Commuting to Opportunity: Reducing Employment Barriers with Short-Term Subsidies

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Abstract

This paper studies the role of monetary and non-monetary barriers in preventing residents of disadvantaged communities from accessing employment opportunities in nearby labor markets. To shed light on the nature and importance of these barriers, we analyze a large-scale randomized controlled trial in Israel in which treated jobseekers in disadvantaged localities were offered a short-term subsidy conditional on finding employment outside their locality of residence. A second experimental arm offered the same employment subsidy without the job-location requirement, allowing us to disentangle the role of non-monetary barriers from that of short-term financial incentives. The conditional subsidy generated persistent employment and earnings gains among jobseekers with weak prior labor market attachment. For the same group, the unconditional subsidy increased employment only in the short run, concentrated in local jobs. Together, these findings highlight the importance of non-monetary supply-side barriers as an obstacle to accessing broader geographic labor market opportunities among disadvantaged jobseekers.

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1. Introduction

Persistent disparities in economic conditions and living standards exist across regions, cities, and neighborhoods within cities. While a substantial literature explores the impact of policies aimed at either improving conditions in disadvantaged localities or providing incentives for residents to relocate, significantly less attention has been given to interventions that incentivize commuting from distressed areas to adjacent, high-demand labor markets. This omission is surprising given substantial dispersion in labor market conditions even within small geographic areas, suggesting that commuting could serve as a vital mechanism for reducing local unemployment.[1] While distances between distressed and prosperous areas may be short, various supply-side monetary and non-monetary barriers, such as transportation costs, information gaps, and restrictive social norms, may prevent jobseekers from pursuing better employment opportunities nearby.

In this paper, we provide evidence on the nature and importance of these barriers by analyzing a program designed to incentivize commuting among jobseekers in disadvantaged localities in Israel.[2] The program was implemented as a large-scale randomized controlled trial (RCT) in collaboration with the Israeli Employment Service (IES), covering 26 employment offices that serve 315 socioeconomically distressed and peripheral localities. Within these offices, jobseekers were randomly assigned to either a control group receiving routine services or a treatment group that was offered a Conditional Employment Subsidy: a monthly payment of 550 NIS (approximately 12% of a full-time minimum-wage salary) for individuals who secured employment outside their locality of residence. The subsidy could be collected for a total of five months during the first year of employment. By conditioning the subsidy on job location, the program specifically aimed to incentivize jobseekers to search for and accept jobs across broader geographic areas.

To separately identify the monetary versus non-monetary barriers that prevented the target population from accepting jobs outside their locality of residence, we implemented a unique

[1] For example, Gobillon et al. (2011) show large differences in unemployment rates and job opportunities for 1,300 municipalities in the Paris region and Schmutz and Sidibé (2019) document large and persistent differences in unemployment rates across metropolitan areas in France. Similarly, Manning and Petrongolo (2017) document very different labor market conditions across small areas in the U.K.

[2] In the Israeli context, a locality (yishuv) is an officially recognized populated place with defined administrative boundaries (e.g., a city, town, or village).

experimental design. In a second phase of the experiment, half of the participating offices were randomly assigned to switch to an Unconditional Subsidy program, which provided the same payment to jobseekers who secured employment regardless of the job's location. With the exception of the removed requirement regarding job location, all other program parameters remained identical to those of the main program. Therefore, one would expect the unconditional subsidy to have a larger effect on labor market outcomes in the absence of non-monetary barriers. However, in the presence of non-monetary barriers, such as information frictions and restrictive social norms, the job-location requirement embedded in the conditional subsidy may induce individuals to search for and accept jobs they would not otherwise take. This may, in turn, generate larger and more persistent effects on labor market outcomes. Comparing the effects of the *conditional* and *unconditional* subsidies allows us to disentangle the role of non-monetary barriers from that of short-term financial constraints. Overall, around 65,000 jobseekers participated in both arms of the experiment.

Our analysis combines comprehensive administrative data from the IES, the National Insurance Institute (social security), and the Israeli Tax Authority. This integrated dataset provides a rich set of baseline socio-demographic characteristics, along with monthly records of employment status, earnings, and social benefit payments, allowing us to track participants' trajectories before and after their random assignment to treatment and control groups.

Our results show that the Conditional Subsidy induced a substantial and persistent increase in employment, specifically concentrated among jobseekers with weak prior attachment to the labor market. For this "Non-attached" group, defined as individuals who worked six months or less in the three years preceding the intervention, the program increased the probability of employment by 2.1 percentage points, representing a 13% gain relative to the control group mean. Critically, these gains persisted well beyond the exhaustion of the subsidy payments. In contrast, for jobseekers with stronger prior labor market ties (the "Attached" group), the subsidy acted only as a transitory stimulus. While the subsidy accelerated return to work in the short run, the effect dissipated once it ended, suggesting the intervention primarily pulled forward employment that would have occurred regardless.

The program also generated meaningful improvements in total income and a reduced reliance on government transfers for the Non-attached group. Cumulative earnings for this group increased

by 12% over the 12-month post-intervention period, while savings from reduced social transfers per treated individual exceeded the average subsidy payments, indicating that the program was cost-effective from a fiscal perspective for this population.

To isolate the mechanism behind these persistent gains, we compare the Conditional and Unconditional Subsidy treatment arms using two complementary approaches. First, since offices were randomly assigned to program type, we directly compare treatment effects across the two interventions. Second, to account for potential differences in population characteristics across offices, we leverage the sequential nature of the experimental design and implement a triple-differences (DDD) estimator. This strategy exploits the fact that all offices administered the conditional subsidy in the first phase, while only half switched to the unconditional subsidy in the second. We can therefore difference out any pre-existing disparities between office groups by subtracting the difference in treatment effects observed in the first phase from the corresponding difference in the second.

Both approaches show that while the Unconditional Subsidy yielded a larger immediate employment effect, primarily in local jobs, this impact proved transitory and vanished once payments ceased. Conversely, the Conditional Subsidy induced a persistent shift in employment, increasing the probability of working outside the locality of residence in the long run. This transition was accompanied by a significant improvement in job quality. Collectively, these findings suggest the Conditional Subsidy helped jobseekers overcome structural, non-monetary barriers, such as information frictions, weak networks, or restrictive social norms, by inducing them to establish stable employment in higher-opportunity labor markets.

Our study makes several contributions to different strands of the literature. First, we contribute to the growing literature on spatial mismatch and the geography of unemployment. A large body of work documents persistent disparities in unemployment rates and economic disadvantage across regions (Kuhn et al., 2026; Bilal, 2023; Jung et al., 2023; Amior and Manning, 2018; Ganong and Shoag, 2017; Kline and Moretti, 2013; Moretti, 2011; Overman and Puga, 2002). The primary policy responses have been place-based policies that attract jobs to disadvantaged localities (Bartik, 2020; Slattery and Zidar, 2020; Busso et al., 2013; Gobillon et al., 2012; Neumark and Kolko, 2010) and policies that incentivize migration away from them (Chiodi et al., 2025; Chyn et al., 2025; Bergman et al., 2024; Chyn, 2018; Caliendo et al., 2017; Chetty et al., 2016; Kling

et al., 2007).[3] We show that, because labor market conditions vary substantially even within small geographic areas, incentivizing commuting through location-contingent employment subsidies offers a complementary and cost-effective tool for increasing employment in disadvantaged peripheral areas.

Second, we contribute to the literature on supply-side barriers to accessing employment opportunities that require a longer commute. Prior work has documented that workers exhibit substantial and heterogeneous disutility from commuting (Le Barbanchon et al., 2021; Marinescu and Rathelot, 2018; Manning and Petrongolo, 2017; Bonhomme and Jolivet, 2009; Van den Berg and Gorter, 1997), and that this heterogeneity contributes to observed wage and employment gaps across demographic groups (Caldwell and Danieli, 2021; Le Barbanchon et al., 2021). Beyond the direct disutility of commuting, jobseekers in disadvantaged areas may face additional barriers to accessing jobs in high-opportunity areas due to the absence of relevant referral networks (Hellerstein et al., 2014, 2011; Bayer et al., 2008; Calvo-Armengol and Jackson, 2004; Topa, 2001) and social norms regarding employment (Wilson, 1987; Eugster et al., 2017). Furthermore, the literature has documented the role of information frictions and biased beliefs in the job search process (Jäger et al., 2024; Abebe et al., 2025; Altmann et al., 2018; Spinnewijn, 2015; Babcock et al., 2012), which are likely to be amplified in the presence of spatial mismatch precisely because of segregation and the lack of social networks (Banerjee and Sequeira, 2023). Our paper provides large-scale experimental evidence showing that such non-monetary barriers are quantitatively important. The fact that the persistent employment gains are driven entirely by the job-location condition – and not by the financial incentive alone – directly identifies non-monetary barriers as an important obstacle to access to labor market opportunities over a broader geographic area among the most disadvantaged jobseekers.

Third, we contribute to the literature on policies aimed at expanding the geographic scope of job search. Recent studies have explored how providing information about jobs in broader geographic areas (Belot et al., 2019) or subsidizing job search transport costs (Abebe et al., 2021; Franklin, 2018; Phillips, 2014; Banerjee and Sequeira, 2023; Caliendo et al., 2026) affects search behavior and labor market outcomes. These interventions address frictions in the search process itself, but their employment effects are modest and short-lived at best.In contrast, we show that a

[3] See Glaeser and Gottlieb (2008) for an earlier discussion and Neumark and Simpson (2015) for a survey.

financial incentive conditioning on the *location* of the job obtained, rather than on search effort or transportation costs, produces durable employment effects that persist well after the financial incentive ends.[4]

Finally, we contribute to the literature on active labor market programs (ALMPs) and employment subsidies.[5] The evidence in this literature documents mainly short-run effects: employment subsidies tend to increase employment and shorten unemployment spells, but these effects largely dissipate once the financial incentive is removed (see Card et al. 2018; Kluve 2010; Crépon and Van Den Berg 2016; Le Barbanchon et al. 2024 for recent reviews of the ALMP literature, and Card and Hyslop 2005; Van der Klaauw and Van Ours 2013; Bobonis et al. 2022 for recent studies on employment subsidies).[6] The persistent employment gains we document for the Non-attached are therefore unusual and speak to a broader debate about the conditions under which ALMPs can generate lasting change. We show that persistence arises not from the financial incentive per se, but from the behavioral change it induces: by incentivizing individuals to take jobs outside their locality of residence, the job-location condition helps them establish employment relationships in better labor markets that remain productive even after the subsidy ends. This mechanism, using a temporary incentive to shift jobseekers onto a new and more favorable employment trajectory, is consistent with recent theoretical and empirical work on the role of employer quality and match quality in generating durable employment effects (see, e.g., Nekoei and Weber, 2017; Lachowska et al., 2020; Farooq et al., 2020). Our results also highlight the importance of targeting: the same subsidy that generates persistent gains for the Non-attached produces only transitory effects for the Attached, suggesting that heterogeneity in barriers should inform the design and targeting of such programs.

The remainder of the paper is organized as follows. Section 2 describes the institutional

[4]Our findings are consistent with Bryan et al. (2014), which shows in the context of seasonal migration, what matters is not information about wages and job availability, which had no effect, but rather inducing households to experiment with migration, which generates persistent re-migration after the incentive is removed.

[5]Employment subsidies take several forms: direct payments to employers to incentivize the hiring of certain types of workers; vouchers provided to jobseekers that can be handed to a prospective employer; and lump-sum bonuses paid directly to jobseekers upon finding employment, as in the program discussed in this paper. These instruments differ in the barriers they aim to overcome. Common to all is that the subsidy is paid for a defined period and is usually conditional on employment being secured within a defined eligibility window.

[6]There is also a large literature on reemployment bonuses analyzing the effects of the demonstration programs implemented in the United States in the 1980s (see, e.g., Woodbury and Spiegelman 1987; Meyer 1995; O'Leary et al. 2005). These studies generally find that cash bonuses reduce the duration of unemployment with negligible or null effects on earnings in the short-run.

background, the target population, and the program. Section 3 presents the experimental design, data, and estimation strategy. Section 4 reports the effects of the conditional subsidy on employment, earnings, and government transfers. Section 5 examines the mechanisms by comparing the conditional and unconditional subsidy programs. Section 6 concludes.

2. Institutional Background, Target Population and Program Description

2.1. Institutional Background

All non-employed individuals in Israel are required to report weekly to their local IES office in order to be eligible for unemployment or welfare benefits.[7] The IES operates about 70 offices distributed across the country; residents of smaller or peripheral localities are assigned to the nearest office, which may require commuting to a neighboring city.

The routine treatment that jobseekers receive at these offices is minimal. They record their attendance using self-service biometric fingerprint scanners, and once every three weeks (or when relevant) they meet with an employment coordinator who may offer job referrals from a pool of vacancies collected by the IES. This pool is limited in scope, covering a relatively small and selected subset of available positions in the labor market, and the vast majority of jobseekers find employment through other channels. Therefore, finding a job depends mostly on jobseekers' own initiative and effort. Failure to report to the employment office or rejection of an IES-referred job offer results in the suspension of benefits for a specified period.

2.2. Geographical Disparities and Target Population

Similar to other countries, Israel is characterized by wide dispersion in unemployment across labor markets, even within small geographic areas. This is illustrated in Figure 1, which shows locality-level unemployment rates in 2015. As an example, consider the area around Haifa, Israel's third-largest city. Haifa had an unemployment rate of 4.2%, while in nearby towns less than an hour's

[7] Individuals over age 50 are only required to report once a month. To be completely exempted from reporting, younger individuals must prove that they are incapable of working.

drive away, unemployment rates were as high as 20%.

Motivated by these disparities, in 2015 the IES introduced an employment subsidy program targeting jobseekers aged 18 to 55, without disabilities, who reside in disadvantaged peripheral localities. The localities chosen to participate in the program were those ranked low on the socioeconomic and peripherality indices calculated by the Israeli Central Bureau of Statistics (CBS).[8] As we discuss in more detail below, the program's main purpose was to encourage jobseekers in these localities to broaden their geographic search by seeking jobs in tighter labor markets that require slightly longer commutes, thereby increasing short- and long-term employment probabilities and reducing chronic dependence on government transfers. To this end, the subsidy was paid only if the job was located outside the jobseeker's locality of residence.

Overall, 315 localities were selected to participate in the program, served by 26 employment offices throughout Israel. Their geographic distribution is shown in Appendix Figure A1, where participating localities are marked in blue and the four main metropolitan cities of Israel —Tel Aviv, Jerusalem, Be'er Sheva, and Haifa—are highlighted in pink. Table 1 presents descriptive statistics for participating localities (Column (1)) and compares them to all other localities in the country (Column (2)), revealing substantial disparities between the two groups.[9] By construction, both the socioeconomic and peripherality indices are markedly lower in participating localities. Consistent with this, unemployment rates are more than twice as high (10% compared to 4% on average), and average income is approximately 27% lower. We also compare access to cars, given that roughly 60% of workers in Israel commute by car (Bleich, 2018). Eligible localities have about 16% fewer cars per adult. Consistent with the program's selection criteria, participating localities are situated further from Israel's four major metropolitan centers – 29.8 km (18.5 mi) versus 16.5 km (10.3 mi) – and from primary employment hubs – 20.0 km (12.4 mi) versus 9.4 km (5.8 mi) – than all other localities.[10] However, given the small size of the country, these distances

[8]The CBS assigns each locality a socioeconomic index based on factors such as income, housing, education, and employment, where lower values indicate lower socioeconomic status. Localities are then ranked and grouped into 10 internally similar clusters based on this index (see Characterization and Classification of Geographical Units by the Socio-Economic Level of the Population 2015, CBS press release, in Hebrew). The peripherality index assigns low values to areas that are remote from employment opportunities, as well as from centers of economic activity, health services, and educational institutions more generally. The index also places substantial weight on distance from the Tel Aviv district border, reflecting the importance of this region as Israel's primary economic center (see Peripherality Index of Localities and Local Authorities 2015, CBS press release, in Hebrew).

[9]Reported values are population-weighted averages across localities.

[10]Primary employment hubs are defined as localities with over 25,000 employees according to the 2008 population

are relatively small.

Finally, the table highlights the ethnic composition of the localities and the extent of residential segregation between Arab and Jewish populations. Participating localities have disproportionately larger shares of Arab residents (59%, compared to 11% in all other localities) and a greater representation of exclusively Arab communities (30% vs. 4%).[11]

Taken together, participating localities combine high unemployment with relative geographic proximity to tighter labor markets, a combination that motivated the program's focus on broadening the geographic scope of job search.

2.3. Barriers to Employment Outside the Locality of Residence

To evaluate the extent to which specific barriers prevent jobseekers from broadening the geographic scope of their search and taking jobs outside their locality of residence, we conducted a baseline survey within the program's target population. The survey was administered before treatment and control assignment and was based on a representative sample of the target population.

Panel A of Table 2 reports survey evidence on non-employed individuals' availability for work in general and for work outside their locality of residence. While a majority of respondents (72%) report being available for work in general, only about 46% indicate that they would accept a job outside their locality of residence. We then asked jobseekers who reported being available for work only within their locality of residence why they were unwilling to work outside it. Panel B of Table 2 shows that more than half of respondents cite transportation availability and cost, as well as family obligations, as barriers to working outside their locality of residence. While the first two factors can be interpreted as monetary barriers to working outside the locality of residence, the remaining two factors are non-monetary in nature. About 23% cite social norms as a barrier to working outside the locality of residence, and 15% of respondents report that they do not believe it is economically worthwhile to do so.[12] This latter response might be consistent

census, which provides the most granular information on place-of-work patterns proximate to the program's inception. These hubs include Tel Aviv, Jerusalem, Haifa, Be'er Sheva, Netanya, Nazareth, Hadera-Caesarea, Ashkelon, and Eilat.

[11]The Israeli population is highly segregated residentially: approximately 85% of Arab citizens live in Arab localities, where they constitute nearly the entire population; about 10% reside in mixed Arab-Jewish localities (mostly larger towns or cities).

[12]Survey respondents were permitted to select multiple responses. The "Social Norms" indicator is a binary variable

with an information barrier, whereby jobseekers are overly pessimistic about job opportunities and the wage premia of jobs outside their locality of residence. This interpretation is supported by evidence from Porat Hirsh (2026), who shows that Israeli workers from peripheral areas who commute longer distances earn 20-30% more than similar workers employed near their locality of residence, suggesting substantial returns to commuting that jobseekers may underappreciate.

Finally, the survey included an additional module on reservation wages, designed primarily to quantify the disutility of commuting by measuring the elasticity of reservation wages with respect to commuting time. Specifically, we collected data on respondents' desired number of workdays per week, desired daily hours, hourly reservation wage, and maximum commute time given these work hours and reservation wage. We then asked what reservation wage they would require if their daily round-trip commute increased by 30 or 60 minutes. The results, summarized in Appendix Table A1, show that for an additional 30 minutes of daily commuting, the median respondent required a 2.9% higher reservation wage, equivalent to approximately 180 NIS per month. For an additional 60 minutes, the required increase was 12.5%, equivalent to approximately 780 NIS per month.[13] This nonlinear increase in required compensation for longer commutes (above 30 minutes), such as those likely needed to reach jobs in other localities, suggests that the marginal cost of commuting time rises substantially beyond a certain threshold.

In sum, the survey evidence suggests that jobseekers face a mix of monetary and non-monetary barriers that deter them from working outside their locality of residence. At the same time, it suggests that financial incentives may affect willingness to commute. The Conditional Subsidy is designed to address these barriers by offsetting the disutility of commuting and encouraging jobseekers to consider jobs in higher-opportunity areas.

2.4. The Employment Subsidy Program

As discussed above, the program's goal was to encourage jobseekers in disadvantaged peripheral localities to seek jobs in tighter local labor markets that require slightly longer commutes. To achieve this objective, the IES introduced a short-term monthly employment subsidy, hereafter the

equal to one if a respondent reported that their family or spouse does not support them working outside the locality, or that such employment is considered socially inappropriate and zero otherwise.

[13] Monthly amounts are calculated by multiplying the hourly reservation wage by the desired number of monthly hours, using 4.33 weeks per month.

Conditional Subsidy, offered to individuals from eligible localities who obtained a job outside their locality of residence within a three-month eligibility window. The subsidy could be collected for a total of five months during the first year of employment. It was designed to address the opportunity cost and any additional disutility involved in commuting, complementing the direct commuting costs that Israeli law already requires employers to cover.[14] Accordingly, its amount was set to cover the time cost of about one commute hour per day at the minimum wage rate, a monthly total of 550 NIS (141 dollars in 2015).[15] This represented a non-negligible 12% supplement to a full-time minimum-wage monthly salary and is well aligned with the compensation respondents reported in the baseline survey for an additional 30 to 60 minutes of daily commuting documented in the previous section.

Jobseekers assigned to the program were notified of their eligibility by their employment coordinator and received a brochure explaining the program details. Because many of the eligible jobseekers belong to the Arabic-speaking minority in Israel, the brochure was issued in both Hebrew and Arabic. Appendix Figure A2 presents the brochures in their original languages along with an English translation (Appendix Figure A3).

Notably, the claiming process for this subsidy was fairly simple. As the brochure explains, upon finding a job, jobseekers had to inform the employment office, provide the identity and contact details of their employer, and download a designated mobile app. The app was used to report their arrival to work for at least eleven days each month, with GPS verifying the workplace location.[16] At the end of each month, those who met the required conditions received a text message inviting them to collect the subsidy in cash at the nearest post office.

In addition to the Conditional Subsidy program, an *Unconditional Subsidy* was implemented as a separate treatment arm targeting the same eligible population, but without the requirement that the job be located outside the locality of residence. The rationale for this secondary program was to disentangle the impact of the job-location condition from the effect of the financial incentive, thereby isolating the specific mechanisms driving the program effects of the Conditional Subsidy

[14]Under Israeli labor law, employers are required to reimburse employees for their daily commuting expenses, up to a maximum daily cap. This mandatory reimbursement is typically based on the cost of the cheapest available public transportation route between the employee's residence and the workplace.

[15]The hourly minimum wage in 2015 was 25 NIS. The amount was updated over time according to changes in the minimum wage.

[16]Those who lacked a smartphone were instructed to call from their employer's location to a designated IES phone number to report their daily arrival.

program. With the exception of the removed requirement regarding job location, all other program parameters, including the subsidy amount, duration, and reporting protocols, remained identical to those of the main program.

A comparison of the impacts of the two programs offers a unique opportunity to shed light on the barriers faced by the unemployed in disadvantaged areas. Larger impacts of the conditional subsidy relative to the unconditional subsidy would suggest that the job-location requirement is key to overcoming non-monetary barriers, such as information frictions or restrictive social norms regarding employment outside the locality of residence. Importantly, such a pattern would be inconsistent with explanations based on liquidity constraints, which are alleviated by both types of subsidies, or on other factors expected to produce similar effects under both programs, such as a transition from informal to formal employment.

3. Experimental Design and Empirical Strategy

3.1. Randomization Procedure

Both programs were implemented as randomized controlled trials (RCTs). The first phase began with the implementation of the conditional subsidy, which was introduced gradually, beginning with a pilot in two employment offices in October 2015 and expanding to all 26 offices by February 2017. In the second phase, beginning in April 2018, half of these offices were randomly chosen to switch to the unconditional subsidy program.[17] Offices were selected using a pairwise randomization procedure (Bruhn and McKenzie, 2009), where pairs were matched based on the characteristics of the specific localities that participated in the program within each office.[18] The two programs continued to operate concurrently until November 2019.

[17] Treatment arms were randomized across offices rather than at the individual level in order to minimize the administrative burden of communicating two distinct programs within the same office and to reduce the risk of providing incorrect program information to jobseekers, given the high similarity between the two programs.

[18] These characteristics included geographical area, average local unemployment rates, average population size, and detailed characteristics of the target population (schooling, employment history, ethnicity, and immigration). Additionally, offices were matched according to the share of jobseekers who continued to report to the IES five months post-allocation in the experiment's first phase. While the ideal matching criterion would have been the first-phase treatment effect on employment, these administrative data were unavailable at the time of assignment. We therefore utilized the treatment effect on the probability of continued reporting to the IES as a proxy for the program impact among the first-phase cohorts across offices.

Appendix Table A2 presents the descriptive statistics for the localities assigned to the conditional and unconditional subsidies, as well as for the remaining localities in the country. The differences between localities assigned to the conditional and unconditional subsidy programs are small, with minor variations often shifting in opposite directions across indicators. For example, while localities assigned to the conditional subsidy are slightly closer to major cities (27.26 km versus 31.45 km), they are slightly farther from the main employment hubs (20.82 km versus 18.79 km) than those assigned to switch to the unconditional subsidy. These small differences stand in sharp contrast to the substantial disparities observed between localities in the two treatment arms and the rest of the country.

In both programs, jobseekers were randomized on a monthly basis into treatment and control groups from a pool of "active" jobseekers (defined as individuals who had reported to the employment office at least once in the preceding month) who satisfied the program's eligibility criteria. Each month, only a subset of this pool was allocated to either group; the remaining individuals remained in the sampling frame for potential assignment in subsequent months, provided they maintained their active status. Individuals in the treatment group were notified of their eligibility and received an informational brochure during their next scheduled visit to the employment office. Those in the control group continued their routine treatment at the IES offices and did not receive any information about the subsidy program. Importantly, individuals assigned to the control group were not eligible to participate in the program at any point in the future. Furthermore, an individual's treatment or control status remained in effect even if he or she moved to another locality, stopped reporting to the employment office, or reregistered with the IES after a certain period.

Randomization was carried out centrally, and employment offices were informed only of the individuals assigned to treatment.[19] It was stratified by employment office and by three population subgroups: (i) unemployment benefits claimants (*Unemployed*), (ii) short-term (up to two months) welfare claimants (*Welfare flow*), and (iii) long-term (more than two months) welfare claimants (*Welfare stock*). This classification was intended to proxy jobseekers' degree of labor market attachment, a dimension shown to be critical for the effectiveness of active labor market programs.

[19]This information was sometimes distributed to local offices with a delay of several days. Our analysis uses the actual distribution date as the relevant timing of the intervention, assigned equally to treatment and control individuals randomized within the same month and office.

In the absence of detailed employment records at the time of random assignment, the classification relied on benefit eligibility and IES attendance records. Thus, the randomization unit was defined by the interaction of the allocation month, employment office, and subgroup. Overall, close to 65,000 jobseekers participated in the two phases of the experiment.

3.2. Data

To analyze the effects of the intervention, we merge administrative data from several sources. The IES operational database provides a rich set of socio-demographic characteristics, including locality of residence, gender, age, marital status, number of children, ethnicity (Arab/Jewish), education, and self-reported health limitations. These records also include detailed, high-frequency data on jobseekers' activity at the employment office and in particular, on program participation and subsidy payments.

Social security records provide monthly information on benefit receipt, including welfare payments, unemployment benefits, and other transfers such as disability benefits. Monthly employment and earnings data come from tax records, which also report anonymized employer identifiers, employer location, and firm size. However, employer location is based on the employer's registered address for tax purposes and may not fully correspond to the worker's actual job location, which is of interest in our context due to the nature of the intervention.[20]

Our main outcome measures are based on this detailed information for the years 2012 to 2021, allowing us to study dynamic treatment effects, month by month, and to control for employment, earnings and benefit collection for up to three years pre-allocation. Moreover, these data were used to re-classify the participants into two categories by their actual labor market experience, rather than proxy measures of attachment based on IES activity records. We classify jobseekers as "Non-attached" if they worked a total of six months or less during the three years prior to allocation, and as "Attached" otherwise. In most of our analysis, we split the samples by these two distinct categories of jobseekers.

[20]For example, if the worker is employed by a supermarket chain, we observe the address of their main offices, and are not able to distinguish those who work in a store located inside or outside their locality of residence.

3.3. Estimation

Building on random assignment, we estimate treatment effects by comparing post-intervention outcomes for individuals in treatment and control groups within randomization unit.[21] To increase precision and account for potential differences derived from randomization in a finite sample, we also control for jobseekers' demographic characteristics and for their employment and benefit history.[22]

The equation estimated is therefore:

$$y_{ijtg} = \eta_{jtg} + \beta T_i + X_i'\tau + \varepsilon_{ijtg} \tag{1}$$

where y_{ijtg} is the outcome for individual i from employment office j allocated at date t and belonging to claimant type group g. T_i is the treatment indicator; η_{jtg} is a fixed effect for the randomization cell (employment office interacted with randomization date and claimant type); X_i is a vector of individual characteristics measured before randomization including age, gender, marital status, number of children, immigration status, education level, indicators for self-reported health limitation, single mother, Ultra-Orthodox Jew, Arab, and vectors of that summarize welfare, earnings, and employment-history during the three years preceding randomization. We cluster standard errors by randomization unit, allowing for correlation between the error terms of those who belong to the same claimant type-group and employment office and were randomized at the same time.[23]

The coefficient of interest is β, which captures the average effect of being offered a subsidy on a broad set of labor market outcomes, including monthly employment and earnings. We also estimate treatment effects on social benefits collection and total income. We follow these outcomes up to 24 months post-intervention to establish whether the effects persist beyond subsidy exhaustion,

[21] This interpretation relies on the SUTVA assumption that the program had no effect on individuals assigned to the control group. Potential externalities could operate in either direction. On one hand, displacement effects could generate negative spillovers onto the control group; however, this seems unlikely given that the conditional subsidy induced treated jobseekers to seek employment primarily in larger metropolitan areas outside their locality of residence. On the other hand, the control group could benefit positively from reduced competition for local jobs, or from information spillovers and shifts in social norms regarding employment outside the locality of residence. To the extent that such positive spillovers exist, our estimated treatment effects would be biased downward.

[22] All results are robust to excluding these controls.

[23] We adopt a conservative approach by clustering our standard errors since treatment assignment probabilities varied within clusters (see Abadie et al., 2023).

indicating a permanent change in the barriers faced by jobseekers rather than a transitory response.

To disentangle the impact of the job-location condition from the effect of the financial incentive in the conditional subsidy program, we estimate and compare treatment effects across the offices assigned to the conditional and unconditional subsidy arms during the second phase of the experiment. This design allows us to isolate the specific impact of the locational requirement from the effect of the monetary incentive. In addition to evaluating impacts on employment and earnings, we examine several dimensions of job and firm quality, including firm size, job stability, and a proxy for employer location, to identify potential shifts in the types of job matches induced by each program. Given the paired-randomization research design, the difference in treatment effects between the two arms identifies the causal impact of the job-location condition. As discussed above, by comparing the impacts of the two programs, we can disentangle the role of non-monetary barriers from that of liquidity constraints.

While the paired randomization procedure is designed to ensure balance across offices, the relatively small number of offices may still result in small differences in population characteristics across offices assigned to the two treatment arms. We leverage the unique sequential nature of the experimental design to further account for such differences and implement a triple-differences (DDD) approach. Our identification strategy exploits the fact that all offices implemented the same conditional subsidy program in the first phase of the experiment, while only half of them switched to the unconditional subsidy in the second phase. This allows us to difference out any pre-existing disparities between offices by subtracting the difference in treatment effects between the two groups of offices in the first phase from the corresponding difference in the second phase.

Formally, we estimate the following DDD specification:

$$y_{ijtg} = \mu_{jtg} + \gamma_1 T_i + \gamma_2 (T_i \times Post_t) + \gamma_3 (T_i \times S_j) + \gamma_4 (T_i \times Post_t \times S_j) + X_{it}' \phi + \nu_{ijtg} \qquad (2)$$

where T_i indicates that individual i was assigned to the treatment group in one of the subsidy programs, $Post_t$ indicates that the allocation took place during the second phase of the experiment, and S_j equals 1 if office j belongs to the set of offices that stayed with the conditional subsidy in the second phase. X_{it} is a vector of individual characteristics as discussed above interacted with $Post_t$ and S_j. As before, we include a vector of fixed effects for the randomization unit that also absorb the phase and switching indicators ($Post_t$ and S_j) and their interaction. Thus, γ_4 identifies

the differential impact of the conditional subsidy (relative to the unconditional subsidy) under the assumption that in the absence of the switch in subsidy type, treatment effects on jobseekers in offices that switched would follow the same trend as those in offices that did not switch.

3.4. Samples, Descriptive Statistics and Balance Tests

The analysis samples include individuals allocated to treatment and control groups for whom outcomes are observed over at least a 12-month horizon following randomization and prior to the onset of the COVID-19 pandemic (up to February 2020).

Main Sample The main sample consists of all jobseekers allocated to either treatment or control in the conditional subsidy program through February 2019. This includes individuals randomized in the first phase of the experiment as well as those registered at employment offices that did not switch to the unconditional subsidy in the second phase. These individuals can be followed for at least 12 months post-allocation. The sample comprises 53,262 jobseekers, of whom 23,665 were assigned to the treatment group, providing sufficient statistical power to detect even small effects and to examine heterogeneous treatment effects across subgroups.

Table 3 reports summary statistics and balance tests for this sample (Columns (1)-(2)), as well as separately for the "Non-attached" (Columns (3)-(4)) and "Attached" (Columns (5)-(6)) sub-groups of jobseekers, where "Attached" denotes individuals who worked at least six months during the 36 months before randomization, and "Non-attached" those who worked less than six months during this period.[24]

In the full sample, average characteristics are very similar across treatment and control groups, with no statistically significant differences. Within each subgroup, differences are small and largely insignificant, consistent with random assignment. At the bottom of the table, we report p-values from regressions testing whether all covariates jointly predict treatment status, conditional on randomization cell fixed effects. These p-values are reassuringly high, implying that jobseekers' treatment status cannot be predicted based on their observed characteristics, as expected under random assignment.

[24]This classification refines the randomization stratification by population subgroups by assigning all jobseekers to labor market attachment status based on their actual employment history.

The table also shows that participants come from relatively disadvantaged backgrounds, particularly those classified as Non-attached. In the full sample, employment rates are at or below 60% for each of the three years prior to allocation, and average annual earnings are persistently low, at around 30,000 NIS (approximately $7,500 in 2015).[25] Educational attainment is similarly low relative to the general population, with about 26% having less than 12 years of schooling. In addition, 65.6% of jobseekers in the sample are Arab compared to roughly 20% in the population. This pattern is consistent with the broader tendency for minority populations to be concentrated in disadvantaged areas, both in Israel and elsewhere, as also reflected in the locality-level statistics shown in Table 1 above. Lastly, 36% of participants report having a health limitation, even though individuals with officially recorded disabilities were not eligible for the program and are therefore not included in our sample.

These aggregate averages mask substantial heterogeneity by attachment status. By construction, the two groups differ markedly in their employment histories, with employment rates among the Non-attached as low as 6.7% in the three-year pre-allocation period. This leads to a dramatic earnings difference over this period: total earnings among the Attached subgroup are roughly two orders of magnitude higher than those of the Non-attached. Consistent with this, the Non-attached receive substantially higher benefits, averaging around 14,000 NIS annually (approximately $3,500), compared to an average of about 4,500 NIS among the Attached (approximately $1,125). The larger amounts for the Non-attached reflect the fact that their transfers consist primarily of welfare payments, whereas the Attached predominantly receive unemployment benefits, which are limited to six months. The Non-attached is characterized by a significantly higher representation of both Arab and female jobseekers. Specifically, the share of Arab participants among the Non-attached is considerably more pronounced than in the Attached group (84% vs. 53%), and women comprise 68.5% of the former compared to approximately 50% of the latter. On average, Non-attached individuals are seven years older and have nearly twice as many children, averaging approximately four, compared to just over two among the Attached. Taken together, these patterns suggest that the Non-attached may face greater barriers and constraints in terms of labor market opportunities and commuting. Lastly, Ultra-Orthodox Jews are generally underrepresented in the sample (3.8%, compared to roughly 11% in the general population), and even more so in the

[25] This corresponds to about 25% of average annual earnings in Israel in 2015, the year the program was introduced.

Non-attached subgroup (2.4%).[26]

24-Months Horizon Sample We also focus on a subsample of participants from the main sample allocated up to February 2018, for whom we can track for an extended 24-month horizon. Examining this subsample allows us to assess whether the effects of the subsidy persist in the long run. Appendix Table A3 presents the mean characteristics and balance tests for this subsample, both overall and by attachment subgroups. Overall, this subsample closely resembles the main sample and remains well balanced across treatment and controls. While this sample has the advantage of a longer horizon, it does not allow us to study the impact of the unconditional subsidy program because of the onset of the COVID-19 pandemic in early 2020. As such, we use this sample as a secondary sample, corroborating the results obtained for the conditional subsidy using the main sample.

Offices in the two treatment arms Table A4 reports descriptive statistics and covariate balance tests separately for the offices assigned to the conditional and unconditional subsidy programs. Across both samples, differences between treatment and control groups are generally small and statistically insignificant, as expected under random assignment. Furthermore, joint tests of whether all covariates predict treatment status conditional on the randomization cell yield large p-values, supporting the ignorability of treatment assignment.

A comparison of jobseekers in offices assigned to the Conditional subsidy (Column (1)) and Unconditional subsidy (Column (3)), reveals broad similarities in employment histories, earnings, baseline benefit levels, and most demographic characteristics, consistent with the matched pair randomization design used for office assignment. However, given the relatively small number of offices, the matching procedure did not yield perfect balance across all dimensions. Specifically, offices assigned to the Conditional subsidy exhibit a higher proportion of Arab participants (67.9% vs. 61.8%) and Ultra-Orthodox individuals (4.9% vs. 2.5%). These disparities are reflected in

[26]Ultra-Orthodox Jews (Haredim) in Israel are a religious community characterized by strict adherence to Jewish law, distinctive lifestyles, and relatively limited integration into secular education and labor markets, particularly among men. Their relatively small representation in our sample is not surprising. Despite their generally low socioeconomic status and high reliance on welfare, Ultra-Orthodox communities are largely concentrated in urban areas, often within or near large cities (The Ultra-Orthodox Community Yearbook 2025, the Israel Democracy Institute, in Hebrew).

correlated characteristics, such as higher fertility rates and a lower share of immigrants within the Conditional subsidy group.

To ensure that the estimated differences in treatment effects across programs are not confounded by these compositional disparities, we implement a triple-differences (DDD) framework (as explained in Section 3.3). This approach differences out baseline (Phase 1) treatment effects within each set of offices, and thus relies on a parallel trends assumption in treatment effects rather than on cross-sectional balance in levels.

The unconditional subsidy sample Finally, Appendix Table A5 reports the descriptive statistics and balance tests for the experimental arm assigned to the unconditional subsidy. As detailed in Section 3.3, we leverage this sample to estimate the impact of the unconditional subsidy, comparing the effects both to the conditional program and to the treatment effects observed in the same offices during the first phase of the experiment (the triple-differences (DDD) specification). The unconditional subsidy was implemented only during the second phase of the experiment (beginning April 2018), which provides a 12-month follow-up window for jobseekers assigned through February 2019. This subsample is considerably smaller than the main sample, also because a large share of the eligible population, particularly those who had been reporting to the IES for extended periods, had already been assigned to treatment or control during the first phase.

For Non-attached jobseekers, in Columns (3)-(4), the treatment and control groups are well balanced, consistent with the patterns observed for the conditional subsidy. For the sample of Attached individuals, in Columns (5)-(6), we observe small discrepancies in employment in the year prior to the program, benefits received three years pre-allocation, and marital status. While these differences are minor, they do lead to a statistically significant joint F-test for the predictive power of observed characteristics on treatment assignment. However, their economic significance is negligible and does not suggest any systematic bias. Nonetheless, as discussed in Section 3.3 above, we account for these minor imbalances in observables by including the full set of covariates in our regression models. Moreover, our primary focus in comparing the two programs is on the Non-attached subgroup, as explained below.

4. The Effect of the Conditional Subsidy

4.1. Subsidy Payments

We first examine subsidy-collection rates for treated jobseekers, both overall and by attachment category in Figure 2.[27] Panel (a) shows the cumulative share of treated individuals who received at least one monthly subsidy payment, while panel (b) shows the share of jobseekers receiving a payment in each specific month post-allocation. Per program protocols, jobseekers were required to secure employment within three months of being informed of their eligibility. While the three-month mark represents the primary take-up window, the cumulative rate continues to rise through month six at a slower rate. This pattern is consistent with minor administrative lags in the dissemination of eligibility information to participants. Notably, the share claiming the subsidy stabilizes by the sixth month, suggesting that nearly all participants who successfully transitioned into eligible employment had initiated their claims by this point.

Importantly, consistent with the program design, the flow of payments reveals that the financial incentive was largely exhausted around the tenth month, with negligible payments occurring thereafter. Consequently, any treatment effects estimated after month 10 can be interpreted as sustained long-term impacts that persist after the active financial incentive has ceased.

The descriptive data on subsidy payments also reveal substantial disparities in collection rates when disaggregated by baseline labor market attachment. While approximately 16% of jobseekers in the Attached group claimed the subsidy, the rate among the Non-attached was considerably lower, at roughly 4%. However, it is important to emphasize that subsidy collection rates and differences between groups do not directly translate into the program's causal effect. Instead, they may simply reflect initial tendencies in employability. Furthermore, the lower collection rates observed among the Non-attached may also reflect standard barriers to benefit take-up among more disadvantaged groups of those who qualified for the subsidy, which are well documented in the literature on social assistance programs.

[27] We focus in this figure on the 24-month horizon sample, for which outcomes can be tracked over a longer period. The pattern observed up to 12 months using the main sample is virtually identical.

4.2. Labor Market Outcomes

Focusing on our main sample, Table 4 presents the effects of the conditional subsidy on employment, earnings, social benefits (including welfare, disability, and unemployment insurance), and total income at 3, 6, and 12 months post-allocation. Total income is defined as the aggregate of labor earnings, social transfers, and realized subsidy payments. The estimates at the three- and six-month horizons capture the short-run behavioral response during the period of subsidy payments. Conversely, the twelve-month estimates evaluate the persistence of treatment effects following the exhaustion of subsidy eligibility. For each outcome, the control group mean is reported below the point estimate and its corresponding standard error.

On average, the program increased employment at all three horizons (Columns (1)-(3)). In the short term, employment rises by 1.3 and 2.2 percentage points after 3 and 6 months, respectively, with both estimates statistically significant and corresponding to increases of approximately 5% relative to the control mean. The effect remains highly significant after 12 months, when employment is still 1.2 percentage points higher in the treatment group, equivalent to a 2.5% increase relative to the control mean. Earnings (including zeros) show a similar pattern, with positive effects throughout and significant increases of about 5% at 6 months and 2% at 12 months. The similar magnitudes of the earnings and employment effects suggest that the former are driven largely by the extensive margin of employment. The third row of Table 4 shows that the program also increased job stability, with "Stable job" defined as continuous employment with the same employer for at least four months (by month 6) or six months (by month 12).[28] Beyond serving as a proxy for job quality, this outcome is informative about whether the treatment translated into a more sustained employment relationship rather than transitory employment.[29]

The persistence of employment effects through month 12 in the absence of subsidy payments, which, as discussed above, were exhausted after about 10 months, suggests that the intervention induced a sustainable transition into the labor market that remains profitable even without the direct monetary incentive. This finding points toward the program's success in alleviating barriers or frictions that previously limited employment. Consistent with this, we would expect jobseekers

[28] This outcome is measured for all individuals regardless of their employment status at months 6 or 12.

[29] Results for stable employment in month three are not reported, as this outcome is undefined for short-term employment.

facing greater labor market barriers to exhibit larger treatment effects. We address this by examining the labor market effects of the subsidy separately for the Non-attached (columns 4-6) and the Attached (columns 7-9) populations. The barriers facing the Non-attached population are immediately evident in the control group means: at the six-month horizon, the employment rate for the Non-attached control group is only 12.6%, compared to 54.8% for the Attached group (Columns 5 and 8). While both subgroups exhibit statistically significant increases in employment and earnings in the short run, the relative magnitudes and dynamic patterns vary substantially. At the six-month mark, the employment effects are similar in absolute terms, 2.5 and 2.1 percentage points, respectively. However, when scaled by the control group mean, the impact on the Non-attached represents a nearly 20% increase in employment, whereas the increase for the Attached group is only 3.8%. For the Non-attached jobseekers, the subsidy clearly induced a substantial increase in employment relative to their very low counterfactual. These differences become even more pronounced in the longer run, as a positive employment effect persists only for the Non-attached. For this subgroup, the 12-month effect remains at 2.1 percentage points, very similar to the short-term impact. Given that the control mean for this group rises to only 16.3% by month 12, this represents a 13% increase in the probability of employment. In contrast, the treatment effect for the Attached group dissipates by the one-year mark, falling to a statistically insignificant 0.8 percentage points against a control group employment rate of 66%. The effects on earnings qualitatively mirror the employment effects: for the Non-attached, the program induced a persistent 13% to 15% increase in earnings through month 12, while the Attached group experienced a transitory 4.7% earnings spike in the medium term that dissipated by the end of the year. Results for the subsample followed for 24 months (Appendix Table A6) corroborate our primary findings, showing that the employment gains for the non-attached persist in the longer run even as effects for the attached group fully dissipate.

The estimated effects on employment and earnings point to distinct economic roles for the subsidy across the two populations. For the Attached group, the subsidy primarily accelerated their transition back to employment without altering long-run employment. For the Non-attached, the persistence through month 12 indicates a significant and permanent increase in employment rates. This interpretation is further corroborated by the "Stable job" estimates. For the Non-attached, the program increased the chances of working for the same employer for at least 6 months by 2.1

percentage points (18%). Meanwhile, the effect for the Attached group was significantly smaller both in absolute terms and relative to the control mean (1 percentage point or 2%). These results suggest that the subsidy facilitates more permanent labor market transitions for individuals who likely faced the highest search frictions and structural barriers to entry.

To further examine these dynamics, Figure 3 plots the month-by-month estimates for the effects on employment (panel a) and earnings (panel b), extending from 24 months before to 12 months after treatment assignment. As expected under random assignment, pre-treatment differences between the treated and control groups are centered around zero and statistically insignificant for both groups. Following the intervention, both effects on employment and earnings emerge almost immediately and follow a similar upward trajectory during the first six-months for both the Non-attached and the Attached. However, the treatment effects for the two groups diverge sharply after the six-month mark. For the Non-attached, the treatment induces a persistent increase; employment gains remain stable at approximately 2 percentage points through the end of the 12-month window, with labor earnings following a similarly sustained path. In contrast, the effects for the Attached group prove transitory. Both employment and earnings effects peak around month six, coinciding with the period of maximal subsidy collection, and subsequently decline, becoming statistically indistinguishable from zero by month nine.

Finally, Figure 4 decomposes the long-run employment effects at month 12 into underlying labor market transitions, separately for the Non-attached and Attached groups. Panel (a) separates the employment effect into the probability of staying employed among those who found work in the short-term, up to six months post treatment (*stayed employed*) and the probability of gaining employment in month 12 among those not employed in the short-term (*gained employment*). Panel (b) provides the corresponding decomposition for nonemployment, distinguishing between jobseekers who remained out of work throughout the entire observation window (*stayed non-employed*) and those who were employed in the short term but subsequently exited the labor market (*exited employment*).

The figure shows that the long-run employment effects differ not only in size but also in composition across the two groups. Among the Non-attached, the positive employment effect is primarily driven by a higher probability of remaining employed throughout the year, supplemented by a smaller marginally significant increase in job entry during the latter half of the period. This is

mirrored in panel (b) by a substantial decline in persistent non-employment, as treated individuals in this group were significantly less likely to remain out of work for the entire 12 month duration. This reduction in persistent non-employment is slightly offset by an increase in the probability of exiting employment by month 12 among those who had successfully found work in the first six months.

In contrast to the persistent gains observed for the Non-attached, the results for the Attached group reveal a distinct pattern of offsetting transitions that explain their negligible long-term employment effect. As shown in Panel (a), while the treatment initially helped some Attached individuals remain in employment, this gain is almost entirely canceled out by a significant reduction in late job entry. This suggests that, for this group, the subsidy primarily pulled forward employment that would have occurred anyway in the absence of the intervention, leading to fewer job entries in the latter half of the year. The divergence between the two groups is further highlighted in Panel (b). While the Non-attached experienced a large and durable reduction in persistent non-employment, the corresponding reduction for the Attached is much smaller and is nearly offset by a significant increase in exits from the labor market. These patterns confirm that for the Non-attached, the subsidy facilitated sustained employment gains, whereas for the Attached it primarily shifted the timing of employment without fundamentally altering their long-term trajectory.

4.3. Government Transfers and Total Income

Consistent with improved labor market outcomes, the program also reduced reliance on government transfers, although these effects are less precisely estimated. As shown in the fourth row of Table 4, for the Non-attached, we observe a sustained, albeit modest, reduction in the likelihood of receiving social benefits, which for this subgroup consist primarily of welfare payments, that persists through the one-year horizon.[30] For the Attached group, the short-run reduction in benefits reflects an accelerated exit from the unemployment insurance (UI) system as treated individuals returned to work sooner than those in the control group. By month 12, however, this effect for the Attached group converges to zero, as UI eligibility is typically exhausted by this

[30]Employed individuals can receive welfare payments in the form of an income supplement if their earnings are below a certain threshold.

point for both the treatment and control groups.

Subsidy payments for both groups are substantial in the short and medium term but fall to near zero by month 12, confirming that the financial incentive was no longer in effect by the end of the first year.[31]This finding demonstrates that the differential labor market long-run effects between the two groups are not driven by differential timing of take-up of the subsidy itself. Instead, the divergence between the two groups suggests that while the temporary subsidy accelerated return to employment for the Attached, it facilitated a more fundamental and lasting transition into the labor market for the Non-attached.

Finally, total income, including the subsidy payments, increased meaningfully at all three horizons, with the largest gains relative to the control group after 6 months. Consistent with the divergence between the Non-attached and the Attached in labor market outcomes, the gains in total income remain significant for the Non-attached through the 12-month horizon, while the corresponding effect for the Attached group becomes statistically indistinguishable from zero by the end of the year.

4.4. Cumulative Effects and Longer-run Outcomes

Table 5 reports the cumulative treatment effects aggregated over the 12 month post randomization period, providing a summary measure of the intervention total impact on labor market outcomes and government transfers. The results are consistent with those reported in Table 4. The estimated effect on total months of employment is positive for all groups, but it is notably larger for the Non-attached subgroup in both absolute and relative terms. Likewise, we see a significant increase of 12% in total earnings for the Non-attached and a modest increase of 2% for the Attached. The impact on job stability, measured by the number of months with the same employer, is also larger for the Non-attached. For the Non-attached, average savings from social transfers per treated individual exceed the average subsidy payments, indicating that the program pays for itself for this group. For the Attached, average subsidy payments per treated participant outweigh average savings in social benefits, resulting in a net fiscal cost for this group.[32] Taken together, our

[31]The control mean of subsidy payments is not identically zero because a negligible number of individuals in the control group were erroneously assigned the subsidy.

[32]We expect government revenue from income tax payments to be negligible. For the Non-attached, earnings typically fall below the minimum income tax threshold. Regarding the Attached group, the relatively small difference

findings are consistent with the idea that a temporary financial incentive is especially effective for individuals with weaker prior attachment, helping them overcome initial barriers to entering and remaining in employment. The persistent employment effect for the Non-attached contrasts with most of the previous literature on employment subsidies, which documents primarily short-term effects that fade once the financial incentive ends. For the Attached group, however, the incentive appears to have functioned as a transitory stimulus that accelerated reentry without fundamentally altering their long-term employment path. A potential explanation for this divergence, particularly the sustained gains among the Non-attached, is the unique job location condition imposed by the program. We further explore this hypothesis in Section 5.

5. Mechanisms

Having established that the persistent employment effects are concentrated among the Non-attached, this section focuses on this group and explores the mechanisms underlying the longer-run gains. We compare the conditional subsidy to an unconditional subsidy to isolate the role of the job-location condition and assess whether the persistent effects are driven by the financial incentive itself or by the requirement that recipients work outside their locality of residence. In addition to revisiting the effects on employment and earnings, we examine whether the two subsidy programs increased employment *outside the locality of residence*, thereby providing direct evidence on commuting and job-location choices and led to higher-paying jobs. We also examine firm size as an additional job characteristic to assess whether the programs shifted participants into different types of jobs.

5.1. Comparing Employment and Earnings Effects of the Conditional and Unconditional Subsidy Programs

As described in Section 3, in the second phase of the experiment, half of the participating offices were randomized to switch to the unconditional subsidy, which was identical to the conditional one

in the cumulative number of months worked implies that the intervention did not generate a substantial increase in income tax revenue. The intervention was designed to be low-cost and scalable. The program's marginal costs were limited to the distribution of informational flyers during routine employment office visits, while fixed costs were restricted to the initial development of a digital application used to monitor daily work attendance.

except that it was paid regardless of job location. Removing the job-location condition expands the set of incentivized jobs to include those within the locality of residence and should therefore increase subsidy claiming rates. Appendix Figure A4 presents these rates for the Non-attached in the second phase by subsidy type, confirming that slightly more than 6% claimed at least one monthly payment in the unconditional subsidy offices, compared to below 5% in the conditional subsidy offices. Moreover, the figure reveals that the timing of claiming differs across the two subsidy types: unconditional subsidies are claimed earlier, which is consistent with the longer search time required to find employment outside the locality of residence.

In the absence of barriers related to information about remote labor markets, networks, or social norms, we would expect the unconditional subsidy to generate larger effects both in the short run and in the long run. Panel A of Table 6 reports the estimated effects of the two programs on employment and earnings, at the three-, six-, and twelve-months horizons for the Non-attached. The unconditional subsidy yields a significantly larger employment response in the short run. At the third month post randomization, employment increases by 3.9 percentage points compared to a negligible and statistically insignificant effect under the conditional subsidy. A similar contrast appears in earnings, where the unconditional subsidy yields a positive point estimate, whereas the effect of the conditional subsidy is negative and insignificant. By month six, the point estimates for both programs are positive and similar in magnitude, but very noisy. These magnitudes closely resemble those estimated in the full sample of the conditional subsidy experiment reported in Table 4, where the estimates are highly significant. This suggests that the lack of significance in the second phase likely stems from the smaller sample size, which includes approximately 2,000 observations for each program compared to 19,500 in the main sample.

In the long run, the pattern reverses completely. By month 12, the employment effect of the unconditional subsidy dissipates and is no longer significant, while the effect of the conditional subsidy reaches 3.9 percentage points (15% increase). The unconditional subsidy also has no lasting effect on earnings, whereas the conditional subsidy raises earnings by 17% relative to the control mean.

The employment response to the conditional subsidy appears later in this subsample compared to our primary results. Moreover, as discussed in Section 3.4, the balancing table reveals some preexisting differences in background characteristics across the offices randomized into the two

program arms, which are also reflected in differences in outcome means between the control groups of the two programs. To account for these factors, we employ a triple differences estimation (eq. 2). This approach compares the treatment control difference in the second phase against the corresponding difference in the first phase for the same offices. By netting out time invariant differences between the randomized offices, the estimator ensures that our results isolate the impact of the job location requirement rather than preexisting office level characteristics. Furthermore, any shifts in labor market conditions between the first and second phase of the experiment are accounted for by time fixed effects. This framework also allows us to formally test for the differential impact between the conditional and unconditional subsidies and evaluate whether the treatment effects in the two groups of offices followed parallel trends prior to their randomization into the separate treatment arms.

Figure 5 presents the event study estimates and associated confidence intervals from the triple differences specification, providing a granular view of the dynamic response to the job location requirement of the conditional subsidy. The coefficients capture the difference in treatment effects between offices that maintained the conditional subsidy and those that transitioned to the unconditional subsidy, relative to the baseline treatment effects estimated within those same offices during the first phase. For both employment and earnings, the coefficients in the pre-treatment period are close to zero and statistically insignificant, given that treatment and control groups were balanced in the pre-period. In the post-treatment period, the estimates of the effects of the conditional subsidy relative to the unconditional subsidy reveal a distinct reversal over time. While the differential impact is initially negative, consistent with the unconditional subsidy generating a larger immediate employment effect, the difference between the conditional and unconditional subsidy effects becomes positive and increases in subsequent periods, becoming statistically significant by month 12. The point estimates from the DDD specification are very similar to the difference between the program effects estimated in Table 6 (Column (6) minus Column (3)), confirming that the comparison between the two treatment arms already yields causal estimates of the difference between the two programs, as expected under the random assignment of offices. This is further corroborated by the negligible and statistically insignificant estimate of γ_3 in equation 2, which captures the difference in treatment effects between the two sets of offices during the first phase, when both were implementing the conditional subsidy (employment: est. =

0.006, s.e. = 0.013; earnings: est. = 13.75, s.e. = 55.01).

Overall, the differences in the effects between the two programs confirm that the job location condition, rather than the financial incentive alone, is the key driver of the persistent employment gains observed for the Non-attached group.

Decomposition of the Employment Effects Figure 6 further clarifies the mechanisms behind the contrasting results of the conditional and unconditional subsidies by decomposing the month 12 employment effect into underlying labor market transitions. Under the conditional subsidy, the positive long-run employment effect is driven by both a higher probability of remaining employed among those who worked in the short run (up to month 6) and a positive contribution from new job entry. This is mirrored in panel (b) by a decline in non-employment, driven primarily by a lower probability of remaining unemployed throughout the entire observation window.

In contrast, the unconditional subsidy shows no evidence of a comparable improvement in long-run employment. Its month-12 employment effect is slightly negative, reflecting small declines in both continued employment among those who worked in the short run and job entry. This is mirrored in Panel (b), which shows a higher probability of exiting employment by month 12 among those who were employed in the short run. Although these decomposition estimates are statistically noisy due to the smaller samples, the overall pattern is consistent with our main findings: the conditional subsidy facilitates sustained employment gains, whereas the unconditional incentive produces only a transitory increase in work that dissipates once the subsidy ends.

5.2. Comparing the Effects on Job Location and Quality

Panel B of Table 6 reports the effects of the two programs on employment and earnings outside the locality of residence, where these variables are defined as zero for individuals who are nonemployed or who work within their own locality. This comparison speaks directly to the mechanism, as the defining distinction between the two programs is whether employment outside the locality of residence is required for subsidy eligibility. We present these results with some caution, however, as the recording of employer location is measured with error.

The unconditional subsidy has essentially no effect on employment outside the locality at any horizon. By contrast, the conditional subsidy raises the probability of working outside the locality

by 3.5 percentage points by month 12. This effect is statistically significant, amounts to about 19% relative to the control mean, and is very similar to the overall treatment effect on employment reported in Panel A. The corresponding estimates for earnings outside the locality display the same pattern: under the unconditional subsidy, they remain close to zero throughout the period, whereas under the conditional subsidy, they become positive, similar to the overall earnings effect, and statistically significant in the long run. These results provide direct evidence that the job-location condition changes commuting and job-location choices in a way that persists beyond the subsidy period.

Figure 7 illustrates these dynamics month by month. Panel A shows that under the conditional subsidy, the employment effect emerges gradually and becomes increasingly positive over time. This pattern is driven almost entirely by jobs *outside* the locality of residence, whereas the effect on jobs inside the locality remains close to zero throughout. Panel B shows a very different trajectory for the unconditional subsidy, where short-term increases in employment are mostly driven by employment inside the locality of residence, and none of the effects persist in the long-term.

The last row of Panel B of Table 6 examines employment at a large firm (defined as having more than 80 employees, approximately the median firm size) as an additional job quality measure. The two programs differ sharply in their impact on this outcome. The unconditional subsidy increases the probability of working for a large firm by less than half of the overall employment effect after three months, with no lasting improvements. The conditional subsidy, by contrast, significantly increases this outcome in the long run. After 12 months, it raises the probability of working in a large firm by 3.7 percentage points (a 26% increase), suggesting that the persistent employment effect is almost entirely driven by employment with large employers. Notably, the proportional increase in large-firm employment (26%) exceeds the proportional increase in overall employment (15%), indicating that the conditional subsidy participants access higher-quality jobs. Collectively, these findings suggest that the job location requirement not only increases employment but also facilitates access to better jobs.

Overall, the comparison between the two subsidy programs points to a clear conclusion. A temporary financial incentive by itself can generate short-run employment gains and accelerate return to work, but these effects fade quickly and do not alter longer-run employment trajectories. Persistent gains arise only when the subsidy is tied to job location, and are accompanied by a

durable increase in employment outside the locality of residence and by improvements in job characteristics. This pattern is difficult to reconcile with a pure liquidity-based explanation. Instead, it suggests that the job-location condition helps relax barriers related to commuting and access to more distant jobs, such as limited information, weak networks, restrictive social norms, or employer-side frictions.

6. Conclusion

Individuals in disadvantaged and peripheral communities often face significant barriers, monetary and non-monetary alike, that prevent them from accessing employment opportunities in nearby labor markets, contributing to persistent local unemployment. This paper examines whether a location-contingent employment subsidy can help such individuals overcome these barriers. We exploit a large-scale RCT implemented by the Israeli Employment Service across socioeconomically distressed peripheral localities, in which jobseekers were randomly assigned to a program that offered a short-term subsidy conditioned on finding work outside their locality of residence.

Our main finding is that the Conditional Subsidy, which required recipients to find work outside their locality of residence, generated substantial and lasting employment gains, particularly for jobseekers with weak prior labor market attachment. These gains were accompanied by meaningful improvements in earnings, job stability, and reductions in reliance on government transfers. For jobseekers with stronger prior attachment, the subsidy functioned primarily as a temporary stimulus, accelerating reemployment without altering long-run trajectories.

To separately identify the monetary versus non-monetary barriers that prevented the target population from accepting jobs outside their locality of residence, we designed and implemented an Unconditional Subsidy program identical to the Conditional Subsidy in all parameters except the job-location requirement. The comparison between the two subsidies reveals that the Unconditional Subsidy generated short-run employment gains, mostly in local jobs, but these effects faded quickly. The Conditional Subsidy, by contrast, induced jobseekers to take up jobs outside their home communities and to stay in them, generating a persistent shift in employment location and a significant improvement in job characteristics. This pattern suggests that the

job-location condition helped jobseekers overcome barriers related to commuting and access to broader geographic labor market opportunities, such as limited information, weak networks, restrictive social norms, or employer-side frictions. More broadly, our findings indicate that location-contingent employment subsidies can provide a low-cost and scalable complement to traditional place-based policies for reducing unemployment in disadvantaged peripheral areas.

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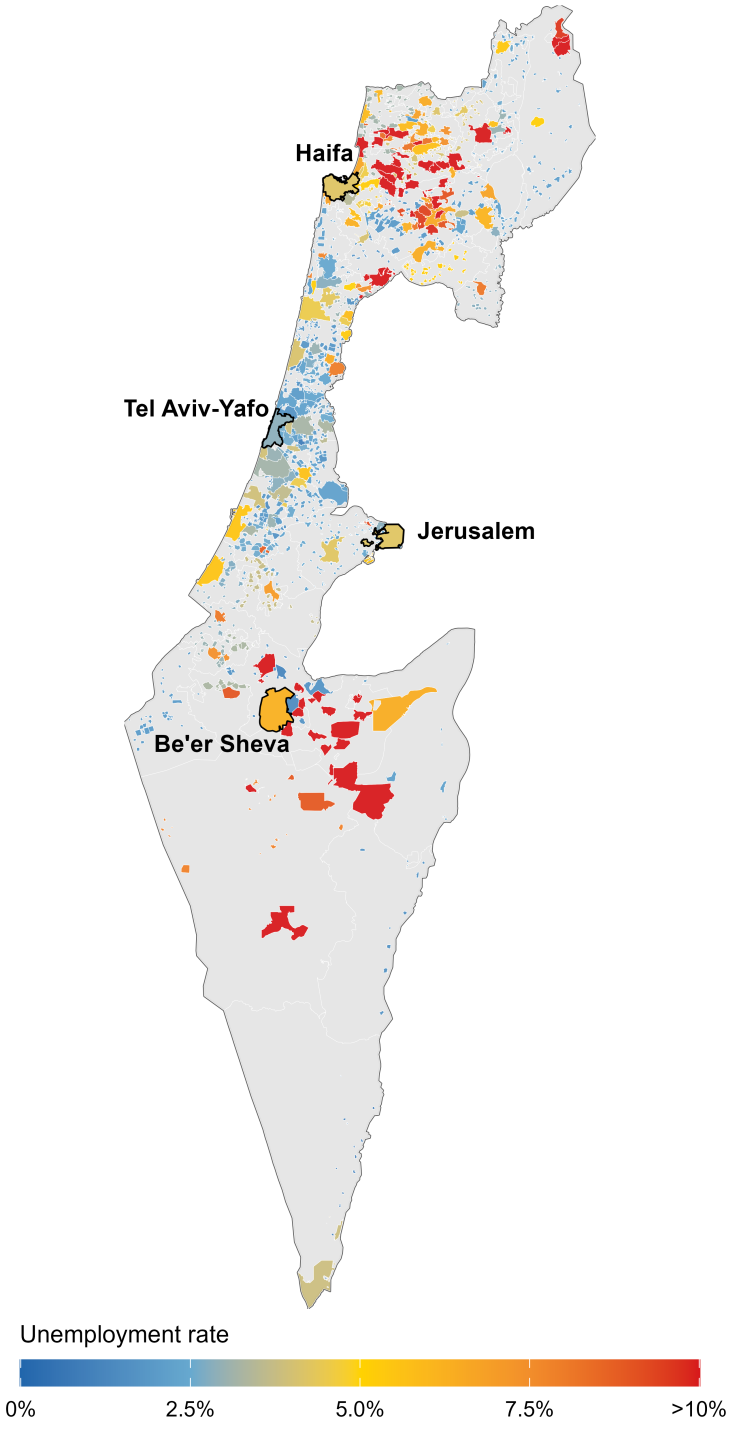
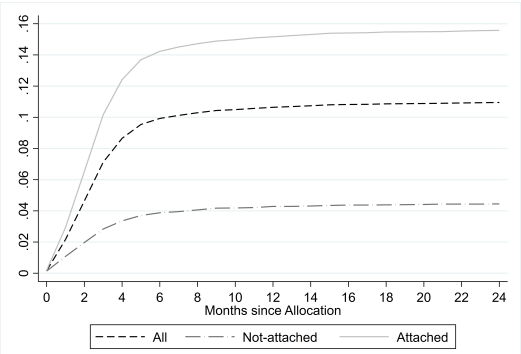
Figure 1: Unemployment Rates by Locality



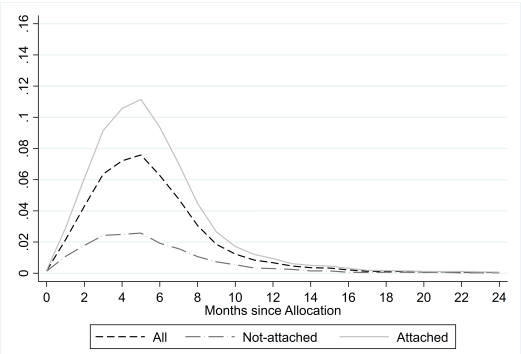
Notes: The figure presents locality-level unemployment rates from 2015, based on administrative records from the Israeli Employment Service. Unemployment rates for small localities are reported at the regional council level.

Figure 2: Subsidy Claiming Rates

(a) Share receiving at least 1 month of subsidy



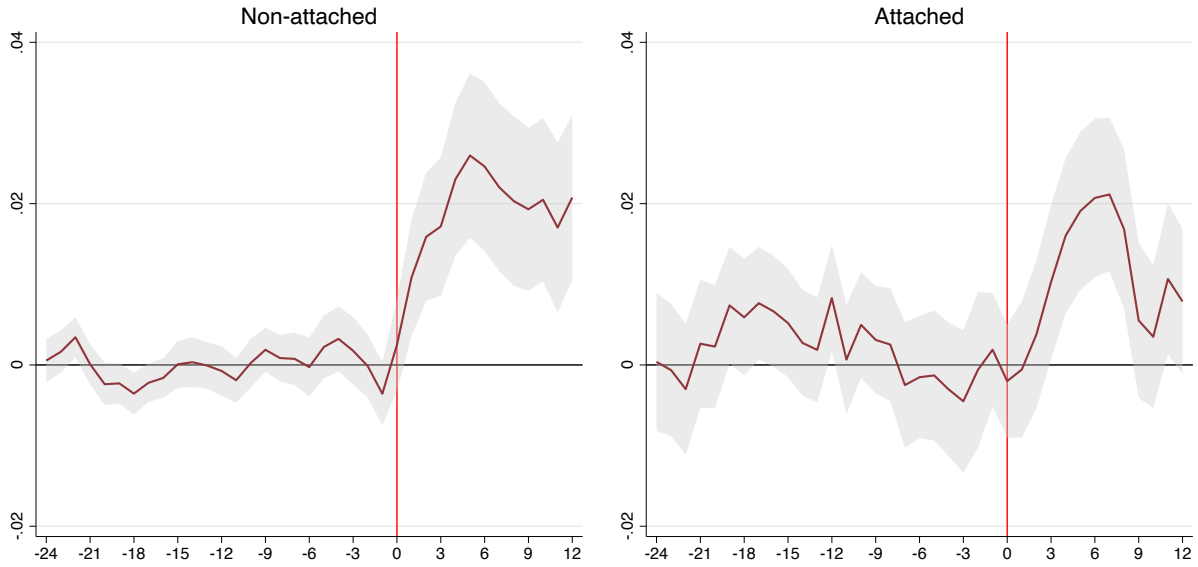
(b) Share receiving subsidy each month



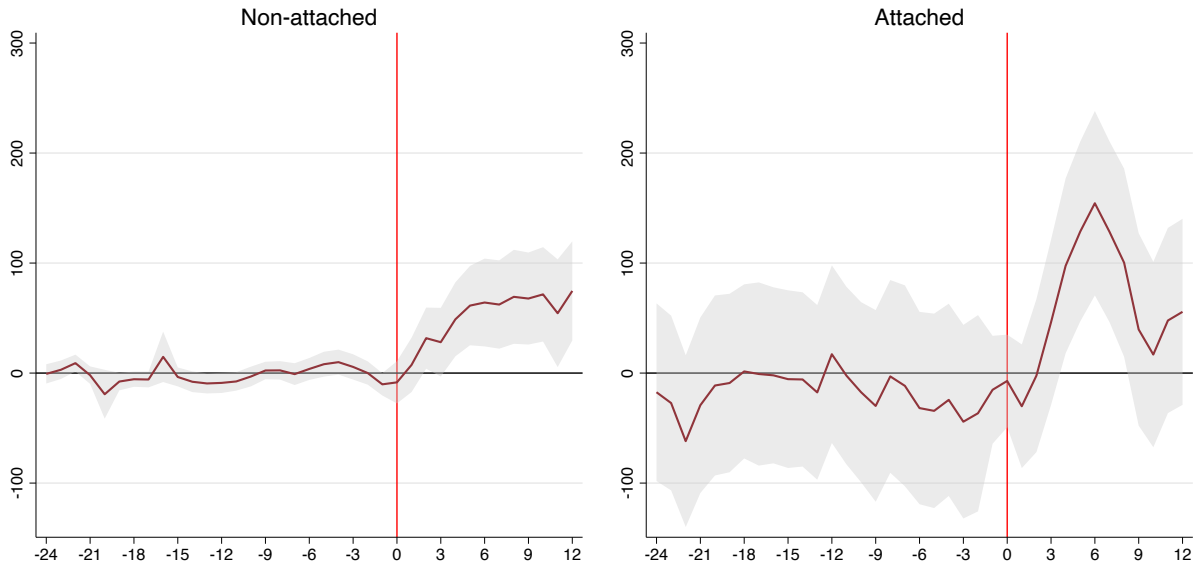
Notes: The figures plot claiming rates of the conditional subsidy for the 24-month horizon sample. Panel (a) shows the cumulative share of treated individuals who received at least one monthly payment, while panel (b) shows the share receiving a payment in each specific month post-allocation.

Figure 3: Dynamic Effect on Employment and Earnings

(a) Employment



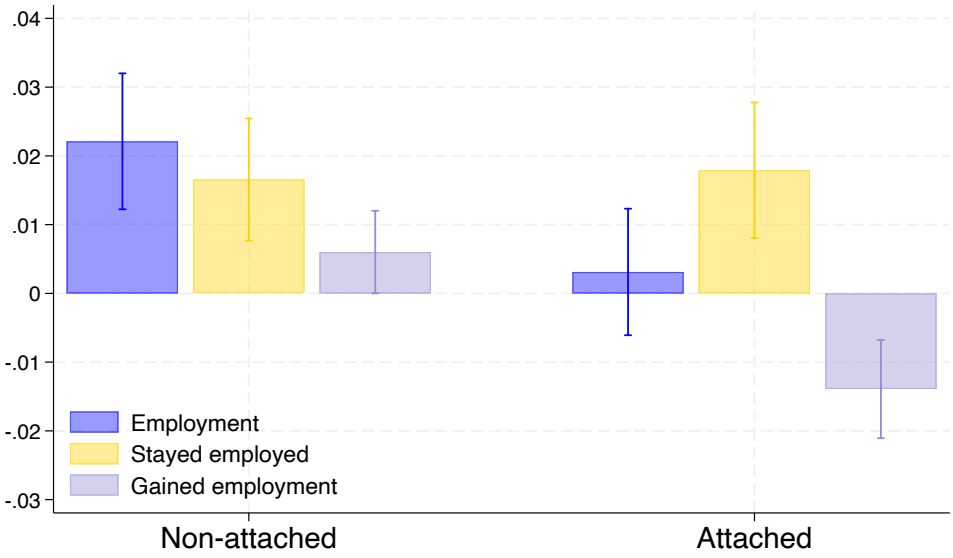
(b) Earnings



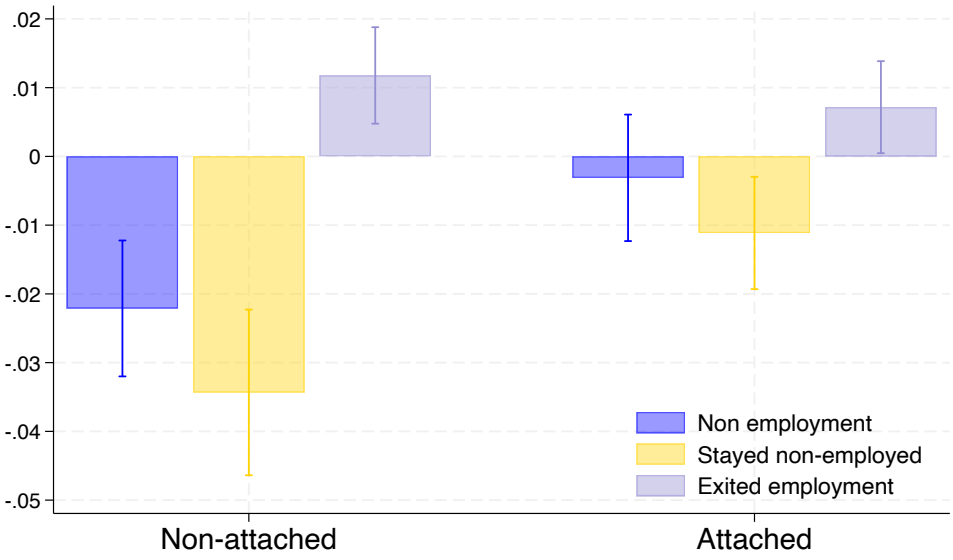
Notes: The figure reports treatment effects on employment (Panel A) and earnings (Panel B) over time, with 95% confidence intervals, controlling for age, gender, marital status, number of children, immigration status, education level, indicators for self-reported health limitation, single mother, Ultra-Orthodox Jew, Arab, and randomization block fixed effects. The left panels correspond to the Non-attached group, defined as individuals who worked six months or fewer in the three years preceding the intervention; the right panels correspond to the Attached group, defined as individuals who worked more than six months in the three years preceding the intervention. Month zero corresponds to the month of random assignment. Standard errors are clustered at the randomization unit level.

Figure 4: Decomposing the Employment Effect

(a) Decomposition of Employment at Month 12



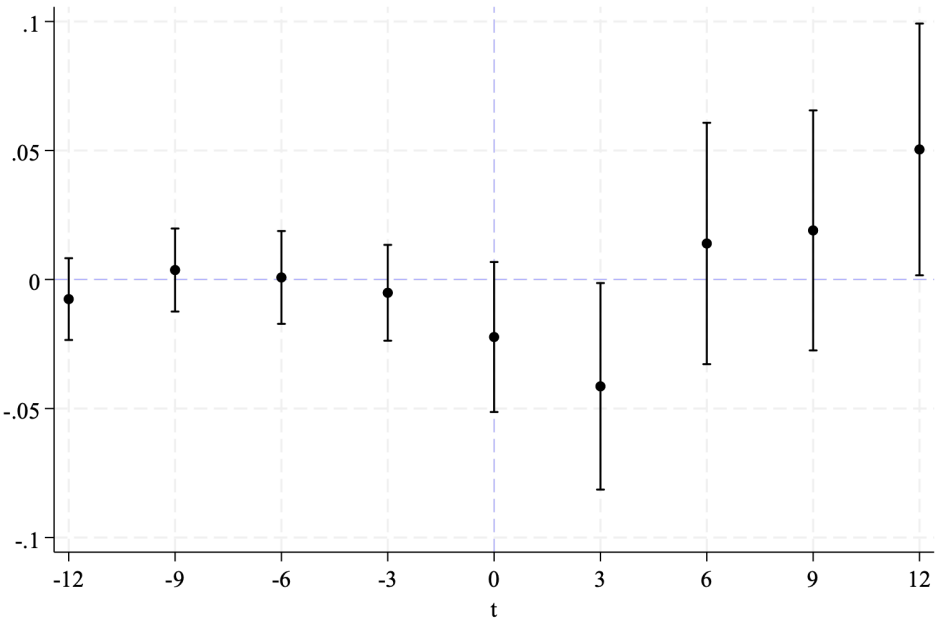
(b) Decomposition of Non Employment at Month 12



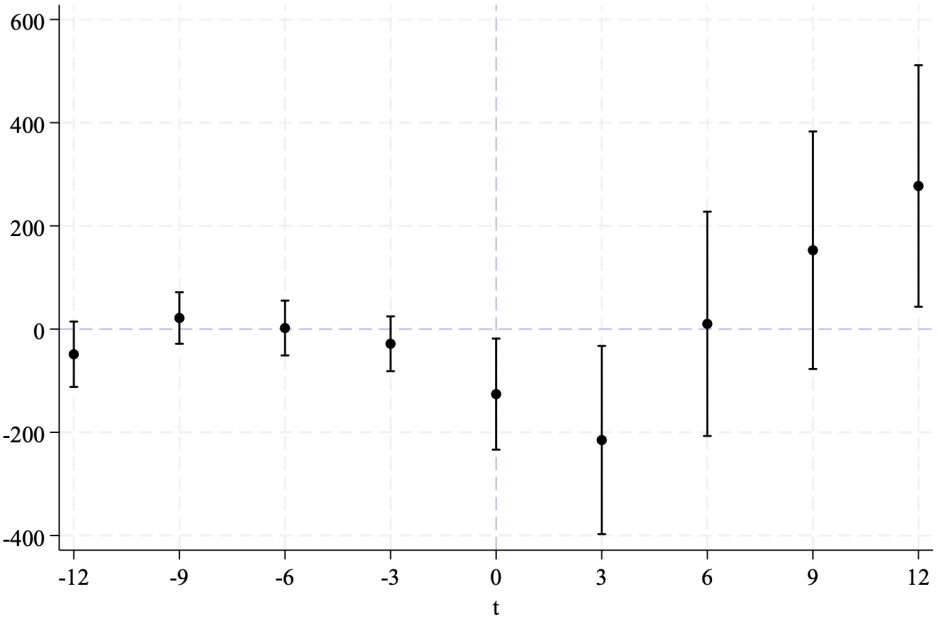
Notes: The figures decompose the employment effect at month 12 into underlying labor market transitions, separately for the Non-attached and Attached groups. The three bars in Panel (a) report treatment effects and 95% confidence intervals for three outcomes: employment at month 12; employed within the first six months *and* employed at month 12 (*Stayed employed*); not employed within the first six months *and* employed at month 12 (*Gained employment*). Panel (b) provides the corresponding decomposition for nonemployment. The three bars report treatment effects and 95% confidence intervals for: nonemployment at month 12; not employed within the first six months *and* not employed at month 12 (*Stayed non-employed*); employed within the first six months *and* not employed at month 12 (*Exited employment*). All regressions include the full set of controls specified in Table 3.

Figure 5: Triple Difference Estimates of Conditional versus Unconditional Subsidies

(a) Employment



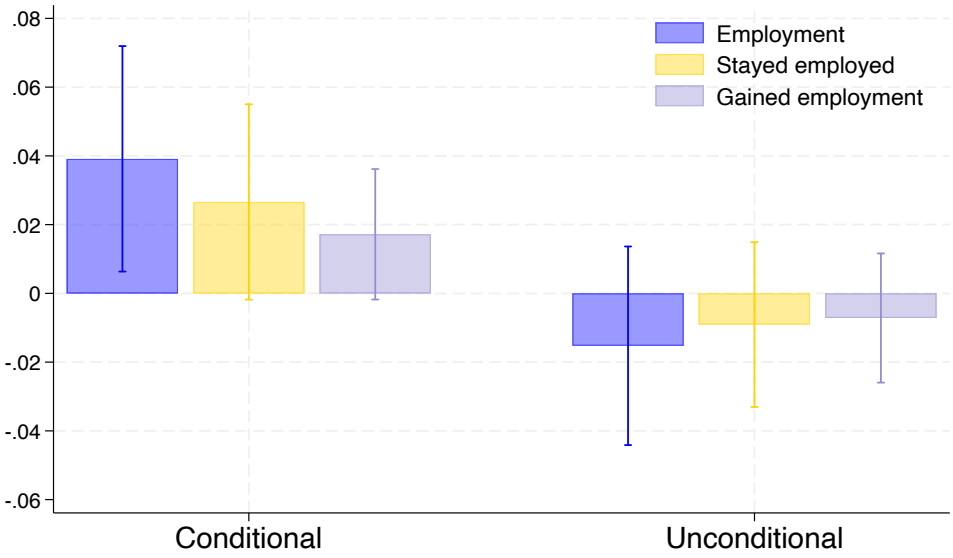
(b) Earnings



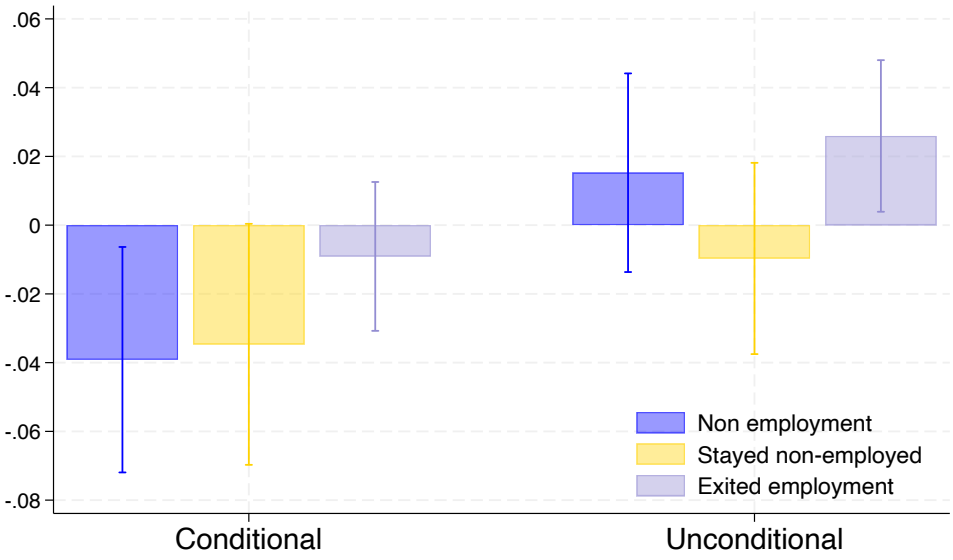
Notes: The figures plot estimates and 95% confidence intervals from a triple-differences specification for employment (Panel A) and earnings (Panel B) for the Non-attached group. The coefficients capture the differential treatment effect between offices that maintained the conditional subsidy and those that transitioned to the unconditional subsidy, relative to the baseline treatment effects estimated within those same offices during the first phase. All models also control for individual characteristics interacted with the treatment arm and the *Post* indicator, and include randomization-unit fixed effects. Month zero corresponds to the month of random assignment. Standard errors are clustered at the randomization unit level.

Figure 6: Decomposing the Employment Effect: Conditional vs. Unconditional Subsidies

(a) Decomposition of Employment at Month 12

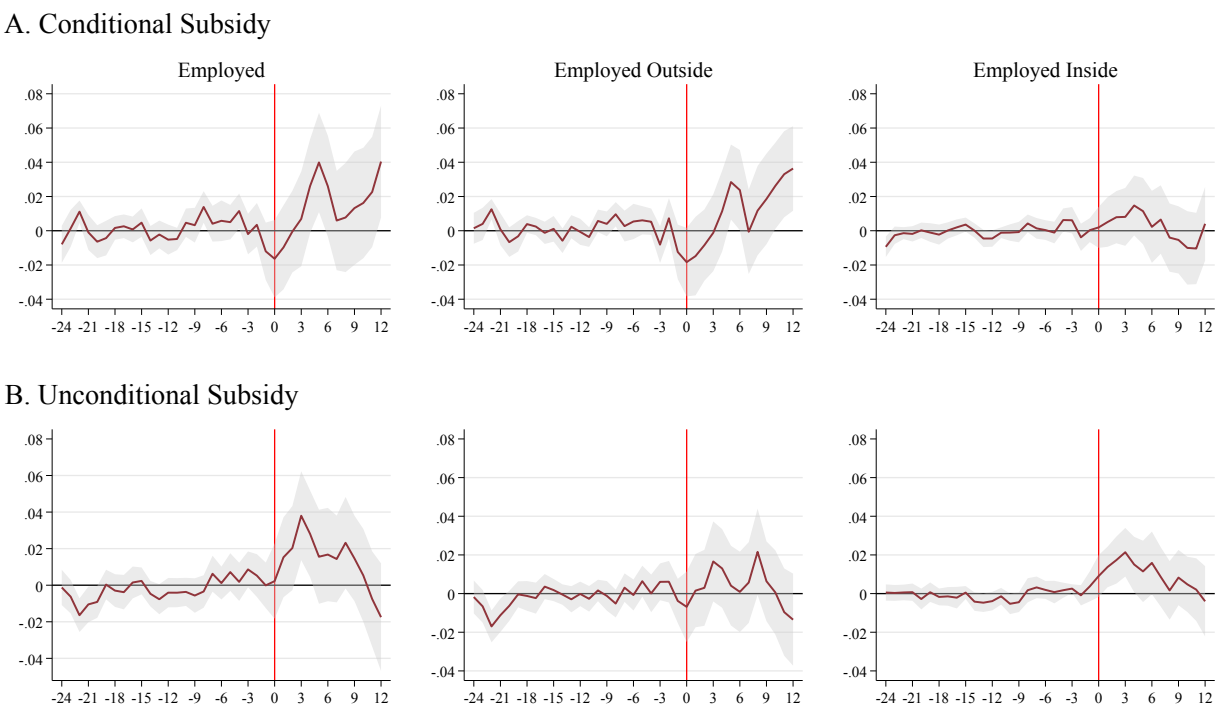


(b) Decomposition of Non Employment at Month 12



Notes: The figures decompose the employment effect at month 12 into underlying labor market transitions for each treatment arm for the Non-attached group. The left plots correspond to the Conditional Subsidy and the right plots to the Unconditional Subsidy. The three bars in Panel (a) report treatment effects and 95% confidence intervals for three outcomes: employment at month 12; employed within the first six months *and* employed at month 12 (*Stayed employed*); not employed within the first six months *and* employed at month 12 (*Gained employment*). Panel (b) provides the corresponding decomposition for nonemployment. The three bars report treatment effects and 95% confidence intervals for: nonemployment at month 12; not employed within the first six months *and* not employed at month 12 (*Stayed non-employed*); employed within the first six months *and* not employed at month 12 (*Exited employment*). All regressions include the full set of controls specified in Table 3.

Figure 7: Employment Effects of Conditional vs. Unconditional Subsidies



Notes: The figures present treatment effects for the Non-attached group across three outcomes: overall employment, employment outside the locality of residence, and employment within the locality. Panel (a) and (b) correspond to the Conditional and Unconditional Subsidy programs, respectively. Estimates are treatment-control differences with 95% confidence intervals controlling for age, gender, marital status, number of children, immigration status, education level, indicators for self-reported health limitation, single mother, Ultra-Orthodox Jew, Arab, and randomization block fixed effects. Month zero corresponds to the month of random assignment. Standard errors are clustered at the randomization unit level.

Table 1: Descriptive Statistics: Participating vs. All Other Localities

	Participating localities (1)	All other localities (2)
SES Index (1–10)	3.28	5.66
Peripherality Index (1–10)	4.29	7.49
Unemployment rate	0.10	0.04
Average income (NIS)	6,380	8,709
Private cars per adult (aged 20+)	0.41	0.49
Distance to closest big city (km)	29.83	16.50
Distance to closest employment hub (km)	20.04	9.38
Arab population share	0.59	0.11
Share of Arab localities	0.30	0.04
Average locality size (population)	5,284	7,418
Number of localities	315	1,132

Notes: The table presents descriptive statistics for localities participating in the program (column 1) and all other localities in Israel (column 2). Averages are weighted by population size. Data are from published CBS statistics for 2015, except for the unemployment rate, which is based on published IES statistics. Lower values on the SES index indicate greater socio-economic disadvantage. The peripherality index assigns lower values to areas more remote from employment opportunities, centers of economic activity, health services, and educational institutions. Data on average income include both employed and self-employed individuals.

Table 2: Availability for Work and Barriers to Employment Outside the Locality

Panel A: Availability for work:	
Available for work	0.718
Available for work outside locality of residence	0.455
Observations	6,543
Panel B: Barriers to employment outside locality:	
Transportation availability and cost	0.559
Family obligations	0.517
Social norms	0.225
It is not economically worthwhile	0.150
Observations	1,719

Notes: The table reports descriptive statistics from the baseline survey. Panel A includes all survey respondents who were unemployed. Panel B is restricted to the subset of baseline survey respondents who reported being available for work only within their locality of residence. Survey respondents were permitted to select multiple responses. The "Social Norms" indicator is a binary variable equal to one if the respondent reported that their family or spouse does not support working outside the locality, or that such employment is considered socially inappropriate, and zero otherwise.

Table 3: Descriptive Statistics and Balance Tests

	All		Non-Attached		Attached	
	Treatment Mean (1)	Difference T-C (2)	Treatment Mean (3)	Difference T-C (4)	Treatment Mean (5)	Difference T-C (6)
Employed [-12;-1]	0.606	0.001	0.106	-0.005	0.937	0.001
	(0.489)	(0.003)	(0.307)	(0.005)	(0.243)	(0.002)
Employed [-24;-13]	0.604	0.000	0.067	-0.005	0.959	-0.001
	(0.489)	(0.003)	(0.251)	(0.004)	(0.197)	(0.002)
Employed [-36;-25]	0.561	0.002	0.078	-0.003	0.881	0.001
	(0.496)	(0.003)	(0.269)	(0.004)	(0.324)	(0.004)
Annual earnings [-12;-1]	31,608	-168.516	584	-7.271	52,148	-441.087
	(44,296)	(332.391)	(2,698)	(38.600)	(46,875)	(531.507)
Annual earnings [-24;-13]	34,537	-108.226	351	-42.948	57,171	-366.523
	(48,030)	(363.206)	(2,465)	(35.015)	(50,439)	(569.808)
Annual earnings [-36;-25]	29,500	-48.858	641	65.353	48,606	-294.849
	(45,144)	(372.858)	(3,656)	(55.726)	(49,619)	(584.596)
Total benefits [-12;-1]	9,744	68.934	14,829	199.197	6,378	16.189
	(9,851)	(76.613)	(10,087)	(157.844)	(8,092)	(81.033)
Total benefits [-24;-13]	7,893	53.418	14,381	73.741	3,597	69.730
	(10,515)	(79.993)	(10,492)	(165.247)	(8,036)	(78.744)
Total benefits [-36;-25]	7,774	13.554	13,843	53.478	3,755	14.861
	(10,399)	(80.687)	(10,580)	(154.642)	(8,068)	(87.201)
Female	0.578	0.007	0.685	0.001	0.507	0.011*
	(0.494)	(0.005)	(0.465)	(0.007)	(0.500)	(0.006)
Age	39.001	0.138	42.971	0.367***	36.373	0.044
	(10.477)	(0.085)	(10.057)	(0.129)	(9.908)	(0.113)
Arab	0.656	-0.002	0.841	0.002	0.534	-0.005
	(0.475)	(0.003)	(0.365)	(0.005)	(0.499)	(0.004)
Ultra-orthodox	0.038	-0.002	0.024	-0.001	0.047	-0.004*
	(0.190)	(0.002)	(0.152)	(0.002)	(0.212)	(0.002)
Married	0.599	0.002	0.584	0.011	0.609	-0.002
	(0.490)	(0.005)	(0.493)	(0.007)	(0.488)	(0.006)
Highschool graduate	0.743	-0.002	0.535	-0.007	0.880	-0.000
	(0.437)	(0.004)	(0.499)	(0.008)	(0.325)	(0.004)
Number of children	2.876	0.028	3.985	0.073	2.142	0.012
	(3.064)	(0.025)	(3.606)	(0.049)	(2.374)	(0.027)
Single mother	0.086	0.001	0.118	0.000	0.065	0.002
	(0.281)	(0.003)	(0.323)	(0.005)	(0.247)	(0.003)
Immigrant	0.080	-0.000	0.039	-0.000	0.107	0.000
	(0.271)	(0.002)	(0.194)	(0.003)	(0.309)	(0.003)
Self-reported health limitation	0.360	-0.000	0.561	0.002	0.227	-0.000
	(0.480)	(0.004)	(0.496)	(0.008)	(0.419)	(0.005)
P-value for joint significance		.908		.301		.903
Observations	23665	53262	9426	19497	14239	33554

Notes: The table presents average characteristics and balance tests for the main sample (columns 1-2), Non-Attached sample (columns 3-4), and Attached sample (columns 5-6). Attached individuals worked at least six months during the 36 months before randomization; Non-Attached worked less than six months. Odd-numbered columns show treatment group means (standard deviations in parentheses). Even-numbered columns show treatment-control differences conditional on randomization unit fixed effects, with standard errors clustered at the randomization unit level (in parentheses). The bottom row reports p-values for joint significance of all covariates from a linear probability model of treatment status conditional on randomization unit fixed effects. Employment, earnings, and benefits history cover months 1-12, 13-24, and 25-36 before randomization. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 4: Treatment Effects of the Conditional Subsidy Program

	All			Non-Attached			Attached		
	3rd Month (1)	6th Month (2)	12th Month (3)	3rd Month (4)	6th Month (5)	12th Month (6)	3rd Month (7)	6th Month (8)	12th Month (9)
Employed	0.013***	0.022***	0.012***	0.018***	0.025***	0.021***	0.010*	0.021***	0.008
	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)
	0.284	0.402	0.489	0.097	0.126	0.163	0.383	0.548	0.660
Earnings	43.876	121.187***	63.211**	28.898	63.498***	74.238***	51.767	162.600***	66.186
	(28.984)	(32.000)	(31.500)	(18.640)	(23.973)	(26.995)	(45.359)	(49.743)	(47.585)
	1587.042	2380.121	3111.293	313.443	418.740	581.705	2259.627	3415.922	4447.162
Stable job		0.010**	0.013***		0.017***	0.021***		0.006	0.010**
	—	(0.004)	(0.004)	—	(0.005)	(0.006)	—	(0.006)	(0.006)
		0.227	0.360		0.078	0.114		0.305	0.490
Received social benefits	-0.001	-0.010**	-0.005	-0.004	0.004	-0.013**	0.000	-0.019***	-0.001
	(0.004)	(0.004)	(0.003)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.004)
	0.607	0.431	0.329	0.770	0.737	0.697	0.498	0.269	0.134
Monthly social benefits	-16.058	-28.651***	-10.785	-19.142*	-6.665	-20.461	-14.380	-45.923***	-9.318
	(14.440)	(8.892)	(7.653)	(11.668)	(12.063)	(13.043)	(21.801)	(12.426)	(9.548)
	1545.191	808.312	605.870	1299.892	1253.262	1211.832	1674.732	573.335	285.863
Subsidy payments	39.490***	40.818***	4.394***	15.155***	12.600***	1.637***	54.065***	57.924***	6.092***
	(1.457)	(1.512)	(0.402)	(1.346)	(1.120)	(0.396)	(1.806)	(1.792)	(0.586)
	0.020	0.041	0.000	0.000	0.000	0.000	0.031	0.062	0.000
Total income	73.184***	123.534***	54.004*	25.251	66.774***	45.383*	100.715***	160.626***	63.779
	(24.543)	(30.959)	(31.605)	(19.657)	(23.540)	(26.388)	(37.784)	(48.012)	(48.119)
	3132.273	3188.474	3698.284	1613.336	1672.002	1793.585	3934.421	3989.319	4734.261
Observations	53263	53263	53263	19498	19498	19498	33554	33554	33554

Notes: The table shows the effects of the Conditional Subsidy for outcomes measured at months 3, 6, and 12 post-randomization. Results are presented separately for the full sample (columns 1-3), the Non-attached sample (columns 4-6), and the Attached sample (columns 7-9). Each cell reports the estimated coefficient from a separate regression. Controls include age, gender, marital status, number of children, immigration status, education level, indicators for self-reported health limitation, single mother, Ultra-Orthodox Jew, Arab, and vectors of welfare, employment, and earnings history during the three years preceding randomization. The third row of each cell reports control group means. Standard errors clustered at the randomization unit level in parentheses. "Stable job" is defined as continuous employment with the same employer for at least four months (by month 6) or six months (by month 12). Total income includes earnings, social benefits and subsidy payments. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 5: Cumulative Treatment Effects of the Conditional Subsidy 12 Months Post-Randomization

	All (1)	Non-Attached (2)	Attached (3)
Total months employed	0.168***	0.246***	0.132**
	(0.040)	(0.061)	(0.052)
	4.734	1.553	6.414
Total earnings	745.190**	634.151***	872.727*
	(305.932)	(239.373)	(469.613)
	28130.402	5191.955	40244.137
Total months with same employer	0.127***	0.201***	0.098**
	(0.036)	(0.055)	(0.046)
	3.993	1.393	5.365
Total months received social benefits	-0.044	-0.038	-0.057
	(0.033)	(0.062)	(0.038)
	6.378	9.701	4.623
Total social benefits	-160.670*	-139.994	-207.456*
	(88.229)	(124.480)	(120.385)
	14945.489	16498.100	14125.561
Total subsidy payments	300.144***	107.453***	417.220***
	(9.700)	(9.098)	(10.970)
	0.366	0.061	0.528
Total income	884.664***	601.610**	1,082.491**
	(286.660)	(242.380)	(437.340)
	43076.258	21690.115	54370.227
Observations	53263	19498	33554

Notes: The table shows the effects of the Conditional Subsidy for cumulative outcomes aggregated over the 12 month post randomization period. Results are presented for the full sample (column 1), the Non-attached sample (column 2), and the Attached sample (column 3). Each cell reports the estimated coefficient from a separate regression. Controls include age, gender, marital status, number of children, immigration status, education level, indicators for self-reported health limitation, single mother, Ultra-Orthodox Jew, Arab, and vectors of welfare, employment, and earnings history during the three years preceding randomization. The third row of each cell reports control group means. Standard errors clustered at the randomization unit level in parentheses. Total income includes earnings, social benefits and subsidy payments. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 6: Treatment Effects of the Unconditional vs. Conditional Subsidy

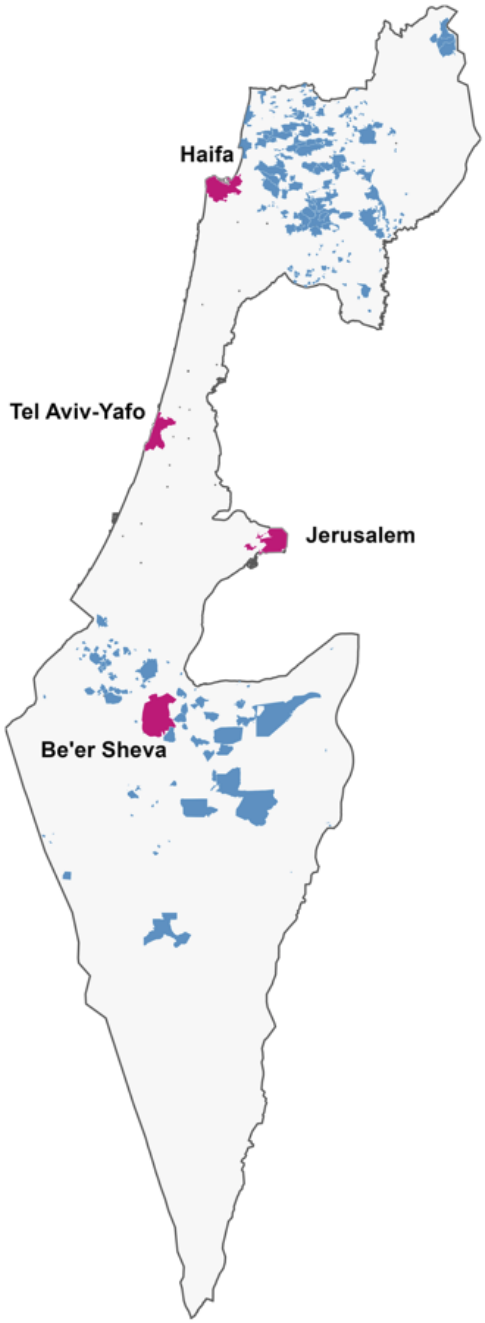
	Unconditional Subsidy			Conditional Subsidy		
	3rd Month (1)	6th Month (2)	12th Month (3)	3rd Month (4)	6th Month (5)	12th Month (6)
Panel A: Main Outcomes						
Employed	0.039**	0.017	-0.015	0.005	0.023	0.039*
	(0.015)	(0.015)	(0.018)	(0.016)	(0.018)	(0.020)
	0.139	0.170	0.203	0.176	0.217	0.255
Earnings	82	51	-146	-63	92	194**
	(68)	(72)	(83)	(73)	(85)	(91)
	573	784	987	731	930	1145
Panel B: Job Location and Quality						
Employed outside	0.017	0.000	-0.012	-0.002	0.022	0.035**
	(0.012)	(0.012)	(0.014)	(0.013)	(0.014)	(0.015)
	0.103	0.125	0.150	0.126	0.153	0.183
Earnings outside	26	-1	-107	-70	74	172**
	(62)	(70)	(77)	(68)	(78)	(76)
	460	636	810	573	725	902
Employed in large Firm	0.018*	-0.006	-0.012	-0.002	0.008	0.037***
	(0.010)	(0.013)	(0.015)	(0.011)	(0.014)	(0.013)
	0.085	0.107	0.117	0.098	0.120	0.142
Observations	2176	2176	2176	2140	2140	2140

Notes: The table shows the effects of the Unconditional Subsidy (cols. 1-3) and the Conditional Subsidy (cols. 4-6) for outcomes measured at months 3, 6, and 12 post-randomization. Each cell reports the estimated coefficient from a separate regression. Controls include age, gender, marital status, number of children, immigration status, education level, indicators for self-reported health limitation, single mother, Ultra-Orthodox Jew, Arab, and vectors of welfare, employment, and earnings history during the three years preceding randomization. The third row of each cell reports control group means. Standard errors clustered at the randomization unit level in parentheses. "Employed outside" and "Earnings outside" denote employment and earnings from jobs located outside the locality of residence and equal zero for those not employed or working within the locality. "Large firm" equals one if employed at a firm with more than 80 employees. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

A. Appendix

A1. Appendix Figures and Tables

Figure A1: The Geographic Distribution of Participating Localities



Notes: The map show the localities participating in the program (in blue) and the four main metropolitan cities of Israel, Tel Aviv, Jerusalem, Be'er Sheva, and Haifa (in pink).

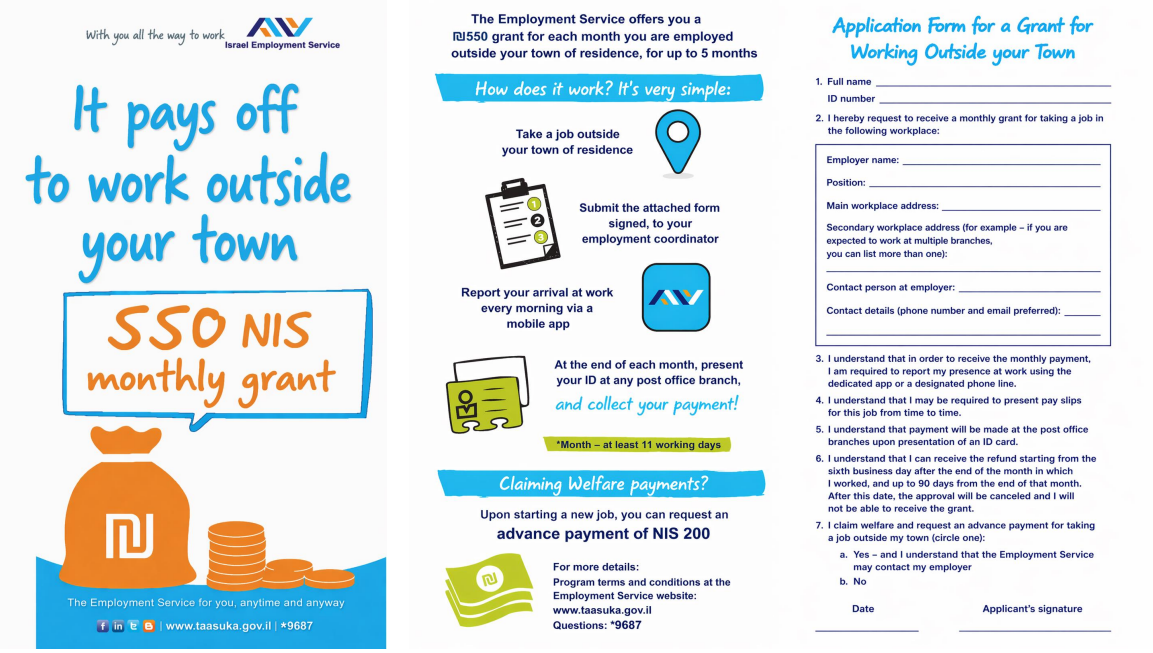
Figure A2: The Conditional Subsidy Program Brochures

(a) Hebrew

(b) Arabic

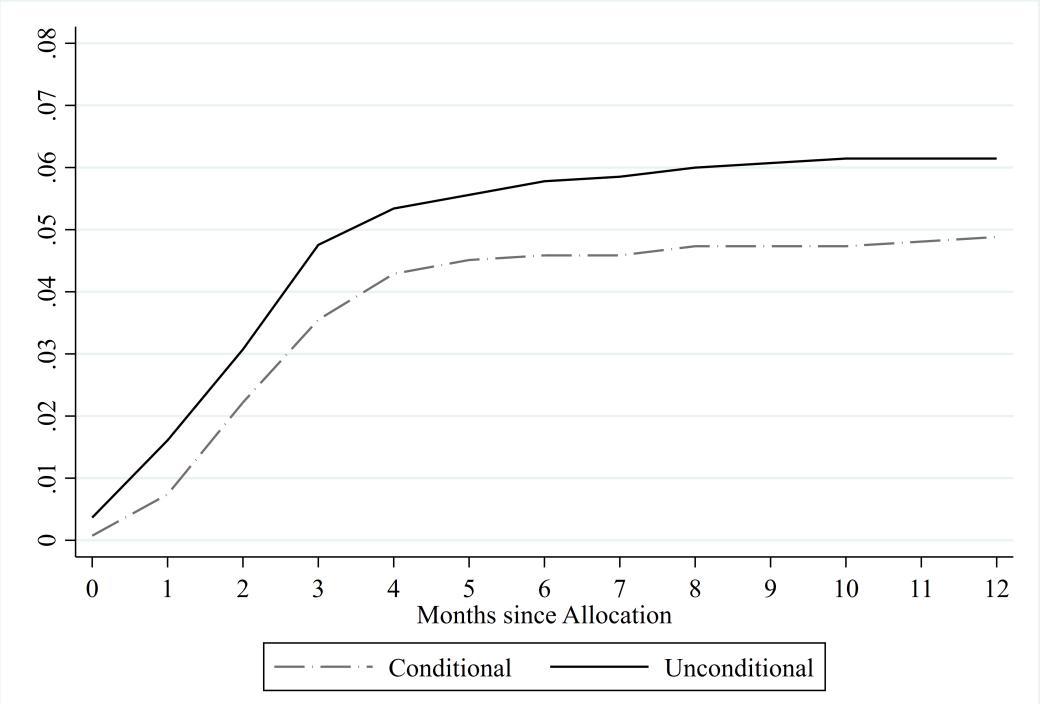
Notes: The figure presents the brochure distributed to program participants in Hebrew (Panel A) and Arabic (Panel B).

Figure A3: Program Brochure: English Translation



Notes: The figure shows an English translation of the brochure distributed to program participants.

Figure A4: Conditional vs. Unconditional Subsidy Claiming Rates



Notes: The figure shows the cumulative proportion of treated individuals who received at least one monthly subsidy payment under the Conditional and Unconditional Subsidy programs, measured at each month following randomization.

Table A1: Reservation Wage and Commute Time

	Desired Work	Desired Work	Reservation Wage	Commute Time	RW % Change Additional Commute	
	Days (Weekly)	Hours (Daily)	NIS (Hourly)	Minutes (Daily)	30 Min.	60 Min.
	(1)	(2)	(3)	(4)	(5)	(6)
Mean	5	8	46	86	8.9%	20.9%
Median	5	8	36	60	2.9%	12.5%

Notes: The table reports reservation wage information from unemployed respondents in the baseline survey. Mean and median values are shown for desired weekly workdays, daily hours, hourly reservation wage, and maximum acceptable commute time. Columns (5)-(6) show the percentage increase in reservation wage respondents would require if their daily round-trip commute increased by 30 or 60 minutes. All commute times refer to daily round trips.

Table A2: Descriptive Statistics – Localities by Type of Program vs. All Other Localities

	Conditional subsidy (1)	Unconditional subsidy (2)	All other localities (3)
SES Index (1–10)	3.08	3.48	5.66
Peripherality Index (1–10)	4.20	4.38	7.49
Unemployment rate	0.11	0.09	0.04
Average income (NIS)	6,267	6,495	8,709
Private cars per adult (aged 20+)	0.39	0.42	0.49
Distance to closest big city (km)	27.26	31.45	16.50
Distance to closest employment hub (km)	20.82	18.79	9.38
Arab population share	0.58	0.62	0.11
Share of Arab localities	0.29	0.32	0.04
Average locality size (population)	5,209	5,271	7,418
Number of localities	154	157	1,132

Notes: The table presents descriptive statistics for localities participating in the Conditional Subsidy program (column 1), the Unconditional Subsidy program (column 2), and all other localities in Israel (column 3). Averages are weighted by population size. Data are from published CBS statistics for 2015, except for the unemployment rate, which is based on published IES statistics. Lower values on the SES index indicate greater socio-economic disadvantage. The peripherality index assigns lower values to areas more remote from employment opportunities, centers of economic activity, health services, and educational institutions. Data on average income include both employed and self-employed individuals.

Table A3: Descriptive Statistics and Balance Tests – 24-Months Horizon Sample

	All		Non- Attached		Attached	
	Treatment Mean (1)	Difference T-C (2)	Treatment Mean (3)	Difference T-C (4)	Treatment Mean (5)	Difference T-C (6)
Employed [-12;-1]	0.589	0.000	0.100	-0.005	0.936	0.000
	(0.492)	(0.003)	(0.300)	(0.005)	(0.245)	(0.003)
Employed [-24;-13]	0.586	-0.002	0.063	-0.007	0.957	-0.003
	(0.493)	(0.003)	(0.242)	(0.004)	(0.202)	(0.002)
Employed [-36;-25]	0.546	0.001	0.074	-0.006	0.881	0.002
	(0.498)	(0.004)	(0.262)	(0.004)	(0.323)	(0.004)
Annual earnings [-12;-1]	30,655	-403.704	545	-3.812	52,059	-841.455
	(43,780)	(371.282)	(2,639)	(40.786)	(46,601)	(624.719)
Annual earnings [-24;-13]	33,287	-346.926	328	-18.841	56,716	-754.373
	(47,467)	(395.342)	(2,512)	(34.158)	(50,291)	(653.214)
Annual earnings [-36;-25]	28,724	-91.666	621	56.156	48,703	-355.358
	(44,752)	(401.312)	(3,722)	(59.389)	(49,559)	(662.862)
Total benefits [-12;-1]	10,092	22.571	15,313	108.207	6,381	-10.757
	(9,974)	(85.706)	(9,933)	(164.239)	(8,181)	(94.048)
Total benefits [-24;-13]	8,363	35.212	14,956	14.553	3,677	65.899
	(10,609)	(89.836)	(10,217)	(170.550)	(8,094)	(90.939)
Total benefits [-36;-25]	8,101	-39.034	14,283	-2.548	3,706	-44.731
	(10,484)	(89.748)	(10,437)	(158.723)	(8,008)	(99.651)
Female	0.583	0.010**	0.685	0.001	0.511	0.017**
	(0.493)	(0.005)	(0.465)	(0.008)	(0.500)	(0.007)
Age	39.315	0.187**	43.265	0.320**	36.507	0.143
	(10.526)	(0.093)	(10.018)	(0.136)	(9.962)	(0.128)
Arab	0.657	-0.003	0.850	0.001	0.519	-0.007
	(0.475)	(0.004)	(0.357)	(0.006)	(0.500)	(0.005)
Ultra-orthodox	0.034	-0.003*	0.021	0.000	0.043	-0.006**
	(0.180)	(0.002)	(0.143)	(0.002)	(0.202)	(0.002)
Married	0.598	0.001	0.582	0.007	0.610	-0.002
	(0.490)	(0.005)	(0.493)	(0.008)	(0.488)	(0.007)
Highschool graduate	0.733	-0.002	0.519	-0.010	0.886	0.003
	(0.442)	(0.005)	(0.500)	(0.009)	(0.318)	(0.004)
Number of children	2.855	0.024	3.963	0.055	2.066	0.016
	(3.038)	(0.027)	(3.578)	(0.052)	(2.279)	(0.029)
Single mother	0.085	0.002	0.114	-0.000	0.065	0.004
	(0.279)	(0.003)	(0.317)	(0.005)	(0.246)	(0.003)
Immigrant	0.082	0.001	0.037	0.001	0.113	0.001
	(0.274)	(0.003)	(0.189)	(0.003)	(0.317)	(0.004)
Self-reported health limitation	0.364	-0.002	0.563	0.001	0.223	-0.002
	(0.481)	(0.004)	(0.496)	(0.008)	(0.416)	(0.005)
P-value for joint significance		.581		.486		.229
Observations	19434	42575	8075	17481	11359	24940

Notes: The table presents average characteristics and balance tests for the subsample of participants from the main sample who can be tracked for an extended 24-month horizon. Columns (1)-(2) refer to the full sample, columns (3)-(4) to the Non-Attached sample, and columns (5)-(6) to the Attached sample. Odd-numbered columns show treatment group means (standard deviations in parentheses). Even-numbered columns show treatment-control differences conditional on randomization unit fixed effects, with standard errors clustered at the randomization unit level (in parentheses). The bottom row reports p-values for joint significance of all covariates from a linear probability model of treatment status conditional on randomization unit fixed effects. Employment, earnings, and benefits history cover months 1-12, 13-24, and 25-36 before randomization. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table A4: Descriptive Statistics and Balance Tests — Conditional and Unconditional Subsidy

	Cond. subsidy offices		Uncond. subsidy offices	
	Treatment Mean (1)	Difference T-C (2)	Treatment Mean (3)	Difference T-C (4)
Employed [-12;-1]	0.612	0.001	0.622	-0.002
	(0.487)	(0.004)	(0.485)	(0.004)
Employed [-24;-13]	0.608	0.000	0.626	-0.001
	(0.488)	(0.004)	(0.484)	(0.004)
Employed [-36;-25]	0.562	0.002	0.590	-0.000
	(0.496)	(0.004)	(0.492)	(0.004)
Annual earnings [-12;-1]	31,524	-92.264	33,157	-416.561
	(44,208)	(390.810)	(45,296)	(480.095)
Annual earnings [-24;-13]	34,577	180.787	36,665	-719.922
	(48,425)	(469.745)	(48,833)	(496.727)
Annual earnings [-36;-25]	29,038	46.806	31,738	-138.329
	(45,340)	(497.896)	(46,249)	(490.968)
Total benefits [-12;-1]	9,527	205.749**	9,650	-26.999
	(9,807)	(100.084)	(9,842)	(95.005)
Total benefits [-24;-13]	7,827	149.253	7,257	-62.317
	(10,569)	(104.475)	(10,341)	(98.948)
Total benefits [-36;-25]	7,885	106.807	7,124	33.180
	(10,535)	(104.278)	(10,144)	(103.046)
Female	0.577	0.008	0.576	0.009
	(0.494)	(0.006)	(0.494)	(0.006)
Age	38.665	0.135	38.797	0.103
	(10.106)	(0.113)	(10.762)	(0.112)
Arab	0.679	-0.003	0.618	-0.004
	(0.467)	(0.003)	(0.486)	(0.005)
Ultra-orthodox	0.049	-0.002	0.025	-0.003
	(0.215)	(0.002)	(0.156)	(0.002)
Married	0.606	0.003	0.585	0.005
	(0.489)	(0.006)	(0.493)	(0.006)
Highschool graduate	0.751	-0.000	0.749	-0.004
	(0.432)	(0.005)	(0.434)	(0.005)
Number of children	3.059	0.045	2.607	-0.005
	(3.199)	(0.030)	(2.854)	(0.033)
Single mother	0.093	0.000	0.083	0.003
	(0.290)	(0.003)	(0.275)	(0.003)
Immigrant	0.067	-0.002	0.101	0.003
	(0.249)	(0.003)	(0.301)	(0.003)
Self-reported health limitation	0.355	-0.003	0.353	0.005
	(0.479)	(0.005)	(0.478)	(0.005)
P-value for joint significance		.820		.412
Observations	13380	31641	14661	32891

Notes: The table presents average characteristics and balance tests for the offices assigned to the Conditional Subsidy (columns 1-2) and the offices assigned to the Unconditional Subsidy (columns 3-4). Odd-numbered columns show treatment group means (standard deviations in parentheses). Even-numbered columns show treatment-control differences conditional on randomization unit fixed effects, with standard errors clustered at the randomization unit level (in parentheses). The bottom row reports p-values for joint significance of all covariates from a linear probability model of treatment status conditional on randomization unit fixed effects. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table A5: Descriptive Statistics and Balance Tests – Unconditional Subsidy

	All		Non- Attached		Attached	
Variable	Treatment Mean (1)	Difference T-C (2)	Treatment Mean (3)	Difference T-C (4)	Treatment Mean (5)	Difference T-C (6)
Employed [-12;-1]	0.677	-0.012*	0.119	-0.008	0.931	-0.008*
	(0.468)	(0.006)	(0.324)	(0.014)	(0.254)	(0.004)
Employed [-24;-13]	0.691	-0.004	0.087	-0.015	0.965	0.003
	(0.462)	(0.006)	(0.282)	(0.012)	(0.184)	(0.003)
Employed [-36;-25]	0.657	-0.003	0.113	-0.014	0.904	0.003
	(0.475)	(0.007)	(0.316)	(0.016)	(0.295)	(0.006)
Annual earnings [-12;-1]	36,538	-705.278	697	39.546	52,820	-708.349
	(47,141)	(854.199)	(3,163)	(118.954)	(48,772)	(1063.062)
Annual earnings [-24;-13]	41,786	-1142.713	507	-308.573*	60,539	-1166.743
	(51,447)	(959.376)	(2,449)	(161.755)	(52,160)	(1191.041)
Annual earnings [-36;-25]	35,584	-47.248	958	27.925	51,315	235.149
	(49,103)	(957.330)	(4,193)	(180.506)	(52,023)	(1199.683)
Total benefits [-12;-1]	8,763	168.738	12,318	-21.714	7,149	165.534
	(9,645)	(162.170)	(10,828)	(429.818)	(8,586)	(169.014)
Total benefits [-24;-13]	5,562	-24.440	10,962	-192.678	3,109	-59.552
	(9,892)	(164.431)	(11,820)	(516.943)	(7,721)	(152.482)
Total benefits [-36;-25]	5,939	340.400*	10,707	-105.316	3,772	325.282*
	(9,866)	(176.840)	(11,296)	(562.310)	(8,282)	(166.988)
Female	0.569	0.014	0.696	0.011	0.512	0.010
	(0.495)	(0.010)	(0.460)	(0.023)	(0.500)	(0.011)
Age	37.293	0.024	39.246	-0.383	36.405	0.098
	(10.210)	(0.219)	(10.674)	(0.514)	(9.867)	(0.239)
Arab	0.595	-0.010	0.803	-0.007	0.501	-0.011
	(0.491)	(0.008)	(0.398)	(0.015)	(0.500)	(0.009)
Ultra-orthodox	0.028	-0.001	0.020	-0.001	0.032	-0.001
	(0.166)	(0.004)	(0.139)	(0.007)	(0.177)	(0.004)
Married	0.573	0.012	0.554	-0.004	0.582	0.018
	(0.495)	(0.010)	(0.497)	(0.024)	(0.493)	(0.011)
Highschool graduate	0.790	-0.001	0.577	0.018	0.887	-0.002
	(0.407)	(0.007)	(0.494)	(0.022)	(0.317)	(0.007)
Number of children	2.534	-0.027	3.709	-0.161	2.000	0.003
	(2.838)	(0.053)	(3.702)	(0.173)	(2.140)	(0.046)
Single mother	0.092	0.002	0.134	-0.001	0.073	0.002
	(0.290)	(0.005)	(0.341)	(0.014)	(0.261)	(0.006)
Immigrant	0.109	0.005	0.055	0.001	0.134	0.008
	(0.312)	(0.006)	(0.228)	(0.010)	(0.341)	(0.008)
Self-reported health limitation	0.319	0.008	0.491	0.024	0.241	0.006
	(0.466)	(0.009)	(0.500)	(0.022)	(0.428)	(0.010)
P-value for joint significance		.016		.479		.013
Observations	4,376	11,270	1,367	2,024	3,009	9,190

Notes: The table presents average characteristics and balance tests for the sample assigned to the Unconditional Subsidy program. Columns (1)-(2) refer to the full sample, columns (3)-(4) to the Non-Attached sample, and columns (5)-(6) to the Attached sample. Odd-numbered columns show treatment group means (standard deviations in parentheses). Even-numbered columns show treatment-control differences conditional on randomization unit fixed effects, with standard errors clustered at the randomization unit level (in parentheses). The bottom row reports p-values for joint significance of all covariates from a linear probability model of treatment status conditional on randomization unit fixed effects. Employment, earnings, and benefits history cover months 1-12, 13-24, and 25-36 before randomization. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table A6: Treatment Effects of the Conditional Subsidy Program up to 24 Months Post-Randomization

	All		Non- Attached		Attached	
	12th Month	24th Month	12th Month	24th Month	12th Month	24th Month
	(1)	(2)	(3)	(4)	(5)	(6)
Employed	0.015***	0.006	0.019***	0.016***	0.013**	0.000
	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)
	0.452	0.458	0.158	0.183	0.658	0.649
Earnings	68.161**	10.967	59.186**	49.290	80.808	-8.586
	(33.802)	(40.032)	(27.474)	(34.346)	(53.416)	(64.286)
	2819.660	3110.502	554.317	717.658	4401.944	4781.843
Observations	42576	42576	17482	17482	24940	24940

Notes: The table shows the effects of the Conditional Subsidy for outcomes measured at months 12 and 24 post-randomization. Results are based on the subsample of individuals who can be tracked for an extended 24-month horizon. Columns (1)-(2) refer to the full sample, columns (3)-(4) to the Non-Attached sample, and columns (5)-(6) to the Attached sample. Each cell reports the estimated coefficient from a separate regression. Controls include age, gender, marital status, number of children, immigration status, education level, indicators for self-reported health limitation, single mother, Ultra-Orthodox Jew, Arab, and vectors of welfare, employment, and earnings history during the three years preceding randomization. The third row of each cell reports control group means. Standard errors clustered at the randomization unit level in parentheses. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$