DISCUSSION PAPER SERIES

DP20494

JOB SEARCH ASSISTANCE FOR OLDER UNEMPLOYED IN THE DIGITAL ERA: EVIDENCE FROM A LARGE SCALE RCT

Ari Kutai, Itay Saporta-Eksten and Analia Schlosser

LABOUR ECONOMICS

CEPR

ISSN 0265-8003

JOB SEARCH ASSISTANCE FOR OLDER UNEMPLOYED IN THE DIGITAL ERA: EVIDENCE FROM A LARGE SCALE RCT

Ari Kutai, Itay Saporta-Eksten and Analia Schlosser

Discussion Paper DP20494
Published 27 July 2025
Submitted 26 July 2025

Centre for Economic Policy Research
187 boulevard Saint-Germain, 75007 Paris, France
2 Coldbath Square, London EC1R 5HL
Tel: +44 (0)20 7183 8801
www.cepr.org

JOB SEARCH ASSISTANCE FOR OLDER UNEMPLOYED IN THE DIGITAL ERA: EVIDENCE FROM A LARGE SCALE RCT

Abstract

Digital hiring tools challenge older workers accustomed to traditional job search methods. Using a large-scale RCT, we evaluate a job search training program for high-skilled older unemployed individuals focused on the use of online platforms and social networks. The program increased earnings and job stability for men, especially in jobs requiring complex matching, suggesting improved match quality, but had no positive impact on women's earnings and reduced their employment. Gender differences cannot be explained by observable characteristics or differential compliance but appear to derive from treated women's increased reservation wages and lower returns to online search tools.

JEL Classification: J08, J14, J16

Keywords: N/A

Ari Kutai - Ari.kutai@gmail.com
Tel Aviv University and Bank of Israel

Itay Saporta-Eksten - itaysap@tauex.tau.ac.il
University of Manchester, Tel Aviv University, CEPR and IZA

Analia Schlosser - analias@tauex.tau.ac.il
Tel Aviv University, CEPR, CESifo and IZA

Acknowledgements
Saporta-Eksten thanks the support of The Israel Science Foundation (Grant No. 1679/20). Schlosser gratefully acknowledges the financial support of the Foerder Institute for Economic Research and the Pinhas Sapir Center for Development at Tel Aviv University.

Job Search Assistance for Older Unemployed in the Digital Era: Evidence from a Large Scale RCT*

Ari Kutai[1], Itay Saporta-Eksten[2], and Analia Schlosser[3]

[1]Tel Aviv University and Bank of Israel

[2]University of Manchester, Tel Aviv University, CEPR and IZA

[3]Tel Aviv University, CEPR, CESifo and IZA

July 25, 2025

Abstract

Digital hiring tools challenge older workers accustomed to traditional job search methods. Using a large-scale RCT, we evaluate a job search training program for high-skilled older unemployed individuals focused on the use of online platforms and social networks. The program increased earnings and job stability for men, especially in jobs requiring complex matching, suggesting improved match quality, but had no positive impact on women's earnings and reduced their employment. Gender differences cannot be explained by observable characteristics or differential compliance but appear to derive from treated women's increased reservation wages and lower returns to online search tools.

Keywords: ALMP, JSA, RCT, automation, gender gap, reservation wages, unemployment

*Saporta-Eksten thanks the support of The Israel Science Foundation (Grant No. 1679/20). Schlosser gratefully acknowledges the financial support of the Foerder Institute for Economic Research and the Pinhas Sapir Center for Development at Tel Aviv University.

1. Introduction

The nexus of aging populations and technological shifts, transforming labor markets, has generated increased interest in the labor market prospects of older individuals. Encouraging older individuals to remain in the workforce has become a top priority in most OECD countries. The scientific literature has identified several barriers to employment faced by older adults. On the employer side, there is evidence of age-based discrimination (see, e.g., Farber et al. (2019); Neumark, Burn and Button (2019)) and lower labor demand due to the higher labor costs associated with employing older workers (Allen (2019); Kaiser Family Foundation (2019)). On the worker side, studies have identified barriers related to skill depreciation and the preferences of many older workers for flexible or reduced hours (Maestas et al. (2018); Clark et al. (2019); and Hudomiet et al. (2021)). However, less attention has been given to frictions in the job matching process between older job seekers and firms. This is particularly important for two reasons. First, the growing digitalization of the hiring process presents new challenges for older workers. Many such individuals have built their careers with fewer job transitions than today, having searched for jobs in a very different environment using search methods that differ significantly from current practices. As a result, they often lack the skills and experience necessary for job searching in the digital era. Second, older individuals, especially high-skilled ones, have accumulated substantial experience, and as such, poor matching for them may result in substantial productivity losses for the economy as a whole.

In this paper, we evaluate the impact of a training program aimed to reduce job matching frictions for medium- to high-skilled older unemployed individuals with a special focus on the use of online job search tools. The program targeted unemployed individuals aged 45-63 from central areas in Israel. It was implemented through a large field experiment and administered by the Israeli Employment Service (IES) between November 2016 and November 2019. Participation was voluntary and included two components: a weekly workshop covering a variety of job search tools, with an emphasis on digital platforms, and access to a job search hub. Our analysis combines comprehensive administrative data from the IES and the National Insurance Institute (social security) – including employment status, earnings, and welfare and disability payments – with survey data to track participants' outcomes before and after their random assignment to treatment and control groups.

Our analysis yields two sets of findings. First, our results suggest that the program improved the job match quality for men, as reflected in three main outcomes: a substantial increase in earnings (primarily driven by higher wages rather than increased employment); a positive effect on job stability; and treatment effects on earnings concentrated in occupations and skills that require more complex matching and hiring processes. As the program did not enhance human capital but focused solely on the search process, these findings indicate that matching frictions play an important role in explaining the large earnings decline following a job loss, at least among older men.

Second, the program's impact on women differed markedly: we find no effect on earnings, slightly negative effects on employment, and positive (but insignificant) effects on observed wages. These gender differences in the program's impact cannot be attributed to observable characteristics (as shown through a re-weighting analysis) or to gender differences in program compliance. Women's utilization of program tools was largely similar to men's. We also rule out the hypothesis that gender differences in treatment effects arose because men accepted jobs less attractive to women, by examining the program's impact on job attributes such as work hours and commute time. Finally, using a Roy model combined with collected reservation wage data, we show that the program led to higher reservation wages among treated women than their counterparts in the control group. Taken together, our findings suggest that the gender disparity in the program effects stems from two sources: differential returns to job search tools taught in the program and distinct dynamics in reservation wages.

Our study relates to four strands of the literature. First, it builds on extensive research documenting the labor market challenges faced by older unemployed workers. A striking finding in this literature is that long-term earning losses following job loss are substantially larger for older workers than their younger counterparts (e.g., Couch (1998), Chan and Stevens (2004), and Couch and Placzek (2010) for the U.S.; Von Wachter, Song and Manchester (2009) for Germany). Our findings suggest that – at least for high-skilled and potentially highly productive workers – frictions in the job matching process play a crucial role in explaining this steep earnings decline. This insight has important policy implications: interventions targeting the matching process could help mitigate the substantial earnings losses older workers face after job loss.

Second, our paper relates to the literature on Active Labor Market Policies (ALMPs), particularly the branch that focuses on job search assistance (JSA) programs. This literature has typically focused on younger, low-skilled workers. Recent reviews show that JSA programs usually have positive effects on employment and reduce unemployment duration (e.g., Kluve (2010); Crépon and Van Den Berg (2016); Card, Kluve and Weber (2018); and Le Barbanchon, Schmieder and Weber (2024)). Most of the examined JSA programs combined job search counseling with job search monitoring and sanctions. Some programs also included financial incentives for reemployment targeted at the unemployed individual, the employer, or the employment agency. As a result, it is difficult to isolate the effects of job search assistance from those of these additional program elements. Furthermore, most JSA programs adopt a "work first" approach that encourages participants back into employment quickly in order to avoid long-term unemployment. While such programs typically improve employment outcomes, they tend to have modest or no effects on wages (Cottier et al. (2018)), largely because they neither prioritize job match quality nor provide skill enhancement. Our paper contributes to this literature by evaluating a program with two distinctive features: a clear JSA component without confounding elements such as monitoring or sanctions, and an explicit focus on job match quality rather than rapid placement. Our findings demonstrate that, for older, higher-skilled unemployed workers, this approach can significantly improve job match quality, at least for men. The differential program effects by gender highlight the distinct needs of high-skilled older unemployed across gender groups in the design of ALMPs.

Third, with the growing use of digital platforms for job search, recent research has focused on the effects of job search tools made available through these platforms. These interventions are attractive because they can reach large numbers of job seekers and offer personalized advice and recommendations at virtually no marginal cost. However, their impacts on employment and earnings have been mixed (e.g. Belot, Kircher and Muller (2019) and Belot, Kircher and Muller (2022) found positive effects, while Ben Dhia et al. (2022) and Le Barbanchon, Hensvik and Rathelot (2023) found small or null effects). The intervention analyzed in this paper differs from those discussed above. Rather than offering *online job recommendations* (as in Belot, Kircher and Muller (2019)) or *online advice* (as in Ben Dhia et al. (2022)), it provides *in-person advice on how to use online tools*.[1] Compared to the mixed results found in the literature, our finding of a large treatment effect (for men) suggests that the impact of online job search tools and advice can be

[1] A related study is Wheeler et al. (2022), who evaluate an intervention in South Africa that trains job seekers to join and use LinkedIn, finding a 10% treatment effect on employment.

amplified when complemented by human interaction and training in the use of digital platforms. This type of training may be especially important for older workers, who tend to have longer and more complex CVs, are often less fluent with digital platforms, and may not have searched for a job in many years.

Finally, a substantial body of literature has studied the contribution of the asking or negotiation gap to the gender earnings gap (e.g., Babcock and Laschever (2003); Dittrich, Knabe and Leipold (2014); Leibbrandt and List (2015); Card, Cardoso and Kline (2016); Biasi and Sarsons (2022); and Roussille (2024).) Recent studies have emphasized that "leaning in" – that is, more intensive negotiation by women – does not necessarily lead to better outcomes for them (see Bowles, Babcock and Lai (2007); Exley, Niederle and Vesterlund (2020); and the review in Recalde and Vesterlund (2020)). Our finding that part of the null effect on women's earnings is driven by an increase in reservation wages among treated women, compared to the control group, that was not accompanied by improved labor market outcomes highlights the specific barriers women face when trying to attain highly skilled positions.

The rest of the paper is organized as follows: Section 2 details the institutional background and describes the program. We delineate our empirical methodology and our data in Sections 3 and 4, and discuss results in Section 5. Section 6 concludes.

2. Institutional Context and Program Description

2.1. Background

In Israel, unemployed individuals aged 45-67 are eligible for 175 days of unemployment benefits. The benefits period begins when individuals register with the Israeli Employment Service (IES). Job quitters must undergo a 90-day waiting period before they can start claiming benefits. Benefits are progressive, with high replacement rates at the lower end of the income distribution, up to a cap based on the national average monthly wage. To receive them, claimants in this age group must report once a month to their local IES office and record their attendance using a self-service biometric fingerprint scanner. During these monthly visits, job seekers may be required to meet with a caseworker who provides job referrals. Failure to report to the employment office or rejection of a suitable job offer results in denial of that month's unemployment benefits. Older individuals and those in skilled occupations are rarely required to meet with caseworkers. During the period analyzed in this study, the retirement age was 63-64 for women (depending on birth cohort) and 67 for men. Upon reaching retirement age, individuals become eligible for old-age benefits.

2.2. Program Description

In November 2016, the IES launched a program aimed at assisting older skilled unemployed job seekers to find their next job. The program was operated as a joint venture with a private provider specializing in job search training for older, skilled job seekers. The target population included individuals aged 45-63 who claimed unemployment benefits and reported to IES offices in central locations in Israel. Given differences in retirement age between genders, the eligible age range was capped at 63 for men and 59 for women. The IES filtered eligible job seekers based on age, employment office location, and a predefined list of skilled occupations. Table A.1 shows the occupational distribution of the eligible population. The most prominent occupation was management (40.8%).

The program was voluntary, and its main goal was to help older, skilled unemployed individuals reintegrate into employment and realize their earnings potential. It equipped participants with modern tools needed for job search in today's labor market. Individuals assigned to the program received a voucher from their caseworker to redeem with the private provider operating the program. The voucher covered all program costs and was non-transferable. Valued at 4,000 ILS (approximately 1,100$), it represented about 20%-30% of the average pre-unemployment monthly wage for men and women, respectively.

The program included two main components: (1) a workshop consisting of 10-12 weekly group meetings, and (2) access to a job-hub, where participants could conduct their job search in a shared office space and receive support from other job seekers and program coordinators. Most participants completed the program within six months of their treatment assignment. A new training cycle was launched every few weeks.

The workshop was specifically designed for older-skilled job seekers, with job search strategies and career advice tailored to their needs. The private provider developed a customized course for this population by consulting with corporate HR specialists and career psychologists, taking into account the needs and psychological barriers faced by older-skilled unemployed individuals during the job search process. Initial meetings focused on defining realistic career prospects for each job seeker, including, for example, determining their reservation wage. Subsequent meetings covered a range of job search tools, including training in how to use digital job search platforms, how to tailor CVs to each posted vacancy, and how to optimize CVs to pass automated screening used in recruitment platforms. The latter was especially important given the typically long CVs of older workers. The program emphasized training in the effective use and expansion of digital social and professional networks, such as Facebook and LinkedIn, for job search. Participants were also taught how to set and self-monitor job search objectives and tasks.

The program's second component was a job search hub, which participants were encouraged to attend. The hub aimed to prevent job seekers from feeling isolated during their job search, promote networking and mutual support among participants, and provide advice and guidance from program counselors. The space was designed to resemble the open-plan layout of a high-tech company, creating an environment where participants felt they were actively working on their job search and fostering social interaction.

It is important to note two features of classical job search training programs examined in the literature that were absent from this program. First, most programs target low-skilled individuals, with the aim of placing them quickly into the labor market, taking a work-first approach. Second, many such programs include monitoring tools to verify active job search and applications. In contrast, this program was voluntary. Moreover, while encouraging job seekers to be proactive in their search process, it focused on teaching skilled unemployed individuals how to effectively use social networks and digital tools to realize their potential and find optimal job matches.

Overall, about 7,000 individuals were assigned to the program from its inception in November 2016 through November 2019. As discussed below, our analysis focuses on those assigned to the treatment and control groups up to 9 or 18 months prior to the onset of COVID-19.

2.3. Experimental design

The program operated on a rolling basis across ten employment offices. A few weeks before the start of a new workshop cycle, the IES system generated a list of eligible individuals based on the selection criteria, randomly assigning two thirds to the treatment group and one third to the control group within each employment office. The randomization protocol was implemented by a computerized algorithm in the IES research department. Treatment status was then updated in the central IES operational database, and each local employment office received its list of treatment group participants. Upon their next office visit, treated individuals were referred to a caseworker, who informed them about the program and provided them with a voucher to redeem with the service provider. They also received a reminder via text message and a phone call from their caseworker. Individuals in the control group received no information about the program or their assignment status.

Both treated and control group members continued to report to the employment office on a monthly basis until their unemployment benefits expired. As noted above, they mainly recorded their attendance but were unlikely to receive referrals from caseworkers since caseworkers do not require skilled older individuals to attend personal meetings.[2] An individual's treatment or control status remained unchanged even if they moved to another city, stopped reporting to the employment office, or later re-registered with IES.

A small fraction of individuals in the control group (less than 3%) participated in the program. This occurred because they applied privately to the service provider after hearing about the program independently from commercial ads and paid the full fee. Below, we discuss how our empirical approach addresses this issue.

3. Empirical Framework

Due to the RCT design, we can infer the program's effect by estimating the difference in post-program outcomes between the treatment and control groups, controlling for the randomization unit.[3] To estimate the intention to treat (ITT) effect of the program, we regress various outcomes

[2] Caseworkers offered no additional treatment to those randomized into the treatment group beyond providing them with the voucher for the program provider. Moreover, as discussed below, program participants exhausted their benefits before completing the training, meaning they neither attended the employment office nor interacted with caseworkers during their post-program job search.

[3] Overall, about 7,000 individuals were allocated to the treatment group over 4 years. This is a small number compared to the size of the high-skilled labor market in central Israel, making displacement effects on the control group unlikely. Moreover, given the low frequency of required attendance at IES offices, other externalities, such as information sharing, were also unlikely. Therefore, we can reasonably assume that the program had no externalities on the control group.

on a treatment indicator, controlling for the randomization cell – defined as the combination of employment office and randomization date.

To increase precision and account for minor differences between the treated and control groups that may arise from randomization in a finite sample, we augment the basic model by incorporating a vector of covariates. The estimating equation is as follows:

$$Outcome_{ijt} = \alpha_0 + \alpha_1 Treatment_i + X_i^{'} \alpha_2 + \gamma_{jt} + \varepsilon_{ijt} \tag{1}$$

where $Outcome_{ijt}$ is the outcome of job seeker i assigned to employment office j, and randomized at time t; $Treatment_i$ is an indicator for whether job seeker i was assigned to treatment; γ_{jt} is a fixed effect for the randomization cell: employment office$\times$ randomization date; and X_i is a vector of individual characteristics measured before randomization, including age, gender, indicators for marital status, number of children, immigration status, college degree, and single-mother status, along with vectors recording employment and earnings history in the three years preceding randomization.[4] Standard errors are clustered by randomization unit, allowing for correlation in error terms among individuals assigned to the same office who were randomized at the same time.[5]

Given that the program was voluntary and that a small share of individuals in the control group also attended the program, we apply an instrumental variable strategy using the randomized treatment assignment as an instrument for program participation (*Participated*) to estimate the average treatment effect on the treated (ATT).[6] Note that as there are almost no "always-takers", the local average treatment effect (LATE) estimated via the instrumental variable strategy can also be

[4]We control for the number of children using five dummy variables (one to four, and five or more). Employment and earnings history are measured by variables that record total months employed and total earnings in each of the three 12-month periods prior to randomization: months 1-12, 13-24, and 25-36.

[5]We adopt a conservative approach and cluster standard errors at the randomization unit level, as treatment assignment varied within clusters (see Abadie et al. (2023)).

[6]We define participation as an indicator that equals one if the individual attended at least one workshop session.The number of workshop sessions attended could have been useful for constructing a measure of participation intensity. Unfortunately, while at least one workshop participation per individual was always recorded for billing purposes, more detailed attendance information was not accurately collected.

interpreted as ATT. The first- and second-stage equations are as follows:

$$Participated_i = \delta_0 + \delta_1 Treatment_i + X_i^{'} \delta_2 + \xi_{jt} + u_{ijt} \tag{2}$$

$$Outcome_{ijt} = \beta_0 + \beta_1 Participated_i + X_i^{'} \beta_2 + \mu_{jt} + v_{ijt} \tag{3}$$

4. Data Sources and Sample

4.1. Data construction

We combine administrative and survey data to track individuals before, during, and after the program's implementation. The first administrative data source is the operational database of the IES, which contains basic socio-demographic characteristics of all job seekers registered with the IES, dates of assignment to treatment and control groups, and details of their pre-randomization occupational history.

The second administrative data source is operational records from the National Insurance Institute of Israel, including monthly unemployment benefits and supplementary benefits such as welfare and disability payments. We merged this information with two additional administrative datasets: (1) tax data containing information on monthly employment, earnings, and employer industry code; and (2) the population registry, which allows us to link spouses' administrative tax records and obtain information on their employment and earnings. Our data span the years 2010 to 2020, providing comprehensive information on unemployment, welfare, and labor market outcomes for RCT participants and their spouses before, during, and after the intervention.[7]

We further enhance our dataset with a baseline and a follow-up survey that provide important insights into the program's impact and its mediating channels. These supplementary data sources allow us to explore dimensions not captured in administrative data records. The baseline survey was administered before randomization; the follow-up survey was conducted nine to ten months after randomization. The surveys cover a range of questions designed to quantify job search skills and strategies, as well as labor market performance. Baseline survey questions targeted task content for the last job, job search intensity, and employment status. The follow-up survey asked

[7] Additionally, we have tax records for the self-employed up to 2018. Given the shorter time horizon for these data, we do not include self-employment in our primary analysis but use them to assess the robustness of our results.

about job search intensity, job-finding methods, and employment status. In the final module of the follow-up survey, treated individuals were asked about program utilization and satisfaction. Both surveys also collected information on reservation wages among unemployed individuals, as well as on economic and life satisfaction. More information about the surveys is provided in Appendix A.

4.2. Sample and Descriptive Statistics

Our primary analysis focuses on the sample of the 5,356 individuals allocated to the treatment and control groups for whom we observe outcomes over an eighteen-month horizon following randomization and before the onset of the COVID-19 pandemic (up to February 2020). Table A.2 presents descriptive statistics and balancing tests by treatment status for this sample. As most of our findings are analyzed separately by gender, we present these statistics separately for men and women.

By virtue of randomization, differences in covariates between the treatment and control groups are minor and, in most cases, statistically insignificant. Notably, we find no significant differences in employment or earnings history between treatment and control groups. Furthermore, the full set of covariates cannot jointly predict treatment status for either men or women.

Regarding sample demographics, 60% of individuals in the treatment group are men. The average age of treated men is 56 and the average age of treated women is 55.[8] Men have a higher marriage rate than women (72% vs. 56%) and higher educational attainment, with 56% holding a college degree or higher, compared to 47% of women. 15% of treated individuals are foreign-born, with equal proportions among men and women. Twelve percent of the women are single mothers.

The individuals in our sample have a strong attachment to the labor force, as evidenced by their employment history. In the year preceding their unemployment, their employment rate was 92% and they averaged eleven months of employment. Employment rates prior to randomization were similar by gender. Nevertheless, in the year preceding their unemployment, women's earnings were about 35% lower than men's.

Table A.3 compares the labor earnings of men and women in our sample to those of the Israeli

[8] Despite the gender differences in the age limit for program eligibility (63 for men and 59 for women), the gender difference in average age is small, given the low share of men aged 60 or older in the sample.

population of similar age.[9] Individuals in our sample have higher earnings than the general population, reflecting the program's focus on white-collar, high-earning occupations (see Table A.1 for the occupational distribution of the target population). Indeed, their labor earnings resemble those of college graduates and of the population re-weighted to match the occupational distribution of the program's eligible population.[10] The gender earnings gap observed in our sample is similar to that observed among college graduates and among the population re-weighted by occupation. Specifically, women earn approximately 35% less than men.

5. Results

5.1. Program Take-Up and Compliance Rates

Overall, 28% of of those assigned to the treatment group who received the voucher participated in the program. Table A.4 displays the probability of program participation as a function of predetermined characteristics estimated using a linear probability model for the full treated sample (column 1) and stratified by gender (columns 2 and 3). The analysis reveals that college graduates were more likely to participate, while immigrants were less likely to participate.[11] Program participation was also higher among individuals with above-median pre-treatment earnings and those with higher employment rates in the year before randomization. Column 1 shows no gender difference in participation rates after controlling for observable characteristics. Importantly, the selection patterns for program participation are broadly similar for men and women, as reported in columns 2 and 3, indicating no systematic gender differences in compliance in terms of demographic characteristics or pre-unemployment labor market performance.

[9] Data are taken from the Central Bureau of Statistics Household Expenditure Survey and include only Jewish individuals, to match the profile of treated individuals, of whom fewer than 4% were Arab. Given that most of our sample is aged 50 and above, we compare it to the population aged 50-64 for men and 50-59 for women.

[10] The Household Expenditure Survey records were weighted to match the one-digit occupation distribution of our sample.

[11] The age coefficient is negative and significant, but its magnitude is very small, implying no meaningful differences in participation rates by age.

5.2. The Dynamic Effects of the Program

We begin by discussing the dynamic effects of the program on employment and earnings, as plotted in Figures 1 and 2. Each figure shows treatment and control group means (left panels) and controlled differences between the groups with 90% confidence bands (right panels). Both figures cover two years before randomization to eighteen months after randomization.[12]

Figure 1 shows that the treatment and control groups followed very similar employment trajectories prior to randomization, confirming the validity of our empirical design. As discussed above, both groups had very high employment rates (around 90%-95%) 12-24 months before randomization, consistent with the program's focus on highly skilled workers and the eligibility requirements for unemployment benefits discussed earlier.

In line with patterns commonly observed in ALMP evaluations, employment rates began to decline around twelve months before randomization, with the decline intensifying over time. This pattern reflects the program's unemployment eligibility requirement. Following this initial drop (commonly known as the Ashenfelter dip), employment rates for both groups began to recover, stabilizing at about 60% after nine months – substantially below their pre-unemployment levels. On average, there is no significant difference in post-treatment employment rates between treatment and control groups.

Figure 2 plots average earnings of treatment and control groups. As with employment, the groups show very similar dynamics in the pre-randomization period. Following randomization, both groups experience a partial recovery in earnings, but the recovery is significantly stronger for the treatment group.[13] The earnings advantage for the treatment group emerges about three months after randomization and continues to grow, nearly stabilizing after twelve months – consistent with the program's duration and the fact that 90% of participants began the program within 100 days (the average was 58).

Taken together, Figures 1 and 2 reveal a pattern of program effects that contrasts sharply with

[12]The estimated differences between groups control for gender, age, marital status, college degree, number of children, single-mother, immigrant, and randomization block fixed effects.

[13]Average monthly earnings for the control group declined by approximately 55% between 13-24 months before randomization and 13-18 months after randomization. This large decline aligns with the literature, which documents large earnings losses following job loss among older individuals and unemployment insurance claimants. See Table 1 in Couch and Placzek (2010) for a summary of estimates from the literature.

typical JSA programs which emphasize the work-first approach. Such programs often generate short-lived employment gains with little to no effect on earnings in the medium- or long-term. Most of these programs are mandatory or include a monitoring component to ensure that job seekers are actively engaged in job search. The voluntary program under study adopted a fundamentally different approach: instead of inducing immediate employment through monitoring and sanctions, it focused on improving job match quality. The result was a substantial and persistent rise in earnings that cannot be accounted for by an increase in employment, suggesting that participants found higher-paying jobs.[14]

5.3. Detailed Results by Gender

We now examine gender differential effects of the program. This analysis is important, as several studies have found that ALMPs tend to be more effective for women (see review in Card, Kluve and Weber (2018)), while recent findings point to greater age-based discrimination against women (Neumark, Burn and Button (2019)) and suggest that women face distinct challenges in the job search and matching process (e.g., Cortés et al. (2023)).

Table 1 presents program effects at eighteen months post-randomization. Columns 1-3 report reduced form (ITT) estimates for the full sample, men, and women, respectively. Control group means appear in italics. As discussed above, there is no employment effect for the full sample. However, when examining results by gender, we observe a two percentage-point increase for men and a similar sized decrease for women. Although neither effect is statistically significant at conventional levels, the gender difference is statistically significant at the 10% level.[15]

The ITT effect on labor earnings shows even stronger gender differences. Men's monthly earnings increase by approximately 1,100 ILS (about $300), 12% above the control mean, while there is no effect on women's earnings. Estimates for the program effect on average monthly earnings over months 13-18 are similar. The earnings effect for men is more precisely measured and cannot be explained by the small two percentage-point increase in employment. Again, there is no effect for women.

[14] In a separate analysis, we examine treatment effects on employment and earnings using a smaller sample that can be followed over a 24-month horizon. Both effects are of a similar magnitude and persist through 24 months.

[15] Table A.5 tests these gender differences directly by interacting the treatment indicator with gender for all outcomes.

Given that the program is voluntary, columns 4-6 report LATE estimates of the program's impact on participants, using the initial randomization to treatment as an instrument for program participation. These columns also report outcome means for compliers, computed following Angrist, Hull and Walters (2022).[16]

As shown in the bottom part of Table 1, compliance rates are approximately 25% and nearly identical for men and women.[17] As such, the gender patterns observed in the reduced form estimates are preserved in the LATE estimates. Overall, the program increased the monthly earnings of participating men by about 4,400 ILS (about $1,250), a 52% increase relative to the complier mean. We estimate a similar increase in monthly earnings for men when computed over months 13-18 post-randomization. In contrast, we find no significant effect on women's employment or earnings.[18] Most recent studies on JSA programs focus on young populations or report only employment impact, usually finding short-lived effects. For example, Michaelides and Mueser (2020) examined a JSA program implemented in Nevada that was targeted at new unemployment insurance recipients and provided mandatory staff-assisted job-counseling services. Program participants were relatively young and low-skilled. They experienced a 12% increase in employment and a 19% increase in earnings four quarters after random assignment to the program.[19] While the majority of the earnings effect in that program is explained by increased employment, our results indicate a different pattern, one in which the main program effect is an increase in earnings with minimal impact on employment, highlighting the potential of personalized training in the use of online platforms.

To better understand the magnitude of the program's effects on men, we calculate how much of the earnings decline associated with job loss was recovered by program participants. Using our LATE estimates, along with the change in average monthly earnings for compliers from months 13-24 pre-treatment allocation to months 13-18 post-allocation, we find that participating men

[16] The complier means were estimated using 2SLS models that include, in both the first and second stages, randomization block fixed effects and the full vector of control covariates.

[17] Note that compliance rates are a bit lower than the unconditional participation rates reported in Table A.4 given that about 3% of individuals in the control group participated in the program.

[18] We also examined program effects on employment and earnings including self-employment data, using a smaller sample for which we have self-employment records covering a 12-month horizon post-randomization. In this sample, around 9% of both men and women are self-employed. Reassuringly, the estimated program effects and gender differences remain unchanged when employment and income from self-employment records are included.

[19] See their Table 4, combined with baseline levels from their Table A2.

recovered 28% of their earnings losses. As the program focused solely on the job search process, without affecting human capital, this suggests that matching frictions play an important role in explaining the large earnings declines following job loss for older workers.

Panel B of Table 1 reports cumulative outcomes over the first eighteen months post-randomization. The LATE estimates show patterns consistent with results for month 18: no effect on total months employed for the full sample, though men worked approximately one month more and women one month less over the eighteen-month period (both insignificant). The program's effect on men's cumulative earnings is substantial: over eighteen months, participating men gained more than 66,000 ILS (∼\$18,450) – a 60% increase in earnings relative to the complier mean – while there was no corresponding increase in women's earnings.

The last two rows of Panel B show cumulative effects on unemployment benefits and on total social benefits, (unemployment benefits, disability payments, and income support payments).[20] Overall, the program has no effect on benefit transfers for either men or women. This is consistent with the fact that the program did not shorten unemployment duration, but rather improved match quality.

Additional evidence of improvement in match quality is presented in Table 2, which displays the program's effect on the probability of working for the same employer in month 18 post-randomization as in the individual's first post-randomization employment spell. Panel A includes the entire sample (assigning zero values to the non-employed), while Panel B reports outcomes conditional on employment. In both cases, we find a positive impact among men – a 56% and 37% increase relative to the complier mean, respectively, and no statistically significant effect among women. Section 5.5 presents further evidence of improved match quality for men in our analysis of treatment effect heterogeneity.

Next, we turn to a cost-benefit analysis of the program. First, we use the estimated cumulative effects of the program to assess the extent to which it covered its costs. The program cost per participant was 4,000 ILS (∼\$1,100). Given the absence of a significant effect on transfer payments, the program's impact on the government budget arises solely through income tax collection. Over the first eighteen months post-randomization, the estimated additional income tax collected per participant averaged 9,820 ILS – approximately 17,000 for men and -3,500 (statistically insignifi-

[20] Disability and income support payments constitute a negligible share of total social benefits received by this highly skilled population (less than 2%).

cant) for women. Hence, within eighteen months, the program more than paid for itself on average, though not separately by gender.[21]

Second, following Hendren and Sprung-Keyser (2020), we also compute the Marginal Value of Public Funds (MVPF), defined as the ratio of individuals' willingness to pay (WTP) for the program to its net cost to the government, for the full sample. As discussed above, the net government cost of the program is about -5,820 ILS per participant (reflecting 4,000 ILS in direct costs offset by 9,820 ILS in additional income tax revenue over the eighteen-month post-randomization period). We estimate WTP as the cumulative increase in after-tax earnings – roughly 26,830 ILS – over the same period.[22] This calculation implies an infinite MVPF, as the net cost is negative and the WTP is positive. We also compute the MVPF separately for men and women, although, as pointed out by Hendren and Sprung-Keyser (2020), these calculations should be interpreted with caution, given that the program was not designed to be implemented separately by gender. For men, we find a net cost of -13,272 ILS and a WTP of 49,379 ILS, resulting in an infinite MVPF. For women, given the insignificant effect on total income, we define WTP to be zero, implying an MVPF of zero.[23]

To summarize, the results so far point to a large increase in men's earnings, but no effect on women's earnings. We also find a small negative employment effect for women and a small positive one for men, although neither is statistically significant (while the gender difference is). The employment effect for men can account for only a small portion of their earnings increase. This finding, combined with the increase in job stability observed among men, suggests that the program primarily improved job match quality for men.[24]

[21] In our calculations, we focused on income tax, excluding additional taxes associated with employment, such as health tax and National Insurance contributions. We determined total income tax revenues using the 2018 Israeli marginal tax rates adjusted for gender-based variations in tax benefits. All values are expressed in real 2018 ILS.

[22] The cumulative increase in earnings is 36,648 ILS (Table 1, column 4, Panel B), while the increase in income tax was 9,820 ILS.

[23] We show in Section 5.4.4 that there is no negative effect on participants' life or economic satisfaction.

[24] A relevant question is whether the improved job match among men implies that they took "better" jobs (e.g., in higher-paying sectors) or were more likely to find jobs that better matched their skills. In a complementary analysis (not shown here to save space), we find no evidence of treatment effects for men on the likelihood of working in the high-tech sector – Israel's highest-paying sector – indicating that the program primarily improved skill-job alignment rather than placement in premium sectors.

5.4. Explaining Gender Differences in the Effects of the Program

The striking differences in program effects by gender prompt us to examine the potential channels that might account for these disparities.

5.4.1. Gender Differences in Characteristics

As noted above, while program participation patterns are similar across genders, with no differences in compliance rates or differential selection based on observables, baseline characteristics differ substantially by gender. As shown in Table A.2, women have lower earnings, lower marriage rates, and lower levels of education than men. Their occupational distribution in previous employment also differs substantially from that of men (Table A.1). For example, 47% of men were in management positions in their last job, as opposed to only 33% of women. In contrast, 20.3% of women worked in office or administrative support, as opposed to only 4.7% of men. These underlying differences raise the question of whether the gender differences in program effects simply reflect heterogeneous effects by occupation, earnings potential, or other characteristics. To assess this, we re-weight the women's sample to match the characteristics of the men's sample using various weighting schemes and estimate the program effects on the re-weighted sample of women.

Results presented in Table 3, show the program effect (ITT and LATE) on total months employed and on earnings accumulated up to month 18. Columns 1 and 5 report our main results from the unweighted model for ease of comparison (corresponding to columns 3 and 6 in Table 1). In columns 2 and 6, we re-weight the women's sample to match the occupational distribution of men (see Figure A.1 for the unweighted and re-weighted distributions). In columns 3 and 7, we re-weight the women's sample by deciles of pre-unemployment earnings, measured over months 13-24 prior to randomization (see Figure A.2 for the distributions). Finally, in columns 4 and 8, we re-weight the sample using the full set of covariates outlined in Section 3 – including demographic characteristics, employment history, and earnings history – and also include the occupational distribution.[25] Across all re-weighting specifications, the estimates remain very similar to our main results. If anything, the negative effect on employment becomes more pronounced. Notably, the first-stage estimates also remained largely unchanged across specifications, confirming that com-

[25] Weights are computed using a logistic regression, and odds ratios are winsorized at the top 5%.

pliance patterns were similar for men and women and ruling out differential participation as an explanation for the observed differences in program effects. We therefore conclude that differences in observable characteristics cannot explain the gender differences in treatment effects.

Lastly, if treatment effects vary near retirement age, one possible explanation for the gender differences in treatment effects may be the different retirement ages for women (63-64) and men (67) during this period. However, program eligibility was capped at age 59 for women and 63 for men to account for these gender differences in retirement age. Moreover, our analysis found no heterogeneous treatment effects by age among women (results available upon request), ruling out the possibility that the lack of an effect on women's earnings was driven by differential behavior or treatment of those approaching retirement age.

5.4.2. Program Utilization by Gender

Having ruled out gender differences in participant characteristics as a likely explanation for the observed gender gap in program effects, we explore alternative explanations, turning to our survey data.[26] Appendices A and B provide details on the survey data collection process, variable construction, response rates, and selection into the survey. Table A.7 shows that we are able to reproduce our main results using the sample of the follow-up survey respondents, while Table A.8 shows that treatment and control groups are balanced in this follow-up sample.

One potential explanation for the gender differences in program impact may lie in how men and women utilized the program's tools. While overall compliance rates were similar across genders, program effects may still vary if men and women differed in how they applied the knowledge and tools provided. For instance, participants may have received the same information and advice but implemented the skills and practices differently in their job search.

We begin by examining the program's effects on job search practices, using information collected in our follow-up survey.[27] Table 4 reports treatment effects on the utilization of these job search practices by gender, along with control and compiler means. Given the program's emphasis on the use of digital platforms, respondents were specifically asked about their use of LinkedIn

[26]To maximize power, the sample of the survey includes all individuals for whom we observe outcomes for a nine-month horizon following randomization (rather than eighteen) and before the onset of COVID-19.

[27]The entire follow-up survey sample was queried about their job search practices. Unemployed individuals were asked about their current job search efforts, while employed respondents were asked about their job search practices prior to starting their current jobs.

– the platform most relevant to the program's target population. Based on reported control group means, men are more likely to use LinkedIn than women (43% versus 26%), a gender difference also evident among compliers. While the results are noisy due to the smaller survey sample, the table shows that the program increased LinkedIn use for both genders. Moreover, the larger treatment effect for women (in both relative and absolute terms) narrowed the gender gap in the use of digital platforms.

Another important practice emphasized in the program was tailoring CVs to match the specific requirements of each job posting, taking into account the automated filters used by recruitment platforms. The program indeed enhanced the use of this practice among both men and women. We do not find a significant effect of the program (positive or negative) on other common job search practices, such as using job search websites, leveraging social connections, or participating in conferences and events. This implies that the program did not crowd out other job search channels (at least on the extensive margin). Overall, results suggest that gender differences in the program's effects on earnings cannot be explained by gender differences in its impact on the use of job search tools.

Focusing on the treatment group, we also examine gender differences in particular aspects of active program participation. As described above, job seekers were encouraged to attend the job-hub to socialize and to develop a routine for their job search. Figure 3 shows the intensity of job-hub use by gender. Women were less likely than men to use the hub on both the intensive and extensive margins. We further examine this in Table 5, where columns 1 and 2 report means by gender, and columns 3 and 4 present gender differences after controlling for randomization block fixed effects and, additionally, our set of covariates. In Panel A of the table, we observe that women participants made, on average, three fewer monthly visits (a 30% difference) to the job-hub than men. Consistent with this, Panel B, which reports gender differences in satisfaction with various aspects of the program, shows that women were slightly less satisfied with the statement that social connections built during the program helped them search for and find jobs.[28] In the remaining rows of the table, we report no significant gender differences in satisfaction with other aspects of the program, such as networking with potential employers and program graduates, the content of the workshops, the quality of the lecturers, and the tools and knowledge acquired in the

[28] Participants were asked to rank their satisfaction levels with various aspects of the program using a five-point Likert scale. We created z-scores for the responses to each question.

program.

To summarize, the evidence suggests that the program enhanced the use of practices for online job search, with no differential effect by gender. Furthermore, men and women were equally satisfied with most aspects of the program. The only gender difference we found was that men were more likely to attend the job hub and were somewhat more satisfied with using social connections developed during the program for their job search.

5.4.3. Job Characteristics

Another possible explanation for the gender differences in treatment effects is that the program may have helped men find jobs that were less appealing to women. Such jobs might involve longer workdays, extended commute times, or other job amenities that are less preferred by women.[29] To explore this channel, we estimated treatment effects on job characteristics using data on the characteristics of post-randomization jobs from two sources. First, we collected information on job characteristics through our follow-up survey. Second, to complement the administrative data with job characteristics, we used industry-related features of individuals' first post-randomization job, such as average work hours. We calculate these industry features from data in the 2017-2019 Labor Force Survey, focusing on our target sample.

Table 6 shows treatment effects for work hours and commute time (one-way) conditional on employment, using both data sources. Naturally, the sample is affected by selection into employment. However, given that the treatment effects on employment are small, the resulting selection bias in the estimated effects on job characteristics is also expected to be small. Overall, we do not find meaningful effects of the program on working hours or commuting time for either gender. The only significant effect is a slight decline in work hours among men – measured using the industry features – with no effect for women (the gender difference is significant at the 10% level). The estimate is small in magnitude and indicates that the program did not direct men toward jobs requiring longer hours. Moreover, the results for work hours suggest that the observed increase in men's earnings cannot be explained by increased hours worked.

We also estimate treatment effects on the likelihood of working in "male-dominated" industries.

[29] See Bertrand (2018), Caldwell and Danieli (2024), and Le Barbanchon, Rathelot and Roulet (2021), who highlight the role of commute time in the context of the gender gap.

We find no evidence that the program led participants to jobs in such industries.[30]

Taken together, the results presented in this table contradict the hypothesis that gender differences in the program's effects are driven by men being matched to jobs that women find less desirable.

5.4.4. The Differential Effect on Reservation Wages

Finally, we examine the program's effect on reservation wages as an additional channel that might explain the gender differences in treatment effects on employment and earnings. The impact on reservation wages could plausibly operate in opposing directions. On the one hand, job seekers were encouraged to form realistic expectations about their employment prospects and wages. On the other hand, they were advised to value themselves appropriately and to ask for wages aligned with market wages corresponding to their skills and experience.

We first examine gender differences in reservation wages in the baseline survey, conducted prior to program assignment. As shown in Table A.9, there are marked gender differences: men report reservation wages that are about 50% (40 log points) higher than those reported by women. These differences closely mirror the gender gap in pre-job loss wages.[31] Overall, both men and women report reservation wages that are about 10% below their wages in their last job. Given the substantial earnings declines documented in Section 5.2, these reservation wages appear optimistic, a finding consistent with the literature.

To analyze the program's effect on reservation wages and market wages, we combine follow-up survey data on reservation wages with administrative earnings records. This analysis presents a methodological challenge: reservation wages are only observed for non-employed individuals, while market wages are only available for those who are employed. We use a simple Roy model to account for this selection.

[30] An industry is defined as "male-dominated" when the share of men in that industry is 70% or higher. Results were similar using different cutoffs.

[31] Pre-job loss wages are defined as the log monthly earnings averaged over months 13-24 before randomization, computed from the administrative data.

Consider the following simple Roy model (Roy (1951), Gronau (1974), Heckman (1974)):

$$y_i^w = \mu^w + \varepsilon_i^w$$
$$y_i^r = \mu^r + \varepsilon_i^r,$$

where: y_i^w denotes individual i's log market wages and y_i^r denotes i's log reservation wages; μ^w and μ^r are their respective means, and ε_i^w and ε_i^r are the error terms.

An individual chooses to work, namely $L_i = 1$, if $y_i^w \geq y_i^r$, that is:

$$L_i = 1 \text{ if } \varepsilon_i^r - \varepsilon_i^w \leq \mu^w - \mu^r.$$

We assume that $\varepsilon_i^w, \varepsilon_i^r$ are jointly normally distributed:

$$\begin{pmatrix} \varepsilon_i^w \\ \varepsilon_i^r \end{pmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \sigma_w^2 & \rho \cdot \sigma_w \cdot \sigma_r \\ \rho \cdot \sigma_w \cdot \sigma_r & \sigma_r^2 \end{bmatrix} \right)$$

The model parameters $(\mu_w, \mu_r, \sigma_w, \sigma_r, \rho)$ are identified using five moments: the employment share, the first and second moments of observed wages, and the first and second moments of reservation wages (see Appendix C for a formal identification discussion). We estimate the model allowing for different parameters for men and women, and for treatment and control groups.[32]

To align the timing of the reservation wages reported in the survey with the observed wages from the administrative data, we use a sample that includes all individuals observed nine months after randomization. To reduce measurement error, we use average wages from months 9-11 post-randomization, rather than wages in month 9, winsorized at the top percentile. Before proceeding with the model estimation, we first verify that the results obtained for the main sample also hold in the sample used for the model estimation. Panel A of Table 7 reports the control mean and the ITT effect on earnings for this sample. Consistent with our previous results, we see a positive treatment effect on earnings for men and no effect for women. We also observe a small positive effect on employment for men and a negative effect for women, as before, though the effect for women is somewhat larger in this sample and is now statistically significant (Panel B). We note

[32]Our small survey sample implies a noisy second moment for reservation wages. Hence, we fix ρ rather than estimate it, and conduct a sensitivity analysis using different values of ρ.

that the treatment effects on both earnings and employment for men are somewhat smaller, as we now measure effects over months 9-11 rather than 12-18 (as shown in Table 1).

Panel B of the table reports the moments used in the estimation. The treatment effect on log wages (conditional on employment) is positive for both genders, though statistically significant only for men. The bottom part of the table reports the treatment effect on log reservation wages, measured in the follow-up survey. These results are noisier due to the smaller sample size, but they indicate a negative treatment effect on observed reservation wages. Both treatment effects – on observed wages and reservation wages – are affected by selection into employment, which we account for in the model.

Table 8 reports results from the estimation of the Roy model, where parameters are estimated separately for treatment and control groups, and for men and women. Panel A displays the estimated model parameters, while Panel B reports the average selection bias, defined as the difference between each observed wage (market or reservation) and the mean of its respective unconditional distribution. Columns 1, 2, 4, and 5 report the estimated model parameters for treatment and control groups by gender. Columns 3 and 6 show the treatment effects – that is, differences in the estimated parameters between treatment and control – for men and women, respectively. For men, we find a positive and significant effect on the standard deviation of market wages (σ_w). This result suggests that the program's positive impact on men's earnings arises from increased wages at the right tail of the wage distribution. To further examine this, Figure 4 plots quantile treatment effects (QTE) on men's wages (in black circles), along with confidence bands.[33] We also plot simulated treatment effects (red hollow circles) using the estimated model parameters. The blue solid line indicates the average treatment effect, with its 90% confidence interval shaded in gray. Both QTE estimates and the simulated model results show a positive treatment effect at the highest ventiles of the earnings distribution. This pattern supports the notion that matching frictions are particularly salient in the hiring process for higher-paying jobs, making the program more effective for individuals targeting these types of positions. We return to this point in our analysis of heterogeneous treatment effects discussed in the next section.

For women, Table 8 shows no significant treatment effect on market wage parameters – neither the mean nor the variance. At the same time, there is a large and significant increase in the mean

[33] For comparability with the Roy model, we report ITT effects when presenting QTE results.

of the reservation wage, accompanied by a decrease in its variance. This change in the reservation wage distribution can explain both the negative employment effect and the positive effect on log earnings for women reported in Table 7.[34] In contrast to the results for men, the treatment effect on the selection bias coefficients for women is statistically significant at the 5% percent level, implying that the treatment affected the composition of employed women. Specifically, employed women in the treatment group are more positively selected on market wages than those in the control group. To assess the robustness of our results, we conducted sensitivity analyses using different values of the correlation parameter (ρ) in the model estimation. Our findings remain similar across values of ρ, and in particular, consistently indicate a significant increase in the mean of the reservation wage for women.[35]

Why did the program have different effects on reservation wages for men and women? Studies have shown that women typically report lower reservation wages, even for the same job. Moreover, they tend to ask for lower wages (*"the ask gap"*; (see, e.g., Babcock and Laschever (2003); Dittrich, Knabe and Leipold (2014); Leibbrandt and List (2015); and Biasi and Sarsons (2022)).[36] As noted above, the program emphasized that job seekers should not underestimate themselves. While this could have affected both genders, we note that men outnumbered women in the program, at a ratio of 3 to 2. A plausible explanation, therefore, is that women may have aligned their reservation wages and wage bargaining strategies with those of men.[37]

To evaluate this hypothesis, Table 9 reports the dynamics of the mean of market and reservation wages separately by treatment status and gender. For market wages, we report the change in the mean of the log earnings distribution from the pre-job loss spell to nine months post-randomization, using the estimated mean from the Roy model for the post-period.[38] For reservation wages, we

[34] To assess the contribution of the change in reservation wages to the impact on employment and earnings, we simulated the model while shutting down the treatment effect on reservation wages (i.e., setting $\Delta\mu_r = \Delta\sigma_r = 0$) and using the point estimates for treatment effects on market wages. In this scenario, we find that most of the negative employment effect, as well as the positive effect on observed log wages, disappear. This implies that the observed treatment effects in Table 7 are driven by changes in the reservation wage parameters rather than by the insignificant effect on market wage parameters.

[35] See the full set of results in Tables A.10 and A.11, for ρ values of 0.4 and 0.6, respectively.

[36] There is also evidence that women extract a smaller share of the match surplus. See, for example, Card, Cardoso and Kline (2016).

[37] Unfortunately, attendance at individual workshop sessions was not recorded, so we cannot identify which participants were together in each session, thus preventing us from examining how social interactions affected reservation wages.

[38] Log earnings for the pre-job loss spell are calculated as the average log monthly earnings over months 13 to 24

report the change from the mean at the baseline survey to the estimated mean from the Roy Model nine months post-randomization.

Column 1 of Table 9 shows that for men in the control group, the decline in reservation wages over time is about 11 log points larger than the decline in market wages over time. Column 2 shows a similar pattern for treated men. In contrast, column 3 indicates that for women in the control group, the gap between the decline in reservation wages and the decline in market wages is much larger – about 26 log-points. This suggests that during the unemployment spell, women in the control group became more willing than men to compromise on lower wages. However, among treated women, the difference between the decline in reservation wages and the decline in market wages is 11 log points, mirroring the dynamics observed for men in both the treatment and control groups.

Taken together, these results support the hypothesis that women in the treatment group adopted a reservation wage strategy similar to that of men. Namely, treated women's reservation wages did not decline as much as those in the control group. This behavior resulted in lower employment and higher observed wages for treated women (relative to the control group). Notably, while the employment rate among treated women is similar to that of treated men (last row of Table 9), the employment rate for women in the control group is higher, consistent with greater downward adjustment of reservation wages in that group. These findings align with Exley, Niederle and Vesterlund (2020), who show in a lab experiment that encouraging women to negotiate more can lead to worse financial outcomes (see also Recalde and Vesterlund (2020) for a survey). However, in a quasi-experimental setting using an online hiring platform, Roussille (2024) finds that inducing women to request higher wages generally improves their financial outcomes. As Roussille (2024) highlights, the existence of a "lean-in" penalty depends on the context and conditions of the labor market. Our results suggest that, for older skilled women, an increase in reservation wages may not translate into higher earnings.

The normative interpretation of our results is more nuanced. On the one hand, the treatment may have influenced women's leisure preferences, leading to the observed increase in reservation wages and decline in employment. On the other hand, women in the treatment group may not have fully accounted for the decline in employment probability when they reduced their reservation wages to

before allocation to treatment and control groups.

a lesser extent than women in the control group. To investigate this, we examined the program's impact on life and economic satisfaction using responses from the follow-up survey. The results, reported in Panel A of Table A.12, show no significant effect on either type of satisfaction for men or women. Panel B reports treatment effects on job satisfaction (post-randomization) among employed individuals – specifically, satisfaction with the match between the job and the worker's skills, satisfaction with the wage, and overall job satisfaction. The last row of Panel B shows the impact on a job satisfaction index, constructed as a z-score, aggregating the three job satisfaction measures. Consistent with the earlier evidence on the program's effect on job match quality for men, we find that men in the treatment group tend to report higher satisfaction with their jobs and wages than those in the control group. In contrast, we do not find any effect on job satisfaction for women.

5.5. Heterogeneity of the Effects on Men

Thus far, we have reported a positive treatment effect on men's earnings, with employment playing only a small role in explaining this increase. We further find that effects are larger at the top of the men's earnings distribution. We now complement these results by exploring heterogeneous treatment effects on men's earnings.[39]

Given that the program provided participants with tools intended to improve their matching outcomes, we expect larger treatment effects in cases where the job matching process is more challenging. As empirical evidence shows that matching is more complex for high-skilled, higher-paying positions (see, e.g., Barron, Bishop and Dunkelberg (1985), Blatter, Muehlemann and Schenker (2012), and Marinescu and Wolthoff (2020)), we expect larger effects in these contexts.

To explore this hypothesis, we examine whether the treatment effect is larger in jobs involving a more complex matching process by analyzing heterogeneity in treatment effects based on predetermined characteristics. We focus on three dimensions: education level, occupation, and earnings in the last job. Following the automation literature, we use job task content data from the baseline survey to characterize occupations along two attributes: routine vs. non-routine, and cognitive vs. manual (see Acemoglu and Autor (2011)).[40] We then focus on non-routine cognitive occupa-

[39] We performed an equivalent analysis for women, finding no indication of a positive effect on earnings for any subgroup (results available upon request).

[40] We recover the task content of a job using our survey and following the questionnaire outlined in Autor and

tions – known to be high-paying and high-skill intensive – where job matching is likely to play a more critical role. Figure 5 presents ITT and LATE estimates of the treatment effect on earnings accumulated over the first eighteen months after randomization, across the dimensions described above. For comparison, the estimate for the full sample is reported in the first line. Both the ITT and LATE estimates tell the same story: the program has the largest impact on individuals who held high-paying or high-skilled jobs prior to their unemployment spell. While we find relatively little heterogeneity by education level, we observe large differences in the program's effect by occupation and pre-unemployment earnings. Notably, we find a large impact among those whose last job was in a non-routine cognitive occupation, compared to other occupations. Focusing on heterogeneity by pre-unemployment earnings, we find a null effect below the median and larger effects in the third and especially the fourth quartile of pre-unemployment earnings. As reported in Tables A.13 and A.14, here as well, the effects on earnings cannot be accounted for by the effect on employment. These results further support the hypothesis that the program is more effective for positions that require a more complex hiring process.[41]

The larger effect for high-earning men might alternatively be explained by their greater ability to self-insure through asset accumulation over the life-cycle, which could allow them to search longer, wait for a better job match, or invest more financial resources in the search. While we do not observe assets, we assess this alternative hypothesis by stratifying the sample into quartiles based on spouse's pre-unemployment earnings. If greater liquidity were driving the results, we would expect to find a larger impact on men's earnings among those with high-earning spouses. However, our analysis reveals the opposite: the program had a larger impact among individuals with no spouse or a low-earning spouse (results not reported here to save space).

6. Conclusion

The digitalization of the job search process is a promising technological development that can improve matching between unemployed individuals and jobs. ALMPs that rely on online platforms are especially attractive due to their relatively low cost and scalability. However, mixed results from

Handel (2013).

[41] These findings also suggest that the positive effect on earnings for men cannot be explained by their differential selection into employment, given that we find positive earnings effects even within narrowly defined groups.

previous studies suggest that the introduction of such platforms may need to be complemented by training in how to use them effectively. Analyzing one such training program, we find that it improves the match quality for older skilled unemployed men. The resulting increase in labor earnings is substantial, especially among men in high-earnings jobs and occupations, highlighting that reducing matching frictions can significantly mitigate the large earnings losses experienced by displaced older skilled workers.

For women, we find no effect on earnings and no indication of an improvement in match quality. Instead, we observe a slight decline in employment, a slight increase in observed wages, and a notable increase in reservation wages.

Why was this training not effective for women? We can rule out three potential explanations. First, the lack of effectiveness cannot be attributed to differences in occupations or other observable characteristics across gender. Second, the program did not lead participants to jobs that are inherently less appealing to women. Third, women were equally likely to adopt the search tools taught in the program. We therefore attribute the gender differences in the impact of the JSA program to lower returns to these tools among women – even among those in high-paying jobs, where the program had a large effect on men. This is consistent with the glass ceiling phenomenon, in which women face barriers to advancement in higher-level positions.[42] Supporting this interpretation, we find that while the program raised reservation wages among treated women – consistent with narrowing the "ask gap" – these higher reservation wages did not translate into correspondingly higher observed wages. The stark gender differences in program effects underscore the importance of designing JSA programs that explicitly address the needs and constraints faced by skilled women in the labor market.

[42] See, for example, Goldin (2014), Blau and Lynch (2024), and Blau (2024).

References

Abadie, Alberto, Susan Athey, Guido W Imbens, and Jeffrey M Wooldridge. 2023. "When should you adjust standard errors for clustering?" *The Quarterly Journal of Economics*, 138(1): 1–35.

Acemoglu, Daron, and David Autor. 2011. "Skills, tasks and technologies: Implications for employment and earnings." In *Handbook of labor economics*. Vol. 4, 1043–1171. Elsevier.

Allen, Steven G. 2019. "Demand for Older Workers: What Do Economists Think? What Are Firms Doing?" National Bureau of Economic Research.

Angrist, Joshua, Peter Hull, and Christopher R Walters. 2022. "Methods for Measuring School Effectiveness." National Bureau of Economic Research Working Paper 30803.

Autor, David H, and Michael J Handel. 2013. "Putting tasks to the test: Human capital, job tasks, and wages." *Journal of labor Economics*, 31(S1): S59–S96.

Babcock, Linda, and Sara Laschever. 2003. *Women Don't Ask: Negotiation and the Gender Divide.* Princeton University Press.

Barron, John M, John Bishop, and William C Dunkelberg. 1985. "Employer search: The interviewing and hiring of new employees." *The Review of Economics and Statistics*, 43–52.

Belot, Michele, Philipp Kircher, and Paul Muller. 2019. "Providing advice to jobseekers at low cost: An experimental study on online advice." *The review of economic studies*, 86(4): 1411–1447.

Belot, Michele, Philipp Kircher, and Paul Muller. 2022. "Do the long-term unemployed benefit from automated occupational advice during online job search?" IZA Discussion Papers.

Ben Dhia, Aïcha, Bruno Crépon, Esther Mbih, Louise Paul-Delvaux, Bertille Picard, and Vincent Pons. 2022. "Can a Website Bring Unemployment Down? Experimental Evidence from France." National Bureau of Economic Research.

Bertrand, Marianne. 2018. "Coase lecture–the glass ceiling." *Economica*, 85(338): 205–231.

Biasi, Barbara, and Heather Sarsons. 2022. “Flexible wages, bargaining, and the gender gap.” *The Quarterly Journal of Economics*, 137(1): 215–266.

Blatter, Marc, Samuel Muehlemann, and Samuel Schenker. 2012. “The costs of hiring skilled workers.” *European Economic Review*, 56(1): 20–35.

Blau, Francine. 2024. “Gender Inequality in the Labor Market: Continuing Progress?” National Bureau of Economic Research.

Blau, Francine D, and Lisa M Lynch. 2024. “50 Years of Breakthroughs and Barriers: Women in Economics, Policy, and Leadership.” National Bureau of Economic Research.

Bowles, Hannah Riley, Linda Babcock, and Lei Lai. 2007. “Social incentives for gender differences in the propensity to initiate negotiations: Sometimes it does hurt to ask.” *Organizational Behavior and human decision Processes*, 103(1): 84–103.

Caldwell, Sydnee, and Oren Danieli. 2024. “Outside Options in the Labour Market.” *Review of Economic Studies*, 91(6): 3286–3315.

Card, David, Ana Rute Cardoso, and Patrick Kline. 2016. “Bargaining, sorting, and the gender wage gap: Quantifying the impact of firms on the relative pay of women.” *The Quarterly journal of economics*, 131(2): 633–686.

Card, David, Jochen Kluve, and Andrea Weber. 2018. “What works? A meta analysis of recent active labor market program evaluations.” *Journal of the European Economic Association*, 16(3): 894–931.

Chan, Sewin, and Ann H Stevens. 2004. “How does job loss affect the timing of retirement?” *Contributions in Economic Analysis & Policy*, 3(1): 1–24.

Clark, Robert L, Steven Nyce, Beth Ritter, and John Shoven. 2019. “Employer concerns and responses to an aging workforce.” *The Journal of Retirement*, 6(4): 82–99.

Cortés, Patricia, Jessica Pan, Laura Pilossoph, Ernesto Reuben, and Basit Zafar. 2023. “Gender differences in job search and the earnings gap: Evidence from the field and lab.” *The Quarterly Journal of Economics*, 138(4): 2069–2126.

Cottier, Lionel, Yves Fluckiger, Pierre Kempeneers, and Rafael Lalive. 2018. "Does job search assistance really raise employment?"

Couch, Kenneth A. 1998. "Late life job displacement." *The Gerontologist*, 38(1): 7–17.

Couch, Kenneth A, and Dana W Placzek. 2010. "Earnings losses of displaced workers revisited." *American Economic Review*, 100(1): 572–89.

Crépon, Bruno, and Gerard J Van Den Berg. 2016. "Active labor market policies." *Annual Review of Economics*, 8: 521–546.

Dittrich, Marcus, Andreas Knabe, and Kristina Leipold. 2014. "Gender differences in experimental wage negotiations." *Economic Inquiry*, 52(2): 862–873.

Exley, Christine L, Muriel Niederle, and Lise Vesterlund. 2020. "Knowing when to ask: The cost of leaning in." *Journal of Political Economy*, 128(3): 816–854.

Farber, Henry S, Chris M Herbst, Dan Silverman, and Till Von Wachter. 2019. "Whom do employers want? The role of recent employment and unemployment status and age." *Journal of Labor Economics*, 37(2): 323–349.

French, Eric, and Christopher Taber. 2011. "Identification of models of the labor market." In *Handbook of Labor Economics*. Vol. 4, 537–617. Elsevier.

Goldin, Claudia. 2014. "A grand gender convergence: Its last chapter." *American economic review*, 104(4): 1091–1119.

Gronau, Reuben. 1974. "Wage comparisons–A selectivity bias." *Journal of political Economy*, 82(6): 1119–1143.

Heckman, James. 1974. "Shadow prices, market wages, and labor supply." *Econometrica: journal of the econometric society*, 679–694.

Heckman, James J, and Bo E Honore. 1990. "The empirical content of the Roy model." *Econometrica: Journal of the Econometric Society*, 1121–1149.

Hendren, Nathaniel, and Ben Sprung-Keyser. 2020. "A unified welfare analysis of government policies." *The Quarterly Journal of Economics*, 135(3): 1209–1318.

Hudomiet, Péter, Michael D Hurd, Andrew M Parker, and Susann Rohwedder. 2021. "The effects of job characteristics on retirement." *Journal of pension economics & finance*, 20(3): 357–373.

Kaiser Family Foundation. 2019. "2019 Employer health benefits survey." *KFF*.

Kluve, Jochen. 2010. "The effectiveness of European active labor market programs." *Labour economics*, 17(6): 904–918.

Le Barbanchon, Thomas, Johannes Schmieder, and Andrea Weber. 2024. "Job search, unemployment insurance, and active labor market policies." In *Handbook of Labor Economics*. Vol. 5, 435–580. Elsevier.

Le Barbanchon, Thomas, Lena Hensvik, and Roland Rathelot. 2023. "How can AI improve search and matching? Evidence from 59 million personalized job recommendations." Working Paper.

Le Barbanchon, Thomas, Roland Rathelot, and Alexandra Roulet. 2021. "Gender differences in job search: Trading off commute against wage." *The Quarterly Journal of Economics*, 136(1): 381–426.

Leibbrandt, Andreas, and John A List. 2015. "Do women avoid salary negotiations? Evidence from a large-scale natural field experiment." *Management Science*, 61(9): 2016–2024.

Maestas, Nicole, Kathleen J Mullen, David Powell, Till Von Wachter, and Jeffrey B Wenger. 2018. "The value of working conditions in the United States and implications for the structure of wages." National Bureau of Economic Research.

Marinescu, Ioana, and Ronald Wolthoff. 2020. "Opening the black box of the matching function: The power of words." *Journal of Labor Economics*, 38(2): 535–568.

Michaelides, Marios, and Peter Mueser. 2020. "The labor market effects of US reemployment policy: lessons from an analysis of four programs during the great recession." *Journal of Labor Economics*, 38(4): 1099–1140.

Neumark, David, Ian Burn, and Patrick Button. 2019. "Is it harder for older workers to find jobs? New and improved evidence from a field experiment." *Journal of Political Economy*, 127(2): 922–970.

Recalde, Maria, and Lise Vesterlund. 2020. "Gender differences in negotiation and policy for improvement." National Bureau of Economic Research.

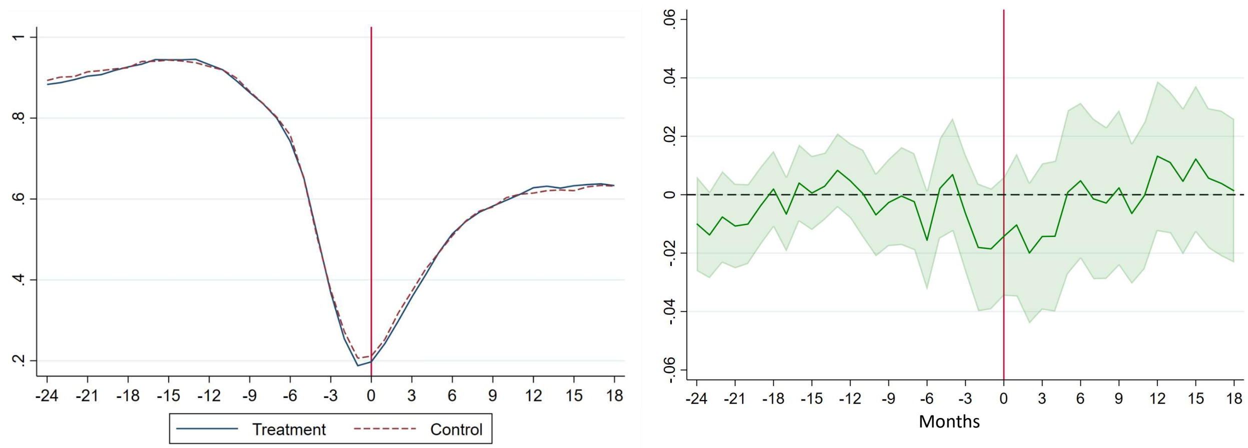
Roussille, Nina. 2024. "The role of the ask gap in gender pay inequality." *The Quarterly Journal of Economics*, qjae004.

Roy, Andrew Donald. 1951. "Some thoughts on the distribution of earnings." *Oxford economic papers*, 3(2): 135–146.

Von Wachter, Till, Jae Song, and Joyce Manchester. 2009. "Long-term earnings losses due to mass layoffs during the 1982 recession: An analysis using US administrative data from 1974 to 2004." *unpublished paper, Columbia University*.

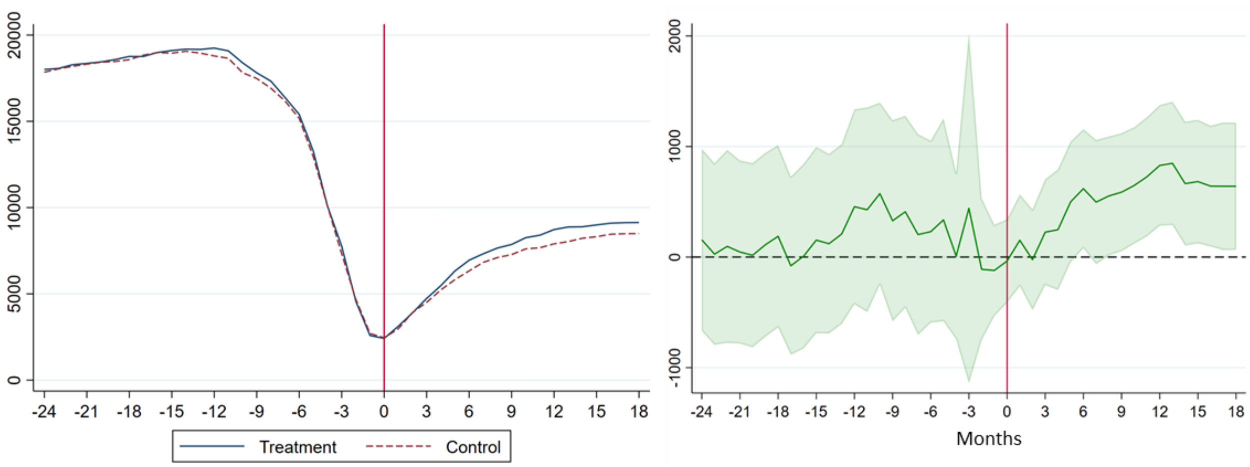
Wheeler, Laurel, Robert Garlick, Eric Johnson, Patrick Shaw, and Marissa Gargano. 2022. "LinkedIn (to) job opportunities: Experimental evidence from job readiness training." *American Economic Journal: Applied Economics*, 14(2): 101–25.

Figure 1: Dynamic Intention to Treat Effect on Employment



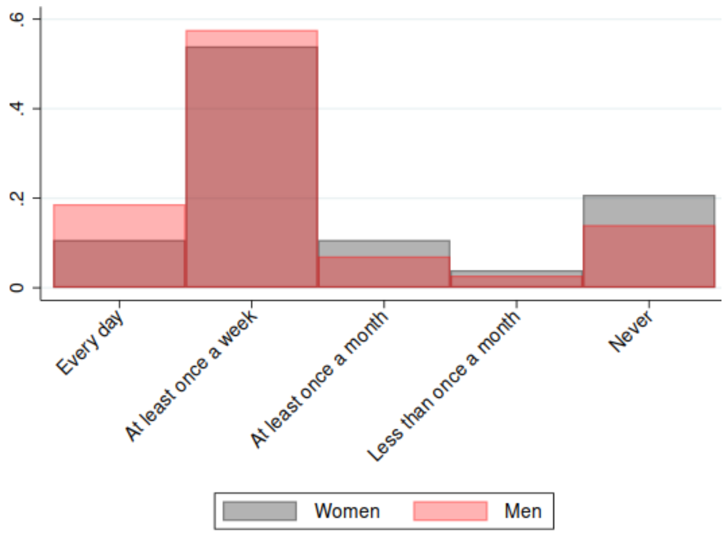
Note: The figure reports employment rates for treatment and control groups (left panel) and the difference in employment rates between the groups, along with a 90% confidence interval (right panel) over time, controlling for gender, age, marital status, education level, number of children, single mother dummy, immigrant dummy, and randomization block fixed effects. Treatment and control groups include 3,669 and 1,687 observations, respectively. Month zero corresponds to the month of random assignment. Standard errors are clustered at the randomization unit level.

Figure 2: Dynamic Intention to Treat Effect on Earnings



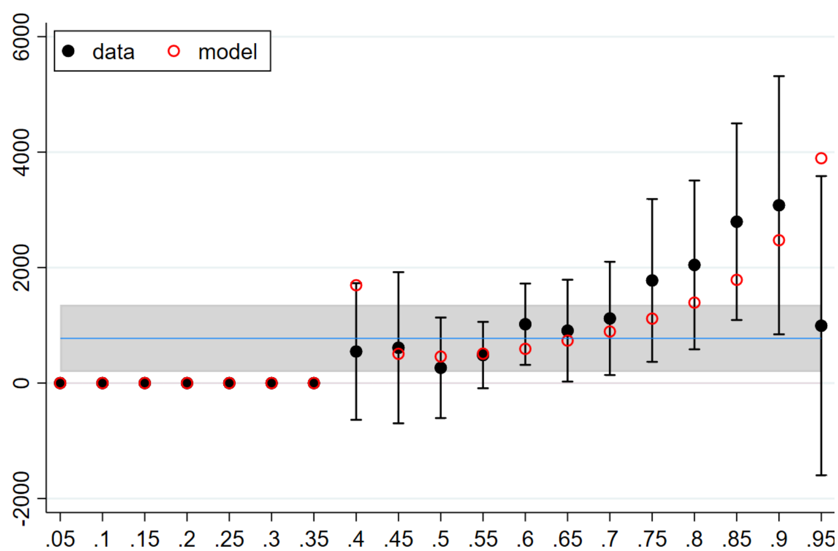
Note: The figure reports labor earnings for treatment and control groups (left panel) and the difference in labor earnings between the groups, along with a 90% confidence interval (right panel) over time, controlling for gender, age, marital status, education level, number of children, single mother dummy, immigrant dummy, and randomization block fixed effects. Treatment and control groups include 3,669 and 1,687 observations, respectively. Month zero corresponds to the month of random assignment. Standard errors are clustered at the randomization unit level.

Figure 3: Distribution of Visits to the Job-Hub by Program Participants



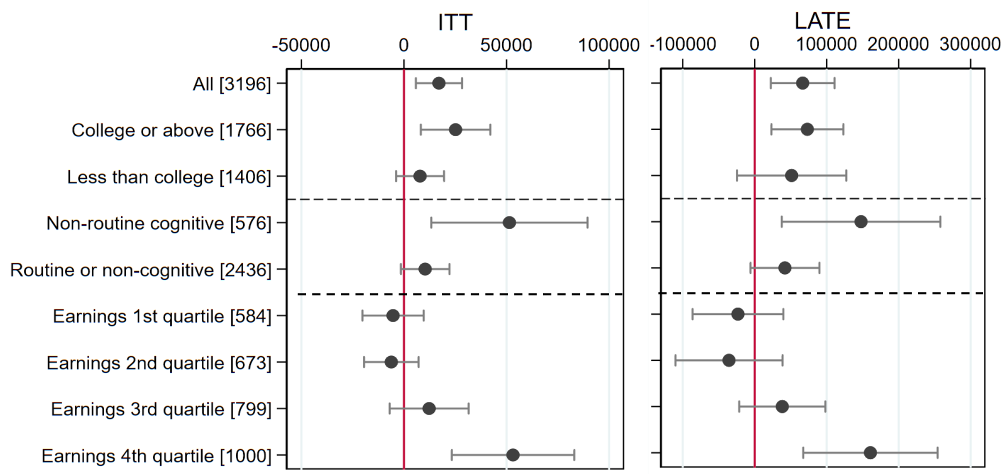
Note: The figure shows histograms for the distribution of job-hub visits of program participants by gender.

Figure 4: Quantile Treatment Effects on Men's Average Earnings at Months 9-11



Note: The black dots in the figure show Quantile ITT Effects on men's average earnings at months 9-11 post-randomization (black circles). The 90% confidence bands were constructed using bootstrapped standard errors with 200 repetitions. The blue line shows the average treatment effects. The red hollow dots show the predictions from the Roy model for the equivalent QTE.

Figure 5: Heterogeneity in Treatment Effect on Men's Cumulative Earnings



Note: The figure illustrates the treatment effect (ITT and LATE) on cumulative earnings during the eighteen months post-randomization for various sub-groups categorized by individuals' characteristics before randomization. The number of observations appears in brackets. The categorization of non-routine cognitive and routine/non-cognitive occupations was done using the task content collected in the baseline survey data (see Appendix A for details). Earnings quartile groups are defined based on average earnings in months 13-24 before randomization. Standard errors are clustered at the randomization unit level.

Table 1: Treatment Effects

Panel A: Effects of the Program 18 Months Post-Randomization	ITT			LATE		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Employed	0.003	0.020	-0.022	0.011	0.079	-0.085
	(0.015)	(0.019)	(0.021)	(0.058)	(0.075)	(0.085)
	0.632	*0.610*	*0.664*	*0.648*	*0.574*	*0.747*
Earnings	600	1,113	-122	2,355	4,349	-482
	(339)	(452)	(409)	(1,324)	(1,781)	(1,606)
	8,496	*9,581*	*6,938*	*8,339*	*8,365*	*8,315*
Average earnings in months 13-18	655	1,125	-67	2,573	4,396	-263
	(321)	(426)	(390)	(1,253)	(1,678)	(1,528)
	8,333	*9,469*	*6,704*	*8,182*	*8,521*	*8,019*
Observations	5,355	3,203	2,152			

Panel B: Cumulative Effects of the Program 18 Months Post-Randomization	ITT			LATE		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Number of months employed	0.012	0.234	-0.310	0.049	0.914	-1.219
	(0.227)	(0.291)	(0.331)	(0.888)	(1.137)	(1.311)
	9.842	*9.553*	*10.255*	*9.301*	*8.499*	*10.531*
Total earnings	9,335	17,052	-2,770	36,648	66,651	-10,895
	(5,158)	(6,842)	(6,390)	(20,159)	(26,930)	(25,104)
	125,613	*141,745*	*102,475*	*109,947*	*110,499*	*115,663*
Total unemployment benefits	261	177	385	1,024	692	1,514
	(438)	(606)	(651)	(1,706)	(2,349)	(2,537)
	19,884	*21,052*	*18,208*	*25,952*	*28,507*	*22,216*
Total social benefits	399	313	464	1,565	1,223	1,824
	(443)	(613)	(669)	(1,719)	(2,369)	(2,606)
	20,121	*21,232*	*18,527*	*25,525*	*28,064*	*22,104*
First stage				0.255	0.256	0.254
				(0.010)	(0.013)	(0.016)
				0.028	*0.034*	*0.020*
Observations	5,355	3,203	2,152			

Note: The table reports the program's effect on outcomes measured at month 18 post-randomization (Panel A) and the cumulative effect during the initial eighteen months post-randomization (Panel B). Columns 1 through 3 show reduced form estimates (ITT) for the entire sample, men and women, respectively. Control group means appear in italics. Columns 4 through 6 display the LATE estimates for the three groups, with compliers' means (non-treated) in italics, calculated based on Angrist, Hull and Walters (2022). All models include randomization block fixed effects and our vector of control variables. Monetary values are expressed in real 2018 ILS. Standard errors are reported in parentheses, clustered at the randomization unit level.

Table 2: Treatment Effect on Job Stability

	ITT			LATE		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Entire Sample						
Same job after 18 months	0.022	0.037	-0.004	0.084	0.143	-0.014
(0 if unemployed)	(0.013)	(0.017)	(0.020)	(0.049)	(0.068)	(0.078)
	0.276	*0.270*	*0.286*	*0.305*	*0.251*	*0.395*
Observations	5,355	3,203	2,152			
Panel B: Conditional on Employment						
Same job after 18 months	0.036	0.045	0.015	0.133	0.165	0.056
(conditional on employment)	(0.019)	(0.025)	(0.030)	(0.070)	(0.091)	(0.115)
	0.434	*0.439*	*0.428*	*0.453*	*0.428*	*0.526*
Observations	3,386	1,996	1,390			

Note: The table reports the program's impact on job stability eighteen months post-randomization for the entire sample (Panel A) and conditional on employment (Panel B). The outcome is a dummy variable that takes the value of one if the individual remains employed with the same employer as in their initial post-randomization job and zero otherwise. Columns 1-3 show the reduced form estimates (ITT) for the entire sample, men and women, respectively. Control group means appear in italics. Columns 4-6 report LATE estimates for the three groups with compliers' means (non-treated) in italics, calculated based on Angrist, Hull and Walters (2022). All models include randomization block fixed effects and our vector of control covariates. Standard errors are reported in parentheses, clustered at the randomization unit level.

Table 3: Treatment Effect on Women Re-weighted to Match Men's Characteristics

	ITT				LATE			
	Benchmark	Weighted by Occupation	Weighted by Past Earnings	Weighted by Occ, Earnings, Demographics	Benchmark	Weighted by Occupation	Weighted by Past Earnings	Weighted by Occ, Earnings, Demographics
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of months employed	-0.310	-0.707	-0.578	-0.958	-1.219	-2.775	-2.022	-3.616
	(0.331)	(0.430)	(0.411)	(0.505)	(1.311)	(1.736)	(1.448)	(1.986)
	10.255	*10.294*	*9.727*	*9.351*	*10.531*	*11.703*	*10.861*	*11.576*
Total earnings	-2,770	-7,612	-14,250	-18,133	-10,895	-29,869	-49,887	-68,426
	(6,390)	(8,530)	(12,354)	(15,490)	(25,104)	(33,981)	(42,796)	(58,984)
	102,475	*112,846*	*133,762*	*138,693*	*115,663*	*138,266*	*171,185*	*179,667*
First stage					0.254	0.255	0.286	0.265
					(0.016)	(0.023)	(0.021)	(0.028)
					0.020	*0.029*	*0.027*	*0.040*
Observations	2,152	2,144	2,152	2,144	2,152	2,144	2,152	2,144

Note: The table reports the program's cumulative effect on women's employment and earnings eighteen months post-randomization. Columns 1 and 5 include main estimates from 1; columns 2 and 6 show estimates after re-weighting the women's sample using the occupational distribution of men; and columns 3 and 7 re-weight the sample by men's earnings deciles in months 13-24 before randomization. In columns 4 and 8, re-weighting incorporates the comprehensive controls outlined in Section 3: demographic characteristics, employment history, earnings history, and, last occupation. All weights are computed using logistic regressions and were winsorized at the top and bottom percentiles. Control group means (columns 1-4) and compliers' means (columns 4-6) appear in italics. Compliers' means (non-treated) are calculated based on Angrist, Hull and Walters (2022). All models include randomization block fixed effects and our vector of covariates. Monetary values are expressed in real 2018 ILS. Standard errors are reported in parentheses, clustered at the randomization unit level.

Table 4: Treatment Effect on Search Practices

	ITT			LATE		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
LinkedIn	0.044	0.045	0.072	0.130	0.133	0.207
	(0.026)	(0.032)	(0.046)	(0.074)	(0.089)	(0.131)
	0.354	*0.425*	*0.264*	*0.447*	*0.548*	*0.304*
Tailored CVs	0.080	0.096	0.089	0.235	0.288	0.251
	(0.027)	(0.036)	(0.041)	(0.076)	(0.102)	(0.114)
	0.472	*0.477*	*0.466*	*0.467*	*0.451*	*0.420*
Job search websites	0.008	0.015	0.033	0.024	0.046	0.093
	(0.021)	(0.031)	(0.032)	(0.061)	(0.091)	(0.089)
	0.680	*0.678*	*0.683*	*0.781*	*0.792*	*0.680*
Social connections	-0.013	-0.016	0.009	-0.039	-0.049	0.026
	(0.022)	(0.032)	(0.030)	(0.063)	(0.097)	(0.083)
	0.739	*0.768*	*0.703*	*0.836*	*0.892*	*0.728*
Conferences & events	-0.005	-0.024	0.049	-0.014	-0.072	0.136
	(0.021)	(0.034)	(0.035)	(0.062)	(0.102)	(0.101)
	0.309	*0.357*	*0.249*	*0.370*	*0.421*	*0.197*
First stage				0.341	0.333	0.357
				(0.022)	(0.030)	(0.032)
				0.048	*0.068*	*0.024*
Observations	1,795	1,010	785	1,795	1,010	785

Note: The table reports the program's impact on current search practices or past practices if employed. Outcomes are represented by dummy variables indicating tool utilization. Columns 1-3 show the reduced form estimates (ITT) for the entire sample, men and women, respectively. Control group means appear in italics. Columns 4-6 display the LATE estimates for the three groups with compliers' means (non-treated) in italics, calculated based on Angrist, Hull and Walters (2022). All models include randomization block fixed effects and our vector of covariates. Monetary values are expressed in real 2018 ILS. Standard errors are reported in parentheses, clustered at the randomization unit level.

Table 5: Job-Hub Utilization and Program Satisfaction

	Men Mean	Women Mean	Difference (women-men)	
	(1)	(2)	(3)	(4)
Panel A: Number of Visits to the Job-Hub per Month				
Program participants	10.316	7.353	-2.963	-2.845
			(0.815)	(0.864)
Observations			386	386
All treatment group	3.673	2.603	-1.070	-0.863
			(0.351)	(0.364)
Observations			1,226	1,226
Panel B: Participants' Satisfaction with Specific Aspects of the Program				
Social connections	0.099	-0.009	-0.108	-0.220
			(0.122)	(0.135)
Graduates and employers network	0.039	0.034	-0.005	-0.101
			(0.156)	(0.162)
Workshops	0.060	0.104	0.044	0.029
			(0.120)	(0.126)
Lecturers	0.002	-0.025	-0.027	-0.068
			(0.135)	(0.155)
Overall satisfaction	0.057	0.089	0.032	-0.054
			(0.146)	(0.142)
Controls			---	V

Note: Panel A of the table reports means by gender and gender differences in visits per month to the job-hub for program participants and the entire treatment group (zero values for non-participants). To calculate visits, we assigned values 25, 9, 2, 0.5, and 0 to the responses: "Every day," "At least once a week," "At least once a month," "Less than once a month," and "Never". Panel B reports z-scores for satisfaction levels with various program aspects among men and women (columns 1 and 2) and differences by gender. Gender differences are computed after controlling for randomization block fixed effects (column 3) and for randomization block fixed effects and our vector of covariates (column 4). Standard errors are reported in parentheses, clustered at the randomization unit level.

Table 6: Treatment Effect on Job Characteristics

Panel A: Data from Follow-up Survey						
	ITT			LATE		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Work hours (weekly)	0.272	0.797	1.469	0.871	2.401	4.536
	(0.892)	(1.142)	(1.784)	(2.840)	(3.428)	(5.510)
	40.337	*43.201*	*37.140*	*41.693*	*41.311*	*35.923*
Commute time (one-way)	1.254	1.166	0.633	3.950	3.511	1.958
	(1.695)	(2.549)	(2.401)	(5.199)	(7.453)	(7.167)
	31.636	*33.140*	*30.000*	*39.581*	*38.390*	*41.283*
Observations	1,021	556	465	1,021	556	465
Panel B: Industry Features of the Individual's First Job						
	ITT			LATE		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Work hours (weekly)	-0.049	-0.244	0.173	-0.189	-0.917	0.703
	(0.116)	(0.126)	(0.226)	(0.440)	(0.477)	(0.914)
	41.858	*42.824*	*40.558*	*42.194*	*43.367*	*40.636*
Commute time (one-way)	0.410	0.428	0.513	1.571	1.605	2.087
	(0.224)	(0.271)	(0.358)	(0.863)	(1.027)	(1.442)
	28.553	*29.052*	*27.882*	*28.136*	*28.661*	*27.036*
Male-dominant industry	-0.008	-0.017	0.003	-0.029	-0.062	0.012
	(0.013)	(0.021)	(0.015)	(0.050)	(0.078)	(0.061)
	0.140	*0.191*	*0.073*	*0.130*	*0.182*	*0.050*
Observations	4,037	2,394	1,643	4,037	2,394	1,643

Note: The table reports the program's impact on job characteristics conditional on employment. Panel A reports results based on the follow-up survey data. Panel B reports outcomes calculated based on the two-digit industry features of the individual's first job, drawing from the Labor Force Survey conducted during 2017-2019, focusing on ages 45-64. Commute time is measured in minutes for a one-way trip. An industry is defined as "Male-dominated" if the share of men in that industry is 70% or higher. Columns 1-3 show the reduced form estimates (ITT), with control group means in italics. Columns 4-6 report LATE estimates with compliers' means (non-treated) in italics, calculated based on Angrist, Hull and Walters (2022). All models include randomization block fixed effects and our vector of control covariates. Standard errors are reported in parentheses, clustered at the randomization unit level.

Table 7: Moments for Roy Model Estimation

		Men		Women	
		Control	T-C	Control	T-C
	Source	(1)	(2)	(3)	(4)
Panel A: ITT effects					
Avg earnings months 9-11	Admin. data	8,303	775	6,059	-170
			(355)		(284)
Observations			4,282		3,308
Panel B: Moments in estimation					
Employment months 9-11	Admin. data	0.587	0.016	0.662	-0.044
			(0.016)		(0.018)
Observations			4,282		3,308
Log(avg wages months 9-11)	Admin. data				
First moment		9.348	0.053	8.936	0.047
			(0.030)		(0.035)
Second moment		87.931	1.059	80.342	0.848
			(0.549)		(0.619)
Observations			2,563		2,092
Log(reservation wages)	Follow-up survey				
First moment		9.632	-0.063	9.176	-0.094
			(0.086)		(0.082)
Second moment		93.162	-1.248	84.555	-1.666
			(1.661)		(1.499)
Observations			335		225

Note: Panel A reports the average level of earnings (defined as zero for the unemployed) and the ITT on earnings. Panel B reports the moments used in the structural estimation of the Roy model: the employment share, the first and second moments of log observed wages, and the first moment of log reservation wages. We also report the second moment of log reservation wages (not used in estimation). Wages are calculated as averages over months 9-11 post-randomization for employed individuals. Wages are winsorized at the top 99%. We report control sample moments separately for men and women in columns 1 and 3 and the differences between treatment and control groups for each gender in columns 2 and 4. Standard errors are reported in parentheses, clustered at the randomization unit level.

Table 8: Roy Model Results

	Men			Women		
	Treatment	Control	T-C	Treatment	Control	T-C
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Model parameters						
μ^w: Mean log market wage	9.217	9.200	0.016	8.807	8.823	-0.016
	(0.022)	(0.029)	(0.035)	(0.025)	(0.031)	(0.039)
μ^r: Mean log res. wage	8.982	9.004	-0.022	8.567	8.462	0.104
	(0.029)	(0.038)	(0.045)	(0.028)	(0.051)	(0.056)
σ^w: S.D. log market wage	0.803	0.754	0.050	0.736	0.722	0.014
	(0.015)	(0.021)	(0.025)	(0.019)	(0.027)	(0.032)
σ^r: S.D. log res. wage	0.962	0.980	-0.019	0.853	0.957	-0.104
	(0.034)	(0.046)	(0.060)	(0.039)	(0.056)	(0.067)
ρ: ϵ^w and ϵ^r correlation	0.5			0.5		
Panel B: Bias factors						
Market wage	0.185	0.148	0.037	0.176	0.113	0.063
	(0.016)	(0.022)	(0.029)	(0.018)	(0.020)	(0.026)
Reservation wage	0.586	0.628	-0.042	0.515	0.713	-0.198
	(0.043)	(0.058)	(0.076)	(0.048)	(0.079)	(0.091)

Note: The table reports estimated parameters of the Roy model separately for men (columns 1-3) and women (columns 4-6), assuming a fixed ρ value of 0.5. Columns 1 and 4 display results for the treatment group; columns 2 and 5 present results for the control group. Columns 3 and 6 show the treatment effect, indicating the differences between treatment and control groups. Standard errors are reported in parentheses, clustered at the randomization unit level.

Table 9: The Change in Reservation Wages and Market Wages

	Men		Women	
	Control	Treatment	Control	Treatment
	(1)	(2)	(3)	(4)
$\Delta\mu^w$: Change in mean log market wage	-0.580	-0.543	-0.527	-0.553
$\Delta\mu^r$: Change in mean log res. wage	-0.686	-0.648	-0.788	-0.663
Employment (post-randomization)	0.587	0.604	0.662	0.618

Note: The table reports dynamics in the market (μ^w) and reservation (μ^r) log (average) wages and employment rates, separately for men (columns 1-2) and women (columns 3-4), and for treatment and control groups. The change in market wages is calculated as the difference between the average log monthly earnings over months 13-24 before randomization and the estimated mean derived from the Roy model nine months post-randomization. The change in reservation wages is computed as the difference between the baseline survey's average and the estimated mean derived from the Roy Model nine months post-randomization. Employment rates are during the time of the follow-up survey (about nine months post-randomization.)

Job Search Assistance for Older Unemployed in the Digital Era: Evidence from a Large Scale RCT

Online Appendix

Not for Publication

Appendix A Survey Description and Classification of Occupations

We conducted a baseline survey and a follow-up survey to measure various aspects of job search and employment that are not recorded in the administrative data. Both surveys were administered by phone through a third-party company. The baseline survey was conducted before randomization; the follow-up survey took place nine to ten months after randomization. Response rates for the baseline survey were approximately 40% for men and 35% for women. Follow-up survey response rates were approximately 35% for men and 30% for women. Below, we describe how we constructed the main variables used in our empirical analysis from the survey data.

Job search practices: We collected data on job search practices by asking respondents about their search methods. Unemployed respondents at the time of the survey were asked about their current search practices, while employed respondents were asked retrospectively about their activities during unemployment. Specifically, individuals were asked whether they used the following search strategies: LinkedIn, CV tailoring for specific job posting, job search websites, social connections, and attendance at events and professional conferences. We created binary indicators to denote the use of each search strategy.

Program utilization and satisfaction: For treated individuals, we included a module at the end of the follow-up survey asking about their participation in the program and utilization of various program components. Specifically, respondents were asked *"During the program, how often did you use the Job-Hub services?"*. Based on this question, we defined a variable denoting the visits to the job-hub per month, assigning values of 25, 9, 2, 0.5, and 0 to the responses: "Every day," "At least once a week," "At least once a month," "Less than once a month," and "Never" to calculate average visits per month. Program participants were also asked about their satisfaction level with program components using a five-point Likert scale: (5) To a very great extent, (4) To a great extent, (3) To a moderate extent, (2) To a low extent, (1) Not at all. The questions were: To what extent are you satisfied with: (1) the social connections made during the program; (2) the program graduates and employers' network; (3) the content of the workshops; (4) the quality of the lecturers. We created z-scores for the responses to each question.

Job characteristics: We collected data on job characteristics of employed workers to supplement the information recorded in the administrative data. Specifically, we gathered information on weekly work hours and daily commute time (one-way) in minutes. Weekly hours were winsorized at the top 99th percentile.

Reservation wages: We collected reservation wage data in both the baseline and follow-up surveys using the following approach. We first asked unemployed respondents to report their preferred occupation(s) or position(s), and then asked them two questions about their desired work schedule: the number of days per week and hours per day they would like to work. We calculated each respondent's desired monthly work hours by multiplying days per week by hours per day, and then by 4.345. Respondents could then choose to report their reservation wage in either hourly or monthly terms by answering the question: *"What is the lowest hourly (monthly) wage you would be willing to work for?"*. For those who provided hourly rates, we converted their responses to monthly terms using their stated desired work hours.

Life, economic, and job match satisfaction: We collected information on individuals' life and economic satisfaction in both surveys using a four-point Likert scale: (4) Very satisfied, (3) Satisfied, (2) Not so satisfied, (1) Not satisfied at all. We asked two questions: (1) *In view of the overall situation, how satisfied are you with your life these days?*; (2) *"How satisfied are you with your economic situation?"*. For both questions, we constructed z-scores by standardizing responses.

The follow-up survey also assessed job satisfaction among employed individuals using three questions rated on a five-point scale: (5) To a very large extent, (4) To a large extent, (3) To a moderate extent, (2) To a small extent, (1) Not at all. The questions were: (1) *"To what extent are you satisfied with your current job?"*; (2) *"To what extent are you satisfied with your salary?"*; (3) *"To what extent do you feel that your job matches your skills?"*. We standardized responses to each question, then constructed an overall job satisfaction index by taking the average of these three z-scores and standardizing the result.

Occupations and task classification: We used our survey data to classify occupations according to their task content. The non-routine-cognitive (NRC) indicator used in Figure 5 was generated in three steps. First, we used the baseline survey module that recorded information on the individual's last job. Following the automation literature, we used the task content collected in the survey to characterize occupations along two dimensions: routine vs. non-routine, and cognitive vs. manual occupations (see, e.g., Acemoglu and Autor (2011)). We measured the task content of a job using the questions outlined in Autor and Handel (2013). A job received a higher manual score if a larger part of the workday was dedicated to performing physical tasks based on responses to the question: *"How much of your workday was dedicated to performing physical tasks such as standing, moving objects, operating machinery or vehicles, manual production, or repair?"*. Responses were ranked on a five-point scale: (1) Never, (2) Rarely, (3) Less than half, (4) More than half, (5) Almost all the time. For the routine score, we analyzed two components: (1) a question determining how much of the workday was dedicated to performing short, repetitive tasks, and (2) four questions on frequency of face-to-face interactions with the following groups: customers, suppliers or contractors, students or trainees, and patients. Face-to-face interaction frequency was coded as (1) Frequently, (2) Occasionally, and (3) Not at all, so that higher values indicate higher routine job content. We constructed standardized routine z-scores by averaging the z-scores of these responses.

The second step used IES occupational data, which classifies each individual's most recent job into one of approximately 700 distinct occupations. We computed the average manual and routine scores for each occupation based on the baseline survey responses, then classified occupations as manual or routine if their scores exceeded the median.[1]

In the third step, we defined an indicator for each individual denoting whether their last job was a non-routine cognitive one. This indicator equals one if the individual's last occupation scored below the median on both manual and routine dimensions, and zero otherwise.

Appendix B Selection into the Follow-up Survey

We provide in this section additional information on the follow-up surveys' data collection, response rates, and selection into the survey.

We examined whether there was differential selection into the follow-up survey by treatment status for men and for women by estimating a linear probability model as a function of treatment status interacted with gender and our full set of controls (including randomization cells). Results reported in Table A.6 suggest some selection into the survey based on predetermined covariates: older individuals, those with a more recent employment history, and college graduates were more

[1] We omitted occupations with fewer than five responses.

likely to respond to the survey, while immigrants were less likely to respond. Nevertheless, there was no differential selection into the survey by treatment status for either gender (p-value for F-test of joint significance = 0.22).

To assess the representativeness of the follow-up survey sample, we reproduced our main results from the administrative data using only the sample of survey respondents. We conducted this analysis in two ways: first using the unweighted sample of survey respondents, and second using the same sample weighted by the inverse probability of selection into the survey.[2] Since the outcomes of the follow-up survey were measured nine months after random assignment, we assessed the comparability between the full sample and the survey sample using the administrative outcomes at month nine. Results are reported in Table A.7. For both men (columns 1-3) and women (columns 4-6), we compare results from three samples: the full sample, the unweighted sample of survey respondents, and the weighted sample of survey respondents. Treatment effects and outcome means remain consistent across all three samples, with only minor differences between the weighted and unweighted survey respondent samples. Based on this consistency, our subsequent analyses of the follow-up survey data use unweighted observations, as the weights do not meaningfully alter our findings.

Finally, we performed a balancing test for the survey sample to verify that treatment and control groups remained balanced, even though we found no differential selection into the survey by treatment status. Results, reported in Table A.8, show that the survey sample is indeed balanced on predetermined characteristics for both men and women. The p-values for joint significance tests of all covariates are 0.53 and 0.22 for men and women, respectively, confirming that randomization integrity is preserved within the follow-up survey sample.

[2]To construct the sample weights, we estimated a logistic regression that predicted the likelihood of survey response as a function of individual characteristics, employment history in the three years preceding randomization, and fixed effects for allocation date and employment office, separately for treatment and control groups. We winsorized the weights, and each observation was then weighted by the inverse of the predicted response probability.

Appendix C Survey Structural Model Identification Equations

The following explains how we identify the parameters of the Roy model discussed in Section 5.4.4.

We denote an individual's log market wage by y_i^w, and their log reservation wage by y_i^r. The model has two main equations: the potential wage equation (A.1) and the reservation wage (A.2):

$$y_i^w = \mu^w + \varepsilon_i^w \tag{A.1}$$

$$y_i^r = \mu^r + \varepsilon_i^r, \tag{A.2}$$

where: μ^w and μ^r are the respective means and ε_i^w and ε_i^r are the error terms, or deviations from group averages. A worker chooses to work ($L_i = 1$) if $y_i^w \geq y_i^r$, that is:

$$L_i = 1 \ \text{ if } \ \varepsilon_i^r - \varepsilon_i^w \leq \mu^w - \mu^r$$

We assume that ε_i^w and ε_i^r are jointly normally distributed:

$$\begin{pmatrix} \varepsilon_i^w \\ \varepsilon_i^r \end{pmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \sigma_w^2 & \rho \cdot \sigma_w \cdot \sigma_r \\ \rho \cdot \sigma_w \cdot \sigma_r & \sigma_r^2 \end{bmatrix} \right),$$

where σ_w and σ_r are the standard deviations of the error terms, and ρ is their correlation. As discussed in Heckman and Honore (1990) and French and Taber (2011), assuming normality, one can identify the joint distribution of (y_i^w, y_i^r) using observed market and reservation wages without an exclusion restriction. The model's five parameters $(\mu_w, \mu_r, \sigma_w, \sigma_r, \rho)$ are exactly identified using the following five equations, which we derive from the properties of the truncated normal distribution:

$$Pr(L_i = 1) = \Phi(c) \tag{A.3}$$

$$E(y_i^{\omega}|L_i = 1) = \mu^w + \tau^w \lambda(c) \tag{A.4}$$

$$E(y_i^r|L_i = 0) = \mu^r + \tau^r \lambda(-c) \tag{A.5}$$

$$Var(y_i^{\omega}|L_i = 1) = \sigma_w^2 + (\tau^w)^2 \left[-c \cdot \lambda(c) - \lambda^2(c)\right] \tag{A.6}$$

$$Var(y_i^r|L_i = 0) = \sigma_r^2 + (\tau^r)^2 \left[c \cdot \lambda(-c) - \lambda^2(-c)\right] \tag{A.7}$$

where $\Phi(\cdot)$ is the CDF of the Standard Normal distribution, and $\lambda(\cdot) \equiv \frac{\phi(\cdot)}{\Phi(\cdot)}$ is the inverse Mill's ratio.[3] c , τ_j^w, and τ_j^r defined as:

$$c \equiv \frac{\mu^w - \mu^r}{\sqrt{\sigma_w^2 + \sigma_r^2 - 2\rho \cdot \sigma_w \cdot \sigma_r}} \tag{A.8}$$

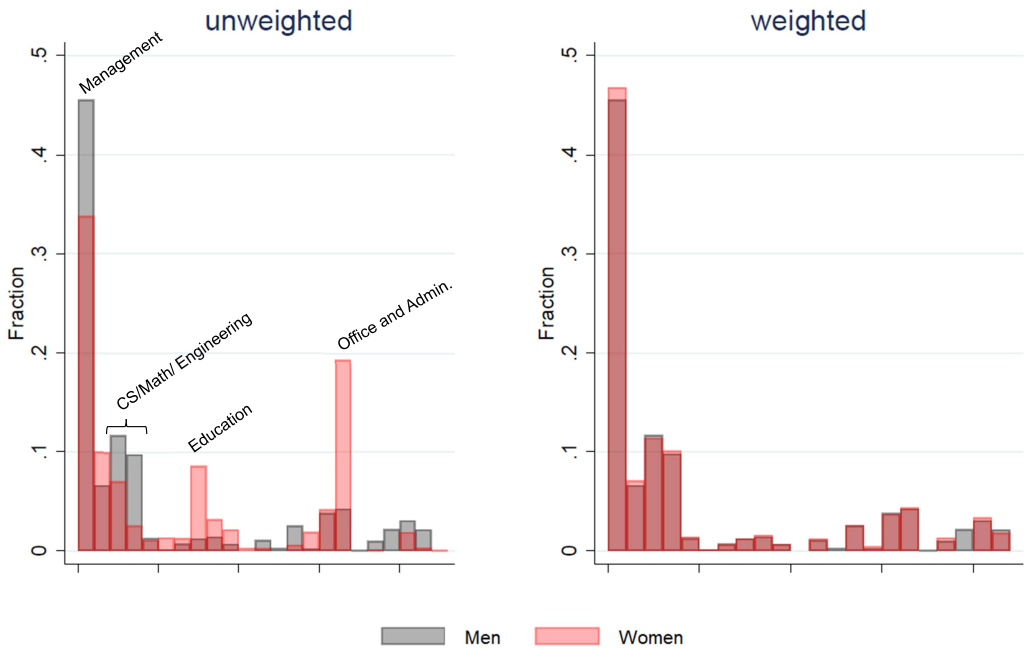
$$\tau^k \equiv \frac{\sigma_k^2 - \rho \cdot \sigma_w \cdot \sigma_r}{\sqrt{\sigma_w^2 + \sigma_r^2 - 2\rho \cdot \sigma_w \cdot \sigma_r}}, \quad k \in \{w, r\} \tag{A.9}$$

We estimate the model, allowing for different parameters for men and women, as well as for treatment and control groups. Our small survey sample size implies a noisy second moment for the reservation wages. Hence, we omit this moment (equation A.7) from our system, and set $\rho = 0.5$ rather than estimate it. We conduct a sensitivity analysis to assess the robustness of our results to alternative values of ρ.

[3]Namely, $\lambda(\cdot) \equiv \frac{\phi(\cdot)}{\Phi(\cdot)}$,where $\phi(\cdot)$ and $\Phi(\cdot)$ are the PDF and CDF of the Standard Normal distribution.

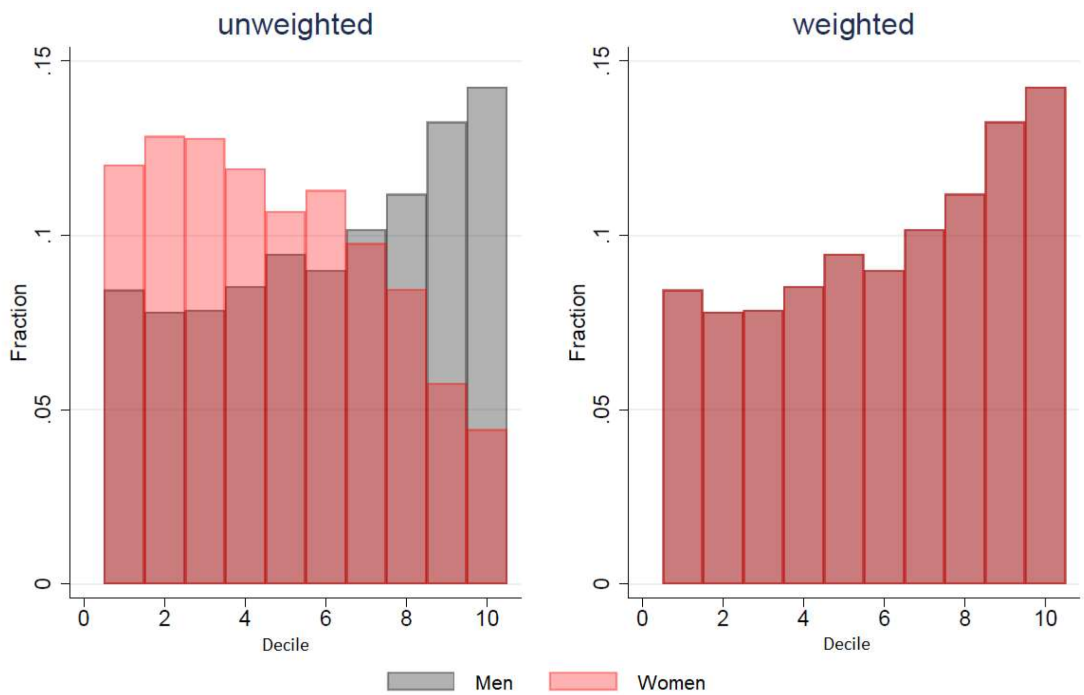
Appendix D Appendix Tables and Figures

Figure A.1: Re-Weighting Women to Match Men's Occupational Distribution



Note: The figure illustrates the occupational distributions of men and women in their pre-unemployment job using the main 20 SOC categories(2010). The left figure illustrates the actual distributions, while the right figure shows the distributions after we re-weight women's records to match the occupational distribution of men. Weights are computed using logistic regression of a dummy variable "female" on the 20 main SOC categories as explanatory variables.

Figure A.2: Re-weighting Women to Match Men's Earnings Distribution



Note: The figure illustrates the earnings distributions of men and women in months 13-24 before randomization. The left figure illustrates the actual distributions, while the right figure shows the distributions after we re-weight women's records to match the earnings distribution of men. Weights are computed using a logistic regression of a dummy variable "female" on earnings deciles in months 13-24 before randomization.

Table A.1: Occupational Distribution Based on Last Job

	All	Men	Women
Management	40.8%	47.0%	33.0%
Business and Financial Operations	8.1%	7.0%	9.6%
Computer and Mathematical	9.8%	11.9%	7.1%
Architecture and Engineering	6.4%	9.7%	2.4%
Life, Physical, and Social Science	1.3%	1.5%	1.1%
Legal	1.3%	0.9%	1.7%
Education, Training, and Library	5.0%	1.4%	9.5%
Arts, Design, Entertainment, Sports, and Media	2.4%	1.7%	3.3%
Healthcare Practitioners and Technical	1.4%	0.7%	2.2%
Maintenance	1.7%	2.5%	0.5%
Personal Care and Service	1.1%	0.5%	1.9%
Sales and Related	4.3%	4.1%	4.6%
Office and Administrative Support	11.6%	4.7%	20.3%
Production	2.6%	3.2%	1.7%
Transportation and Material Moving	1.6%	2.5%	0.5%
Undefined	0.6%	0.6%	0.7%

Note: The table shows the distribution of pre-unemployment occupations in our main sample, categorized by the 20 major SOC (Standard Occupational Classification) groups. Occupations with fewer than ten observations are excluded.

Table A.2: Descriptive Statistics and Balancing Tests

	Men			Women		
	Treatment	Control	Difference	Treatment	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Age	56.075	55.828	0.287	54.596	54.637	-0.111
	(4.022)	(4.017)	(0.159)	(2.918)	(2.928)	(0.137)
Married	0.724	0.697	0.020	0.559	0.522	0.041
	(0.447)	(0.460)	(0.017)	(0.497)	(0.500)	(0.021)
College graduate	0.561	0.541	0.021	0.472	0.456	0.002
	(0.496)	(0.499)	(0.019)	(0.499)	(0.498)	(0.024)
Number of children	2.437	2.478	-0.049	2.206	2.250	-0.052
	(1.322)	(1.295)	(0.051)	(1.246)	(1.251)	(0.061)
Single mother				0.121	0.131	-0.008
				(0.327)	(0.338)	(0.018)
Immigrant	0.155	0.134	0.016	0.155	0.130	0.018
	(0.362)	(0.341)	(0.013)	(0.362)	(0.336)	(0.015)
Months employed (-1 to -12)	7.950 (3.096)	8.025 (2.997)	-0.037 (0.101)	7.748 (3.021)	8.095 (3.039)	-0.083 (0.116)
Months employed (-13 to -24)	10.969 (2.474)	11.025 (2.420)	-0.052 (0.101)	11.184 (2.129)	11.170 (2.136)	0.063 (0.104)
Months employed (-25 to -36)	10.295 (3.369)	10.177 (3.580)	0.127 (0.152)	10.408 (3.225)	10.473 (3.233)	-0.082 (0.152)
Labor earnings (months -1 to -12)	188,025 (270,631)	190,204 (201,646)	1,123 (8,224)	115,777 (126,923)	113,639 (106,542)	7,097 (4,695)
Labor earnings (months -13 to -24)	259,067 (296,608)	263,100 (249,790)	-937 (9,802)	169,907 (164,529)	164,646 (163,847)	4,179 (7,295)
Labor earnings (months -25 to -36)	241,748 (263,575)	240,350 (235,146)	4,603 (8,874)	156,134 (161,193)	149,254 (142,484)	4,953 (6,566)
Observations	2,210	994		1,459	693	
F-test for joint significance			0.330			0.100

Note: The table presents average characteristics and balancing tests for men (columns 1-3) and women (columns 4-6) in our main sample. Columns 1 and 4 show the average characteristics (standard deviation in parentheses) of participants in the treatment group; columns 2 and 5 show the same for the control group. Columns 3 and 6 display the estimated difference between treatment and control groups conditional on randomization unit fixed effects. Standard errors of the differences (in parentheses) are clustered at the randomization unit level. At the bottom, we report the p-value for the joint significance of all covariates based on a linear probability model of treatment status on all covariates conditional on randomization unit fixed effects. Employment and earnings history are displayed for months 1-12, 13-24, and 24-36 before randomization. Monetary values are expressed in real 2018 ILS.

Table A.3: Labor Earnings: Main Sample Compared to General Population

	Men (1)	Women (2)
RCT sample	22,658	14,595
	(23,518)	(13,710)
Population means		
All	16,992	11,495
	(14,981)	(9,449)
College graduate	23,910	15,626
	(17,766)	(11,511)
Re-weighted by occupations	22,634	14,324
	(17,653)	(10,680)

Note: The table compares monthly labor earnings between our analysis sample and the Israeli Jewish population, aged 50-64 for men and 50-59 for women. The first row displays average monthly earnings during months 13-24 before randomization. Means for the general population are based on the Household Expenditure Survey (2018-2019). They show earnings for the entire Jewish population of the relevant age, for college graduates only, and for the population re-weighted to match our sample's one-digit occupation distribution. Standard deviations appear in parentheses.

Table A.4: Program Participation

	All (1)	Men (2)	Women (3)
Women	-0.002		
	(0.018)		
Married	0.021	0.035	-0.003
	(0.018)	(0.023)	(0.030)
College graduate	0.166	0.190	0.133
	(0.014)	(0.019)	(0.025)
Age	-0.006	-0.005	-0.008
	(0.002)	(0.002)	(0.004)
Number of children	-0.012	-0.008	-0.017
	(0.006)	(0.007)	(0.010)
Single mother	0.061		0.045
	(0.035)		(0.041)
Immigrant	-0.112	-0.122	-0.091
	(0.019)	(0.024)	(0.031)
Months employed	0.014	0.015	0.011
(-1 to -12)	(0.002)	(0.003)	(0.003)
Months employed	-0.012	-0.014	-0.007
(-13 to -24)	(0.004)	(0.004)	(0.006)
Months employed	-0.002	-0.003	-0.001
(-25 to -36)	(0.002)	(0.003)	(0.004)
Labor earnings - Q2	0.028	-0.007	0.066
(months -13 to -24)	(0.022)	(0.030)	(0.028)
Labor earnings - Q3	0.110	0.105	0.115
(months -13 to -24)	(0.023)	(0.031)	(0.034)
Labor earnings - Q4	0.115	0.087	0.156
(months -13 to -24)	(0.024)	(0.031)	(0.039)
Compliance rate	0.279	0.290	0.264
Observations	3,668	2,209	1,459

Note: The table reports estimates from a linear probability model for the probability of participation in the program by the treatment group, as a function of personal characteristics, employment history (number of months employed in months 1-12, 13-24, and 24-36 before randomization), and earnings quartile during months 13-24 before randomization. Standard errors reported in parentheses are clustered at the randomization unit level.

Table A.5: Gender Differences in Treatment Effects

Panel A: Treatment Effects 18 Months Post-Randomization				
	ITT		LATE	
	Treatment	Treatment X Women	Treatment	Treatment X Women
	(1)	(2)	(3)	(4)
Employed	0.021	-0.044	0.081	-0.176
	(0.019)	(0.026)	(0.074)	(0.104)
Earnings	1,052	-1,113	4,110	-4,392
	(442)	(549)	(1,735)	(2,158)
Average earnings in months 13-18	1,080	-1,046	4,217	-4,117
	(417)	(516)	(1,638)	(2,025)
Observations	5,355			

Panel B: Cumulative Treatment Effects 18 Months Post-Randomization				
	ITT		LATE	
	Treatment	Treatment X Women	Treatment	Treatment X Women
	(1)	(2)	(3)	(4)
Number of months employed	0.266	-0.624	1.043	-2.488
	(0.284)	(0.396)	(1.109)	(1.575)
Total earnings	16,492	-17,625	64,424	-69,546
	(6,690)	(8,269)	(26,262)	(32,402)
Total unemployment benefits	107	378	412	1,532
	(598)	(890)	(2,323)	(3,519)
Total social benefits	253	359	980	1,465
	(606)	(914)	(2,347)	(3,612)
First stage			0.257	-0.005
			(0.013)	(0.019)
Observations	5,355			

Note: The table reports differences in treatment effects (ITT and LATE) by gender. Panel A displays outcomes in month 18 post-randomization, while Panel B shows the cumulative effect over the eighteen-month period. Columns 1 and 3 present the effect for men; columns 2 and 4 present the differential effects for women relative to men. All models include randomization block fixed effects and our vector of control covariates. Monetary values are expressed in real 2018 ILS. Standard errors reported in parentheses are clustered at the randomization unit level.

Table A.6: Selection into the Follow-up Survey

Treatment X men	0.016	Immigrant	-0.087
	(0.018)		(0.018)
Treatment X women	0.029	Months employed	0.009
	(0.020)	(-1 to -12)	(0.003)
Women	-0.016	Months employed	-0.002
	(0.021)	(-13 to -24)	(0.003)
Married	0.010	Months employed	0.000
	(0.017)	(-25 to -36)	(0.002)
College graduate	0.067	Labor earnings	0.006
	(0.015)	(months -1 to -12)	(0.008)
Age	0.005	Labor earnings	0.001
	(0.002)	(months -13 to -24)	(0.010)
Number of children	0.005	Labor earnings	0.004
	(0.006)	(months -25 to -36)	(0.010)
Single mother	-0.014		
	(0.027)		
Participation rate	0.333		
Observations	5,416		

Note: The table reports estimates from a linear probability model for the probability of participation in the follow-up survey, as a function of treatment status interacted with gender, personal characteristics, and employment and earnings history (number of months employed and earnings in months 1-12, 13-24, and 24-36 before randomization). Monetary values are expressed in hundreds of thousands of real 2018 ILS. Standard errors reported in parentheses are clustered at the randomization unit level.

Table A.7: Treatment Effects Reproduced in the Follow-up Survey Sample

	Men			Women		
	Main sample	Survey Sample		Main sample	Survey Sample	
	Unweighted	Unweighted	Weighted	Unweighted	Unweighted	Weighted
	(1)	(2)	(3)	(4)	(5)	(6)
Number of months employed	0.075	0.259	0.212	-0.229	-0.400	-0.394
	(0.156)	(0.254)	(0.269)	(0.185)	(0.356)	(0.364)
	4.110	*3.463*	*3.440*	*4.457*	*4.372*	*4.444*
Total earnings	5,640	5,813	5,758	-1,337	1,336	1,271
	(3,235)	(6,326)	(5,761)	(2,975)	(5,364)	(4,898)
	57,860	*59,168*	*54,659*	*42,044*	*41,199*	*40,960*
Observations	3,196	988	988	2,138	760	760

Note: The table reports the program's effect on the follow-up survey sample, focusing on the cumulative effect during the initial nine months to align with the timing of the survey. Earnings are winsorized at the top 99%; unemployed are set to zero earnings. Columns 1 and 4 display unweighted specifications for the main sample. Columns 2 and 5 display unweighted specifications for the follow-up survey sample, while columns 3 and 6 display weighted specifications for this sample. We apply re-weighting to match the characteristics of our main sample. Weights are computed using a logistic regression (see Appendix B for details). Columns 1-3 display reduced form estimates (ITT) for men; columns 4-6 do the same for women. Control group means appear in italics. Monetary values are expressed in real 2018 ILS. Standard errors are reported in parentheses, clustered at the randomization unit level.

Table A.8: Descriptive Statistics and Balancing Tests for Follow-up Survey

	Men			Women		
	Treatment	Control	Difference	Treatment	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Age	56.587	56.477	0.161	53.705	53.595	0.033
	(4.105)	(4.110)	(0.324)	(3.788)	(3.576)	(0.252)
Married	0.743	0.723	0.023	0.575	0.561	0.017
	(0.438)	(0.448)	(0.031)	(0.495)	(0.497)	(0.050)
College graduate	0.613	0.598	0.021	0.534	0.589	-0.039
	(0.487)	(0.491)	(0.034)	(0.499)	(0.493)	(0.038)
Number of children	2.491	2.614	-0.116	2.327	2.146	0.099
	(1.217)	(1.270)	(0.097)	(1.311)	(1.174)	(0.110)
Single mother				0.126	0.138	-0.005
				(0.332)	(0.346)	(0.034)
Immigrant	0.104	0.109	0.010	0.102	0.103	-0.005
	(0.305)	(0.313)	(0.022)	(0.303)	(0.304)	(0.022)
Months employed	8.384	8.363	-0.094	7.996	8.431	-0.290
(-1 to -12)	(2.413)	(2.379)	(0.149)	(2.446)	(2.370)	(0.179)
Months employed	11.226	11.299	-0.131	11.249	11.372	-0.046
(-13 to -24)	(2.012)	(1.957)	(0.147)	(1.984)	(1.840)	(0.164)
Months employed	10.626	10.386	0.105	10.538	10.289	0.349
(-25 to -36)	(3.045)	(3.345)	(0.291)	(3.233)	(3.474)	(0.261)
Labor earnings	223,330	244,133	-29,301	135,480	136,876	7,203
(months -1 to -12)	(249,267)	(269,897)	(18,404)	(131,988)	(106,967)	(10,546)
Labor earnings	298,675	326,037	-32,032	193,671	188,644	14,579
(months -13 to -24)	(281,140)	(360,515)	(25,341)	(175,480)	(142,625)	(13,124)
Labor earnings	282,093	294,888	-13,050	176,911	171,125	13,277
(months -25 to -36)	(281,419)	(341,017)	(25,736)	(166,440)	(142,265)	(12,881)
Observations	703	311		539	253	
F-test for joint significance			0.529			0.222

Note: The table presents average characteristics and balancing tests for men (columns 1-3) and women (columns 4-6) in the follow-up survey sample. Columns 1 and 4 show means (standard deviation in parentheses) of participants in the treatment group; columns 2 and 5 show the same for the control group. Columns 3 and 6 display the estimated difference between treatment and control groups conditional on randomization unit fixed effects. Standard errors of the differences (in parentheses) are clustered at the randomization unit level. At the bottom, we report the p-value for the joint significance of all covariates based on a linear probability model of treatment status on all covariates conditional on randomization unit fixed effects. Employment and earnings history are displayed for months 1-12, 13-24, and 24-36 before randomization. Monetary values are expressed in real 2018 ILS.

Table A.9: Pre-job Loss Wages vs. Reservation Wages at Baseline

	Men	Women
Log(pre-job loss wages)	9.764	9.359
	(0.768)	(0.716)
Observations	4,222	3,275
Log(reservation wage)	9.650	9.239
	(0.535)	(0.548)
Observations	823	677

Note: The table reports the pre-job loss log wages from the administrative data and the mean log of reservation wages from the baseline survey. Pre-job loss wages are defined as the log monthly earnings averaged over months 13-24 before randomization, computed from the administrative data. Standard deviations appear in parentheses.

Table A.10: Roy Model Robustness: $\rho = 0.4$

	Men			Women		
	Treatment	Control	T-C	Treatment	Control	T-C
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Model parameters						
μ^w: Mean log market wage	9.187	9.168	0.018	8.781	8.798	-0.017
	(0.022)	(0.029)	(0.036)	(0.025)	(0.032)	(0.039)
μ^r: Mean log res. wage	8.923	8.948	-0.025	8.512	8.393	0.119
	(0.031)	(0.040)	(0.048)	(0.029)	(0.055)	(0.060)
σ^w: S.D. log market wage	0.813	0.762	0.050	0.746	0.729	0.017
	(0.016)	(0.022)	(0.026)	(0.019)	(0.028)	(0.033)
σ^r: S.D. log res. wage	0.994	1.016	-0.022	0.881	0.997	-0.116
	(0.037)	(0.050)	(0.065)	(0.042)	(0.061)	(0.073)
ρ: ϵ^w and ϵ^r correlation	0.4			0.4		
Panel B: Bias factors						
Market wage	0.215	0.180	0.035	0.202	0.137	0.065
	(0.016)	(0.022)	(0.028)	(0.018)	(0.021)	(0.026)
Reservation wage	0.645	0.684	-0.038	0.571	0.783	-0.212
	(0.045)	(0.061)	(0.079)	(0.051)	(0.084)	(0.096)

Note: The table reports estimated parameters of the Roy model separately for men (columns 1-3) and women (columns 4-6), assuming a fixed ρ value of 0.4. Columns 1 and 4 display results for the treatment groups; columns 2 and 5 present results for the control groups. Columns 3 and 6 show the treatment effect, indicating the differences between treatment and control groups. Standard errors are presented in parentheses and are clustered at the randomization unit level.

Table A.11: Roy Model Robustness: $\rho = 0.6$

	Men			Women		
	Treatment	Control	T-C	Treatment	Control	T-C
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Model parameters						
μ^w: Mean log market wage	9.248	9.234	0.014	8.835	8.848	-0.013
	(0.022)	(0.029)	(0.035)	(0.025)	(0.031)	(0.038)
μ^r: Mean log res. wage	9.043	9.063	-0.020	8.625	8.534	0.091
	(0.028)	(0.036)	(0.043)	(0.028)	(0.046)	(0.052)
σ^w: S.D. log market wage	0.794	0.745	0.049	0.726	0.716	0.010
	(0.015)	(0.020)	(0.024)	(0.019)	(0.027)	(0.032)
σ^r: S.D. log res. wage	0.930	0.943	-0.013	0.824	0.916	-0.092
	(0.031)	(0.042)	(0.055)	(0.036)	(0.052)	(0.062)
ρ: ϵ^w and ϵ^r correlation	0.6			0.6		
Panel B: Bias factors						
Market wage	0.153	0.114	0.039	0.149	0.087	0.061
	(0.017)	(0.023)	(0.030)	(0.018)	(0.021)	(0.026)
Reservation wage	0.525	0.569	-0.044	0.457	0.641	-0.184
	(0.040)	(0.056)	(0.072)	(0.046)	(0.074)	(0.085)

Note: The table reports estimated parameters of the Roy model separately for men (columns 1-3) and women (columns 4-6), assuming a fixed ρ value of 0.6. Columns 1 and 4 display results for the treatment groups; columns 2 and 5 present results for the control groups. Columns 3 and 6 show the treatment effect, indicating the differences between treatment and control groups. Standard errors are presented in parentheses and are clustered at the randomization unit level.

Table A.12: Treatment Effects on Life, Economic, and Job Satisfaction

	ITT			LATE		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Entire Sample						
Life satisfaction	0.002	0.066	-0.044	0.006	0.212	-0.127
	(0.051)	(0.072)	(0.076)	(0.155)	(0.226)	(0.217)
	-0.018	*0.004*	*-0.045*	*-0.014*	*-0.134*	*-0.003*
Economic satisfaction	-0.012	0.065	-0.055	-0.038	0.209	-0.158
	(0.052)	(0.072)	(0.078)	(0.157)	(0.229)	(0.224)
	0.023	*0.112*	*-0.087*	*-0.020*	*-0.143*	*0.034*
Observations	1,782	995	787	1,782	995	787
Panel B: Conditional on Employment						
Satisfaction with the job-skills match	0.065	0.097	0.020	0.205	0.294	0.060
	(0.074)	(0.105)	(0.115)	(0.236)	(0.320)	(0.339)
	-0.020	*0.073*	*-0.121*	*-0.231*	*-0.198*	*-0.235*
Wage satisfaction	0.062	0.163	-0.009	0.193	0.492	-0.026
	(0.060)	(0.094)	(0.104)	(0.188)	(0.284)	(0.291)
	0.046	*0.076*	*0.013*	*-0.077*	*-0.196*	*-0.095*
Job satisfaction	0.113	0.184	-0.035	0.359	0.558	-0.106
	(0.076)	(0.109)	(0.129)	(0.244)	(0.322)	(0.381)
	-0.032	*-0.096*	*0.042*	*-0.333*	*-0.451*	*-0.052*
Job satisfaction index	0.104	0.197	-0.027	0.327	0.597	-0.078
	(0.074)	(0.110)	(0.119)	(0.237)	(0.339)	(0.340)
	-0.028	*-0.002*	*-0.056*	*-0.308*	*-0.422*	*-0.124*
Observations	1,046	567	479	1,046	567	479

Note: The table reports the program's impact on participants' life and economic satisfaction for the full sample (Panel A) and its impact on job satisfaction for those employed (Panel B) based on responses in the follow-up survey. All variables are defined as z-scores. The last row in Panel B (Job satisfaction) shows the impact on an index of overall job satisfaction, which is computed by averaging the three measures of job satisfaction. Columns 1-3 report reduced form estimates (ITT), with control group means in italics. Columns 4-6 report LATE estimates with compliers' means (non-treated) in italics, calculated based on Angrist, Hull and Walters (2022). All models include randomization block fixed effects and our vector of control covariates. Standard errors are reported in parentheses, clustered at the randomization unit level.

Table A.13: Heterogeneity in Treatment Effect by Men's Past Earnings - ITT

Panel A: Effects of the Program 18 Months Post-Randomization				
	ITT			
	Q1	Q2	Q3	Q4
	(1)	(2)	(3)	(4)
Employed	-0.033	-0.007	0.035	0.067
	(0.053)	(0.039)	(0.038)	(0.036)
	0.602	*0.679*	*0.653*	*0.533*
Earnings	-924	-805	1,192	3,585
	(643)	(509)	(786)	(1,300)
	5,579	*7,351*	*9,689*	*13,366*
Avg earnings	-331	-732	1,108	3,216
months 13-18	(604)	(495)	(710)	(1,193)
	5,395	*7,299*	*9,615*	*13,225*
Observations	629	709	831	1,034

Panel B: Cumulative Effects of the Program 18 Months Post-Randomization				
	ITT			
	Q1	Q2	Q3	Q4
	(1)	(2)	(3)	(4)
Number of months	-0.830	0.041	0.342	1.202
employed	(0.720)	(0.590)	(0.561)	(0.545)
	10.241	*10.748*	*10.050*	*7.938*
Total earnings	-5,315	-6,170	12,274	53,159
	(9,076)	(8,075)	(11,702)	(18,149)
	83,123	*110,418*	*147,735*	*192,695*
First stage	0.228	0.173	0.321	0.331
	(0.030)	(0.025)	(0.024)	(0.026)
Observations	629	709	831	1,034

Note: The table reports the program's heterogeneous treatment effect (ITT) for men according to pre-unemployment earnings. Panel A shows outcomes in month 18 post-randomization; Panel B shows the cumulative effect during the initial eighteen months post-randomization. Pre-unemployment earnings, stratified by quartiles, are based on average earnings in months 13-24 before randomization. Control group means appear in italics. Monetary values are expressed in real 2018 ILS. Standard errors are reported in parentheses, clustered at the randomization unit level.

Table A.14: Heterogeneity in Treatment Effect by Men's Past Earnings - LATE

Panel A: Effects of the Program 18 Months Post-Randomization				
	LATE			
	Q1	Q2	Q3	Q4
	(1)	(2)	(3)	(4)
Employed	-0.143	-0.040	0.109	0.202
	(0.224)	(0.223)	(0.117)	(0.106)
	0.619	*0.636*	*0.580*	*0.465*
Earnings	-4,047	-4,662	3,719	10,839
	(2724)	(2864)	(2414)	(4,004)
	7,703	*10,645*	*8,086*	*8,661*
Avg earnings	-1,448	-4,242	3,457	9,726
months 13-18	(2560)	(2797)	(2191)	(3,691)
	6,664	*11,212*	*8,333*	*9,156*
Observations	629	709	831	1,034
Panel B: Cumulative Effects of the Program 18 Months Post-Randomization				
	LATE			
	Q1	Q2	Q3	Q4
	(1)	(2)	(3)	(4)
Number of months	-3.635	0.237	1.066	3.636
employed	(2.977)	(3.371)	(1.737)	(1.649)
	12.033	*9.232*	*8.871*	*5.628*
Total earnings	-23,283	-35,735	38,278	160,747
	(38,342)	(45,237)	(36,379)	(56,747)
	101,478	*130,119*	*126,198*	*99,307*
First stage	0.228	0.173	0.321	0.331
	(0.030)	(0.025)	(0.024)	(0.026)
Observations	629	709	831	1,034

Note: The table reports the program's heterogeneous treatment effect (LATE) for men according to pre-unemployment earnings. Panel A shows outcomes in month 18 post-randomization; Panel B shows the cumulative effect during the initial eighteen months post-randomization. Pre-unemployment earnings, stratified by quartiles, are based on average earnings in months 13-24 before randomization. Compliers' means (non-treated), in italics, are calculated based on Angrist, Hull and Walters (2022) with randomization block fixed effects and our vector of control covariates. Monetary values are expressed in real 2018 ILS. Standard errors are reported in parentheses, clustered at the randomization unit level.