

LONG-TERM EFFECTS OF TRAINING VOUCHERS FOR THE UNEMPLOYED

ITAMAR YAKIR¹

Abstract

Although there are various justifications for governments to subsidize vocational training for the unemployed, it is not entirely clear whether and when such programs are effective, and to what extent such programs can benefit participants in improving their performance beyond their historical level. This paper studies the long-term effects of training vouchers on the labor market outcomes of unemployed in Israel using a reweighting-based matching. After 4 years from treatment, significant and positive effects are found for employment (+6%) and for earnings (+7%). A significant effect can also be identified 6 years after treatment. The effect is stronger among women, Arabs, low-skilled workers, and the long-term unemployed, but is rather limited among recipients of income support. These findings suggest that training vouchers are more effective for groups that are characterized by relatively low attachment to the labor market, but not for the least-advantaged individuals whose attachment to the labor market is extremely low. Assignment to subsidized training does not seem to increase survival rates in the labor market or to trigger an increase in wages beyond historical personal records. Thus, this paper suggests that short training for the unemployed delivered via vouchers is at most an effective tool for moderating the negative impact caused by unemployment and for restoring employment rates, but not as an opportunity to improve individuals' performance to new levels.

¹ Itamar Yakir – Yale University – Email: itamar.yakir@yale.edu

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1. INTRODUCTION

Most governments in advanced economies regard economic equality as a main policy goal. In some economies, and in Israel specifically (Dahan, 2021), inequality is directly related to unemployment, so that reducing unemployment is one key for restraining inequality. Reducing unemployment can also be achieved by providing employment services, such as active labor market programs (ALMPs). These programs can also serve another main policy goal of increasing levels of productivity among current workers and labor market returnees.

Vocational training (VT) is a central measure in ALMPs (Card et al., 2018; McCall et al., 2016; Vooren et al., 2019). It can be delivered in various manners: privately financed, fully funded, and subsidized. One way to subsidize VT is through vouchers—aimed at offering a wider choice relative to programs directly delivered by the government. In turn, a wider choice is assumed to be key in achieving greater efficiency (Tomini et al., 2016). The connection between choice and efficiency is based on two assumptions: first, that individuals have an advantage over government officials in choosing a relevant training that fits their preferences and skills; second, that over time training providers will be better than government in identifying demand and offering relevant training accordingly.²

Ensuring the effectiveness of subsidized training is important both from the point of view of trainees' opportunity cost of time and from the point of view of appropriate use of taxpayers' money. However, training may be effective for some types of workers or for some social groups, but not for others, which means that studying the heterogeneous effect of training should be a primary goal in studies that focus on training effectiveness.

Training vouchers—or subsidized training at large—has three main target populations: youth (Attanasio et al., 2011, 2017; Card et al., 2011), employed workers (Hidalgo et al., 2014; Novella et al., 2018; Schwerdt et al., 2012) and the unemployed (see below). While randomized control trials (RCTs) have been used in numerous cases for studying the effect of training on the outcomes of youth and employed workers, using RCTs for studying the effect on the unemployed is less common. Studying the effectiveness of training for the

² It is worth distinguishing between two contexts in which the use of vouchers can lead to increased efficiency—at least when comparing vouchers to a parallel situation in which the state is responsible for both funding and providing training. The first context is the demand side: assuming that individuals are best placed to know what is the appropriate and desirable training for them, leaving the choice to them is more efficient than a situation in which they are assigned to training or to a narrow range of courses by the government (although it is possible to take into account a situation in which the state provides a wide range of training options from which individuals can choose). The second context is the supply side: the government's focus on funding training, while leaving the provision to private companies, may contribute to efficiency to the extent that the latter are better than the government at identifying promising training areas; or, for example, insofar as they are able—in a way that the government is not—to identify and train individuals who are likely to be successful in the labor market.

unemployed in a nonexperimental setup is challenging for a few reasons. First, it is a nontrivial question who among the unemployed is offered a voucher. Possible candidates are those who are assumed to benefit the most from training or those with the highest potential to utilize the vouchers or complete training (*cream-skimming*). The next closely related question is who should be included in the comparison group, and how the timing of treatment (in this case, the beginning of training) should be constructed for that control group.

Matching is a common design for addressing these challenges, and it has indeed been used in observational studies to estimate the effect of training on the outcomes of various groups including the unemployed. Based on matching, Andersson et al. (2013) find training to have positive but limited effects on employment and wages for adult US workers, but not for dislocated workers. For the unemployed in Germany, Huber et al. (2018) find short-term negative and long-term positive effects of training on employment—that is, the unemployed who go through training experience delayed reintegration into the labor market relative to other unemployed (*lock-in effect*). Furthermore, Doerr et al. (2017) find similar results for employment, with no effect on earnings, and a stronger effect among the low-skilled. Focusing on women only, Doerr (2022) finds large long term effects on both employment and earnings, stronger among the low skilled. These papers use the allocation of vouchers to study the effect of training on labor market outcomes by comparing the outcomes of voucher awardees to relevant control and matched-control groups that were constructed using various matching algorithms.

In addition to identifying what drives effectiveness, and measuring the heterogeneous effect, an important objective in such studies is identifying the behavioral mechanism that translates the receipt of vouchers into outcomes in the labor market. While one mechanism is enhanced human capital, additional options include increased motivation, originating in the very act of being assigned to the program, and exposure to new job opportunities created by entering new networks of course participants and teachers.

Uncovering mechanisms is also important for drawing more general conclusions regarding the potential of ALMPs. The question in this regard is whether ALMPs for the unemployed are only good enough for restoring employment levels to pretreatment levels, or can they also generate a significant leap forward in terms of skills and employability, manifested through new wage levels higher than the historical records (relative to the control group). This difference is crucial for a country such as Israel in which training for current workers is very limited. Under such circumstances, events of unemployment *coincidentally* create an opportunity to improve the human capital of the labor force.³

³ Indeed, during the period under consideration, the individuals in question mature and accordingly their human capital grows by virtue of the increase in work experience and by virtue of the increase in tenure and specialization in a specific firm. Hence, the wage after treatment may exceed its historical level not only due to training but also due the experience accumulated. Accordingly, the assessment of effectiveness in this context is focused on the question of whether this change in wage, relative to its historical level, is stronger in the

Several studies have shown that it may take a long while for the unemployed to go back to their historical wage and employment levels. Thus, for example, Doerr (2022) finds a very strong treatment effect of training vouchers on the employment of German women returning to the labor market, of more than 15 percentage points, relative to the control group. However, six years after treatment, this group had not restored its historical wage levels, and had only restored historical employment rates at the very end of the period without surpassing them.

Overall, long-run studies of training effectiveness are not very common, especially when considering programs for the unemployed (Card et al., 2018). However, because government expenditure per participant may be considerable, and because exiting the labor market in specific ages and personal circumstances may be detrimental, there is a high importance for locating what works within and across program types, countries, and across various types of workers.

Against this backdrop, this paper explores the long-term effectiveness of training vouchers for the unemployed. It investigates in what ways these vouchers benefit participants and examines whether certain groups of participants benefit more than others, and if so, why.

The paper is based on a unified dataset that joins together administrative data from three sources: the National Insurance Institute (NII), the Israeli Employment Service (IES) and the Labor Ministry. Using this data, via reweighting-based matching, I compare the outcomes of two groups to assess the effect of training vouchers. The treated are unemployed who received employment counseling and also received vouchers for participating in training courses. By contrast, the control group includes a random sample of unemployed individuals who only received counseling but had not been awarded a training voucher. The effect of VT on various labor market outcomes is examined in the medium and long term, and up to 70 months after treatment (counseling).

Assuming that the matching process has adequately solved the challenges arising from selection, it is possible to interpret the findings in a causal manner. The findings show that being assigned to the program had a significant and positive effect on employment (+6%), lasting in the long run. That is, the advantage created by receiving a voucher (that in turn proxies but does not align completely with participating in training) remains stable over time, and nonparticipants do not catch up. I also find an effect on cumulative earnings, reaching 7–9%, although this effect is less stable across specifications and horizons.

The effect on employment is stronger for women than for men, for individuals aged 40 and older relative to younger individuals,⁴ for individuals with no college education relative to those with some college education, and for Arabs relative to Jews. These results suggest that the program is more effective for the least advantaged groups who encounter more difficulties in labor market integration, while stronger workers are more likely to manage on

treatment group than the corresponding change in the control group, which also experiences an increase in experience.

⁴ The median age among the study population is 40.

their own (this is reflected in the fact that among these groups the control group catches up). In addition, the vouchers have not increased participants' level of earning, conditional on being employed.

The paper contributes to the relevant literature in several ways. First, it is the first study that systematically examines the long-term effectiveness of VT in Israel. It thus relates to a relatively small number of studies that examine the effect of training on the unemployed over the long term, showing results up to 70 months from treatment, and observing pretreatment results for a similar duration. Examining the long term is especially important because, as numerous studies show (Huber et al., 2018; Doerr and Strittmatter, 2021), the effect of training in the long term may differ from short-term effects, given the lock-in period in which individuals are trained and invest less in job searching. Hence, long-term comparisons are especially important from a cost-benefit point of view, because some programs may become cost effective only in the long term but not in shorter spells.

Second, I utilize socio-demographic data to study effect heterogeneity across various strata and social groups. Although focusing on these social groups is in part a unique attribute of the local Israeli setup, this analysis has relevance for studying the effect of such programs on the outcomes of migrants and minority groups in advanced economies.⁵ Migrants and minority groups are in many cases characterized by lower labor market participation due to cultural reasons, or because they encounter challenges stemming from assimilating in a new country (Reimers, 1985). For example, Arab women in Israel have a very low participation rate in the labor market due to family and community norms (Yashiv and Kasir, 2013).

Finally, I show that VT for the unemployed—as applied in Israel during the 2010s—was mostly effective as a means for helping participants to go back to employment but without further raising labor market performance to new levels beyond the historical ones.

The rest of the paper is structured as follows. Section 2 describes the vouchers program. Section 3 presents the data sources used in the paper and the outcomes analyzed. The design is discussed in Section 4. Section 5 presents the main results, Section 6 includes a discussion of the results, and Section 7 concludes.

⁵ Often, effect heterogeneity in activation and training is examined by gender, age group, and level of education. However, analysis at the level of population groups is less common. Sarvimäki and Hämäläinen (2016), for example, studied the effect of activation programs designed for immigrants, but not in comparison to the native population or the majority group, but in comparison to past immigrants.

2. THE PROGRAM

VT in Israel includes fully funded and privately financed tracks as well as subsidized programs. Among the latter, one central channel is the Vocational Training Vouchers Program (VTVP) that was first introduced in 2006. It serves both the unemployed (directed via the IES) and employed workers (directed by governmental and semigovernmental directing centers) in roughly equal proportions. The direct costs of the subsidy over the years 2013–19 for all the participants (more than 30,000 participants of both types) can be estimated around NIS 165–210 million (the equivalent of roughly USD 47–60 million), net of administrative costs.

The program resembles the US and German programs in most central characteristics, with a few exceptions. First, while in the German program participants only pay the out-of-pocket difference between the tuition and subsidy, in the Israeli case participants first pay the full sum upfront, then gradually get refund payments. This means that liquidity and tuition levels may have been an issue for potential participants. Second, technical and technological trainings in Israel are of a small scale, and nontechnical vocational tracks are relatively less developed when it comes to accreditation and organizational matters.

As part of the program, each eligible applicant chooses a course from a closed list, which the Ministry of Labor authorizes, with a tuition set by the school where they intend to study (mean ~ NIS 9,100; s.d. ~ NIS 4,300). After paying the tuition upfront, participants are eligible to three refund payments, conditional on progress through the program. The voucher's value is computed based on a multiplication of the subsidy rate by the sum of the tuition, or by the subsidy cap, the lower of the two (see eq. 1 below). The subsidy rate differs by course type and population group (see Table 1). The effective subsidy, i.e., the value of the voucher, which is endogenous to participants' choice-making, was on average, NIS 6,150.

The sum of the subsidy in the relevant period was computed based on the following formula:

$$(1) \textit{Subsidy}_{g,c} = r_{g,p(c)} \times \textit{Min}[\textit{tuition}_c, \textit{cap}]$$

where the subsidy for course c , applied for by an individual who belongs to participants' group $g - g \in (\textit{priority1}, \textit{priority2})$ —equals the product of the subsidy rate r —that is unique for each combination of participants' group g and course type $p - p \in (\textit{preferred}, \textit{not})$ —and the minimum value among the requested tuition and the subsidy cap, which was uniform in the period examined, at NIS 9,000.

3. DATA

The data used in this paper is a unified data set originating from three administrative sources. The first source is the program data provided by the Ministry of Labor, which contains details about the program's participants and their choices in training. The second source is provided

by the Israeli Employment Service (IES) and includes a list of anonymized IDs of unemployed persons who received employment counseling in the years 2013–19, some of whom have also been directed to the vouchers program (see below).⁶ This data also includes the monthly dates of counseling and voucher assignment. The third source includes data on monthly earnings and benefits, and was provided by the National Insurance Institute (NII). The NII data itself incorporates data from several administrative sources including the Israel Tax Authority and the Population Registry.

The comparison that stands at the core of the study was structured as follows. The treatment group includes unemployed individuals who received employment counseling from the IES and were also directed to the vouchers program (the counseling was provided 1 month before or, in most cases, in the very same month of being directed to the program). Specifically, the treatment group includes *all* the people who the IES referred to a voucher in the years 2013–19.

The control group includes individuals who were randomly drawn from the group of unemployed who received employment counseling from the IES without receiving a voucher (the total number of this group is very large). Randomly drawing observations was made within sampling cells, where a cell for this matter is a combination of the local IES office and the month of counseling.

Thus, the raw sample constructed included almost 20,000 individuals who received a voucher from the IES following their counseling (treatment), and almost 10,000 individuals who only received counseling (control). This sample was further restricted to meet a few conditions. Following Huber et al. (2018), the sample was restricted by age (18–61) and, following Doerr et al. (2017), also by unemployment insurance benefits (UIB) status. This means that individuals were included in the sample only if they received UI benefits at least once during the 3 months before receiving employment counseling. From this group, individuals were dropped if they had been directed to the vouchers program by different organizations other than the IES.⁷ Removing individuals by age reduces the sample size by 8%. Further removing individuals by UI status further reduces the sample size by 50% (see Tables 12 and 13 in the appendix).

⁶ The fact that the control group was not drawn from the entire population of the unemployed but only from a subset of individuals who received employment counseling strengthens the comparability of the groups, because all voucher recipients had received employment counseling prior to receiving a voucher. In addition, by restricting the sample to individuals who had undergone counseling, the study excludes, for example, other types of unemployed, such as short-term unemployed who turned to the Employment Service primarily to bridge a temporary drop in income during the transition between jobs; i.e., those among the unemployed who do not face significant barriers to reentry into the labor market.

⁷ This removal was possible because the data provided by the Ministry of Labor include all those who received a voucher during the period in question under the program, whether by the Employment Service (about half the vouchers) or by other agencies.

The justification for restricting the sample based on UI status is to focus on a more homogeneous group of the unemployed, namely, those who have received unemployment benefits for at least a minimum period and are during or at the end of their unemployment spell. In this way, individuals who came only for employment counseling while still employed, and individuals who are no longer eligible for unemployment benefits because they had already exhausted their eligibility period, are excluded from the sample. These two latter groups differ from the group that remained in the analysis in terms of the incentives and constraints they faced at the relevant point in time. Thus, for example, individuals who have exhausted their eligibility are characterized by a higher intensity of job search and stronger liquidity constraints. As a result, their availability and willingness to begin vocational training, and in particular long-term vocational training, are expected to be lower. The justification for restricting the sample based on age is to focus on individuals for whom the training may still bear fruit in the labor market in the future.

Further restricting the sample to individuals whose labor market outcomes can be observed for at least 24 months from counseling brings the final sample to 7,566 observations. In addition, the main analysis focuses on a 48-month horizon, and in some cases, I also refer to the 70-month horizon. Stretching the horizon of analysis leads to smaller samples (Table 3). The month of receiving employment counseling is defined as t_0 . This is also the month when most participants were referred to a voucher (in some cases it occurs 1 month later). The horizons are then computed as the number of months observed in the data, from the month of counseling, so that a 48-month *horizon* refers to all the people for which income data is observed over 48 months from treatment, i.e., 48 months from counseling (based on these constraints the COVID-19-period is not included in the analysis).

The main outcome variables examined are employment and wages. Various background variables are used, including socio-demographic (age, gender, marital status, number of children and ethnicity) and historic labor market characteristics (wages, benefits and number of months employed in the 3 years prior to counseling).

4. DESIGN

The main methodological challenge in this type of study revolves around finding or constructing a relevant control group. Figure 1 shows that for every 1,000 job seekers who were in contact with the Employment Service each month and received unemployment benefits, the number of vouchers provided by the service was, for most of the study period, lower than 4. This means that the probability of receiving a voucher, provided that the individual became unemployed, is rather low.⁸ In such a situation, the existence of a selection

⁸ According to the Employment Service annual report, in 2019, employment counseling was provided to about 40,300 individuals, and vouchers were given to 2,535 individuals. That is, about 6% of those who received counseling, or 1 in every 16 individuals who received

bias depends on the ability and willingness of the IES caseworkers to identify a small and distinct group from among counseling recipients, which is markedly different from the rest of the group, and which includes, for example, the strongest or weakest candidates in the group.⁹

Thus, the validity of the analysis depends on the assumption that individuals in the two groups had similar chances of being referred to a voucher, so that the results are not biased by selection. However, since the study is not explicitly based on random assignment of vouchers to the unemployed, the possibility of selection bias cannot be entirely ruled out. As this is a typical problem in public policy analysis, it is worthwhile considering the incentives faced by caseworkers when thinking about voucher distribution. It is possible that caseworkers tend to give the vouchers to stronger candidates, on the assumption that they are the most likely to show good results after training (*cream skimming*). This option is apparently desirable from the point of view of the organization's reputation. However, in contrast, caseworkers probably realize that giving vouchers to the most competent candidates is problematic from the point of view of a prudent use of public resources, since the strongest candidates are those who are most likely to manage on their own without a voucher. It is difficult to predict a priori which of the considerations mentioned is more dominant, and hence it is difficult to determine a priori to what extent the comparison—without reweighting—is liable to suffer from a selection bias stemming from caseworkers' discretion.

In this context, it is important to note that IES caseworkers were not directly incentivized to distribute vouchers, or to achieve a certain training completion rate. In addition, the number of vouchers distributed by the organization to the unemployed each year is rather low relative to the number of potential recipients, and also relative to the organization's ability to distribute the number of vouchers it receives. Thus, the motivation of caseworkers or excess vouchers are not factors that are likely to have had a direct impact on the way candidates were selected for a voucher.¹⁰

Another possible source of selection bias lies in the unemployed themselves. In this respect, it is important to distinguish between two stages at which self-selection may occur: the stage of voucher allocation and the stage of voucher redemption. As the effect estimated

employment counseling, received a voucher. This ratio is higher than the ratio shown in Figure 1—which includes all those who received unemployment benefits, with and without employment counseling—but it is still relatively small in terms of the ability to identify a suitable subgroup among those who received employment counseling. As noted, those who received employment counseling are a subgroup of those who received unemployment benefits; and those who received a voucher are a subgroup of those who received employment counseling. The control group are those who received unemployment benefits and counseling but not a voucher.

⁹ Hirshleifer et al. (2015) examine the impact of vocational training in Turkey based on excess registration. However, they exploit this situation to randomly assign candidates to training – in advance.

¹⁰ Based on conversations with IES officials.

is the effect of being referred to the program (*intention-to-treat*), and not the effect of voucher redemption (i.e., actual training) self-selection bias is, by construction, irrelevant to the redemption stage.¹¹ That is, if only a certain type of people among the unemployed redeem the voucher, out of all those who received it, this does not bias the estimated effect of receiving the voucher. In contrast, if self-selection occurred at the stage of voucher assignment—that is, if certain types of persons could influence their own chances of being referred to the voucher, e.g., by opposing the issuance of the voucher for them—this may bias the estimated effects.

Against this background, and to address the possibility of selection, the analysis in this study is based on a reweighted matching technique. This matching technique is based on assigning a specific weight to each observation (each individual) in the control group, so that the similarity between the reweighted control group and the treatment group is higher than the similarity between the treatment group and the raw control group, in which the weight of each individual is 1.

Several algorithms have been developed for reweighting observations, and the one used here—*Entropy balancing*—was proposed by Hainmueller (2012). Hainmueller's methodology and algorithm are based on a recursive reweighting of the observations in the control group until the reweighted control group meets the prespecified constraints. The purpose of these constraints is to mimic the distribution of the treatment group, thereby increasing the similarity between the groups. Specifically, for each variable, one can define in terms of which of the moments (in this case: mean, variance, and skewness), some or all of them, the distribution of the control group should resemble that of the treatment group. The reweighted sample is then used to generate descriptive statistics or estimates based on a weighted linear regression. A major advantage of this reweighting method over propensity score matching (PSM) is that the latter is based on matching distributions of a single variable (in the case of PSM it is the propensity score obtained from estimating the probability of receiving treatment based on the various background variables), while the former is based on matching the joint distribution of all background variables.

¹¹ The need to settle for an estimate of the effect of receiving the voucher—rather than the effect of actual training—originates from the quality of the data. While there is indirect evidence in the data for training (based on the records of refund payments to participants), a more thorough examination revealed that this data, as received for the purposes of this study, was not good enough for the desired analysis. In particular, there is uncertainty about the timing of the payments relative to the course, and about the payments in general. A conservative use of the program data suggests that the proportion of those who began their training out of the total number of voucher recipients is close to 75%. The proportion of those who completed the training out of the total number of voucher recipients is somewhat above 50%, but this figure too is of course a lower bound given that it is inferred only indirectly from the data on refund payments, which may be incomplete.

The validity of a reweighting-based analysis depends on the assumption that the set of observed characteristics used for performing the match is an exhaustive set, i.e., that the difference between the groups in terms of observed characteristics also captures the differences in unobserved characteristics that are correlated with the probability of receiving a voucher. If, for example, peoples' cognitive abilities—which are not observed in this dataset—explain the gap between the groups in terms of the probability of receiving a voucher, and if this ability is not correlated with observed characteristics used for weighting, then the estimated results may be biased. Specifically, if individuals with higher cognitive abilities have a lower probability of receiving a voucher than individuals with lower abilities, then the estimated results are biased downward, since in this case the control group is stronger in its abilities relative to the treatment group. In contrast, if individuals with strong ability—or strong motivation—are more likely to receive a voucher, then the estimated results are biased upward. In this case, the treatment group is stronger in terms of employment and earnings potential than the control group, due to ability or motivation, and irrespective of training, and thus the treatment effect may capture the advantage of the treatment group that would have been manifested even in the absence of training.

The control group was matched on a long series of variables, as described in Table 4. These variables include socio-demographic and geographic characteristics, including age and gender, marital status, and region of residence; and characteristics of the individual's employment history, including the economic sector in which the individual was employed prior to unemployment, and income from wages in the periods preceding unemployment.¹²

As noted, the relevant interpretation of the estimated effect is *intention-to-treat*, since the results under consideration are based on a comparison between those who only received employment counseling (the control group) and those who received in addition a voucher (the treatment group), irrespective of whether members of the treatment group actually made use of the voucher and were trained. In short, the estimated effect is in fact the effect of receiving a voucher, rather than the effect of vocational training per se.

The main analysis is based on estimating the following equation:

$$Y_{ist} = \beta_1 Voucher + X_i' \theta + \delta_{st} + \varepsilon_{ist}$$

where Y_{ist} is the outcome variable for individual i who received employment counseling at office s in quarter t . $X_i' \theta$ is a vector of control variables (age, number of children and binary variables for gender, immigrant, married and ever married), and δ_{st} is a fixed effect for IES-office by quarter of counseling, and it therefore reflects the fact that people who exit the labor market in different quarters of the year, and who live in different cities, may be systematically different. The standard errors are clustered at the level of office-by-quarter of counseling.

¹² For the dynamic results shown in the figures that also include the treatment effect of receiving a voucher on various outcome variables before the month of counseling, the creation of the weights did not include employment history variables.

In order to evaluate the quality of the match between the groups, Table 2 presents the balancing in background variables, the variables that were used to create the new weights for the match (in a 48-month horizon sample). Columns 1–6 present the three central moments in the treatment and the control groups before reweighting. After reweighting, the distributions of the control group and the treatment group become more similar, as evidenced in columns 7–8, which show the standardized difference before and after reweighting. The standardized difference is the difference between the means of the two groups in terms of the standard deviation of the relevant variable. In the process of estimating the heterogeneous effect, this process of reweighting was performed each time anew, at the level of the subgroup being examined (e.g., among Jews only, among Arabs only, and so on).¹³

Next, I examine the similarity between the groups by referring to a result that should not have been affected by the treatment. Figure 2 shows the proportion of individuals—in each group—who received unemployment benefits during the 48 months prior to the date of the employment counseling. The fact that employment trajectories in both groups are very similar provides further indication that the reweighting process was done well. This is also an indication that the timing of the artificial treatment of the control group—captured by the common event experienced by both groups (employment counseling)—is appropriate for the analysis, regardless of the reweighting. Further evidence that the groups are comparable lies in the similarity of the employment trends prior to the date of the treatment, which is presented in the figures mentioned below in the *Results* section.

Finally, it must be stressed again that the validity of the reweighting-based analysis depends on the assumption that the observed variables used in the process are a subset of the variables that may be correlated with receiving a voucher. Otherwise, the use of this method may not be enough for the creation of comparable groups, which weakens the possibility of causally interpreting the estimates.

5. RESULTS

In this section, I present the main results for the main sample and for a few subgroups. The main horizon examined is 48 months after treatment, and outcomes are shown for the 70-month horizon sample as well. For some subgroups, shorter horizons are used to examine large enough samples. In addition to estimates after 24 or 48 months, and to outcomes accumulated over such periods, I also analyze the differences between the groups over time.

¹³ It should also be noted that for each, the reweighting was done separately for two series of results. For cumulative results—for which the start period is the month of counseling—employment history variables (i.e., employment before the month of counseling) were included; and for non-cumulative results—for which pre- and post-counseling results were computed—reweighting was done without these variables.

The two main outcomes examined are employment (the probability of being employed in a certain month and the number of cumulative months of employment up to that point) and wages (in a specific month or as accumulated up to that point).

I also estimate the assignment effect on a number of outcomes, all of which are variations built around employment and wages. The variable “ever back” is a binary outcome that equals 1 when the individual is employed for the first-time post-treatment, and it stays equal to 1 from this point on. Thus, it goes beyond the group-level performance because the outcome “employed” is not sensitive to the case in which one person from the group exits the labor market and another one enters.

The second outcome is “never-out.” This outcome is measured relative to the first month of post-treatment employment, when it equals 1. It turns to 0 in the first month of a subsequent non-employment period, and it stays 0 from this point on. It thus measures the strength of attachment to the labor market post-treatment, assuming that more successful cases are associated with longer consecutive employment periods.

The next outcome variables focus on wages. The third outcome is a binary variable that equals 1 for those months in which individuals’ post-treatment wages surpass their highest pre-treatment wage (as documented in the 36 months preceding treatment). This outcome captures whether assignment to training has improved participants’ human capital or at least extended their work intensity in terms of hours worked. The fourth outcome is positive earnings, that is, wages excluding zero, i.e., wages conditional on employment.

Main effect

Table 5 shows the results for the probability of being employed and for wages, 24 months before treatment and 24 and 48 months after treatment, using the main sample. Every cell in the table shows the estimate in the top row, standard errors in the middle row and the control group mean in the bottom row. The different cells in the table reflect different models: with and without control variables, with and without fixed effects for IES office, and quarter-by-office fixed effects (the quarter is the quarter of counseling). Overall, the estimates are pretty similar across the different models.

The results show that pre-treatment estimates for both employment and wages are insignificant. That is, the control and the treatment groups show similar performance in that period, which reaffirms their comparability. Post-treatment estimates for employment are statistically significant and in the range of 4–5 percentage points 24 months after treatment and 3–4 percentage points 48 months after treatment. Relative to the control mean (employment rate of 71–72%), these estimates reflect a 4–7% difference. Estimates for wage (including zero) are statistically insignificant, although this is the case when the comparison is made in a specific month, while the comparison of the groups in the cumulative wage component yields a statistically significant and large effect (see below in the next paragraph and the results shown in Figures 4 and 6).

Table 6 shows estimates for the effect of voucher receipt on employment and wages accumulated over 48 months from treatment (upper panel) and estimates for the same outcomes accumulated after 70 months (lower panel). The results show additional 1.5–2 months of employment relative to 33 months in the control group after 48 months (a 4.5%–6% difference). Estimates for wages are significant in most of the models and reflect a 7%–9% difference. The lower panel of the table shows results 70 months after treatment. In this case, the treatment effect on cumulative employment is around 5–6 additional months, more than a 10% difference relative to the control group mean (47.9 months). A strong and significant effect can also be observed for cumulative earnings (18%–20%). Table 11 in the appendix shows outcomes after 48 months in the two samples: 48- and 70-horizon samples. Overall, in the 70-horizon sample, which is by construction, a smaller sample, the performance of the control group is weaker, hence the treatment effect is stronger.

The fact that the effect of the vouchers on wages is not as stable and robust as the effect on employment suggests that the program's impact was felt through the extensive margin rather than the intensive margin. That is, the program increased the employment rate in the treatment group beyond the corresponding increase in the control group, but has not necessarily caused people in the treatment group to increase their monthly working hours beyond that of the control group. Alternatively, this could indicate that receiving a voucher had a limited effect on job quality as measured by hourly wage. It is difficult to determine which of these possibilities is correct because information on hours worked is not observed.

Figure 3 complements the main analysis and shows employment trajectories for the two groups and the estimated effect over the 8-year period: 48 months before and 48 months after counseling. The results are based on regressions that were ran separately every 3 months. It is evident that prior to the month of counseling, the two groups follow the same trend, and that around 6 to 12 months after being directed to the program, the employment rate among the treated bypasses that of the control group. The timing of the separation is consistent with the training duration of most courses, which indeed falls in the range of 6 to 12 months (see Table 2). The employment gap between the groups reaches a maximum of 8 percentage points difference after 18 months and remains positive and significant, around 4–5 percentage points, up to 48 months after counseling, which means the program had a profound and lasting effect on participants.

Figure 5 complements Figure 3, showing the levels in the treatment and the control groups and the estimated effect for the cumulative number of months of employment. Over the 48 months the treatment effect grows steadily and gains 2 additional months—compared to about 33 months of employment accumulated in the control group.

As for effect on wages, Figure 4 shows there is no significant effect of receiving a voucher on earnings, when measuring earnings at the monthly level. However, over time, the small differences in wages accumulate into a more significant difference—as shown in Figure 6, which shows estimates for cumulative wages over the 48 months after treatment.¹⁴

Figures 9–10 in the Appendix replicate the main analysis for a longer period of 70 months from treatment, for a smaller sample with greater seniority. The results are consistent with the results derived from the main sample, however, they are somewhat stronger in the common period of the first 48 months.

The next two appendix figures examine the effect of vouchers on wages in different manners. Figure 11 shows that there is no significant difference between the control and the treatment groups in terms of the probability of earning wages above historical levels. The share of individuals among both groups that earn wages beyond their historical record (as computed within 36 months pretreatment) is relatively the same, and moves around 10% throughout most of the period examined.

Figure 12 shows trajectories and the estimated effect for positive wages, i.e., wages conditional on employment. The results capture two phenomena. First, mean wage increases toward t_0 and then returns to almost similar levels. This trend is consistent with the possibility that those who lose their job at the latest point relative to counseling are the economically stronger workers who earn more. Hence, as their relative share gradually increases when approaching t_0 , the overall mean increases toward their wage levels. Likewise, as people start going back to the labor market the opposite process occurs: low-wage earners are the last to rejoin the market, so that moving away from the month of counseling, post-treatment wages gradually decrease and stabilize around a level slightly higher than pretreatment. Second, this process looks very similar across the two groups, showing that also when conditioning on employment, receiving a voucher does not trigger any effect on wage levels (note that for this outcome, unlike other outcomes, the sample is not fixed, as people go in and out of the sample).

Finally, Figure 13 in the Appendix shows the cumulative income from UIB during the period following the treatment. The figure shows that the two groups are very similar in this respect so that receiving a voucher has no effect on UIB. This result shows that the rate of exhaustion of UIB is similar across the two groups (as indicated by the trend in the first six months following the counseling); and that the receipt of a voucher did not bring about a change in the chances of existing to unemployment during the period following the treatment.

¹⁴ Note that the impact of the vouchers on employment becomes significant only after 18 months of counseling, as shown in Figure 4. This means that an analysis based on a short-to-medium term outcomes (12–24 months), would not have been sufficient in this case to determine that the program was effective.

Heterogeneous effect

In this section, I present a heterogeneous analysis of the program impact, by different strata: demographic attributes (gender, ethnicity and age), human capital attributes, and employment history. Results in this part show that the effect of receiving a voucher was stronger among individuals with weak labor market attachment, but not among those with the weakest attachment level.

By employment history

Table 7 shows estimates by employment history, which is informative of labor market attachment. Employment history is examined in this case by the number of months employed in the pre-treatment period, out of 36 possible months. The population is thus divided into two groups, according to the median value of pre-treatment employment: people with up to 27 of months of employment, and people with 28 months or more. The results indicate a more pronounced effect of voucher award among those with weaker employment history—3.1 (s.e., 1.3) additional months—compared to those with a stronger pre-treatment employment record who saw only a 1.6 (0.7) increase in months of employment—48 months after treatment. In addition, the estimates of the two groups at 48 months post-treatment are significantly different (at a level of statistical significance greater than 0.05). In other words: individuals with high labor market attachment and individuals with low labor market attachment respond quite differently to the treatment.

The next group examined in this section is income support recipients, and the results for this group are presented in Table 8. The split in this case is between people who received income support at least once during the 12 months prior to the treatment (columns 3–4) and people who did not receive income support at all during that period (columns 1–2).¹⁵

The findings in Table 8 show that income support recipients are a distinct group within the study population, with a very low attachment to the labor market and very low accumulated work experience compared to the rest of the sample, recipients of UIBs. The effect of receiving the voucher on the cumulative number of months of employment among income support recipients is not statistically significant. In contrast, the effect on cumulative earnings from wages is significant and is higher by around 20% compared to the control group's mean. Among individuals who did not receive income support the results are very similar to those obtained for the main sample (see above). In this case, too, there is a significant difference between the groups in terms of the estimates of the main results: employment and earnings accumulated over 48 months.

Figure 14 in the appendix presents the dynamic estimates for income support recipients. The findings show that for this group the effect on employment is modest and insignificant for most of the period; and becomes statistically significant only after 3.5 years from the

¹⁵ The non-income-support recipients' group is very similar to the main sample, which is defined by receiving UIB, which cannot be received concurrently with income support.

receipt of the voucher. As for survival in the labor market, and regarding the probability of earning above the historical personal peak, receiving a voucher had no significant effect among income support recipients. However, regarding the outcome variable which examines whether the individual has ever been employed in the period following the treatment, the results show a significant effect of between 5 and 8 percentage points—24 and 48 months after treatment.

By human capital and demographic attributes

Columns 2–3 in Table 9 present the results by level of education, defined here by a binary variable that equals 1 for individuals who appeared in the register of post-secondary education, and 0 otherwise. Many of these individuals did not complete their academic studies, and therefore this variable corresponds to the commonly used category of *any-college* in American set-ups. The results show a stronger treatment effect on employment among individuals without any post-secondary education, compared to the group of those with some college: 3.2 additional months of employment among the former, compared to only 0.6 among the latter. Figure 7 and Figure 8 present the dynamic results among these groups—for the probability of being employed and for cumulative employment, respectively. Notably, among those with some college education, both the employment effect and the cumulative employment effect hover around zero throughout the entire period examined.

The remainder of Table 9 presents results for the effect of receiving a voucher on the main outcome variables, by gender, ethnicity, and age-group. The effect on cumulative employment is stronger among women than among men, and among those aged 40 and over than among younger individuals. At the ethnicity level, the long-term effect among Jews is statistically significant and of considerable magnitude: an additional 2 months (compared to 33 months in the control group). Among Arabs the effect is stronger, although not statistically significant, probably due to the small sample size. Indeed, when a shorter analysis horizon is used, which includes a larger enough sample, the treatment effect on cumulative months of employment, in the medium term, is statistically significant and stronger among Arabs than among Jews (see Table 10). In all the four strata presented in the table, the difference between the two groups (e.g., between those with some college and those without) is significant and of large magnitude.

Table 10 presents results—for a 24-month horizon—among Arabs and Jews, and also for another social group whose program's outcomes are of particular importance, ultra-Orthodox Jews (Haredim). The results in the table show that while the program had a significant effect on employment and earnings among Arabs, it had no effect on employment and earnings among ultra-Orthodox Jews, although the small sample requires some caution when drawing conclusions regarding this group.

Cost-benefit

The treatment effect on wages after four years, shown in Table 6, is in the range of 2,465–3,252 NIS, hence mid-range value is 2,859. In contrast, the average subsidy was 6,150. This means that after 4 years, 46% of the costs per participant are paid back in the form of increased earnings. Based on a naïve imputation that assumes a linear trend of the treatment effect, the gains associated with the program should surpass the direct costs after 103 months (8.6 years). That is, the program is cost-effective only in the very long term, if effectiveness is only referred to in monetary (wage-based) terms.

Nevertheless, the analysis is based on intention-to-treat estimates, i.e., on comparing receiving to not-receiving a voucher, rather than on comparing being-trained to not-being trained. This implies that the numbers mentioned above may reflect underassessment of the program's benefits (considering the effect of training itself on wages). Hence, if the share of voucher recipients who started training is 75%, program costs can be covered after 77 months (6.4 years) from treatment.

Finally, increased employment means additional savings of UI benefits and income support that may accumulate over time, which further increases gains relative to costs. This element also means that the program is more cost effective than described above.

6. DISCUSSION

In this section, I position the findings in light of the existing literature, and thus try to explain the differences between groups regarding the program's impact.

The discussion is constructed around an overarching question: What are ALPMs good for? Does the added value of these programs lie—solely or mainly—in minimizing the damages caused to individuals by layoffs and other life cycle and business cycle events? Or can they also improve and increase productivity, employability and employment stability beyond pre-treatment levels, thereby turning “a crisis into an opportunity”, at least at the individual level?

The main results show that training vouchers are effective for the unemployed, and that they trigger an increase in employment that lasts over a long period: increase of 5 percentage points after 2 years, and 4 percentage points after 4 years. These effects are close to and slightly below the range documented in the meta-analysis of [Card et al., \(2018\)](#),¹⁶ which, for the relevant categories—training for the unemployed in the medium and long term—documented a 6–7 percentage point increase. Relative to more similar setups of long-term voucher studies, it falls within the range of the findings by [Doerr et al., \(2017\)](#) that document

¹⁶ In their meta-analysis, Card and his colleagues find that the average effect of vocational training programs on employment is ~6.6 percentage points (for the medium and long term), and that among the unemployed (across all types of programs) the effect ranges from 4.3 to 8.5 percentage points.

a 2 percentage points increase after 48 months and no effect on earnings, and [Doerr \(2022\)](#) that documents a 7 percentage points increase after 48 months, and a modest effect on earnings.¹⁷

The effect on wages is rather limited or statistically insignificant, although it does become significant over time when considering cumulative wages. If the employment rate increases more strongly among the treated relative to the control, but wages (that include zero wage) increase in similar rates among the two groups, then, on average, wages among the treated increase by a lower rate (when excluding zeros). This implies that training has benefited assignees via additional employment in partial or low-quality jobs; or that the program's effect was largely driven by low-skilled participants and by participants with lower attachment to the labor market.

The heterogeneous effect lends support to this last possibility that the program indeed had a stronger impact among those with lower attachment to the labor market. Specifically, a stronger effect among women relative to men, those without college relative to those with some college, and among the long-term relative to the short-term unemployed is in line with findings in the literature that document higher returns for ALMPs, and training in specific, among women and the low-skilled ([Card et al., 2018](#)). One explanation for this pattern is that the latter are characterized by lower labor market attachment, so they are more likely to benefit from reintegration programs such as training. In contrast, men and the highly skilled are more likely to get along on their own and reintegrate in the labor market without external help.

However, the relation between labor market attachment and program effectiveness is not monotonic: The impact of the program among the group with the weakest labor market attachment—income support recipients—is rather limited compared to other groups. This gap suggests that some minimal level of attachment to the labor market is necessary, below which training is less effective, if anything. This is consistent with the notion of training as a means of *restoration*, because there must be some basic level of human capital that can be restored. In contrast, income support recipients who experienced very long periods outside the labor market may be characterized by *hysteresis* (i.e., an erosion of the very basic employment skills due to long periods without any job) and hence need support in strengthening more basic and soft skills ([Schlosser and Shanan, 2022](#)).¹⁸

In addition to a number of conventional outcomes, I have also examined the probability that individuals will earn above their pretreatment wages. As opposed to [Doerr \(2022\)](#) who finds a significant effect for a similar outcome, I do not find any evidence that vouchers had

¹⁷ In contrast to these studies, I find no clear evidence of a lock-in effect. The difference may be explained by the fact that the types of training examined in this study are mostly short-term courses of several months' duration, some of which can be completed while working part-time.

¹⁸ The weak labor market attachment hypothesis may also be considered in the case of the ultra-Orthodox Jews, a minority group characterized by low labor force participation rates.

any impact in this regard. The difference may be attributed to the different populations examined (unemployed here, versus female “job-returnees” in Doerr’s paper), and to the intensity of the training (short training in this paper versus training of various durations, including long trainings, in Doerr’s paper).

Therefore, it seems that even when focusing on the populations who benefit the most from it, short subsidized training for the unemployed is first and foremost a means of restoration and only to a lesser extent can it be regarded as a human capital enhancer that can increase labor market performance beyond pretreatment levels. (As noted, this conclusion should also be seen in light of the fact that the effect estimated is the Intention-to-treat, which examines the effect of receiving the voucher, rather than the direct effect of the training itself). In addition, short training seems to have limited effects for those with a very low labor market attachment. These empirical observations can guide optimal policymaking in locating what subgroups among the unemployed should be directed to training, given the constrained resources for training subsidies.

7. CONCLUSION

This paper examines the long-term effects of subsidized vocational training on the labor market outcomes of the unemployed. I use reweighting-based matching to assess the medium- and long-term effects of being assigned to the Vocational Training Vouchers Program, on the labor market outcomes of a group of unemployed that were treated by the Israeli Employment Service. Specifically, I do so by comparing the outcomes of two groups of the unemployed: employment counseling recipients that were assigned a voucher and a random sample of individuals who also received employment counseling but did not receive a voucher.

I find statistically significant and positive effects on employment that remain stable up to 6 years after treatment. In addition, there is a positive effect on participants’ cumulative wages.

The effects are pronounced among women, low-skilled workers, and for people with low labor market attachment, which is consistent with parallel findings in the literature. However, while labor market attachment seems to be negatively correlated with program effectiveness, this is not true for every level of attachment. Those with very low labor market attachment—for example, income support recipients—seem to benefit less relative to the rest, which may be due to lacking basic and generic soft skills.

In sum, the program was found to be effective primarily in terms of increasing employment rates among voucher recipients, relative to the control group. However, there was no significant leap in terms of wages. This is consistent with the more pronounced effect among individuals with relatively low labor market attachment. The vocational training programs examined, which were mostly short-term training, contributed to increasing the

likelihood of unemployed individuals returning to employment but did not increase their human capital beyond its level at the outset—at least when deduced from wages.¹⁹

Future research can advance in several directions. First, examining the impact of vouchers in Israel relative to non-subsidized or non-voucherized training programs, in order to assess the importance of the financial scheme of the program, independent of the training itself. A similar analysis was conducted in the case of the German voucher program (Rinne et al., 2013; Doerr and Strittmatter, 2021), and a replication of such an analysis in another country could advance the understanding of researchers and governments regarding the best way to operate and finance training programs in general, and in particular when serving unique population groups.

Second, a focus on the impact of training and activation programs on the employment stability of individuals. In this context, the amount and frequency of individuals' transitions between firms, and the patterns of transitions between different industries, can be considered—especially because these results are important for the wellbeing of individuals and their ability to increase their income potential in the longer term.

Third, it is worthwhile examining the impact of vocational training programs, subsidized and unsubsidized, on workers in Israel, in addition to the unemployed population, given the low unemployment rate that has prevailed in the country in recent years, and given the various motivations for increasing labor productivity, also by improving the human capital of the country's salaried workers.

¹⁹ Since wage level is also affected by supply and demand trends (for certain professions), it is possible to imagine a situation in which the quality of the human capital of voucher recipients has indeed risen considerably, but at the same time the supply of workers in the professions and industries in which this group is employed has also increased, which leads to a restraint of wages.

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Table 1
Subsidy rate by population type and course type

Population group*	Profession in demand	Other professions
Group A	85%	80%
Group B	80%	75%

* Group A includes ultra-Orthodox men, Arab women, individuals with disabilities and Ethiopian immigrants; Group B includes the rest of the program's target populations, including ultra-Orthodox women, Arab men, at-risk youth and other groups that reach the program through the Israeli Employment Service, such as those receiving unemployment benefits and those receiving income support who are not included in the other groups. "Preferred in demand" – refers to a closed list of professions that were defined by the Labor ministry as having a special importance; and which, due to this, will receive higher subsidization than other professions.

Table 2
Course distribution (vouchers for which there is information)

Field	N	Share	Mean course period (in months)
Administration	4236	29.1%	9.7
Cosmetics	2869	19.7%	6.4
Transportation	2006	13.8%	3.6
Computers and Technology	1316	9.1%	7.7
Education and Child Care	1202	8.3%	8.6
Photography and Production	592	4.1%	8.4
Fashion and Textile	447	3.1%	8.7
Construction & Environment	412	2.8%	7.8
Electronics and Electricity	378	2.6%	9.4
Metal and Machinery	362	2.5%	5.1
Hospitality and Hotels	210	1.4%	5.6
Other	210	1.4%	6.6
Paramedical	118	0.8%	8.8
Automotive	74	0.5%	9.6
Sales and Marketing	68	0.5%	6.4
Rescue (Sea and Pool)	28	0.2%	3.9
Jewelry Making	12	0.1%	2.7
Total	14,540	100.0%	7.52

Note: Partial data are presented - based on vouchers for which information exists. For most of the fields, there was not a severe shortage of workers during the period, except for the field of transportation, in some specializations (buses and trucks).

Table 3a
Sample selection

Group	<i>N</i>
Raw sample (voucher recipients in 2013-2019 and a random sample of employment counseling recipients)	29,723
Excluding individuals that appeared in the control group (in the IES data) and also in the program data (of the Labor ministry)	29,565
Of which: 18–62 year olds not referred to the program via other channels	27,206
Of which: received unemployment benefits at least once during the three months preceding the employment counseling	11,640
Of which: have a 24-months horizon of analysis from the month of counseling	7,566

Table 3b
Samples used in the study

<i>Sample horizon in months</i>	<i>N</i>	<i>Treatment</i>	<i>Control</i>	<i>Youngest cohort of unemployed in sample*</i>
24	7566	5276	2290	December 2017
48	3922	2782	1140	December 2015
70	1349	954	395	December 2013

Note: This table describes the filtering of the sample that stands at the center of the study, and the size of the samples according to the main horizons used.

* "The Young Unemployed Cycle" – does not refer to the age of the unemployed but to the month in which they received employment counseling at the Employment Service. The tenure of the group therefore also determines the analysis horizon, i.e., the time during which the results for the sample members from this month can be observed until December 2019 – subject to the fact that all the members of the sample are observed for a similar-sized time window. Thus, the sample that includes the youngest group who reached the IES in December 2017 was observed for 24 months, and so on.

Table 4
Balancing (for 48-months sample)

	Mean		Variance		Skewness		Standardized difference	
	Treatment	Control	Treatment	Control	Treatment	Control	<i>Pre</i> weighting	<i>Post</i> weighting
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Demography</i>								
Age	38.92	38.33	71.4	70.3	0.23	0.36	0.07	0.00
Woman	0.63	0.57	0.23	0.25	-0.56	-0.28	0.14	0.00
Migrant (1989+)	0.21	0.18	0.16	0.15	1.44	1.68	0.07	0.00
Married	0.66	0.63	0.23	0.23	-0.7	-0.6	0.05	0.00
Ever married	0.83	0.75	0.14	0.19	-1.74	-1.18	0.197	0.00
Children	2.08	1.86	2.59	2.71	1.03	0.94	0.13	0.00
<i>Employment history</i>								
Wages (-24,-13)	9947	10737	5.44E+07	1.04E+08	2.86	5.28	-0.11	0.00
Wages (-36,-25)	9228	9969	5.77E+07	1.16E+08	2.83	9.47	-0.097	0.00
<i>Economic branch</i>								
Industry	0.14	0.12	0.12	0.11	2.13	2.31	0.04	0.00
Water & Electricity	0.00	0.01	0.00	0.01	13.51	12.6	-0.01	0.00
Construction	0.07	0.05	0.06	0.05	3.47	3.97	0.05	0.00
Commerce	0.20	0.16	0.16	0.14	1.53	1.82	0.08	0.00
Transportation	0.03	0.03	0.03	0.03	5.39	5.13	-0.02	0.00
Hospitality	0.04	0.05	0.04	0.05	4.78	4.09	-0.06	0.00
Info. & commun.	0.06	0.09	0.06	0.09	3.58	2.77	-0.13	0.00
Financial services	0.04	0.04	0.04	0.04	4.52	4.51	0.00	0.00
Real estate	0.01	0.01	0.01	0.01	10.00	8.86	-0.03	0.00
T technical services	0.10	0.12	0.09	0.11	2.68	2.29	-0.08	0.00
Administration	0.07	0.08	0.07	0.07	3.25	3.21	-0.01	0.00
Gov. & local admin.	0.03	0.03	0.03	0.03	5.64	5.28	-0.02	0.00
Education	0.04	0.03	0.04	0.03	4.48	5.28	0.05	0.00
Health & welfare	0.06	0.05	0.06	0.05	3.64	3.97	0.04	0.00
Art & Leisure	0.02	0.02	0.02	0.02	7.42	7.77	0.01	0.00
Other services	0.04	0.03	0.04	0.03	4.99	5.28	0.02	0.00

Table 4 (continued)										
<i>Residential district</i>										
Jerusalem	0.08	0.06	0.08	0.06	3.05	3.75	0.09	0.00		
North	0.19	0.15	0.15	0.13	1.60	2.00	0.11	0.00		
Haifa	0.19	0.14	0.15	0.12	1.62	2.04	0.11	0.00		
Center	0.19	0.26	0.15	0.19	1.58	1.10	-0.18	0.00		
Tel-Aviv	0.10	0.21	0.09	0.17	2.68	1.40	-0.38	0.00		
South	0.20	0.15	0.16	0.13	1.47	1.93	0.13	0.00		

* This table shows for each variable the central moments in the treatment- and the control group before reweighing, and the standardized difference between the groups before and after reweighing.

Table 5
Estimates for employment and wages at three points in time (48-months horizon)

Months	Employment					Wage income				
To Treat.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
-24	0.006 [0.011] <i>0.863</i>	0.005 [0.011] <i>0.863</i>	0.005 [0.015] <i>0.863</i>	0.001 [0.015] <i>0.863</i>	0.001 [0.015] <i>0.863</i>	-20 [28] <i>790</i>	-18 [27] <i>790</i>	-36 [38] <i>790</i>	-33 [38] <i>790</i>	-40 [39] <i>790</i>
24	0.047*** [0.014] <i>0.706</i>	0.047*** [0.014] <i>0.706</i>	0.053*** [0.015] <i>0.706</i>	0.053*** [0.017] <i>0.706</i>	0.051*** [0.017] <i>0.706</i>	1 [33] <i>709</i>	3 [33] <i>709</i>	0 [43] <i>709</i>	-7 [44] <i>709</i>	-17 [48] <i>709</i>
48	0.030** [0.014] <i>0.72</i>	0.029** [0.014] <i>0.72</i>	0.037** [0.016] <i>0.72</i>	0.042** [0.021] <i>0.72</i>	0.042** [0.021] <i>0.72</i>	10 [36] <i>772</i>	9 [36] <i>772</i>	10 [38] <i>772</i>	29 [40] <i>772</i>	24 [44] <i>772</i>
N	3922	3922	3919	3902	3902	3922	3922	3919	3902	3902
Controls		X	X	X			X	X	X	
Office FE			X					X		
Office-by-Quarter FE				X	X				X	X

Note: This table shows the results of the main sample (48-months horizon) for three different distances, two years before, two years after, and four years after the employment counseling. Employment in this table is a binary variable measured monthly and equal to 1 in those months in which the individual received a wage, and 0 otherwise. The wage income is the monthly income of the individual, including a zero income.

*p<0.1 **p<0.05 ***p<0.01

Table 6
Estimates for cumulative employment and cumulative wages in two time-horizons
(48 & 70 months)

		Cumulative outcomes (0,48)									
		Cumulative months of employment					Cumulative wage income				
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
TE	1.54*** [0.46] 32.86	1.81*** [0.46] 32.86	1.98*** [0.53] 32.86	2.12*** [0.62] 32.86	2.02*** [0.62] 32.86	2,499*** [885] 34014	3,252*** [847] 34014	3,061** [1247] 34014	3,111*** [1123] 34014	2,465* [1270] 34014	
N	3922	3922	3919	3902	3902	3922	3922	3919	3902	3902	
		Cumulative outcomes (0,70)									
		Cumulative months of employment					Cumulative wage income				
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
TE	5.01*** [1.06] 46.6	5.01*** [1.06] 46.6	5.01*** [1.92] 46.6	4.87** [2.04] 46.6	4.89** [2.03] 46.6	6,095*** [2096] 49441	6,095*** [1962] 49441	6,016* [3462] 49441	7,478*** [2706] 49441	6,735** [2935] 49441	
N	1523	1523	1518	1475	1475	1523	1523	1518	1475	1475	
Controls		X	X	X			X	X	X		
Office FE			X					X			
Office-by-Q. FE				X	X				X	X	

Note: This table shows the results of the main sample (48-month horizon) and a smaller sample with a 70-month horizon; and for each sample at the maximum possible distance, 48 and 70 months, respectively, from the month of employment counseling. Employment in this table is the cumulative number of months of employment until each month. The cumulative months are the months in which the individual received a salary. Salary income is the cumulative monthly salary income of the individual, until each month.

*p<0.1 **p<0.05 ***p<0.01

Table 7**Estimates for cumulative employment and cumulative, by employment history**

Sample	28 months or more (of 36)		27 months or less (of 36)	
	(1)	(2)	(3)	(4)
Analysis horizon	24	48	24	48
& outcome timing	(1)	(2)	(3)	(4)
Cumulative months of employment	0.78*** [0.286] 16.1	1.58** [0.738] 34.5	0.85* [0.481] 14	3.10** [1.292] 29.5
Cumulative wage income	665 [505] 18432	684 [1638] 38516	1,544** [710] 13591	2,947 [1823] 27113
N	5194	2699	2342	1152

Note: Comparison of the estimates in the two groups—with respect to the main outcome variable: cumulative months of employment—reveals that the difference between them is significantly different from 0 at the $p < 0.001$ level.

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table 8
Estimates for cumulative employment and cumulative, by income support status

Sample	Have not received income support in (-12,-1)		Received income support in (-12,-1)	
	24	48	24	48
Analysis horizon and outcome timing	(1)	(2)	(3)	(4)
Employed	0.052*** [0.013] .693	0.037* [0.021] .733	0.071** [0.028] 0.449	0.081** [0.033] 0.483
Ever employed post-treatment	0.036*** [0.009] .886	0.027*** [0.010] .93	0.087*** [0.025] 0.667	0.084*** [0.021] 0.775
Cumulative months of employment	0.932*** [0.237] 15.26	2.076*** [0.641] 32.64	1.025* [0.59] 9.54	1.503 [1.429] 20.25
Cumulative wage income	1,527*** [431] 16363	2,549* [1339] 34099	1,257*** [480] 5638	2,231** [1012] 12390
<i>N</i>	7383	3801	3405	1965

Note: This table shows estimation results for four different outcome variables analyzed for the population of income support recipients. The results are presented in two time horizons (24 and 48 months from the month of counseling), and for each sample at the maximum possible distance. The four upper results are as described in the previous tables. It should be noted that the sample of income support recipients is an exceptional sample in that it is not restricted by the condition of having received unemployment benefits during the three months prior to the vocational counseling (and not by UIB receipt). Comparing the estimates in the two groups – with respect to the main outcome variable: cumulative months of employment – reveals that the difference between them is significantly different from 0 at the $p < 0.001$ level.

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table 9
Estimates for cumulative employment and cumulative, in sub-groups
(48-months horizon)

Outcome variable	Full Sample	Post-seoncdary education		Gender		Nationality		Age	
		None	Any	Men	Women	Arabs	Jews	<40	40+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(9)	(10)
Cumulative months of employment	2.02*** [0.62] 32.86	3.20*** [0.890] 31.57	0.61 [1.040] 34.47	1.79** [0.89] 33.98	2.65*** [0.87] 32.03	3.62 [2.46] 28.24	2.12*** [0.68] 33.02	1.64** [0.81] 33.13	3.55*** [1.2] 32.34
Cumulative wage income	2,465* [1270] 34014	3,282** [1380] 30098	4,440* [2460] 40176	4,722** [1976] 44088	2,023 [1444] 28079	1,809 [4611] 28951	2,918** [1370] 34555	1,078 [1484] 33518	4,639* [2370] 34417
N	3902	2525	1323	1461	2384	557	3313	2240	1603
Office-by quarter FE	X	X	X	X	X	X	X	X	X

Note: See note to Table 6. In the course of the research, the category "Jews" includes the subgroup of migrants from the former Soviet Union who are not defined in the official data as "Jews." Comparing the estimates for all four pairs—with respect to the main outcome variable: cumulative months of employment—shows that the differences within each pair are significantly different from 0 at the level of $p < 0.001$.

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table 10
Estimates for main outcomes, by population groups (24-months horizon, 24 months from treatment)

	Arabs	Ultra-orthodox Jews	Non-Ultra-orthodox Jews
Employed	.113** [.046] 0.605	-0.036 [.06] 0.724	0.039*** [0.014] .713
Ever employed post-treatment	0.068** [0.034] 0.867	-0.001 [0.029] 0.915	0.033*** [0.009] .886
Cumulative months of employment	1.730** [0.845] 13.83	0.571 [0.864] 14.23	0.769*** [0.251] 15.63
Cumulative wage income	4,182*** [1069] 12198	1,824* [985] 13418	904* [501] 17441
<i>N</i>	1172	615	5677

Note: See note to Table 8. For each of the populations, the estimates for four different outcome variables are presented. In the course of the study, the category "non-ultra-Orthodox Jews" includes the subgroup of migrants from the former Soviet Union who are not officially defined as "Jews" in the official data.

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Figure 1
Vouchers allocated by the Israeli Employment Service, per 1,000 recipients of unemployment benefits

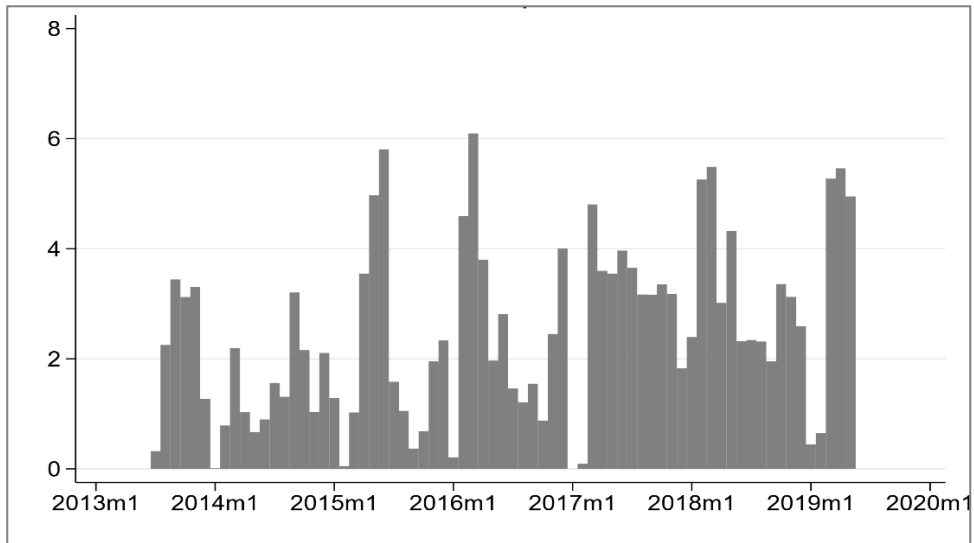


Figure 2
The probability of receiving UIB in the treatment and control groups, relative to the month of counseling

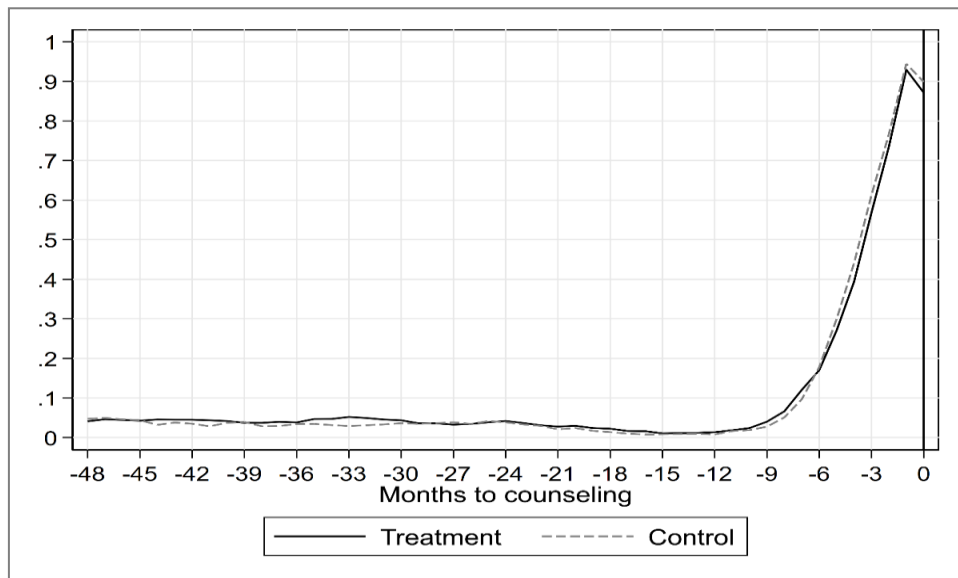


Figure 3

The probability to be employed – levels and estimated treatment effect

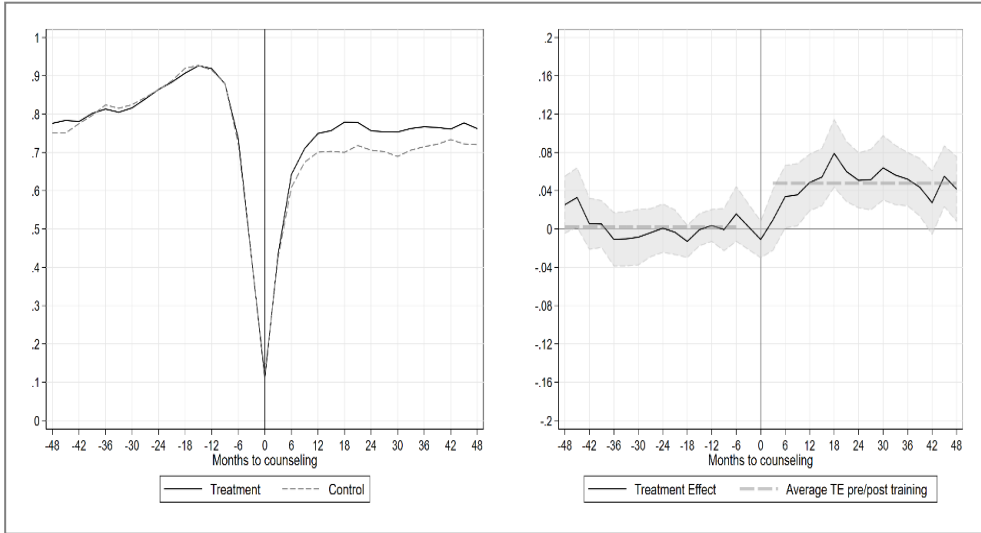


Figure 4

Wage-based income – levels and estimated treatment effect

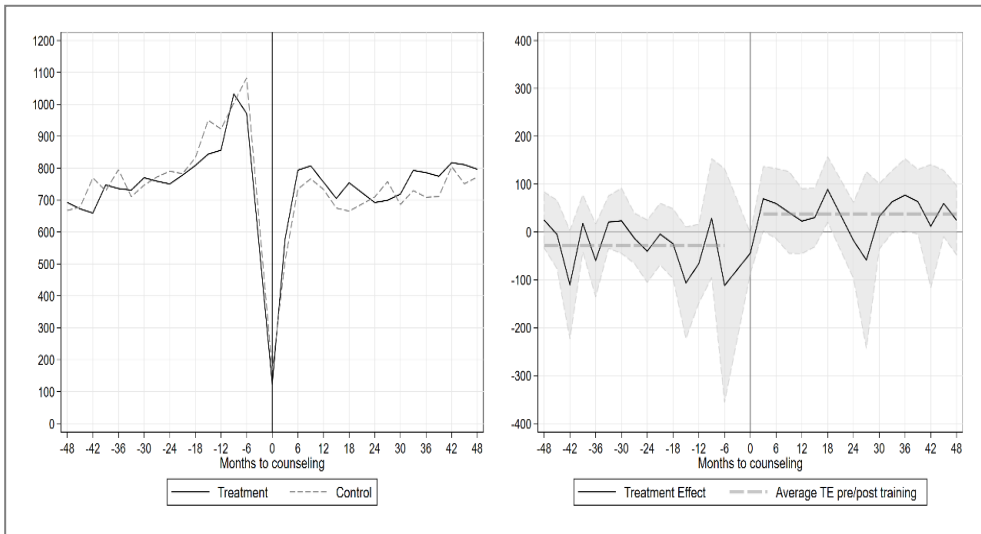


Figure 5
Cumulative months of employment – levels and estimated treatment effect

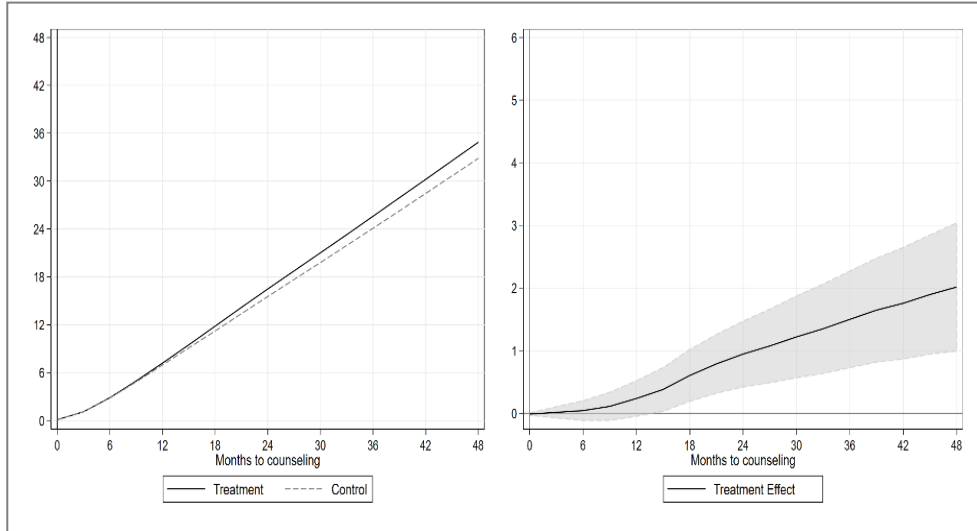


Figure 6
Cumulative wage-based income – levels and estimated treatment effect

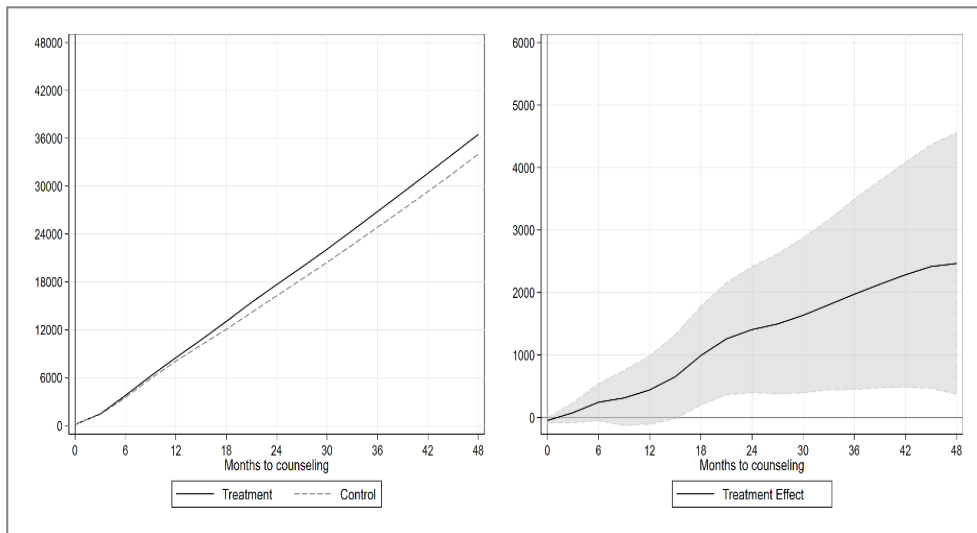


Figure 7a

The probability to be employed, by post-secondary education: Some college

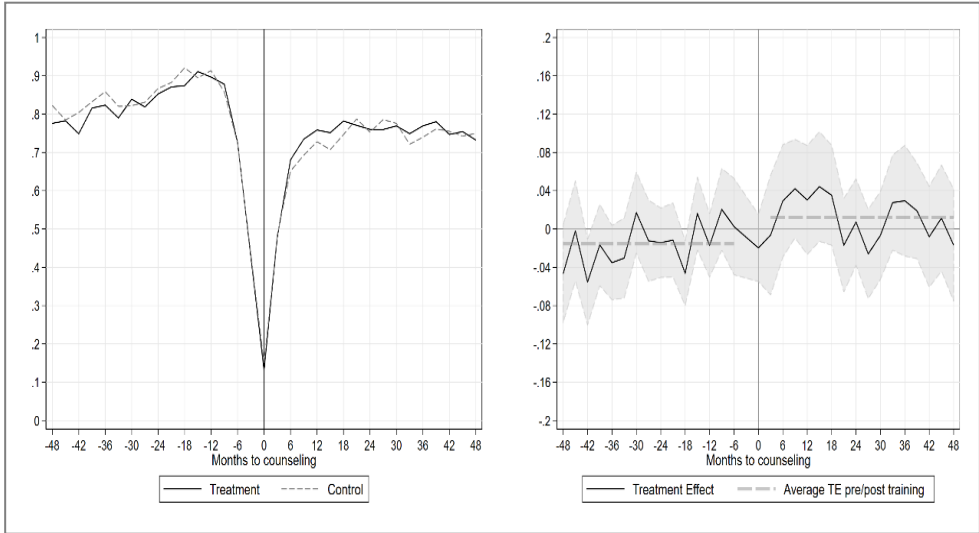


Figure 7b

The probability to be employed, by post-secondary education: None

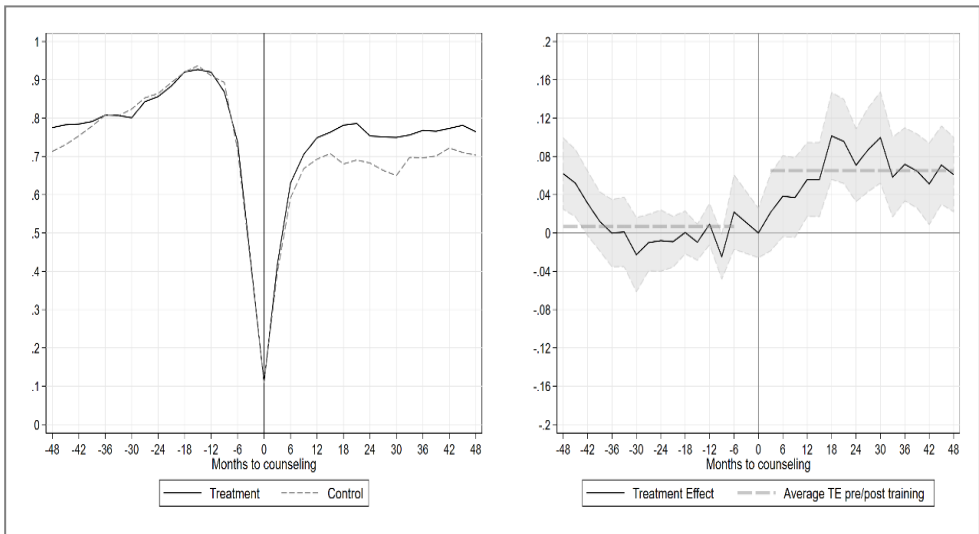


Figure 8a
Cumulative months of employment, by post-secondary education: Some college

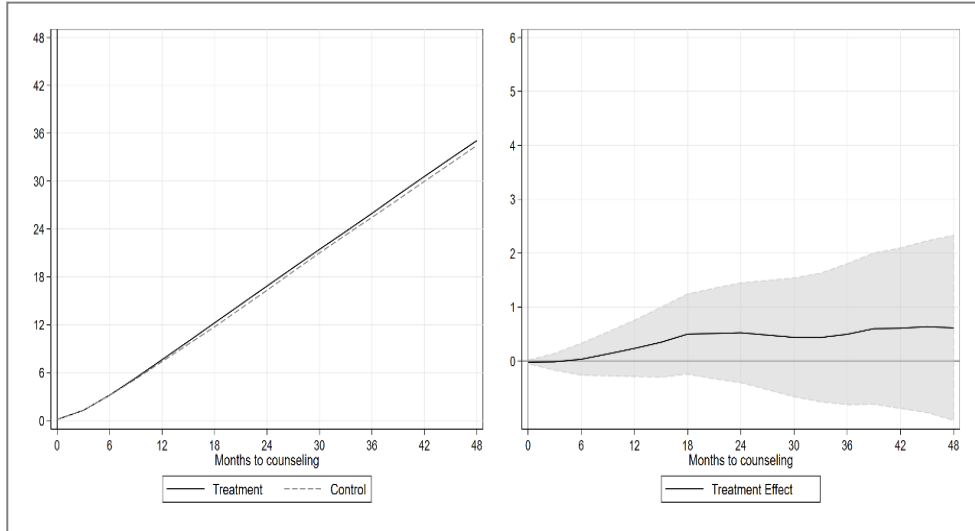


Figure 8b
Cumulative months of employment, by post-secondary education: None

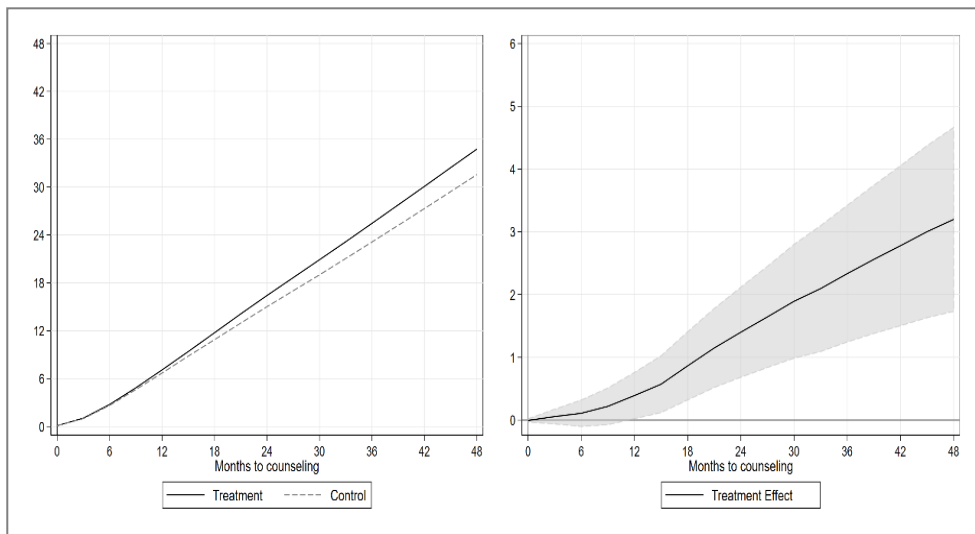


Figure 9
The probability to be employed, over 140 months – levels and estimated effect (70-months sample)

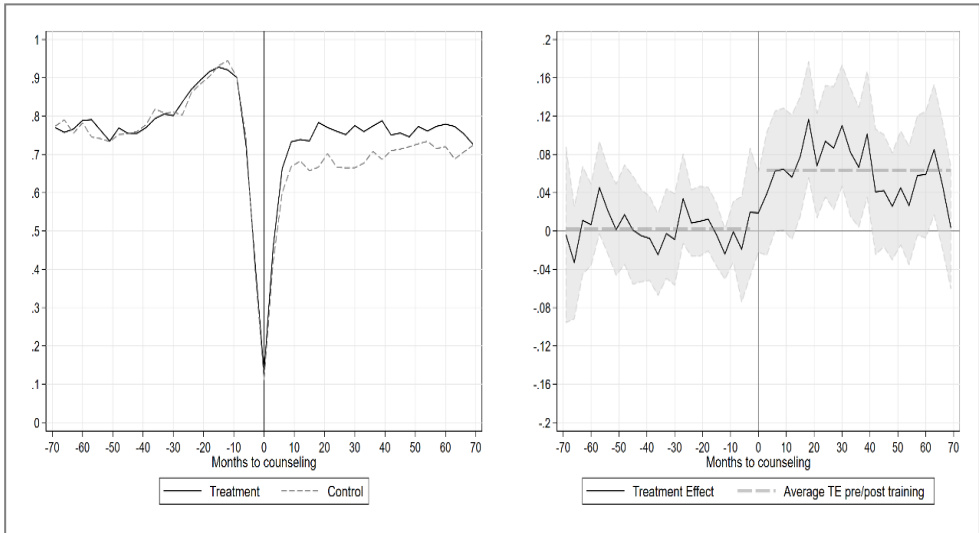


Figure 10
Cumulative months of employment – levels and estimated effect (70-months sample)

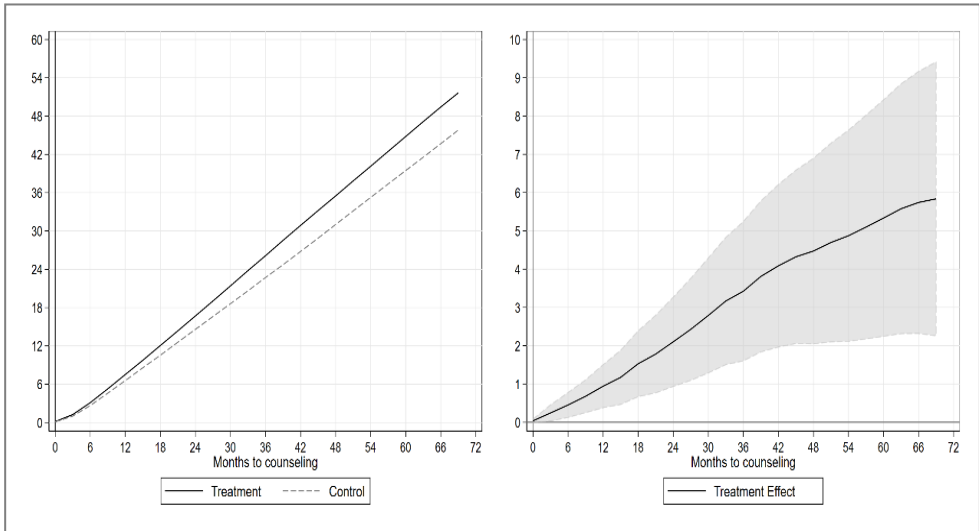


Table 11

Estimates for main outcomes in 48-months (48 months from treatment) and 70-months horizons (48 and 70 months from treatment)

	48-months horizon		70-months horizon			
	Cumul. outcomes (0,48)		Cumul. outcomes (0,48)		Cumul. outcomes (0,70)	
	(1)	(2)	(3)	(4)	(5)	(6)
Cumulative months of employment	2.12*** [0.62] 32.86	2.02*** [0.62] 32.86	3.650*** [1.340] 31.29	3.630*** [1.340] 31.29	4.87** [2.04] 46.6	4.89** [2.03] 46.6
Cumulative wages	3,111*** [1123] 34014	2,465* [1270] 34014	5,015*** [1851] 32893	4,516** [2023] 32893	7,478*** [2706] 49441	6,735** [2935] 49441
N	3902	3902	1475	1475	1475	1475
Controls	X		X		X	
Office-by-Quarter FE	X	X	X	X	X	X

*p<0.1 **p<0.05 ***p<0.01

Figure 11

The probability to earn above pre-treatment record – levels and treatment effect

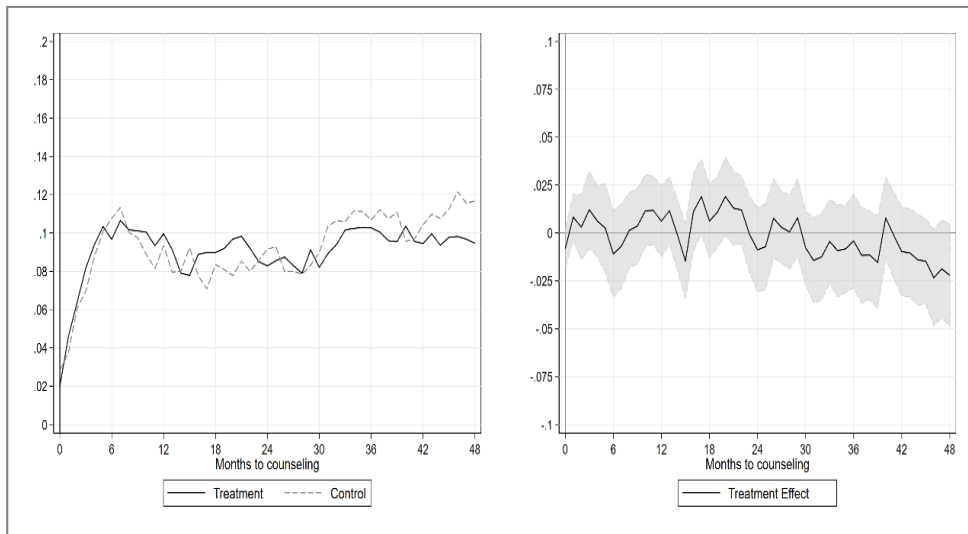


Figure 12
Positive wage – levels and treatment effect

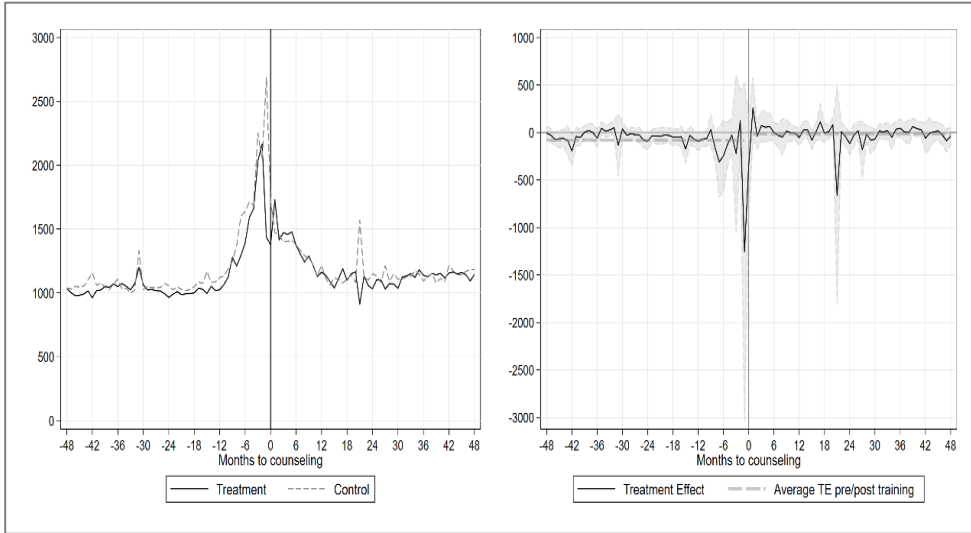


Figure 13
Cumulative UIB income – levels and treatment effect

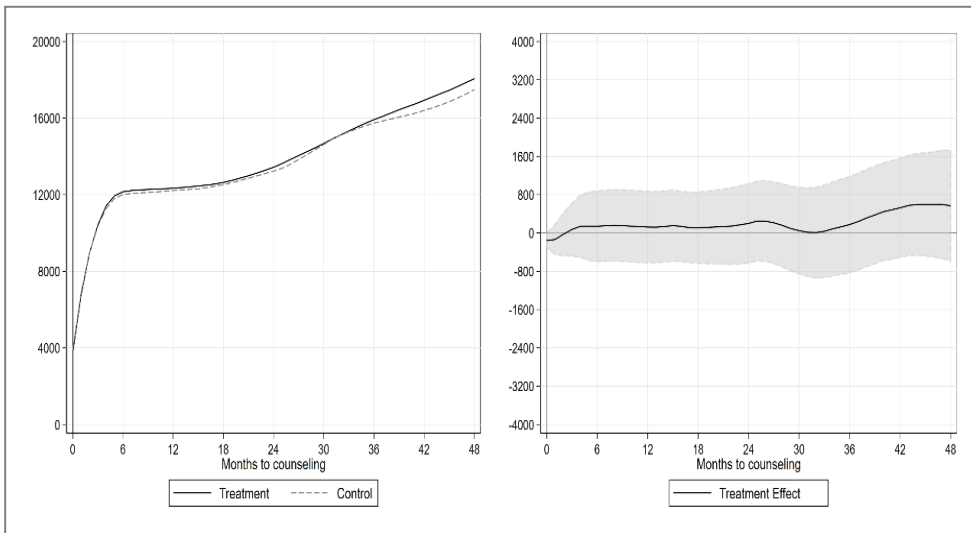
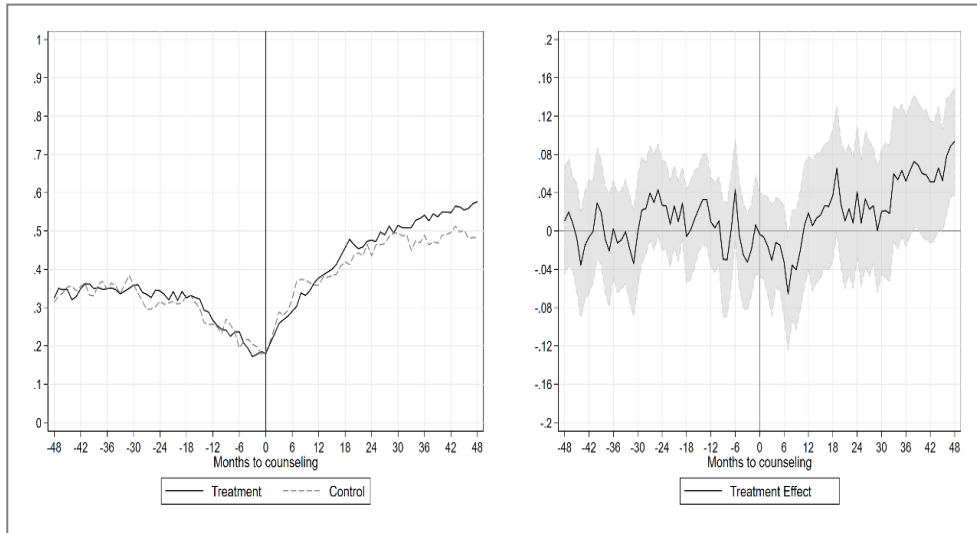
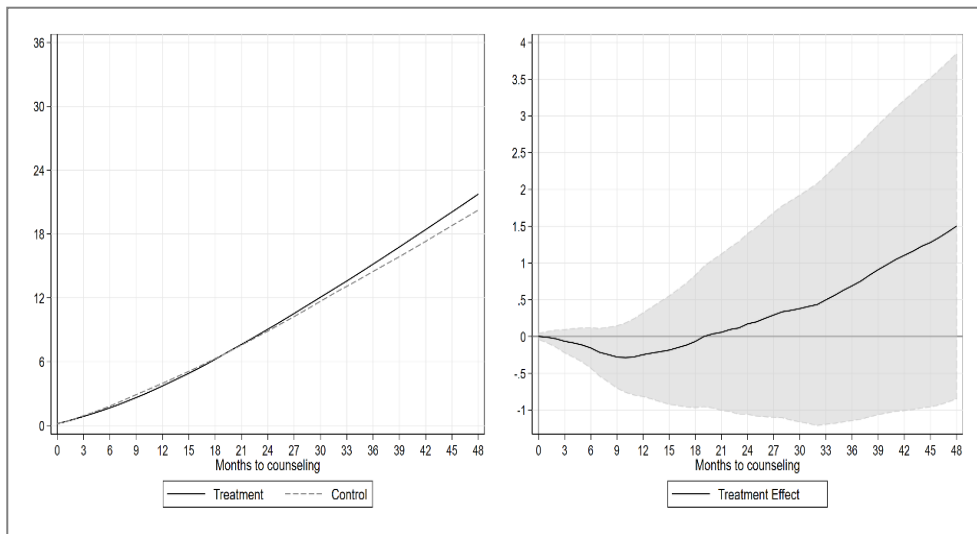


Figure 14

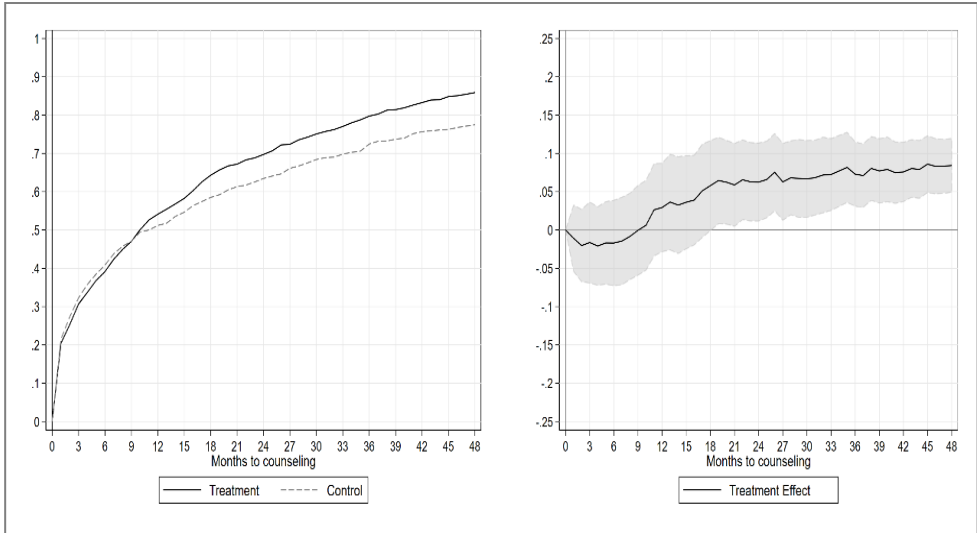
a. Main outcomes among income support recipients - employment



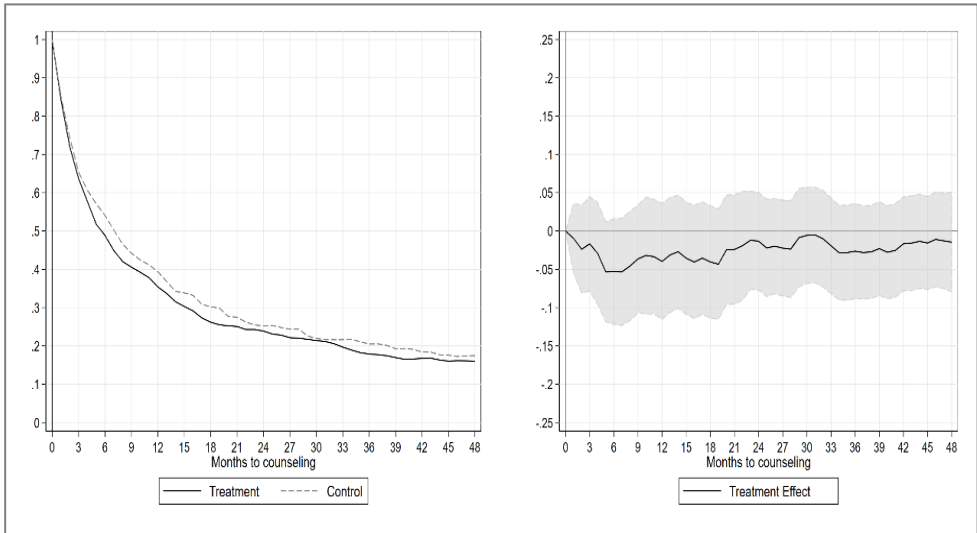
b. Main outcomes among income support recipients – cumulative months of employment



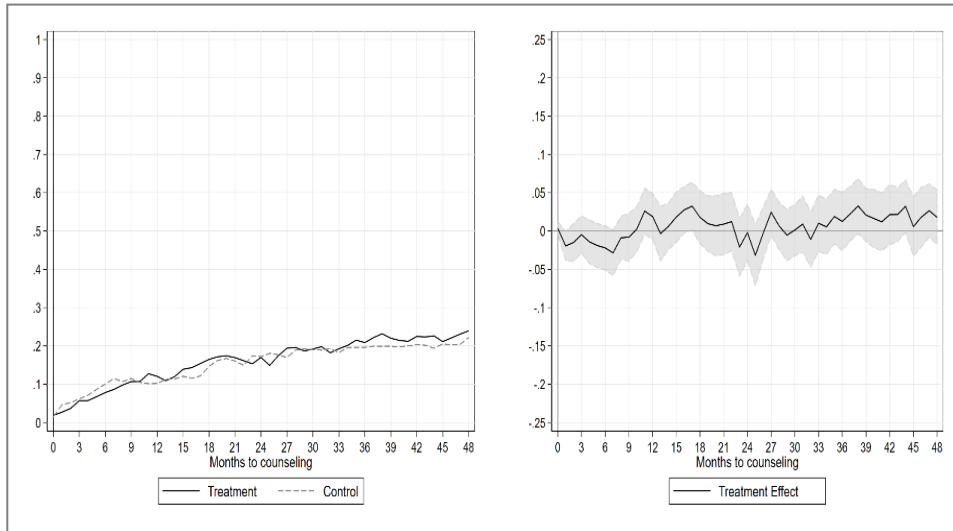
c. Main outcomes among income support recipients – ever employed post-treatment



d. Main outcomes among income support recipients – Ever out again post-treatment



e. Main outcomes among income support recipients – probability to earn above pre-treatment record



Note: Income support recipients are those who received income support at least once in the 36 months prior to employment counseling. In this figure, the estimation was done separately for each month (rather than every three months). The outcome "probability to earn above pre-treatment record" is based on a binary variable calculated for each month, and refers to the earnings from wages in that month. The variable is equal to 1 for the months in which the individual's earnings from wages were higher than the maximum value of his earnings during the three years preceding the counseling, and 0 otherwise. The variable "Ever out again post-treatment..." is a binary variable that takes the value 1 starting from the first month (after counseling) in which the individual was employed. After that month, the variable takes the value 0 if the individual re-enters unemployment—i.e., does not receive positive earnings—and remains at 0 from that month onwards. Note that in the case of persistence in the labor market, the number of months is defined from the first month of first re-employment and not from the month of counseling.

Table 12

**Share of individuals in the treatment and control groups, by primary condition:
pre-treatment unemployment receipt**

Group	Received UIB at least once in the 3 months pre-treatment		Total	Share meeting the criterion
	No (1)	Yes (2)		
Control	5,990	4,682	10,672	0.439
Treatment	11,144	7,907	19,051	0.415
Total	17,134	12,589	29,723	0.424

Table 13

**Share of individuals in the treatment and control groups, by secondary condition:
age-range**

Group	Aged 18-62		Total	Share meeting the criterion
	No (1)	Yes (2)		
Control	457	10,215	10,672	0.957
Treatment	215	18,836	19,051	0.989
Total	672	29,051	29,723	0.977