The Effect of Economic Inequality on Assortative Matching: The Formation and Dissolution of Marriages

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Abstract:

The increase in assortative matching in marriage markets observed across many nations worldwide is a contributing factor to rising income inequality. This paper suggests that the causal chain also runs in reverse: deepening labor market inequality could trigger greater assortative matching in the marriage market. To establish causality, we study the Israeli kibbutzim that transitioned from equal sharing to market economies. A reform that followed a staggered adoption pattern across kibbutzim abolished egalitarian income sharing, generating inequalities by linking wages to education for the first time. This enabled us to conduct a series of difference-in-differences analyses to examine the impact of the reform on divorce and marriage patterns. First, we find that the rise in economic inequality led to divorce among couples with unequal education, but only when the wife was more educated than the husband. This finding is consistent with a violation of the norm that dictates the husband should be the primary breadwinner. Second, we find that the reform increased assortative matching in education, leading to a significant decrease in educational differences within newly married couples. Importantly, we find that assortative matching existed in the kibbutzim even before liberalization, when earnings were not related to education. This suggests that assortative matching on education is not only driven by income, but also by a preference for marrying a partner who is similarly educated. Overall, we conclude that assortative matching increased following the reform, both through the formation of new marriages and the selection of spouses, as well as the dissolution of existing marriages. These results demonstrate how increased labor market inequality may impact inter-household inequality by increasing assortative matching in the marriage market.

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1. Introduction

The deepening of economic inequalities is one of the central forces shaping the trajectory of modern societies (Piketty, 2014; Saez and Zucman, 2020). Understanding the mechanisms behind rising inequality is a core question in economics. One important mechanism is assortative mating, the tendency for individuals to marry others with similar education or income. When high-income or highly educated individuals predominantly marry each other, differences between households are amplified. Empirical evidence shows that assortative mating has increased in several countries, including the US, UK, Germany, Denmark, and China (Eika et al., 2019; Almar et al., 2025; Gao, 2025). For instance, Eika et al. (2019) estimate that marital assortative matching accounted for 5% of the American Gini coefficient in 2013, while Almar et al. (2025) find that it explains roughly 40% of the increase in Danish inequality since 1980.

Modern economic theory interprets assortative mating through the lens of marriage-market models with transferable utility (Becker, 1973, 1981; Choo & Siow, 2006; Chiappori, Salanié, & Weiss, 2017). In these models, individuals match to maximize the household's joint surplus. Positive complementarities between education, earnings potential, or social traits generate equilibrium assortative mating. Changes in the economic environment—such as rising returns to education—alter the surplus associated with different matches and therefore the equilibrium sorting pattern (Chiappori et al., 2025).

While most research focuses on the contribution of assortative mating to inequality, this study also examines the reverse causal channel: whether exogenous changes in economic inequality alter assortative mating. When the returns to education increase, highly educated individuals become more desirable partners, leading the equilibrium marriage market to favor matches between similarly educated spouses (Burdett & Coles, 1997; Fernandez & Rogerson, 2001). Theoretically, this follows from matching models: an increase in the marital surplus associated with a high-high match relative to a mixed match shifts the distribution of marriages even if preferences themselves remain unchanged (Chiappori et al., 2025).

To illustrate, consider the transition from a more egalitarian to a market-based economy. In the Eastern Bloc after 1989, Poland experienced dramatic institutional and labor-market changes. Using census data (Ruggles et al., 2024), Figure 1 shows that assortative matching in Poland fell during the communist era but rose after liberalization. Following Abramitzky et al. (2011), we measure assortative matching as the ratio of the actual share of couples

with identical education to the share expected under random matching. This measure captures departures from random matching and is consistent with modern empirical work on marriage markets (Chiappori et al., 2025). The observed lag after 1989 aligns with marriage planning and the gradual implementation of liberalization reforms.

While suggestive, these patterns do not establish causality. Many changes occurred simultaneously in Poland in the late 1980s. Despite the literature documenting correlations between economic institutions and marriage outcomes → causal evidence linking exogenous changes in inequality to assortative matching remains scarce (Chiappori et al., 2025).

We address this gap by exploiting a quasi-experimental shock: the liberalization of Israeli kibbutzim. Founded as egalitarian communities in the early 20th century, kibbutzim pooled all labor income and distributed it equally among members. Starting in the mid-1990s, a series of reforms abolished this egalitarian system and introduced differential wages linked to market returns (Abramitzky, 2008, 2018; Abramitzky, Ben-Porath & Lavy, 2025). These reforms generated within-community inequality in education returns, providing exogenous variation to study the impact of income inequality on marital sorting.

The reform substantially increased the returns to education. Under the prior egalitarian system, highly educated members received the same income as less educated members; after reform, they out-earned the rest (Abramitzky & Lavy, 2014). In marriage-market terms, the reform increased the marital surplus associated with high-high matches, allowing us to test whether changes in economic gains translate into changes in assortative mating.

We examine two channels. First, we study divorce among existing marriages. The reform created within-couple income differentials overnight, particularly affecting couples with unequal education. Using a staggered Difference-in-Differences design, we find that divorce increased among couples in which the wife was more educated than the husband, consistent with the literature on breadwinning norms and marital stability (Bertrand et al., 2015; Bursztyn et al., 2017; Folke & Rickne, 2020). This aligns with the matching-surplus framework: matches whose realized joint surplus falls below the outside option dissolve.

Second, we study assortative matching in new marriages. Even before reform, highly educated individuals were more likely to marry each other, indicating that non-economic factors (shared interests, social status) contribute to sorting. After reform, the probability that a highly educated member married another highly educated individual doubled. The

magnitude of this increase is consistent with predictions of the matching model: increasing economic returns to education increase the equilibrium prevalence of high-high matches.

We address potential confounders, including endogeneity of education, by restricting the sample to individuals too old to alter their schooling after the reform. Results remain robust, reinforcing the causal interpretation.

Our study contributes to several literatures. First, we contribute to the literature on assortative mating and inequality (Gary Becker, 1981; Greenwood et al., 2014; Eika et al., 2019; Chiappori et al., 2025). Much of this literature studies how assortative mating amplifies inequality by concentrating human capital and earnings within households. Our contribution is to provide causal evidence on the reverse mechanism: we show that an exogenous increase in the returns to education and within-community income inequality leads to stronger assortative matching. By exploiting the staggered liberalization of Israeli kibbutzim, we demonstrate that assortative matching responds to changes in the economic value of education through both the formation of new marriages and the dissolution of existing marriages, highlighting an important feedback mechanism between inequality and household formation.

Second, we contribute to the literature on the interaction between education returns and marriage-market outcomes (Lafortune, 2013; Andrew and Adams, 2025). Prior research has shown that conditions in the marriage market can influence individuals' incentives to invest in education. Our findings document the reverse channel: when the labor market returns to an increase in education, individuals adjust their partner choices in the marriage market. The kibbutz reform provides a unique setting in which the returns to education increased sharply within a previously egalitarian system, allowing us to isolate the role of economic incentives in shaping marital sorting.

Third, we contribute to research on marital stability and breadwinning norms (Bertrand et al., 2015; Bursztyn et al., 2017; Folke and Rickne, 2020). We show that the reform increased divorce rates primarily among couples in which the wife was more educated than the husband. This pattern is consistent with theories emphasizing the role of gender norms in marriage, according to which deviations from the traditional male-breadwinner structure may generate tensions within households. By exploiting an exogenous shock to spouses' relative earnings potential, our setting provides new evidence on how changes in economic incentives interact with social norms to affect marital stability.

Finally, our research adds to the economic history literature on the Israeli kibbutzim (Abramitzky, 2007, 2008, 2018; Abramitzky and Lavy, 2014; Ebenstein et al., 2016). The kibbutz reforms represent one of the most dramatic transitions from an egalitarian to a market-based system within a modern society. By examining how this transition affected marriage formation and divorce decisions, our study sheds light on how institutional changes and the emergence of income inequality reshape family structure and marital sorting within communities.

The remainder of the paper is organized as follows. Section 2 provides a historical review of the kibbutzim and describes the data. Section 3 details the datasets and sample construction. Sections 4 and 5 present results on divorce and marriage, respectively, including descriptive statistics, methodology, and robustness checks. Section 6 concludes.

2. Historical Background

Established mostly in the 1930s and 1940s (with the first established in 1910), the Israeli kibbutzim are voluntary communities where members have lived with relatively high income and low inequality for decades. Throughout most of the 20th century, kibbutzim functioned as semi-independent economic units, with the majority of members working within the kibbutz and the kibbutz employing only a small number of laborers from outside the community. Starting in the mid-1980s, especially in the early 1990s, the kibbutzim's economic system began facing growing pressures. Not only did they recover from a major financial crisis, but they also faced declines in agricultural goods prices, bad financial management, and a high-tech boom that drew members away from kibbutzim (Abramitzky, 2018).

To address these pressures, kibbutzim have implemented reforms that have radically transformed their economy. Of greatest importance were reforms that replaced egalitarian subsidies with a competitive market-based wage system. People working outside kibbutzim (approximately one-fourth of all members) no longer surrendered their income, and those working within kibbutzim began earning compensation determined by the kibbutzim based on the market value of their marginal product.

Each kibbutz decided, by democratic vote, if and when to implement this labor market reform. Consequently, different kibbutzim adopted it at different times. As of 2022, there were 265 kibbutzim throughout Israel, with a total of 200,000 members, accounting

for less 2% of Israel's Jewish population. Of these kibbutzim, all but roughly 40 have adopted the reform. In this paper, we disregard 16 religious kibbutzim, which differ in several dimensions.

Throughout their history, kibbutzim prided themselves on having egalitarian gender norms. Women were expected to work and participate in the democratic process in the kibbutzim. However, reality often did not align with ideology, and women were frequently relegated to specific tasks in both the labor force and administration (Abramitzky, 2018).

A special institution that kibbutzim adopted was the joint sleeping arrangement. Children in the kibbutz did not sleep in their household, but in a kibbutz children's house. The motivation for this arrangement was to reduce inequality between households and cultivate shared values among children. Another related goal of this communal sleeping arrangement was to promote gender equality between women and men. Part of the vision of kibbutz ideology was the liberation of women from their traditional roles as mothers and housekeepers. The 'children's house' institution meant to enable women to work equally in the kibbutz, instead of being tied exclusively to the home and childcare.[4] Starting in the late 1970s and mostly throughout the 1980s, kibbutzim shifted away from this arrangement to the traditional familial sleeping arrangement. By the beginning of our data collection in 1995, none of the kibbutzim still had communal sleeping arrangements.

3. Data

This paper utilizes three primary datasets. The first dataset includes, for each kibbutz, the year of the reform, i.e., the year it liberalized its labor market and transitioned to a wage-based economy. This variable was collected by the Institute for the Research of the Kibbutz and the Cooperative Idea at the University of Haifa. It has been previously used in other studies (e.g., Abramitzky et al., 2025). We use this variable to construct the treatment event. Figure A1 plots for every year spanning 1995-2020, the number of reformed and unreformed kibbutzim.

[4] However, in actual practice, the ideal of full gender equality was only partially realized. Despite the ideology, many women still found themselves concentrated in "women's jobs" (education, childcare, cleaning) while men held more of the leadership and agricultural roles [add references].

The second dataset is the Israeli Civil Register. This dataset provides detailed demographic information for every resident in Israel for the years 1995, 2000, and 2002-2018. Specifically, the data include information on the place of residence, marital status, age, religion, ethnicity, and sex. Importantly for our endeavors, the Register allows us to link each individual to the person they were married to as of 2018.[5] We restrict our attention to Israeli Jews since the share of non-Jews in kibbutzim is negligible.

Lastly, we use the *Educational Census* compiled by the Israeli Central Bureau of Statistics (CBS). This dataset combines various sources to list for almost all Israeli residents the highest educational diploma they have received.[6] The Educational Census classifies diplomas into seven different types: elementary schooling, secondary schooling, matriculation (*bagrut*), post-secondary non-academic, BA, MA, and PhD. For some exercises, we use these categories, while for others, we transform them into a continuous measure of approximated schooling years.[7]

Combining these three datasets enables us to generate a panel dataset of kibbutz members with near-annual frequency spanning 1995-2020 (omitting 1996-1999 and 2001). Each kibbutz member is linked to their kibbutz, allowing us to assign treatment (the year in which the kibbutz reformed). Since we know where every member lived, we can exclude people who migrated away from the kibbutz each year. The data's frequency also allows us to determine when each couple married and, if they divorced, when. Finally, this rich dataset enables us to identify the educational gap between the kibbutz member and their spouses.

4. Theoretical and Empirical Framework

5. Divorce Patterns

In this section, we examine the impact of changes in the market value of education on marital dissolution. Our sample comprises all couples who married in a kibbutz *before* the reform, but at or after the year 2002. We start at 2002, because we do not have information

[5] We observe only spouses in 2020. This means we must exclude re-married individuals, because we do not know who they were married to before the latest spouse.
[6] Since the year of attainment is included only for a partial subsample, we do not use this variable.
[7] We convert the 7 diplomas we have in the data to the following schooling years: Middle-School: 9, High-School: 12, Matriculation: 12, Post-Secondary: 14, BA: 15, MA: 17, PhD: 21.

for the years 1996-1999, and 2001. We ensure that couples are kibbutz members by only considering couples for which at least one of the partners lived in a kibbutz in 1995.[8] If a kibbutz did not reform, we would keep all married couples until the year 2020. The couples enter the sample when they are married and remain in it until 2020. We require that couples be aged 28 or older at the time of marriage, to ensure that education is not endogenous to their decision to match. We define the divorced outcome variable to be 0 if the couple is still married. However, if and when they divorce, the outcome variable changes to 1 and remains at 1 until the end of the sample. Overall, our sample includes 14,477 couples observed in 350,798 different year-couple observations.

Table XXX presents descriptive statistics of this sample. In particular, we provide information on the means and standard deviations of some key demographic variables, including year of birth, age at marriage, likelihood of marrying a kibbutz member, and ethnicity. In Table B1, we present the entire joint distribution of husbands' educational attainment in our sample. Overall, 4,719 couples had the same level of education. In 3,828, the husband was more educated, and in 5,930, the wife was more educated. These numbers indicate that, in our sample, women were more educated than men.

We begin by presenting a graph illustrating the main results of this section. We classify each couple-year observation as either before or after the reform implemented in their kibbutz. The classification depends on the kibbutz the couple lives in, its reform year, and the year they are observed in the study period. We further stratify the sample into three groups: couples in which the wife is more educated, those in which the husband is more educated, and couples with identical education levels. Figure 2 plots the divorce rate of these six groups within the entire kibbutzim sample.

Evidently, the divorce rate in reformed kibbutzim is higher by roughly 4-5 percentage points. As we show later, this is not driven by the reform increasing divorce rates in all groups. Rather, this pattern is driven by time confounding the reform; over time, the share of divorced individuals increases in our sample. The divorce rate is comparable across the three groups before the reform; however, it is slightly higher when the wife is more educated. In contrast, in the post-reform period, divorce rates are significantly higher when the woman is more educated, compared to other groups. For

[8] Some individuals lived in the kibbutz, but were not kibbutz members. This meant even though their residence was within the kibbutz, they were not part of the cooperative arrangement of complete redistribution. Since these people joined kibbutzim almost exclusively after 1995, using the place of residence in 1995 allows ensuring they are not part of the sample.

couples in which the wife is more educated than the husband, the increase in the divorce rate is 6 percentage points; in the groups where the wife is equally educated as the husband or less educated than him, the corresponding figure is 3-4 percentage points. Hence, this evidence indicates that in couples in which the reform favored the wife over the husband, marital stability was hindered.

To rigorously demonstrate this result, we estimate a staggered DiD specification, exploiting the differential years in which the reform was adopted across kibbutzim. In our specification, the outcome at the couple-year level is an indicator of divorce in that year. On the right-hand side, we include a variable equal to 1 if and only if the kibbutz the couple lives in was reformed before the year. We interact this variable with indicators for three types of couples: the wife is more educated, the husband is more educated, and equal education. We control for time and kibbutz fixed effects. We note that the sample is unbalanced because couples are included only when they marry. Formally:

$$Divroce_{ckt} = \alpha + \sum_{j=1} (\beta_j Reformed_{kt} \cdot Gap_{j(c)} + \gamma_j \cdot Gap_{j(c)}) + \tau_t + \kappa_k + \epsilon_{ikt}$$

Where $Divroce_{ckt}$ is a dummy for whether the couple c living in a kibbutz k is divorced at year t. $Reformed_{kt}$ is a dummy variable for the kibbutz k implementing the reform before the year t. $Gap_{j(c)}$ is a categorical variable, partitioning the sample of couples into three categories: (1) the husband is more educated than the wife, (2) the spouses have the same education, (3) the wife is more educated than the husband. τ_t and κ_k are year and kibbutz fixed effects, capturing the local time-invariant divorce rate, and the joint trend in the divorce rate. The coefficients of interest are $\beta_1, \beta_2, \beta_3$ which capture the reform's differential effect by the education level. Standard errors are clustered at the kibbutz level.

The coefficients are presented in Table 2, column (1). The estimates in this column suggest that the reform did not increase the propensity to divorce among couples in which the husband is more educated (group 1) and among couples where both partners have the same level of education (group 2). The estimated coefficients are small in magnitude, and the confidence intervals allow ruling out any positive effect beyond a marginal one. These results confirm that the post-reform increase in the divorce rate observed in Figure 2 for couples where the wife is less educated is entirely attributable to time.

In contrast to groups (1) and (2), the reform increased the propensity to divorce by 2.21 percentage points among couples where the wife was more educated (group 3). This is a 20% increase over the pre-reform baseline of 10% divorce rate among couples in group 3. Moreover, the effect size of roughly 2 percentage points is comparable to the gap observed in Figure 2. This suggests that the simple exercise of taking means does not seem to be heavily biased. Overall, we conclude both from Table 2 and Figure 2 that the reform had a substantial adverse impact on marital stability when the wife was more educated than the husband.

In the remaining columns in Table 2, we stratify the sample by the couple's joint educational status. The results are null when both the wife and the husband are academics, meaning the reform hurt marital stability only when the wife was more educated than a non-academic husband. We believe this pattern stems from the fact that when both spouses hold at least a BA degree and the woman has a higher degree, such as an MA or PhD, her income is unlikely to be significantly higher than that of the man. Such a situation can result, for example, when a husband and wife hold academic degrees in different fields of study, each with varying rates of return on schooling and therefore different income levels. An example is the husband completing a BA degree in engineering or computer science while the wife is pursuing an MA or PhD in the humanities.

The estimates in columns 3-4 show that the reform increased the divorce rate among couples in which both partners' educational attainment is below an academic degree, and among couples in which the wife has an academic degree and the husband does not. In these couples, it is indeed likely that the educational gap translates directly to income gaps.

Figure 3 demonstrates the event studies corresponding to column (1) in Table 2. We estimate these event studies by splitting the sample into three groups based on the educational gap. The only difference in the specification compared to Equation (1) is that the $Reformed_{kt}$ dummy is replaced with a series of period dummies capturing the difference between the year and the reform year in the kibbutz. These event studies clearly show that the parallel pre-trends assumption holds. They further exemplify that the effect of the reform starts quickly (from year 1) and lasts a long time.

5. Marriage Patterns

5.1: Sample and Descriptive Statistics

In this section, we examine the formation of new marriages. We include couples in the sample once they are married. We require that both the husband and the wife be at least 28 years old at the time of marriage, to ensure educational attainment is completed before the marriage decision. We exclude married individuals aged 45 or older, because we believe their marital considerations may be significantly different from those of the rest of the sample. Because we only know the characteristics of the last spouse, in the benchmark specification, we exclude individuals who have ever been divorced or widowed. Finally, we proceed to omit 96 couples for which the education of one of the spouses is unknown.

To identify kibbutz members, we restrict our attention to people who lived in a kibbutz in 1995, the first year of our analysis period (and data collection). We also require that, at the time of marriage, both the member and their spouse lived in the same kibbutz as the member in 1995. The choice of 1995 is convenient, as it precedes both reform and the start of the construction of expansion neighborhoods in kibbutzim whose residents were not kibbutz members.[9] This sample restriction prevents the inclusion of non-kibbutz members in the study sample.

The ultimate dataset we use for the analysis in this chapter includes every couple, observed once, in the year of marriage. We focus on the years 2000-2020, with specific years varying depending on the exercise we execute. Overall, our sample includes XXX people in kibbutzim who have ever reformed, XXX people in kibbutzim who have never reformed, and XXX couples who are not defined as kibbutzim members. Appendix Table B1 summarizes for every group and by sex, a list of observables: birth year, age (at marriage), sex, likelihood of marrying a kibbutz member, and ethnicity. Appendix Figure A2 plots by group and sex the share of people having every diploma. Table 3 presents descriptive statistics for the sample analyzed in this section.

5.2: Graphical Analysis

We start with graphical evidence. We divide the sample into the same three groups: non-kibbutzim, reforming kibbutzim, and never-reformed kibbutzim. The reforming

[9] From the late 1990s, some kibbutzim have started gradually building extension neighborhoods, adjacent to the kibbutz, for non-kibbutz members. Data we collected from 66 kibbutzim show that merely 14 have started building extensions until 2003, only one before 1998. Note that we have data on the beginning of the construction of the extensions and populating them is likely to happen several years later.

kibbutzim include those that reformed between 2001 and 2005. Drawing on Abramitzky et al. (2011), we calculate the absolute difference in schooling years between the husband and wife for each couple. For example, the husband completed high school while the wife has an MA, the difference would be 5 years. Then, we collapse this average difference by group and period. We consider two periods: 1996-2000, the pre-reform period, and 2007-2011, the post-reform period. We choose these periods because 1996-2000 is the first set of years we can consider as a pre-period.

Figure 4 presents the average difference in years of schooling between spouses for each of the three groups, both before and after the reforms. The left-hand first three bars correspond to the pre-period, and the right-hand last 3 to the post-period. The blue denotes non-kibbutzim, which we view as an always-treated group. The red denotes reforming kibbutzim, which are the treatment group. Lastly, the green denotes never-reformed kibbutzim, serving as a control group that has never been treated. The confidence intervals are of one standard deviation. A higher number corresponds to less assortative matching.

Consistent with egalitarian economies having less assortative mating, during the pre-period, the average gap in schooling years between married kibbutz members was roughly 2, compared to 1.75 for non-kibbutzim Israelis. This was true for both never-reformed and reforming kibbutzim, demonstrating that the treatment and control groups are well balanced with respect to pre-reform assortative matching. In the post-period, there is hardly a difference in the patterns in the reformed kibbutzim. However, in the two control groups, there is a trend towards less assortative mating. Consequently, in relative terms, the assortative mating increased in the treatment group. Strikingly, the mean assortative matching in reformed kibbutzim converged to equal the rate in the non-kibbutzim population in Israel. Hence, we conclude that following the labor market reforms in kibbutzim (which increased the rate of return to schooling from zero to the market rate of about 8 percent, Abramitzky and Lavy 2014), the trends of assortative matching in reformed kibbutzim converged to the pattern observed in the always-treated group.

We consider the within-couple difference in years of schooling used in Figure 4 to be our benchmark evidence of the reform's effect on assortative matching in the marriage market for two reasons. First, it enables us to use the full and granular educational information we have. Second, it does not require us to make any assumptions about the

structure of the marriage markets. However, to ensure robustness, we consider two additional outcomes.

In Figure 5, Panel A, we use the difference in log schooling, instead of linear schooling. This ensures that changes in the distribution of upper-tail education do not drive the results. In the same Figure, Panel B, we use the odds ratio, which Chiappori et al. (2025) find attractive. The results hold with both of these modifications.

Section 5.3: Difference-in-Differences (DiD) Estimates

While Figure 2 captures one of the main findings in this paper, it does not fully utilize the variation in our settings regarding the timing of treatment. In this section, we implement a staggered DiD specification estimation using an individual-level specification. To this end, we construct a dummy variable that indicates the benchmark of whether the kibbutz member's spouse has an academic education (a college or university BA or higher). Clearly, this outcome variable does not capture assortative matching but rather the propensity to marry someone highly educated. To make this framework fit an analysis of assortative matching, we estimate the heterogeneous effect across two samples. The first includes individuals with an academic degree, and the second includes individuals with a non-academic degree. We interpret the difference between these two estimates as a statistic capturing a change in assortative mating. Formally:

$$AcademicSpouse_{ikt} = \zeta_o + \zeta_1 \cdot Reformed_{kt} \cdot Academic_i + \zeta_2 \cdot Reformed_{kt} \cdot NonAcademic_i + \tau_p + \kappa_k + \epsilon_{kt}$$

Table 5, Panel A, columns (1) and (2) present the staggered Diff-in-Diff estimation results, with an effect estimated heterogeneously for academics and non-academics. In each column, the sample includes kibbutz members from one gender. In columns (3) and (4), we repeat the procedure, using years of schooling, rather than a discrete dummy for being an academic. We estimate that the gap in the likelihood of marrying an academic woman between academics and non-academic men increases by 28% following the reform. For women, the gap widens to 34%. The estimates, which use years of schooling rather than an indicator of academic education, show the same pattern. The corresponding increases in the gaps in the spouse's number of school years are 1.32 and 1.58.

In Panel B, we replicate that analysis, dropping the year 2000. Since the year 2000 captures people who married between 1996 and 2000, dropping it ensures our entire sample is annual. Panel C drops never-reformed kibbutzim, to ensure that underlying differences between them do not drive the results, and they were ever-reformed. Panel D includes only couples in which both members were kibbutz members in 1995. This ensures that results are not driven by changes in the propensity to marry outside the kibbutzim movement. In all alternative panels, the results grow in magnitude. In Panel D, the differences between men and women are negligible by construction.

Figure 3 plots an Event Study that pools the two sexes, in a specification corresponding to Table 5, Panel A, columns (1)-(2). The upper Panel shows the results for individuals with an academic education, while the lower Panel shows the results for those without one. Overall, the event study evidence aligns with the results presented in Table 5. All the point estimates in the first sample are positive, some statistically different from zero. And all the point estimates in the second sample are negative and mostly insignificant.

6: Conclusions

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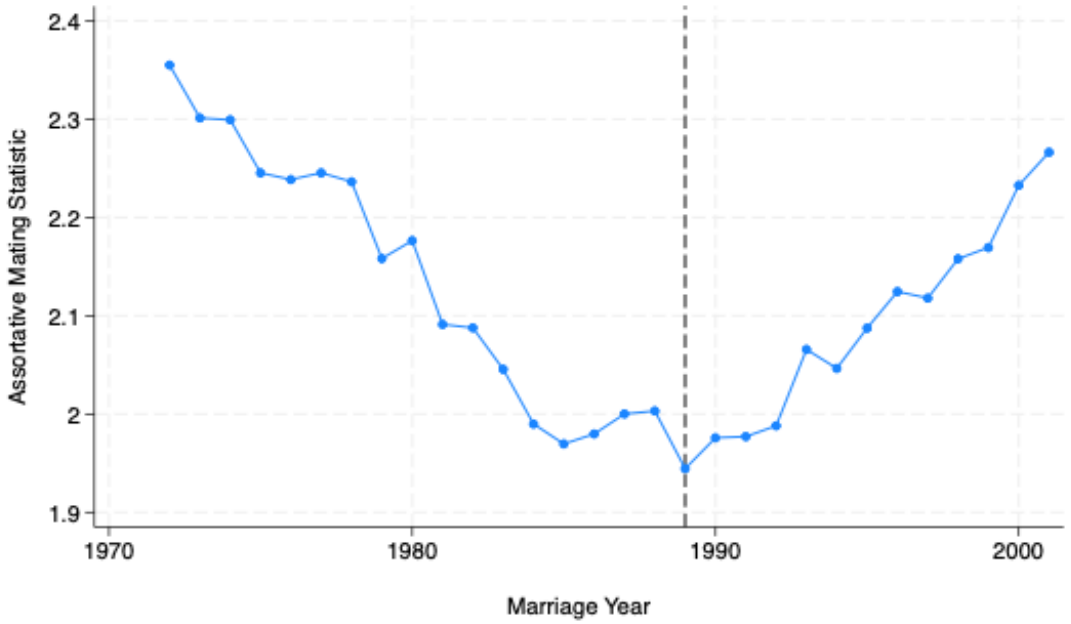
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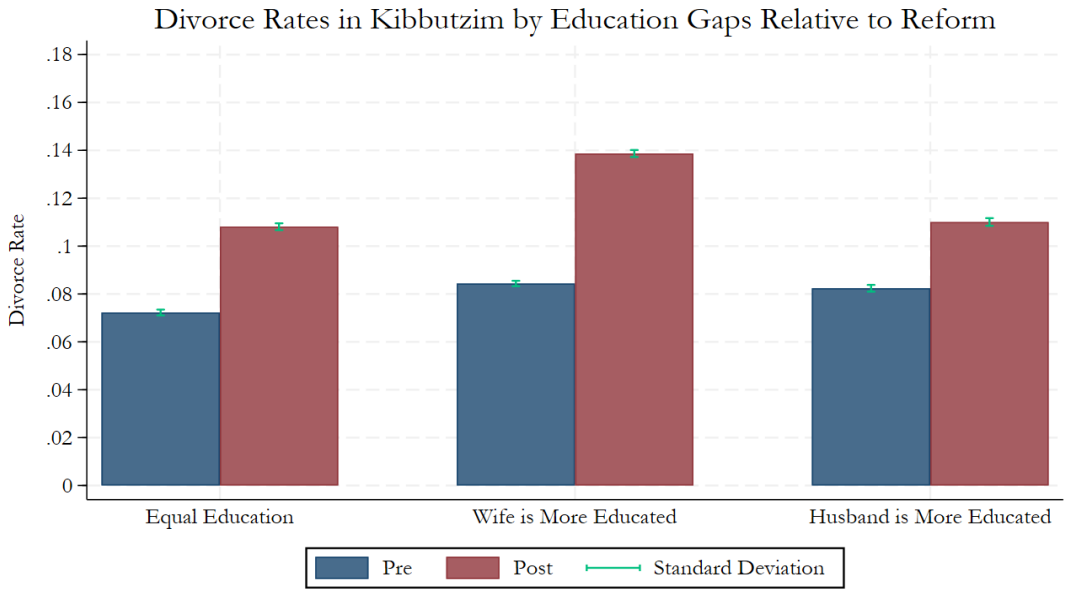
Figures

Figure 1: Assortative Matching in Poland



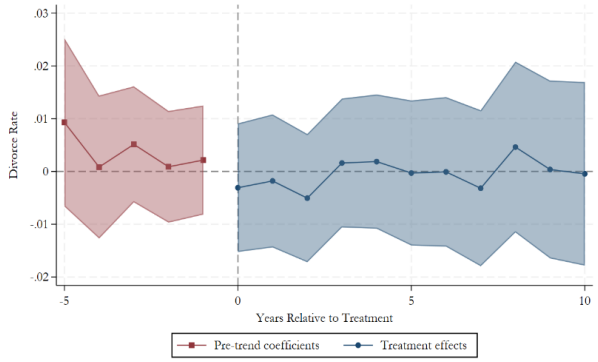
***Notes**: We draw information from the publicly available 2002 (10% sample) Polish census data (Ruggles et al. 2024). We keep samples of married, native-born Polish men. Using the variables of duration of marriage, we determine their marriage year and include in our sample only men who were between 20 and 35 years old at the time of marriage. We then calculate the share of men who married a woman with an education level equal to their own. We calculate the share of men who would have married a woman with similar educational attainment for each year separately under random matching. We plot the ratio between the former and latter numbers for every year. The vertical line depicts the fall on the Berlin wall in 1989.*

Figure 2: Divorce Rates by Educational Inequality and Reform Status



Notes: *Every bar represents the average divorce rate over 2003-2020 of divorced couples in kibbutzim. We demonstrate this average before and after the kibbutz was reformed for three groups: Couples in which the wife is more educated, couples in which the husband is more educated, and couples that attained an equal level of education. Couples are included from age 28 and up. The pre-period means also include kibbutzim that were never reformed. Standard deviations are included in the graph.*

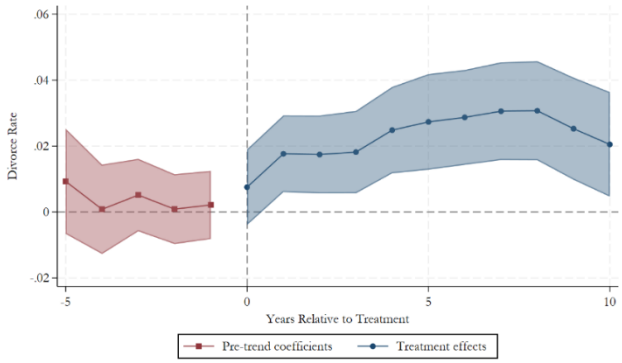
Figure 3: Divorce Rates -- Event Study



(I) Husband more Educated



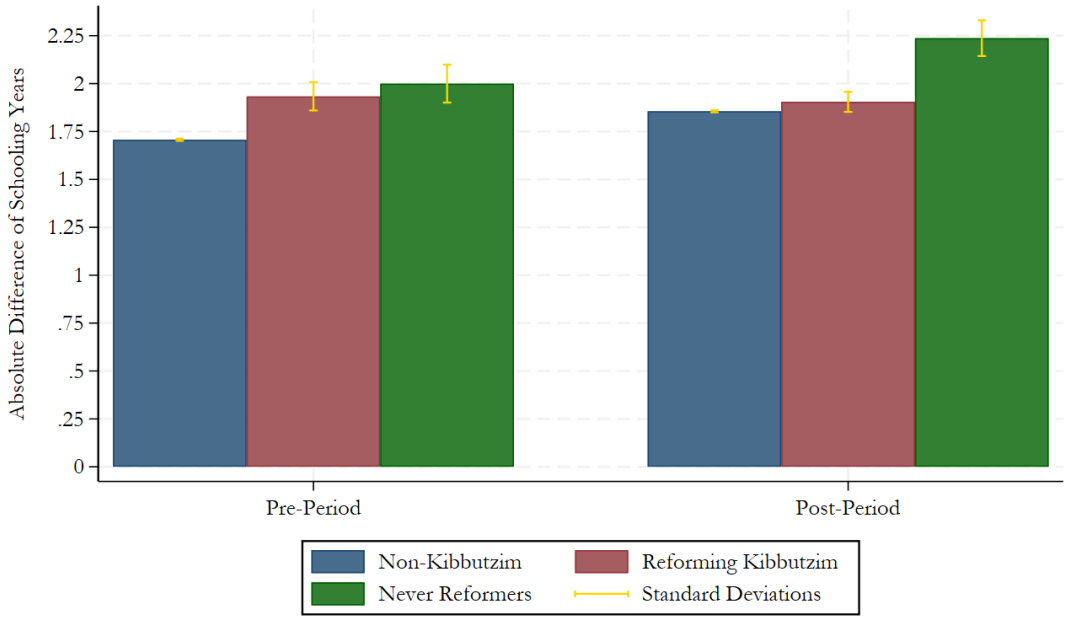
(II) Equal Education



(III) Wife more Educated

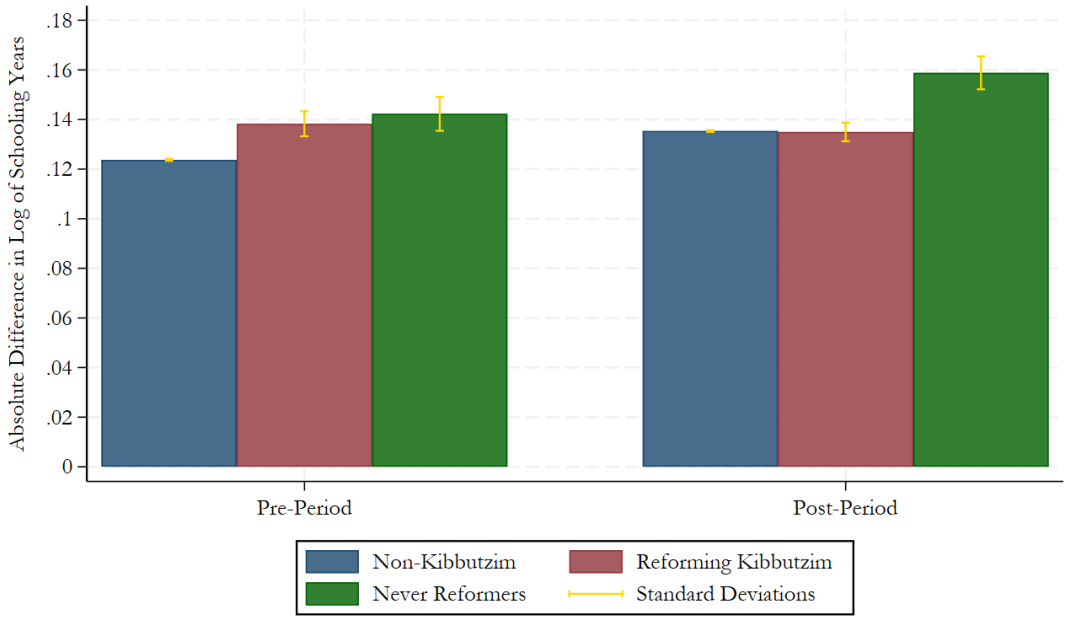
Notes: *We estimate the event studies corresponding to Equation 1. The sample is at the couple-year level, and the outcome is a dummy variable for being divorced. We split the sample into three groups when estimating the event studies: husband more educated, equal education, and wife more educated.*

Figure 4: Assortative Matching in Reformed-, Unreformed-, and Non-Kibbutzim.



Notes: *Every bar represents the average absolute difference in schooling years between a newly married woman and her husband. We demonstrate this average for three groups: kibbutzim that never reformed, kibbutzim that reformed between 2001-2005, and non-kibbutzim. Furthermore, we present the results for a pre-period spanning 1996-2000 and a post-period spanning 2007-2011. Standard deviations are included in the graph.*

Figure 5: Log Schooling Years

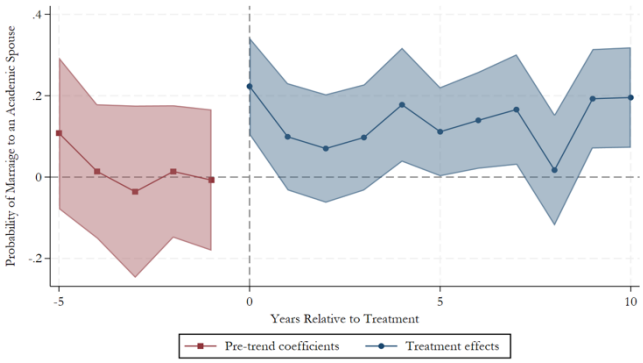


Notes: *We consider non-kibbutzim members (blue), members of kibbutzim that reformed between 2000 and 2005 (red), and members of never-reformed kibbutzim. For each group, we take the absolute difference in schooling years between the wife and the husband. We plot the mean for each group for a pre-period (1996-2000) and a post-period (2006-2011).*

Figure 6: Staggered Event Study Using Couple-Level Observations



3.1. Non-Academics



3.2. Academics

Tables

Table 1: Joint Distribution of Educational Attainment

Husband / Wife	Phd	MA	BA	Post-Secondary	Matriculation	High School or Lower	Total
PhD	26	143	114	16	18	20	337
MA	96	1094	855	136	153	194	2528
BA	93	1289	1725	371	308	616	4402
Post-Secondary	15	334	517	343	185	400	1794
Matriculation	12	209	390	109	223	209	1152
High School or Lower	41	608	1154	420	448	1593	4264
Total	283	3677	4755	1395	1082	3032	14477

Notes: *We take the entire sample of the divorce analysis. We split the sample into 36 bins by the educational attainment of the husband and the wife. We group together people who finished high school without matriculation and people who did not finish high school, since very few people did not finish high school. In the Table, we plot the number of couples in every bin.*

Table 2: Reform's Effect on Divorce Rate by Education Gap

	Divorce Rate			
	Full	**Both Academic**	**Non Academic**	**One Academic**
Husband more Educated x Post	-0.00396	0.0101	0.00344	0.00851
	(0.00740)	(0.0138)	(0.00711)	(0.0113)
Equal Education x Post	-0.00596	0.00111	-0.0184**	-0.00310
	(0.00620)	(0.00871)	(0.00802)	(0.00675)
Wife more Educated x Post	0.0221***	-0.0140	0.0242***	0.0236**
	(0.00677)	(0.0111)	(0.00736)	(0.00101)
Observations	350,798	123,736	248,740	225,794
Pre-Treatment Mean				
Husband more Educated	0.0863	0.0573	0.0763	0.0856
Equal Education	0.0816	0.0532	0.0532	0.0816
Wife more Educated	0.101	0.0512	0.0875	0.0978

***Notes**: We estimate a staggered difference-in-difference specification at the couple-year level, where the outcome is being divorced. We use the correction offered by Borusyak et al., and estimate the specification by splitting the coefficients by the relative education in the household. In columns (2)-(4), we limit the sample by the education level of the partners. The sample includes couples that were married before the reform. Standard errors are clustered at the kibbutz level.*

Table 3: Descriptive Statistics of Marriage Sample

	N	Mean Year of Birth	Mean Age at Marriage	Probability of Marrying a Kibbutz Member
Men	2201	1975.01	33.02	0.40
		(7.06)	(3.73)	(0.49)
Women	1532	1976.57	31.24	0.55
		(6.67)	(3.09)	(0.5)

Notes: *We take the entire sample of the marriage analysis. We split it by sex, and provide the means of the birth year, age at marriage, and probability of marrying another kibbutz member.*

Table 4: Staggered DiD Using Kibbutz-Level Observations

	Odds Ratio			
	Benchmark	**Year > 2000**	**Ever Reformed**	**Annual**
Weighted				
Reform	4.163***	7.979***	4.908***	2.140**
	(1.117)	(1.687)	(0.979)	(0.827)
Observations	496	279	259	675
Pre-Treatment Mean	2.455	1.772	2.595	2.041
Unweighted				
Reform	2.117**	3.141***	2.730**	1.352*
	(0.758)	(1.019)	(0.743)	(0.434)
Observations	496	279	259	675
Pre-Treatment Mean	1.731	1.301	1.697	1.019

Notes: *We define the odds ratio statistic following Chiappori et al. for every kibbutz, in three-year bins. We estimate a staggered DiD specification, in the kibbutz-bin level, using the correction offered by Borusyak et al. (2024). In Panel A results are weighted by the population of the kibbutz, and in Panel B they are unweighted. In column (2), we restrict attention to years after 2000. In column (3), we drop never-reformed kibbutzim. In column (4), we split the 3-year bins into annual bins. Standard errors are clustered at the kibbutz level.*

Table 5: Staggered DiD Estimates Using Couple-Level Observations

	Spouse is Academic		Spouse's Schooling	
	Men	**Women**	**Men**	**Women**
A. Benchmark				
Non-Academic x Reform	-0.0667*	-0.240***	-0.235	-1.033***
	(0.0404)	(0.0436)	(0.185)	(0.181)
Academic x Reform	0.195***	0.0850*	1.007***	0.527***
	(0.0334)	(0.0436)	(0.163)	(0.184)
Observations	2,201	1,532	2,201	1,532
Difference	0.261	0.325	1.242	1.56
	(0.0265)	(0.0368)	(0.116)	(0.171)
Pre-Treatment Mean				
Non-Academic	0.568	0.334	14.18	13.21
Academic	0.824	0.634	15.45	14.63
B. Post-2000				
Non-Academic x Reform	-0.118**	-0.104**	-0.440*	-0.717***
	(0.0489)	(0.0486)	(0.240)	(0.264)
Academic x Reform	0.179***	0.243***	1.006***	0.890***
	(0.0576)	(0.0460)	(0.236)	(0.246)
Observations	1,304	870	1,304	870
Difference	0.296	0.346	1.446	1.606
	(0.0297)	(0.0319)	(0.143)	(0.194)
Pre-Treatment Mean				
Non-Academic	0.599	0.358	14.22	13.28
Academic	0.844	0.656	15.52	14.67
C. Ever-Reformed				
Non-Academic x Reform	-0.0388	-0.164***	-0.445*	-1.198***
	(0.0472)	(0.0468)	(0.258)	(0.237)
Academic x Reform	0.272***	0.178***	1.155***	0.459**
	(0.0392)	(0.0459)	(0.214)	(0.219)
Observations	1,234	849	1,234	849
Difference	0.311	0.342	1.6	1.656
	(0.0339)	(0.0443)	(0.156)	(0.21)
Pre-Treatment Mean				
Non-Academic	0.550	0.340	14.13	13.23
Academic	0.831	0.637	15.52	14.69
D. Spouse is a Kibbutznik				
Non-Academic x Reform	-0.0646	-0.295***	-0.357	-1.108***
	(0.0519)	(0.0475)	(0.225)	(0.217)
Academic x Reform	0.231***	-0.00962	1.078***	0.311
	(0.0429)	(0.0537)	(0.202)	(0.216)
Observations	843	834	843	834
Difference	0.295	0.286	1.436	1.419

	(0.0471)	(0.0459)	(0.216)	(0.218)
Pre-Treatment Mean				
Non-Academic	0.533	0.326	14.07	13.28
Academic	0.764	0.590	15.19	14.54

Notes: *In columns (1)-(2), Panel A, we estimate a staggered DiD specification, using the correction offered by Borusyak et al. (2024). The outcome is marrying an academic spouse, and the coefficients are calculated separately for academic and non-academic kibbutzim members. The regression is estimated separately for men and women. In columns (3)-(4) we use the schooling years of the spouse. In Panel B, we limit the sample to individuals who married after 2000. In Panel C, we dropped ever-reform kibbutzim, In Panel D, we keep in the sample only couples in which both partners are kibbutzim members. Standard errors are clustered at the kibbutz level.*