Beyond Training: The Long-Term and Intergenerational Effects of Soft-Skill Enhancement for Welfare Recipients

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Abstract

We examine the long-term and intergenerational effects of a large-scale active labor market program targeting the soft skills of income support claimants through mentoring, counseling, and group therapy. Leveraging a randomized controlled trial comprising over 17,500 participants and comprehensive administrative records, we track participants and their families across a rich set of outcomes over eight years following the program. We find a substantial and persistent increase in employment and earnings and lower criminal activity among participants. Furthermore, we document positive employment spillovers on spouses, with household income gains exceeding the direct effects on participants. Notably, we find long-term benefits for children, including improved educational attainment and a reduction in criminal activity. These results demonstrate that soft-skills interventions can produce lasting gains for participants while generating meaningful spillovers on their families. By improving human capital and behavioral outcomes in the second generation, such programs may play an important role in weakening the intergenerational transmission of economic disadvantage.

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1 Introduction

How to bring disadvantaged individuals into stable employment is a long-standing challenge for anti-poverty policy. Beyond its direct effects on the individual, the transition into work can reshape the household's economic trajectory through responses in spouses' labor supply, the level and composition of family income, and the human capital and attitudes that children acquire. Yet whether these broader household gains materialize and persist in the long run remains largely unexplored.

We study these questions in the context of an Active Labor Market Program (ALMP) focused on soft-skills training. The program, "Employment Circles," was launched by the Israeli Employment Service in 2014 and provides income-support claimants with mentoring, group therapy, and job-search coaching aimed at building self-efficacy, self-esteem, and workplace readiness. Using a large-scale randomized controlled trial (RCT) that assigned more than 17,500 individuals to treatment and control groups, and linking participants to comprehensive administrative data from tax, police, education, and population registries, we track participants and their families across a rich set of economic and social outcomes for up to eight years after random assignment.

In Schlosser and Shanan (2025), we document the short- to medium-run effects of the program: twelve months after assignment, treated individuals were 8 percentage points more likely to be employed and earned 12 percent more than the control group. The short-run evidence also showed positive effects on participants' soft skills, including job-search self-efficacy, work self-efficacy, grit, and self-esteem, concentrated among those with no recent employment history. Building on these results, this paper extends the analysis in three directions. First, we examine whether these gains persist in the long run and manifest in additional outcomes, including criminal activity, fertility, and marital status. Second, we examine spillover effects on spouses' employment and total household labor income. Third, we estimate the intergenerational impacts on children, focusing on human capital, criminal activity, and labor market outcomes.

While an extensive literature examines the impact of ALMPs on participants, the broader consequences for their families remain largely unexplored. Participants' spouses may be affected because couples often make joint decisions about time allocation across market work, home production, and leisure. Additional channels may operate through changes in household social norms and attitudes toward employment, as well as the transmission of information about job vacancies. Moreover, because marriages involve risk-sharing, income and productivity shocks to one spouse can affect the labor supply of the other. Children are also likely to be affected through changes in the level or composition of family income induced by parental program participation, or changes in parental time use following employment. Furthermore, gains in parental human capital and non-cognitive skills may instill new attitudes and expectations toward education and work in their children potentially breaking the intergenerational cycle of welfare dependence.

We find that the program's effects on participants also persist in the long run: higher employment rates and earnings remain evident eight years after random assignment. Importantly, we also find a significant reduction in criminal activity among participants. Beyond the individuals themselves, our results indicate positive spillovers on spouses, driven by higher employment rates and earnings that increase total household income by amounts exceeding the gains of the program participants alone. Moreover, we find significant long-term benefits for children, including lower high-school dropout rates, improved performance in the matriculation exams, and reduced involvement in crime. Notably, these effects emerge across individuals from the most disadvantaged backgrounds, including the long-term unemployed and the Arab population. The program's ability to generate lasting gains among this population underscores its relevance for the groups that are typically hardest to serve.

Our findings contribute to several literatures. Most directly, we advance the evidence on ALMPs by providing experimental estimates of a program centered on soft skills training over a long-term horizon and across a comprehensive set of outcomes that extend well

beyond the employment and welfare measures typically examined (see Kluve, 2010; Card et al., 2018; Crépon and Van Den Berg, 2016; and Le Barbanchon et al., 2024 for recent reviews). The persistent employment gains we document are consistent with non-cognitive skills playing a durable role in labor market success (see e.g. Heckman et al., 2006), particularly for workers in low-skilled occupations (Aghion et al., 2019). Whereas the returns to non-cognitive skills have been studied primarily in childhood and adolescence, we show that targeted interventions to improve these skills can produce lasting gains among disadvantaged adults.[1]

We also contribute to the growing body of work on how parental economic circumstances affect children (see Page, 2024 and Nybom et al., 2026 for reviews). This work has mainly focused on two settings: changes in family income generated by cash transfers, welfare, or tax reforms (e.g., Dahl and Lochner, 2012; Aizer et al., 2016; Milligan and Stabile, 2011; Hoynes et al., 2016; Bastian and Michelmore, 2018; Aizer et al., 2023), and negative shocks to employment driven by plant closures or mass layoffs (e.g., Oreopoulos et al., 2008; Rege et al., 2011; Hilger, 2016; Carneiro et al., 2022). Most of these studies find that parental income and employment matter for children's outcomes, but the source of the change in parental circumstances matters as well.

Our setting differs from both literatures. In contrast to studies of family income, we examine an intervention that raises labor income by moving non-employed parents into work, building their skills and reshaping their attitudes toward employment. Unlike the job-loss literature, we capture a positive transition into employment rather than a negative shock. Furthermore, we study long-term unemployed individuals who often lack the work habits and soft skills that displaced workers likely possessed. To our knowledge, this paper provides among the first experimental evidence on the causal intergenerational effects of an active labor market program.

Finally, by examining spousal labor supply responses, we contribute to the literature

[1]Our results are consistent with Bobonis et al. (2025), who find lasting benefits of an intervention designed to address both the economic and psychosocial barriers faced by welfare recipients.

on intra-household time allocation and labor supply (see, e.g., Lundberg, 1985; Stephens, 2002; Blundell et al., 2005; Blundell et al., 2016; Blundell et al., 2018), a margin that has received almost no attention in the ALMP literature. While a growing literature emphasizes the importance of accounting for both partners in the design of tax and welfare policies (see, e.g., Eissa and Hoynes, 2004; Autor et al., 2019), ALMP evaluations have typically focused on participants in isolation.[2] Our findings suggest that accounting for household-level responses is essential for a complete assessment of program impacts and their implications for policy design.

The remainder of the paper proceeds as follows. Section 2 describes the program and summarizes its short- to medium-term impacts documented in Schlosser and Shanan (2025). Section 3 describes the data and samples used in the analysis. Section 4 details the empirical strategy and presents descriptive statistics and balance tests for each analysis sample. Section 5 presents the results, focusing in turn on the long-term effects on participants, spillover effects on spouses and household income, and intergenerational effects on children. Section 6 concludes.

2 Background

The Employment Circles Program is an Active Labor Market Program (ALMP) implemented by the Israeli Employment Service (IES) that focuses on soft skills training with the goal of integrating unemployed income-support claimants into the labor force and reducing welfare dependency and long-term unemployment. The target population consists of income-support claimants aged 20-50 who are unemployed and report weekly to their local employment office.[3]

[2] A notable exception is Kugler et al. (2022) who found positive spillovers of a training program in Colombia on the likelihood of attaining tertiary education among participants' relatives.

[3] Income-support claimants who are considered capable of working (i.e., healthy, under age sixty, and, for single parents, having children older than two years of age) are required to report weekly (or monthly for those above age fifty) to their local employment office in order to receive monthly income-support transfers.

The program aims to enhance participants' soft skills through personalized treatment comprising weekly sessions with occupational trainers, therapeutic group meetings with coaches, and job-search assistance workshops. A key component consists of group workshops in which coaches focus on identifying participants' strengths, enhancing their motivation, job-search efficacy, work self-efficacy, and self-image, and fostering a proactive work attitude. Participants are required to visit their local employment office three times per week: twice for workshops and meetings with occupational trainers (3–5 hours per visit) and once for a regular meeting with their caseworker. The program lasts between two and seven months, depending on the participant's needs. Participants may leave the program at any time upon finding employment, in which case they may continue to receive income-support benefits in the form of an earnings supplement, depending on their labor income. After seven months, participants who remain unemployed and continue to report to the employment office return to the regular weekly-visit track.

The program was launched in 2014 across fourteen IES employment offices and subsequently expanded to all employment offices in Israel. During the first two years of implementation, the program was administered as a randomized controlled trial (RCT) for evaluation purposes. During this experimental phase, individuals who submitted new income-support claims (the *flow sample*) and a subset of existing claimants (the *stock sample*) were randomized into treatment and control groups. Randomization was conducted weekly, separately for each sample and employment office. Treatment status was assigned at the household level: in cases where both partners were registered at the employment office, both were assigned to the same group.[4] In practice, as discussed below, in most cases only one household member was assigned to the program, since the other partner was not registered with the employment office. This feature allows us to examine spillover effects of the program on non-treated partners.

[4]This also applies when an individual was registered at the employment office at the time of program assignment but whose partner registered with the IES afterward; in such cases, the partner was assigned to the program upon registration.

Upon their next visit to the employment office, treated individuals recorded their visit using self-service biometric fingerprint scanners and received a notification that required them to meet with a designated caseworker who informed them that they had been selected for the program. Individuals randomly assigned to the control group received no notification and continued to follow the usual protocol of a weekly visit to the employment office and meetings with caseworkers for job referrals. An individual's treatment or control status remained in effect even if he or she moved to another city, stopped reporting to the employment office, or reregistered with the IES after a certain period.

In Schlosser and Shanan (2025), we document the short- to medium-term effects of the program using the sample of individuals randomly assigned in 2014 (6,151 individuals). Twelve months after randomization, the program raised participants' employment rates by 8 percentage points relative to the control group (a 24% increase), increased earnings by 12%, and reduced welfare dependency by 11 percentage points (a 26% decline). These effects persisted 18 months after program assignment.[5] Effects were larger among the stock sample (i.e., claimants with a longer history of income-support recipiency) than among the flow sample (i.e., those who had recently transitioned from employment). Consistent with this, we found improvements in soft skills (including job-search self-efficacy, work self-efficacy, general self-efficacy, self-esteem, and grit) that were concentrated almost exclusively among the stock sample.

The program also had larger effects among individuals with lower employment prospects, including Arab participants, high-school dropouts, and those with self-reported health limitations. We further find no evidence of displacement effects on the control group. Finally, we document that within twelve months, savings on government welfare transfers more than offset the program's per-participant cost, establishing it as cost-effective and implying a Marginal Value of Public Funds (MVPF) of infinity.

In this paper, we extend the analysis in several directions. First, we substantially ex-

[5]These results are based on a subsample of 1,643 individuals who could be followed over an 18-month post-randomization horizon.

pand the sample to include all individuals randomly assigned during the two years of the RCT (17,906 individuals). Second, we estimate the program's long-term effects by following individuals over eight years, examining its impact on employment and earnings as well as on outcomes that have received limited attention in the ALMP literature, including crime, fertility, marital stability, and residential location. Third, we examine spillover effects on participants' spouses, focusing on employment and earnings and estimating the overall effect on household income. Fourth, we estimate the effects on participants' children, focusing on educational attainment, criminal activity, and early-adulthood labor market outcomes.

3 Data and Samples

3.1 Data

Our empirical analysis draws on administrative data from several sources, merged at the research room of the Central Bureau of Statistics (CBS) using individual identifiers assigned to each record. Data on experiment participants, including treatment status, IES office, and date of assignment, were transferred from the IES to the CBS. Demographic characteristics, including religion, age, marital status, the number and ages of children, and residential location, are drawn from the population registry, which we also use to track changes over time in fertility, marital status, and residential location. Information on educational attainment and postsecondary enrollment is obtained from the education registry, and employment and earnings are measured using tax records. Criminal activity is drawn from police administrative records. Using the population registry, we identify the spouses of experiment participants at the time of program assignment and measure their monthly employment and earnings from tax records. We construct household income by summing the earnings of both spouses.

For the children of experiment participants, which we identify using the population

registry, we define the following educational outcomes during adolescence: eligibility for a matriculation certificate, total credit units awarded, and a dropout indicator, using administrative data from the Ministry of Education, as well as juvenile criminal records (prior to age 18) from police administrative records.[6] For children who were below 18 at the time of their parent's random assignment but reach age 18 during the follow-up period, we define the following early adulthood outcomes: criminal activity, employment, and earnings, using the same data sources described above for experiment participants. We further define post-secondary enrollment using administrative records from all Israeli higher education institutions and vocational and technological training colleges.

Our main sample consists of all 17,906 individuals randomly assigned to the treatment (11,032) or control (6,874) groups from March 2014 through December 2015. We track labor market outcomes from before random assignment through eight years after it. Additional outcomes are defined as follows: marital status reflects the value observed in the latest available year in the population registry records (2021); fertility is measured as the cumulative number of births recorded between randomization and 2021; and criminal activity is an indicator for any criminal record in the period from one year after randomization through 2021. For the analysis of household income and spousal labor market outcomes, we restrict the main sample to individuals who were married at the time of program assignment and follow the original couple (defined by the spouse recorded at the time of assignment) throughout the study period.

The analysis of children's outcomes is based on all children born before September 2003 who were younger than 18 at the time of parental random assignment. This restriction reflects data availability: since the latest year for which matriculation records are available is 2022, finalized high school outcomes cannot be observed for cohorts who

[6]The matriculation certificate is earned by passing national exams in core and elective subjects, with each exam awarding 1–5 credit units depending on the proficiency level of the subject. A minimum of 20 credit units is required to qualify for a matriculation certificate. The certificate is a prerequisite for university admission and represents one of the most economically important educational milestones. Analogous credentials exist in many countries, such as the French baccalaureate and the New York Regents Examinations. The dropout indicator identifies students who did not complete 12 years of schooling.

started first grade after the 2009/10 academic year. Consequently, children in this sample were between 11 and 18 years old at the time of parental assignment to either the treatment or control group. For this sample, we estimate the impacts of parental program assignment on high school dropout, matriculation outcomes, and juvenile crime through age 18. We additionally examine postsecondary enrollment and labor market outcomes measured between ages 19 and 26, depending on birth cohort, while adult criminal charges are tracked from age 18 through the end of 2021.

4 Empirical Strategy and Balance Tests

We begin by estimating the long-term effects of the program on participants, regressing each outcome on a treatment indicator, controlling for the randomization unit: employment office $\times$ randomization date $\times$ claimant type (flow/stock).[7] To increase precision and to control for small differences between treatment and control groups that derive from randomization in a finite sample, we augment the basic model with a vector of covariates that include individuals' demographic characteristics, employment, and earnings measured before randomization and reported in Table A1. The estimating equation can be written as follows:

$$Outcome_{ijtp} = \beta\, Treatment_i + X_i'\varphi + \gamma_{jtp} + \varepsilon_{ijtp} \tag{1}$$

where $Outcome_{ijtp}$ is the outcome of jobseeker i assigned to employment office j, randomized at time t, who belongs to claimant type p (i.e., flow or stock); $Treatment_i$ is an indicator for whether jobseeker i was assigned to treatment; γ_{jtp} is a fixed effect for the randomization cell (employment office interacted with randomization date and claimant type); and X_i is a vector of pre-randomization individual characteristics, including age,

[7] We aggregate the randomization unit at the month level instead of the week to avoid cases of singletons and enhance precision. In practice, the estimates are virtually identical in both cases.

sex, marital status, number of children, immigration status, education level, indicators for self-reported health limitations, single-mother, Ultra-Orthodox Jew, and Arab, number of months employed and earnings during 12 months preceding randomization. We cluster standard errors by randomization unit, allowing for correlation between the error terms of those who belong to the same group and employment office and were randomized at the same time.

We also use equation (1) to estimate the effects on participants' spouses and household income. We estimate this separately for two subsamples: households in which both partners qualified for the program and were jointly assigned to the treatment or control group, and households in which only one partner qualified and was assigned to treatment or control while the other was not part of the experiment (but is identified in our data). This setup allows us to examine program effects among individuals whose partner was also treated (compared to households in which both partners were assigned to the control group), as well as spillover effects of the program on untreated spouses of treated individuals (compared to the spouses of control group participants).

To estimate intergenerational impacts, we compare outcomes of children whose parents were assigned to the treatment and control groups. The estimating equation follows equation (1) with two modifications: first, the treatment indicator refers to the parent's assignment status; second, the set of covariates is expanded to include child-specific characteristics, namely gender and age at the time of the parent's randomization. To account for within-family correlation among siblings, standard errors are clustered at the household level.

For children's labor market outcomes, we construct a panel of annual earnings and employment records beginning at age 19 and extending through 2023, the latest year for which earnings data are available. Since the data end in 2023, the age range over which we observe each child depends on their age at the time of parental assignment. To address the resulting variation in life-cycle coverage, our specification controls also

nonparametrically for age at the time of each earnings observation.[8]

By virtue of randomization, we expect treatment and control groups to have similar characteristics after controlling for the randomization unit. Appendix Table A1 shows that this is indeed the case. Column (1) of the table reports basic demographic characteristics, employment, and welfare history of the treatment group as recorded before they were randomized into the program. Column (2) of the table reports balance tests for each of the individual variables based on regressing each outcome on a treatment dummy and indicators for the randomization block. The table also reports the F-statistic and p-value of a regression that examines whether all covariates can jointly predict treatment status within the randomization cell.

Among program participants, 56% are women, and the average age is 36. In terms of family structure, 49% are married and 20% are single parents, with an average of 2.5 children at the time of program assignment. The sample is drawn from diverse demographic groups within the Israeli population: 43% are Arab, 16% are Ultra-Orthodox Jewish, and 19% are immigrants. Educational attainment is low: only 11% have more than twelve years of schooling, 45% have exactly twelve years, and 36% have fewer. Additionally, 39% report a health limitation that affects their ability to work.

We find no systematic differences between the treatment and control groups. Importantly, employment history and earnings in the period preceding randomization are balanced across groups.[9] A joint significance test of all covariates fails to reject the null, suggesting that the ignorability assumption holds conditional on randomization cell.

Table A2 presents descriptive statistics for the children sample (column 1) and balance tests (column 2). Approximately 48% of the children are female, with a mean age of 15 at the time of parental assignment to the program. In most cases (65%), the assigned

[8]We could alternatively control for year fixed effects.

[9]The only exception is a slightly higher share of Arab participants in the treatment group (1.3 percentage points, 3%). Given the small magnitude of this difference, it is unlikely to bias our results. Moreover, since the Arab population has lower employment rates and earnings, any resulting bias would work against finding a significant treatment effect. In any case, we address this by controlling for this covariate in all specifications and by performing a separate analysis for the Arab subsample.

parent was the mother; the average age of the assigned parent at baseline was 42. 27% of children come from single-parent households; 53% belong to Arab households, and 22% to Ultra-Orthodox Jewish households. These groups are over-represented relative to the general population, reflecting their larger average family sizes and higher rates of program eligibility due to socioeconomic disadvantage.

Parental socioeconomic status at the time of program allocation emphasizes the disadvantaged nature of the sample: 49% of eligible parents have fewer than 12 years of schooling, while only 7% have some post-secondary education. The sample also exhibits low levels of parent labor market attachment and earnings. Consistent with the randomization design, the children's subsample is well balanced across nearly all covariates, except for a slight imbalance in the share of females (the treatment group has 2 percentage points fewer females). Importantly, a joint significance test confirms that the full set of covariates cannot predict parental treatment status, supporting the validity of the experimental design for this subsample.

5 Results

5.1 Long-term effects on participants

Labor market outcomes. Figures 1 and 2 provide a visual summary of the dynamic effects of the program on employment and earnings. The left panel of each figure illustrates the means of the treatment and control groups and the right panel displays the estimated treatment effect alongside a 95% confidence band. We report results for the full sample, the stock and flow subsamples, and for Arab and Jewish participants separately.

Treatment and control groups follow nearly identical trajectories throughout the 48-month period preceding randomization into the program. This pre-allocation balance across all subsamples provides strong evidence for the validity of the experimental design and the comparability of the two groups at baseline. Turning to employment, Figure 1

shows that the program generated a substantial increase in employment rates following randomization. The treatment effect rises in the first months after program assignment, reaching its peak within 12 to 18 months, and then gradually declines but remains positive and statistically significant throughout the entire eight-year observation window.

Treatment and control groups experienced a sharp decline in employment and earnings in the post-randomization period that coincides with the COVID-19 pandemic. The treatment group nonetheless proved more resilient: despite the common shock, it maintained its advantage relative to the control group throughout the pandemic, and the gap between the two groups persisted after the recovery.

Treatment effects are considerably larger and more persistent for the stock sample (panel b) than for the flow sample (panel c). The employment gains are particularly pronounced and sustained among Arab participants (panel d), while the treatment and control groups converge for Jewish participants (panel e), where the estimated treatment effect is small and largely indistinguishable from zero after two years.

Figure 2 presents a broadly similar picture for monthly earnings. The treatment group exhibits higher earnings than the control group for the post-randomization months, with the gap most pronounced for the stock sample and Arab participants. As with employment, the earnings advantage narrows for the flow sample and is negligible for Jewish participants, suggesting that the program's labor market benefits were concentrated among the more disadvantaged segments of the sample.

Table 1 reports the long-term effects of the program eight years post-randomization. We present results for the full sample (columns 1–3) and the stock sample (columns 4–6), each broken down by ethno-religious group: Arabs and Jews. Throughout this and the following tables, the control group mean is reported in italics. Given that the flow sample exhibited only short-lived effects that did not persist, we focus our subsequent analysis on the full and stock populations and their respective ethnic components. Detailed results for the flow sample are provided in Appendix Table A3, where significant gains are observed

only for the Arab subgroup.

The program significantly increased the probability of ever being employed during the eight-year observation window. In the full sample, treated participants were 4.4 percentage points more likely to have ever been employed relative to a control mean of 81.1%, a 5.4% increase. The effect is concentrated among Arab participants, who were 8.9 percentage points more likely to have ever been employed (an increase of 11.8% compared to a control mean of 75.3%). In the stock sample, the corresponding effects are larger: 7.6 percentage points overall and 12.5 percentage points (a 17.6% increase) for Arab participants. The estimate for Jewish participants in this sample is positive but small (2.4 percentage points, a 2.9% increase relative to a control mean of 83.1%) and statistically insignificant. The program also generated substantial and statistically significant gains in cumulative months of employment over the eight-year observation window. In the full sample, treated participants accumulated approximately 3.7 additional months of employment relative to the control group. The effects are entirely driven by Arab participants who accumulated 7.4 additional months of employment (a 24% increase relative to the control mean of 30.9 months), while the estimated effect for Jewish participants is close to zero and statistically insignificant. The pattern is even more pronounced in the stock sample, where Arab participants accumulated 10 additional months of employment (a 36% increase), while the effect for Jewish participants remains negligible.

The results for employment at month 96 are consistent with this pattern, though estimated with less precision: in the full sample, the treatment increased the probability of employment at month 96 by 1.4 percentage points relative to a control mean of 42.7%, a 3.3% increase. For Arab participants in the full sample, the effect is 3.8 percentage points against a control mean of 38%, representing a 10% increase, while there is no effect for Jewish participants. The stock sample shows larger effects: the overall estimate for this sample is 3.3 percentage points (an 8.4% increase), with Arab stock participants driving most of this result (6 percentage points, a 17.3% increase). The effect for Jewish stock

participants is negligible and not statistically significant.

Turning to earnings, we find that cumulative earnings over 96 months increased by approximately NIS 21,300 for Arab participants in the full sample (a 15% increase) and by NIS 31,250 for Arab participants in the stock sample (a 26% increase). In contrast, the estimated earnings effects for Jewish participants are small and not statistically significant. The aggregate effect for the full sample is positive but not statistically significant. Monthly earnings at month 96 follow a similar pattern, with positive and significant effects for Arab participants and for the stock sample, and negligible effects for Jewish participants.

Table A4 examines the effect of the program on the quality of employment. A well-documented challenge among welfare recipients is not only finding a job but also remaining employed for a sustained period. To capture employment stability, we define the maximum employment spell as the largest number of consecutive months of uninterrupted employment over the 96-month period and estimate the effect of the program on this outcome. Our results show that the program not only increased employment but also significantly increased employment persistence: the maximum employment spell in the full sample increased by 12%, a greater gain in percentage terms than the increase in overall employment. Consistent with our previous results, the gains are larger for participants in the stock sample (17%) and for the Arab population (23%), the groups with lower counterfactual means. As an additional measure of employment quality, we examine the impact on the probability of working for a large firm, defined as one with 80 employees or more (the sample median). The increase in this probability is similar in magnitude to the increase in employment, suggesting that although the program drew more marginal individuals into the labor force, they found jobs in firms similar to those employing individuals who would have worked even in the absence of the program.

To summarize, the results for labor market outcomes show that the program generated large and persistent gains in employment and earnings for participants in the stock sam-

ple and for Arab participants, the most disadvantaged groups with weaker labor market prospects. The program also increased employment among Jewish participants and those with stronger labor force attachment, but these effects did not persist, as the control group eventually caught up.

Crime. Participation in the program may reduce crime through several channels: by raising the opportunity cost through higher earnings, through an incapacitation effect while participants attend the program or work, by imparting non-cognitive skills that reduce criminal activity, or by shifting participants' lifestyles and social environments (see Hjalmarsson et al., 2024 for a recent review). Beyond being of interest in its own right, quantifying the program's effect on crime is important for a full accounting of the program's returns. The last row of Table 1 shows that the program indeed reduced criminal activity among participants. In the full sample, the probability of a criminal charge in the post-randomization period fell by 0.8 percentage points, an 11% decline relative to the control mean of 7%. The effect is larger in the stock sample (1.5 percentage points, 21%) and particularly pronounced among Jewish stock participants (2.4 percentage points, a 25% decline relative to their control mean of 9.5%). Notably, the reduction in crime among Jewish participants emerges despite the absence of persistent employment gains for this group, suggesting that part of the reduction in criminal behavior stems from channels other than sustained employment.

Fertility, marital status, and residential location. Employment may also affect family formation. Higher earnings and increased labor force attachment raise the opportunity cost of time devoted to childbearing, particularly for women, while the accompanying income gains may push fertility in either direction, as higher income may increase the demand for children or shift resources toward child quality rather than quantity. Improved economic prospects may also affect the formation and stability of marriages. Finally, higher employment and income may enable participants to relocate, toward stronger lo-

cal labor markets or better locations, shaping long-run economic outcomes and children's opportunities (e.g., Chetty et al., 2016).

We examine the impact on these additional outcomes in Table A5. Estimates show a reduction in fertility in the post-randomization period. In the full sample, treated participants had 0.03 fewer children on average, with the effect concentrated among Arab participants (0.07 fewer children, an 8% reduction relative to the control mean of 0.83). Effects on marital status are small and generally not statistically significant across subgroups.[10] Finally, we examine the effect on residential location by estimating the probability that participants reside, in month 96, in the same locality recorded at the time of allocation.[11] Our intention was to examine the socioeconomic characteristics of destination localities conditional on finding evidence of mobility. However, we find no evidence that the program affected residential mobility across localities, either overall or in any of the subsamples.

To summarize, the program generated substantial and persistent labor market gains, reduced criminal activity, and lowered fertility among participants, with larger effects among the most disadvantaged groups: Arab participants and the stock sample of long-term welfare recipients. We find no evidence of changes in residential mobility, although our geographic information may be too coarse to detect short-distance moves (i.e., moves within localities).

[10] In a supplementary analysis, we estimate the impact on marital status and fertility separately for individuals who were married at randomization and those who were not. Across both subsamples, we find a reduction in fertility among Arab participants of approximately 6–8%. The estimated effect on marital status is also negative but negligibly small in magnitude (approximately 2%) and only marginally significant, suggesting that the program had no meaningful impact on the marital status of participants.

[11] In the Israeli context, a locality (*yishuv*) is an officially recognized populated place with defined administrative boundaries (e.g., a city, town, or village). This is the smallest geographic unit recorded in our data.

5.2 Impact at the household level

Table 2 examines the program's impact within households where both partners were registered at the employment office and were consequently assigned jointly to either the treatment or control group.[12] In these dual-treatment households, employment and earnings rise substantially: the program increased the employment of each partner as well as the probability that both partners were ever employed. Over the eight-year horizon following assignment, the effect on total household earnings exceeds the effect on individual earnings, implying that both partners contributed to the household gain. These effects are more pronounced in the stock sample and among Arab couples: cumulative household earnings are 11% higher for treated couples in the full sample, 21% higher for Arab couples, and 13% higher in the stock sample, with the latter driven primarily by the Arab subsample. We find no evidence of substitution in labor supply between partners. Overall, the findings from jointly allocated households indicate that providing soft-skills training to both partners triggers a joint transition into employment that substantially raises long-term household income.

Table 3 provides evidence of household spillover effects by focusing on households in which only one partner was eligible for the program, allowing us to examine how the treatment of one partner affects the labor market outcomes of the untreated spouse. The employment effects for participants in this sample are broadly consistent with those reported for the full sample in Table 1, confirming that the labor market gains are not specific to dual-treatment households or to individuals without partners. More importantly, we find a positive spillover to the untreated partner: the program increased the probability that the untreated spouse was ever employed by 4.1 percentage points in the full sample (a 6.9% increase) and by 7.6 percentage points in the stock sample (a 12% in-

[12]The date of program assignment does not necessarily coincide across partners, since each individual is assigned upon reporting to the employment office. Partners share the same assignment date only if both were registered at the employment office at the time of allocation. When assignment dates differ, we use the earlier date to define program assignment at the household level.

crease). Notably, this spillover on spousal employment appears among both Arab and Jewish households. Thus, even though the employment effects for Jewish participants did not persist in the long run (as the control group eventually caught up), the program succeeded in inducing spouses of treated participants to work. Overall, the program increased the probability that both partners were ever employed during the eight-year follow-up period by 13%. Consistent with our previous findings, the increases are larger for the Arab sample (15%) and the stock sample (27%). Over the eight-year horizon, the gains in total household earnings substantially exceed the earnings gains of the treated partner alone. For example, in the stock sample, the cumulative increase in household earnings is NIS 56,071 (an 18% increase), whereas the earnings gain of the treated partner is NIS 20,665 (a 14% increase).

These results show that soft-skills training does not merely benefit the individual trainee; it can reshape the household's economic trajectory by encouraging the untreated partner to increase their labor market attachment. Such dynamics may stem from intra-household changes in motivation or norms, the adoption of shared job-search strategies, the sharing of information about job opportunities and access to the treated partner's workplace network, or a broader shift in the perceived returns to employment. Table A6 provides suggestive evidence that information sharing or social networks explain part of the effect on spouses, based on treatment effects on the probability that both spouses worked for the same employer (fourth row).[13] Estimates show positive effects on this outcome that are larger in percentage terms than the increase in employment. For the full sample, the probability that the spouse was ever employed increases by 14%, while the probability that both partners worked for the same employer increases by 22%. For Arab participants, the corresponding increases are 18% and 30%, respectively.

The larger increase in the probability that both spouses work for the same employer

[13]The indicator for both spouses ever working for the same employer equals one if both partners have an employment spell with the same employer during the post-allocation period (not necessarily at the same time).

relative to the increase in spousal employment, indicates that part of the spousal spillover operates through information sharing or network effects, although other channels are likely at play as well. A full examination of the underlying drivers of the spousal response is beyond the scope of this study, but our findings offer promising directions for the design and targeting of ALMPs. More broadly, our results point to an understudied dimension in the evaluation of ALMPs: their effects extend beyond the treated individual to the household. Assessments that focus solely on participants may substantially understate the returns of these interventions.

5.3 Effects on Children

Children growing up in households that rely on income support are disproportionately exposed to poverty, instability, and limited parental resources, all of which are known to adversely affect educational attainment, cognitive development, and long-run economic outcomes. A program that successfully moves parents into stable employment may therefore generate benefits that extend beyond the treated individual, by improving the economic and social environment in which children develop, providing positive parental role models, and transmitting stronger norms around work and self-sufficiency to the next generation. At the same time, increased parental employment may reduce the time parents spend with their children, potentially offsetting some of the benefits associated with higher household income. The net effect on children is therefore an empirical question, and one that has received limited attention in the ALMP literature.

Effects on parents in the children's sample. Before turning to children's outcomes, we first verify that the program effects documented for the full participant sample in Section 5.1 persist within the specific sample of parents. As shown in Table 4, the program's impact on parents remains substantial and statistically significant. Among all parents in the children's sample, treated participants accumulated approximately 5.6 additional

months of employment over eight years relative to the control group (a 18% increase), compared to 3.7 months (a 10% increase) in the full sample of program participants (reported in Table 1), indicating that effects in this subsample are in fact larger. The effects are again concentrated among Arab participants, who accumulated 9.5 additional months of employment (a 39% increase), with even larger effects in the Arab stock sample (12.9 additional months, a 58% increase). The corresponding cumulative earnings gains follow a similar pattern, with large and positive effects for Arab participants and the stock sample. Taken together, these labor market gains translated into a substantially richer household economic environment for the children of treated parents: household labor income over the 8-year period examined increased by approximately 8% in the full sample, rising to 19% for Arab parents and 22% for the stock sample, with the largest gains among Arab stock parents, for whom the increase amounts to approximately NIS 68,000, a 36% increase relative to the control mean. The point estimates for criminal activity are negative for Arab participants and the stock sample, although they are imprecise and do not reach conventional levels of statistical significance, likely due to the smaller sample sizes. The control means for this sample are lower than in the full sample, pointing to a lower incidence of criminal activity among parents.

Finally, we examine marital status, fertility, and residential mobility in this sample and report the results in Table A7. As in the full sample, we find no effect on marital status. We estimate a small but statistically significant reduction in the number of children born post-allocation for Arab families in the stock sample (-0.07 children relative to a mean of 0.264). Given that the average parental age at allocation was 42, the scope for fertility responses in this sample was limited to begin with. Treated parents are slightly more likely to remain in the same locality, although the effect is small both in absolute terms (2.1 ppt) and relative to the outcome mean (2.4%).

Effects on children during adolescence. Table 5 reports the estimated program effects on children's outcomes during adolescence. The results reveal meaningful improvements in educational attainment and reductions in juvenile criminal activity, with effects concentrated among Arab children and children in the stock sample. These findings track the patterns observed for the parents: significant effects on children appear in precisely the groups where the program most improved parental labor market outcomes, namely the stock and Arab samples. This alignment suggests that the intergenerational benefits are likely driven by the successful integration of the parents into the labor market and the associated improvements in household economic circumstances.

The most notable results are observed for the dropout rate. In the full sample, children of treated parents were 2.1 percentage points less likely to drop out of high school (an 11% reduction). This impact is slightly larger for the stock sample, where the dropout rate fell by 3.3 percentage points, a 17% reduction. Among children of Arab stock participants, the effect is particularly large: the dropout rate decreased by 5.3 percentage points, a 25% reduction relative to the mean of 21.1%.

Turning to matriculation outcomes, there are no discernible effects on eligibility or credit units in the full sample. However, meaningful effects emerge in the stock sample. Children of treated parents in the stock sample were 3.3 percentage points more likely to obtain a matriculation certificate (a 10% increase) and accumulated approximately 1.8 additional credit units (a 13% increase). These effects are driven by Arab children in the stock sample, for whom the probability of matriculation eligibility increased by 4.2 percentage points (a 12.5% increase) and total credit units increased by 2.5 (an 18% increase).

The positive impact on children extends to an important social outcome: juvenile criminal activity. In the full sample, children were 0.8 percentage points less likely to have a juvenile criminal record (a 25% reduction). In the stock sample, the overall effect is 1.3 percentage points (a 34% reduction), driven primarily by Arab stock children (2.0 percentage points, a 53% reduction), while the effect for Jewish stock children is small and

insignificant.

Effects on children during early adulthood. Table 6 reports the estimated impacts of parental program assignment on outcomes measured in early adulthood. These outcomes are observed up to age 26, depending on the birth cohort, with longer tracking periods available for children who were older at the time of parental assignment. Consequently, results from this analysis should be interpreted as an early indication of potential gains rather than as fully realized effects. Results for Jewish young adults warrant additional caution, as the majority of this group is engaged in mandatory military service between ages 18 and 21 (and sometimes beyond), which is likely to attenuate any detectable effects.

Consistent with the educational gains documented during adolescence, we find positive effects on postsecondary enrollment among children of stock participants. These gains are primarily driven by Arab children, who experience a 2.1 percentage point increase in enrollment, a 31% improvement over the control group mean. While point estimates for employment and earnings in this group are also positive, they are not statistically significant. This likely reflects the relatively young age of the observed cohorts, suggesting that a longer observation window is required to fully capture the returns to the documented educational gains.

Finally, we find a reduction in criminal activity (defined as having any criminal record during the observed time span) among the most affected group. For children of Arab participants in the stock sample, the probability of an adult criminal charge fell by 1.6 percentage points. This result indicates that the behavioral improvements documented during adolescence persist well into early adulthood, suggesting a lasting shift in the life-course trajectory of these children.

Heterogeneity by child's age at parental assignment We also examine heterogeneous effects by children's age at the time of parental program assignment.[14] Specifically, we

[14] In cases where both parents were assigned at different dates, we use the earliest assignment date.

distinguish between children who were aged 11–14 and those who were aged 15–18 at the time of assignment. This stratification is motivated by two considerations. First, age 15 is roughly the median age of the children sample. Second, students begin taking matriculation exams at age 15, meaning that many of the key decisions shaping educational investment in high school are made before this age.[15] For this stratification, we focus exclusively on outcomes during early adolescence, as the younger children aged 11–14 are still too young to exhibit meaningful labor market outcomes. Moreover, unlike adolescence outcomes, which are observed at the same age for all children, labor market outcomes are observed at different ages because of censoring: the age at which we observe them depends on the child's age at parental randomization. This prevents us from disentangling life-cycle effects, driven by the heterogeneous ages at which outcomes are measured, from the true effect of age at the time of program exposure.

Appendix Table A8 reports the results for early adolescence outcomes. We focus here on the children of the stock and Arab participants, given that these are the two groups who were affected. Sample sizes are smaller within each age group, which limits statistical power. Nevertheless, a clear pattern emerges: younger children tend to benefit more along educational dimensions. Specifically, children aged 11–14 at the time of parental program assignment exhibit larger reductions in dropout rates and larger gains in matriculation units, a pattern that is particularly pronounced among the stock and Arab samples. This is consistent with the view that earlier exposure to improvements in household circumstances, stemming from parental employment and enhanced soft skills, translates into greater human capital improvements during the critical years of high school.

Turning to juvenile crime, all estimates are negative, although not precisely measured due to a lack of statistical power to formally reject the null hypothesis of equality of the effects between these groups. Notably, there is a substantial reduction in criminal activity

[15]Note that because parental program participation has persistent effects on the household, we cannot disentangle the age at which children first experienced the change in household trajectory from the length of time they were affected by it. This limitation is common to any policy that produces a meaningful and sustained change in household circumstances.

of 2.8 percentage points among Arab adolescents in the stock sample who were aged 15–18 at the time of parental program assignment. This finding suggests that behavioral outcomes may be particularly sensitive to shifts in household circumstances even when those shifts occur later in childhood, as their effects are more immediately realized at this developmental stage. Taken together, the results indicate that while improvements in the household economic environment foster human capital accumulation most effectively when they occur earlier in childhood, such shifts can still generate meaningful gains for older children by mitigating criminal behavior during high-risk years.

We also examined heterogeneity by child's gender and report the results in Table A9. Although these estimates are less precise given the smaller samples, they suggest larger effects on academic outcomes for girls and larger effects on criminal activity for boys, particularly in the Arab stock sample. Finally, we examined heterogeneity by which parent was assigned to the program: the mother, the father, or both. We find no significant differences along this dimension, though the estimates are imprecise given the reduced statistical power of these smaller samples. Moreover, since the program also affected the employment and earnings of the untreated parent, the distinction between treated and untreated parents may matter less than the household-level exposure to the program. As additional years of data accumulate, future research will be able to track larger cohorts over longer horizons, including children who were younger at the time of parental assignment, allowing for a more granular investigation of the mechanisms through which parental employment and skill development shape the long-term trajectories of the next generation.

6 Conclusion

A central question for social policy is whether it can break the intergenerational transmission of disadvantage. We address this by evaluating the long-term and intergenerational

impacts of a large-scale Active Labor Market Program in Israel that provided soft-skills training for welfare recipients. We leverage a randomized controlled trial comprising over 17,500 participants and comprehensive administrative records linked across multiple data sources to track participants and their families over an eight-year horizon.

Among participants, the program generated substantial and persistent gains in employment and earnings, alongside meaningful reductions in criminal activity. These effects are concentrated among the most disadvantaged subgroups, namely Arab participants and those with a longer history of income-support recipiency, suggesting that soft-skills interventions may be particularly effective where the barriers to stable employment are highest. The employment and earnings gains also proved resilient to a large aggregate shock, with treated individuals maintaining their labor market advantage throughout the COVID-19 pandemic and its aftermath.

Beyond the direct effects on participants, we document significant impacts at the household level. In households where both partners were assigned to the program, the intervention triggered a joint transition into employment that substantially raised long-term household income. Moreover, in households where only one partner participated, we find meaningful spillover effects on the labor market outcomes of the untreated spouse, with total household income gains that exceeded the direct earnings effects on the treated participant alone.

The intergenerational findings highlight the broader value of the program. Children of treated parents, particularly those from the most disadvantaged backgrounds, experience significant improvements in school persistence and academic achievement, and show a marked reduction in juvenile criminal activity. While it remains too early to observe full labor market returns in their early adulthood, the gains in human capital and the persistent reduction in adult crime among the most affected groups suggest significantly improved life chances for the next generation. Taken together, our findings provide evidence that well-designed policies that bring parents into stable employment and self-

sufficiency can effectively break the cycle of poverty across generations.

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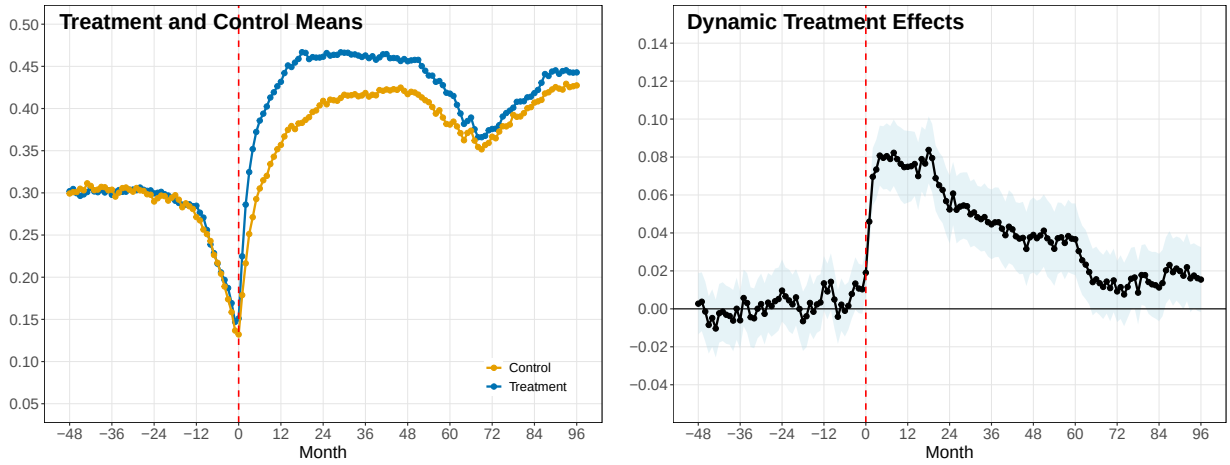
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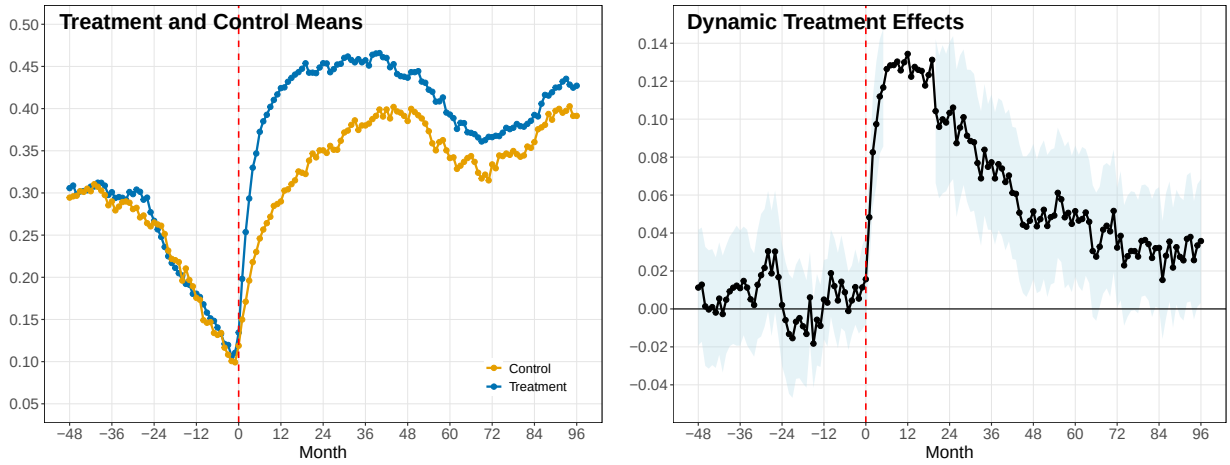
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Figure 1: Employment

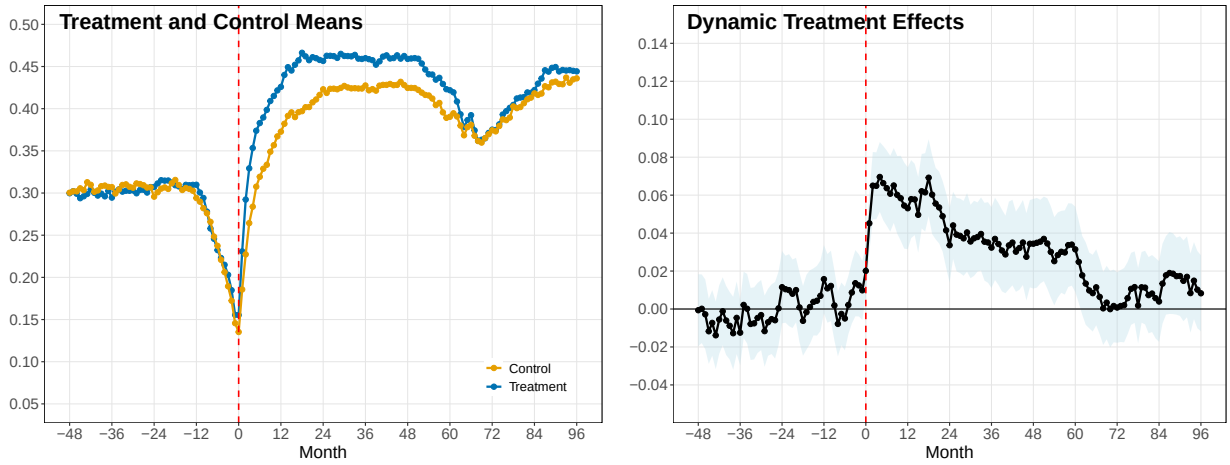
(a) Full Sample



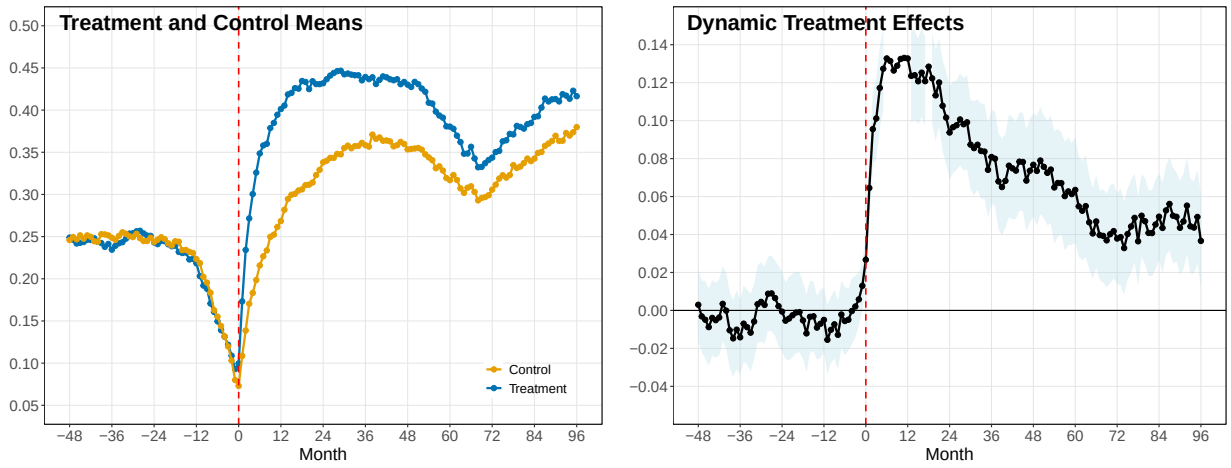
(b) Stock Sample



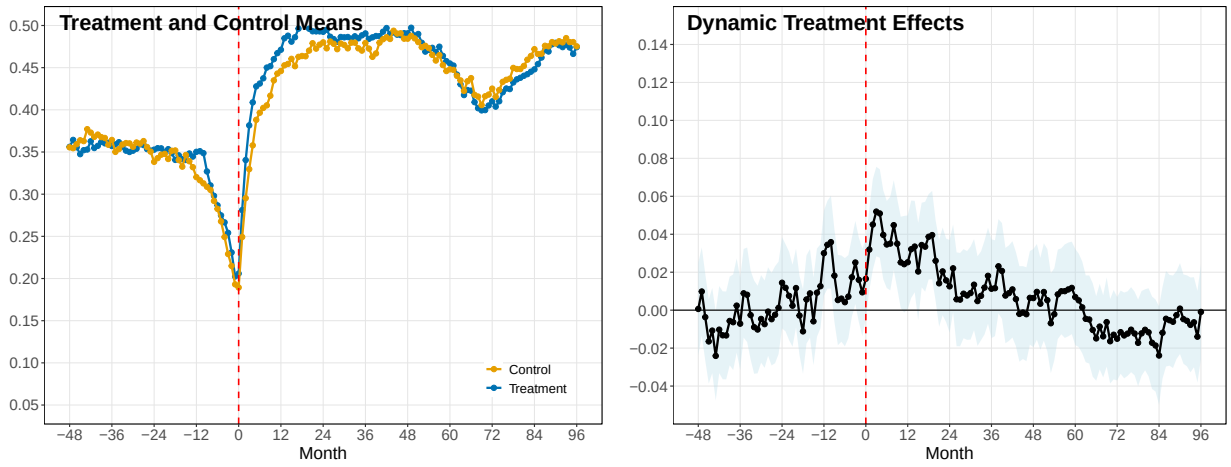
(c) Flow Sample



(d) Arabs



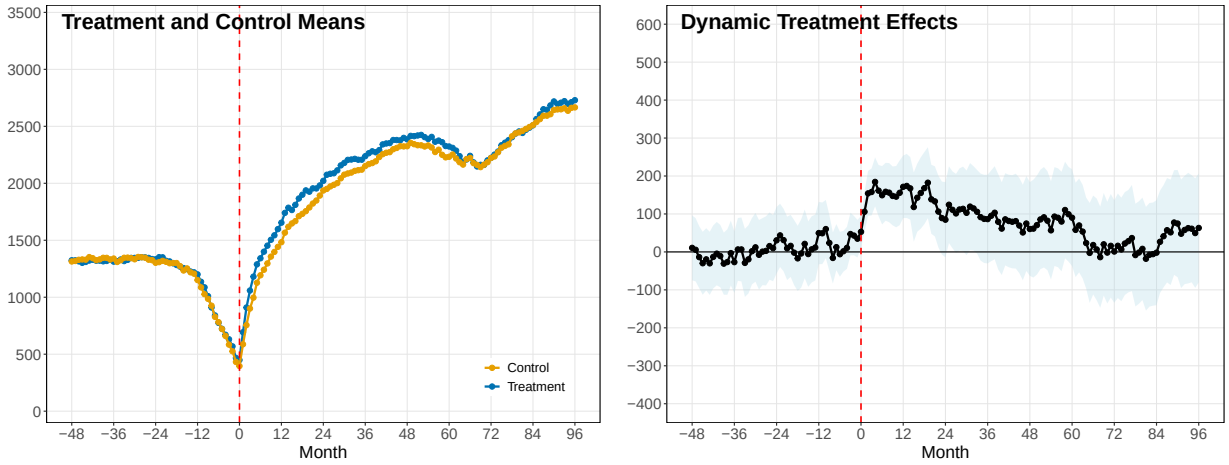
(e) Jews



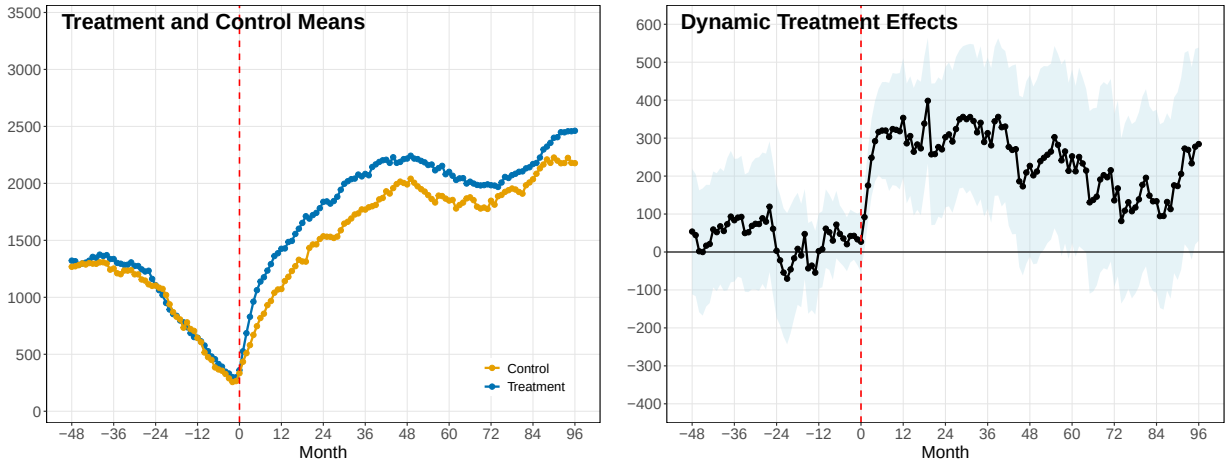
Note: The figure reports employment rates for treatment and control groups (left panel) and the difference in employment rates between the groups, along with 95% confidence intervals (right panel) over time, controlling for gender, age, marital status, education level, number of children, single mother dummy, immigrant dummy, and randomization block fixed effects. Month zero corresponds to the month of random assignment. Standard errors are clustered at the randomization unit level.

Figure 2: Earnings

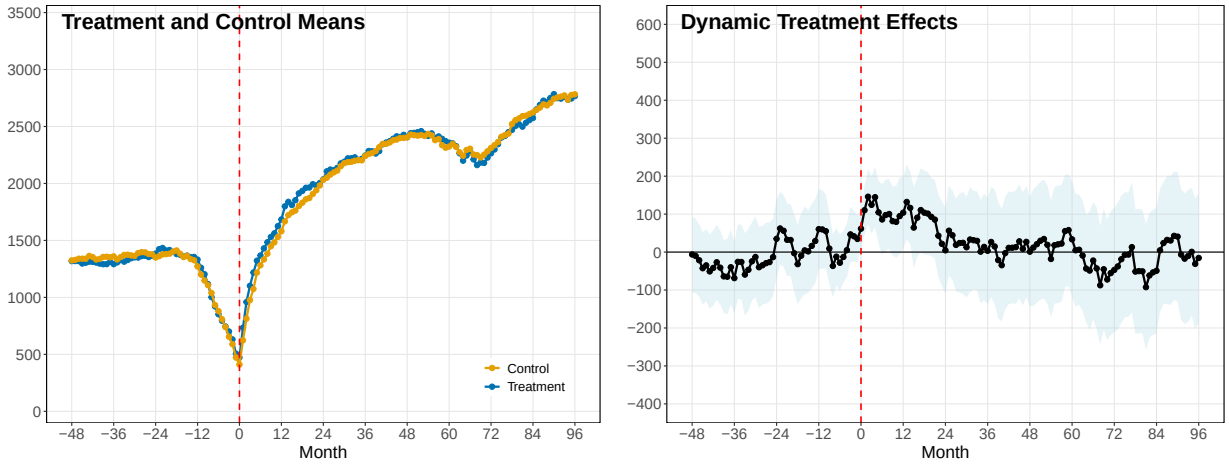
(a) Full Sample



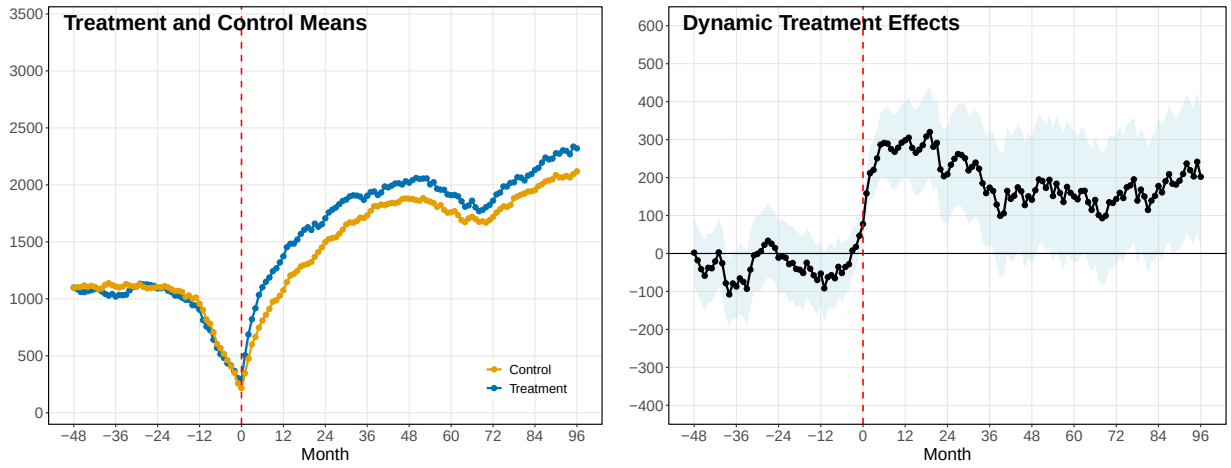
(b) Stock Sample



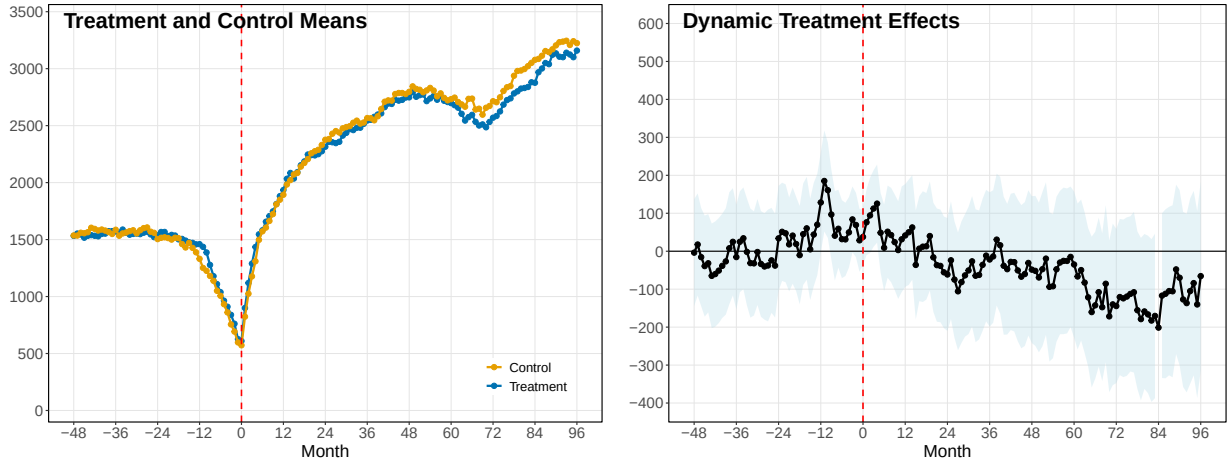
(c) Flow Sample



(d) Arabs



(e) Jews



Note: The figure reports earnings for treatment and control groups (left panel) and the difference in earnings between the groups, along with 95% confidence intervals (right panel) over time, controlling for gender, age, marital status, education level, number of children, single mother dummy, immigrant dummy, and randomization block fixed effects. Month zero corresponds to the month of random assignment. Standard errors are clustered at the randomization unit level.

Table 1: Program effects on participants

	Full Sample			Stock Sample		
Outcome	All (1)	Arabs (2)	Jews (3)	All (4)	Arabs (5)	Jews (6)
Ever employed (months 0 to 96)	0.044	0.089	0.003	0.076	0.125	0.024
	(0.007)	(0.010)	(0.008)	(0.015)	(0.018)	(0.019)
	0.811	*0.753*	*0.875*	*0.765*	*0.709*	*0.831*
Employed (month 96)	0.014	0.038	-0.011	0.033	0.060	0.008
	(0.009)	(0.012)	(0.012)	(0.017)	(0.024)	(0.024)
	0.427	*0.380*	*0.479*	*0.391*	*0.347*	*0.444*
Earnings (month 96)	68.279	203.053	-79.757	249.099	363.104	158.826
	(57.889)	(78.416)	(85.893)	(106.403)	(154.577)	(163.286)
	2,411.6	*1,951.0*	*2,915.4*	*2,039.8*	*1,660.6*	*2,491.3*
Total months employed (months 0 to 96)	3.661	7.372	-0.168	5.970	10.057	1.353
	(0.612)	(0.818)	(0.688)	(1.148)	(1.469)	(1.448)
	37.248	*30.931*	*44.158*	*33.320*	*27.642*	*40.081*
Total earnings (months 0 to 96)	9,173.1	21,311.6	-4,959.8	19,872.3	31,250.1	6,877.0
	(3,498.7)	(4,624.8)	(4,618.3)	(6,137.1)	(8,328.0)	(9,594.6)
	181,316.2	*142,225.7*	*224,074.2*	*152,539.4*	*121,562.5*	*189,423.9*
Any criminal charge (months 0 to 96)	-0.008	-0.008	-0.008	-0.015	-0.008	-0.024
	(0.004)	(0.006)	(0.006)	(0.008)	(0.010)	(0.013)
	0.070	*0.055*	*0.085*	*0.072*	*0.053*	*0.095*
Number of observations	17,906	8,364	9,542	5,294	2,671	2,623

Notes: This table shows the long-term effects of the program on its participants. Results are presented separately for the full sample (columns 1–3) and the stock sample (columns 4–6), each broken down by ethno-religious group (Arabs and Jews). Each cell reports the estimated coefficient from a separate regression. Controls include sex, marital status, age, number of children, schooling level, indicators for new immigrant, single mother, Arab, ultra-orthodox Jew, self-reported health limitations, earnings and months worked before randomization, and randomization unit fixed effects. Monetary values in real 2020 NIS. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses.

Table 2: Program effects when both spouses are treated

	Full sample			Stock Sample		
Outcome	All (1)	Arabs (2)	Jews (3)	All (4)	Arabs (5)	Jews (6)
Ever employed (months 0 to 96)	0.078	0.107	0.048	0.095	0.127	0.022
	(0.019)	(0.024)	(0.029)	(0.034)	(0.043)	(0.061)
	0.748	*0.717*	*0.815*	*0.737*	*0.714*	*0.800*
Spouse ever employed (months 0 to 96)	0.067	0.083	0.054	0.115	0.128	0.089
	(0.017)	(0.021)	(0.029)	(0.029)	(0.034)	(0.051)
	0.752	*0.727*	*0.806*	*0.691*	*0.655*	*0.790*
Both ever employed (months 0 to 96)	0.093	0.120	0.079	0.122	0.157	0.054
	(0.022)	(0.027)	(0.038)	(0.037)	(0.043)	(0.070)
	0.581	*0.545*	*0.662*	*0.556*	*0.516*	*0.667*
Total earnings (months 0 to 96)	15,399.2	26,334.3	8,063.6	14,935.0	25,278.6	-4,677.2
	(7,637.0)	(8,491.5)	(15,522.7)	(10,899.7)	(12,561.7)	(22,589.1)
	143,370.8	*121,917.2*	*191,061.8*	*147,024.5*	*133,304.7*	*184,525.6*
Total household earnings (months 0 to 96)	31,229.0	51,877.5	17,917.7	35,620.7	50,337.5	7,496.2
	(14,181.6)	(15,087.6)	(28,503.4)	(19,623.2)	(21,665.9)	(34,181.0)
	287,168.2	*246,228.8*	*378,175.6*	*272,513.4*	*239,287.3*	*363,331.3*
Number of observations	3,431	2,175	1,256	1,355	902	453

Notes: This table shows the program's effects within households where both partners were registered at the employment office and were consequently assigned jointly to either the treatment or control group. Results are presented separately for the full sample (columns 1–3) and the stock sample (columns 4–6), each broken down by ethno-religious group (Arabs and Jews). Each cell reports the estimated coefficient from a separate regression. Controls include sex, age, number of children, schooling level, indicators for new immigrant, Arab, ultra-orthodox Jew, self-reported health limitations, earnings and months worked before randomization, and randomization unit fixed effects. Monetary values in real 2020 NIS. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses.

Table 3: Program effects on participants and their untreated spouses

	Full Sample			Stock Sample		
Outcome	All (1)	Arabs (2)	Jews (3)	All (4)	Arabs (5)	Jews (6)
Ever employed (months 0 to 96)	0.056	0.087	0.004	0.104	0.145	0.040
	(0.013)	(0.018)	(0.019)	(0.025)	(0.030)	(0.043)
	0.784	*0.729*	*0.879*	*0.727*	*0.670*	*0.826*
Spouse ever employed (months 0 to 96)	0.041	0.029	0.066	0.076	0.070	0.096
	(0.015)	(0.018)	(0.026)	(0.034)	(0.041)	(0.056)
	0.596	*0.579*	*0.626*	*0.631*	*0.613*	*0.661*
Both ever employed (months 0 to 96)	0.064	0.066	0.063	0.129	0.146	0.110
	(0.016)	(0.020)	(0.027)	(0.033)	(0.041)	(0.055)
	0.479	*0.432*	*0.560*	*0.480*	*0.425*	*0.579*
Total earnings (months 0 to 96)	9,312.0	16,768.4	-9,411.0	20,665.5	38,569.8	-11,440.8
	(6,329.5)	(6,921.9)	(11,607.8)	(13,883.8)	(16,903.4)	(24,989.0)
	173,524.0	*138,467.4*	*234,106.5*	*153,025.4*	*113,779.8*	*221,786.5*
Total household earnings (months 0 to 96)	12,898.9	22,744.1	-12,906.7	56,071.2	63,677.1	48,944.7
	(10,476.3)	(12,232.8)	(19,898.0)	(25,379.2)	(33,656.2)	(38,498.6)
	341,959.3	*288,292.5*	*434,702.5*	*313,056.5*	*257,816.3*	*409,841.0*
Number of observations	5,166	2,923	2,243	1,360	816	544

Notes: This table shows the program's effects within households in which only one partner was eligible for the program. Results are presented separately for the full sample (columns 1–3) and the stock sample (columns 4–6), each broken down by ethno-religious group (Arabs and Jews). Each cell reports the estimated coefficient from a separate regression. Controls include sex, age, number of children, schooling level, indicators for new immigrant, Arab, ultra-orthodox Jew, self-reported health limitations, earnings and months worked before randomization, and randomization unit fixed effects. Monetary values in real 2020 NIS. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses.

Table 4: Program effects on parents

	Full Sample			Stock Sample		
Outcome	All (1)	Arabs (2)	Jews (3)	All (4)	Arabs (5)	Jews (6)
Ever employed (months 0 to 96)	0.077	0.126	0.005	0.094	0.151	0.007
	(0.014)	(0.020)	(0.018)	(0.026)	(0.034)	(0.037)
	0.719	*0.632*	*0.853*	*0.699*	*0.638*	*0.801*
Total months employed (months 0 to 24)	2.489	3.962	0.518	3.476	4.961	1.225
	(0.315)	(0.356)	(0.460)	(0.544)	(0.625)	(0.719)
	6.402	*4.269*	*9.733*	*5.388*	*3.593*	*8.411*
Total earnings (months 0 to 24)	4,966.2	8,419.3	132.472	8,310.5	13,185.4	577.462
	(1,214.7)	(1,285.2)	(2,169.3)	(1,984.1)	(2,201.9)	(3,412.8)
	22,957.8	*14,575.9*	*36,047.3*	*18,131.9*	*10,692.6*	*30,666.5*
Total household earnings (months 0 to 24)	8,996.3	13,044.9	2,345.3	12,792.1	19,225.7	2,218.0
	(1,835.4)	(2,339.7)	(2,920.9)	(3,289.4)	(4,042.1)	(5,518.0)
	42,432.3	*34,619.3*	*54,633.5*	*36,044.8*	*27,336.7*	*50,717.5*
Total months employed (months 0 to 96)	5.579	9.523	0.178	8.621	12.937	1.964
	(1.166)	(1.504)	(1.652)	(1.997)	(2.526)	(2.626)
	31.905	*24.120*	*44.063*	*28.946*	*22.228*	*40.267*
Total earnings (months 0 to 96)	11,703.7	25,751.4	-8,447.2	34,459.0	49,112.0	10,964.4
	(6,538.0)	(7,540.8)	(11,450.3)	(10,418.6)	(12,842.7)	(16,560.2)
	144,331.3	*99,164.3*	*214,866.1*	*121,334.2*	*84,017.4*	*184,210.4*
Total household earnings (months 0 to 96)	20,747.9	40,651.6	-9,189.8	49,779.3	67,974.5	22,493.1
	(9,910.4)	(12,484.1)	(16,495.3)	(17,754.5)	(23,299.3)	(26,898.9)
	254,618.1	*213,123.4*	*319,418.1*	*225,007.4*	*188,313.1*	*286,834.8*
Any criminal charge (months 0 to 96)	0.001	-0.006	0.010	-0.004	-0.016	0.007
	(0.007)	(0.009)	(0.014)	(0.012)	(0.015)	(0.025)
	0.044	*0.037*	*0.056*	*0.046*	*0.045*	*0.048*
Number of observations	5,067	2,715	2,352	1,774	1,000	774

Notes: This table shows program effects for participants with children aged 11–18 at the time of program allocation. Results are presented separately for the full sample (columns 1–3) and the stock sample (columns 4–6), each broken down by ethno-religious group (Arabs and Jews). Each cell reports the estimated coefficient from a separate regression. Controls include sex, marital status, age, number of children, schooling level, indicators for new immigrant, single mother, Arab, ultra-orthodox Jew, self-reported health limitations, earnings and months worked before randomization, and randomization unit fixed effects. Monetary values in real 2020 NIS. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses.

Table 5: Program effects on children during adolescence

	Full Sample			Stock Sample		
Outcome	All (1)	Arabs (2)	Jews (3)	All (4)	Arabs (5)	Jews (6)
Dropout	-0.021	-0.025	-0.013	-0.033	-0.053	0.007
	(0.011)	(0.014)	(0.016)	(0.019)	(0.024)	(0.031)
	0.194	*0.207*	*0.169*	*0.195*	*0.211*	*0.162*
Matriculation eligibility	-0.002	0.003	-0.022	0.033	0.042	-0.007
	(0.013)	(0.017)	(0.021)	(0.023)	(0.030)	(0.038)
	0.351	*0.354*	*0.345*	*0.328*	*0.336*	*0.312*
Matriculation units	0.504	0.806	-0.323	1.797	2.503	-0.111
	(0.427)	(0.540)	(0.687)	(0.754)	(0.937)	(1.251)
	14.336	*14.573*	*13.874*	*13.397*	*13.664*	*12.854*
Juvenile criminal record	-0.008	-0.006	-0.010	-0.013	-0.020	0.002
	(0.004)	(0.005)	(0.008)	(0.008)	(0.009)	(0.015)
	0.032	*0.025*	*0.046*	*0.038*	*0.038*	*0.038*
Number of observations	10,026	5,843	4,183	3,446	2,081	1,365

Notes: This table shows program effects for participants' children who were between 11 and 18 years old at the time of their parents' program assignment. Results are presented separately for the full sample (columns 1–3) and the stock sample (columns 4–6), each broken down by ethno-religious group (Arabs and Jews). Each cell reports the estimated coefficient from a separate regression. Controls include child gender, child age at the time of parental program assignment, and the same controls used in the participants' regressions. Standard errors clustered at the household level in parentheses.

Table 6: Program effects on children during early adulthood

	Full Sample			Stock Sample		
Outcome	All (1)	Arabs (2)	Jews (3)	All (4)	Arabs (5)	Jews (6)
Post-secondary studies	0.002	0.005	-0.004	0.014	0.021	0.003
	(0.005)	(0.007)	(0.006)	(0.009)	(0.012)	(0.010)
	0.059	*0.069*	*0.041*	*0.059*	*0.068*	*0.040*
Annual months employed	0.055	0.128	-0.110	0.068	0.129	-0.256
	(0.090)	(0.111)	(0.147)	(0.161)	(0.191)	(0.261)
	4.024	*3.771*	*4.508*	*4.327*	*4.155*	*4.660*
Annual earnings	-171.411	234.111	-906.984	277.475	1,111.9	-2,055.5
	(562.991)	(698.526)	(904.652)	(961.984)	(1,150.0)	(1,594.1)
	20,692.5	*20,057.6*	*21,909.9*	*21,918.1*	*21,455.6*	*22,816.9*
Any criminal charge (ages 18+)	-0.003	-0.005	0.001	-0.003	-0.016	0.020
	(0.003)	(0.004)	(0.006)	(0.007)	(0.008)	(0.013)
	0.016	*0.011*	*0.025*	*0.020*	*0.021*	*0.019*
Number of observations	48,221	27,943	20,278	16,704	9,924	6,780

Notes: This table shows program effects for participants' children who were between 11 and 18 years old at the time of their parents' program assignment. Results are presented separately for the full sample (columns 1–3) and the stock sample (columns 4–6), each broken down by ethno-religious group (Arabs and Jews). Each cell reports the estimated coefficient from a separate regression. Controls include child gender, child age at the time of parental program assignment, and the same controls used in the participants' regressions. Standard errors clustered at the household level in parentheses.

A Appendix

A1 Appendix Tables

Table A1: Balance Table - Program Participants

	Mean Treated (1)	T-C (2)
Female	0.558	−0.012
		(0.009)
Age	35.902	−0.025
		(0.159)
Married	0.494	−0.004
		(0.009)
Number of children	2.513	−0.019
		(0.048)
Single parent	0.201	−0.002
		(0.007)
Arab	0.433	0.013
		(0.006)
Ultra-Orthodox	0.155	0.007
		(0.005)
Immigrant	0.187	−0.015
		(0.007)
Less than 12 years of schooling	0.362	−0.004
		(0.008)
12 years of schooling	0.452	0.006
		(0.009)
More than 12 years of schooling	0.107	0.000
		(0.005)
Missing schooling data	0.079	−0.002
		(0.005)
Self-reported health limitation	0.387	0.007
		(0.008)
Total months employed (months -12 to -1)	2.666	0.068
		(0.065)
Total earnings (months -12 to -1)	8,772.3	239.943
		(257.391)
F Stat for joint significance		1.242
P value		0.241
Number of observations	11,032	17,906

Table A2: Balance Table - Children Sample

	Mean Treated (1)	T-C (2)
Child female	0.480	−0.021
		(0.013)
Child age	14.621	−0.059
		(0.040)
Parent female	0.646	−0.026
		(0.017)
Parent age	41.975	−0.325
		(0.170)
Parent number of children	4.924	−0.039
		(0.088)
Single parent	0.265	0.006
		(0.015)
Parent Arab	0.534	0.003
		(0.010)
Parent Ultra-Orthodox	0.217	−0.002
		(0.009)
Parent immigrant	0.136	−0.013
		(0.010)
Parent less than 12 years of schooling	0.488	−0.004
		(0.018)
Parent 12 years of schooling	0.383	0.010
		(0.017)
Parent more than 12 years of schooling	0.073	0.007
		(0.008)
Parent missing schooling data	0.055	−0.013
		(0.011)
Parent self-reported health limitation	0.425	−0.006
		(0.018)
Parent total months employed (months -12 to -1)	1.883	0.085
		(0.117)
Parent total earnings (months -12 to -1)	5,924.0	366.898
		(418.944)
F Stat for joint significance		1.114
P value		0.337
Number of households	3,197	5,049
Number of observations	6,196	10,026

Table A3: Program effects on participants – Flow sample

	Flow Sample		
Outcome	All (1)	Arabs (2)	Jews (3)
Employed (month 96)	0.007	0.029	-0.017
	(0.010)	(0.014)	(0.013)
	0.436	*0.388*	*0.487*
Earnings (month 96)	3.130	140.657	-157.381
	(68.526)	(90.055)	(99.660)
	2,500.0	*2,023.5*	*3,010.8*
Total months employed (months 0 to 96)	2.825	6.324	-0.710
	(0.721)	(0.972)	(0.773)
	38.183	*31.753*	*45.076*
Total earnings (months 0 to 96)	5,246.1	17,512.9	-9,080.2
	(4,180.5)	(5,476.3)	(5,193.8)
	188,162.0	*147,389.7*	*231,870.5*
Married (month 96)	-0.001	-0.017	0.010
	(0.007)	(0.012)	(0.010)
	0.566	*0.728*	*0.392*
Number of children (months 0 to 96)	-0.031	-0.067	-0.006
	(0.020)	(0.030)	(0.027)
	0.769	*0.873*	*0.657*
Any criminal charge (months 0 to 96)	-0.006	-0.008	-0.003
	(0.005)	(0.007)	(0.007)
	0.069	*0.056*	*0.083*
Number of observations	12,592	5,668	6,901

Notes: This table shows the long-term effects of the program on participants from the flow sample. Each cell reports the estimated coefficient from a separate regression. Controls include sex, marital status, age, number of children, schooling level, indicators for new immigrant, single mother, Arab, ultra-orthodox Jew, self-reported health limitations, earnings and months worked before randomization, and randomization unit fixed effects. Monetary values in real 2020 NIS. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses.

Table A4: Program effects on participants – additional employment outcomes

	Full Sample			Stock Sample		
Outcome	All (1)	Arabs (2)	Jews (3)	All (4)	Arabs (5)	Jews (6)
Maximum employment spell	2.055	4.513	-0.582	3.585	6.088	0.675
	(0.470)	(0.657)	(0.540)	(0.877)	(1.240)	(1.136)
	23.816	*19.718*	*28.298*	*21.718*	*17.946*	*26.209*
Employed in a large firm (month 96)	0.010	0.025	-0.008	0.015	0.038	-0.009
	(0.008)	(0.011)	(0.010)	(0.014)	(0.018)	(0.023)
	0.253	*0.201*	*0.310*	*0.219*	*0.166*	*0.282*
Ever employed in a large firm	0.019	0.049	-0.012	0.042	0.089	-0.011
	(0.009)	(0.013)	(0.011)	(0.017)	(0.025)	(0.023)
	0.587	*0.489*	*0.693*	*0.522*	*0.418*	*0.647*
Number of observations	17,906	8,364	9,542	5,294	2,671	2,623

Notes: This table shows the long-term effects of the program on its participants. Results are presented separately for the full sample (columns 1–3) and the stock sample (columns 4–6), each broken down by ethno-religious group (Arabs and Jews). Each cell reports the estimated coefficient from a separate regression. Controls include sex, marital status, age, number of children, schooling level, indicators for new immigrant, single mother, Arab, ultra-orthodox Jew, self-reported health limitations, earnings and months worked before randomization, and randomization unit fixed effects. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses.

Table A5: Program effects on participants – marriage, fertility, and residential location

	Full Sample			Stock Sample		
Outcome	All (1)	Arabs (2)	Jews (3)	All (4)	Arabs (5)	Jews (6)
Married (month 96)	-0.005	-0.019	0.005	-0.016	-0.023	-0.010
	(0.006)	(0.009)	(0.009)	(0.011)	(0.014)	(0.019)
	0.573	*0.735*	*0.395*	*0.600*	*0.763*	*0.406*
Number of children born (months 0 to 96)	-0.029	-0.068	0.000	-0.021	-0.071	0.021
	(0.016)	(0.024)	(0.022)	(0.027)	(0.037)	(0.040)
	0.738	*0.827*	*0.641*	*0.609*	*0.641*	*0.570*
Same locality (month 96)	0.006	0.004	0.006	0.013	0.019	0.008
	(0.007)	(0.008)	(0.011)	(0.012)	(0.015)	(0.020)
	0.823	*0.892*	*0.746*	*0.854*	*0.918*	*0.775*
Number of observations	17,301	8,206	9,095	5,178	2,637	2,541

Notes: This table shows the long-term effects of the program on its participants. Results are presented separately for the full sample (columns 1–3) and the stock sample (columns 4–6), each broken down by ethno-religious group (Arabs and Jews). Each cell reports the estimated coefficient from a separate regression. Controls include sex, marital status, age, number of children, schooling level, indicators for new immigrant, single mother, Arab, ultra-orthodox Jew, self-reported health limitations, earnings and months worked before randomization, and randomization unit fixed effects. Monetary values in real 2020 NIS. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses.

Table A6: Program effects on participants and their spouses – combined sample

	Full Sample			Stock Sample		
Outcome	All (1)	Arabs (2)	Jews (3)	All (4)	Arabs (5)	Jews (6)
Ever employed	0.066	0.096	0.019	0.102	0.137	0.029
	(0.010)	(0.014)	(0.016)	(0.020)	(0.025)	(0.032)
	0.769	*0.724*	*0.855*	*0.732*	*0.695*	*0.814*
Spouse ever employed	0.045	0.048	0.048	0.080	0.086	0.080
	(0.012)	(0.014)	(0.020)	(0.023)	(0.028)	(0.038)
	0.660	*0.643*	*0.693*	*0.663*	*0.637*	*0.721*
Both ever employed	0.074	0.088	0.058	0.116	0.145	0.065
	(0.013)	(0.017)	(0.023)	(0.026)	(0.032)	(0.042)
	0.521	*0.481*	*0.598*	*0.521*	*0.477*	*0.619*
Both ever employed at the same employer	0.024	0.027	0.019	0.033	0.040	0.026
	(0.009)	(0.011)	(0.017)	(0.016)	(0.020)	(0.028)
	0.107	*0.089*	*0.143*	*0.106*	*0.088*	*0.146*
Number of observations	8,597	5,098	3,499	2,715	1,718	997

Notes: This table shows the program's effects across all households that included a spouse, pooling the samples of Tables 2 and 3 (households where both partners were treated and households where only one partner was eligible). Results are presented separately for the full sample (columns 1–3) and the stock sample (columns 4–6), each broken down by ethno-religious group (Arabs and Jews). Each cell reports the estimated coefficient from a separate regression. Controls include sex, age, number of children, schooling level, indicators for new immigrant, Arab, ultra-orthodox Jew, self-reported health limitations, earnings and months worked before randomization, and randomization unit fixed effects. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses.

Table A7: Program effects on parents – marriage, fertility, and residential location

	Full Sample			Stock Sample		
Outcome	All (1)	Arabs (2)	Jews (3)	All (4)	Arabs (5)	Jews (6)
Married (month 96)	0.003	-0.011	0.033	-0.005	0.004	-0.009
	(0.011)	(0.012)	(0.021)	(0.016)	(0.018)	(0.034)
	0.652	*0.791*	*0.434*	*0.666*	*0.780*	*0.473*
Number of children born (months 0 to 96)	-0.022	-0.025	-0.022	-0.046	-0.070	-0.029
	(0.024)	(0.031)	(0.037)	(0.036)	(0.044)	(0.061)
	0.355	*0.356*	*0.352*	*0.281*	*0.264*	*0.308*
Same locality (month 96)	0.021	0.014	0.034	0.019	0.015	0.029
	(0.011)	(0.013)	(0.021)	(0.018)	(0.021)	(0.038)
	0.861	*0.917*	*0.773*	*0.883*	*0.939*	*0.789*
Number of observations	4,935	2,664	2,271	1,740	988	752

Notes: This table shows program effects for participants with children aged 11–18 at the time of program allocation. Results are presented separately for the full sample (columns 1–3) and the stock sample (columns 4–6), each broken down by ethno-religious group (Arabs and Jews). Each cell reports the estimated coefficient from a separate regression. Controls include sex, marital status, age, number of children, schooling level, indicators for new immigrant, single mother, Arab, ultra-orthodox Jew, self-reported health limitations, earnings and months worked before randomization, and randomization unit fixed effects. Monetary values in real 2020 NIS. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses.

Table A8: Effects on children during adolescence by age at time of parental assignment

	All		Arabs		Stock		Arabs in Stock	
Outcome	Ages 11-14 (1)	Ages 15-18 (2)	Ages 11-14 (3)	Ages 15-18 (4)	Ages 11-14 (5)	Ages 15-18 (6)	Ages 11-14 (7)	Ages 15-18 (8)
Dropout	-0.027	-0.018	-0.037	-0.013	-0.044	-0.030	-0.066	-0.042
	(0.015)	(0.014)	(0.020)	(0.018)	(0.026)	(0.026)	(0.034)	(0.031)
	0.177	*0.207*	*0.185*	*0.225*	*0.166*	*0.215*	*0.187*	*0.227*
Matriculation eligibility	0.015	-0.013	0.017	-0.009	0.077	0.008	0.087	0.008
	(0.019)	(0.016)	(0.025)	(0.021)	(0.035)	(0.029)	(0.044)	(0.037)
	0.385	*0.324*	*0.393*	*0.324*	*0.334*	*0.323*	*0.340*	*0.333*
Matriculation units	1.336	-0.027	1.795	0.087	3.485	0.669	4.592	0.934
	(0.751)	(0.422)	(0.937)	(0.536)	(1.387)	(0.748)	(1.665)	(0.943)
	18.479	*11.157*	*18.868*	*11.305*	*16.997*	*10.947*	*17.024*	*11.477*
Juvenile criminal record	-0.007	-0.008	-0.002	-0.007	-0.012	-0.010	-0.007	-0.028
	(0.005)	(0.007)	(0.005)	(0.008)	(0.011)	(0.012)	(0.011)	(0.014)
	0.016	*0.044*	*0.013*	*0.034*	*0.028*	*0.045*	*0.029*	*0.044*
Number of observations	4,195	5,831	2,409	3,434	1,314	2,132	771	1,310

Notes: This table shows program effects on outcomes for participants' children, estimated separately by age at the time of parental program assignment (ages 11–14 vs. ages 15–18). Results are presented separately for the full sample (columns 1–2), the Arab sample (columns 3–4), the stock sample (columns 5–6), and Arabs within the stock sample (columns 7–8). Each cell reports the estimated coefficient from a separate regression. Controls include child gender, child age at the time of parental program assignment, and the same controls used in the participants' regressions. Standard errors clustered at the household level in parentheses.

Table A9: Effects on children during adolescence by gender

	All		Arabs		Stock		Arabs in Stock	
Outcome	Boys (1)	Girls (2)	Boys (3)	Girls (4)	Boys (5)	Girls (6)	Boys (7)	Girls (8)
Dropout	-0.022	-0.020	-0.007	-0.035	-0.021	-0.036	-0.027	-0.062
	(0.016)	(0.013)	(0.021)	(0.017)	(0.029)	(0.023)	(0.038)	(0.027)
	0.268	*0.122*	*0.277*	*0.139*	*0.268*	*0.121*	*0.292*	*0.136*
Matriculation eligibility	-0.006	0.001	-0.007	0.015	0.002	0.067	0.003	0.088
	(0.016)	(0.020)	(0.020)	(0.026)	(0.028)	(0.036)	(0.036)	(0.045)
	0.237	*0.462*	*0.221*	*0.482*	*0.225*	*0.433*	*0.214*	*0.451*
Matriculation units	0.493	0.448	0.134	1.590	1.333	2.245	1.413	3.583
	(0.541)	(0.627)	(0.658)	(0.782)	(0.920)	(1.151)	(1.128)	(1.408)
	10.007	*18.585*	*9.927*	*19.082*	*9.325*	*17.574*	*9.023*	*18.033*
Juvenile criminal record	-0.012	-0.003	-0.011	0.001	-0.017	-0.004	-0.032	-0.002
	(0.008)	(0.004)	(0.010)	(0.002)	(0.014)	(0.007)	(0.017)	(0.006)
	0.053	*0.011*	*0.049*	*0.002*	*0.063*	*0.013*	*0.074*	*0.004*
Number of observations	5,121	4,905	2,945	2,898	1,797	1,649	1,056	1,025

Notes: This table shows program effects on outcomes for participants' children, estimated separately by gender. Results are presented separately for the full sample (columns 1–2), the Arab sample (columns 3–4), the stock sample (columns 5–6), and Arabs within the stock sample (columns 7–8). Each cell reports the estimated coefficient from a separate regression. Controls include child gender, child age at the time of parental program assignment, and the same controls used in the participants' regressions. Standard errors clustered at the household level in parentheses.