# 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. We also document positive spillovers on spouses, with household income gains exceeding the direct effects on participants, and significant long-term benefits for children, including improved educational attainment and reduced crime. Our results demonstrate that soft-skills interventions can produce lasting gains for participants while generating meaningful spillovers on their families.

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# 1 Introduction

Active labor market programs (ALMPs) are a common policy tool for reducing unemployment and welfare dependence, yet evidence on their long-run effectiveness remains limited (see Kluve, 2010; Card et al., 2018; Le Barbanchon et al., 2024). A main concern is that even programs that succeed in moving participants into jobs may fail to produce lasting change if the underlying barriers to sustained employment, such as deficits in motivation, self-efficacy, and workplace norms, are left unaddressed (Crépon and Van Den Berg, 2016). A further limitation of the existing evidence is its near-exclusive focus on the participants themselves, potentially overlooking effects on spouses and children, thereby yielding an incomplete picture of program impacts.

This paper provides evidence on the long-term and intergenerational effects of a large-scale soft-skills ALMP. We focus on "Employment Circles", a program launched by the Israeli Employment Service in 2014 that provides income-support claimants with mentoring, group therapy, and job-search coaching aimed at building self-efficacy, motivation, and workplace readiness. Leveraging a randomized controlled trial (RCT) that assigned over 17,500 individuals to treatment and control groups and linking participants to comprehensive administrative records from multiple government agencies (tax records, police records, education registries, 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 program's short- to medium-run effects of the program: twelve months after assignment, treatment-group members were eight percentage points more likely to be employed and earned roughly 16 percent more than controls. The short-run evidence also reveals positive effects on participants' soft skills, including job-search self-efficacy, work self-efficacy, and self-esteem, primarily among those with no recent employment history. Our paper extends this analysis by examining whether the employment and earnings gains persist over a longer horizon, consider-

ing outcomes beyond employment and earnings, and estimating effects on participants' spouses and children.

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, improvements in parental human capital and noncognitive skills can transmit new attitudes toward work, potentially breaking the intergenerational cycle of welfare dependence.

We find that the program's effects on participants persist also 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, characterized 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 long-run experimental estimates of a soft-skills program's effect on a comprehensive set of outcomes that extend well beyond the employment and welfare measures typically examined in the literature. The persistent employment gains we document are consistent with the view that non-cognitive skills play a durable role in labor market success (see e.g. Heckman et al., 2006) and suggest that the program's emphasis on non-cognitive skills development, which are particularly important for workers in low-skilled occupations (Aghion et al., 2019), may underlie the sustained labor market effects we observe. Our results thus complement the literature on the returns to non-cognitive skills that has focused primarily on childhood and adolescence, by showing that targeted interventions can produce lasting gains among disadvantaged adults.[1]

We also contribute to the growing body of work on the effects of parental economic circumstances on children (see, e.g., Dahl and Lochner, 2012; Aizer et al., 2014; Milligan and Stabile, 2011; Hoynes et al., 2016; Bastian and Michelmore, 2018; Aizer et al., 2022; Aizer et al., 2023). While much of this literature studies income transfers, we provide evidence from a program that operates through parental soft-skills enhancement and employment rather than cash. A related literature examines the impact of parental job loss on children's outcomes (Oreopoulos et al., 2008; Rege et al., 2011; Hilger, 2016; Carneiro et al., 2022). Our setting differs in two important ways: it captures a positive transition into employment rather than a negative shock, and participants are long-term unemployed individuals who often lack the work habits and soft skills that job losers likely possessed. To our knowledge, this is among the first papers to document causal intergenerational effects of an ALMP.

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

[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.

sizes 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. 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.[2]

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 mo-

[2]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.

tivation, 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.[3] 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.

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

[3]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.

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 income by 12%, and reduced welfare dependency by 11 percentage points (a 26% decline). These effects persisted 18 months after program assignment.[4] 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, improvements in soft skills (including job-search self-efficacy, work self-efficacy, general self-efficacy, self-esteem, and grit) 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 show that the program generated no externalities 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 an MVPF of infinity.

In this paper, we extend the analysis in several directions. First, we substantially expand the sample to include all individuals randomly assigned during the two years of the RCT (17,906 individuals). Second, we estimate the long-term effects of the program on employment, earnings, and outcomes that have received limited attention in the ALMP literature, including crime, fertility, and marital stability, by following individuals over

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

eight years. Third, we examine spillover effects on participants' spouses, focusing on employment and earnings, and also 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 fictitious unique identifiers assigned to each individual 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, number of children, and children's ages at the time of program assignment, are drawn from the population registry, and educational attainment and postsecondary enrollment from the education registry. Employment and earnings are measured using monthly tax records. Criminal activity is obtained from police administrative records. Marital status and fertility following program assignment are drawn from the population registry. Using the population registry, we identify the spouses of experiment participants at the time of program assignment and define their employment and wages using tax records. We also construct a measure of 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.[5] For children who are at least 18

[5]The matriculation certificate is earned by passing national exams in core and elective subjects, with each

years old during the post-program 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) group during the initial phase of program implementation, from March 2014 through December 2015. We track labor market outcomes before random assignment and eight years after it. For marital status, we report the value observed in the latest available year (2021). For fertility, we report the total number of children born following randomization through 2021. For criminal activity, we construct an indicator for whether the individual has any criminal record between one year after randomization and 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 them and their spouses as recorded at program assignment over time.

The analysis of children's outcomes is based on two samples. The first, used to estimate adolescent outcomes, includes all children born before September 2003 who were under 18 at the time of parental random assignment. The restriction to pre-2003 birth cohorts reflects data availability, as finalized high school outcomes are not observed for cohorts entering school after 2009/10. This implies that children in this sample were between 11 and 18 years old at the time of their parent's program assignment. The second sample, used to estimate outcomes in early adulthood, is restricted to children who were 14 to 18 at the time of parental random assignment; this age range reflects the availabil-

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.

ity of tax records through 2023. For this sample, we estimate effects on postsecondary enrollment and labor market outcomes measured at ages 19 to 23. Criminal charges in adulthood are measured from age 18 through 2021.

# 4 Empirical Strategy and Balance Tests

To estimate the long-term effects of the program on participants, we regress each outcome on a treatment indicator, controlling for the randomization unit: employment office $\times$ randomization date $\times$ claimant type (flow/stock).[6] 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; $\gamma_{jtp}$ 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, 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

[6] 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.

the same time.

We also apply equation (1) to estimate the impact 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 the impact on children, we compare outcomes of children of individuals assigned to the treatment and control groups. The estimating equation follows equation (1) with two modifications: (a) treatment status refers to the parent's treatment assignment; and (b) the set of control variables is augmented with child-level characteristics, including the child's gender and age at the time of the parent's allocation to the program. For the analysis of children's outcomes, standard errors are clustered at the household level to account for within-family correlation across siblings.

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.[7] 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 parent was the mother; the average age of the assigned parent at baseline was 42. 27% of children come from single-parent households. 43% of the children belong to Arab households and 16% 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 further underscores the disadvantaged nature of the sample: 49% of eligible parents have fewer than 12 years of schooling, while 38% completed high school. 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 fe-

[7] 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.

males). 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

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. Importantly, however, the treatment group proved more resilient: despite the common shock, it maintained its advantage relative to the control group throughout the pandemic period, 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.

**Labor market outcomes.** The program 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 labor income over 96 months increased by approximately NIS 20,200 for Arab participants in the full sample (a 13% increase) and by NIS 30,500 for Arab participants in the stock sample (a 24% increase). In contrast, the estimated earnings effects for Jewish participants are not statistically significant. The aggregate effect for the full sample is positive but not statistically significant. Monthly labor income at month 96 follows a similar pattern, with positive and significant effects for Arab participants and for the stock sample overall, and negligible effects for Jewish participants.

**Fertility and marital status.** The program is associated with 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.[8]

[8]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.

**Crime.** The program also reduced criminal activity among participants. In the full sample, the probability of having a criminal charge in the post-randomization period fell by 0.8 percentage points, representing an 11% reduction relative to the control mean of 7%. The effect is larger in the stock sample (1.5 percentage points), and particularly pronounced among Jewish stock participants (2.4 percentage points, a 25% reduction relative to their control mean of 9.5%). Notably, this reduction in criminal activity among Jewish participants occurs despite the absence of persistent long-term employment gains for this group, suggesting that the program may have had a meaningful impact on crime even if employment gains did not persist over the longer horizon examined here.

Taken together, the results in Table 1 show that the program produced substantial and persistent labor market gains, reduced criminal activity, and lowered fertility, with effects concentrated among Arab participants and the stock sample.

## 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.[9] In these dual-treatment households, we observe significant increases in employment and earnings. Over the first two years following program assignment, both participants and their spouses in the full sample increased their cumulative employment and experienced corresponding gains in earned income. These effects are notably more pronounced in the stock sample and among Arab couples. Over an eight-year horizon, cumulative household earnings are 10% higher for treated couples in the full sample, 18% higher for Arab couples, and 13% higher for the stock sample, with the latter primarily driven by the Arab subsample. Overall, the findings from jointly al-

[9]The date of program assignment does not necessarily coincide across partners, since each individual is randomized upon registration at the employment office. Partners share the same assignment date only if both registered at the employment office at the time of allocation. When estimating the program effect on household earnings, we use the earlier assignment date when the assignment dates differ.

located households suggest that providing soft-skills training to both partners triggers a joint transition toward employment that significantly 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, thereby allowing us to observe how treatment of one partner influences the labor market outcomes of an untreated spouse. The participant-level effects 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. Over the first two years following program assignment, participants in the full sample increased their employment and earned income, with larger effects among Arab participants and the stock sample. Extending the horizon to eight years, we find that employment and earnings gains remain positive and continue to be concentrated among Arab and stock participants.

We observe remarkable positive spillover effects on the labor market outcomes of untreated spouses, particularly within the Arab and stock samples. These spillovers are especially striking when examining total household income; for households in the stock sample, the cumulative increase in labor income over eight years amounted to NIS 54,895 (16% increase relative to the control group mean). Notably, the total household gain substantially exceeds the direct earnings increase of the treated partner alone, suggesting the presence of an intra-household multiplier effect.

These results suggest that soft-skills enhancements do not merely benefit the individual trainee; they can fundamentally alter 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, or a broader shift in the household's perceived returns to labor market participation. While the underlying drivers of these dynamics cannot be fully disentangled in this study, they offer promising directions for the design and targeting of future interventions. More broadly, our results underscore a critical yet understudied dimension

in the evaluation of ALMPs: the need to account for household spillover effects.

## 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, fostering 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 5.4 additional months of employment over eight years relative to the control group (a 17% increase), compared to 3.7 months (a 10% increase) in the full sample of program participants (reported in Table 1), suggesting that effects in this subsample are, if anything, somewhat larger. The effects are again concentrated among Arab participants, who accumulated 9.4 additional months of employment (a 38% increase), with even larger effects in the Arab stock sample (12.1 additional months, a 53% 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 7% in the full sample, rising to 18% for Arab parents and 20% for the stock sample, with the largest gains among Arab stock parents, for whom the increase amounts to approximately NIS 65,000, a 32% increase relative to the control mean.

The program had no discernible effects on marital status or fertility in this sample. The estimated coefficients for both outcomes are small in magnitude and statistically insignificant across all groups. These null results are consistent with the composition of this sample: parents who already had children at the time of program assignment and were, on average, 42 years old, leaving limited scope for the program to affect either fertility or marital status. The point estimates for criminal activity are negative, suggesting a reduction in criminal charges among treated parents, though these estimates are imprecise and do not reach conventional levels of statistical significance, probably due to the smaller sample size.

**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 are highly consistent with the patterns observed for the parents. We see significant effects on children exactly among the groups where the program most effectively improved parental labor market outcomes: 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 2.2 percentage points less likely to drop out of high school (an

11% reduction). This impact is even more substantial for the stock sample, where the dropout rate fell by 3.4 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, which constitutes a 25% reduction compared 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 3.2 percentage points more likely to obtain a matriculation certificate (a 9.8% 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 0.9 percentage points less likely to have a juvenile criminal record (a 28% reduction). In the stock sample, the overall effect is 1.3 percentage points (a 34% reduction), driven primarily by Arab stock children (1.9 percentage points, a 50% reduction), while the effect for Jewish stock children is essentially zero.

**Effects on children during early adulthood.** Table 6 reports program effects on outcomes measured in early adulthood (ages 19–25) for children who were between the ages of 14 and 18 at the time of parental program assignment. Across nearly all outcomes and subsamples, the estimated effects on post-secondary enrollment, employment, and labor income are small in magnitude and statistically insignificant. The results for Jewish young adults should be interpreted with particular caution, as the majority of this group is engaged in mandatory military service during ages 18–21, which is likely to attenuate any detectable effects on employment and earnings during this period.

Among children of Arab stock participants, point estimates for earnings are positive,

consistent with the educational gains documented in Table 5 beginning to translate into early labor market advantages, though these effects are imprecisely estimated. Despite the limited labor market effects, we observe a statistically significant reduction in criminal activity among the most affected group: the probability of an adult criminal charge fell by 2.5 percentage points among children of Arab participants in the stock sample. This finding suggests that the behavioral improvements observed during adolescence persist into early adulthood. More broadly, the absence of significant labor market effects at this stage likely reflects the fact that the cohort is still in the early stages of its working life, and a longer observation window will be needed to fully capture the returns to the educational gains documented during adolescence.

**Child's age at parental assignment** We also examine heterogeneous effects by children's age at the time of parental program assignment.[10] Specifically, we distinguish between children who were aged 11–15 and those who were aged 16–18 at the time of assignment. This stratification is motivated by two considerations. First, the childhood sample consists of children aged 11 to 18, while the early-adulthood sample consists of children aged 14 to 18; the age-16 threshold falls between the median age of each sample. Second, students begin taking matriculation exams at age 16, meaning that the key educational decisions regarding effort and human capital investment in high school occur predominantly before that age.[11]

Appendix Table A4 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

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

[11] 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.

along educational dimensions, while older children show larger reductions on criminal behavior. Specifically, children aged 11–15 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. In contrast, children aged 16–18 at the time of parental program assignment display larger reductions in juvenile criminal records. This pattern may reflect the fact that older adolescents are at a stage where behavioral outcomes are more immediately responsive to changes in household circumstances. Our findings suggest that while positive shifts in the household economic environment during early adolescence are most effective at fostering children's human capital accumulation, positive transitions during late adolescence serve as a critical deterrent against criminal behavior during high-risk years.

Appendix Table A5 reports the results for outcomes in early adulthood. To ensure a sufficient tracking window (ages 19 to 23), we restrict the younger cohort to children aged 14 to 15 at the child's age at parental assignment. Estimates are noisy and imprecise across both age groups, likely reflecting the limited statistical power from the smaller subsamples and the limited tracking period for labor market outcomes. However, we document a notable persistence in the reduction of criminal activity among those who were older (ages 16 to 18). This effect is particularly pronounced for arabs in the stock sample, where the likelihood of facing a criminal charge in early adulthood declines significantly. These results align with the juvenile behavioral improvements reported in Table A4 and suggest that the program leads to a durable reduction in criminal engagement that extends beyond adolescence.

We also examined potential heterogeneity based on the gender of the child and which parent was assigned to the program: mother, father, or both. In these analyses, our

estimates remained imprecise, reflecting the reduced statistical power inherent in these smaller subsamples. As more 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. This will allow 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

The transformative potential of social policy rests on its capacity to break the intergenerational transmission of disadvantage by generating benefits that propagate from targeted individuals to their spouses and children. This paper addresses this question by examining the long-term and intergenerational effects of a large-scale Active Labor Market Program that provided soft-skills training to welfare recipients in Israel. 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. 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 further underscore 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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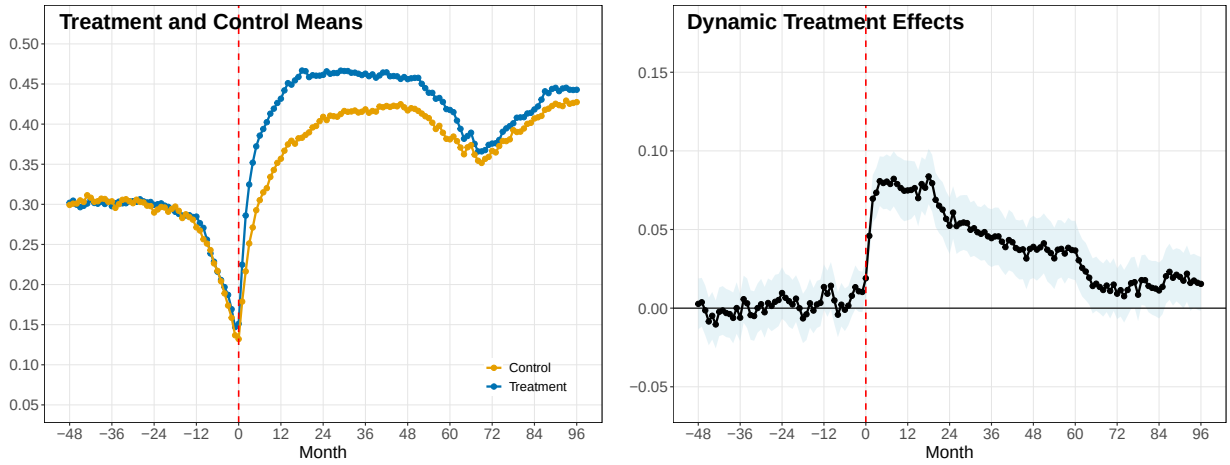
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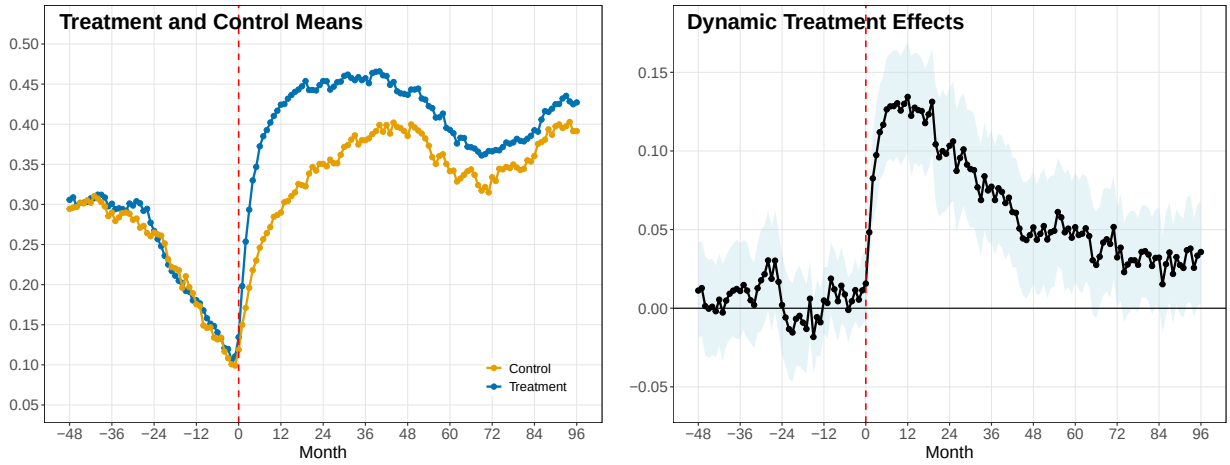
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Figure 1: Employment

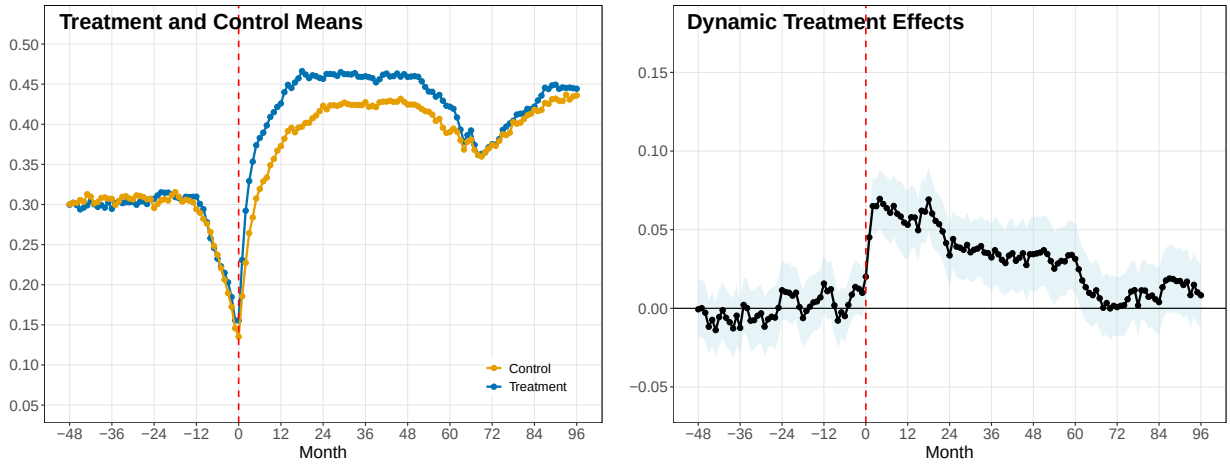
(a) Full Sample



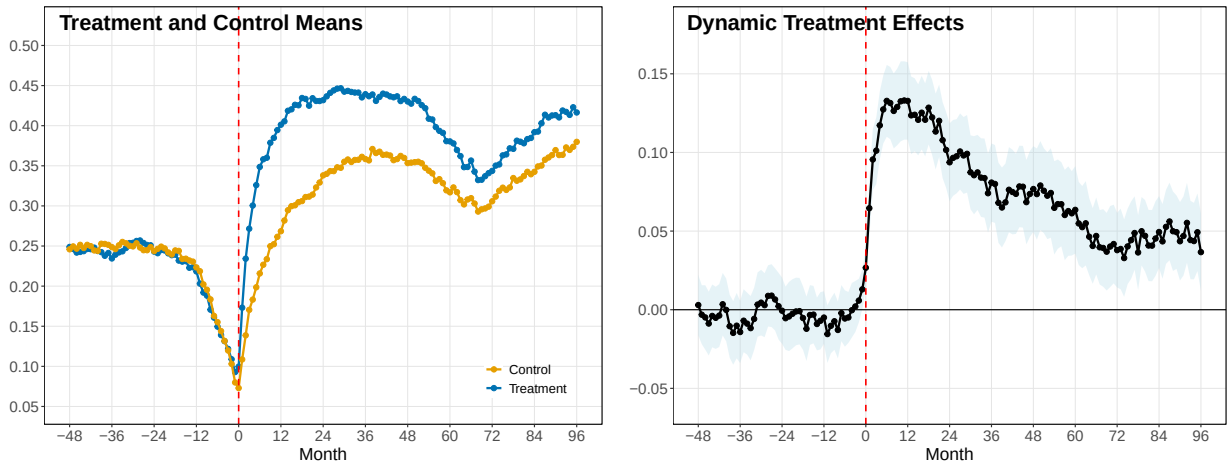

(b) Stock Sample



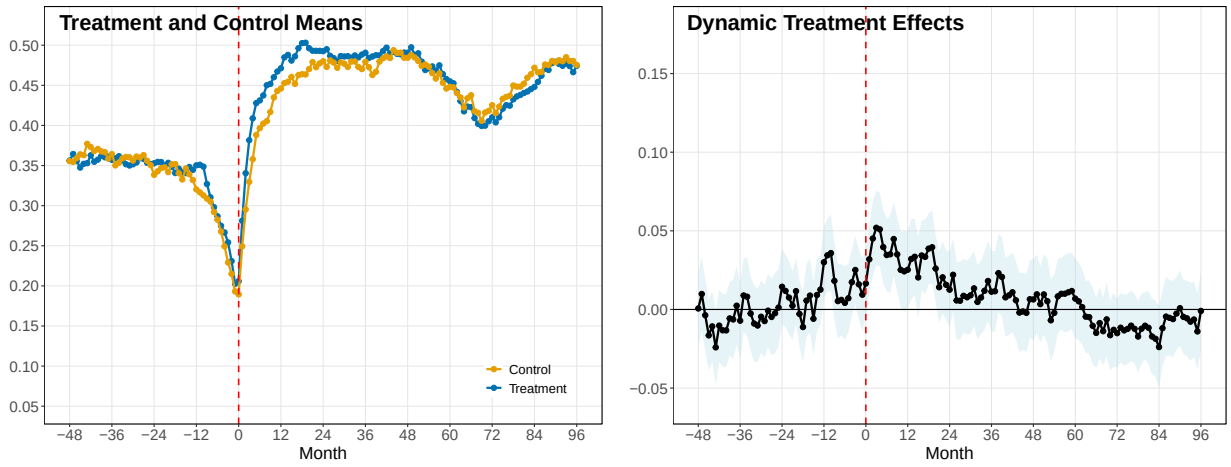

(c) Flow Sample



(d) Arabs



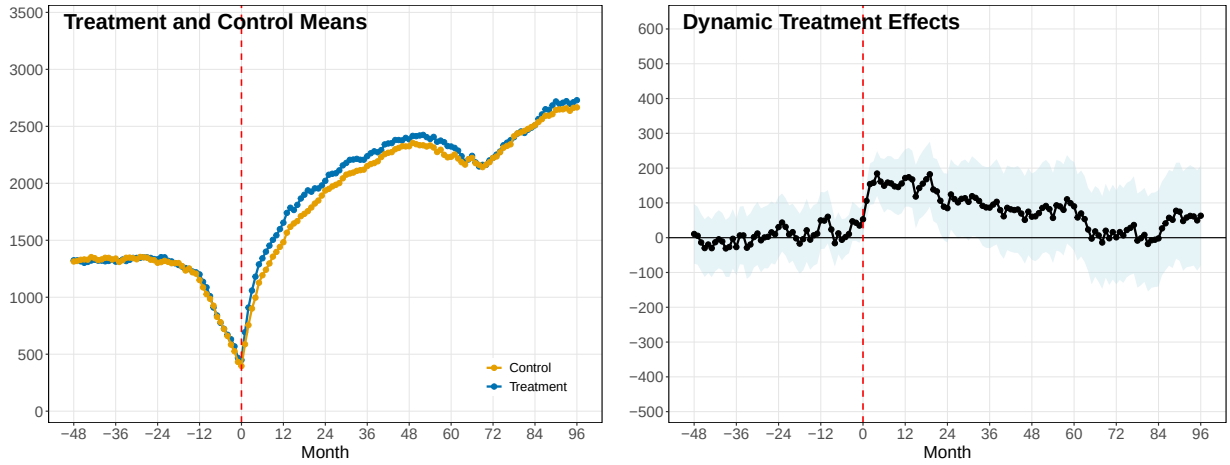

(e) Jews



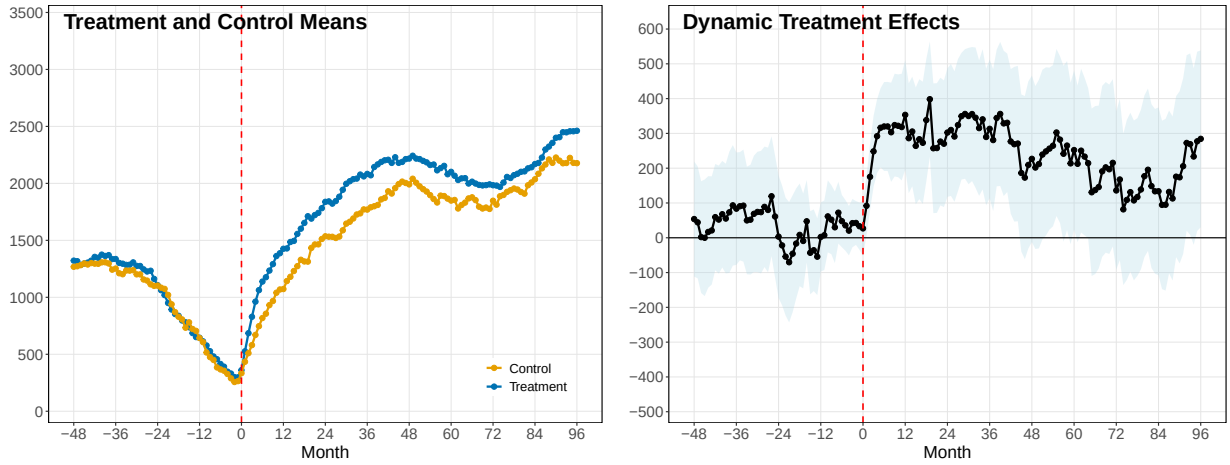

Note: The figure reports employment rates for treatment and control groups (left panel) and the difference in employment rates between the groups, along with 95% confidence intervals (right panel) over time, controlling for gender, age, marital status, education level, number of children, single mother 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

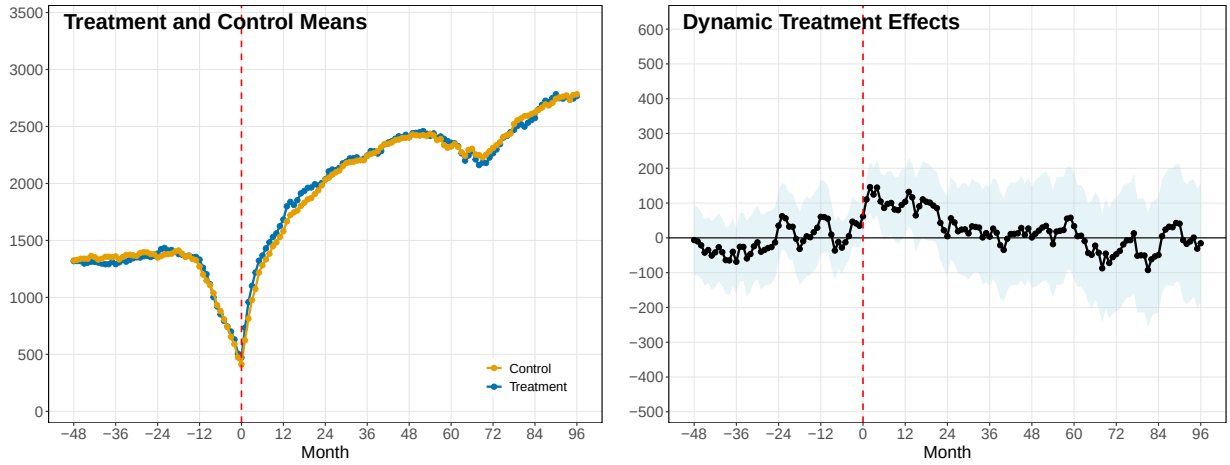
(a) Full Sample



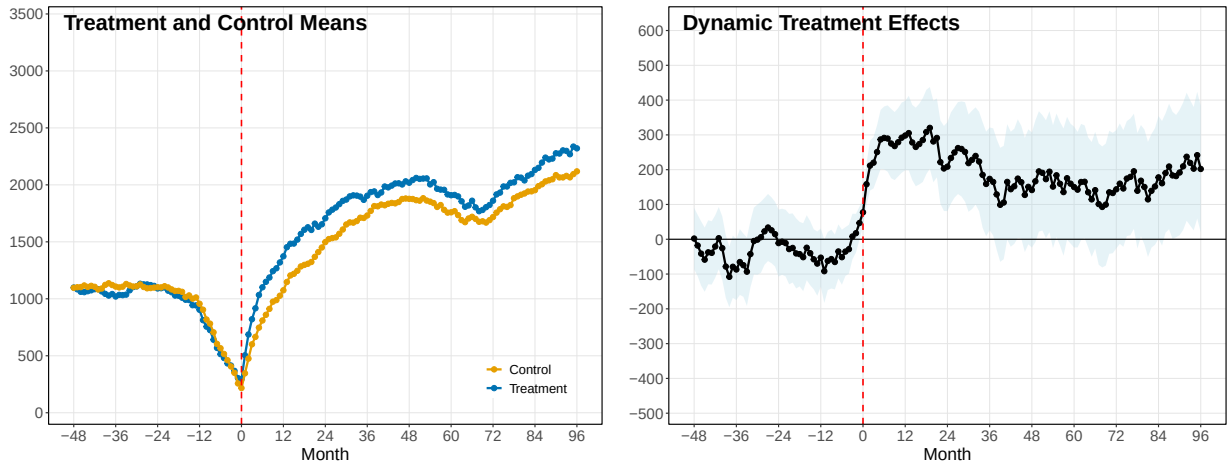

(b) Stock Sample



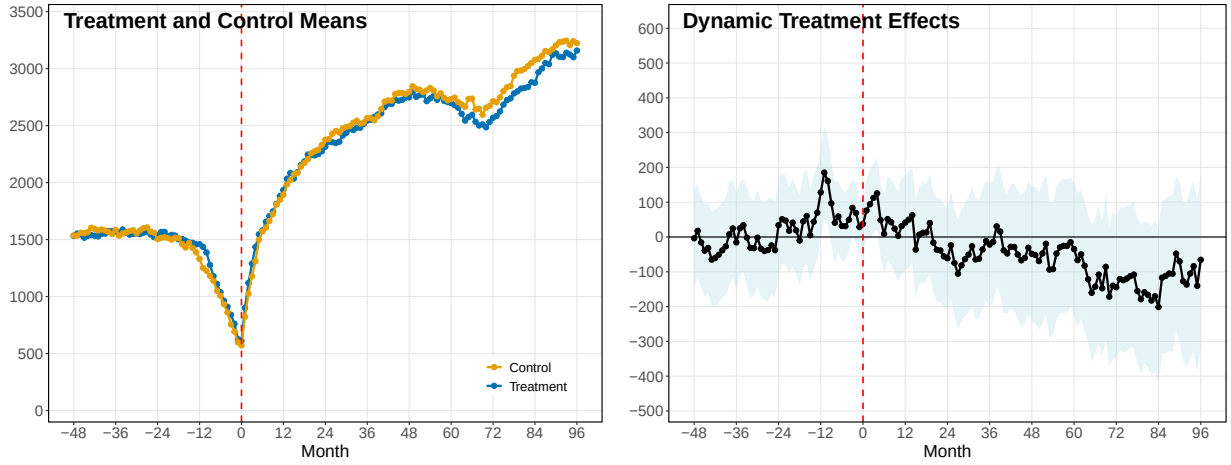

(c) Flow Sample



(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) |
| 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) | 51.169 | 216.205 | -129.081 | 249.554 | 358.855 | 156.341 |
| | (73.744) | (91.497) | (117.658) | (124.261) | (165.191) | (208.280) |
| | *2666.2* | *2118.3* | *3265.5* | *2177.4* | *1743.6* | *2693.8* |
| Total months employed (months 0 to 96) | 3.660 | 7.370 | -0.165 | 5.976 | 10.054 | 1.362 |
| | (0.612) | (0.819) | (0.687) | (1.144) | (1.464) | (1.435) |
| | *37.248* | *30.931* | *44.158* | *33.320* | *27.642* | *40.081* |
| Total earnings (months 0 to 96) | 6520.8 | 20235.1 | -9464.1 | 20237.4 | 30458.5 | 8330.6 |
| | (4477.1) | (5582.7) | (6310.0) | (7195.5) | (8996.0) | (12385.8) |
| | *201127.4* | *156620.5* | *249809.7* | *163084.9* | *129183.9* | *203451.2* |
| 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 (months 0 to 96) | -0.029 | -0.068 | 0.000 | -0.021 | -0.071 | 0.021 |
| | (0.016) | (0.024) | (0.022) | (0.028) | (0.037) | (0.040) |
| | *0.738* | *0.827* | *0.641* | *0.609* | *0.641* | *0.570* |
| 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 2016 NIS. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses.

Table 2: Program effects on participants and their treated spouses

| | Full sample | | | Stock Sample | | |
|---|---|---|---|---|---|---|
| Outcome | All (1) | Arabs (2) | Jews (3) | All (4) | Arabs (5) | Jews (6) |
| Total months employed (months 0 to 24) | 2.543 | 3.594 | 1.026 | 3.547 | 4.302 | 2.015 |
| | (0.345) | (0.407) | (0.636) | (0.574) | (0.713) | (0.963) |
| | *6.277* | *5.171* | *8.736* | *6.105* | *5.192* | *8.600* |
| Total earnings (months 0 to 24) | 5361.3 | 7733.2 | 2297.9 | 8481.2 | 10390.1 | 3023.9 |
| | (1617.3) | (1774.8) | (3605.7) | (2142.1) | (2239.8) | (5364.8) |
| | *23912.9* | *19754.1* | *33157.8* | *21248.2* | *18135.2* | *29756.9* |
| Total household earnings (months 0 to 24) | 11953.6 | 17005.8 | 6372.7 | 17322.9 | 20815.0 | 7670.9 |
| | (2997.7) | (3104.8) | (6794.6) | (3835.2) | (3831.2) | (10079.9) |
| | *47016.0* | *38184.9* | *66647.4* | *37637.1* | *30672.6* | *56673.3* |
| Total months employed (months 0 to 96) | 5.086 | 8.254 | 0.645 | 5.894 | 8.660 | 0.109 |
| | (1.302) | (1.501) | (2.447) | (2.162) | (2.498) | (3.950) |
| | *31.338* | *27.236* | *40.455* | *31.913* | *28.805* | *40.410* |
| Total earnings (months 0 to 96) | 16216.5 | 24660.5 | 14955.2 | 14776.7 | 23447.2 | -785.238 |
| | (8840.4) | (10322.8) | (17298.3) | (12182.1) | (13782.1) | (26931.9) |
| | *152656.2* | *129864.0* | *203322.5* | *154400.3* | *141337.5* | *190105.5* |
| Total household earnings (months 0 to 96) | 31576.9 | 48321.2 | 29246.8 | 36627.4 | 48243.5 | 18964.4 |
| | (16476.2) | (18402.6) | (31893.8) | (21814.8) | (24075.5) | (38932.4) |
| | *306737.0* | *262674.0* | *404687.8* | *284795.8* | *252110.2* | *374136.4* |
| 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 2016 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) |
| Total months employed (months 0 to 24) | 2.199 | 2.652 | 1.459 | 2.908 | 4.051 | 1.317 |
| | (0.260) | (0.314) | (0.456) | (0.551) | (0.628) | (0.890) |
| | *7.545* | *6.135* | *9.983* | *6.474* | *4.873* | *9.281* |
| Total earnings (months 0 to 24) | 4573.8 | 7119.5 | -62.510 | 3217.9 | 9003.4 | -5797.8 |
| | (1446.7) | (1618.4) | (2946.8) | (3240.0) | (3714.5) | (5217.1) |
| | *32705.3* | *24695.0* | *46548.2* | *27645.7* | *18845.9* | *43063.5* |
| Spouse total months employed (months 0 to 24) | 0.616 | 0.140 | 1.268 | 1.517 | 0.931 | 2.235 |
| | (0.345) | (0.405) | (0.605) | (0.724) | (0.931) | (1.136) |
| | *8.289* | *7.771* | *9.180* | *8.410* | *7.730* | *9.595* |
| Spouse total earnings (months 0 to 24) | 3045.0 | 2819.2 | 2705.9 | 8861.1 | 5142.9 | 15638.7 |
| | (1903.3) | (2270.5) | (3546.5) | (4401.6) | (5593.1) | (6392.6) |
| | *37662.6* | *33716.1* | *44466.7* | *34963.8* | *31725.9* | *40609.9* |
| Total household earnings (months 0 to 24) | 7636.2 | 9977.7 | 2645.0 | 12136.4 | 14226.7 | 9840.9 |
| | (2481.8) | (2982.2) | (4627.4) | (6302.7) | (8228.0) | (9530.7) |
| | *70312.2* | *58332.4* | *91014.9* | *62504.4* | *50422.1* | *83673.4* |
| Total months employed (months 0 to 96) | 4.525 | 5.839 | 1.627 | 7.400 | 10.531 | 2.543 |
| | (0.994) | (1.194) | (1.676) | (2.066) | (2.591) | (3.349) |
| | *35.550* | *30.162* | *44.860* | *32.153* | *25.830* | *43.231* |
| Total earnings (months 0 to 96) | 1180.6 | 15612.0 | -30797.3 | 15764.4 | 37246.6 | -21741.1 |
| | (7732.2) | (7732.2) | (15037.5) | (15347.1) | (17969.9) | (28683.9) |
| | *192257.4* | *147468.4* | *269658.4* | *164721.3* | *121072.0* | *241197.8* |
| Spouse total months employed (months 0 to 96) | 0.921 | -0.115 | 2.383 | 5.316 | 2.995 | 9.983 |
| | (1.191) | (1.447) | (2.133) | (2.583) | (3.389) | (3.633) |
| | *33.219* | *30.795* | *37.402* | *34.060* | *31.981* | *37.686* |
| Spouse total earnings (months 0 to 96) | 4044.3 | 7341.3 | -6877.1 | 38872.3 | 22773.7 | 74091.9 |
| | (8291.3) | (9962.0) | (16202.2) | (18701.9) | (23446.9) | (28420.7) |
| | *184704.2* | *162653.5* | *222751.5* | *173742.4* | *156302.2* | *204154.7* |
| Total household earnings (months 0 to 96) | 5084.1 | 22711.9 | -37667.9 | 54895.8 | 60218.2 | 52350.8 |
| | (12646.2) | (13810.6) | (24961.9) | (29228.1) | (37864.4) | (46422.7) |
| | *376779.3* | *309868.5* | *492409.9* | *337942.0* | *276636.9* | *445352.5* |
| 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 2016 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) |
| Total months employed (months 0 to 24) | 2.476 | 3.920 | 0.542 | 3.424 | 4.858 | 1.333 |
| | (0.305) | (0.347) | (0.445) | (0.526) | (0.618) | (0.705) |
| | *6.404* | *4.302* | *9.665* | *5.495* | *3.692* | *8.524* |
| Total earnings (months 0 to 24) | 4663.7 | 8461.9 | -625.424 | 8016.2 | 13146.2 | -0.793 |
| | (1456.5) | (1435.6) | (2749.5) | (2253.9) | (2337.3) | (4540.6) |
| | *25303.1* | *15913.2* | *39866.3* | *20615.2* | *12008.6* | *35076.7* |
| Total household earnings (months 0 to 24) | 9089.5 | 13404.4 | 1902.2 | 12635.3 | 18539.6 | 2443.0 |
| | (2240.6) | (2711.6) | (3860.0) | (4105.1) | (4941.3) | (7984.9) |
| | *46899.4* | *38011.7* | *60683.6* | *40785.3* | *30966.3* | *57283.9* |
| Total months employed (months 0 to 96) | 5.434 | 9.378 | 0.137 | 7.871 | 12.131 | 1.654 |
| | (1.128) | (1.454) | (1.581) | (1.958) | (2.452) | (2.595) |
| | *32.147* | *24.634* | *43.799* | *29.477* | *22.826* | *40.653* |
| Total earnings (months 0 to 96) | 8662.1 | 25771.6 | -15075.4 | 29114.0 | 43572.3 | 7035.2 |
| | (7564.3) | (8109.7) | (13669.7) | (12187.1) | (13879.3) | (22565.9) |
| | *157067.4* | *106582.2* | *235366.8* | *134177.1* | *93365.1* | *202752.5* |
| Total household earnings (months 0 to 96) | 19388.4 | 41759.0 | -13946.6 | 48975.9 | 64969.0 | 23554.2 |
| | (11738.1) | (13959.3) | (20594.0) | (20917.9) | (26139.0) | (37789.5) |
| | *277452.3* | *229833.8* | *351305.6* | *245178.3* | *203728.3* | *314825.6* |
| Married (month 96) | 0.004 | -0.010 | 0.034 | -0.004 | 0.006 | -0.010 |
| | (0.011) | (0.012) | (0.020) | (0.016) | (0.018) | (0.034) |
| | *0.656* | *0.793* | *0.444* | *0.668* | *0.781* | *0.476* |
| Number of children (months 0 to 96) | -0.021 | -0.024 | -0.021 | -0.035 | -0.050 | -0.034 |
| | (0.023) | (0.030) | (0.037) | (0.036) | (0.045) | (0.061) |
| | *0.353* | *0.355* | *0.349* | *0.279* | *0.263* | *0.306* |
| Any criminal charge (months 0 to 96) | -0.001 | -0.010 | 0.010 | -0.005 | -0.017 | 0.005 |
| | (0.007) | (0.008) | (0.013) | (0.012) | (0.014) | (0.025) |
| | *0.045* | *0.037* | *0.056* | *0.046* | *0.045* | *0.048* |
| Number of observations | 5,207 | 2,799 | 2,408 | 1,831 | 1,040 | 791 |

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 2016 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 | -0.022 | -0.026 | -0.014 | -0.034 | -0.053 | 0.007 |
| | (0.010) | (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 | -0.021 | 0.032 | 0.042 | -0.008 |
| | (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 | 0.545 | 0.831 | -0.258 | 1.765 | 2.477 | -0.156 |
| | (0.426) | (0.539) | (0.685) | (0.753) | (0.935) | (1.257) |
| | *14.336* | *14.573* | *13.874* | *13.397* | *13.664* | *12.854* |
| Juvenile criminal record | -0.009 | -0.005 | -0.011 | -0.013 | -0.019 | 0.003 |
| | (0.004) | (0.005) | (0.008) | (0.008) | (0.010) | (0.015) |
| | *0.032* | *0.025* | *0.046* | *0.038* | *0.038* | *0.038* |
| Number of observations | 10,054 | 5,854 | 4,200 | 3,455 | 2,086 | 1,369 |

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 | | |
|---|---|---|---|---|---|---|
| | All | Arabs | Jews | All | Arabs | Jews |
| Outcome | (1) | (2) | (3) | (4) | (5) | (6) |
| Post-secondary studies | 0.003 | 0.001 | 0.006 | 0.011 | 0.005 | 0.018 |
| | (0.012) | (0.015) | (0.018) | (0.023) | (0.027) | (0.037) |
| | *0.134* | *0.147* | *0.108* | *0.153* | *0.168* | *0.121* |
| Total months employed (ages 19 to 23) | 0.293 | 0.759 | -0.875 | -0.229 | -0.050 | -2.119 |
| | (0.685) | (0.828) | (1.162) | (1.200) | (1.366) | (2.197) |
| | *23.228* | *20.950* | *27.720* | *25.087* | *23.227* | *29.094* |
| Total earnings (ages 19 to 23) | -795.470 | 772.849 | -4486.9 | -258.655 | 1936.8 | -10660.5 |
| | (4352.1) | (5284.5) | (7345.6) | (7430.3) | (8513.8) | (14063.6) |
| | *126341.6* | *118074.9* | *142648.2* | *134157.3* | *127420.1* | *148671.5* |
| Total income (ages 19 to 23) | -351.059 | 1937.2 | -4610.1 | 12.257 | 3547.4 | -13012.3 |
| (including self-employment) | (4419.9) | (5361.0) | (7465.6) | (7404.0) | (8439.7) | (14307.7) |
| | *130183.7* | *121259.5* | *147787.3* | *137957.8* | *129988.6* | *155126.2* |
| Any criminal charge (ages 18-) | -0.003 | -0.006 | 0.004 | -0.005 | -0.025 | 0.034 |
| | (0.005) | (0.006) | (0.011) | (0.012) | (0.013) | (0.023) |
| | *0.026* | *0.018* | *0.041* | *0.032* | *0.031* | *0.034* |
| Number of observations | 5,845 | 3,440 | 2,405 | 2,137 | 1,313 | 824 |

Notes: This table shows program effects for participants' children who were between 14 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) | 9719.596 | 284.948 |
| | | (324.059) |
| F Stat for joint significance | | 1.268 |
| P value | | 0.218 |
| Number of observations | 11,032 | 17,906 |

Table A2: Balance Table - Children Sample

| | Mean Treated (1) | T-C (2) |
|---|---|---|
| Child female | 0.479 | −0.022 |
| | | (0.013) |
| Child age | 14.619 | −0.059 |
| | | (0.040) |
| Parent female | 0.653 | −0.031 |
| | | (0.017) |
| Parent age | 41.950 | −0.298 |
| | | (0.168) |
| Parent number of children | 4.916 | −0.045 |
| | | (0.087) |
| Single parent | 0.267 | 0.009 |
| | | (0.015) |
| Parent Arab | 0.534 | 0.003 |
| | | (0.010) |
| Parent Ultra-Orthodox | 0.216 | −0.003 |
| | | (0.009) |
| Parent immigrant | 0.136 | −0.012 |
| | | (0.010) |
| Parent less than 12 years of schooling | 0.490 | −0.002 |
| | | (0.018) |
| Parent 12 years of schooling | 0.380 | 0.008 |
| | | (0.016) |
| Parent more than 12 years of schooling | 0.073 | 0.007 |
| | | (0.008) |
| Parent missing schooling data | 0.056 | −0.013 |
| | | (0.010) |
| Parent self-reported health limitation | 0.424 | −0.004 |
| | | (0.018) |
| Parent total months employed (months -12 to -1) | 1.870 | 0.056 |
| | | (0.116) |
| Parent total earnings (months -12 to -1) | 6707.912 | 574.609 |
| | | (492.960) |
| F Stat for joint significance | | 1.226 |
| P value | | 0.243 |
| Number of households | 3,304 | 5,207 |
| Number of observations | 6,224 | 10,054 |

Table A3: Program effects on participants – Flow sample

| | Flow Sample | | |
|---|---|---|---|
| Outcome | All (1) | Arabs (2) | Jews (3) |
| Employed (month 96) | 0.007 | 0.028 | -0.017 |
| | (0.010) | (0.014) | (0.014) |
| | *0.436* | *0.388* | *0.487* |
| Earnings (month 96) | -22.129 | 159.678 | -225.095 |
| | (88.646) | (108.107) | (137.825) |
| | *2782.5* | *2212.0* | *3394.2* |
| Total months employed (months 0 to 96) | 2.814 | 6.309 | -0.710 |
| | (0.721) | (0.972) | (0.771) |
| | *38.183* | *31.753* | *45.076* |
| Total earnings (months 0 to 96) | 1415.1 | 16274.5 | -15644.2 |
| | (5412.1) | (6743.0) | (7119.2) |
| | *210177.3* | *163477.3* | *260240.3* |
| 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 | 17,906 | 8,364 | 9,542 |

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 2016 NIS. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses.

Table A4: Effects on children during adolescence by age at time of parental assignment

| | All | | Arabs | | Stock | | Arabs in Stock | |
|---|---|---|---|---|---|---|---|---|
| Outcome | Ages 11-15 (1) | Ages 16-18 (2) | Ages 11-15 (3) | Ages 16-18 (4) | Ages 11-15 (5) | Ages 16-18 (6) | Ages 11-15 (7) | Ages 16-18 (8) |
| Dropout | -0.032 | 0.011 | -0.037 | 0.021 | -0.048 | 0.005 | -0.065 | -0.008 |
| | (0.012) | (0.021) | (0.015) | (0.026) | (0.021) | (0.036) | (0.028) | (0.042) |
| | *0.192* | *0.201* | *0.206* | *0.212* | *0.195* | *0.194* | *0.218* | *0.195* |
| Matriculation eligibility | -0.002 | -0.004 | -0.001 | -0.008 | 0.018 | 0.064 | 0.028 | 0.064 |
| | (0.015) | (0.024) | (0.019) | (0.029) | (0.026) | (0.040) | (0.034) | (0.049) |
| | *0.367* | *0.305* | *0.368* | *0.314* | *0.347* | *0.279* | *0.349* | *0.302* |
| Matriculation units | 0.599 | 0.279 | 0.850 | 0.017 | 1.691 | 1.605 | 2.666 | 1.514 |
| | (0.510) | (0.611) | (0.644) | (0.733) | (0.914) | (1.040) | (1.125) | (1.250) |
| | *15.605* | *10.670* | *15.777* | *11.158* | *14.734* | *9.977* | *14.806* | *10.745* |
| Juvenile criminal record | -0.003 | -0.025 | -0.002 | -0.013 | -0.004 | -0.034 | -0.010 | -0.039 |
| | (0.005) | (0.011) | (0.005) | (0.013) | (0.008) | (0.022) | (0.008) | (0.028) |
| | *0.027* | *0.047* | *0.021* | *0.035* | *0.030* | *0.059* | *0.029* | *0.060* |
| Number of observations | 7,343 | 2,711 | 4,263 | 1,591 | 2,440 | 1,015 | 1,477 | 609 |

Notes: This table shows program effects on outcomes for participants' children, estimated separately by age at the time of parental program assignment (ages 11–15 vs. ages 16–18). 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 A5: Effects on children during early adulthood by age at time of parental assignment

| | All | | Arabs | | Stock | | Arabs in Stock | |
|---|---|---|---|---|---|---|---|---|
| Outcome | Ages 14-15 (1) | Ages 16-18 (2) | Ages 14-15 (3) | Ages 16-18 (4) | Ages 14-15 (5) | Ages 16-18 (6) | Ages 14-15 (7) | Ages 16-18 (8) |
| Post secondary studies (ages 19 to 23) | -0.002 | 0.011 | -0.015 | 0.018 | 0.000 | 0.026 | -0.011 | 0.030 |
| | (0.014) | (0.018) | (0.019) | (0.022) | (0.028) | (0.034) | (0.034) | (0.040) |
| | *0.117* | *0.154* | *0.138* | *0.158* | *0.133* | *0.176* | *0.169* | *0.168* |
| Total months employed (ages 19 to 23) | 0.172 | 0.203 | 0.473 | 0.883 | -0.585 | 0.718 | -0.017 | 0.008 |
| | (0.791) | (1.227) | (0.961) | (1.446) | (1.373) | (2.120) | (1.562) | (2.362) |
| | *20.557* | *26.432* | *18.743* | *23.552* | *22.597* | *27.869* | *20.930* | *25.879* |
| Total earnings (ages 19 to 23) | -3707.0 | 1567.9 | -3526.5 | 4465.0 | -5519.1 | 9777.0 | -4138.3 | 10514.4 |
| | (5037.3) | (7743.7) | (6101.3) | (9295.9) | (9040.0) | (12469.1) | (10561.2) | (13712.6) |
| | *111232.2* | *144472.9* | *104766.3* | *133763.0* | *124495.7* | *144950.4* | *118772.1* | *137403.1* |
| Total income (ages 19 to 23) | -3163.2 | 1641.8 | -1216.1 | 4377.8 | -7411.8 | 10977.6 | -4323.2 | 13382.3 |
| (including self-employment) | (5141.0) | (7849.9) | (6262.8) | (9386.0) | (9053.8) | (12515.9) | (10563.6) | (13499.3) |
| | *114208.1* | *149354.4* | *107406.1* | *137589.6* | *128807.5* | *148179.7* | *122474.6* | *138662.5* |
| Any criminal charge (ages 18-) | -0.003 | -0.006 | 0.004 | -0.016 | -0.003 | -0.019 | -0.004 | -0.055 |
| | (0.006) | (0.009) | (0.007) | (0.010) | (0.013) | (0.018) | (0.012) | (0.022) |
| | *0.019* | *0.035* | *0.012* | *0.026* | *0.024* | *0.041* | *0.012* | *0.054* |
| Number of observations | 3,134 | 2,711 | 1,848 | 1,591 | 1,122 | 1,015 | 704 | 609 |

Notes: This table shows program effects on outcomes for participants' children, estimated separately by age at the time of parental program assignment (ages 14–15 vs. ages 16–18). 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.