Fostering Soft Skills in Active Labor Market Programs: Evidence from a large-scale RCT *

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Abstract

The long-term unemployed sometimes lack basic soft skills needed to enter and succeed in the labor market. We examine whether it is possible to develop or enhance these skills among adults by using a large-scale randomized control trial (RCT) to evaluate the effectiveness of an Active Labor Market Program (ALMP) that targets income-support claimants in Israel. In this program, participants receive personalized treatment composed of weekly sessions with occupational trainers and motivational group workshops. We find that the program increased participants' employment rate by 8 percentage points (a 24% increase) and decreased income support recipiency by 11 percentage points (a 26% decline) relative to the control group. The effects are larger among individuals with a lower attachment to the labor market and lower likelihood of employment such as high-school dropouts and those with a longer history of welfare dependence. Income from work increased both for treated individuals and for their untreated spouses suggesting that the program had positive spillovers within the household. There is no evidence of displacement effects on the control group. The analysis of the mechanisms at work shows that the program had positive and significant effects on participants' soft skills, mainly among those with no recent employment spell, who gradually joined the labor market after participation in the program. In contrast, it induced individuals who had a recent employment spell to go back to employment soon after their allocation to the program. The program effects persist in the long run, even during the Covid-19 crisis, about five to six years after its implementation. We conclude that unemployed income-support claimants with no recent employment spells can benefit considerably from interventions that aim to improve their soft skills.

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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 are 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 et al., 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 dependence.

In this paper, we examine whether fostering welfare recipients' soft skills 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 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 randomly assigned to the program receive individual coaching and participate in therapeutic group workshops for two to seven months, receiving also job search assistance. Overall, 48,000 individuals were allocated to the program from its inception in March 2014 to December 2018. Our paper focuses on the population allocated into the treated 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 Social Security records on employment, earnings, welfare, and disability benefits together with survey data to build a comprehensive picture of the individuals before, during, and after their allocation 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 allocation to the program. The impact of the program was greater among high-school dropouts and those with lower labor-force attachment, a longer history of income-support recipiency, or self-reported health limitations. The program also had spillover effects within the household leading also to an increase in labor income of untreated spouses. 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 allocation to the treated group due to the additional burden of the program's requirements. These individuals were primarily those who registered to the employment office just before allocation to the program. Other participants, mainly those who reported to the employment office for a longer period, 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 their employment rates. Our results show that the savings on welfare transfers offset the per-participant costs within twelve months. Treatment effects persist also in the long-run: five to six years after allocation to the program and just before the onset of the Covid-19 pandemic on February 2020, treated individuals were 37% less likely to report to the employment office. Moreover, the gaps between treated and controls persist even during the Covid-19 crises.

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 is increasing over time (see recent reviews by Kluve, 2010, and Card et al., 2018; and earlier work by Greenberg, 2003; and Greenberg et al., 2005).[1] ALMPs vary not only in their target populations and the local socioeconomic conditions they face but also in their approach toward the best way to tackle unemployment. The evidence suggests substantial heterogeneity among the effects of different type of programs. Kluve (2010), for example, finds that programs that focus on counseling and monitoring, job-search assistance (JSA), and corresponding sanctions in case of noncompliance outperform programs that focus on human capital enhancing measures, private-sector-incentive schemes, and direct employment. Card et al. (2018), find that job-search assistance and sanction programs have relatively large short-term impacts whereas training and private-sector employment programs have smaller short-term impacts but larger

[1] Kluve's (2010) meta-analysis, for example, includes only nine RCTs among 137 studies reviewed. Only one-fifth of the estimates reported by Card et al. (2018), are based on experimental studies. 40 percent of the studies reviewed by Card et al. (2018) reported positive significant effects in the short-term and 61 percent did so for the longer term.

effects in the medium and longer run. In general, there is a wide consensus that public sector employment subsidies have negligible or negative impacts.

Overall, while some programs are found to be beneficial, less is known about why they work and under what circumstances. Several evaluations consider the possibility that participants in mandatory programs may immediately forgo their claims and exit welfare or unemployment in order to avoid the additional "cost" associated with the program (Black et al., 2003, Dolton and O'Neill, 2002). This mechanism may explain the larger short-term effects of "work-first" programs. Other than this, the literature is rather silent about the underlying mechanisms of successful ALMPs. Remarkably, there is limited empirical evidence on programs that focus on enhancing soft skills among the unemployed. Recent developments in the literature that stress the importance of soft skills make research on these types of programs crucial. As Crépon and van den Berg (2016) point out, many unemployed individuals have been disconnected from the labor market for long periods and lack basic traits needed to reintegrate. Traditional ALMPs may be poorly designed for such reintegration. Instead, it might be important to focus on programs that boost participants' self-esteem and other personality traits through mentoring, therapy, and group treatments, in which similarly disadvantaged individuals may stimulate 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, several studies have found that the variance of many later-life outcomes explained by soft measures sometimes rivals that explained by measures of cognitive ability (see, e.g., Heckman et al., 2006, Humphries and Kosse, 2017, and Lindqvist and Vestman, 2011).

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 Headstart and the Perry Preschool program were found to enhance soft skills and, consequently, promote higher social and economic success (Carneiro and Heckman, 2003; Kautz et al., 2014). There is scarce evidence, however, on returns to investments in soft skills later in life. A recent example of such evidence is given in a study on an intervention in Liberia among criminally engaged men (Blattman et al., 2017). The authors' findings imply that self-control and self-image are malleable in adults and that investments in enhancing these skills may mitigate crime and violence.

Additional evidence is provided by Heller et al. (2017) who find a reduction in crime among disadvantaged youths in Chicago who participated in two different behavioral cognitive therapy programs. The authors, however, did not find that the programs produced significant changes in participants' emotional intelligence, social skills, self-control or grit. Two recent labor market interventions that included some soft skills component are examined by Acevedo et al. (2020) and Groh et al. (2012). Both focus on programs targeted at a selective group of young individuals who are making their first steps in the labor market in developing countries. While these studies suggest that some soft skills can be improved, especially among women, it is unclear whether their results can be extrapolated to older individuals who have been unemployed or on welfare for a long period of time.

This study provides several contributions to the existing literature. First, it extends the literature that evaluates the effect of ALMPs on labor-market performance by examining its impacts using a clean experimental design, employing a large and heterogeneous sample, and not only analyzing standard labor-market outcomes but also examining the impact of the program on soft skills. It also provides a detailed evaluation of the dynamic effects of the intervention using administrative records on employment, earnings, and welfare recipiency before, during, and after allocation into treatment and control groups showing how the different components of the program work for different individuals. Second, the study contributes to the growing literature that examines the development of soft skills and their importance for life outcomes by showing that some of these skills are malleable later in life and have an important role in enhancing employability of low-skilled individuals. Third, we examine the impact of the intervention not only at the individual level but also at the household level, demonstrating, importantly, that the benefits of these types of programs may be larger than previously thought.[2] Finally, we show evidence on an intervention that can help low skilled individuals to cope better with negative shocks to the labor market such as the Covid-19 crises.

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 program effect on a range of outcomes from administrative datasets, examines spillover effects on non-treated spouses and

[2] The only study we found that assessed spillovers effects of ALMPs on the household is Kugler et al. (forthcoming) who detected positive spillovers of a training program in Colombia on the likelihood of attaining tertiary education among participants' relatives.

on the control group, shows dynamic treatment effects and analyzes heterogeneity of the program impact. Section VI explores the mechanisms that underlie the impact of the program, focusing on the program effect on soft skills. Section VII provides evidence on the long-term effects of the program just before and during the Covid-19 crises. Section VIII concludes.

II. Background

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, age below sixty, and, among single parents, having children older than two years of age) must report weekly (or monthly for those above age fifty) to one of seventy-five local employment offices run by the Israeli Employment Service (IES).[3] Treatment at the employment office is minimal: individuals are required to attend their local employment office every week and record their attendance 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, 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.

[3] Exempt are prisoners currently performing community service or under house arrest, ex-prisoners during the first couple of months after their release, alcohol or drugs addicts, pregnant women, women in women's shelters, caregivers of a sick household member, and supervisors of a household member under house arrest.

In February 2014, IES launched an ALMP called "Employment Circles" in fourteen of its employment offices with the purpose of integrating unemployed income-support claimants into the labor force and preventing welfare dependency and long-term 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 in accordance with 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 efficacy, work self-efficacy, and self-image; and developing a proactive work attitude.[4] The workshops and the meetings with occupational trainers also focus on imparting skills conducive to secure stable employment, for example, by simulating workday situations and instilling basic concepts of work life along with training on job search skills. Appendix 1 elaborates on program 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 between 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 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 different channels. First, the workshops and individual sessions may enhance their motivation, sense of job-search 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 attend

[4] 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 for the Israeli context by the Israeli employment incubator JDC-Tevet.

the employment office three times a week instead of once and the additional time that participants must spend there. These extra requirements raise the non-monetary costs of claiming welfare benefits. In addition, the extra attendance requirements at the employment office make it more difficult for one to work in the informal sector while declaring oneself unemployed and claiming benefits. While the program is not designed to test the contribution of each channel separately, we present below several bits of evidence that suggest that both channels are in place, affecting different groups of individuals.

Experimental Design

The program was implemented gradually using an experimental research design executed in two waves. The first wave started on 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 welfare 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.[5] 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 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 and a fraction of existing claimants in the welfare system were randomized into control and treatment groups. Randomization took place on a weekly basis separately for the incoming flow of jobseekers (i.e., new and returning claimants) and the stock of current jobseekers (the existing pool of claimants) at each employment office. 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 allocated to the

[5] The Arab population accounts for about one-fifth of Israel's population.

treatment group on a weekly basis. Treatment status was assigned at the household level. Namely, in cases where both partners attend the employment office, both were assigned to one group: treatment or control.[6] 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 during the period we analyze. This allow us to examine the effect of the program on non-treated spouses.

Upon their next visit to the employment office, treated individuals recorded their attendance 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. 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 re-registered with 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.[7] Accordingly, we estimate the average treatment effect of the program by regressing various outcomes on a treatment dummy while controlling for the randomization cell.[8] A small fraction of the treatment group (around 1%) did not receive the services of the program for various reasons ranging from administrative errors to total exemption on grounds of serious physical- or mental-health issues.[9] 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

[6] This is also the case when only one partner is registered at the employment office on the allocation date but the other partner registers a few months later. If the jobseeker's partner was already assigned to treatment, he/she is informed about assignment to the program upon the next visit.

[7] This is under the assumption that the program has no externalities to the control group. We assess this assumption in Section V and Appendix 2 below.

[8] We aggregate the randomization cell 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.

[9] 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.

identical estimates. To increase precision and to control for small differences between treated and control groups that derive from randomization in a finite sample, we augment the basic model with a vector of covariates that include individuals' demographic characteristics, employment, and 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 jobseeker *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 jobseeker *i* was assigned to treatment; γ_{jtp} is a fixed effect for the randomization cell (employment office interacted with randomization date and claimant type); 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 (employment office-randomization month-claimant type), allowing for correlation between the error terms of those who belong to the same pool and office and were randomized at the same time.[10]

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 IES, dates of assignment to treatment and control groups, and information on their weekly visits to the employment office. The database includes also the ID number of the jobseeker's spouse as recorded in the Israeli population registry.

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[10] Abadie et a. (2017) discusses clustering adjustment of standard errors. The authors note that in stratified RCTs where treatment assignment is constant within strata there is no need for adjustment. In our case, we cluster standard errors due to 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 analyzed only data of individuals randomized in the first 12-18 months of the program implementation and from 14 employment offices that participated in the pilot). From an experimental design viewpoint, due to logistical issues, treatment assignment probabilities varied across clusters. An additional justification is provided by Deeb and de Chaisemartin (2021) 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.

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 combined these data with tax records to determine monthly employment and earnings. 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 add important insights on the impact of the intervention and the mediating channels. The surveys were administered by 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.

Sample Construction

The IES operational dataset and the survey data were transferred to the NII Research Department, where they were merged with welfare and tax records hosted at NII using the unique ID number that every Israeli citizen receives upon birth or upon immigration to Israel. The datasets were anonymized and each individual (and spouse, if relevant) was assigned an internal ID number. Given the time frame of the earnings data (available for this study only until December 2015), we limited the sample to those individuals who were allocated to the treatment or control groups during 2014 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.[11] 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 IES before knowing their treatment status.

[11] 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.

Our final analysis sample includes 6,151 individuals: 3,201 in the control group and 2,950 treated. 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 block. The table also reports the F-statistic and p-value of a regression that examines whether all covariates can jointly predict treatment status within the randomization cell.

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. 36.8% 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. There are no systematic differences between the treatment and control groups.[12] Particularly important is that welfare and employment history of the groups during the three years preceding randomization is balanced. Moreover, the joint significance of all covariates is rejected, suggesting that the ignorability assumption holds, conditional on randomization cell.

V. Results

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.

[12] We find significant differences in only four out of twenty-five covariates examined. Three differences are significant at the 10% level and only one difference is significant at the 5% level. Moreover, these differences are small in economic terms and are not consistent across covariates.

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 Shekel – NIS in 2016 prices, s.e.=65.48).

Overall, program participants accumulated NIS 2,206 more in labor income twelve months after being assigned to the program than did 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 is allocated to treatment.

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 to a time horizon of at least eighteen months after randomization. The increase in employment at twelve months among 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 at least in the medium term. Concurrently, the positive gap in cumulative earnings between the treatment and the control groups and the negative gap in cumulative income-support payments continued to widen. Thus, the program continues to generate fiscal savings in the longer term.

We also estimate the main effects of the program using individual fixed effects, exploiting our ability to follow individuals before and after randomization into treatment and control groups. We do this by comparing an individual's cumulative income and months employed in the twelve months preceding randomization with the same outcome during the twelve months after randomization, between treated and control individuals. This model can be expressed as follows:

$$(2)\ Outcome_{i\tau} = \beta_1 + \beta_2\, Treatment_i + \beta_3 post_\tau + \beta_4\, Treatment_i * post_\tau + \delta_i + \varepsilon_{i\tau}$$

where $Outcome_{i\tau}$ is the outcome of jobseeker in period τ (i.e. the year preceding/following the randomization); $Treatment_i$ is the indicator for whether jobseeker *i* was assigned to treatment; $post_\tau$ denotes the post-randomization period; and δ_i are individual fixed effects.

The estimates, reported in Table 4, show that the program induced participants to work one additional month (s.e.=0.188) and earn NIS 2,366 (s.e.=916) more than non-participants during the first twelve months after their being assigned to the program. Compared with the control group, this reflects a 30% increase in employment and a 19% increase in annual labor income. The program led to a decrease of similar magnitude in annual income support (NIS -2,559), leaving total annual income unchanged. These results are reassuring because they strongly resemble the cumulative-outcome estimates reported in Table 3, further supporting the ignorability assumption.

Household-Level Results

An interesting feature of the program and our data is that we can also examine the program effect at the household level. Recall that in cases where both partners were eligible, they were jointly assigned to either the treated or the control group. Table 5 reports program effects stratifying the sample by program participation of each partner (both, only one, or single). For comparison purposes, we also report in Column (1) the program effect for the full sample. Overall, we find that the program boosts total household labor income accumulated during twelve months both in households where only one partner was treated and in those where both partners were treated. More interestingly, in two-partner households (columns 2 and 3), the increase in total accumulated household labor income exceeds that in individuals' labor income, implying that the program raises the labor income of both partners. This might be expected among households in which both partners participate in the program (Column 2) but it is an important finding for those 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, social networks for employment, and more. Although they cannot be assessed in the context of this study, they provide interesting directions for the design of additional interventions.

Externalities

In addition to its direct effects on its participants and indirect effect on their partners, the program might have potential indirect effects on non-participants. It may affect workers' behavior and options when competing with other participants in the labor market or the firms that employ them. Such externalities make take the form of displacement effects (i.e., program participants taking jobs at non-participants' expense—see, e.g., Blundell et al., 2003; Crépon et al., 2013) or general equilibrium effects through impacts on wages or vacancies (e.g., Gautier et al., 2018). Positive externalities may exist via information sharing or network effects (e.g. Bayer et al., 2008; Hellerstein et al., 2011), peer effects (Manski, 1993) or changes in employment-related social norms (Eugster et al., 2017).

We cannot test each channel individually, but we take a first step to assess whether there is any evidence of externalities. Similar to the analysis of Crépon et al., (2013), we examine whether the treatment effect is related to the share of income-support claimants assigned to treatment at each employment office in any given month and whether this share affects outcomes of the control group. We discuss the analysis in more detail in Appendix 2 and present the results in Appendix Table A16, where we show that the share treated in a given office at a given month is not related to the probability of reporting to IES for the treatment or the control group.

Dynamic Effects of the Program

We examine the impact of the program over time, by estimating its effects on a monthly basis. Figure 1a reports the share employed among the treatment and control groups and Figure 1b reports treatment vs. control differences in employment along with confidence bands from three years before random allocation to the program to twelve months after that event.[13] 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

[13] 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 cell.

because eligibility for the program was based on being unemployed.[14] 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 interesting insights on how the program works. In particular, whether the impacts of the program are driven by the additional requirement that its participants report to IES three times a week instead of one (the threat effect) or by the program's workshops. If the additional requirements push the participants to exit welfare and go to work, we would expect the participants to make an early exit to work, before receiving most of the reemployment services provided by the program, and to show non-existent or negligible exit rates several months into the program.[15] The figure on employment effects suggests that there is an immediate response to treatment in the first two months after assignment to the program but the gaps between the groups widen considerably from month 2 onwards. The treatment effect appears to stabilize around eight months after treatment, consistent with the seven-month maximum duration of the program. The dynamic effects on employment suggest that the program has immediate impacts after enrollment and further impacts after active participation.[16] We show further evidence on dynamic effects on employment in the next section where we discuss the heterogeneous effects of the program.

Figure 2 adds more evidence about the dynamic effects of the intervention by showing treatment and control means and treatment effects on the probability of attending the employment office. By design, all income support claimants attended 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

[14] Note that the employment rates do not drop to zero at the allocation date because the NII employment records refer to a calendar month while the allocation 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 will be recorded as employed on the allocation date. In practice, this creates a slight measurement error for employment spells close to the allocation 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.

[15] Participants usually start attending workshops one month after being assigned to the program.

[16] An alternative interpretation is that program participants find costlier over time to participate in the workshops and the same job offers become gradually more attractive.

(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 allocation 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 additional program attendance requirements 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, we plot in Figure 3 the share of individuals who do not attend 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 attending the employment office although they had no formal income (from income support or from work). The gap between the treatment and control group appeared around two or three months after allocation to the program and remained constant thereafter. The drop shortly after allocation 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 available to them once they have to spend several hours per week at the employment office. Some supporting evidence on this is shown in Appendix Figure A1, were we plot the relative likelihood of the characteristics of individuals who stopped attending the employment office without having any formal income (from work or social benefits) within two months after random assignment for different demographic groups.[17] 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 rate) are less likely to stop reporting 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

[17] 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 attending 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.

individuals with no recent income support spells, individuals with no recent formal employment spells, and individuals who live in areas with low local unemployment rates.

Parsing the Average Treatment Effects

The evidence presented in Tables 3 and 4 show that there is no change, on average, in total income (from work and social benefits) 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, as shown in Appendix Table A2, we see 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 allocation and twelve months after allocation to the program 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 situation of treated individuals twelve months after randomization.

Column 1 of Table A2 reports the change in income of individuals who are formally employed twelve months after their allocation to the program. These individuals experience an increase in income from all sources between the pre- and post-program period. They earn, on average, NIS 2,000 more than what they earned twelve months before allocation to the program and experienced no significant change in income-support transfers, leaving their total income NIS 2,068 higher on average.[19] In contrast, the total income of those who neither work nor receive income support twelve months after allocation 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 allocation and do not work: they experience a slight increase in total income (NIS 290) because they gain more from income support than they lose in labor income.

These descriptive statistics suggest that the zero effect of the program on total registered income hides differential effects among individuals. To parse the average treatment effects, we estimate unconditional quantile treatment effects on total income (from work and from income support) report

[18] We focus on twelve months before program allocation instead of the months just before allocation in order to avoid a pre-program period that is inherently related to the negative shock that program participants experienced that made them eligible to the program.
[19] Note that some employed individuals continue to receive income support in the form of an income supplement (provided their labor income is below a certain threshold).

them in Figures 4.[20] The program does not affect the total income of those at the bottom of the income distribution, who report no income from any source according to the NII records. As noted above, the program induced some individuals to stop reporting to the employment office (and, accordingly, forgoing income support) without obtaining formal employment (an effect of 7 percentage points). As a result, we see a negative treatment effect in the total income of individuals in the 40–50-quantile of total income distribution. A positive treatment effect on total income is observed among individuals in income-distribution quantiles 65–75. Treatment effects on the earnings distribution are plotted in Figure 5. There are no differences for the lowest quantiles given that 59 percent of the treatment group do not work. We see positive treatment effects of the program for individuals located between quantiles 65 through 80 of the earnings distribution.

Heterogeneous Effects

We also examine heterogeneous treatment effects by individuals' socio-demographic characteristics and pre-program labor-market attachment and welfare dependence. Figure 6 presents the estimated treatment effects on employment for different subgroups along with their confidence band. Sample sizes for each subsample are reported in square brackets. Appendix Table A3 reports estimates of all outcomes for these subsamples. The program increased employment and reduced welfare dependence among almost all groups but had a larger effect (both in absolute terms and relative to the outcome mean of the control group) on some subsamples than others, e.g., a larger increase in employment among women than among men—8 percentage points (29%) vs. 6 percentage points (16%), respectively. The program was also highly effective among the Arab population, boosting its employment rates by 14 percentage points (an increase of 62%). Positive effects are also observed among the Ultra-Orthodox: the estimate for employment is 0.065 (s.e.=0.044), implying a 16% increase, although the sample is too small to provide a precise estimate. We do observe a positive and significant impact for this population on the number of months worked during the twelve months after allocation to the program: Ultra-Orthodox participants worked, on average, one more month than did non-participants during that time, implying a 29% increase.

[20] We estimate unconditional quantile treatment effects as developed by Firpo, Fortin, and Lemieux (2009), controlling for randomization cell by applying the algorithm developed by Borgen (2016). Note that this method does not identify the distribution of treatment effects but rather provides estimates for treatment effects on income distribution.

The program is also highly effective among high-school dropouts and those aged thirty-five or older, increasing the employment rate of both groups by 11 percentage points, implying a 40% improvement. Interestingly, the program has a large impact on those who report health limitations when they register with the employment office, i.e., those who do not receive disability benefits but report to IES upon registration that they have health limitations that impede them from working. Twelve months after randomization, the employment rate of the treated group was 14 percentage points higher than the 24% rate among the control group. The program also raised the monthly income (from work and welfare transfers) of this treated group by NIS 190, which is also reflected in an increase of almost NIS 2,000 (11%) in total income accumulated in the twelve months after randomization. The effect of the program on the employment rate of those with no self-reported health limitations was also significant but smaller: 5 percentage points relative to a control mean of 37%.

Two additional groups highly affected by the program are those who have no employment spells in the twenty-four months before randomization into the program and those already on welfare during that period.[21] The program boosted the employment rate of those in the former group by 9 percentage points (relative to a 17% employment rate in the control group) and of those in the latter group by 11 percentage points (relative to 28% in the control group). We obtain a similar pattern when stratifying the sample according to claimant type (stock versus flow). The increase in employment for the stock subsample (existing claimants at time of randomization) is 14 percentage points as opposed to an increase of 6 percentage points for the flow subsample (new or re-registering claimants). Altogether, the different stratifications show that the program had a larger impact among individuals who were less attached to the labor market and did not have recent employment spells.

We also examine the heterogeneous effects of the program by local unemployment rates. We define low (<7.5%) and high (>=7.5%) unemployment rates relative to the median local unemployment rate (7.5%) across all employment offices participating in the program in 2012, before the program was launched.[22] Consistent with previous studies (e.g. Card et al., 2018), the effect of the program on participants reporting to offices in high-unemployment areas was larger both in absolute terms and

[21] These two groups do not completely overlap. Roughly 40 percent of individuals who have no employment spells during this two-year period receive no income support benefits at the time.

[22] The median unemployment rate across all locations of employment offices countrywide is identical to that in the localities of the employment offices analyzed in the sample. The average unemployment rate in Israel during this period (2012) was 6.9%. The interpretation of the results stratified by local unemployment rate should be viewed with caution because we cannot determine whether the larger program impact in high-unemployment areas traces to specific characteristics of welfare claimants, program administrators, or other conditions in these areas.

relative to the control mean. Twelve months after randomization, the employment rate of the control group reporting to offices in low-unemployment areas was 42% while that of the control group reporting to offices in high-unemployment areas was only 28%. The program leads to a 10 percentage-point (34%) increase in employment in high-unemployment areas and a 6 percentage-point (13%) upturn in low-unemployment areas. Similarly, income-support recipiency decreased by 13 percentage points (29%) and 6 percentage points (19%) in high-unemployment and low-unemployment areas respectively.

We also take a complementary approach to examine the heterogeneous effects of the program by applying endogenous stratification (Abadie et al., 2018) to look at heterogeneity in treatment effects on three different outcomes, all measured 12 months after randomization: employment, the likelihood of reporting to the employment office, and the likelihood of not reporting to the employment office while having no formal income (from work or social benefits).[23] Consistent with our previous findings, results reported in Table 6 show that the program had the largest impact on employment among individuals with the lowest chances to be employed. Likewise, it reduced the chances to continue reporting the employment office among those who had the higher probabilities of reporting to the employment office. There is no clear pattern of the program effects when we stratify the sample by the chances to stop reporting to the employment office without having any formal income.

Given the large differences in the program effect by prior labor force attachment, we provide a last piece of evidence on the heterogeneous effects of the program by plotting in Figure 7 the dynamic effects on employment stratifying the sample by claimant type: stock versus flow. We observe a very different pattern for the two groups: employment rates of the stock subsample (subfigure a) increase constantly over the whole period after assignment to treatment while for the flow subsample (subfigure b), the increase in employment takes place mainly in the first months after assignment. This figure reveals that the threat effect (i.e. the extra requirements of the program) is the main force behind the employment effect for the flow subsample whereas for the stock subsample the employment effect appears to follow workshops' participation. We expand on this point below where we examine the mechanisms of the program effects.

[23] Following Abadie et al. (2018) procedure, we use all covariates and the outcome in the control group to predict each potential outcome if untreated for each individual in the treated group. We then stratified 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.

VI. Assessing the Mechanisms

We present in this section the analysis of the survey data 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.[24] Treated 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 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.[25] Roughly two-thirds of the observations came from the first survey and the rest from the second.[26]

We examine whether there is differential selection into the survey by treatment status by estimating a linear probability model that estimates the probability of response as a function of personal characteristics and a treatment dummy, controlling for the randomization cell. Results reported in Column 1 of Appendix Table A4, suggest survey response is associated with individuals' characteristics. Namely, the probability of response is higher for individuals with self-reported health limitations, at least twelve years of schooling, income-support recipiency before random assignment, Ultra-Orthodox Jewish identity, and Israeli born. Nevertheless, treatment status is not associated with the probability of responding to the survey. In Column 2, we test for differential selection of treated individuals by personal characteristics by also including interactions between all covariates and the treatment dummy. Only two of the twenty-two treatment indicators are statistically significant. Specifically, we find a negative coefficient only for the interaction of treatment with health limitation and a positive coefficient for the interaction between treatment and Arab indicators. Overall, despite these small imbalances, we

[24] 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 to thirty-four months after random assignment. The vast majority (86 percent) were polled at least six months after randomization. The average time was fifteen months and the median ten months.

[25] 567 individuals participated in both surveys.

[26] The second survey wave was larger, comprising 1,854 additional individuals who were randomized into treatment and control groups from January 2015 to 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 duration of at least twelve months after random assignment.

do not observe a consistent picture of differential selection into the survey in accordance with treatment status.

To analyze the data yielded by the survey respondents, we construct survey weights to account for nonresponse in order to reflect the characteristics of the entire research population. We estimate a logistic regression model that predicts the likelihood of survey response as a function of treatment assignment, individual characteristics, the interaction between the two, and randomization cell fixed effects (the estimates are reported in Appendix Table A5). Each observation is then weighted by the inverse of the predicted response probability, except for observations of individuals surveyed in both survey waves, which we reweight by half of their assigned weight. In Appendix Table A6, we report the results of a balancing test for the reweighted survey sample, which shows that there are no significant differences between the treatment and comparison groups, both in terms of observable individual characteristics and in the time passed between random assignment to the survey date.[27] This table also shows that the average characteristics of the survey sample are virtually identical to those of the full sample reported in Table 2. Furthermore, we are able to replicate our main results in administrative outcomes obtained for the full sample using the reweighted survey sample (see Appendix Table A7). This is important because it strengths our confidence in using the survey sample to draw conclusions about the effects of the program for the full population.

Survey results

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. In particular, we can assess whether the program also affected labor-force participation (by including active job search) and examine hours worked. We estimate the same model as in our main analysis, controlling for survey date. Table 7 displays the program treatment effect on labor-force participation, 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. Thus, it not only boosted employment but also raised the share of individuals who are actively searching for jobs. We also find

[27] There may still be a systematic correlation between unobservables and the propensity to be included in the sample. We cannot entirely rule out this possibility, even though the lack of differences in the observables hints that the presence of a strong correlation in the unobservables is very unlikely, especially if these unobservables are correlated with the observed covariates.

that the effects are larger for the stock subsample than for the flow, 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.[28] The estimated program effects on the total number of weekly hours 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 treated group.[29]

Soft skills: Having shown that the program improved labor-market outcomes and reduced income-support recipiency, we now examine whether the program affected participants' soft skills. We note that we present here evidence on a limited number of soft skills that we measure in the follow up surveys, because we cannot test every possible mediator. In addition, we cannot individually manipulate each of the skills and assess their effects on labor market outcomes. Nevertheless, we provide important and novel evidence on skills that are affected by the program.

The survey included a series of questions designed to assess individuals' soft skills and self-perception. These questions were grouped in five modules containing thirty-four items in total. For each individual item, participants were asked to specify the extent to which they agree with various statements 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 individual's confidence in his/her ability to successfully search for a job and perform specific job-search tasks.[30] The second module examines work self-efficacy, with which workers' confidence in managing workplace situations such as respecting schedules and collaborating with colleagues is assessed. The third module examines general self-efficacy, which assesses a person'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 individuals' 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.

[28] The estimate for full time employment for the stock subsample is positive but very imprecisely measured.
[29] If we assume 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 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*23.12*93). This estimate is virtually identical to the estimate we obtain: NIS 141.
[30] Job search self-efficacy can be affected by learned skills and self-perception.

The survey questions in each module and their sources are set forth in Appendix 3. To facilitate the interpretation of the results, we reverse the scale of the items so that a higher value denotes a better score and transform each of the items and the aggregate indices into z-scores. In Appendix Table A8, we report the inter-item correlations and Cronbach's Alpha reliability coefficients for the different modules and in Appendix Table A9 we present the correlations among 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).[31]

We start by examining the association between these 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.[32] For this purpose, we regress each of the survey labor-market outcomes (labor-force participation, employment, full-time employment, weekly hours worked, and labor earnings) on the mean standardized scores of each of the five modules while controlling for individual characteristics. The results (Table 8) show that all skills are positively correlated with better labor-market outcomes.

We then examine the effect of the program on these skills by plotting in Figures 8-12 the cumulative distributions (CDFs) of these skills for the treatment and control groups along p-values for Mann-Whitney tests of stochastic dominance.[33] Given the stark differences we found in the dynamic effects of the program on employment for the stock and the flow subsample, we plot the CDFs for the whole sample and then separately for the stock and the flow subsample. Focusing on the full sample, we see that the CDFs of the treatment group for job-search 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 Mann-Whitney tests that reject the null hypothesis for equality of distributions between the treated and control groups. In contrast, no significant differences emerge between the CDFs of the treatment and control groups for grit or general

[31] 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.

[32] Conducting an equivalent exercise using the available administrative labor-market outcomes, we found a similar pattern (results not shown).

[33] To compare the distributions, we use residualized z-scores that we obtain by regressing each z-score on the vector of individual's characteristics. To account for the randomization block 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 treated in the randomization block). We then adjust the weights for those surveyed twice by dividing by two, trim weights to the 90^{th} 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.

self-efficacy. The stratification by claimant type plotted in subfigures (b) and (c) reveals that the improvement in soft skills arise almost exclusively from the stock subsample. For this subsample, we observe that the CDFs of all soft skills of treated individuals dominate CDFs of the controls. In contrast, for the flow subsample there is only a small shift for self-esteem while none of the p-values for the Mann-Whitney tests are significant.

Reported next are regression coefficients of average treatment effects for each category, based on a system of seemingly unrelated regressions based on equation (1) that treat the items in each category as a family of outcomes (Table 9). This method takes into account that the outcomes in each category are correlated by allowing for individual-level correlation of the error terms across equations (see Kling et al., 2007).[34] The effects on each individual item are presented in Appendix Tables A10-A14. 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 8-12, we see a significant and positive effect of the program on its participants' soft skills for the stock subsample. For this group, treatment effect estimates show an improvement in self-reported job-search efficacy (19%), work self-efficacy (13%), general self-efficacy (15%), grit (15%), and self-esteem (23%). In contrast, estimates for the flow subsample, are small, have inconsistent signs across outcomes and are not significant.

The findings on soft skills and the dynamics of employment effects for the stock and the 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 to the employment office, are affected by the threat effect of the program and return to work relatively fast without benefiting from the workshops. In contrast, the stock subsample, who joined the program while being on welfare and after a longer disconnection from the labor market, improved their soft skills and enhanced their employment rates.

A relevant question is whether the improvement in soft skills observed among the treatment group is a direct result of the workshops, which in turn, enhanced participants' labor market outcomes or whether the causal chain between employment and soft skills runs in the opposite direction. Namely, the

[34] That is, we define the average treatment effect for category 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 category c, π_{kc} is the effect on outcome *k* included in category 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.

program increased employment rates through its threat effect and the improvement in participants' soft skills stems from their employment. While we cannot completely rule out this alternative interpretation, we note that we observe an improvement in soft skills only among the stock group whose employment rates started to increase more gradually. In contrast, the flow group, who shows a faster increase in employment rates that takes place almost immediately after allocation to the program, probably due to the threat effect, does not experience any increase in soft skills.

Following the causal channel hypothesis of an increase in soft skills that led to an increase in employment among the stock subsample, we can perform a simple back of the envelope calculation combining estimates from the program effect of on soft skills from column (2) of Table 9 and the associations between soft stills and employment from row (2) of Table 8. This calculation shows that the improvement in soft skills of the stock subsample can explain 39% of their 12 percentage points increase in employment based on the survey results (reported in column 2 of Table 7) or 34% percent of their 13.8 percentage points increase in employment based on the administrative data (see Table A3c).[35] Note, that this calculation should be taken with extra 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 Crises

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 to report to the employment office five to six years after randomization and the status of the treated and control groups during the first year of the COVID-19 crisis. Using the same specification as in equation (1) we report program effects and outcome means of the control group in Table 10. As opposed to the previous results, we report the status of the individuals measured at a specific calendar date and not as a function of months since randomization. We begin by reporting in the first two entries of the table, treatment effects on the share of individuals reporting to

[35] 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: 0.189x0.065+0.129x0.065+0.148x0.034+0.154x0.065+0.231x0.049=0.047.

IES on January and February 2020 (just before the onset of the Covid-19 Pandemic in Israel).[36] Estimates show that that the program effect persist also after five-six years. Treated individuals are 14 percent (or 6.6. percentage points) less likely to report to the employment office relative to the control group on January 2020. As before, we find larger gaps among the stock subsample relative 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.

On March 11th 2020, Israel began enforcing social distancing and on March 19th, a national state of emergency was declared posing several restrictions on citizens' movement with further restrictions imposed on March 25th. All non-critical 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.[37] All workers over 20 who were laid off (either temporarily or permanently) and completed a qualifying period of six months of work during the last 18 months preceding the day of their registration with the Employment Service, were eligible to claim unemployment benefits. They had to register online at the Israeli Employment Service and at the National Insurance Institute of Israel. Due to social distancing and lockdown measures, both unemployment and welfare benefits were paid to eligible claimants without the requirement to attend the employment office. In addition, UI eligibility period was extended and other eligibility requirements were either lifted or relaxed.[38] We report in the third cell of the table, the long-term effect of the program on the probability of claiming benefits (either welfare or unemployment) on April 2020, the month with the highest number of registered individuals at the IES from February 2020 to August 2021 (roughly 1.13 million individuals) . As seen in the table, the share of individuals claiming benefits almost doubled from February 2000 to April 2000 among the control group, increasing from 0.171 to 0.330. Nevertheless, the increase was less dramatic among the treated group. Overall, the gap in the share of individuals claiming benefits between treated and control groups narrowed during the onset of the Covid-19 crises, but the share was still lower for the treated group relative to the control group – a gap of 13% (or 4.4. percentage points). Differences are again more pronounced for the stock rather than the flow subsample (15% versus 13%). The fourth cell of the

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

[37] See https://www.gov.il/he/departments/policies/dec4994_2020 for the lockdown regulation in Hebrew and box 1.1 on page 10 at OECD (2020) for an English summary of the main policies.

[38] On July 7th 2020 it was announced that payment of unemployment benefits will be extended until July 1st 2021. On February 2021, it was decided to provide a further extension until December 2021 for individuals aged 45 and above. See Gal and Madhala (2020) for more information on the changes in the Israeli unemployment insurance and welfare program during the Covid-19 crisis.

table reports differences between groups on March 2021, just at the end of the third wave of the pandemic and the end of the third lockdown. The gaps between groups persisted, especially among the stock subsample. At the bottom part of the table, we focus on those individuals who were still claiming benefits on March 2021, and examine controlled differences between treated and control groups in their status upon registration. 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.[39] First, we note that conditional on claiming benefits, treated individuals were more likely to claim unemployment benefits rather than welfare benefits. Moreover, they were more likely to register as being on furlough. Both estimates suggest that conditional on claiming benefits, treated individuals appear more attached to the labor market (as being unemployed or on furlough) relative to individuals in the control group. Overall, evidence reported here clearly suggests that the program effects not only persisted in the long-term but they are also evident during the Covid-19 crises. Moreover, it seems that the largest benefits of the program during the Covid-19 crises appear among the stock subsample, who experienced a significant improvement in soft skills.

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 study examines the impact of an active labor-market program implemented in Israel that focuses on enhancing welfare recipients' soft skills in order to prepare them for successful immersion in 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 works.

The results show that the program had positive and significant effects on labor-force participation, employment rates, and labor income. We also find a significant negative effect on income-support recipiency and, correspondingly, on the size of income-support payments received by those assigned to the program, with no evidence of substitution with alternative benefits (e.g., disability). The cost of the program per participant is more than outweighed by savings on government welfare transfers within twelve months. Interestingly, the program had also positive spillovers within the household increasing

[39] There were no differences in unemployment benefits paid to individuals who resigned or reported to be dismissed or on furlough.

not only labor income of treated individuals but also labor income of their spouses. We find no evidence of spillover effects among 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 longer history in the welfare system, fewer than twelve years of schooling, self-reported health limitations, and individuals who were already on welfare when allocated to 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 allocated to the program (the stock subsample), show a gradual increase in employment that is consistent with workshops' participation.

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, as such, appear to mediate part of the impact of the program on employment. Our study shows that it is possible to enhance 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 all the more attractive. Moreover, the benefits of the program are still evident in the long term, five to six years after its implementation and even persist during the Covid-19 crises, providing evidence on an effective intervention that helped disadvantaged groups to better cope with adverse labor market shocks.

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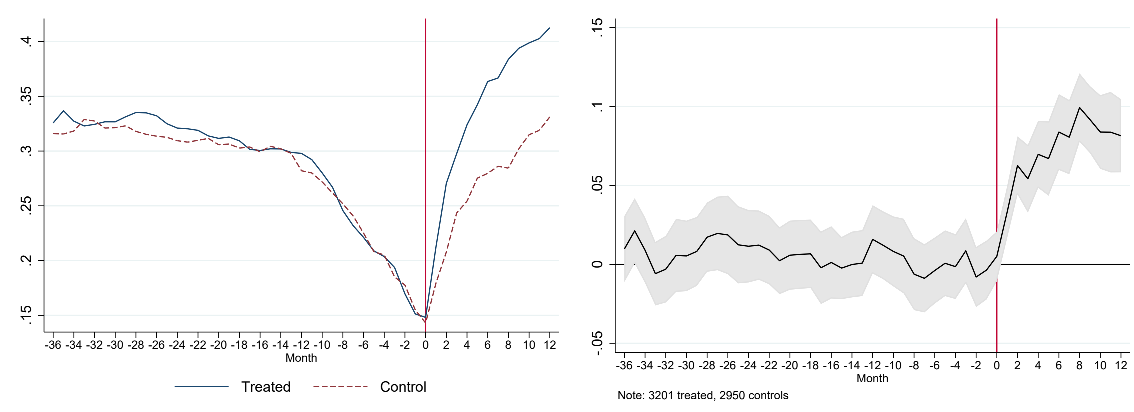
Figure 1: Dynamic effects - employment

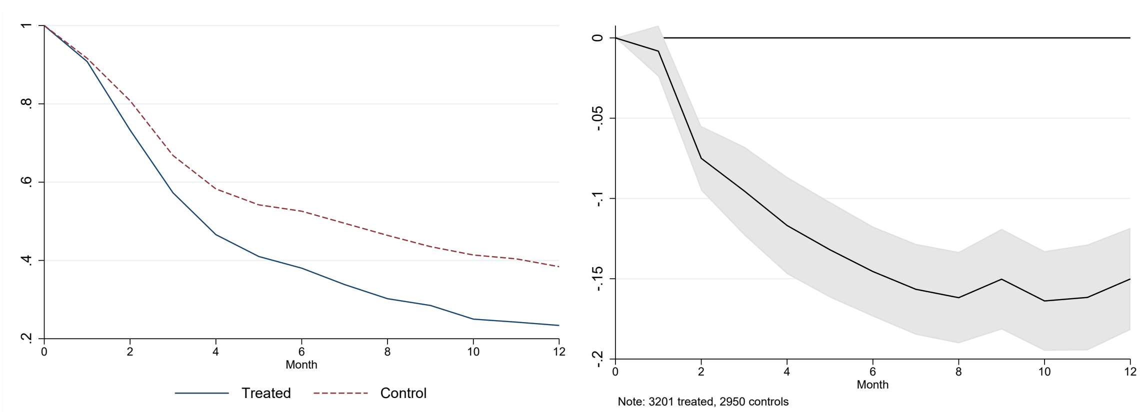


(a) Levels

(b) Treatment-Control

Notes: The figure reports employment rates for the treated and the control groups (left panel), and the difference in employment rates between the treated and control groups along with a 90 percent confidence interval (right panel), over time. Month zero corresponds the month of random assignment.

Figure 2: Dynamic effects - share reporting to employment office

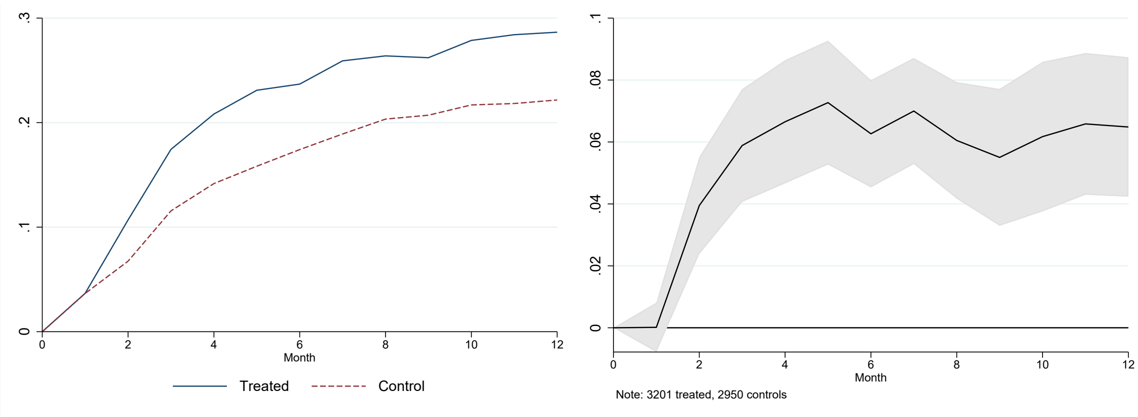


(a) Levels

(b) Treatment-Control

Notes: The figure reports the share reporting to the employment office among the treated and the control groups (left panel), and the difference in reporting rates between the treated and control groups along with a 90 percent confidence interval (right panel), over time. Month zero corresponds the month of random assignment.

Figure 3: Dynamic effects - share not employed, not reporting to employment office and not recieving income support

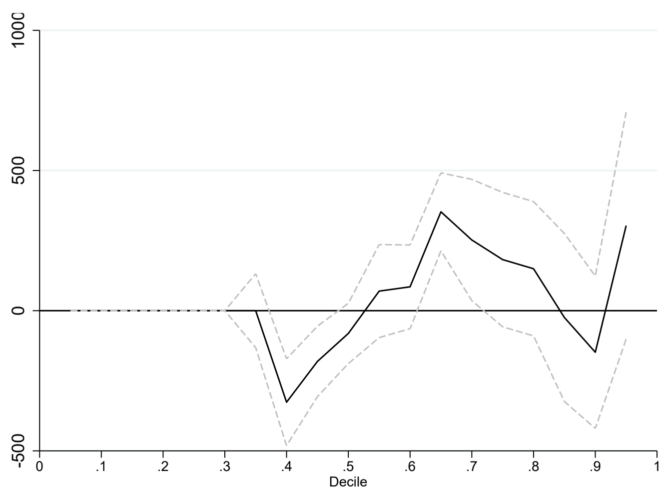


(a) Levels

(b) Treatment-Control

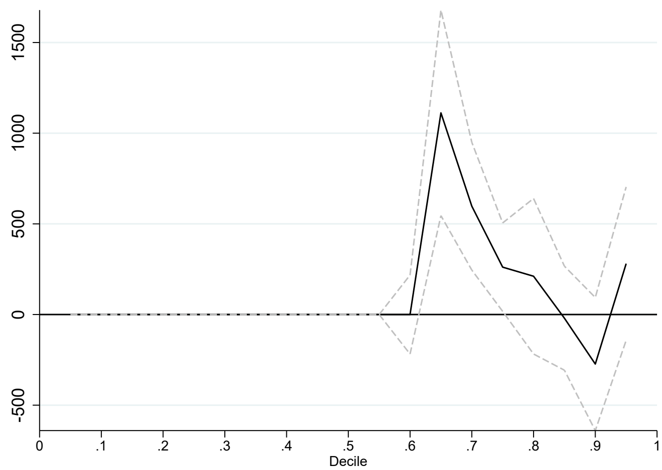
Notes: The figure reports the probability of not reporting to the employment office while not working nor receiving income support benefits for the treated and control groups (left panel) and the difference in this share between both groups with a 90 percent confidence interval (right panel), over time. Month zero corresponds the month of random assignment.

Figure 4: Quantile treatment effects on the distribution of total income



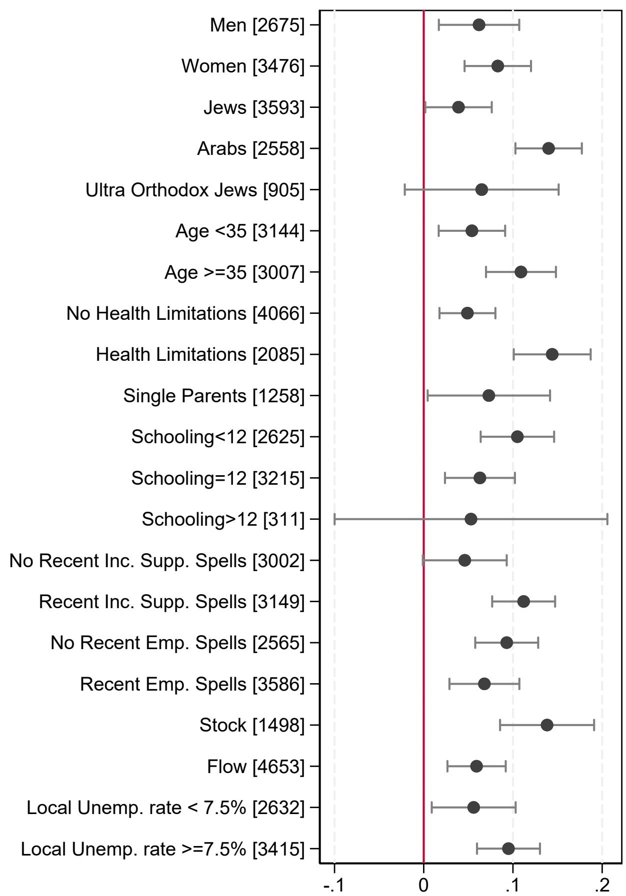
Notes: The figure reports the program effect for each ventile of the total income (i.e labor earnings and income support) distribution 12 months after random assignment with a 90 percent confidence interval.

Figure 5: Quantile treatment effects on the earnings distribution



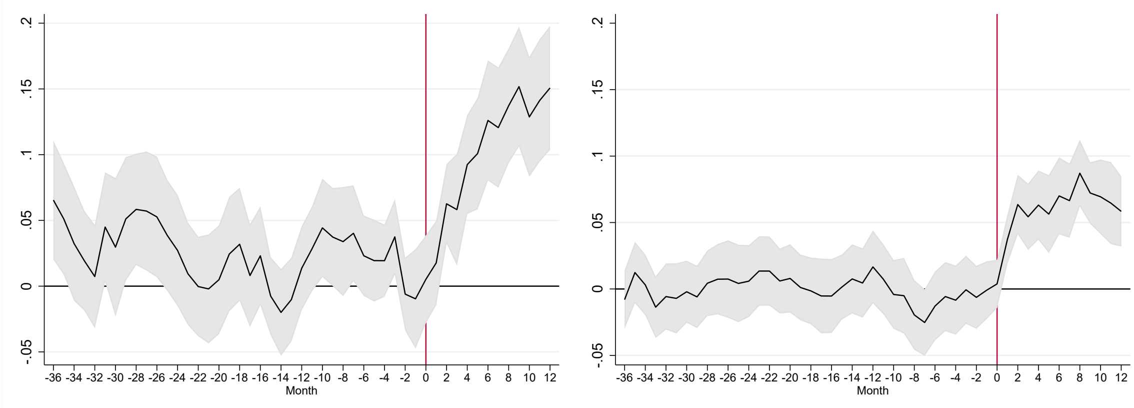
Notes: The figure reports the program effect for each ventile of the labor earnings distribution 12 months after random assignment with a 90 percent confidence interval.

Figure 6: Heterogeneous Employment Effects of the Program



Notes: The figure reports the program's impact on employment across different subpopulations with 95 percent confidence intervals. Number of observations are reported in brackets.

Figure 7: Dynamic effects - employment by claimant type

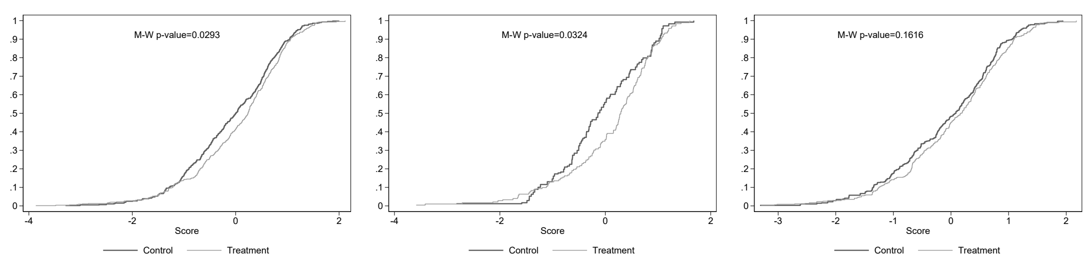


(a) Stock

(b) Flow

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 allocation to the program. Month zero corresponds to month of random assignment.

Figure 8: Program Effect on Self-Esteem



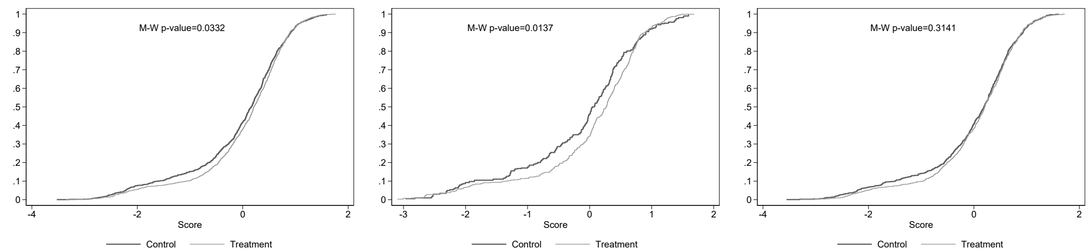
(a) All

(b) Stock

(c) Flow

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 allocation to the program. Reported p-values refer to the results of the Mann-Whitney tests of stochastic dominance.

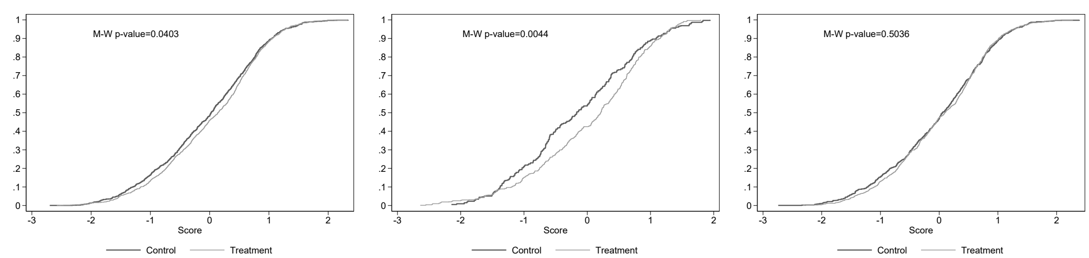
Figure 9: Program Effect on Work Self-Efficacy



(a) All (b) Stock (c) Flow

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 allocation to the program. Reported p-values refer to the results of the Mann-Whitney tests of stochastic dominance.

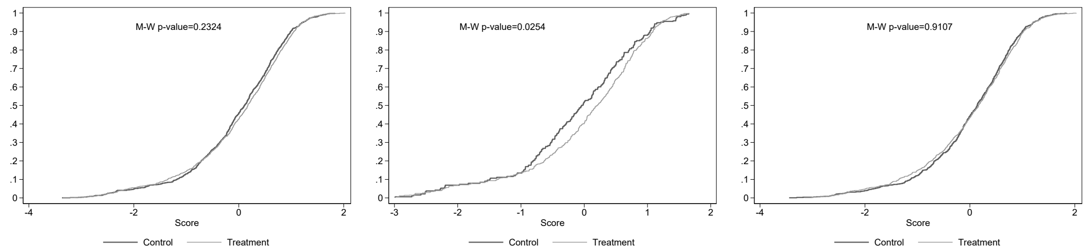
Figure 10: Program Effect on Job-Search Self-Efficacy



(a) All (b) Stock (c) Flow

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 allocation to the program. Reported p-values refer to the results of the Mann-Whitney tests of stochastic dominance.

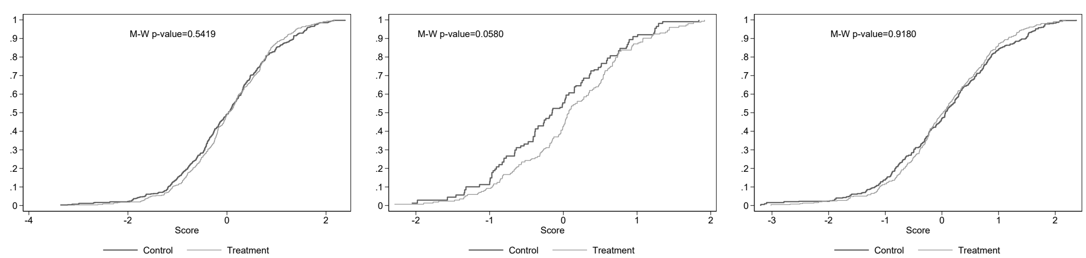
Figure 11: Program Effect on Self-Efficacy



(a) All (b) Stock (c) Flow

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 allocation to the program. Reported p-values refer to the results of the Mann-Whitney tests of stochastic dominance.

Figure 12: Program Effect on Grit



(a) All (b) Stock (c) Flow

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 allocation to the program. Reported p-values refer to the results of the Mann-Whitney tests of stochastic dominance.

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

	Employment offices in the RCT (1)	All other employment offices (2)
Number of active job-seekers	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

	Treated (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.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3. Program Effect 12 and 18 Months After Randomization

	12 months horizon sample	18 months horizon sample	
	Impact after 12 months	Impact after 12 months	Impact after 18 months
	(1)	(2)	(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	1498	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 < 0.05, ** p < 0.01, *** p < 0.001.

Table 4. Program Effects from Individual Fixed Effects Model:
12 Months After Randomization - 12 Months Before Randomization

	Total months employed (1)	Cumulative income from work (2)	Cumulative income support (3)	Total cumulative income (4)
Post	0.53***	2716***	3711***	6427***
	(0.533)	(756)	(299)	(780)
Treatment * Post	1.003***	2366***	-2591***	-224
	(0.188)	(912)	(386)	(969)
Constant	2.783***	9673***	5541***	15214***
	(0.057)	(261)	(136)	(268)
N	12,302	12,302	12,302	12,302

Notes: The table reports the program effect on participants' cumulative outcomes while controlling for individual fixed effects. The sample includes two observations per individual: one measurement for cumulative outcomes for the year that preceded randomization and the second measurement for cumulative outcomes for the twelve months post-randomization. Monetary values in real 2016 NIS. Standard errors clustered at the randomization unit level in parentheses.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 5. Program Effects at the Individual and Household Level

	All	Both spouses assigned	Only one spouse assigned	Singles
	(1)	(2)	(3)	(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 UI or other)	-0.009	0.006	0.007	-0.02
	(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 allocated to the program. Column 3 reports treatment effects for individuals from households where only one partner was allocated 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.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 6: Program Effect by Predicted Risk Levels

Predicted risk level:	Low			Medium			High		
	Control Group Mean (1)	Repeated Split Sample (2)	Leave One Out (3)	Control Group Mean (4)	Repeated Split Sample (5)	Leave One Out (6)	Control Group Mean (7)	Repeated Split Sample (8)	Leave One Out (9)
Employed	0.133	0.116***	0.139***	0.333	0.084***	0.092***	0.550	0.039	0.021
		(0.017)	(0.021)		(0.024)	(0.029)		(0.024)	(0.031)
Reporting to employment office	0.202	-0.064***	-0.039*	0.339	-0.131***	-0.135***	0.610	-0.261***	-0.277***
		(0.019)	(0.022)		(0.019)	(0.023)		(0.021)	(0.025)
Does not work\receives Income support\reports to employment office	0.119	0.065***	0.081***	0.215	0.081***	0.083***	0.314	0.061**	0.051*
		(0.017)	(0.021)		(0.018)	(0.026)		(0.024)	(0.027)

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 randomization unit fixed effects. Standard errors in parentheses.* p < 0.05, ** p < 0.01, *** p < 0.001.

Table 7. 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 randomization unit fixed effects. 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 < 0.05, ** p < 0.01, *** p < 0.001.

Table 8. 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 Paricipation	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	2.235***	1.845***	1.245**	2.978***	3.037***
(zero for the unemployed)	(0.525)	(0.399)	(0.581)	(1.070)	(0.852)
	10.009	*10.009*	*10.009*	*10.009*	*10.009*
Monthly income from work	262.431***	210.214***	148.454**	245.482*	289.712**
(zero for the unemployed)	(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. 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. 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 < 0.05, ** p < 0.01, *** p < 0.001.

Table 9. Program Effect on Soft Skills

	Full Sample	Stock	Flow
	(1)	(2)	(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 A11-A15. All regressions control for the same set of covariates reported in Table 3 and include also survey month and randomization unit fixed effects. Observations are weighted by survey weights. Number of observations in italics. Standard errors clustered at the randomization unit level in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 10. Long-Term Effects of the Program and Impact During the Covid-19 Crises

	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
Conditional on claiming benefits on March 2021:			
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 on 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 conditional on claiming benefits on March 2021. All regressions include the same set of controls as in Table 3. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001.

Appendix

Appendix 1: Program Details

Employment Circles is an Active Labor Market Program that aims to re-integrate chronically unemployed income-support claimants into the labor force by providing them with personalized treatment composed of various occupational workshops.

After being assigned to the program, participants start with an initial intake meeting where their ability and motivation to go back to work are assessed. Intake comprises two individualized meetings with an occupational trainer who diagnoses the participant in terms of employability, level of motivation, and barriers to employment, and makes a recommendation for a specific program track based on this diagnoses. A final decision is then made by the head of the employment office and the caseworker for program participants in the office. The personalized program track is composed of weekly meetings with the caseworker and a combination of some or all of the following four workshops:

- *Purpose-focused preparatory workshop*
  Designed to prepare relatively low and medium motivated individuals for the job-search phase or the personal-skills workshop, focusing on improving job-search motivation and boost self-esteem and self-efficacy. Main objectives: increase participant's motivation, identify his/her strengths, and foster his/her career self-image and belief in work capacity. Consists of group sessions and personal meetings, four hours a week (two hours twice a week) for three weeks. Group sessions are devoted to identifying each participant's strengths and skills, familiarizing him/her with different types of work environments, and setting career aspirations and employment goals.
- *Job-placement-focused preparatory workshop*
  Designed to prepare medium or relatively high motivated individuals and those who finished the *Purpose-focused preparatory workshop* for the job-search phase while focusing on providing job-seeking skills. Consists of group sessions and two-hour personal meetings held twice a week for three weeks. Content includes fostering self-introduction skills, acquiring job-search skills with emphasis on entry-level jobs, writing a résumé, and job-interview and assessment-center simulations. At the meetings, each participant defines a set of entry-level jobs and builds a program to achieve the job search goals.
- *Personal-skills workshop*
  An intensive workshop designed to build a career path and foster self-motivation, work self-efficacy, and interpersonal skills of program participants with low-to-medium job readiness. Consists of group sessions and personal meetings, ten hours per week (five hours twice per week) for 6 weeks. Content includes vocational guidance, positive self-talk, conflict resolution, dealing with personal obstacles and new tasks, better handling of feedback, and fostering excellence on the job. The workshop puts a special emphasis on the group dynamics in order to build social support and push participants to progress together as a group.
- *Job-search workshop and group coaching*
  Supervised pro-active job search in a computer lab, four hours per week for up to four months. Participants are encouraged to search for suitable entry-level jobs that match their capabilities and aspirations and have a future growth trajectory, all in accordance with the participant's

personal job-search program and goals. The meetings include both group and individualized coaching to provide feedback and group support in the job-search process.

Program participants must report to the labor office three times per week: twice for workshop participation and once for an individual meeting with their caseworker. The workshops are conducted by qualified occupational trainers and coaches that provide each participant with the personal attention needed to identify and remove the obstacles that stand in the way of his/her success in the workplace.

Appendix 2. Externalities

To examine the program externalities, we test whether the share of income support claimants assigned to the program in a given office and month is associated with outcomes for the treated or the control group. We have information only on treated and control individuals, so we cannot assess the effects on individuals outside this sample. Still, we think that given the focus of the program on individuals who receive welfare benefits, the most relevant group that may be affected are other welfare recipients because they have similar skills, earnings, and employment potential. In addition, given the small size of the treated population relative to the size of the labor market, we assume that the likelihood of general equilibrium effects of the program on the labor market even at the local level is rather low.

For this analysis, we expand the sample to include jobseekers who were randomized into the program between January 2015 and February 2016 and focus on the effect of the program on the probability of reporting to IES twelve months after randomization. We select this larger sample in order to obtain greater variation in the proportion of treated individuals within employment offices over time and to increase power (increasing the chances of detecting externalities in case they exist). The sample expansion leads us to focus on the probability of reporting to IES as the main outcome of interest because data on this outcome are available to us over a longer time horizon (as opposed to employment and welfare transfers, which are available only up to 2015).[1] We restrict the sample to jobseekers who were randomized from the incoming flow of claimants and define the fraction of job seekers assigned to treatment as the share of treated individuals in the monthly incoming flow of income support claimants at each employment office.[2] The share of monthly treated individuals varies considerably across employment offices and over time due to regular fluctuations in the incoming flow of claimants and the capacity of the program at the employment office. Appendix Figure A2 presents the overall distribution of the share of treated individuals across offices and time. A variance decomposition analysis indicates that within-office variation accounts for nearly 80% of total variation. The residual variation in the monthly share of treated individuals, controlling for employment office and month fixed effects is shown in

[1] Any effect on employment is expected also to be reflected in the probability of reporting to IES. Thus, the absence of an effect on the probability of reporting to IES is a good indicator of the lack of an effect on employment.

[2] In principle, we could have focused on the share of treated individuals in the same locality of residence rather than the locality of the employment office attended. However, given that many job seekers reside in relatively small localities and that the catchment areas of employment offices largely overlap with local labor markets, we prefer to focus on the latter definition. In addition, we defined the share treated based on the monthly incoming flow of welfare claimants because it is clearly defined unlike the share treated among the welfare stock. Our results are robust to alternatives that include the incoming flow of UI claimants in the denominator (results not shown).

Appendix figure A3. This is the variation exploited in the analysis. We show in Appendix Table A15 that within office fluctuations in the share of treated individuals are not related to jobseekers' characteristics either overall or specifically among members of the treated or the control group. We also find no evidence that fluctuations in the share of treated income support claimants are related to changes in the incoming flow of new UI claimants.[3]

To assess the possibility of program externalities, we estimate the following equation:

$$(3)\ IES\_attendance_{ijt} = \beta_0 + \beta_1\, Treatment_i + \beta_2 Share\_treated_{jt} + \beta_3\, Treatment_i * Share\_treated_{jt} + X_i^{'}\varphi + \gamma_j + \delta_t + \varepsilon_{ijt}$$

where, as before, *i* indexes individuals, *j* employment office, and *t* randomization month. $IES\_attendance_{\mathrm{ijt}}$ is an indicator for reporting to the employment office twelve months after randomization; $Treatment_i$ is an indicator that denotes whether jobseeker i was assigned to treatment; $Share\_treated_{jt}$ is the share of jobseekers assigned to treatment from the incoming flow in employment office *j* in month *t*; X_i is a vector of individual characteristics; γ_j are employment office fixed effects; and δ_t are month fixed effects. The coefficients of interest are β_2 and β_3, which provide evidence on whether the share treated at the same office and in the same month is associated with the likelihood of reporting to the employment office twelve months after randomization for individuals in the control (β_2) or the treatment group ($\beta_2 + \beta_3$).

The results are presented in Appendix Table A16. Column (1) reports the effect of treatment on the probability of reporting to the employment office before the share of treated individuals is added into the model (a simple model that does not include β_2 or β_3). The estimate based on this extended sample and alternative model is similar in magnitude to that reported in Table 3, showing that the program reduced the probability of reporting to a labor office twelve months after randomization by 12.5 percentage points (s.e.=0.011). This is an important result because it shows that this alternative specification and an extended sample yield a similar treatment effect. The treatment coefficient changes little after we control for the share treated in the same office and month as reported in column (2). In column (3) we also

[3] If a higher share of income support claimants was associated with higher unemployment rates, it could create a spurious relationship between the share treated and employment rates or the share of individuals attending the local employment office. We examine this concern by regressing the share treated on the incoming flow of new UI claimants at the employment-office-month level while controlling for employment-office and month fixed effects. The resulting coefficient is highly insignificant (p-value = 0.96).

introduce the interaction term between shared treated and the treatment indicator. Both coefficients are small and not significant, ruling out the possibility of externalities among the treated and the control group (or at least suggesting that if these externalities exist, they may have positive and negative effects that cancel each other out). As an additional robustness check, we also report in column (4) the estimates after controlling for the monthly flow of new UI claimants. There is no change in the size or significance of the estimates.

Appendix 3. Survey Questions for Assessment of Soft-Cognitive Skills

In addition to standard demographic, employment, and earnings questions, both surveys (Wave 1 and Wave 2) included additional modules meant to measure respondents' soft cognitive skills. For logistical reasons that limited survey length, Wave 1 did not include the grit and self-esteem module. In addition, as detailed below, some domains included only a selected number of items.

Job search self-efficacy module (Waves 1 and 2)

I will now read a series of statements. For each statement, please note whether you agree and whether you think it describes you accurately, using the following scale:

1-Strongly agree, 2-Agree, 3-Moderately agree, 4-Disagree, 5-Strongly disagree

1. I am confident in my ability to search for a job.
2. I am confident in my ability to use the internet in order to find a job.
3. I am confident in my ability to write a résumé.
4. I am confident in my ability to pass a job interview.

Source: Israel Employment Service

Work self-efficacy module (Waves 1 and 2)

I will now read a series of statements. For each statement, please note whether you agree and whether you think it describes you accurately, using the following scale:

1-Strongly agree, 2-Agree, 3-Moderately agree, 4-Disagree, 5-Strongly disagree

Thinking of my current or future work, I feel I will be able to…

1. Achieve goals that will be assigned.
2. Respect schedules and working deadlines.
3. Learn new working methods.
4. Concentrate all my energy on work.
5. Collaborate with other colleagues.
6. Have good relationships with my superiors.
7. Be courteous to customers.
8. Get to work on time.

Source: selected items from Pepe, Silvia J., et al., "Work Self-Efficacy Scale and Search for Work Self-efficacy Scale: A Validation study in Spanish and Italian Cultural Contexts." *Revista de Psicología del Trabajo y de las Organizaciones* 26.3 (2010): 201–210.

General self-efficacy module (Waves 1 and 2)

I will now read a number of statements. For each statement, please respond on a 5-point scale as to what extent it describes you.

1-Describes me very well, 2-Describes me well, 3-Describes me somewhat, 4-Doesn't describe me well, 5-Doesn't describe me at all

1. I can always manage to solve difficult problems if I try hard enough.
2. If someone opposes me, I can find the means and ways to get what I want.
3. It is easy for me to stick to my aims and accomplish my goals.
4. I can usually handle whatever comes my way.

Source: selected items in Schwarzer, R., and Jerusalem, M. (1995). "Generalized Self-Efficacy Scale," In J. Weinman, S. Wright, and M. Johnston, *Measures in Health Psychology: A User's Portfolio. Causal and Control Beliefs* (pp. 35-37). Windsor, UK: NFER-NELSON.

Grit Module (Wave 2)

I will now read a number of statements. For each statement, please respond on a 5-point scale as to what extent it describes you.

1-Describes me very well, 2-Describes me well, 3-Describes me somewhat, 4-Doesn't describe me well, 5-Doesn't describe me at all

1. New ideas and projects sometimes distract me from previous ones.
2. Setbacks don't discourage me.
3. I have been obsessed with a certain idea or project for a short time but later lost interest.
4. I am a hard worker.
5. I often set a goal but later choose to pursue a different one.
6. I have difficulty maintaining my focus on projects that take more than a few months to complete.
7. I finish whatever I begin.
8. I am diligent.

Items 1, 3, 5, and 6 are reverse-scored.

Source: "The Short Grit Scale," in Duckworth, Angela Lee, and Patrick D. Quinn, "Development and Validation of the Short Grit Scale (GRIT–S)." *Journal of Personality Assessment 91.2 (2009)*: 166–174.

Self-esteem module (Wave 2)

I will ask you to relate to a number of statements dealing with your general feelings about yourself. Please respond using the following 4-point scale as to how strongly you agree or disagree with each statement.

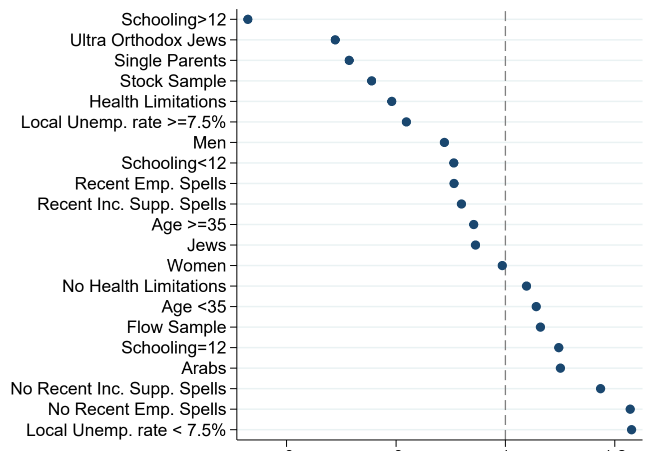
1-Strongly agree, 2-Agree, 3-Disagree, 4-Strongly disagree

1. On the whole, I am satisfied with myself.
2. At times I think I am no good at all.
3. I feel that I have a number of good qualities.
4. I am able to do things as well as most other people.
5. I feel I do not have much to be proud of.
6. I certainly feel useless at times.
7. I feel that I am a person of worth, at least on an equal plane with others.
8. I wish I could have more respect for myself.
9. All in all, I am inclined to feel that I am a failure.
10. I take a positive attitude toward myself.

Items 2, 5, 6, 8, and 9 are reverse-scored.

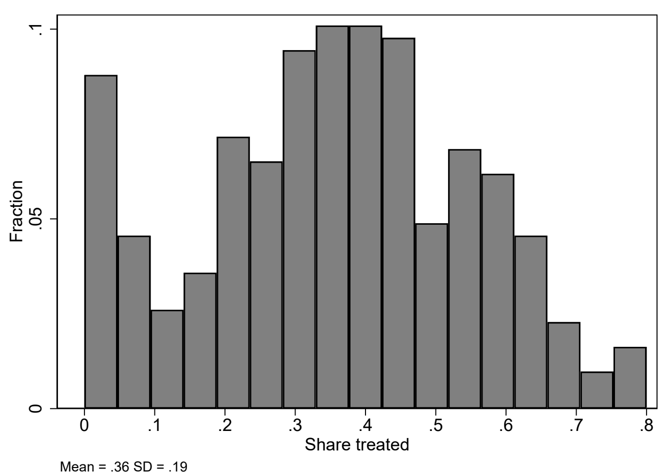
Source: “The Rosenberg Self-Esteem Scale” in Rosenberg, Morris, "Rosenberg Self-Esteem Scale (RSE)." *Acceptance and Commitment Therapy.* Measures Package 61.52 (1965): 18.

Figure A1: Characteristics of individuals who have no formal income within two months after random assignment



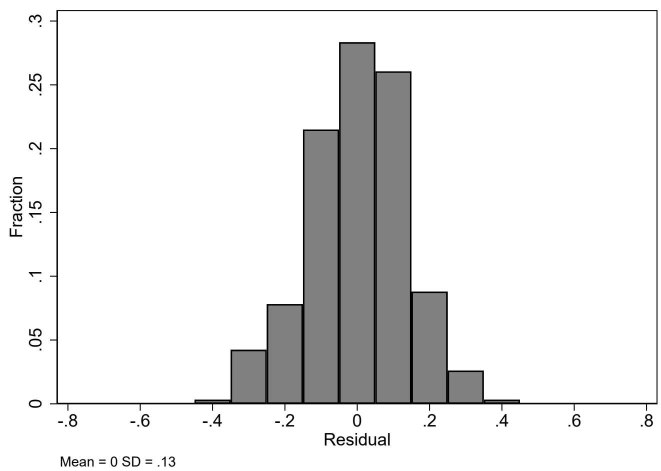
Notes: The figure reports the relative likelihood of the characteristics of individuals who had no formal income and stopped attending the employment office within two months after random assignment to the program.

Figure A2: Local labor market treatment intensity across individuals



Notes: The figure reports the distribution of the local labor market treatment intensity among individuals in our sample according to their employment office and month of assignment.

Figure A3: Residual variance of labor market treatment intensity



Notes: The figure reports the residual variation in local labor market treatment intensity when controlling for employment office and month fixed effects.

Table A1. Probability to stop reporting to the employment office before the randomization lists are transferred

Treated	0.005 (0.008)	More than 12 years of schooling	0.012 (0.017)
Female	-0.003 (0.008)	Received income support months [-12;0]	-0.074*** (0.011)
Age	-0.002*** (0.000)	Received income support months [-24;-11]	0.010 (0.009)
Married	0.001 (0.012)	Received income support months [-36;-23]	-0.013 (0.011)
Children	0.001 (0.002)	Months worked months [-12;0]	-0.003 (0.002)
Single parent	-0.032*** (0.011)	Months worked months [-24;-11]	-0.001 (0.002)
Immigrant	0.002 (0.011)	Months worked months [-36;-23]	0.001 (0.002)
Self-reported health limitation	-0.032*** (0.007)	Total earnings months [-12;0]	0.000 (0.000)
Arab	-0.012 (0.014)	Total earnings months [-24;-11]	-0.000 (0.000)
Ultra Orthodox	-0.004 (0.015)	Total earnings months [-36;-23]	0.000 (0.000)
12 years of schooling	0.001 (0.008)	N	6,744

Notes: The table reports estimates from a linear probability model. The outcome is an indicator for stop reporting to the employment office before the randomization lists are transferred. Control variables include treatment status, individual's characteristics, and randomization unit fixed effects. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A2. Income Changes by Employment and Welfare Status 12 Months After Randomization

	Works (1)	Does not work and does not get income support (2)	Gets income support and does not work (3)
Income from work 12 months after randomization	3678	0	0
Income from work 12 months before randomization	1654	1004	638
Difference	2023	-1004	-638
Income support 12 months after randomization	331	0	1667
Income support 12 months before randomization	286	212	740
Difference	44	-212	928
Total Income 12 months after randomization	4008	0	1667
Total Income 12 months before randomization	1940	1216	1378
Difference	2068	-1216	290
Number of observations	1370	1060	618

Notes: The table reports a decomposition of program particpants' income 12 months before and after assignment to treatment according to their employment status 12 months after random assignemnt. Monetary values in real 2016 NIS.

Table A3a. Heterogeneous Effects of the Program

	Men	Women	Jews	Arabs	Ultra Orthodox Jews	Age <35	Age >=35
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reporting to employment office	-0.138**	-0.158**	-0.102**	-0.229**	-0.111**	-0.102**	-0.203**
	(0.024)	(0.023)	(0.017)	(0.032)	(0.047)	(0.023)	(0.027)
	0.347	0.410	0.305	0.466	0.378	0.290	0.493
Employed	0.062**	0.083**	0.039**	0.140**	0.065	0.054**	0.109**
	(0.023)	(0.019)	(0.019)	(0.019)	(0.044)	(0.019)	(0.020)
	0.391	0.289	0.432	0.227	0.351	0.380	0.276
Number of months employed	0.729**	0.899**	0.474**	1.462**	0.981**	0.716**	1.026**
	(0.215)	(0.131)	(0.141)	(0.159)	(0.386)	(0.155)	(0.165)
	3.745	2.950	4.461	2.048	3.401	3.775	2.705
Income from work	119.303	137.172**	47.625	318.729**	156.105	144.262	166.096*
(Including zeroes)	(130.904)	(62.308)	(85.633)	(86.227)	(171.893)	(101.781)	(92.010)
	1935.163	932.438	1799.845	873.418	1123.021	1501.937	1165.413
Cumulative income from work	1,730.610	1,854.732**	965.299	3,400.894**	2,932.931*	2,001.274**	2,003.713**
(Including zeroes)	(1136.883)	(490.095)	(705.443)	(772.319)	(1671.310)	(902.638)	(694.702)
	17417.234	8718.058	17060.232	7357.457	9854.810	13923.503	10434.754
Received Income support	-0.086**	-0.115**	-0.071**	-0.160**	-0.039	-0.083**	-0.123**
	(0.023)	(0.020)	(0.017)	(0.028)	(0.043)	(0.022)	(0.025)
	0.342	0.455	0.339	0.480	0.458	0.336	0.492
Income support payments	-149.996**	-183.391**	-136.940**	-232.385**	-117.990*	-140.148**	-199.517**
(Including zeroes)	(33.400)	(39.876)	(31.130)	(44.355)	(64.815)	(41.197)	(40.730)
	478.856	728.082	556.633	696.899	631.099	515.894	751.423
Cumulative income support	-1710.018**	-1914.658**	-1704.820**	-2250.202**	-1490.953**	-1559.621**	-2164.527**
(Including zeroes)	(285.972)	(415.274)	(312.237)	(451.096)	(740.744)	(377.436)	(389.080)
	7152.017	9975.647	8048.302	9606.670	8614.368	7609.141	10196.957
Total Income	-30.693	-46.219	-89.315	86.344	38.115	4.114	-33.421
(Including zeroes)	(131.042)	(69.374)	(84.041)	(103.340)	(172.272)	(118.080)	(87.931)
	2414.020	1660.521	2356.479	1570.317	1754.119	2017.831	1916.837
Total cumulative income	20.592	-59.926	-739.520	1,150.692	1,441.978	441.653	-160.814
(Including zeroes)	(1117.231)	(611.742)	(732.331)	(954.673)	(1748.272)	(1059.662)	(720.385)
	24569.250	18693.705	25108.535	16964.127	18469.178	21532.643	20631.711
Received other welfare	-0.007	-0.006	-0.009	-0.007	-0.008	-0.004	-0.019
payments (disability or UI or	(0.012)	(0.012)	(0.014)	(0.012)	(0.027)	(0.011)	(0.014)
other)	0.092	0.124	0.136	0.086	0.104	0.079	0.149
Number of observations	2,675	3,476	3,593	2,558	905	3,144	3,007

Notes: The table reports the program effect on different sub-populations. Controls include the relevant set from the main control list: 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.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A3b. Heterogeneous Effects of the Program

	No Self Reported Health Limitations (1)	Self Reported Health Limitations (2)	Single Parents (3)	Less Than 12 Years Of Schooling (4)	12 years of schooling (5)	more than 12 years of schooling (6)
Reporting to employment office	-0.110**	-0.238**	-0.169**	-0.205**	-0.119**	0.044
	(0.020)	(0.030)	(0.031)	(0.025)	(0.024)	(0.088)
	0.340	0.479	0.425	0.467	0.328	0.173
Employed	0.049**	0.144**	0.073**	0.105**	0.063**	0.053
	(0.016)	(0.022)	(0.035)	(0.021)	(0.020)	(0.078)
	0.374	0.238	0.358	0.260	0.376	0.547
Number of months employed	0.592**	1.502**	0.929**	1.170**	0.716**	-0.090
	(0.135)	(0.206)	(0.282)	(0.158)	(0.160)	(0.729)
	3.763	2.216	3.534	2.508	3.711	6.113
Income from work	31.544	433.296**	203.857	192.277**	166.580**	-284.022
(Including zeroes)	(86.427)	(103.874)	(135.130)	(82.856)	(83.121)	(867.399)
	1550.668	896.815	1213.928	1023.876	1413.231	3617.648
Cumulative income from work	961.157	4,392.611**	2,693.079**	2,703.320**	1,780.159**	-3666.443
(Including zeroes)	(755.428)	(774.413)	(1083.990)	(635.666)	(689.910)	(8500.292)
	14440.458	7624.496	11180.235	9079.598	12985.942	35009.859
Received Income support	-0.085**	-0.153**	-0.084**	-0.144**	-0.084**	0.091
	(0.019)	(0.023)	(0.035)	(0.022)	(0.020)	(0.088)
	0.387	0.456	0.455	0.492	0.355	0.167
Income support payments	-140.091**	-242.835**	-158.917*	-213.227**	-158.266**	176.674
(Including zeroes)	(35.238)	(39.000)	(86.292)	(39.188)	(35.609)	(163.567)
	604.770	670.603	894.952	754.050	540.825	267.320
Cumulative income support	-1574.414**	-2456.417**	-2270.409**	-2447.090**	-1634.693**	1,525.121
(Including zeroes)	(343.518)	(423.796)	(956.902)	(360.131)	(344.355)	(1579.506)
	8547.170	9393.063	12170.298	10221.449	7938.194	4389.525
Total Income	-108.548	190.460*	44.940	-20.950	8.314	-107.348
(Including zeroes)	(94.300)	(110.324)	(141.113)	(87.714)	(85.536)	(860.100)
	2155.438	1567.418	2108.880	1777.926	1954.056	3884.968
Total cumulative income	-613.258	1,936.194**	422.670	256.230	145.466	-2141.323
(Including zeroes)	(839.169)	(862.647)	(1378.514)	(715.668)	(748.589)	(8251.045)
	22987.629	17017.559	23350.533	19301.047	20924.137	39399.383
Received other welfare payments	-0.011	-0.008	-0.031	-0.015	-0.005	-0.094**
(disability or UI or other)	(0.009)	(0.019)	(0.031)	(0.013)	(0.012)	(0.048)
	0.079	0.183	0.283	0.114	0.111	0.087
Number of observations	4,066	2,085	1,258	2,625	3,215	311

Notes: The table reports the program effect on different sub-populations. Controls include the relevant set from the main control list: 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.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A3c. Heterogeneous Effects of the Program

	No Recent Income Support History (1)	Recent Income Support History (2)	No Recent Employment History (3)	Recent Employment History (4)	Stock (5)	Flow (6)	Local Unemploment rate < 7.5% (7)	Local Unemploment rate >=7.5% (8)
Reporting to employment office	-0.127**	-0.170**	-0.206**	-0.115**	-0.2***	-0.134***	-0.117**	-0.173**
	(0.025)	(0.021)	(0.030)	(0.018)	(0.025)	(0.023)	(0.020)	(0.029)
	0.314	0.464	0.491	0.304	*0.508*	*0.360*	0.300	0.438
Employed	0.046*	0.112**	0.093**	0.068**	0.138***	0.059***	0.056**	0.095**
	(0.024)	(0.018)	(0.018)	(0.020)	(0.027)	(0.017)	(0.024)	(0.018)
	0.373	0.284	0.166	0.455	*0.295*	*0.338*	0.415	0.282
Number of months employed	0.509**	1.191**	1.102**	0.727**	1.11***	0.782***	0.481**	1.122**
	(0.185)	(0.163)	(0.155)	(0.176)	(0.233)	(0.141)	(0.162)	(0.155)
	3.782	2.704	1.378	4.701	*2.964*	*3.337*	4.368	2.638
Income from work	51.285	259.203**	121.553*	193.397**	377.071***	96.811	70.504	206.718**
(Including zeroes)	(110.641)	(85.647)	(69.276)	(95.343)	(113.702)	(78.721)	(103.866)	(79.532)
	1603.272	1052.078	612.126	1895.182	*1100.516*	*1392.310*	1763.493	1082.507
Cumulative income from work	1,305.442	2,599.766**	2,121.021**	1,949.087**	2917.22***	1760.241***	1,053.616	2,472.658**
(Including zeroes)	(966.799)	(705.347)	(628.718)	(799.037)	(954.510)	(677.303)	(792.328)	(725.886)
	14894.933	9349.802	4590.745	18081.299	*10049.708*	*12731.909*	16887.566	9515.750
Received Income support	-0.090**	-0.118**	-0.129**	-0.091**	-0.148***	-0.089***	-0.062**	-0.135**
	(0.023)	(0.020)	(0.022)	(0.021)	(0.024)	(0.020)	(0.019)	(0.026)
	0.270	0.566	0.537	0.312	*0.538*	*0.384*	0.324	0.467
Income support payments	-138.395**	-202.724**	-217.213**	-143.504**	-282.4***	-128.431***	-134.578**	-193.341**
(Including zeroes)	(38.943)	(36.636)	(39.369)	(35.774)	(43.131)	(33.603)	(38.480)	(43.362)
	392.579	890.352	837.237	466.647	*859.958*	*580.539*	520.581	699.210
Cumulative income support	-1504.281**	-2224.101**	-2073.753**	-1761.189**	-3002.002***	-1435.743***	-1729.152**	-1942.050**
(Including zeroes)	(356.120)	(344.917)	(449.648)	(353.713)	(367.774)	(325.136)	(388.979)	(400.936)
	5727.101	12323.116	11118.897	7083.726	*11780.957*	*8244.457*	7696.883	9598.610
Total Income	-87.110	56.478	-95.659	49.893	94.671	-31.621	-64.075	13.377
(Including zeroes)	(117.751)	(84.294)	(75.653)	(100.255)	(119.066)	(86.690)	(97.507)	(98.527)
	1995.850	1942.429	1449.363	2361.828	*1960.474*	*1972.848*	2284.073	1781.717
Total cumulative income	-198.838	375.666	47.267	187.898	-84.782	324.498	-675.536	530.608
(Including zeroes)	(1096.140)	(774.294)	(739.422)	(910.179)	(998.780)	(797.759)	(806.437)	(932.220)
	20622.033	21672.918	15709.643	25165.023	*21830.664*	*20976.367*	24584.449	19114.359
Received other welfare payments	-0.023*	0.002	-0.008	-0.011	-0.005	-0.011	-0.021	-0.001
(disability or UI or other)	(0.013)	(0.011)	(0.015)	(0.012)	(0.018)	(0.010)	(0.017)	(0.009)
	0.131	0.089	0.107	0.114	*0.124*	*0.109*	0.146	0.092
Number of observations	3,002	3,149	2,565	3,586	1,498	4,653	2,632	3,415

Notes: The table reports the program effect on different sub-populations. Recent income support history refers to individuals who had at least one spell of income support during the two years prior to randomization. Recent employment history refers to individuals who had at least one employment spell during the two years prior to randomization. The Stock subsample refers to income support claimants who were already reporting to the employment office at randomization date. The flow subsample refers to new or re-registering claimants. Controls include the relevant set from the main control list: 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 < 0.05, ** p < 0.01, *** p < 0.001.

Table A4. Selection into the Survey

	(1)	(2)
Treated	0.014	-0.084
	(0.015)	(0.073)
Female	0.014	0.005
	(0.018)	(0.025)
Age	0.001	0.001
	(0.001)	(0.001)
Married	0.038*	0.041
	(0.020)	(0.027)
Children	0.003	-0.001
	(0.005)	(0.006)
Single parent	0.026	0.013
	(0.019)	(0.029)
Immigrant	-0.063***	-0.032
	(0.021)	(0.027)
Self-reported health limitation	0.049***	0.087***
	(0.015)	(0.023)
Arab	0.016	-0.026
	(0.021)	(0.024)
Ultra Orthodox	0.084***	0.076*
	(0.026)	(0.044)
12 years of schooling	0.095***	0.082***
	(0.016)	(0.022)
More than 12 years of schooling	0.194***	0.145***
	(0.031)	(0.048)
Received income support months [-12;0]	-0.001	0.008
	(0.019)	(0.025)
Received income support months [-24;-11]	0.042**	0.000
	(0.020)	(0.034)
Received income support months [-36;-23]	-0.016	0.004
	(0.020)	(0.033)
Months worked months [-12;0]	-0.001	-0.003
	(0.004)	(0.006)
Months worked months [-24;-11]	0.002	0.004
	(0.003)	(0.005)
Months worked months [-36;-23]	0.006*	0.009**
	(0.003)	(0.004)
Total earnings months [-12;-0]	-0.001	0.000
	(0.008)	(0.011)
Total earnings months [-24;-11]	0.005	0.004
	(0.007)	(0.009)
Total earnings months [-36;-23]	-0.001	-0.001
	(0.005)	(0.007)
First survey pop. sample	0.350***	0.333***
	(0.021)	(0.016)

	(1)	(2)
Treated * Female		0.023
		(0.035)
Treated * Age		0.001
		(0.002)
Treated * Married		-0.014
		(0.040)
Treated * Children		0.009
		(0.008)
Treated * Single parent		0.017
		(0.043)
Treated * Immigrant		-0.053
		(0.036)
Treated * Self-reported health limitation		-0.075**
		(0.030)
Treated * Arab		0.095***
		(0.035)
Treated * Ultra Orthodox		0.017
		(0.051)
Treated * 12 years of schooling		0.024
		(0.029)
Treated * More than 12 years of schooling		0.086
		(0.065)
Treated * Received income support months [-12;0]		-0.011
		(0.037)
Treated * Received income support months [-24;-11]		0.080
		(0.050)
Treated * Received income support months [-36;-23]		-0.033
		(0.044)
Treated * Months worked months [-12;0]		0.002
		(0.007)
Treated * Months worked months [-24;-11]		-0.003
		(0.008)
Treated * Months worked months [-36;-23]		-0.005
		(0.005)
Treated * Total earnings months [-12;0]		-0.001
		(0.016)
Treated * Total earnings months [-24;-11]		0.001
		(0.016)
Treated * Total earnings months [-36;-23]		0.002
		(0.010)
Treated * First survey pop. sample		0.045
		(0.028)
Treated * Claimant type		-0.006
		(0.034)
F-Stat for joint significance		4.875
P-value		<0.001
N	6,713	6,713

Notes: The table reports the probability of survey response as a function of personal characteristics and program assignment, conditional on randomization unit fixed effects. The F-stat is for a test of joint significance of treatment and all interactions with treatment.

Standard errors clustered at the randomization unit level in parentheses.* p < 0.05, ** p < 0.01, *** p < 0.001.

Table A5. Estimation of Survey Weights - Probability of Inclusion into Survey Sample

Treated	0.066 (0.295)	Treated * Female	0.086 (0.123)
Female	0.029 (0.090)	Treated * Age	-0.001 (0.007)
Age	0.004 (0.005)	Treated * Married	-0.070 (0.170)
Married	0.179 (0.123)	Treated * Children	0.034 (0.036)
Children	-0.001 (0.025)	Treated * Single parent	0.104 (0.182)
Single parent	0.020 (0.135)	Treated * Immigrant	-0.177 (0.156)
Immigrant	-0.153 (0.116)	Treated * Self-reported health limitation	-0.276** (0.123)
Self-reported health limitation	0.313*** (0.090)	Treated * Arab	0.413*** (0.151)
Arab	-0.084 (0.126)	Treated * Ultra Orthodox	0.089 (0.199)
Ultra Orthodox	0.328** (0.158)	Treated * 12 years of schooling	0.134 (0.121)
12 years of schooling	0.279*** (0.088)	Treated * More than 12 years of schooling	0.257 (0.266)
More than 12 years of schooling	0.646*** (0.194)	Treated * Received income support months [-12;0]	-0.179 (0.132)
Received income support months [-12;0]	0.199* (0.103)	Treated * Received income support months [-24;-11]	0.402** (0.188)
Received income support months [-24;-11]	-0.028 (0.141)	Treated * Received income support months [-36;-23]	-0.188 (0.186)
Received income support months [-36;-23]	-0.013 (0.139)	Treated * Months worked months [-12;0]	0.016 (0.029)
Months worked months [-12;0]	-0.016 (0.021)	Treated * Months worked months [-24;-11]	-0.030 (0.030)
Months worked months [-24;-11]	0.027 (0.022)	Treated * Months worked months [-36;-23]	-0.014 (0.025)
Months worked months [-36;-23]	0.030* (0.018)	Treated * Total earnings months [-12;0]	-0.019 (0.064)
Total earnings months [-12;-0]	0.013 (0.046)	Treated * Total earnings months [-24;-11]	0.032 (0.057)
Total earnings months [-24;-11]	0.004 (0.040)	Treated * Total earnings months [-36;-23]	0.001 (0.044)
Total earnings months [-36;-23]	-0.010 (0.031)	Constant	-0.951** (0.406)
		N	6,117

Notes: The table reports the estimates of a logistic regression that estimates likelihood of survey response as a function of personal characteristics and program assignment, conditional on randomization unit fixed effects. Standard errors clustered at the randomization unit level in parentheses.* p < 0.05, ** p < 0.01, *** p < 0.001.

Table A6. Descriptive Statistics and Balancing Tests - Survey Sample

	treated (1)	T-C (2)		treated (1)	T-C (2)
Female	0.54	-0.024	Months worked	2.84	-0.061
		(0.023)	months [-12;0]		(0.199)
Age	34.56	0.129	Months worked	3.96	0.098
		(0.492)	months [-24;-11]		(0.242)
Married	0.47	0.007	Months worked	4.31	0.223
		(0.020)	months [-36;-23]		(0.254)
Children	2.00	0.014	Total earnings	9846	150
		(0.092)	months [-12;0]		(696)
Single parent	0.22	0.002	Total earnings	16341	1220
		(0.021)	months [-24;-11]		(1294)
Immigrant	0.20	-0.018	Total earnings	18284	1100
		(0.019)	months [-36;-23]		(1536)
Self-reported health limitation	0.36	0.009	Total income support	6106	140
		(0.021)	months [-12;0]		(424)
Arab	0.35	-0.002	Total income support	4040	250
		(0.014)	months [-24;-11]		(389)
Ultra Orthodox	0.19	0.025*	Total income support	3263	90
		(0.013)	months [-36;-23]		(318)
Less than 12 years of schooling	0.39	-0.033	Months since random assignm	13.60	-0.464
		(0.024)			(0.000)
12 years of schooling	0.56	0.032	F-Stat for joint significance	0.693	
		(0.024)			
			P-value	0.835	
More than 12 years of schooling	0.05	0			
		(0.010)	Number of observations	1,702	3,044
Received income support	0.52	-0.015			
months [-12;0]		(0.028)			
Received income support	0.28	0.003			
months [-24;-11]		(0.021)			
Received income support	0.24	0.004			
months [-36;-23]		(0.019)			

Notes: The table reports the average characteristics of treatment group (column 1) alongside the estimated difference with the control group, conditional on randomization unit fixed effects (column 2). The sample is restricted on survey respondent. The reported F statistic tests the joint significance of all covariants in a linear probability model that predicts treatment status conditional on randomization unit fixed effects. Observations are weighted by survey weights. Monetary values in real 2016 NIS. Standard errors clustered at the randomization unit level in parentheses.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A7. Main Results Based on Survey Sample

	Full sample (1)	Stock (2)	Flow (3)
Reporting to employment office	-0.157***	-0.241***	-0.122***
	(0.024)	(0.039)	(0.027)
	0.409	*0.566*	*0.378*
Employment	0.089***	0.109**	0.074***
	(0.023)	(0.050)	(0.025)
	0.355	*0.306*	*0.365*
Income from work	119	290	38
(Including zeroes)	(107)	(190)	(131)
	1,477	*1,121*	*1,546*
Cumulative income from work	1510	1296	1365
(Including zeroes)	(973)	(1543)	(1200)
	13,501	*10,160*	*14,157*
Received Income support	-0.083***	-0.117***	-0.066**
	(0.023)	(0.041)	(0.029)
	0.423	*0.550*	*0.398*
Income support payments	-131***	-277***	-76
(Including zeroes)	(43)	(68)	(52)
	621	*875*	*572*
Cumulative income support	-1364***	-2862***	-757
(Including zeroes)	(469)	(647)	(583)
	8,776	*12,020*	*8,139*
Total Income	-12	13	-38
(Including zeroes)	(111)	(204)	(135)
	2,098	*1,995*	*2,118*
Total cumulative income	146	-1566	608
(Including zeroes)	(1037)	(1547)	(1268)
	22,276	*22,180*	*22,295*
Received other welfare payments	-0.003	-0.012	-0.004
(disability or UI or other)	(0.013)	(0.028)	(0.015)
	0.109	*0.112*	*0.108*
N	3064	840	2224

Notes: The table reports the program effect on participants' outcomes. The sample is restricted to survey respondents. All regressions control for the same set of covariates reported in Table 3 and include randomization unit fixed effects. Observations are weighted by survey weights. Monetary values in real 2016 NIS. Control group means in italics. Standard errors clustered at the randomization unit level in parentheses.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A8. Reliability Coefficients of Survey Constructs

Item	Obs (1)	Sign (2)	Item-test correlation (3)	Item-rest correlation (4)	Average interitem covariance (5)	Alpha (6)
Search efficacy					0.612	0.863
I am confident in my abilities to search for a job	2750	+	0.835	0.689	0.623	0.832
I am confident in my ability to use the internet in order to find a job	2725	+	0.816	0.660	0.643	0.844
I am confident in my ability to write a resume	2775	+	0.864	0.738	0.591	0.813
I am confident in my ability to pass a job interview	2701	+	0.861	0.737	0.591	0.813
Work self-efficacy					0.760	0.962
Achieve goals that will be assigned	2729	+	0.875	0.832	0.766	0.958
Respect schedules and working deadlines	2756	+	0.889	0.850	0.761	0.957
Learn new working methods	2719	+	0.862	0.816	0.769	0.959
Concentrate all energy on work	2738	+	0.887	0.848	0.761	0.957
Collaborate with other colleagues	2747	+	0.912	0.882	0.752	0.955
Have good relationships with my superiors	2733	+	0.912	0.881	0.753	0.955
Be courteous to customers	2711	+	0.901	0.867	0.756	0.956
Get to work on time	2748	+	0.886	0.847	0.762	0.957
General self-efficacy					0.609	0.862
I can always manage to solve difficult problems if I try hard enough	2794	+	0.850	0.713	0.604	0.821
If someone opposes me, I can find the means and ways to get what I want	2753	+	0.850	0.717	0.600	0.818
It is easy for me to stick to my aims and accomplish my goals	2785	+	0.831	0.682	0.624	0.833
I can usually handle whatever comes my way	2757	+	0.842	0.704	0.608	0.823

Table A8. (cont.) Reliability Coefficients of Survey Constructs

Item	Obs (1)	Sign (2)	Item-test correlation (3)	Item-rest correlation (4)	Average interitem covariance (5)	Alpha (6)
Grit					0.137	0.559
New ideas and projects sometimes distract me from previous ones (reversed)	831	+	0.429	0.172	0.151	0.555
Setbacks don't discourage me	924	+	0.368	0.100	0.166	0.583
I have been obsessed with a certain idea or project for a short time but later lost interest (reversed)	848	+	0.533	0.299	0.130	0.511
I am a hard worker	889	+	0.453	0.197	0.148	0.549
I often set a goal but later choose to pursue a different one (reversed)	866	+	0.476	0.227	0.140	0.533
I have difficulty maintaining my focus on projects that take more than a few months to complete (reversed)	838	+	0.572	0.356	0.122	0.494
I finish whatever I begin	938	+	0.609	0.388	0.117	0.481
I am diligent	929	+	0.609	0.384	0.120	0.488
Self esteem					0.268	0.785
On the whole, I am satisfied with myself	976	+	0.642	0.492	0.263	0.763
At times I think I am no good at all (reversed)	947	+	0.581	0.432	0.268	0.768
I feel that I have a number of good qualities	955	+	0.637	0.501	0.261	0.761
I am able to do things as well as most other people	950	+	0.647	0.513	0.259	0.758
I feel I do not have much to be proud of (reversed)	872	+	0.410	0.246	0.294	0.790
I certainly feel useless at times (reversed)	877	+	0.612	0.475	0.262	0.762
I feel that I am a person of worth, at least on an equal plane with others	919	+	0.572	0.429	0.270	0.769
I wish I could have more respect for myself (reversed)	879	+	0.476	0.317	0.285	0.782
All in all, I am inclined to feel that I am a failure (reversed)	853	+	0.653	0.532	0.257	0.757
I take a positive attitude toward myself	933	+	0.637	0.503	0.259	0.759

Notes: The table reports the inter-item correlations and Cronbach's alpha for the different soft skills domains included in the survey.

Table A9. Correlations Between Survey Constructs

	Job search self efficacy score (1)	Work self efficacy score (2)	Self efficacy score (3)	Grit score (4)	Self esteem score (5)
Job search self efficacy score	1.000	0.636	0.518	0.364	0.436
Work self efficacy score	0.636	1.000	0.603	0.447	0.477
Self efficacy score	0.518	0.603	1.000	0.464	0.542
Grit score	0.364	0.447	0.464	1.000	0.517
Self esteem score	0.436	0.477	0.542	0.517	1.000

Notes: The table reports the variance-covariance matrix of the standardized aggregate soft skills scores in the survey sample.

Table A10. Program Effect on Search Efficacy

	Full sample (1)	Stock (2)	Flow (3)
I am confident in my abilities to search for a job	0.042	0.153	0.005
	(0.048)	(0.115)	(0.054)
	2750	*746*	*2004*
I am confident in my ability to use the internet in order to find a job	0.069*	0.195**	0.033
	(0.038)	(0.077)	(0.039)
	2725	*735*	*1990*
I am confident in my ability to write a resume	0.054	0.191**	0.019
	(0.041)	(0.084)	(0.044)
	2775	*754*	*2021*
I am confident in my ability to pass a job interview	0.068	0.226**	0.012
	(0.044)	(0.096)	(0.047)
	2701	*736*	*1965*

Notes: The table reports the program effect on participants' standardized job search self-efficacy items. All regressions control for the same set of covariates reported in Table 3 and include also survey month and randomization unit fixed effects. Observations are weighted by survey weights. Number of observations in italics. Standard errors clustered at the randomization unit level in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A11. Program Effect on Work Self-Efficacy

I Feel I can…	Full sample (1)	Stock (2)	Flow (3)
Achieve goals that will be assigned	0.060	0.158*	0.021
	(0.044)	(0.088)	(0.051)
	2729	*734*	*1995*
Respect schedules and working deadlines	0.072*	0.132*	0.042
	(0.042)	(0.068)	(0.049)
	2756	*744*	*2012*
Learn new working methods	0.072*	0.137	0.043
	(0.044)	(0.089)	(0.048)
	2719	*730*	*1989*
Concentrate all energy on work	0.100**	0.091	0.092*
	(0.047)	(0.082)	(0.055)
	2738	*740*	*1998*
Collaborate with other colleagues	0.107**	0.183**	0.067
	(0.045)	(0.086)	(0.051)
	2747	*747*	*2000*
Have good relationships with my superiors	0.073	0.132	0.055
	(0.051)	(0.083)	(0.061)
	2733	*739*	*1994*
Be courteous to customers	0.103**	0.122	0.089
	(0.048)	(0.086)	(0.055)
	2711	*733*	*1978*
Get to work on time	0.094**	0.098	0.086
	(0.047)	(0.083)	(0.054)
	2748	*742*	*2006*

Notes: The table reports the program effect on participants' standardized work self-efficacy items. All regressions control for the same set of covariates reported in Table 3 and include also survey month and randomization unit fixed effects. Observations are weighted by survey weights. Number of observations in italics. Standard errors clustered at the randomization unit level in parentheses.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A12. Program Effect on Self-Efficacy

	Full sample (1)	Stock (2)	Flow (3)
I can always manage to solve difficult problems if I try hard enough	-0.064	0.074	-0.102*
	(0.051)	(0.105)	(0.056)
	2794	*750*	*2044*
If someone opposes me, I can find the means and ways to get what I want	0.084	0.146*	0.080
	(0.052)	(0.075)	(0.063)
	2753	*737*	*2016*
It is easy for me to stick to my aims and accomplish my goals	-0.029	0.188*	-0.084
	(0.055)	(0.097)	(0.058)
	2785	*746*	*2039*
I can usually handle whatever comes my way	0.030	0.193**	-0.008
	(0.044)	(0.092)	(0.048)
	2757	*738*	*2019*

Notes: The table reports the program effect on participants' standardized general self-efficacy items. All regressions control for the same set of covariates reported in Table 3 and include also survey month and randomization unit fixed effects. Observations are weighted by survey weights. Number of observations in italics. Standard errors clustered at the randomization unit level in parentheses.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A13. Program Effect on Grit

	Full sample (1)	Stock (2)	Flow (3)
New ideas and projects sometimes distract me from previous ones (reversed)	-0.003	0.048	-0.049
	(0.089)	(0.228)	(0.095)
	831	*241*	*590*
Setbacks don't discourage me	0.098	-0.008	0.143
	(0.078)	(0.168)	(0.088)
	924	*270*	*654*
I have been obsessed with a certain idea or project for a short time but later lost interest (reversed)	-0.128	-0.043	-0.138
	(0.080)	(0.191)	(0.096)
	848	*252*	*596*
I am a hard worker	0.097	0.227	0.062
	(0.083)	(0.162)	(0.105)
	889	*258*	*631*
I often set a goal but later choose to pursue a different one (reversed)	-0.153	0.077	-0.210**
	(0.093)	(0.230)	(0.101)
	866	*252*	*614*
I have difficulty maintaining my focus on projects that take more than a few months to complete (reversed)	-0.014	0.275	-0.092
	(0.098)	(0.197)	(0.110)
	838	*242*	*596*
I finish whatever I begin	-0.141	0.228	-0.208**
	(0.085)	(0.151)	(0.099)
	938	*273*	*665*
I am diligent	0.056	0.407**	-0.027
	(0.069)	(0.156)	(0.077)
	929	*272*	*657*

Notes: The table reports the program effect on participants' standardized grit items. All regressions control for the same set of covariates reported in Table 3 and include also survey month and randomization unit fixed effects. Observations are weighted by survey weights. Number of observations in italics. Standard errors clustered at the randomization unit level in parentheses.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A14. Program Effect on Self-Esteem

	Full sample (1)	Stock (2)	Flow (3)
On the whole, I am satisfied with myself	-0.018	0.107	-0.029
	(0.080)	(0.171)	(0.090)
	976	*290*	*686*
At times I think I am no good at all (reversed)	0.046	0.248	-0.019
	(0.078)	(0.163)	(0.090)
	947	*278*	*669*
I feel that I have a number of good qualities	0.093	0.171	0.070
	(0.099)	(0.162)	(0.122)
	955	*283*	*672*
I am able to do things as well as most other people	0.082	0.341*	0.029
	(0.091)	(0.191)	(0.100)
	950	*283*	*667*
I feel I do not have much to be proud of (reversed)	-0.000	0.124	-0.054
	(0.095)	(0.160)	(0.117)
	872	*264*	*608*
I certainly feel useless at times (reversed)	-0.011	0.182	-0.038
	(0.076)	(0.140)	(0.087)
	877	*261*	*616*
I feel that I am a person of worth, at least on an equal plane with others	0.124	0.382**	0.031
	(0.109)	(0.171)	(0.134)
	919	*270*	*649*
I wish I could have more respect for myself (reversed)	0.167**	0.426***	0.102
	(0.079)	(0.143)	(0.093)
	879	*260*	*619*
All in all, I am inclined to feel that I am a failure (reversed)	0.016	0.196	0.008
	(0.080)	(0.194)	(0.089)
	853	*252*	*601*
I take a positive attitude toward myself	0.091	0.094	0.102
	(0.088)	(0.200)	(0.103)
	933	*281*	*652*

Notes: The table reports the program effect on participants' standardized self-esteem items. All regressions control for the same set of covariates reported in Table 3 and include also survey month and randomization unit fixed effects. Observations are weighted by survey weights. Number of observations in italics. Standard errors clustered at the randomization unit level in parentheses.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A15. Balancing Tests by Share Treated in Employment Office

	Female (1)	Age (2)	Married (3)	Number of children (4)	Single parent (5)	Immigrant (6)	Self-reported health limitation (7)	Arab (8)	Ultra Orthodox (9)	Less than 12 years of schooling (10)	12 years of schooling (11)	More than 12 years of schooling (12)
Share treated	0.006 (0.042)	-0.026 (0.823)	-0.028 (0.043)	-0.111 (0.201)	0.030 (0.032)	0.044 (0.039)	-0.066* (0.037)	-0.031 (0.031)	-0.032 (0.026)	-0.059 (0.050)	0.062 (0.050)	-0.002 (0.021)
Treated	-0.003 (0.022)	0.264 (0.360)	-0.008 (0.020)	0.128 (0.093)	0.030** (0.015)	-0.006 (0.020)	-0.001 (0.020)	0.009 (0.014)	-0.006 (0.013)	0.002 (0.021)	0.011 (0.022)	-0.013 (0.013)
Treated * Share treated	-0.026 (0.053)	-0.625 (0.944)	-0.014 (0.055)	-0.184 (0.236)	-0.041 (0.039)	-0.018 (0.050)	0.060 (0.049)	0.006 (0.041)	0.021 (0.036)	0.004 (0.058)	-0.026 (0.059)	0.021 (0.029)
N	16,635	16,635	16,635	16,635	16,635	16,635	16,635	16,635	16,635	16,635	16,635	16,635

Notes: The table reports the association between the share of monthly treated individuals in each employment office and individuals' characteristics. Controls include employment office and month fixed effects. Standard errors clustered at the employment-office-month level in parentheses.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A16. The Relationship Between Share Treated and Attendance at the Employment Office

	Attendance at the employment office 12 months after random assignment			
	(1)	(2)	(3)	(4)
Treatment	-0.125***	-0.121***	-0.141***	-0.141***
	(0.011)	(0.011)	(0.022)	(0.022)
Share Treated		-0.052	-0.078	-0.078
		(0.039)	(0.051)	(0.051)
Share Treated X Treatment			0.057	0.057
			(0.059)	(0.058)
Flow of UI claimants (in thousands)				0.001
				(0.019)
N	13,058	13,058	13,058	13,058

Notes: The table reports the probability to report to the employment office 12 months after random assignment as a function of treatment status, the share of monthly treated individuals at the employment office and the interaction between both variables. All regressions control for the same set of covariates reported in Table 3 and include also employment office and month fixed effects. Standard errors clustered at the employment-office-month level in parentheses.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.