in the fields and were in charge of the economy. Women were also underrepresented in kibbutz leadership, and they tended to be quieter in general meetings. In this sense, lingering gender roles in the kibbutz might reduce women's responsiveness to financial returns to schooling.

Overall, the evidence in Table 5 shows that women, not just men, are highly responsive to changes in financial returns to schooling, with some gender differences across field of study choice.[15] 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 proportionally 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 the extensive margin. Instead, they increase their choice of some subjects (those subjects we show below to be more financially rewarding) at the expense of field of studies with 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, here we find that young adult women were highly responsive to the increase in financial returns to schooling.

[15]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.

6.4 Treatment Effect Estimates on Expected Wages

We next map fields of studies into expected earnings and show a positive effect of the reform on majors with higher expected earnings.[16] [17]

Table 6 shows that the effect of the pay reform is skewed towards BA degrees in fields with higher expected earnings. We define three different measures of expected wages. The first is actual 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 For each of these outcomes, we present pre and post cross section regression estimates and simple and controlled difference-in-differences estimates. We show evidence based on the full sample as well as for men and women separately. Focusing on the difference in differences estimates, we find that the pay reform expanded BA degree attainment in fields of study in the top quartile of the wage distribution, both for men and for women. The likelihood of obtaining an academic degree in fields with expected wages in the upper quartile of 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 is lower,[18] this absolute increase in expected earnings translates to higher proportional increase for women.

[16] 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.

[17] 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.

[18] 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.

6.5 Validation of the Causal Interpretation and Robustness Checks

We next show evidence of two sets of placebo treatment effect on pre-reform outcomes that were 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 end of high school outcomes: receiving a matriculation diploma, number of matriculation credit units, matriculation units in English and matriculation units in math. These results are presented in Table 7 for the full sample (columns 1-3), for men (columns 4-6) and for women (columns 7-9). All 12 controlled cross section estimates that are presented in columns 3, 6, and 9 are small and not statistically different from zero. Second, In Table 8 we present evidence from a placebo test in which we use a difference-in-differences model to compare two older cohorts who were less likely affected by the reform, namely individuals aged 22-27 in 1989, 1990 and in 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 that there were 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 that we presented in Tables 4, 6 to originate from the sub-sample of those who hold a matriculation diploma. We present these heterogeneity results in Table 9. The sample is split almost evenly between those who have and those who do not have a matriculation diploma. The estimates show that indeed 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 the effect on expected earnings is even more striking: 1078 NIS (se=397) versus 142 NIS (se=110).

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 as a whole. Admission to science-related departments typically requires high-level math in high school. The high school matriculation program is offered at three levels: basic, intermediate and advance. The latter is a pre-requisite for admission to engineering and computer science programs at all universities and most colleges that offer these programs. In Table 10, we present results for the two sub-samples defined by level of math in high school. We group the basic and intermediate math levels together and keep students with advance math in a second sample. As expected, Table 10 shows that most of the effect originates from students in the advance 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).

The analysis so far was based on a sample that included individuals age 22-27. For robustness, in Tables A8-A14 in 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, and balancing tests for the full sample and by gender. Overall, the treatment estimates obtained from this alternative age group are similar to those reported above based on the 22-27 age group.

Our evidence is not changed when using alternative identification strategies and where carrying multiple robustness checks. For example, we also 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 get similar results in this different controlled experiment even though this control group had much better pre-reform outcomes.[19] Therefore, unlike the first set of estimates that were based on late reforming kibbutzim as a control group and reflected only post-reform differences with perfect pre-reform balancing, the results using Tel-Aviv as a control group reflected partial narrowing of the pre-reform gap between treatment and control. These divergent patterns in the difference-in-differences estimates indicate that our treatment estimates are not driven by convergence to the mean following random shocks to outcomes in the treated kibbutzim.

7. Conclusions

This paper provides quasi-experimental evidence on the effect of changes in the skill premium on the propensity of young adults 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 very low rate of return to the level of the market-wide rate of 8-9 percent return to a year of schooling. Our setting thus 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. In particular, 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 evidence in the US context as "the anemic response of skill investment to skill premium growth," and

[19]The results are presented in Tables 11-12 and in online appendix tables A3-A7.

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." Our findings show, in the context of low initial returns, 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 to STEM fields of study. 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 lead to a substantial change in expected gender gap in earnings. However, more work on the occupation choice after schooling is needed in order 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 in fact switch to typically male-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, the majority of people who had the pre-requisites to be admitted to STEM subjects might have preferred to enroll in less financially rewarding majors or to not go to college at all. 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 life style that preceded the pay reform. 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),[20] the abolition of village collectives in China in the 1980s, the 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).

[20]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.

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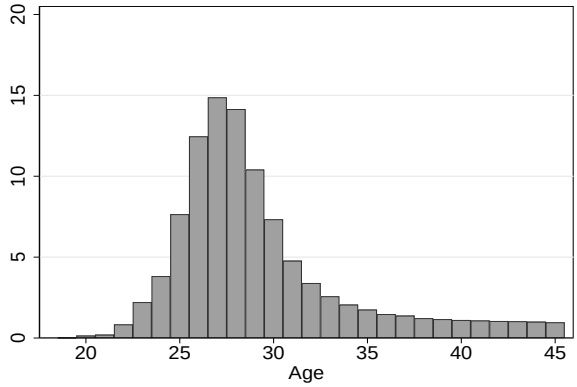
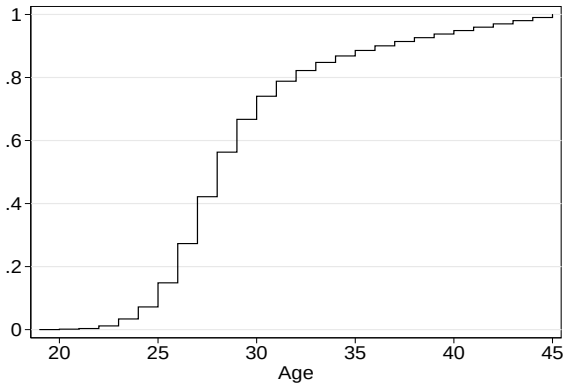
Zafar, B. (2013) College Major Choice and the Gender Gap. *Journal of Human Resources*, **48**, 545–595. URL: http://jhr.uwpress.org/lookup/doi/10.3368/jhr.48.3.545.

Figure 1: Distribution of BA Attainment By Age at Graduation

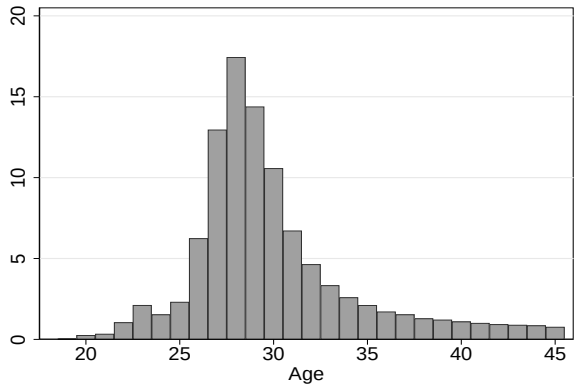
(a) *Full Sample PDF*



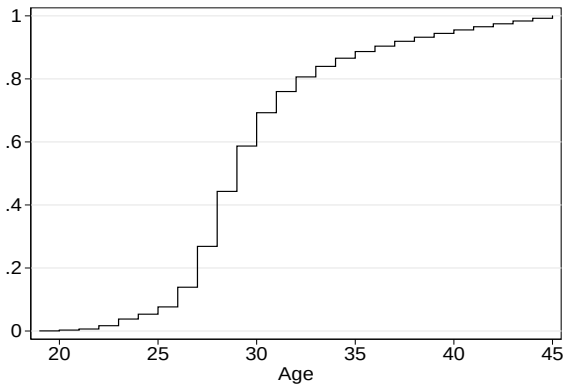
(b) *Full Sample CDF*



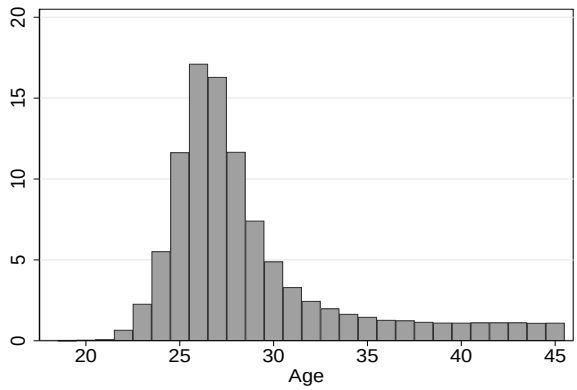
(c) *Male PDF*



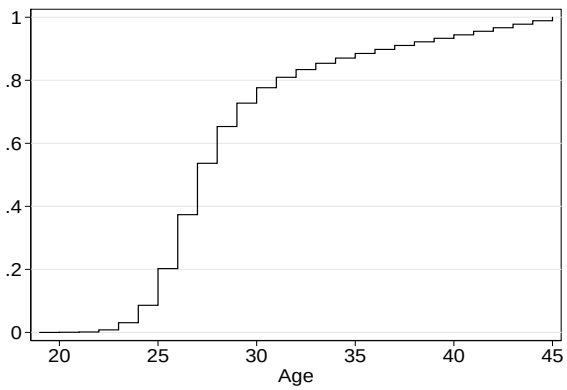
(d) *Male CDF*



(e) *Female PDF*

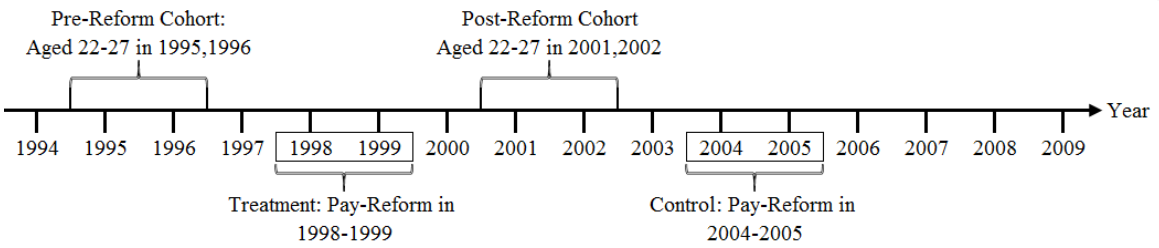


(f) *Female CDF*



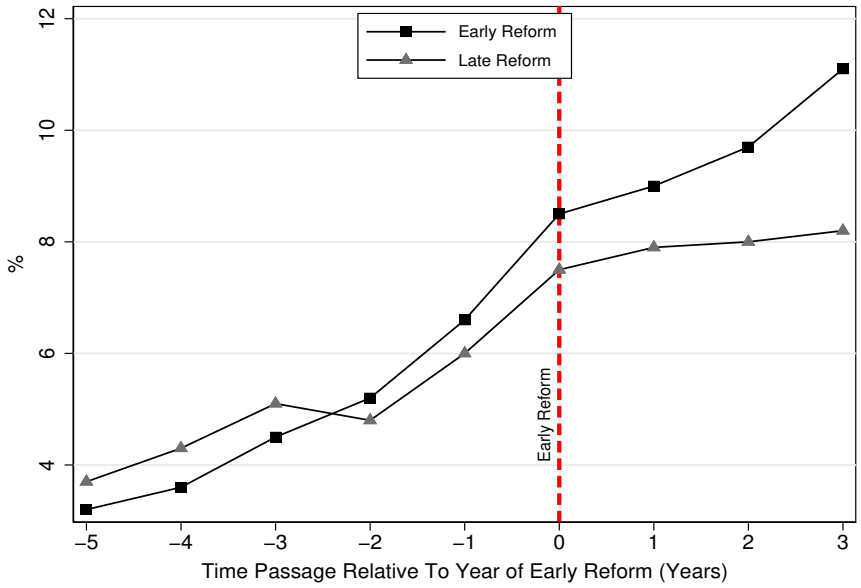
*N*otes: Figure 1 presents a PDF and CDF of the age at graduation for 1990-2015 Israeli Jews aged 18-45. Panel (a) & (b) present the distribution for the full sample (n=502,996), Panel (c) & (d) for Male (n=215,538) and Panel (e) & (f) for Female (n=287,458).
*S*ource: Central Bureau of Statistics, Israel.

Figure 2: Timeline of the Pay Reform



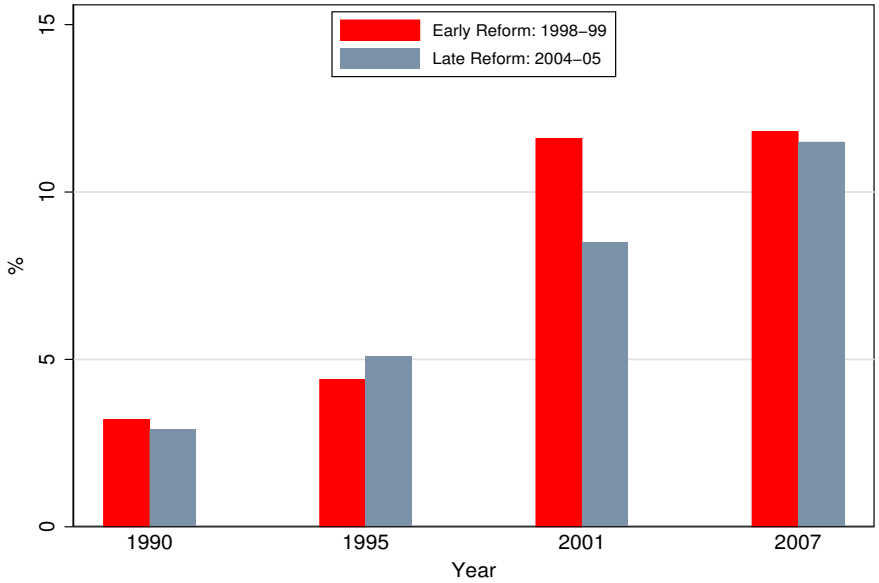
*N*otes: This figure presents the timeline of the pay reform for the selected treatment and control groups and the affected and unaffected cohorts relative to the time of the early and late reform. The treatment group includes kibbutzim that reformed early (1998, 1999) and the control group includes 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.



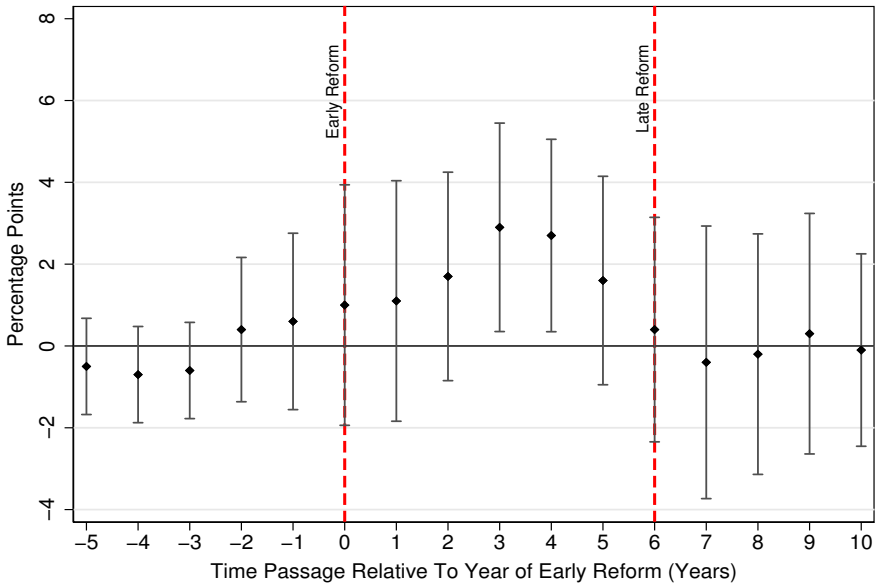
Notes: BA degree attainment rate for 22-27-year-old. The red vertical line represents the year of the early reform.

Figure 4: Proportion Receiving BA Degree



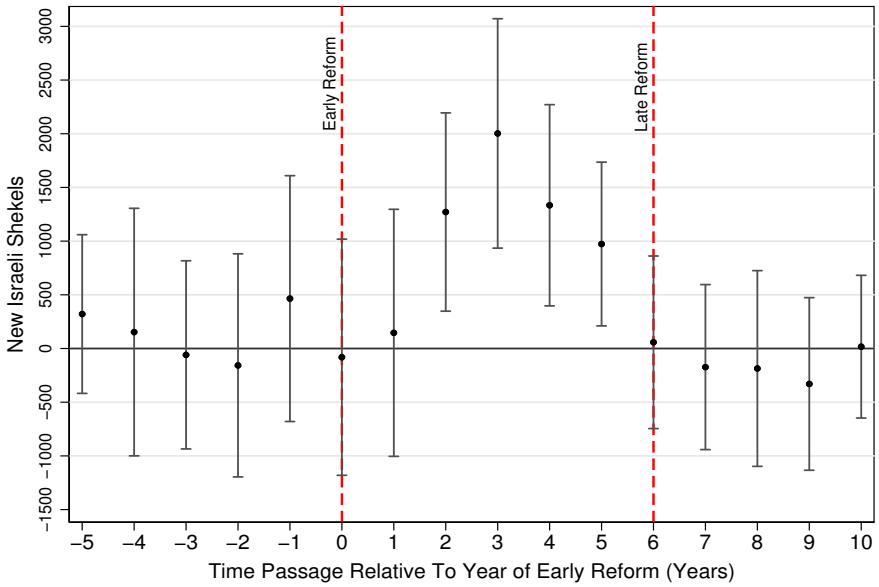
Notes: This figure presents the proportion of BA degree recipients of individuals who were aged 22-27 in four different years (before the early reform: 1990, 1995, after the early reform and before the late reform: 2001, after the late reform: 2007) for treatment group (early reform: 1998, 1999) and 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 Year



Notes: 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.

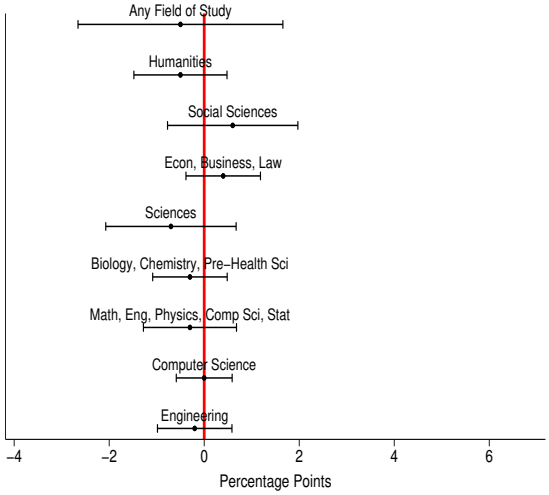
Figure 6: Treatment-Control Wage Differences at 2014, By Year



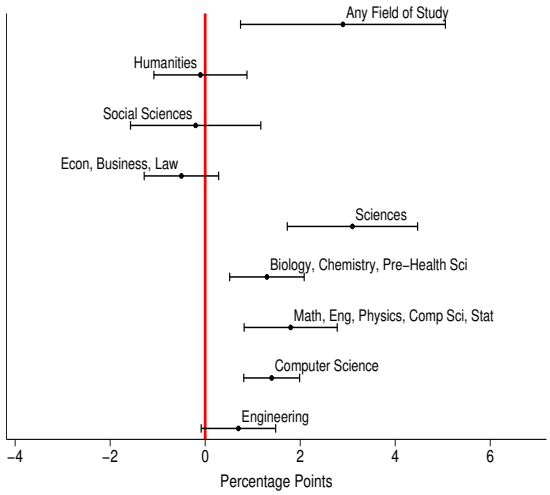
Notes: This figure presents treatment (early reform: 1998, 1999) - control (late reform: 2004, 2005) wage differences at 2014 of individuals who were aged 22-27 in each year, from five years before the early reform until ten years after it. The vertical bands represent 95% confidence interval.

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

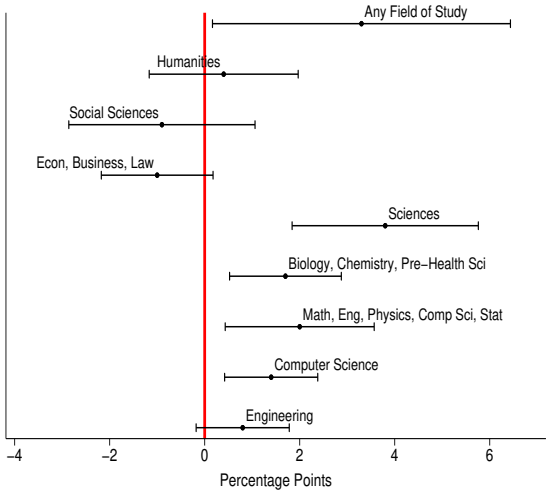
(a) *Pre-Reform*



(b) *Post-Reform*



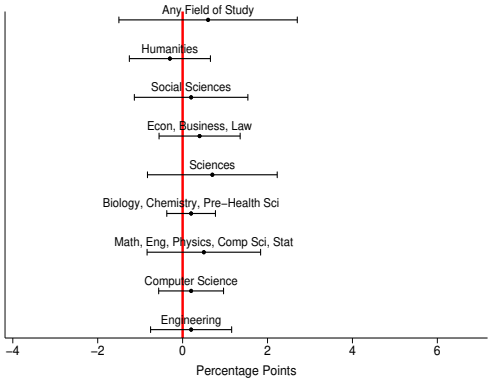
(c) *Difference-in-Differences*



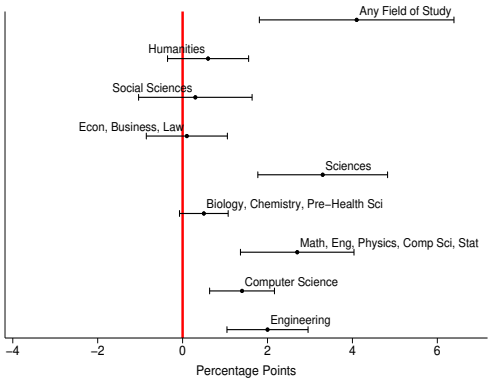
*N*otes: The points in this figure presents treatment-control differences (Panel A,B) and controlled difference-in-differences (panel C) estimates shown in Table 4. The horizontal bands represent 95% confidence interval.

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

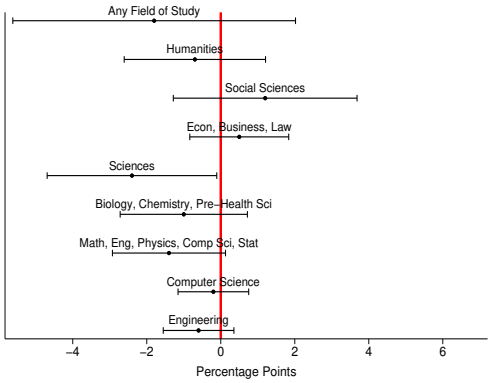
(a) *Male, Pre-Reform Cohorts*



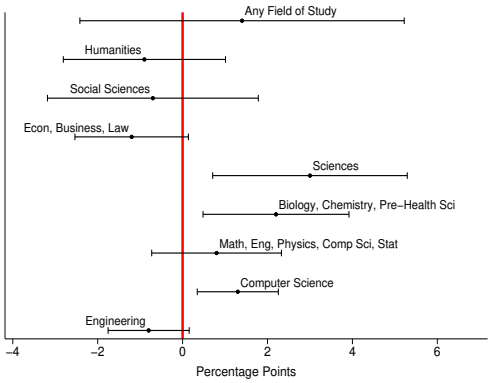
(b) *Male, Post-Reform Cohorts*



(c) *Female, Pre-Reform Cohorts*



(d) *Female, Post-Reform Cohorts*



Notes: The points in this figure present treatment-control differences estimates shown in Table 5. The horizontal bands represent 95% confidence interval.

Table 1: Balancing Tests of Individuals' 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			
	Proportions		Balancing Tests (T-C)		Proportions		Balancing Tests (T-C)	
	Treatment	Control	Coeff	*p-val*	Treatment	Control	Coeff	*p-val*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male	0.555 [0.497]	0.548 [0.498]	0.006	*0.764*	0.546 [0.498]	0.544 [0.498]	0.003	*0.896*
Age	24.366 [1.665]	24.410 [1.671]	-0.043	*0.508*	24.573 [1.710]	24.499 [1.689]	0.074	*0.317*
Number of Siblings	2.757 [1.291]	2.759 [1.290]	-0.002	*0.983*	2.645 [1.170]	2.611 [1.029]	0.034	*0.736*
Ethnic Origin: Africa/Asia	0.171 [0.377]	0.172 [0.377]	-0.001	*0.972*	0.093 [0.290]	0.107 [0.309]	-0.014	*0.436*
Ethnic Origin: Ethiopia	0.000 [0.000]	0.003 [0.052]	-0.003	*0.318*	0.000 [0.000]	0.006 [0.080]	-0.006	*0.133*
Ethnic Origin: FSU Countries	0.032 [0.176]	0.025 [0.155]	0.007	*0.484*	0.017 [0.128]	0.021 [0.145]	-0.005	*0.475*
Ethnic Origin: Europe/America	0.179 [0.383]	0.162 [0.369]	0.016	*0.689*	0.166 [0.372]	0.117 [0.321]	0.049	*0.102*
Ethnic Origin: Israel	0.562 [0.496]	0.553 [0.497]	0.009	*0.881*	0.654 [0.476]	0.662 [0.473]	-0.009	*0.830*
Ethnic Origin: Other	0.056 [0.230]	0.086 [0.280]	-0.030	*0.284*	0.071 [0.257]	0.086 [0.281]	-0.015	*0.495*
Observations	1035	1096	-	-	1025	1078	-	-
Kibbutzim	32	29	-	-	32	29	-	-

Notes: 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 [0.197]	0.046 [0.209]	-0.005	*0.405*	0.110 [0.313]	0.082 [0.274]	0.029	*0.025***
Humanities Any Field	0.013 [0.111]	0.017 [0.131]	-0.005	*0.318*	0.017 [0.128]	0.018 [0.132]	-0.001	*0.867*
Social Sciences Any Field	0.017 [0.131]	0.011 [0.104]	0.006	*0.230*	0.040 [0.196]	0.042 [0.200]	-0.002	*0.802*
Economics, Business, Law	0.008 [0.088]	0.004 [0.060]	0.004	*0.183*	0.011 [0.103]	0.016 [0.125]	-0.005	*0.211*
Sciences Any Field	0.011 [0.103]	0.017 [0.131]	-0.007	*0.134*	0.054 [0.225]	0.022 [0.148]	0.031	*0.000****
Biology, Chemistry, Pre-Health Sci	0.006 [0.076]	0.009 [0.095]	-0.003	*0.453*	0.020 [0.138]	0.006 [0.080]	0.013	*0.009****
Math, Eng, Physics, Computer Sci, Stat	0.005 [0.069]	0.008 [0.090]	-0.003	*0.318*	0.034 [0.182]	0.016 [0.125]	0.018	*0.024***
Computer Science	0.001 [0.031]	0.001 [0.030]	0.000	*0.318*	0.019 [0.135]	0.005 [0.068]	0.014	*0.000****
Engineering	0.002 [0.044]	0.004 [0.060]	-0.002	*0.318*	0.017 [0.128]	0.009 [0.096]	0.007	*0.243*
Observations	1035	1096	-	-	1025	1078	-	-
Kibbutzim	32	29	-	-	32	29	-	-

Notes: 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		Humanities Any Field		Social Sciences Any Field		Sciences Any Field	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Linear Trend Model								
Treatment	0.005	-	0.013***	-	-0.002	-	-0.006	-
	(0.007)	-	(0.004)	-	(0.004)	-	(0.005)	-
Time Trend	0.003***	0.003***	0.002***	0.002***	0.002***	0.002***	-0.001	-0.001
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Treatment X Time Trend	0.000	0.000	-0.002***	-0.002***	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
B. Cohort Dummies Model								
Treatment	0.004	-	0.009*	-	0.000	-	-0.004	-
	(0.010)	-	(0.005)	-	(0.006)	-	(0.006)	-
Treatment X 1990	-0.001	0.000	-0.001	-0.001	0.002	0.001	-0.001	-0.001
	(0.013)	(0.013)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)
Treatment X 1991	0.010	0.010	0.003	0.004	0.005	0.004	0.002	0.002
	(0.013)	(0.013)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)
Treatment X 1992	0.010	0.010	0.000	0.001	0.005	0.004	0.005	0.005
	(0.013)	(0.013)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)
Treatment X 1993	-0.009	-0.008	-0.009	-0.008	-0.001	-0.002	0.000	0.001
	(0.013)	(0.013)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)
Treatment X 1994	-0.012	-0.011	-0.016**	-0.014**	-0.001	-0.002	0.005	0.005
	(0.013)	(0.013)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)
Treatment X 1995	-0.012	-0.012	-0.012*	-0.011*	0.005	0.003	-0.004	-0.004
	(0.013)	(0.013)	(0.006)	(0.006)	(0.007)	(0.008)	(0.008)	(0.008)
Kibbutz Fixed Effects	NO	YES	NO	YES	NO	YES	NO	YES

Notes: This table presents results from OLS regressions where the dependent variable is an indicator of whether the student completed a BA degree (columns 1 and 2), completed a BA degree in humanities (columns 3 and 4), completed a BA degree in social sciences (columns 5 and 6), completed a BA degree in sciences (columns 7 and 8). The sample includes individuals aged 22-27 in each year from 1989 to 1995 (pre reform). The treatment group includes kibbutzim that reformed in 1998, 1999, and the control group includes kibbutzim that reformed in 2004, 2005. In the regressions results reported in panel A, outcomes are allowed to vary according to a linear time (cohort) trend that differs in treatment and control kibbutzim. The regression in panel B includes cohort dummies. Standard errors clustered at the kibbutz level are presented in parentheses. ***, **, * indicate significance at 1%, 5% and 10%.

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	Biology, Chemistry, Pre-Health Sci	Math, Eng, Physics, Comp Sci, Stat	Computer Science	Engineering
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
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
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)
Observations	4233	4233	4233	4233	4233	4233	4233	4233	4233

Notes: 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 consists 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: 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	Biology, Chemistry, Pre-Health Sci	Math, Eng, Physics, Comp Sci, Stat	Computer Science	Engineering
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Male									
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
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									
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
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

Notes: 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). Treatment group consists of kibbutzim that reformed in 1998, 1999. 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 6: Effect of Pay Reform on BA Degree Attainment by Expected Wages and Gender

(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)
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)
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

Notes: 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. 1US 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 studies with expected wages between the different quartile. 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: 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 7: Placebo Experiments, Effects on Pre-Determined High School Matriculation Outcomes

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

	Full Sample			Male			Female		
	Treatment Mean (1)	Control Mean (2)	Controlled Differences (3)	Treatment Mean (4)	Control Mean (5)	Controlled Differences (6)	Treatment Mean (7)	Control Mean (8)	Controlled Differences (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	-

Notes: 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 row 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. ***, **, * indicate significance at the 1%, 5%, and 10% levels respectively.

Table 8: 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	Biology, Chemistry, Pre-Health Sci	Math, Eng, Physics, 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

Notes: 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 9: 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)
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)
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	-	-

Notes: 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 studies with expected wages between the different quartile. The outcome "Expected Wages" is continuous, and the measurement unit is New Israeli Shekels per month. 1US 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). Difference in means significant at ***1% **5% *10%.

Table 10: 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)
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		0.034*	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)
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	-	-

Notes: 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 studies with expected wages between the different quartile. The outcome "Expected Wages" is continuous, and the measurement unit is New Israeli Shekels per month. 1US 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). Difference in means significant at ***1% **5% *10%.

Table 11: 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	Biology, Chemistry, Pre-Health Sci	Math, Eng, Physics, Comp Sci, Stat	Computer Science	Engineering
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Cross Section Regressions									
Pre-Reform	-0.066***	-0.010**	-0.039***	-0.031***	-0.017***	-0.006**	-0.012**	-0.002	-0.005
	(0.011)	(0.005)	(0.008)	(0.006)	(0.006)	(0.003)	(0.005)	(0.003)	(0.004)
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)
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	0.036**	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	91563	91563	91563	91563	91563	91563	91563	91563	91563

Notes: 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 includes only cohort dummies. The controlled difference-in-differences regressions includes 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 12: Tel-Aviv as a Control Group, Effect of Pay Reform on BA Degree Attainment by Expected Wages and Gender

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

	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)
A. Cross Section Regressions									
Pre-Reform	-336.1***	-399.6***	-268.7***	-0.026***	-0.021***	-0.031***	-0.037***	-0.036***	-0.038***
	(75.55)	(119.5)	(84.8)	(0.006)	(0.008)	(0.010)	(0.008)	(0.011)	(0.013)
Post-Reform	72.74	76.74	61.98	0.000	0.007	-0.008	0.003	-0.003	0.011
	(75.14)	(118.6)	(84.5)	(0.006)	(0.008)	(0.010)	(0.008)	(0.010)	(0.013)
B. Difference-in-Differences									
Simple	408.8***	476.3***	330.7***	0.026***	0.028**	0.023*	0.040***	0.034**	0.049***
	(106.6)	(168.4)	(119.7)	(0.009)	(0.012)	(0.014)	(0.011)	(0.015)	(0.018)
Controlled	402.0***	471.2***	345.9***	0.025***	0.027**	0.025*	0.040***	0.033**	0.049***
	(107.2)	(171.3)	(120.9)	(0.009)	(0.012)	(0.014)	(0.011)	(0.015)	(0.018)
Observations	91,563	48,579	42,984	91,563	48,579	42,984	91,563	48,579	42,984

Notes: 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). The treatment group consists of kibbutzim that reformed in 1998, 1999. The control group consists of individuals who lived in Tel-Aviv. In columns 1-3 the dependent variable is continuous and the measurement unit is New Israeli Shekels per month. 1US dollar is currently equal to approximately 3.7 shekels. the dependent variable in columns 4-9 is an indicator of whether the student completed BA. in a field of studies with expected wages between the different quartile. 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: 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.