Fostering Soft Skills in Active Labor Market Programs:

Evidence from a Large-Scale RCT *

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Abstract

Using a large-scale randomized controlled trial, we evaluate the effectiveness of an Active Labor Market Program that focuses on enhancing soft skills of welfare recipients. The program increased participants' employment rates and decreased income-support recipiency. The impacts persist five to six years after the program's implementation, even during the Covid-19 crisis. The analysis of the mechanisms shows positive effects on the soft skills of the participants, mainly among those with no recent employment history, who gradually joined the labor market after participation in the program. In contrast, individuals with a recent employment spell joined the labor market soon after their assignment to the program.

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Supplementary materials are available at the Online Appendix.
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This research uses confidential data from the Israeli Employment Service (IES) and the National Insurance Institute of Israel (NII). The empirical analysis was conducted at the research room of the NII. The data can be obtained by applying to the IES (https://www.taasuka.gov.il/he) and to the NII (https://www.btl.gov.il/Mediniyut/HadarMehkar/Pages/default.aspx). The authors are willing to assist (Yannay Shanan: yannay.shanan@biu.ac.il). We thank the staff from the research department at the Israeli Employment Service for providing the administrative data on the experiment participants and the NII research unit for providing the administrative data on labor market and social assistance.
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I. Introduction

Active Labor Market Programs (ALMPs) include a set of policies that aim to enhance the employability and earning capacity of individuals who are unemployed or on welfare. One of the most prevalent types of ALMPs is training programs (in traditional classrooms or on the job) that provide unemployed individuals with general skills or specific occupational skills in order to enhance their productivity and employability. Many such individuals, however, lack basic soft skills such as motivation, career aspirations, and interpersonal skills that are needed to transition from welfare to work and persevere in employment—skills that strongly predict labor market success (see, e.g., Heckman, Stixrud, and Urzua 2006). Scientific evidence of the possibility of improving these skills, especially among the adult population, is limited, and little is known about the impact of such an improvement on labor market outcomes and welfare dependency.

In this paper, we examine whether fostering soft skills of welfare recipients can enhance their likelihood of employment and subsequent earnings. To do this, we use a large-scale randomized control trial (RCT) to evaluate the effectiveness of an ALMP implemented in Israel. The program is designed to integrate unemployed income-support claimants aged 20–50 into the labor force, preventing welfare dependency and long-term and chronic unemployment. Its main goal is to foster participants' work-related soft skills such as motivation, work self-efficacy, self-esteem, and interpersonal skills. Individuals who submitted new income-support claims (the "flow subsample") and claimants who were already in the welfare system (the "stock subsample") were randomized into treatment and control groups in each of the employment offices participating in the experiment. Those assigned to the treatment group received individual coaching and participated in therapeutic group workshops for two to seven months, receiving also job search assistance. Those assigned to the control group continued to report to their local employment office once a week. Overall, 48,000 individuals were assigned to the program from its inception in March 2014 to December 2018. Our paper focuses on the population assigned to the treatment and control groups during the first year of the program implementation as an RCT: 6,151 individuals.

We combine administrative datasets from the Israeli Employment Service and the National Insurance Institute of Israel on employment, earnings, welfare, and disability benefits together with survey data to build a comprehensive picture of the individuals before, during, and after their randomization into treatment and control groups. Our main results show that twelve months after randomization, the program raised participants' employment rates by 8 percentage points relative to the control group (a 24% increase), lessened their welfare dependency by 11 percentage points (a 26% decline), and lowered the share of treated participants reporting to the employment office by 15 percentage points (a 38% reduction). These effects persisted even eighteen months after assignment to the program. The impact of the program was larger among the existing stock of income-support claimants (who had a longer history of income-support recipiency), high-school dropouts, and those with lower labor-force attachment, or self-reported health limitations. We also find that the program had spillover effects within the household, leading to an increase in labor income of untreated partners. There is no evidence of externalities among the control group.

We find that the program worked through two different channels affecting different individuals: it generated a threat effect for some participants, inducing them to stop reporting to the employment office soon after their assignment to the treatment group due to the additional burden of the program's requirements. These individuals were primarily those who registered at the employment office just before assignment to the program (i.e., the flow subsample). Other participants, mainly those who reported to the employment office for a longer period (i.e., the stock subsample), benefited from the tools imparted by the program, experiencing a significant increase in various dimensions of soft skills (work self-efficacy, job search self-efficacy, self-esteem, general self-efficacy, and grit) and in their employment rates. Our results show that the savings on welfare transfers offset the per-participant costs within twelve months, making this program cost-effective from the point of view of government spending and implying a marginal value of public funds (MVPF) of infinity.[1] Treatment effects persist also in the long run: five to six years after assignment to the program and just before the onset of the Covid-19 pandemic in February 2020, treated individuals were 37% less likely to report to the employment office. Moreover, the gaps between the treatment and control groups persist even during the Covid-19 crisis. These long-term impacts suggest that the program not only succeeded in getting participants to reenter the labor force but also generated persistent gains in their employability.

Our study is related to a large literature that evaluates the effects of ALMPs. While most of the earlier studies were based on non-experimental data, the share of studies based on RCTs has been increasing (see recent reviews by Kluve 2010; Card, Kluve, and Weber 2018; and earlier work by Greenberg, Michalopoulos, and Robins 2003; Greenberg, Cebulla, and Bouchet 2005).[2] These programs typically fall into two primary categories: those aimed at enhancing the productivity of unemployed individuals and those aimed at reducing frictions in the matching process. According to a recent meta-analysis by Card, Kluve, and Weber (2018), training and private sector employment programs ("human capital" programs) tend to have small effects in the short run, and larger effects in the medium and long run. Job search assistance (JSA) programs, which are designed to push participants into the labor market quickly ("work first" programs), have smaller but similar effects in the short and long run.

Overall, while some programs are found to be beneficial, less is known about why they work and under what circumstances. Some studies consider the possibility that participants in mandatory programs may immediately forgo their claims and exit welfare or unemployment to avoid the additional "costs" associated with the program (Black et al. 2003; Dolton and O'Neill 2002). This mechanism may explain some of the short-term effects of "work first" programs. However, the conventional "work first" approach of standard JSA programs, which mainly focus on resume and interview preparation with little or no investment in soft skills beyond job search skills, was found to be less effective among long-term unemployed (Card, Kluve, and Weber 2018). This finding is supported by Carranza and McKenzie's (2024) review of ALMPs in developing countries, which concludes that labor intervention services that equip the unemployed with tools to improve their job search and connect them with jobs have only limited short-term impacts. These programs might help workers find one job, but they are of little help in securing subsequent employment. Our study diverges from this conventional approach by focusing on a program that extends beyond the traditional "work first" philosophy of most JSA programs. This program not only offers JSA but also provides counseling and workshops aimed at enhancing soft skills such as motivation, self-esteem, and work self-efficacy—skills often lacking among the long-term unemployed.

As highlighted by Crépon and van den Berg (2016), many long-term unemployed individuals are disconnected from the labor market for extended periods, lacking basic traits needed to reintegrate. They further argue that traditional ALMPs may not be effectively structured for this purpose. Instead, they recommend programs aimed at enhancing participants' self-esteem and other personality traits through mentoring, therapy, and group treatments, where individuals facing similar challenges can motivate each other.

Soft or non-cognitive skills, much like cognitive skills, can affect preferences, skill-formation technology, and productivity. Soft skills such as motivation, self-efficacy, and perseverance are found to be positively associated with test scores and labor market outcomes (see Brunello and

Schlotter 2011 and Kautz et al. 2014 for a review of the literature). Moreover, studies indicate that the explanatory power of soft skills for many later-life outcomes rivals that of cognitive ability (see, e.g., Heckman, Stixrud, and Urzua 2006; Humphries and Kosse 2017; Lindqvist and Vestman 2011). Additionally, soft skills appear to be particularly important for workers in low-skilled occupations (Aghion et al. 2019).

While personality traits and soft skills are relatively stable across situations, they are not necessarily permanent and some interventions can enhance them in lasting ways (Heckman and Kautz 2012). Early-childhood programs such as Head Start and the Perry Preschool program were found to enhance soft skills, consequently fostering higher social and economic success (Carneiro and Heckman 2003; Kautz et al. 2014). There is scarce evidence, however, on the returns to investments in soft skills later in life. Some indications suggest that investment in soft skills such as self-control and self-image may mitigate crime and violence (Blattman, Jamison, and Sheridan 2017; Heller et al. 2017). Nonetheless, empirical evidence on the causal effects of labor market interventions that include soft skills training remains largely limited. Recent exceptions are the studies of Acevedo et al. (2020), Groh et al. (2016), and Adhvaryu, Kala, and Nyshadham (2023), which find mixed results on labor market outcomes, highlighting the need for further research.[3] Furthermore, existing empirical evidence primarily focuses on interventions involving young individuals entering the labor force or those already employed. Consequently, the effectiveness of soft skills training for individuals who have been unemployed or on welfare for a long period of time remains uncertain.

This study provides several contributions to the existing literature. First, it expands upon the body of research that evaluates the effect of ALMPs on labor market performance by demonstrating that soft skills training can serve as a cost-effective tool to increase employment of long-term unemployed individuals and lower their welfare dependence, with persistent effects in the medium and long term.

Second, our study adds to the growing literature on the development and importance of soft skills for life outcomes by providing unique evidence that some of these skills are malleable later in life and play an important role in enhancing employability of low-skilled individuals.

Third, we show that ALMP programs can yield positive spillover effects within households, particularly in terms of enhancing employment and earnings of participants' partners, demonstrating that the benefits of these types of programs may be larger than previously thought.[4]

Finally, our paper constitutes one of the first studies that show evidence of a successful intervention that aids low-skilled individuals in coping with adverse labor market shocks, such as those precipitated by the Covid-19 crisis.

The remainder of the paper is organized as follows. Section II provides background on welfare support in Israel and describes the program and the experimental design. Section III presents the identification strategy. Section IV describes the data, defines the samples used throughout the study, and examines the effectiveness of the randomization. Section V reports the main estimates of the program's effect on a range of outcomes from administrative datasets and shows the dynamic impacts of the program. Section VI explores the mechanisms that underlie the impact of the program, focusing on the program's effect on soft skills. Section VII provides evidence on the long-term effects of the program just before and during the Covid-19 crisis. Section VIII concludes.

II. Background

A. Institutional Context and Description of the Program

The National Insurance Institute of Israel (NII) provides monthly income-support benefits to residents who cannot ensure themselves a basic minimum income for subsistence. In 2014, approximately 100,000 households, almost 5% of households countrywide, received such benefits. Eligibility for income support is based on age, income, and assets. Claimants who are considered capable of working (healthy, aged below sixty, and, among single parents, having children older than two years of age) must report weekly (or monthly for those aged fifty and younger) to one of seventy-five local employment offices run by the Israeli Employment Service (IES).[5] Treatment at the employment office is minimal: individuals are required to report to their local employment office every week and record their visit using self-service biometric fingerprint scanners. Once every three weeks (or when relevant) they meet with a caseworker who provides them with job referrals. Failure to report to the employment office or rejection of a relevant job offer results in denial of income-support payments. Working individuals who earn below a minimum amount set by law also receive income support; this is known as an *income supplement*. Income-supplement recipients are not required to report to their local employment office every week. Instead, the IES gives them time-limited exemptions, using discretion as to the duration of the exemptions and choosing whether to pursue a more demanding approach, for example, by requiring an increase in hours worked. Income-support recipients also receive reduced-cost services from other government entities such as subsidized daycare, rent assistance, and a lower rate of property tax, in addition to the monthly income-support transfer. The maximal monthly transfer received by the head of household—a function of age, marital status, and number of dependent kin—ranged in 2014 between \$500 and \$1200 a month—40% and 100% of the minimum wage, respectively.

In February 2014, the IES launched an ALMP called "Employment Circles" in fourteen of its employment offices with the aim of integrating unemployed income-support claimants into the labor force and preventing welfare dependency and long-term and chronic unemployment. The target population were income-support claimants aged 20–50 who report to the employment office and are unemployed. The program focuses on enhancing participants' soft skills by providing personalized treatment composed of weekly sessions with occupational trainers, therapeutic group meetings with coaches, and job search assistance workshops. The program begins with two one-on-one meetings with an occupational trainer who diagnoses the participant with respect to employability, motivational level, and barriers to employment, and recommends a specific track of group workshops and personal meetings on this basis. Together with the occupational trainer, participants define their career goals and build a program to attain them. A key component of the program is the group workshops, in which coaches focus on identifying participants' strengths; enhancing their motivation, job search self-efficacy, work self-efficacy, and self-image; and developing a proactive work attitude.[6] The workshops and the meetings with occupational trainers also focus on imparting soft skills conducive to securing stable employment, for example, by simulating workday situations and instilling basic concepts of work life (e.g., accepting criticism, dealing with time and work pressure, conflict resolution, etc.) along with training on job search skills. Appendix A provides more details on the program's content.

Unlike regular income-support claimants, who must report to the employment office once per week, program participants need to visit three times per week: twice for workshops and meetings with occupational trainers (3–5 hours) and once for a regular meeting with their caseworker. The program is mandatory, non-compliance leading to loss of income support. The program lasts two to seven months depending on the participant's specific needs. Participants can leave the program at any time if they find a job. In this case, they may continue to receive income-support benefits in the form of an income supplement depending on the level of their labor income. After seven months, unemployed participants who still report to the employment office return to the regular track of weekly visits.

The program may increase its participants' employment and reduce their welfare dependence through two different channels. First, the workshops and individual sessions are expected to enhance their motivation, self-esteem, job search self-efficacy, and work self-efficacy, and additional traits that may affect job search, employment, and job persistency. A second channel is created by the additional requirement of the program to report to the employment office three times a week instead of once and the additional time that participants must spend there. This extra weekly visits requirement raises the non-monetary costs of claiming welfare benefits and potentially increases employment.[7] While the program is not designed to test the contribution of each channel separately, we present below several pieces of evidence that suggest that both channels are operational, affecting different groups of individuals.

B. Experimental Design

The program was implemented gradually using an experimental research design executed in two waves. The first wave started in February 2014 in seven employment offices; a second wave including seven additional offices followed in August 2014. These fourteen offices constituted the experimental sample for the RCT. The program was then gradually expanded to include almost all employment offices countrywide, and the age limit was raised to fifty-five. Table 1 reports some basic characteristics of the employment offices included in the RCT and all other employment offices. The experiment offices served roughly 45% of unemployed Israeli income-support claimants in 2014. The average jobseeker is thirty-eight years old, has no more than ten years of schooling, and is most likely a woman. Most claimants are Arab, this population being substantially overrepresented in the Israeli welfare system.[8] Overall, the characteristics of the offices included in the experimental phase are highly similar to the remaining offices, both in terms of the population demographics and local labor market conditions (summarized in this table by local unemployment rates and the locality socioeconomic index). This similarity supports the relevance of our findings for the program scale-up.

During the experimental phase of the program, individuals who submitted new income-support claims (the “flow subsample”) and a fraction of existing claimants in the welfare system (the “stock subsample”) were randomized into control and treatment groups. Randomization took place on a weekly basis separately for the flow subsample and the stock subsample at each employment office. Unlike individuals from the flow subsample, individuals from the stock subsample were already reporting to the employment office for nine months, on average, before being assigned to the program. As such, they had lower attachment to the labor market when they entered the program, with roughly 90% of them receiving income-support benefits at some point during the previous year.[9]

The number of individuals assigned to treatment and control groups varied over time due to changes in the incoming flow of claimants and the capacity of the program at the office level. Randomization was achieved by a software protocol that was implemented on the premises of the IES research department office to avoid manipulations. Treatment status was updated in the central IES operational database and the local employment offices received the list of individuals assigned to the treatment group on a weekly basis. Treatment status was assigned at the household level. Namely, in cases where both partners reported to the employment office, both were assigned to one group: treatment or control.[10] In practice, as we will discuss later, in most cases only one household member was assigned to the program because the other partner was not registered with the employment office. This allows us to examine the effect 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 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.

III. Empirical Framework

Through the mechanism of randomization, we can infer the effect of the program by estimating the difference in post-program outcomes between the treatment and the control group after controlling for the randomization unit, thus averting the problem of selection bias.[11] Accordingly, we estimate the average treatment effect of the program by regressing various outcomes on a treatment dummy while controlling for the randomization unit: employment office-randomization date-claimant type (flow/stock).[12] A small fraction of the treatment group (around 1%) did not attend the program for various reasons ranging from administrative errors to total exemption on grounds of serious physical- or mental-health issues.[13] We include them in the treatment group to avoid selection. Therefore, we estimate the intention-to-treat effect. Given the negligible share of treatment group members who were exempted from the program, we do not use an instrumental-variable strategy to estimate the treatment effect on the treated since we expect to obtain almost identical estimates. 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' socio-demographic characteristics, employment, and welfare history measured before randomization. The estimating equation can be written as follows:

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

where $Outcome_{ijtp}$ is the outcome of individual i assigned to employment office j, randomized at time t, who belongs to claimant type group p (i.e., flow/stock); $Treatment_i$ is the indicator for whether individual i was assigned to treatment; γ_{jtp} is a fixed effect for the randomization unit; X_i is a vector of individual characteristics measured before randomization including age, sex, marital status, number of children, immigration status, education level, indicators for self-reported health limitation, single mother, ultra-Orthodox Jew, Arab, and vectors for welfare and employment-history indicators in the three years 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.[14]

IV. Data Sources

We combine detailed data from various sources to produce a comprehensive picture of each individual before, during, and after the program was implemented. The first administrative data source is the Israeli Employment Service operational database (hereinafter: IES data), which contains basic socio-demographic characteristics of all jobseekers registered with the IES, dates of assignment to treatment and control groups, and information on their weekly visits to the employment office.[15] The database also includes the ID number of the jobseeker's partner as recorded in the Israeli population registry.

The second administrative data source comprises the operational records of the National Insurance Institute of Israel (hereinafter: NII data), which records monthly income-support payments and additional transfer benefits (disability, unemployment, etc.). We combine these data with tax records to determine monthly employment and earnings.[16] The data covers the 2010–2015 period, providing a very comprehensive picture of welfare and labor market outcomes before, during, and after the intervention for RCT participants and their partners.

We complemented these data with survey data that provide important insights into the impact of the intervention and the mediating channels. The surveys were administered by the IES through a third-party agency in Hebrew and Arabic (for the Jewish and Arab populations, respectively). The first survey took place 12–16 months after the program was launched; the second survey followed the first at a twelve-month interval. The surveys include a series of questions that aim to measure soft skills and labor market outcomes such as labor force participation, hours worked, and part-time work that administrative data do not elicit. We provide further details on the survey data in Section VI, where we discuss the mechanisms and additional outcomes.

A. Sample Construction

At the time the IES data was transferred to the NII, earnings records were available only until December 2015, and therefore the sample is restricted to individuals assigned to the treatment or control groups during 2014 (the first year of the RCT implementation) in order to be able to follow their labor market outcomes for at least twelve months. The analysis sample includes 6,750 individuals. We dropped 599 individuals from the control and treatment groups collectively (about 9% of the sample) who stopped reporting to the employment office before the randomization lists were transferred to the local employment offices.[17] In Appendix Table A1, we show that there is no differential selection of these individuals according to treatment status. This stands to reason because these individuals stopped reporting to the IES before knowing their treatment status.

Our final analysis sample includes 6,151 individuals: 3,201 in the control group and 2,950 in the treatment group. Table 2 (Column (1)) reports the basic demographic characteristics, employment, and welfare history (all included as controls in the analysis of the program effect) of the treatment group as recorded before they were randomized into the program. The table reports balancing tests for each of the individual variables based on regressing each outcome on a treatment dummy and indicators for the randomization unit. 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 unit.

The program participants come from different demographic strands of the Israeli population: 35% Arabs, 19% ultra-Orthodox Jews, and 21% immigrants. The representation of relatively disadvantaged subgroups is apparent: only 5% have more than twelve years of schooling, 56% have twelve years of schooling, and 39% have fewer than twelve years of schooling. 36% report having some health limitation that prevents them from working, 22% are single parents, 52% received income support during the year before randomization, and 24% received income support in the third year before randomization. Overall, all covariates together cannot jointly predict treatment status as reported by the p-value (= 0.45) that tests for joint significance. The individual balancing tests for each of the covariates show that only four out of the twenty-five covariates examined are not balanced between the treatment group and the control group. Differences are small in economic terms and are not consistent across variables in terms of employability prospects of the treatment group or the control group. Particularly important is that the welfare and employment history of the groups during the three years preceding randomization is balanced.

V. Results

A. Program Effects Twelve and Eighteen Months after Randomization

Table 3 (Column (1)) reports the effects of the program on the employment, earnings, and welfare outcomes of our main analysis sample as observed twelve months after the randomization date and for outcomes accumulated during the twelve months after randomization. Each cell reports the treatment effect for a specific outcome (along with its standard error) and the respective outcome mean for the control group (in italics). Columns (2) and (3) of the table report similar outcomes for a subset of our main analysis sample that we can track for eighteen months given that they were randomized in the first half of 2014.

The results show that the program lowered the probability of reporting to the employment office twelve months after randomization by 15 percentage points (s.e. = 0.019)—a significant drop of 38% relative to the outcome mean of the control group (0.384). The program also produced an 8 percentage-point increase (s.e. = 0.014) in employment, a 24% upturn in employment relative to the control mean (0.331). Concurrently, the program reduced the likelihood of receiving income support by 11 percentage points (s.e. = 0.017), a 26% decline. The program had no effect on the probability of receiving other NII transfers, such as disability or unemployment compensation. This is important in two different respects. First, it implies that individuals in the treatment group did not transition to other transfer benefits that might be easier to claim (by not requiring three weekly visits to the employment office, for example). Second, from a fiscal perspective, it means that the savings from the reduction in income-support payments are not offset by other government transfers. Consistent with the increase in employment, we see a significant 12% increase in monthly labor income relative to the control group (161 New Israeli Shekels (NIS) in 2016 prices, s.e. = 65.48).

Program participants received NIS 2,026 more in annual earnings during the first year after being assigned to the program in comparison to the control group—a 17% upturn relative to the mean of the control group. Concurrently, they received, on average, NIS 1,860 less in income support (a reduction of 21%). The per-participant cost of the program was NIS 1,400, meaning that the program paid for itself twelve months after an individual was assigned to treatment. Appendix Table A2 reports our main results from Column (1) using two alternative adjustments to account for multiple hypothesis testing. Column (3) reports sharpened False Discovery Rate (FDR) q-values (i.e., the expected proportion of rejections that are type-I errors) according to the procedure of Anderson (2008). Column (4) reports adjusted p-values according to Romano and Wolf's (2005) multiple hypothesis correction for family-wise error rate (FWER). For reference, we report in Column (1) the estimated effects and standard errors and in Column (2) the unadjusted p-values. The table shows that our results remain highly robust after adjustment for multiple hypothesis testing.

The effects of the program observed twelve months after randomization persisted after eighteen months as well, as seen in Columns (2) and (3) of Table 3, which report estimates for a subsample of our main analysis sample over a time horizon of at least eighteen months after randomization. The increase in employment at twelve months in this subsample is of the same order of magnitude as the increase in our main analysis sample and remains similar after eighteen months. This suggests that the increase in employment generated by the program persists in the medium term. Concurrently, the positive gap in cumulative earnings between the treatment and control groups and the negative gap in cumulative income-support payments continued to widen. Thus, the program continues to generate fiscal savings.

The estimated medium-term impact on employment (8.2 ppts) is slightly larger than the average impact of training programs (mean effect = 6.6 ppts) and notably exceeds the impact of job search assistance programs (mean effect = 2.0 ppts) reported in Card, Kluve, and Weber's (2018) meta-analysis. This comprehensive review synthesizes results from both experimental and non-experimental studies on ALMPs, targeting various demographic groups, including unemployment insurance recipients and long-term unemployed individuals. Our findings align with Card, Kluve, and Weber (2018) who indicate that ALMPs, particularly those emphasizing human capital development, tend to have larger effects on the long-term unemployed.

Average treatment effects show no change in total income (from work and benefits) in the time horizon analyzed in this study even though the program increases employment and labor income. One possible explanation is that individuals who begin to work lose their eligibility for income support and experience a decline in transfers that fully offsets their gain in income from work. However, we show in Appendix Table A3 that this is not the case. In this table, we compare the change in income (from work and from income-support transfers) between twelve months before assignment to the program and twelve months after assignment for individuals stratified by their employment and income-support status at month 12.[18] We do not intend to claim causality (since we are stratifying by post-treatment outcomes) but to provide a descriptive picture of the income of treated individuals twelve months after randomization.

Column (1) of Appendix Table A3 reports the change in income of individuals who are formally employed twelve months assignment to the program. These individuals experience an increase in income from all sources between the pre- and post-program periods. They earn, on average, NIS 2,000 more than what they earned twelve months before assignment to the program and experienced no significant change in income-support transfers, leaving their total income

NIS 2,068 higher on average.[19] By contrast, the total income of those who neither work nor receive income support twelve months after assignment to the program falls by NIS 1,216. The last group reported in the table is those who receive income support twelve months after program assignment and do not work: they experience a slight increase in total income (NIS 290) because they gain more in income support than they lose in labor income.

These descriptive statistics suggest that the zero effect of the program on total income masks differential effects among individuals. In Figure 1, we examine this by estimating unconditional quantile treatment effects on total income (from work and from income support).[20] The program does not affect total income at the bottom of the income distribution. It has a negative effect on total income at the 40–50 percentiles of the income distribution and a positive effect at the 65–75 percentiles. In Figure 2, we plot quantile treatment effects on earnings. There are no differences at the lowest percentiles given that 59% of the treatment group do not work. Yet, we see positive treatment effects at the 65–80 percentiles of the earnings distribution.

B. Household Spillovers

An interesting feature of the program and our data is that we can also examine the program's effect at the household level. Recall that in cases where both partners were eligible for the program, they were jointly assigned to either the treatment or the control group. Table 4 reports program effects at the individual level, stratifying the sample by the program participation of each partner: both partners treated (Column (2)), only one partner treated (Column (3)), and single-headed households (Column (4)).[21] For comparison, we also report in Column (1) the program effect for the full sample as reported in Column (1) of Table 3.

Overall, we find that the program boosts total household labor income accumulated over twelve months in both households where only one partner was treated and those where both partners were treated. The increase is 6,827 NIS (43%) for households where both partners were treated and 4,574 NIS (14%) for households where only one partner was treated. More interestingly, the increase in total accumulated household labor income exceeds the increase in individual labor income, implying that the program raises the labor income of both partners. This might be expected in households where both partners participate in the program (Column (2)), but it is an important finding for households where only one partner is assigned to the program (Column (3)), as it suggests that the program has positive employment spillovers within the household. This result lends itself to various possible explanations, such as changes in social norms within the household, information sharing, and employment-related social networks. Although these factors cannot be assessed within the context of this study, they offer interesting directions for the design of future interventions. Our findings at the household level highlight an understudied and important aspect that should be considered when evaluating the effectiveness of ALMPs: the spillover effects on other household members.

C. Additional Results: Externalities and Heterogeneity

In the Online Appendix, we discuss additional results. Appendix B1 reports estimates of the program using individual fixed effects, comparing outcomes from the 12 months preceding randomization with the same outcomes from the twelve months following randomization. Estimates are highly consistent with our main results. Appendix B2 examines whether the program had any externalities on the control group, showing no evidence for the existence of such effects. Appendix B3 discusses heterogeneous effects for different subgroups, showing that the program had a more significant impact on individuals with lower employment prospects, no recent employment spells, or a longer history of welfare dependence. The findings from the different stratifications are consistent with results reported in Table 5, where we examine the heterogeneous effects of the program on employment by stratifying the sample by individuals' predicted probability of employment.[22] The results show that the program had the largest impact on employment among individuals with the lowest chances of being employed.

D. Dynamic Effects of the Program

We examine the impact of the program over time by estimating its effects on a monthly basis. Figure 3 displays the dynamic effects on employment. The left panel shows employment rates for treatment and control groups, while the right panel presents treatment versus control differences with confidence bands, spanning from three years before random assignment to the program to twelve months after randomization.[23] The figures show that the treatment and control groups had identical employment trajectories before randomization. Their employment rate was about 32% thirty-six months before randomization. As is typical for populations enrolled in ALMPs, the employment rates of both groups show a decline (the *Ashenfelter dip*) that starts around eighteen months before randomization and accelerates during the year preceding randomization. This is expected because eligibility for the program hinges on unemployment status.[24] The employment rates of the treatment and control groups increase over time but the gap between both groups widens month by month. Twelve months after randomization, the control group converges to the employment rate observed three years before randomization (around 33%) while the treatment group surpasses its pre-program employment rate at a record 41%.

The dynamic effects of the treatment also provide valuable insights into how the program works. In particular, they reveal whether the program's impact stems primarily from the increased reporting requirement (participants must report to the IES three times weekly instead of once) or from its workshop components. If the stricter reporting requirement were the main driver pushing participants toward employment, we would expect to see high early exit rates to work before participants receive most reemployment services, followed by minimal exit rates in subsequent months. However, the figure on employment effects shows a different pattern: while there is an initial response in the first two months after program assignment, the gap between treatment and control groups continues to widen substantially after month 2. The treatment effect eventually stabilizes around eight months after treatment, consistent with the program's seven-month maximum duration. This pattern of dynamic effects on employment - with both immediate impacts after enrollment and additional effects after active program participation - suggests that both program components play meaningful roles in driving the program's effect.[25]

Figure 4 adds more evidence about the dynamic effects of the intervention by focusing on the probability of visiting the employment office. By design, all income-support claimants visited the employment office by the randomization date. During the first two months after those in the treatment group were assigned to the program, their attendance rate declined by 8 percentage points relative to the control group. Some members of the treatment group transitioned to employment (about 6 percentage points more than the control group) but others (as shown in the next figure) stopped reporting to the employment office despite lacking formal employment. The share of individuals reporting to the employment office continued to decline over time and the gap between the treatment and control group widened until it stabilized at 15 percentage points around eight months after assignment to the program. Roughly, about half of the decline in attendance at the employment office can be attributed to early exits that were probably induced by the extra weekly visits requirement while the remaining decline takes place gradually once participants start participating in the workshops.

To complete the picture of the dynamic effects of the program, Figure 5 plots the share of individuals who do not visit the employment office, do not receive income support, and do not have any formal labor income over time. The figure shows that the program induced some individuals (7 percentage points) to stop visiting the employment office although they had no formal income (from income support or work). The gap between the treatment and control groups appears around two or three months after assignment to the program and remains constant thereafter. The drop shortly after program assignment suggests that, for some individuals, the costs associated with the additional program requirement of more intensive attendance to the employment office do not outweigh the benefits of receiving income support. While our data do not allow us to assess this hypothesis formally, it is likely that many of these individuals previously worked in the informal sector and claimed benefits—a behavior no longer feasible for them once they must spend several hours per week at the employment office. Some supporting evidence for this is shown in Appendix Figure A1, where we plot the relative likelihood of the characteristics of individuals who stopped reporting to the employment office without having any formal income (from work or social benefits) within two months after random assignment for different demographic groups.[26] Interestingly, the most disadvantaged groups (e.g., single parents, individuals with health limitations, ultra-Orthodox Jews, claimants from the stock subsample, and individuals residing in areas with high unemployment rates) are less likely to stop reporting to the employment office within two months without having any formal income. The groups that are more likely to stop reporting to the employment office without having any formal income are individuals with no recent income-support spells, individuals with no recent formal employment spells, and individuals who live in areas with low unemployment rates.

We further investigate the dynamic effects of the program by plotting its effects on employment over time in Figure 6, stratifying the sample by claimant type: stock versus flow. The figure reveals a very different dynamic pattern for the two subsamples: employment rates of the stock subsample

(Panel (a)) increase constantly over the whole period after assignment to treatment while for the flow subsample (Panel (b)), the increase in employment takes place mainly in the first months after assignment. This figure suggests that the threat effect (i.e., the extra requirements of the program) is probably the main driver of the employment effect for the flow subsample whereas for the stock subsample the employment effect increases gradually following participation in the workshops. There is also a notable difference in the magnitude of the impact across the two subsamples (reported in Appendix Table B4). The increase in employment rates for the stock subsample is 8 percentage points higher than for the flow subsample (14 ppts versus 6 ppts). Moreover, for the stock subsample, we find that the increase in employment rates explains about 70% of the decline in employment office attendance, while for the flow subsample, the increase in employment rates explains only 44% of the decline. We expand on this point below where we examine the mechanisms of the program effects.

VI. Assessing the Mechanisms

We present in this section the analysis of the survey data in order to provide additional information on the effect of the program on labor market outcomes and various measures of soft skills. These data originate from two follow-up surveys conducted by a third party over two periods—February 2015–June 2015 and April 2016–December 2016—capturing individuals fifteen months on average after random assignment.[27] Treatment and control groups were contacted by an external company by phone and were told that the survey was meant to produce statistics on individuals who report or reported to the IES for the purpose of improving IES customer service. We obtained responses from 2,497 of the 6,151 individuals included in our main analysis sample, a 41% response rate.[28] Roughly two-thirds of the observations came from the first survey and the rest from the second.[29] We discuss in Appendix B4 selection into the survey and report balancing tests for survey respondents, showing that there is no differential selection into the survey according to treatment status. We further discuss in Appendix B4 the construction of survey weights and show that we are able to reproduce our main results based on the administrative data by using the weighted sample of individuals who participated in the survey. This is important because it strengthens our confidence in using the survey sample to draw conclusions about the effects of the program for the full sample.

A. Survey Results

1. Labor market outcomes

We begin the survey analysis by exploring the program effects on additional labor market outcomes that are not recorded in the administrative data. We estimate the same model as in our main analysis, controlling for survey date. Table 6 displays the program treatment effect on labor force participation (defined by either working or searching for a job), employment, weekly hours worked, and labor income for the full sample (Column (1)) and for the stock and flow subsamples (Columns (2) and (3)). Estimates from Column (1) show that program led to increases of 7.1 and 6.4 percentage points in labor force participation and employment rates. The effects are larger for the stock subsample than for the flow subsample, both in absolute terms and relative to the outcome means. We see no program effect on full-time employment, indicating that the increase in employment rates was driven mainly by part-time employment.[30] The estimated program effects on the total number of weekly hours worked and income from work are positive and are larger for the stock subsample. A back-of-the-envelope calculation suggests that the magnitude of these effects almost perfectly corresponds to part-time minimum-wage work by members of the treatment group.[31]

2. Soft skills

Our findings have shown that the program improved labor market outcomes and reduced dependence on income support. We now examine the program's impact on participants' soft skills. While our analysis is limited to the soft skills measured in the follow-up surveys and we cannot individually manipulate skills to test their effects on labor market outcomes, our findings provide important and novel evidence on skills that were affected by the program.

The survey included thirty-four questions designed to assess individuals' soft skills and self-perception. These questions were grouped into five modules. For each question, participants were asked to specify the extent to which they agree with a statement on a four- or five-point Likert scale (from "strongly agree" to "strongly disagree"). The first module assesses job search self-efficacy, which refers to the individual's confidence in his/her ability to successfully search for a job and perform specific job search tasks.[32] The second module examines work self-efficacy, which measures the individual's confidence in his/her ability to manage workplace situations such as respecting schedules and collaborating with colleagues.[33] The third module examines general self-efficacy, which assesses the individual's confidence in taking courses of action in a wide array of situations. The fourth module assesses grit: perseverance and passion to achieve long-term goals. The fifth module focuses on self-esteem, which considers the individual's sense of self-worth and personal value. Three modules—job search self-efficacy, work self-efficacy, and general self-efficacy—were included in both survey waves; the grit and self-esteem modules were added only in the second one. This yielded a larger sample size for some of the skills.

The survey questions in each module and their sources are set forth in Appendix C. To facilitate the interpretation of the results, we reverse the scale of some of the questions so that a higher value denotes a better score, and transform each of the questions and the aggregate indices into z-scores. In Appendix Table A4, we report the inter-item correlations and Cronbach's alpha reliability coefficients for the different modules and in Appendix Table A5 we present the correlations between the different aggregate indices. The job search self-efficacy, work self-efficacy, and general self-efficacy domains show high internal consistency (Cronbach's Alpha 0.86, 0.96, and 0.86, respectively) whereas the grit and self-esteem domains have lower levels of consistency (Cronbach's Alpha 0.56 and 0.79, respectively).[34]

We start by reporting in Table 7 the association between these soft skills and labor market outcomes using the control group. This is not done to establish causality but to examine the informational content of the survey indices.[35] For this purpose, we regress each of the survey labor market outcomes on the mean standardized scores of each of the five soft skills modules while controlling for individual characteristics. The results show that all soft skills are positively correlated with better labor market outcomes.

We then examine the effect of the program on soft skills by plotting in Figures 7–11 the cumulative distribution functions (CDFs) of these skills for the treatment and control groups along p-values for Mann-Whitney tests of stochastic dominance.[36] Given that the randomization was executed separately for the stock and flow subsamples in each employment office and in light of the stark differences in the dynamic effects of the program for the two subsamples, we plot the CDFs for the full sample and then separately for the stock and the flow subsamples.

Focusing on the full sample, we see that the CDFs of the treatment group for job search self-efficacy, work self-efficacy, and self-esteem are shifted to the right relative to those of the comparison group, suggesting that the program indeed improved these skills. This is also confirmed by p-values of the Mann–Whitney tests that reject the null hypothesis for equality of distributions between the treatment and control groups. By contrast, no significant differences emerge between the CDFs of the treatment and control groups for grit or general self-efficacy.

The stratification by claimant type plotted in Panels (b) and (c) reveals important differences between the stock and flow subsamples. We first note that, based on comparisons within the control group, individuals from the stock subsample score lower in all soft skills relative to the flow subsample (22% standard deviations lower, on average).[37] Next, we find that improvement in soft skills occurs almost exclusively in the stock subsample. For this subsample, we observe that the CDFs of all soft skills of treated individuals dominate the corresponding CDFs of the controls. P-values of Mann–Whitney tests are lower than 5% for self-esteem, work self-efficacy, job search self-efficacy, and general self-efficacy, and lower than 10% for grit. By contrast, for the flow subsample there is only a small shift in the distribution for self-esteem while none of the p-values for the Mann–Whitney tests are significant.

We confirm our results in Table 8, where we report regression coefficients of average treatment effects for each skill module, using a system of seemingly unrelated regressions (SUR) based on equation (1) that treat the skills in each module as a family of outcomes. This method takes into account that the outcomes in each module are correlated by allowing for individual-level correlation of the error terms across equations (see Kling, Liebman, and Katz 2007).[38] The effects on each individual skill are presented in Appendix Tables A6–A10. In Column (1) we report estimates for the full sample and in Columns (2) and (3) we report estimates for the stock and flow subsamples. Estimates are reported in terms of standard deviation units.

Consistent with the evidence presented in Figures 7–11, we see a significant and positive effect of the program on its participants' soft skills in the stock subsample. For this subsample, treatment effect estimates show an improvement in self-reported job search self-efficacy (19%), work self-efficacy (13%), general self-efficacy (15%), grit (15%), and self-esteem (23%). By contrast, estimates for the flow subsample are small, have inconsistent signs across outcomes, and are not significant. Simple t-tests can reject the equality of the coefficients between the stock and the flow subsamples for all soft skills except for work self-efficacy.

The findings on soft skills and the dynamics of employment effects for the stock and flow subsamples form a consistent picture of the mechanisms at work in the program. Individuals in the flow subsample, who joined the program soon after registering at the employment office, seem to be mainly affected by the threat effect of the program and return to work relatively fast without benefiting from the workshops and experience almost no improvement in their soft skills. By contrast, individuals in the stock subsample, who joined the program while on welfare and after a longer disconnection from the labor market, appear to improve their soft skills and enhance their employment rates.

A relevant question is whether the observed improvement in soft skills in the treatment group is a direct result of the workshops, and whether this improvement, in turn, enhanced participants' labor market outcomes – or whether the causal chain between employment and soft skills runs in the opposite direction. Specifically, the program may have increased employment rates through its threat effect, such that the improvement in participants' soft skills can be attributed to their employment. While we cannot completely rule out this alternative interpretation, we note that the improvement in soft skills is observed only among individuals in the stock subsample, whose employment rates increased gradually over time, consistent with their program participation. Additionally, these individuals also exhibit an improvement in general skills, such as self-esteem and self-efficacy, which are not directly related to job search or employment. By contrast, individuals in the flow subsample, whose employment rates increase almost immediately after assignment to the program, likely due to the threat effect, do not exhibit any improvement in soft skills.

Following the causal chain hypothesis of an improvement in soft skills that led to an increase in employment among individuals in the stock subsample, we can perform a simple back-of-the-envelope calculation combining estimates of the program effect on soft skills from Column (2) of Table 8 and the associations between soft skills and employment from row 2 of Table 7. This calculation shows that the improvement in soft skills of the stock subsample can explain 39% of their 12 percentage-point increase in employment based on the survey results (reported in Column (2) of Table 6) or 34% percent of their 13.8 percentage-point increase in employment based on the administrative data (reported in Appendix Table B4).[39] Note that this calculation should be viewed with caution since it is based on simple correlations between soft skills and employment and assumes that the improvement in each of the skills enters linearly and additively in the employment function with no interactions, complementarities, or substitution between skills. In addition, other skills could have been improved by the program that were not measured in the survey, which could also improve employment or earnings capacity.

VII. Long-Term Effects and Program Impacts during the Covid-19 Crisis

We conclude our analysis by examining the long-term effects of the program. We obtained an updated data retrieval of the IES operational database from 2021. This allows us to examine the long-term effects of the program on the probability of individuals reporting to the employment office five to six years after randomization and the status of the treatment and control groups during the Covid-19 crisis.[40] Using the same specification as in equation (1), we report program effects and outcome means of the control group in Table 9. In contrast to the previous results, we report the employment status of individuals for a specific calendar date and not as a function of months since randomization. We begin by reporting in the first two rows of the table treatment effects on the share of individuals reporting to the IES in January and February 2020, respectively (just before the onset of the Covid-19 pandemic in Israel).[41] Estimates show that the program effect persists also after five to six years. Treated individuals are 14% less likely to report to the employment office relative to the control group in January 2020. As before, we find larger treatment effects in the stock subsample compared to the flow subsample both, relative to the outcome means and in absolute terms: 41.5% versus 36% (or 8.3 percentage points versus 6.1 percentage points). Estimates are very similar for February 2020. In the third row of the table, we report the long-term effect of the program on the probability of individuals claiming benefits (either welfare or unemployment) in April 2020, the month with the highest number of registered individuals at the IES during the first 18 months that preceded the onset of Covid-19 in Israel—roughly 1.13 million individuals. As seen in the table, the share of individuals claiming benefits almost doubled from February 2000 to April 2000 in the control group, increasing from 0.171 to 0.330. Nevertheless, the increase was less dramatic in the treatment group. Overall, the gap in the share of individuals claiming benefits between the treatment and control groups narrowed during the onset of the Covid-19 crisis, but the share was still lower for the treatment group relative to the control group—a gap of 13% (or 4.4 percentage points). Differences are again more pronounced for the stock than for the flow subsample (15% versus 13%). The fourth row of the table reports differences between the treatment and control groups in March 2021, just at the end of the third wave of the pandemic and the end of the third lockdown. The gaps between the groups persisted, especially for the stock subsample.

At the bottom part of the table, we focus on those individuals who were still claiming benefits in March 2021 and examine controlled differences between the treatment and control groups in their status upon registration at the IES. Individuals could register to claim welfare benefits or unemployment benefits and, within the unemployment category, they could report that they were on furlough or unemployed as the reason for claiming benefits.[42] Although treated individuals are negatively selected given that they are less likely to claim benefits, we observe that, conditional on claiming benefits, they demonstrate stronger labor market attachment compared to the control group. Specifically, treated individuals were more likely to claim unemployment insurance rather than welfare benefits, and exhibited higher rates of furlough registration. These patterns suggest that treated individuals maintained stronger connections to formal employment relative to the control group, even when requiring social assistance. Overall, the evidence reported here indicates that the program's impacts were not only sustained over the long term but also manifested during the COVID-19 crisis, suggesting that the program enhanced individuals' resilience to adverse labor market shocks.

VIII. Conclusions

A growing literature in economics and other social sciences stresses the importance of soft skills for human capital formation and labor market success. Yet, there is little evidence about the returns to investments in these skills, especially among adults. This paper examines the impact of an active labor market program implemented in Israel that focuses on enhancing welfare recipients' soft skills to prepare them for successful integration into the labor market. Using a randomized control trial, we estimate the effect of the program on a wide range of outcomes and examine the mechanisms through which the program operates.

Our findings indicate that the program increased labor force participation, employment rates, and labor income. We also find a reduction in income-support recipiency and, correspondingly, on the size of income-support payments received by those assigned to the program, with no evidence of substitution toward alternative social benefits (e.g., disability insurance). The cost of the program per participant is more than outweighed by savings on government welfare transfers within twelve months, making this program cost-effective and implying a MVPF of infinity. Furthermore, the program also had positive spillovers within the household, increasing not only the labor income of treated individuals but also that of their partners. We find no evidence of spillover effects among individuals in the control group.

The program had a stronger impact on individuals with lower ex-ante employment probabilities, namely, those who have lower labor force attachment and a longer history in the welfare system, fewer than twelve years of schooling, self-reported health limitations, and individuals who were already on welfare when they entered the program.

Overall, the program reduced the share of treated individuals who report to the employment office. The total decrease can be decomposed into two separate channels that affected different individuals. Part of the effect is driven by individuals who stopped reporting to the employment office due to the additional program requirements. Others, mainly individuals who were already claiming welfare benefits when assigned to the program (the stock subsample), show a gradual increase in employment that is consistent with participation in the program's workshops.

The analysis of the survey data supports these findings and shows that the program has a positive impact on the soft skills of the stock subsample. In particular, we observe that the program led to an increase in job search self-efficacy, work self-efficacy, self-esteem, general self-efficacy, and grit. These soft skills are associated with superior labor market outcomes and appear to mediate part of the impact of the program on employment. Our study shows that it is possible to enhance the work-related attitudes and self-perception of long-term unemployed individuals in a cost-effective way, leading to an increase in their employment and earnings. These effects have also positive spillovers within households, making such programs even more attractive. Moreover, the benefits of the program are evident in the long term, five to six years after its implementation, and persisted during the Covid-19 crisis, providing evidence that soft-skills training can help disadvantaged groups better cope with adverse labor market shocks.

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[1] Following Hendren and Sprung-Keyser (2020), we define MVPF as the ratio between individuals' willingness to pay for the program and the net government cost.
[2] Kluve's (2010) meta-analysis, for example, includes only nine RCTs among the 137 ALMPs reviewed. In Card, Kluve, and Weber (2018), only one-fifth of the reported estimates are based on RCTs.
[3] Acevedo et al. (2020) find positive effects on employment among women, but not men, who participated in a program that offered soft skills and vocational training for at-risk youths in the Dominican Republic. Groh et al. (2016) find no employment or wage effects from a 45-hour soft skills training program targeted at women graduating from community colleges in Jordan. Adhvaryu, Kala, and Nyshadham (2023) find productivity gains but no effects on wages or job retention from a program that offered on-the-job soft skills training to Indian garment workers.

[4] The only study we found that assessed spillovers effects of ALMPs on the household is Kugler et al. (2022) who detected positive spillovers of a training program in Colombia on the likelihood of attaining tertiary education among participants' relatives.
[5] Exempt are prisoners currently performing community service or under house arrest, ex-prisoners during the first couple of months after their release, alcohol or drug addicts, pregnant women, women in women's shelters, caregivers of a sick household member, and supervisors of a household member under house arrest.
[6] The content of the training and the workshops is based on the STRIVE international model developed by Strive US (https://strive.org/), which emphasizes personal development and improvement of tools needed to integrate into and excel at a job. The model was adapted to and tested in the Israeli context by the Israeli employment incubator JDC-Tevet.
[7] The extra weekly visits requirement also makes it more difficult for participants to work in the informal sector while declaring themselves unemployed and claiming benefits, prompting some of them to shift to the formal sector. On the other hand, the requirement might induce some participants to stop reporting to the employment office, potentially increasing their informal employment. Therefore, while the program is expected to have a positive effect on formal employment, its effect on informal employment can go in both directions. We lack data on informal employment, and therefore our results pertain to formal employment and formal income. Nevertheless, estimating the impact of the program on formal employment holds its own value as formal employment provides greater job stability, social benefits, and legal rights, making for a higher-quality job overall.
[8] The Arab population accounts for about one-fifth of Israel's population.
[9] By contrast, only 35% of individuals from the flow subsample received income-support benefits in the twelve months preceding random assignment.
[10] This is also the case when an individual was registered at the employment office at the assignment to the program, but his/her partner registered at the IES afterward. In this case, the partner was assigned to the program upon registration to the IES.
[11] This is under the assumption that the program generates no externalities in the control group. We assess this assumption in the Online Appendix B2, where we show that there is no evidence of externalities in the control group.
[12] 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.
[13] Seventy-three income-support claimants were exempted from participating by a committee due to various personal circumstances, out of a total of 5,700 who were randomized into the treatment during the first sixteen months of the program.
[14] We adopt a conservative approach by clustering our standard errors since treatment assignment varied within clusters (see Abadie et al. 2023). We adopt this approach for the following reasons. From a sampling design viewpoint, we estimate the program effects using data from a sample of clusters and not the entire population (i.e., we analyze only data of individuals randomized in the first 12 months of the program's implementation and from 14 employment offices that participated in the pilot). From an experimental design viewpoint, treatment assignment probabilities varied across clusters. An additional justification is provided by Deeb and de Chaisemartin (2022) who show that clustering allows to account for variability in cluster-level shocks that affect the outcome, increasing the external validity of the estimated treatment effects. Overall, our standard errors (not reported in the tables to save space) are smaller without the adjustment, but this matters little given that our estimates for the program effects are highly significant.
[15] Unfortunately, while we observe program assignment, we do not observe participation in the different workshops.
[16] Given that employment and income are based on administrative records, the outcomes we measure are formal employment and formal income. We therefore refer to formal employment and formal income when we discuss the results.
[17] These are individuals whose last visit to the employment office predates their randomization. Compared with the general population of income-support claimants, they are younger, are less likely to report any health limitations, and have a shorter history in the welfare system.
[18] We focus on twelve months before program assignment instead of just before participation to avoid a pre-program period that is inherently related to the negative shock that program participants experienced that made them eligible for the program.
[19] Some employed individuals continue to receive income support in the form of an income supplement (provided their labor income is below a certain threshold).
[20] We estimate unconditional quantile treatment effects as developed by Firpo, Fortin, and Lemieux (2009), controlling for randomization unit by applying the algorithm developed by Borgen (2016). This method does not identify the distribution of treatment effects but rather provides estimates for treatment effects on the income distribution.

[21] Households where both partners are treated are compared to households where both partners were assigned to the control group. Similarly, households where only one partner was treated are compared to households where only one partner was assigned to the control group.
[22] Following Abadie, Chingos, and West (2018), we use all covariates and the outcome in the control group to predict the potential outcome if untreated for each individual. We then stratify the sample into three groups according to levels of the predicted outcome and estimate treatment effects for each subgroup. To avoid the finite sample bias that comes from fitting a prediction regression within sample, we perform this twice using leave-one-out regressions and repeated split samples.
[23] The means of the treatment group are computed by adding the treatment effect to the outcome means of the control group in order to compare treatment and control groups within the same randomization unit.
[24] The employment rates do not drop to zero at the randomization date because the NII employment records refer to a calendar month while the randomization date may occur at any point during the month. For example, if an individual worked until March 5, 2014, and was assigned to the program on March 20, 2014, she would be recorded as employed on the assignment date. In practice, this creates a slight measurement error for employment spells close to the assignment date, but it matters little for our main results because we focus on medium-term effects. In addition, measurement error in these employment spells should be the same in both the treatment and the control group.
[25] An alternative interpretation is that program participants find it costlier over time to participate in the workshops, causing the same job offers to become gradually more attractive.
[26] We plot in the figure the following conditional probability for each of the characteristics defined by X_i : $\frac{P[X_i=1|D_{1i}>D_{0i}]}{P[X_i=1]}$, where $D_i = 1$ if the individual stops reporting to the employment office and does not have any formal income (earnings or benefits) within two months of random assignment and $D_{1i} =$ status when treated and $D_{0i} =$ status when untreated. In practice, this is the ratio of the treatment effect for the specific subgroup divided by the average treatment effect.
[27] Due to IES logistical constraints, it was not possible to survey each individual at a specific time after randomization. Therefore, the number of months between randomization and the survey date varies across individuals but is balanced across treatment and controls. Individuals in our sample were surveyed between four and thirty-four months after random assignment. The vast majority (86%) were polled at least six months after randomization. The average time was fifteen months and the median ten months.
[28] 567 individuals participated in both surveys.
[29] The second survey wave was larger, comprising 1,854 additional individuals who were randomized into treatment and control groups between January 2015 and March 2016. We exclude these observations from the analysis because we wish to focus on the survey sample that coincides with our main sample of individuals who were randomized during 2014, for whom we have complete administrative records on labor market outcomes and welfare benefits for a period of at least twelve months after random assignment.
[30] The estimate for full-time employment for the stock subsample is positive but very imprecisely measured.
[31] If we assume that those who started working because of the program have done so by working in 'half-time' jobs (21.5 hours a week), we would expect an increase of 1.38 hours for the treated group. The estimate we get is just slightly lower (1.24). Similarly, if we assume these jobs are at minimum wage (NIS 23.12 per hour in 2015), and are 'half time' jobs (93 hours a month); we would expect to get an estimated program impact on average monthly income from work of NIS 138 (0.064 x 23.12 x 93). This estimate is virtually identical to the estimate we obtain: NIS 141.
[32] Job search self-efficacy can be affected by learned skills and self-perception.
[33] The questions in this module refer to the individual's current or future job. Individuals who were not working at the time of the survey were asked whether they thought they would be able to handle these situations in their future job.
[34] We obtain very similar results based on McDonald's omega (McDonald 1999): job search self-efficacy = 0.864, work self-efficacy = 0.963, general self-efficacy = 0.863, grit = 0.491, self-esteem = 0.776.
[35] Conducting an equivalent exercise using the available administrative labor market outcomes, we found a similar pattern (results not shown).
[36] To compare the distributions, we use residualized z-scores that we obtain by regressing each z-score on the vector of individual characteristics. To account for the randomization unit fixed effects, we apply inverse probability weighting, weighting treated observations by *1/p* and control observations by $1/(1-p)$ (where p is the proportion of individuals treated in the randomization unit). We then adjust the weights for those surveyed twice by dividing by two, trim weights to the 90th percentile to avoid extreme values, and normalize them to make sure they add up to 1 for each group and reflect the total sample size.

[37] This is based on a regression of each of the soft skills measures on an indicator for the stock subsample and fixed effects for the month of survey, the randomization month, and the employment office. Results not shown but are available upon request.

[38] That is, we define the average treatment effect for module c as $\tau_c = \frac{1}{K_c}\sum_{k=1}^{K_c}\frac{\pi_{kc}}{\sigma_{kc}}$, where K_c is the number of outcomes included in module c, π_{kc} is the effect on outcome k included in module c, and σ_{kc} is the standard deviation of the outcome. We treat (σ_{kc}) as known based on the results of Kling and Liebman (2004) and given that we have a large sample.

[39] This is obtained by multiplying the treatment effect for each of the skills by their coefficient in the employment regression based on the control group: 0.189 x 0.065 + 0.129 x 0.065 + 0.148 x 0.034 + 0.154 x 0.065 + 0.231 x 0.049 = 0.047.

[40] In March 2020, nonessential government and local authority workers were placed on furlough until the end of April and private sector firms exceeding 10 employees were required to limit the staff present in the workplace to 30%, which was further tightened to 15% in the private sector in the first half of April. All laid off workers over 20 who completed a qualifying period of six months of employment during the 18 months prior to the layoffs were eligible to claim unemployment insurance (UI). Due to social distancing and lockdown measures, both UI and welfare benefits were paid to eligible claimants without the requirement to report to the employment office. In addition, the UI eligibility period was extended, and other eligibility requirements were either lifted or relaxed. See Gal and Madhala (2020) for more information on the changes in the Israeli unemployment insurance and welfare program during the Covid-19 crisis.

[41] The first confirmed case of Covid-19 in Israel was on February 21st.

[42] The unemployment benefit levels did not differ between voluntary separations (resignations) and involuntary separations (dismissals and furloughs).

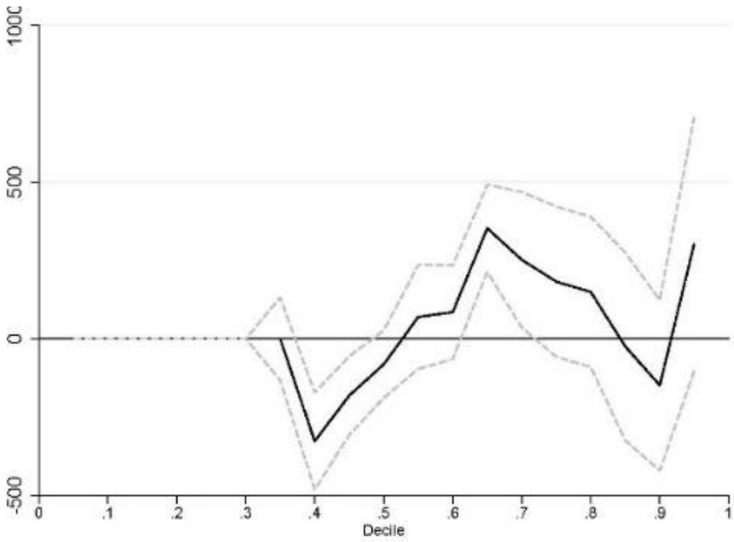


Figure 1
Quantile treatment effects on the distribution of total income

Notes: The figure reports the program effect for each ventile of the total income (i.e. labor earnings and income support) distribution 12 months after random assignment with a 90 percent confidence interval.

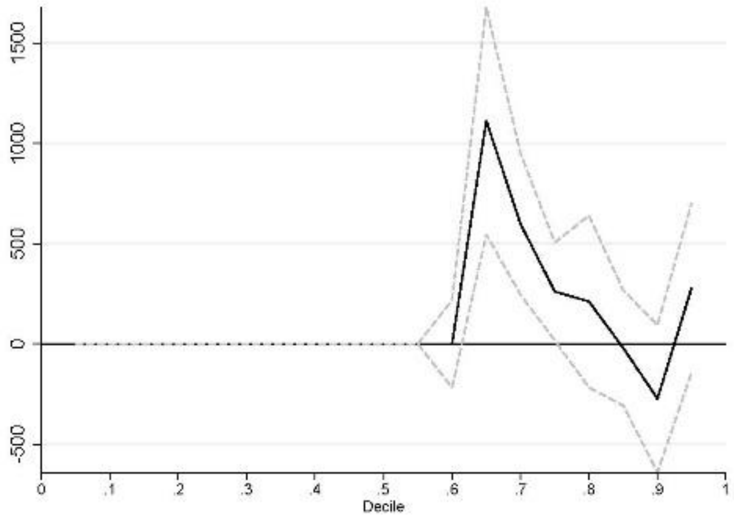
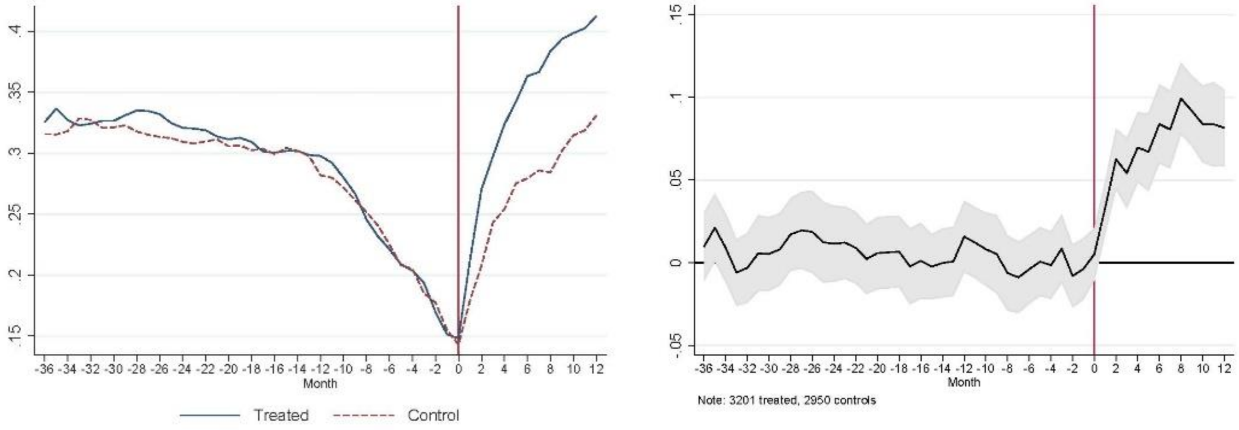


Figure 2
Quantile treatment effects on the earnings distribution

Notes: The figure reports the program effect for each ventile of the labor earnings distribution 12 months after random assignment with a 90 percent confidence interval.

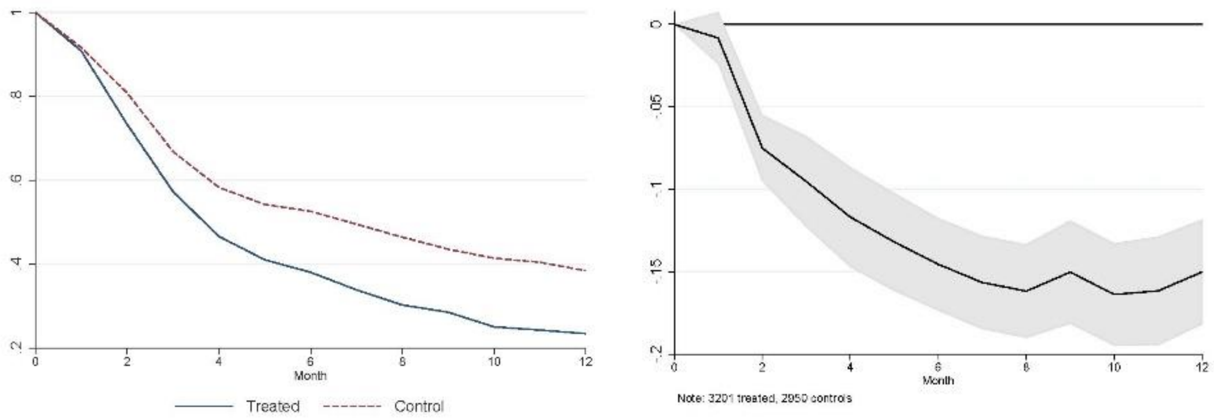


Levels

Treatment-Control

Figure 3
Dynamic effects - employment

Notes: The figure displays the dynamic effects on employment. The left panel shows employment rates for treatment and control groups, while the right panel presents treatment versus control differences with confidence bands, spanning from three years before random assignment to the program to twelve months after randomization. Month zero denotes the month of random assignment.

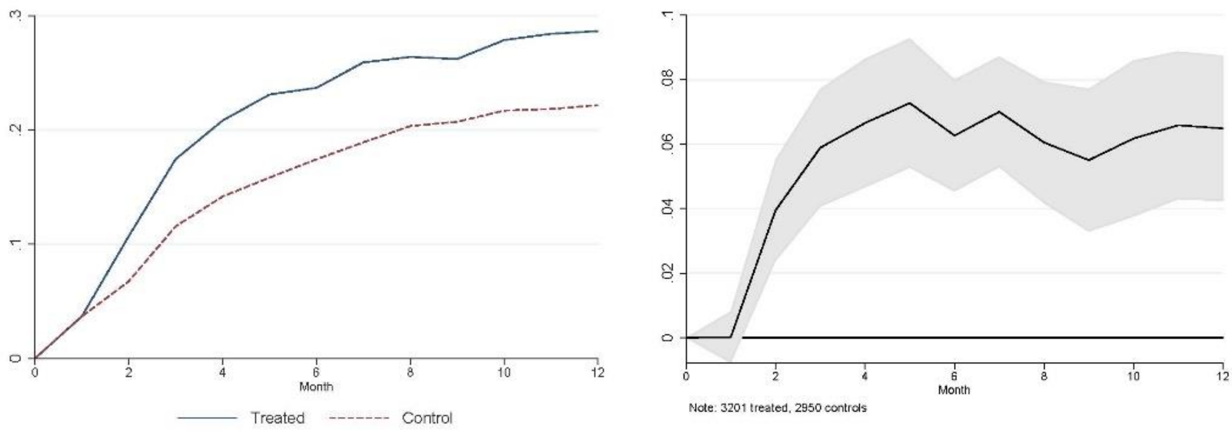


Levels

Treatment-Control

Figure 4
Dynamic effects - share reporting to employment office

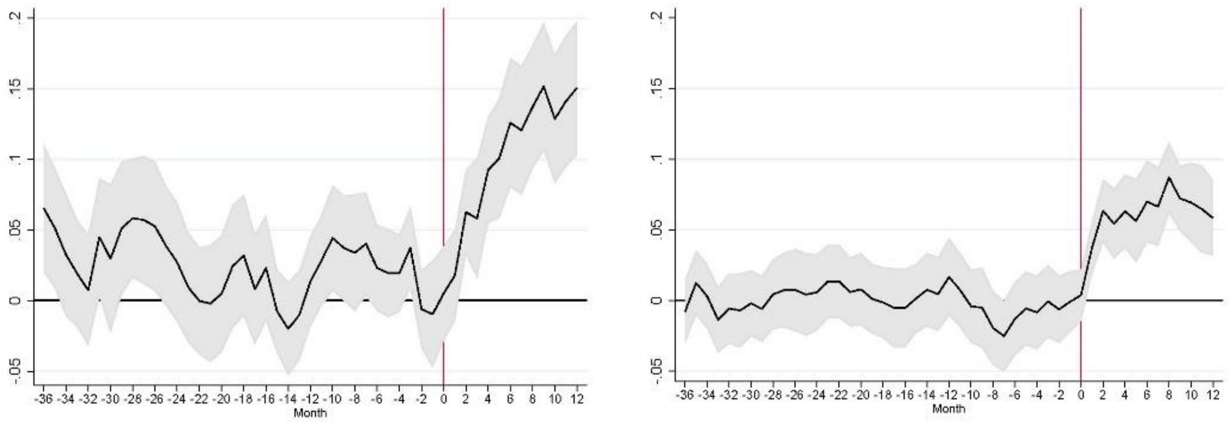
Notes: The figure displays the dynamic effects on the share reporting to the employment office. The left panel shows the share reporting to the employment office for treatment and control groups, while the right panel presents treatment versus control differences with confidence bands, spanning from three years before random assignment to the program to twelve months after randomization. Month zero denotes the month of random assignment.



Levels Treatment-Control

Figure 5
Dynamic effects - share not employed, not reporting to employment office and not receiving income support

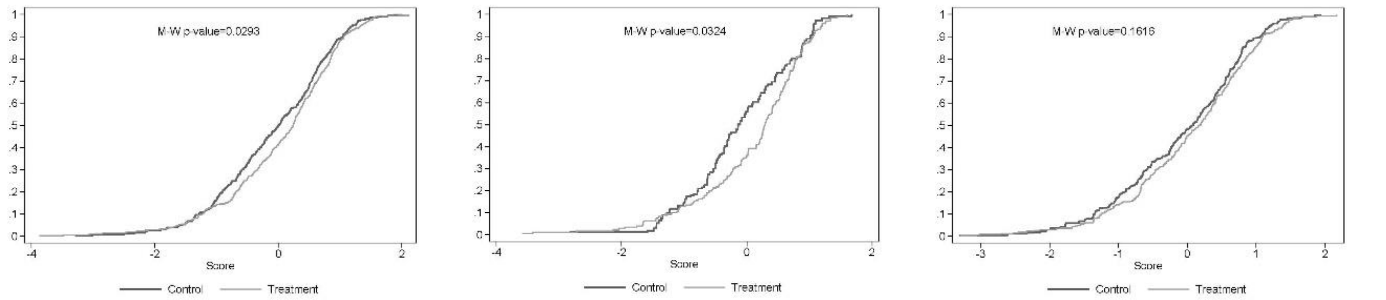
Notes: The figure reports the probability of not reporting to the employment office while not working nor receiving income support benefits for the treatment and control groups (left panel) and the difference in this share between both groups with a 90 percent confidence interval (right panel), spanning from three years before random assignment to the program to twelve months after randomization. Month zero denotes the month of random assignment.



Stock Flow

Figure 6
Dynamic effects - employment by claimant type

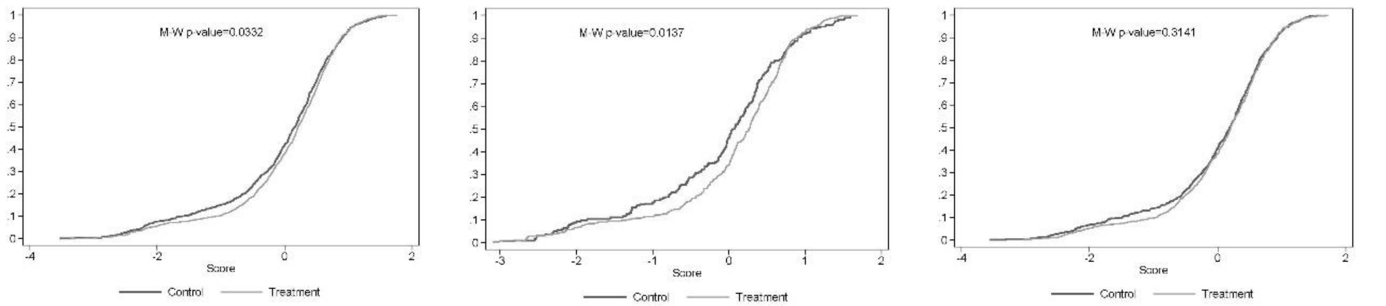
Notes: The figures plot the program effect on employment with a 90 percent confidence interval for samples stratified by claimant type. The stock subsample (left panel) refers to existing claimants and the flow subsample (right panel) refers to new or re-registering claimants at time of assignment to the program. Month zero denotes the month of random assignment.



All Stock Flow

Figure 7
Program Effect on Self-Esteem

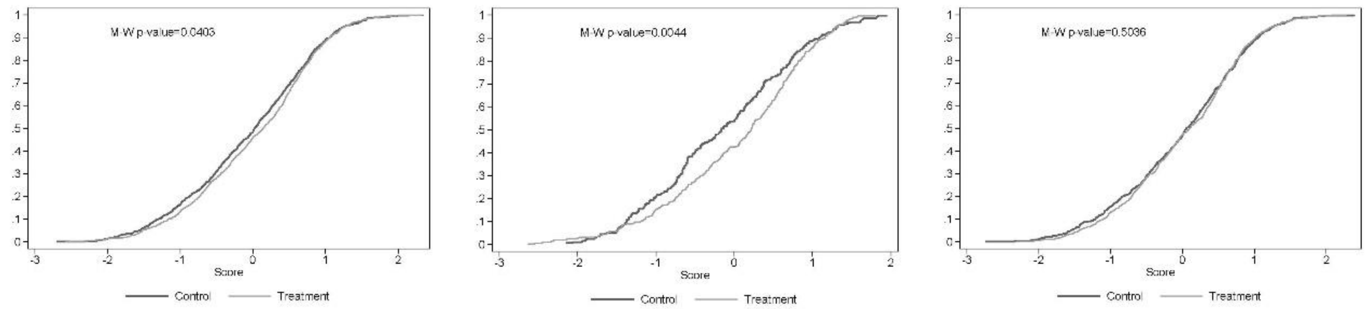
Notes: The figures plot the cumulative distribution functions of the residualized Self-Esteem index by treatment status. Subfigure (a) plots CDFs of the full sample, subfigure (b) plots CDFs of the Stock subsample, and subfigure (c) plots CDFs of the flow subsample. The stock subsample refers to existing claimants and the flow subsample refers to new or re-registering claimants at time of assignment to the program. Reported p-values refer to the results of the Mann-Whitney tests of stochastic dominance.



All Stock Flow

Figure 8
Program Effect on Work Self-Efficacy

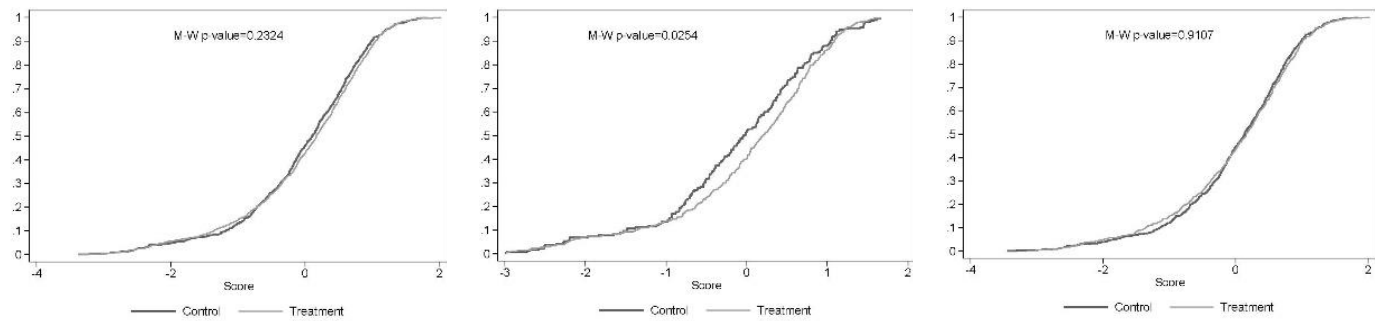
Notes: The figures plot the cumulative distribution functions of the residualized work Self-Efficacy index by treatment status. Subfigure (a) plots CDFs of the full sample, subfigure (b) plots CDFs of the Stock subsample, and subfigure (c) plots CDFs of the flow subsample. The stock subsample refers to existing claimants and the flow subsample refers to new or re-registering claimants at time of assignment to the program. Reported p-values refer to the results of the Mann-Whitney tests of stochastic dominance.



All Stock Flow

Figure 9
Program Effect on Job-Search Self-Efficacy

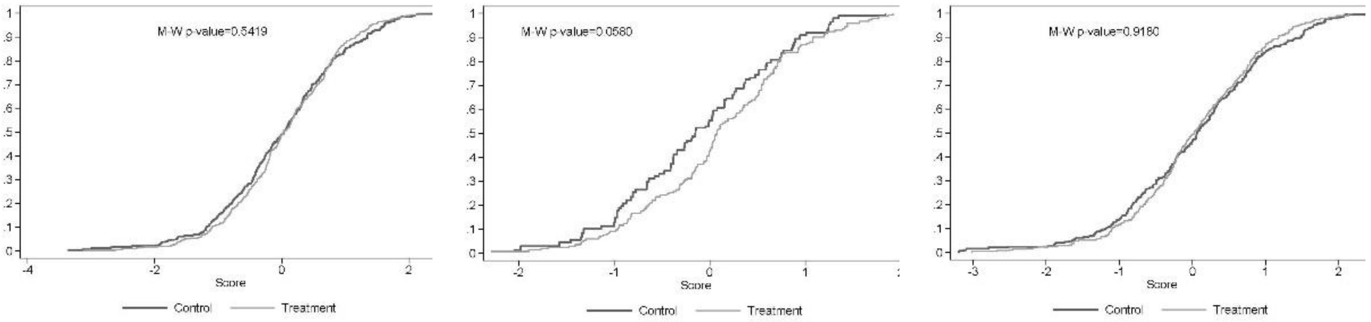
Notes: The figures plot the cumulative distribution functions of the residualized Job-Search Self-Efficacy index by treatment status. Subfigure (a) plots CDFs of the full sample, subfigure (b) plots CDFs of the Stock subsample, and subfigure (c) plots CDFs of the flow subsample. The stock subsample refers to existing claimants and the flow subsample refers to new or re-registering claimants at time of assignment to the program. Reported p-values refer to the results of the Mann-Whitney tests of stochastic dominance.



All Stock Flow

Figure 10
Program Effect on Self-Efficacy

Notes: The figures plot the cumulative distribution functions of the residualized General Self-Efficacy index by treatment status. Subfigure (a) plots CDFs of the full sample, subfigure (b) plots CDFs of the Stock subsample, and subfigure (c) plots CDFs of the flow subsample. The stock subsample refers to existing claimants and the flow subsample refers to new or re-registering claimants at time of assignment to the program. Reported p-values refer to the results of the Mann-Whitney tests of stochastic dominance.



All Stock Flow

Figure 11
Program Effect on Grit

Notes: The figures plot the cumulative distribution functions of the residualized Grit index by treatment status. Subfigure (a) plots CDFs of the full sample, subfigure (b) plots CDFs of the Stock subsample, and subfigure (c) plots CDFs of the flow subsample. The stock subsample refers to existing claimants and the flow subsample refers to new or re-registering claimants at time of assignment to the program. Reported p-values refer to the results of the Mann-Whitney tests of stochastic dominance.

Table 1. Employment Offices in the RCT versus All other Offices

	Employment offices in the RCT (1)	All other employment offices (2)
Number of active jobseekers	25,459	30,973
Age	38.2	38.4
Education	9.3	9.6
Number of supported children	2.8	2.4
Women	0.61	0.64
Married	0.52	0.47
Arab	0.64	0.54
Immigrant	0.13	0.16
Locality S.E.S	5.0	5.1
Local unemployment rate	0.065	0.072
N	14	57

Notes: The table reports the population characteristics and local labor market conditions in employment offices included in the RCT and in the remaining employment offices in Israel. The number of job seekers and their average characteristics are based on all active income support claimants aged 18-50 in the IES system in March 2014. The local unemployment rate is the population-weighted average of localities in the catchment area of the employment offices in each group. Locality S.E.S is the population-weighted average S.E.S index of localities in the catchment area of the employment offices in each group in 2012. The S.E.S index is published by The Central Bureau of Statistics (CBS) and ranges from 1 (lower SES) to 10 (highest SES).

Table 2. Descriptive Statistics and Balancing Tests

	Treatment (1)	T-C (2)
Female	0.544	-0.011
		(0.018)
Age	34.57	0.169
		(0.263)
Married	0.473	0.004
		(0.012)
Children	2.00	0.061
		(0.068)
Single parent	0.219	0.003
		(0.012)
Immigrant	0.208	-0.024*
		(0.013)
Self-reported health limitation	0.362	0
		(0.013)
Arab	0.347	0.011
		(0.011)
Ultra-Orthodox	0.189	0.019**
		(0.009)
Less than 12 years of schooling	0.394	-0.028*
		(0.015)
12 years of schooling	0.555	0.029*
		(0.016)
More than 12 years of schooling	0.050	0
		(0.008)
Received income support months [-12;0]	0.523	0.013
		(0.013)
Received income support months [-24;-11]	0.270	0.004
		(0.016)
Received income support months [-36;-23]	0.236	0.007
		(0.013)
Months worked months [-12;0]	2.82	0.003
		(0.129)
Months worked months [-24;-11]	3.93	0.068
		(0.141)
Months worked months [-36;-23]	4.29	0.143
		(0.149)
Total earnings months [-12;0]	9754	80
		(614)
Total earnings months [-24;-11]	16320	680
		(820)
Total earnings months [-36;-23]	18242	860
		(871)
Total income support months [-12;0]	5946	250
		(326)
Total income support months [-24;-11]	3755	220
		(269)
Total income support months [-36;-23]	3211	190
		(208)
Months since registration	3.36	-0.056
		(0.000)
F-Stat for joint significance	1.01	
P-value	0.45	
Number of observations	3201	6151

Notes: The table reports the average characteristics of treatment group participants (column 1) alongside the estimated difference with the control group conditional on randomization unit fixed effects (column 2). The reported F-statistic tests the joint significance of all covariants in a linear probability model predicting treatment status conditional on randomization unit fixed effects. Monetary values in real 2016 NIS. Standard errors clustered at the randomization unit level in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3. Program Effect 12 and 18 Months After Randomization

	12 months horizon sample	18 months horizon sample		
	Impact after 12 months (1)	Impact after 12 months (2)		Impact after 18 months (3)
Reporting to employment office	-0.15***	-0.171***		-0.133***
	(0.019)	(0.027)		(0.027)
	0.384	*0.405*		*0.330*
Employed	0.079***	0.089***		0.082***
	(0.014)	(0.022)		(0.025)
	0.331	*0.326*		*0.353*
Income from work	161**	200*		276**
(Including zeroes)	(65)	(114)		(121)
	1,345	*1,341*		*1,422*
Cumulative income from work	2026***	2130**		3334**
(Including zeroes)	(563)	(902)		(1404)
	12,301	*11,897*		*20,306*
Received Income support	-0.105***	-0.132***		-0.105***
	(0.017)	(0.024)		(0.022)
	0.408	*0.415*		*0.360*
Income support payments	-170***	-233***		-184***
(Including zeroes)	(29)	(41)		(41)
	625	*651*		*562*
Cumulative income support	-1860***	-2300***		-3507***
(Including zeroes)	(278)	(376)		(558)
	8,813	*8,994*		*12,576*
Total Income	-9	-33		92
(Including zeroes)	(72)	(108)		(119)
	1,971	*1,992*		*1,984*
Total cumulative income	167	-171		-173
(Including zeroes)	(663)	(908)		(1372)
	21,114	*20,891*		*32,881*
Received other welfare payments	-0.009	-0.002		-0.01
(disability or UI or other)	(0.009)	(0.017)		(0.019)
	0.111	*0.112*		*0.134*
N	6151	1643	#	1643

Notes: The table reports the program effect on participants' outcomes. Controls include sex, marital status, age, number of children, schooling level, indicators for new immigrant, single mothers, Arab, ultra-orthodox Jew, self-reported health limitations, vectors for employment, income from work and welfare history, 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.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4. Program Effects at the Individual and Household Level

	All (1)	Both partners treated (2)	Only one partner treated (3)	Singles (4)
Reporting to employment office	-0.15***	-0.233***	-0.14***	-0.133***
	(0.019)	(0.048)	(0.031)	(0.019)
	0.384	*0.526*	*0.350*	*0.349*
Employment	0.079***	0.109***	0.078***	0.075***
	(0.014)	(0.036)	(0.023)	(0.021)
	0.331	*0.231*	*0.308*	*0.382*
Income from work	161**	300*	57	192*
(Including zeroes)	(65)	(159)	(115)	(99)
	1,345	*0,841*	*1,309*	*1,532*
Cumulative income from work	2026***	2407**	2258**	1811**
(Including zeroes)	(563)	(1194)	(1040)	(851)
	12,301	*7,566*	*11,617*	*14,324*
Received Income support	-0.105***	-0.236***	-0.095***	-0.073***
	(0.017)	(0.060)	(0.028)	(0.020)
	0.408	*0.630*	*0.389*	*0.347*
Income support payments	-170***	-324***	-160***	-147***
(Including zeroes)	(29)	(79)	(40)	(43)
	625	*809*	*552*	*615*
Cumulative income support	-1860***	-3140***	-1838***	-1624***
(Including zeroes)	(278)	(699)	(503)	(412)
	8,813	*10,583*	*8,004*	*8,786*
Total Income	-8.9	-24.8	-102.1	45.4
(Including zeroes)	(71.6)	(149.6)	(119.3)	(108.2)
	1,971	*1,650*	*1,860*	*2,147*
Total cumulative income	167	-734	420	187
(Including zeroes)	(663)	(1197)	(1205)	(1002)
	21,114	*18,149*	*19,622*	*23,110*
Received other welfare payments (disability or	-0.009	0.006	0.007	-0.02
UI or other)	(0.009)	(0.014)	(0.016)	(0.014)
	0.111	*0.048*	*0.072*	*0.152*
HH level - Income from work	283***	647*	324	192*
(Including zeroes)	(102)	(343)	(227)	(99)
	2,114	*1,746*	*3,270*	*1,532*
HH level - cumulative Income from work	3399***	6827**	4574**	1811**
(Including zeroes)	(893)	(2716)	(2140)	(851)
	20,213	*15,747*	*32,505*	*14,324*
HH level - Income support payments	-257***	-664***	-255***	-147***
(Including zeroes)	(40)	(155)	(70)	(43)
	0,900	*1,617*	*0,967*	*0,615*
HH level - Cumulative income support	-2844***	-6186***	-3274***	-1624***
(Including zeroes)	(363)	(1300)	(811)	(412)
	12,596	*21,240*	*13,991*	*8,786*
HH level - Total Income	26	-17	69	45
(Including zeroes)	(101)	(313)	(216)	(108)
	3,014	*3,363*	*4,237*	*2,147*
HH level - Total cumulative income	555	641	1301	187
(Including zeroes)	(915)	(2584)	(2088)	(1002)
	32,809	*36,986*	*46,496*	*23,110*
N	6151	1045	1845	3259

Notes: The table reports the program effect on individual and household level outcomes by program participation status of each of the partners. Column (1) reproduces the main results reported in column (1) of table 3. Column 2 reports treatment effects for individuals from households where both partners were assigned to the program. Column 3 reports treatment effects for individuals from households where only one partner was assigned to the program. Column 4 reports treatment effects for individuals from single-headed households. All regressions control for the same set of covariates reported in Table 3 and include 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Program Effect on Employment by Predicted Employment Levels

Predicted employment level	Control Group Mean (1)	Program effects by method of predicting employment: Repeated Split Sample (2)	Leave One Out (3)
Low	0.133	0.116***	0.139***
		(0.017)	(0.021)
Medium	0.333	0.084***	0.092***
		(0.024)	(0.029)
High	0.550	0.039	0.021
		(0.024)	(0.031)

Notes: The table reports the program effect on participants' selected labor market outcomes 12 months after randomization using the Abadie, Chingos, and West (2018) procedure. All regressions control for the same set of covariates reported in Table 3 and include randomization unit fixed effects. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6. Program Effect on Labor Market Outcomes from Survey Data

	Full sample (1)	Stock (2)	Flow (3)
LFP	0.071***	0.082***	0.063***
	(0.018)	(0.027)	(0.022)
	0.562	*0.568*	*0.561*
Employment	0.064***	0.12***	0.041
	(0.023)	(0.036)	(0.027)
	0.344	*0.297*	*0.353*
Full time employment	0.01	0.031	0
	(0.015)	(0.027)	(0.018)
	0.170	*0.127*	*0.179*
Hours worked	1.244*	2.717**	0.686
(zero for the unemployed)	(0.730)	(1.300)	(0.838)
	10.009	*8.317*	*10.338*
Monthly income from work	140.595	352.968**	65.019
(zero for the unemployed)	(90.194)	(156.212)	(104.676)
	1164.280	*882.613*	*1220.291*
Number of observations	3,044	828	2,216

Notes: The table reports the program effect on participants' self-reported labor market outcomes among the survey sample. All regressions control for the same set of covariates reported in Table 3 and include fixed effects for the month of survey and the randomization unit. Observations are weighted by survey weights. The number of observations refers the labor force participation variable and varies slightly due to missing values in some outcomes. Monetary values in real 2016 NIS. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7. Association Between Soft Skills and Labor Market Outcomes Based on the Control Sample

	Job search self-efficacy score (1)	Work self-efficacy score (2)	Self-efficacy score (3)	Self-esteem score (4)	Grit score (5)
Labor Force Participation	0.17***	0.128***	0.065***	0.078***	0.109***
	(0.015)	(0.013)	(0.015)	(0.026)	(0.025)
	0.562	*0.562*	*0.562*	*0.562*	*0.562*
Employment	0.065***	0.065***	0.034**	0.049*	0.065***
	(0.016)	(0.011)	(0.015)	(0.026)	(0.022)
	0.344	*0.344*	*0.344*	*0.344*	*0.344*
Full time employment	0.038***	0.031***	0.032**	0.057**	0.053***
	(0.013)	(0.009)	(0.013)	(0.024)	(0.017)
	0.170	*0.170*	*0.170*	*0.170*	*0.170*
Hours worked (zero for the unemployed)	2.235***	1.845***	1.245**	2.978***	3.037***
	(0.525)	(0.399)	(0.581)	(1.070)	(0.852)
	10.009	*10.009*	*10.009*	*10.009*	*10.009*
Monthly income from work (zero for the unemployed)	262.431***	210.214***	148.454**	245.482*	289.712**
	(70.702)	(54.129)	(71.949)	(147.882)	(115.286)
	1164.280	*1164.280*	*1164.280*	*1164.280*	*1164.280*

Notes: The table reports the association between standardized aggregate soft skills scores and self-reported labor market outcomes among the control group. Each cell reports estimates from a separate regression. All regressions control for the same set of covariates reported in Table 3 and include fixed effects for the month of survey and the randomization unit. Observations are weighted by survey weights. Monetary values in real 2016 NIS. Labor market outcomes means in italics. Standard errors clustered at the randomization unit level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8. Program Effect on Soft Skills

	Full Sample (1)	Stock (2)	Flow (3)
Job search self-efficacy score	0.059*	0.189**	0.017
	(0.035)	(0.08)	(0.036)
	2,700	*735*	*1,965*
Work self-efficacy score	0.085**	0.129*	0.062
	(0.039)	(0.069)	(0.046)
	2,708	*730*	*1,978*
Self-efficacy score	0.005	0.148*	-0.029
	(0.042)	(0.076)	(0.046)
	2,753	*737*	*2,016*
Grit score	-0.023	0.154	-0.065
	(0.042)	(0.096)	(0.047)
	831	*241*	*590*
Self-esteem score	0.059	0.231**	0.020
	(0.049)	(0.109)	(0.058)
	853	*252*	*601*

Notes: The table reports the program effect on participants' soft skills based on a set of seemingly unrelated regressions for each group. Estimates for the individual items are reported in Tables A5-A9. All regressions control for the same set of covariates reported in Table 3 and include fixed effects for the month of survey and the randomization unit. Observations are weighted by survey weights. Number of observations in italics. Standard errors clustered at the randomization unit level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 9. Long-Term Effects of the Program and Impact During the Covid-19 Crisis

	Full Sample (1)	Stock (2)	Flow (3)
Reports to IES - Jan 2020	-0.066***	-0.083***	-0.061***
	(0.011)	(0.025)	(0.013)
	0.173	*0.200*	*0.168*
Reports to IES - Feb 2020	-0.063***	-0.071***	-0.061***
	(0.010)	(0.025)	(0.011)
	0.171	*0.190*	*0.167*
Claims benefits (welfare or UI) - April 2020	-0.044***	-0.053**	-0.042***
	(0.012)	(0.024)	(0.014)
	0.330	*0.346*	*0.327*
Claims benefits (welfare or UI) - March 2021	-0.029**	-0.053*	-0.023
	(0.013)	(0.028)	(0.015)
	0.328	*0.365*	*0.321*
Number of observations	6,145	1,494	4,651
<u>Conditional on claiming benefits on March 2021:</u>			
UI (regular or furlough)	0.099***	0.098*	0.102***
	(0.026)	(0.052)	(0.030)
	0.412	*0.416*	*0.411*
Furlough	0.063***	0.087**	0.053**
	(0.020)	(0.043)	(0.023)
	0.232	*0.191*	*0.241*
Number of observations	1,951	498	1,453

Notes: The table reports the long-term effects of the program on individuals' status registered at IES. The first two entries report program effects on the probability of reporting to the employment office in January and February 2020. The following entries report program effects on the likelihood of claiming benefits on April 2020 and March 2021. The bottom part of the table reports treatment-control differences in individuals' status upon registration at the IES conditional on claiming benefits on March 2021. All regressions include the same set of controls as in Table 3 and include randomization unit fixed effects. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.