# Essays on the effectiveness of policy changes and their impact on minority groups

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by

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## Abstract

This dissertation examines the effectiveness of a number of policy measures, by studying the behavior and situation of different social groups, in several set-ups. It thereby tries to uncover a more general conclusion on how effectiveness unfolds in the case of minority and disadvantaged groups. The dissertation includes three essays, each of them studies effectiveness in a different context: a reform in the allocation of governmental grants to localities, an active labor market program, and a training program. Common to the three essays is the focus on effectiveness, the study of Israeli set-ups, and the reference to minorities. The essays differ in terms of the unit of analysis and the methodology used. The first chapter studies the impact of a natural experiment (reform) on localities' receipt of grants, using a difference-in-differences approach. The second chapter uses a randomized control trial to study the impact of an Active Labor Market Program on the outcomes of income support recipients. The third chapter is an observational study that, based on matching, studies the effect of training vouchers on the outcomes of the unemployed. The findings show that adopting rules for resource allocation is an effective way to restrain political favoritism; that changing the costs of benefits can increase labor market integration and trigger changes in the allocation of labor supply across spouses; and that training vouchers for the unemployed have a significant and long-term impact on employment. The findings shed light on the conditions for achieving effectiveness in various set-ups, and also on the reasons and circumstances in which minorities differ or converge to the situation of majority group members.

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# Contents

# Introduction

In the context of policy, effectiveness refers to the ability to achieve planned outcomes. Hence, effective organizations or effective programs are those that can promote and achieve such outcomes. In many cases, effectiveness is also a matter of scale and intensity, so that a process, organization or a program is very effective to the extent that it achieves or triggers high levels of the outcome discussed, for example, it saves lots of money or it drastically lowers mortality rates. Alternatively, very effective can mean effective beyond any doubt, where doubts are removed based on the many cases examined or based on the various methodologies and models used.

Effectiveness of the different levels of government ensures a wise use of taxpayers' money, and it is key for the stable and consistent provision of services. At the same time, effectiveness in many ways relates to achieving high-level policy goals, such as equality, efficiency and the like. This is because effectiveness signals a functioning government, and because high-level goals are achieved based on successfully completing a long and diverse series of small and local acts in various fields. This is how effectiveness goes beyond technical matters.

Finally, studying effectiveness is not only important for reflecting on institutions' situations. It also enables examining various theoretical predictions by studying people's behavior, for example, when asking why a single process has been effective for one group but not for another. In such cases, exploring effectiveness can merely serve as a platform for studying various economic and social phenomena.

Governmental effectiveness is especially important for minorities. This is because in most countries, disadvantaged populations to a greater extent depend on public services and governmental support – relative to the rest of the population for which private alternatives are more accessible. To the extent that minorities make a disadvantaged group, which is usually the case, it follows that minorities have more sensitivity to the quality and effectiveness of public services and the governmental policy at large.

Nevertheless, achieving effectiveness may be further challenging in the case of minorities because many programs and services were historically designed to serve the majority population; and because in many cases, policy-makers who belong to the majority group design them. Hence, these services are to a greater extent tailored to the majority group's needs, preferences and attributes. This reality can originate in a discriminatory approach and an explicit willingness to channel resources away from the minority group, but it can also appear in the background of majority group members' limited awareness of the minority's needs and preferences.

Although this is not always the case, minorities may encounter unique challenges that deserve special attention and different handling. Some of these challenges are more common among minorities, especially migrants, and even more so among first-generation migrants, but can also be found among non-migrant disadvantaged populations (e.g., single mothers, the working poor). Other types of challenges are inherent to being a minority (e.g., proficiency of the majority language, limited connections with and accessibility to the political and economic elite and state institutions).

With this background, achieving effectiveness – in designing and providing services and programs to minorities – may be uniquely challenging. Specifically, minorities can be the subject of effectiveness in a number of ways. First, minorities can be the explicit objective of reforms, that is, reforms focused on minority groups as such, focusing on their special needs (Arendt et al., 2020). Second, effectiveness regarding minorities can be addressed when examining minorities' behavior vis a vis that of majority group members in response to a single common service or reform.

Thus, what works for minorities? What are the keys for effectiveness in the case of minorities and disadvantaged groups? Furthermore, how can these questions be addressed?

Israel is a very heterogeneous country, home to numerous distinct minority groups. The country populates a large Arab minority (around 20% of the population), and is probably the only advanced country whose population includes a significant group of an Arab-speaking, non-migrant minority. The Arab population includes Muslims, Christians and Druze.[1] In addition, Israel is also home to numerous unique groups of Jewish migrants (especially those who migrated to the country from the former USSR and Ethiopia through the 1990s) and to numerous distinct religious and ethnic sub-groups that are part of the majority Jewish population.

Largely, many of the central social challenges the country encountered throughout the 2000s and the 2010s – especially in the context of labor market integration, shaping the public sphere and the relations between groups – relate to the situation of the two big minority groups in society: the Arabs and the Ultra-orthodox (Haredi) Jews.

Its social heterogeneity has made Israel a unique social lab that enables examining different theoretical questions in the fields of discrimination, labor market integration, the absorption of migrants, norms and identity and many other pressing issues. This social lab has been the source of numerous waves of studies published in the last 25 years that utilize valuable setups that the unique attributes of Israeli society create. Among other issues, these papers focus on migration from the USSR (Lerner and Hendeles, 1996; Chiswick, 1998; Eckstein and Weiss, 2004; Friedberg, 2001; Powell, Clark and Nowrasteh, 2017; Raijman and Semyonov, 1998) and

[1] There are numerous emerging economies that have large non-migrant (non-Arab) Muslim minorities, such as North-Macedonia, Bulgaria and India, as well as numerous advanced countries, mostly in West and Northwest Europe, with Muslim migrant communities. In Qatar and the United Arab Emirates, foreigners make the majority of the population, although most of them are not citizens of the country.

Ethiopia (Lavy, Schlosser and Shany, 2021), the effect of terrorism (Benmelech, Berrebi and Klor, 2015, 2012; Elster, Zussman and Zussman, 2017; Shayo and Zussman, 2011) and the unique economic arrangements and gradual privatization of the *kibbutz* (Abramitzky, 2008, 2011; Abramitzky, Lavy and Segev, 2019; Abramitzky, Lavy and Pérez, 2021; Abramitzky et al., 2022). Another group of papers focuses on studying discrimination; in-group bias; the interactions between Jews and Arabs, and between groups within the Jewish population (Bar and Zussman, 2017; Shayo and Zussman, 2011; Zussman, 2013; Lavy, Sand and Shayo, 2022; Enos and Gidron, 2016); and they utilize the attributes of the Arab community (Ben-Bassat and Dahan, 2012) and the Haredi community (Berman, 2000) that have a unique significance in the Israeli context.

The essays included in this PhD dissertation use the Israeli lab to answer a number of questions that relate to measured effectiveness in the context of minorities and other disadvantages social groups. These questions examine differences in the response of minorities (and other disadvantaged groups) vis a vis that of the majority group to various policy measures. These measures include a comparison in which the two groups' responses are examined separately and unconditionally alongside a case in which the situation of one group depends on that of the other because it involves resource allocation in the form of a zero-sum game.

The first chapter – co-authored with Momi Dahan – studies the effect of a reform that changed the allocation mechanism of grants transferred from the central government to municipalities. The reform was based on a move from discretion-based to rules-based allocation. It asks whether rules-based allocation is an effective tool for restraining political favoritism. Based on a difference-in-differences approach, we find that the reform has triggered an improvement in the situation of low-income and Arab localities relative to high-income and Jewish localities, respectively, regarding the probability to receive grants. This result suggests that political favoritism has characterized the pre-reform period.

Two main lessons can be drawn from this paper. First, adopting rules for the sake of resource allocation can be effective in restraining political favoritism, at least when an external independent organization formulates these rules. This is non-trivial because rules can be set, and indeed have been set in the past, in ways that are advantageous to specific groups. Second, the results strengthen the notion that disadvantaged and minority groups can benefit from the formulization and universalization of allocation processes. This is because discretion-based allocation is more likely to give an advantage to economically stronger groups and the majority, who enjoy higher accessibility to decision-makers.[2]

[2] An opposite assumption, that is also sense-making, would be that in a discretion-based allocation, allocators will prefer the worst-off.

The second chapter studies the effect of an active labor market (ALM) program on the labor market outcomes of disadvantaged income support recipients. Based on a randomized control trial, I find that the program triggered a significant increase in employment, earnings and disability insurance benefits, alongside a decrease in income support benefits. Men, Jews and single participants showed higher responsiveness to treatment relative to women, Arabs and married participants, respectively.

The study shows that a change in the relative costs of benefits is an effective tool for shifting people between benefit types and for incentivizing them to re-integrate in the labor market. In addition, the study demonstrates the existence of intra-household insurance and that it takes place in households that belong to the lower income brackets – as reflected in the adjustment of labor supply among spouses.

The third chapter studies the long-term effects of training vouchers on the labor market outcomes of the unemployed – using re-weighting-based matching. The results show that training vouchers have a significant effect on the employment and earnings of the unemployed that lasts at least up to 6 years after the voucher award. The effects are stronger for Arabs, low-skilled workers and the long-term unemployed, but are lower than average among income support recipients.

A few important lessons can be learned from this paper. First, short training for the unemployed can be effective in the long term, but mostly as a way to restore employment rates (at the level of individuals: *employability*) and mostly without increasing productivity. Second, such training is more effective for those with weak labor market attachment. Third, the match between participants and training is crucial for program long-term effectiveness.

In summary, all three chapters address Israeli setups. They are all focused on measuring the effectiveness of a single policy measure – a reform or a program – and thereby try to understand or highlight a further general phenomenon. In all cases, the phenomenon studied relates to the behavior or situation of minorities and disadvantaged groups in society. In one case, the units of analysis are localities – so that the paper refers to the implied behavior of decision-makers in the central government and perhaps to mayors' behavior. In the two other papers, the units of analysis are individuals, unemployed and income support recipients. While the first chapter is based on a natural experiment, the second is purely experimental and the third is more observational. A short chapter follows the three papers and it discusses the common lessons and concludes.

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# Chapter 1.
# Revealed political favoritism: evidence from the allocation of state lottery grants in Israel

Co-authored with Momi Dahan

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**Abstract**

This paper explores whether rules-based allocation is an effective tool for restraining political favoritism. We exploit a policy shift in distributing state lottery revenues to Israeli municipalities from discretion-based to rules-based allocation to estimate the extent of political favoritism. By comparing the likelihood of receiving grants by two types of localities before and after a policy reform, our approach offers a complementary empirical strategy for studying political favoritism that can be used even in the absence of exogenous variation in political connections; it likewise may reveal the overall impact of multiple political interests and social affiliations on favoritism. We find that political favoritism toward Jewish (versus Arab) and affluent (versus less affluent) municipalities diminished significantly after the reform but has not yet disappeared along the ethnic dimension. Our results suggest that adopting rules-based allocation might be effective in coping with political favoritism.

# 1 Introduction

This paper asks whether rules-based allocation is an effective tool for taming political favoritism that is associated with biased and inefficient allocations of limited resources. A formula is an attractive policy tool for limiting favoritism because it might be more acceptable in the political arena than delegation of resource allocations to an external and independent agency.[1] Formula-based allocation may seem promising in combating political favoritism, but it nevertheless could fail to restrain decision-makers because of ways available to manipulate the variables and weights contained in a formula to unduly favor certain types of players (Banful 2011). It is an open empirical question whether a formula-based policy intervention is superior to other tools.

To examine the effectiveness of rules in restraining political favoritism, we exploit a policy reform that replaced discretion-based allocation with rules-based allocation of state lottery funds to localities in Israel. Political favoritism is measured herein by comparing the likelihood of receiving grants by two types of localities (e.g., disadvantaged localities versus more affluent ones or Arab versus Jewish localities) before and after the shift from rules-based to discretion-based allocation. Our results show that under a discretionary regime, the state lottery board tended to prefer economically stronger and Jewish localities, but that political favoritism along the ethnic dimension has not yet disappeared. Less populous localities seem to have been favored beforehand, but being a geographically remote locality did not appear to affect the probability of receiving a lottery-funded grant.

The present study contributes to the literature on political favoritism by relying on a "natural experiment" setting created by a shift to rules-based policy. Our approach offers a complementary empirical strategy that might be taken advantage of under certain conditions to identify the extent of political favoritism and complementing the standard strategy that rests on measuring the political and social connections between the granting organization and its grantees. First, our approach can be adopted even when no exogenous variation exists in the political or social affiliations of the collective decision-making body. Our study illustrates that political favoritism toward a certain social group (e.g., Jewish localities) might be detected despite the stable time-wise religious composition of the board of directors (one Arab member in most years). Second, our suggested identification strategy allows for multiple political interests and competing interests such as religious or social group affiliations to affect overall political favoritism. However, under our identification approach, the emphasis shifts from process to outcome, leaving the sources of political favoritism hidden and more difficult to interpret, explaining why we label it revealed political favoritism.

Many empirical studies show that the electoral motivations of politicians at the central

[1] Khemani (2007) found that delegation of fiscal policy to an independent agency in India is an effective tool for constraining partisan bias; co-partisan states are less likely to receive transfers of national resources allocated by an independent agency that substitutes for allocations determined by central political bodies.

level may explain why more public funds are allocated to certain localities with mayors of the same political party (political alignment) to increase the chances of winning future elections (electoral motivation). To name only a few, Migueis (2013) found that politically aligned localities in Portugal (1992–2005) received more transfers than unaligned localities. Solé-Ollé & Sorribas-Navarro (2008) found that aligned localities in Spain (1993–2003) received extra transfers from central and regional levels of government. Political alignment in the central-local government context, and corresponding favoritism, likewise have been observed in developed countries such as the United States (Albouy 2013, Berry et al. 2010), Italy (Bracco et al. 2015), Canada (Milligan & Smart 2005), France (Fabre & Sangnier 2017), Norway (Fiva & Halse 2016), Belgium (Jennes & Persyn 2015) and Israel (Rozevitch & Weiss 1993, Alperovich 1984) and developing and emerging economies such as Chile (Toro et al. 2019), Indonesia (Gonschorek et al. 2018), Brazil (Brollo & Nannicini 2012), and India (Pande 2003).

Employing the traditional identification strategy, studies published in recent years have expanded the political favoritism literature and highlighted that politicians might allocate public funds according to identity affiliations, even without serving direct or indirect electoral interests. Hodler & Raschky (2014) found that the birth region of the country's political leader is significantly favored during his term, based on a large cross-country setup (126 countries, 1992–2009). Social identity also has been shown to play important roles in more developed and democratic countries. Carozzi & Repetto (2016) found that Italian members of parliament (1994–2006) tend to favor their birth towns when those towns are located outside their electoral districts. In a similar manner, Fabre & Sangnier (2017) studied the behavior of French ministers with respect to discretionary infrastructure grants (2000–2013), finding that political interests dominate personal ones. Testing for both birth-town bias and for favoritism toward municipalities in which the ministers held office, Fabre & Sangnier found the effect of latter to be dramatic (+ 45% in funds) but not the former. More recently, (Asatryan & Havlik 2020), for a large all-European setup (1959–2016), found that national representatives to the European Investment Bank utilize their positions to favor specific regions more than other regions in their own countries.[2]

The study at hand also relates to the literature on the effects of rules-based policies on socioeconomic outcomes. Such rules, which evolved over the years, are intended to restrain discretion that may lead to time-inconsistency problems (the seminal contributions include

[2]Identity affiliation might be more important than political affiliation in countries for which the control and presence of parties at the local level are weak. That is why in countries like Spain (Solé-Ollé & Sorribas-Navarro 2008) and Portugal (Migueis 2013), characterized by strong party discipline on the part of local players, identity affiliations might be somewhat restrained relative to countries wherein the grip of parties at the local level is weaker, as in the cases of Israel (Kenig & Tuttnauer 2015) and Italy (Gamalerio 2020). Interestingly, for autocratic regimes with no effective party divisions and, hence, political (party) affiliations that are weaker, identity in the broader sense should again be important, as in the case of Vietnam (Do et al. 2017).

Barro & Gordon (1983), Kydland & Prescott (1977)), long-run neglect (e.g., Von Hagen & Harden 1996) and tilted-political playing fields (e.g., Alesina & Tabellini 1990). The actual design of such rules has proven to be difficult, especially in the realm of fiscal policy (Debrun & Jonung 2019); rules have often been accompanied by transparency requirements and independent institutions such as a central bank (delegation institution), a fiscal council (oversight institution) or an ad-hoc public committee responsible for crafting the rule or formula itself.[3] In our context, the main goal of rules-based policy is somewhat different and aims at curbing political favoritism (an equalization grant formula is another example). To focus attention on political favoritism, we abstract here from discussing the merits and limits of rules versus discretion.

The rest of the article is structured as follows. Section 2 presents the institutional background, focusing on the institutional environment that paved the way for the policy change discussed herein. Section 3 presents the data and some descriptive statistics, and Sect. 4 summarizes the methodological strategy. Section 5 presents the results, and Sect. 6 offers a short discussion of the findings and conclusions.

# 2 Institutional background

Israel's state-run lottery (Pais) was established in 1951 as a way of financing municipal infrastructure in the city of Tel Aviv. It has since expanded dramatically and now is a nationwide authorized gambling platform activating through nearly 2500 selling points. Over the years, the legal status and the public image of the organization became controversial. In 2016, the total revenues of the Pais reached 6.7 billion shekels (1.9 billion USD), larger than the total annual budget of the welfare ministry in the same year (6.4 billion shekels). In the same year, 4.3 billion shekels of the revenues was paid in the form of prizes to gamblers. Of the remaining sum, 1.3 billion shekels was allocated in the form of grants. The grants were earmarked for financing the construction of schools and kindergartens (0.82 billion), physical infrastructure for municipal sport and cultural activities (0.82), and an additional 0.14 billion for other purposes, including scholarships and prizes in the fields of art, literature, academia, and the like. Most of the grants expire after a few years if not spent. The grants to localities in the final years studied in the present research project are similar in magnitude to the annual government expenditures on culture (see Israel Central Bureau of Statistics [ICBS], 2018).

Since 2002, the allocation of the Pais grants has been based on a transparent formula, which replaced the previous discretion-based policy that was overseen by its board of directors. The adoption of the rule followed the recommendations of a public committee that was

[3] See Gootjes et al. (2021) for an extensive review of that literature.

established following public and state comptrollers' criticism highlighting suspected biased grant allocations by the Pais. That policy change was part of a more general trend of Israeli authorities at that time toward rules-based policy in several areas, such as a deficit-reduction rule in 1992, an expenditure rule in 2004 (Ben-Bassat & Dahan, 2006), and a new "equalization grants" formula also enacted in 2004 for funds allocated by the central government to localities in response to fiscal instability (Ben-Bassat et al. 2016).

Promoting equality and impartiality was the main motivation behind the shift to rules-based policy.[4] The new formula for allocating Pais grants to municipalities contains four components, each receiving a different weight: the number of residents in a municipality (50%); municipality rank on the socioeconomic index (SEI) (25%) and municipality rank on the Peripheral Index (20%),[5] both on a 1–10 scale; and the share of young cohorts serving in the army (or in civil service) in the municipality (5%).[6] Thus, the formula is constructed to assign priority to economically disadvantaged localities and to localities on Israel's geographical peripheries.

As mentioned above, according to the old regime, grant allocations were determined by the Pais board, which comprises 8–13 members. Two to three members represent the central government; 4–6 are acting mayors, serving for an average period of 5 years; and another three are mayors or treasurers of Israel's three largest cities (Jerusalem, Tel Aviv, and Haifa). In 2013, a public representative joined the board. Overall, local governments constitute a significant board majority, although the chairman of the board is appointed directly by the prime minister. Of the local representatives, four are ex-officio members, and the rest fill rotating seats. The rotating members of the board are appointed jointly by (i) the minister of the interior and the minister of finance; (ii) the head of the regional councils' organization; (iii) the mayors of the three largest cities; and (iv) the head of the municipalities' union (also a mayor). The last four are permanent (ex-officio) members of the board. Such a board of directors represents multiple and conflicting political interests and identities, which undermines the standard identification strategy for uncovering the overall extent of political favoritism.

The policy reform does not reflect a transition from no to full transparency, because the decisions were taken in the old regime by a board of directors representing different types of actors. Yet, the political players still may trade political favors. A shift to a formula-

[4]Interestingly, Dahan & Strawczynski (2013) showed that fiscal rules may improve fiscal discipline but at the cost of wider economic disparities.

[5]The Peripheral Index is composed of the distance from Tel Aviv and an accessibility measure available for the years 2004 and 2015 (the methodology for computing the index was modified slightly in 2015). The socioeconomic index (SEI) contains 14 sub-components and is recalculated every 3 years. For specific purposes, when stated, the 2008 SEI value was relied on herein. Otherwise, the closest available SEI value was used.

[6]For a few years, the formula also included an additional component: the monetary value of existing physical infrastructure in the municipality (weighted at 10%). That component later was dropped, and the 10% was added to the Peripheral Index component, which until then was assigned a weight of 10%.

based allocation might also be seen as a way of enhancing transparency, because it provides external players such as state comptrollers and non-governmental organizations opportunities to compare the actual and prescribed grants. Political favoritism may remain if the formula is engineered to favor certain actors.

# 3 Data

Data on grants by municipality for 26 years (1990–2015) were provided by the Pais (Table 1).[7] These data are complemented by geographical and other sociodemographic information on the 256 municipalities from the Israel Central Bureau of Statistics (ICBS). The first 6 years of the sample, show a seemingly sharp expansion both in total resources allocated and in the number of localities involved (see Fig. 1), but expansion might reflect partially improved data collection because the apparent increase in grants exceeded the actual growth in Pais revenues. That is why the main empirical analysis is based on the period starting in 1996. For robustness checks, we also estimate the extent of political favoritism for the full period (starting in 1990). Note that the average grant per capita is quite stable over the full period when only conditional grants are counted (Fig. 1). The average positive grant per capita in the second period declines by about 14% relative to the first period. That change is expected in light of the universal formula in the second period that enabled a larger number of localities to qualify for grants.

The data on which we rely reflect the grants that were transformed into actual projects rather than just dispersed grants. In the empirical analysis, we assume that the gap between actual and allocated grants is not correlated with the characteristics of localities. Nevertheless, the empirical analysis addresses a potential association along ethnic lines between the two regimes.

To give a general impression of the allocation of funding before and after the policy change, we report some descriptive statistics. The distribution of the total sum of grants before and after the change is depicted graphically in Figure 2, providing a basic indication of the rule's impact. Figure 1 shows that the ratio of the probability of being a grant-receiving poor locality (belonging to the lowest 1–3 socioeconomic deciles) to the probability of grant funding for all other localities (upper 4–10 deciles) rises from 63% in the first period to close to 90% in the second period. The unconditional probability of receiving a grant increased after the move to a rules-based regime for the bottom two socioeconomic deciles and declines for all other deciles. The reduction for the upper 8 deciles is larger the higher the socioeconomic decile

[7] A sharp reduction in grants materialized in 2011. We contacted the Pais, but no explanation was provided for that data point. As can be seen in Figures 4 and 5, a parallel reduction took place that year in grants for all social groups covered by our study. Unsurprisingly, the results are similar after excluding 2011 (not reported here).

is, except for the top decile (Table 2). The sharp decline in political favoritism toward more affluent municipalities also is observed in terms of grants per capita; the ratio of grants per capita in municipalities in the bottom 1–3 to the upper 4–10 socioeconomic deciles increases from 1.1 to 1.6 following the policy change. Those results, however, might be driven by other factors, such as population size or ethnic composition, which will be addressed in the empirical analysis.

Figure 2 shows a fall in political favoritism towards localities with Jewish majorities following the policy change. The ratio of the probability of receiving a grant by localities with Arab majorities to that of Jewish localities increased from 46 to 65%. Note that Arab municipalities are less likely to receive grants even after the policy change. However, conditional on receiving a grant, the grant per capita in Arab municipalities relative to Jewish municipalities is only slightly larger under the rules-based regime than under the old regime. Note that Arab municipalities receive larger grants per capita than Jewish municipalities, but that difference seems to reflect economies of scale in grants. Table 2 (panel B) shows a clear pattern of scale economics: per capita grants decline as the population of a municipality rises. Thus, the difference in grants per capita between the two groups does not account for the number of residents, which is significantly smaller in Arab municipalities than in Jewish ones. Again, the empirical analysis will consider other factors that may be responsible for the observed differences: Arab localities, for example, are significantly poorer and less populous than Jewish localities.

Table 2 does not reveal a noticeable change in the likelihood of receiving grants after the policy change based on the other two municipality characteristics: peripheral locations and population sizes (smaller municipalities gain somewhat less).

# 4 Methodology

To uncover the extent of political favoritism, we estimate the change in the probability of receiving state lottery grants by two types of localities (distinguished by SEI, ethnicity, population size, or geographic location) in the rules-based period relative to the discretionary period. We rely on the same difference-in-difference identification technique to estimate political favoritism, replacing the probability of receiving state lottery grants by grants per capita conditional on receiving a grant. The following statistical model is estimated:

$$Received_{ct} = \beta_0 + \beta_1 X_{ct} + \beta_2 Post + \beta_3 MG_{ct} + \beta_4 \left(Post \times MG_{ct}\right) + \eta_c + \varepsilon_{ct}$$

where $Received_{ct}$ (the outcome of interest) is a binary variable that equals 1 when locality $c$ receives a grant at time $t$; $X_{ct}$ is a vector of time-varying control variables;[8] and $\eta_c$ denotes

[8] We control for population and for the other factors contained in the grant formula. Specifically, when

municipality fixed effects. The explanatory variable Post is a binary variable assigned a value of 1 for the second period (i.e., after the adoption of the formula). We define the post-treated period in our analysis as the years 2005–2015, starting 3 years after the actual shift to rules-based allocation, to account for the time lag between grant authorization (unobserved) and project implementation, which is documented in the Pais dataset. The baseline analysis assumes a 3-year gap because it takes more than 2 years (on average) to finish a building in Israel.

The variable $MG$ ("municipalities group") receives a value of 1 if the municipality belongs to a specific group, where $MG_{ct} \in \{\text{Arab, SEI, Peripheral, PopSize}\}$ . In each case, we identify a particular group of municipalities by a binary variable. In one case, we compare Arab to Jewish municipalities (Arab = 1), and in the other three cases, we compare municipalities that belong to the bottom three deciles (the omitted group contains the top seven deciles) in terms of three different characteristics (socioeconomic rank, Peripheral Index, and population size). In the online appendix we test different groupings (bottom two, bottom four, and so on) to examine the sensitivity of our results. The coefficient $\beta_4$ on the interaction term (Post $\times$ MGct) is the focus of our analysis: it captures the effect of moving between allocation regimes for a specific group of municipalities. We use a linear probability model to estimate the effect of adopting a rules-based formula on any-grant outcome and a log-linear model for estimating the effect on grant per capita.

Given that achieving impartiality was the main intention behind the shift to a rulesbased allocation policy, we consider the four social and economic characteristics of localities in estimating the extent of political favoritism. Attitudes toward each of the four groups may represent either political interests or identity affiliations, or some of both. The first characteristic explored to uncover political favoritism is the socioeconomic rank of a municipality. More affluent municipalities are expected to benefit more under discretionbased policy because they wield more political leverage and are better able to reward grant allocators. Note that the residents of Israel's economically weaker localities tend to gamble more than their more prosperous counterparts do. The socioeconomic profile of gamblers is the same worldwide. As shown by Dahan (2020), low-income areas are disproportionately responsible for generating gambling revenues. Moreover, Dahan (2021) present new evidence that document a spike in gambling revenues on social security paydays and, in particular, on income-support payday. Thus, the channeling of more state lottery funds to more affluent municipalities implies double regressivity of the lottery "tax" as has been suggested by Clotfelter & Cook (1990).

The ethnicity (Jewish versus Arab) of the majority of municipal residents is our second characteristic. Jewish versus Arab municipalities is a natural candidate for identity-based

the main independent variable examined is low socioeconomic decile, we also control for the value of the Peripheral Index and for the locality's ethnic majority.

political favoritism in light of the tensions between those two ethnic groups in Israel.[9] The third characteristic is population size. One could suggest that more populous municipalities may receive more resources owing to their larger shares in the national popular vote and better bureaucratic capacity. As mentioned in the institutional background section, the largest cities are overrepresented on the Pais board of directors, which might be leveraged to channel more grants to populous municipalities under the discretion-based regime. Last, the discretion-based regime might be exploited to favor municipalities at the center of the country versus remote municipalities owing to the former's geographic proximity and closer connections to the central government. Three of the four characteristics considered (socioeconomic status, peripheral location, and population size) enter explicitly into the formula adopted.

As described earlier, the formula states that grant amounts should be proportional to population size as well as inversely related to socioeconomic ranking and the Periphery Index. Thus, a zero coefficient on the interaction term $\beta_4$ implies unchanged political favoritism, meaning that the new regime merely reflects a formulation of the earlier discretion-based allocation policy. For example, the representatives of the large cities may shape the allocation formula to maintain their past grant shares. In contrast, a positive (negative) coefficient suggests that the previous allocation policy favored (disfavored) the better-off localities.

# 5 Results

We first consider the standard identification strategy by estimating the effect of the policy change on localities whose mayors had been a member of the Pais board of directors throughout the period of study. We specifically construct that binary variable to identify localities whose mayors have ever been on the board, not necessarily around the years in which the locality received grants. Thus, we also account for the possibility of mayors trading favors over time. We find no significant effect on the probability of grant-receiving (Table 4: panel A) or on the level of grants per capita for that group of localities (Table 4: panel B). The standard identification strategy might not detect political favoritism, despite its existence, owing to complex trading of favors between members of the board representing multiple political interests and social affiliations; the results in Table 3 illustrate the limitations of the usual identification strategy.

Against that background, we now turn to our approach, which estimates political favoritism by comparing the funds granted before and after the policy change to groups of localities based on their characteristics, regardless of the personal characteristics of the decision-makers. We start by comparing the trends in actual grants over the 1996–2015

[9] A municipality with 50% or more of Arab residents is defined here as an Arab municipality. Except for a few localities with small to medium Arab minorities, almost all Israeli localities are either predominantly Jewish or predominantly Arab.

period for two main groups: socioeconomic status and ethnicity. As can be seen in Fig. 4, the likelihood of receiving any grant by localities in the bottom three SEI deciles is smaller than that of localities in the top seven SEI deciles, but both groups follow parallel trends before the policy reform (Table A14 in the online appendix). In contrast, the probability of receiving any grant after the policy change follows different patterns for those two groups. Note that the change in the likelihood of receiving grants occurs right after the policy reform. Parallel trends likewise are observed in the probabilities of grant receipt in Jewish and Arab localities before the shift from discretion-based allocation to rules-based allocation, but an abrupt change in the likelihood of receiving grants is observed immediately after the policy reform (Fig. 5). We now turn to the estimation results.

Estimates of the probability of economically weak localities (low SEI) receiving a grant imply that disfavoring that group in the first period ranges from 11 to 15% (Table 4). The same finding emerges in all five models. Models 1–5 are linear probability models (LPMs), and models 3 and 5 also include fixed effects for municipalities.[10] The results for grants per capita also are positive and significant (panel B of Table 4), suggesting a reduction in political favoritism toward more affluent municipalities following the policy change.

Table 5 shows that the coefficient of the Arab dummy is negative, implying that the likelihood of Arab municipalities receiving a grant is lower in the first period. The fall in political favoritism resulting from the shift to rules-based allocation also is observed when examining ethnicity (after controlling for the socioeconomic status, population size, and geographic remoteness). The positive and significant coefficient on the interaction between Arab and Post suggests that Arab municipalities have benefitted relatively from the policy change by 7% to 10% (Table 5: panel A). The ethnic dividend of the move to rules-based allocation is important given the fact that the ethnic origins of municipality residents were not included directly in the new formula. We do find a positive effect on grants per capita but it is not significant (Table 5: panel B). It could be that the gap in the probability of receiving a grant favoring Jewish localities found here might be less detectable and therefore more prone to political favoritism than a gap in grants per capita in receiving localities. In contrast, differences in grants per capita are more visible, are easily calculated, and therefore are less suitable candidates for directing funds to favored groups.

The positive and statistically significant coefficient on the interaction term suggests that political favoritism has been reduced after the reform, but one could not infer from the results in Table 5 whether it is still present under the rules-based regime. Figure 6 shows the probability of receiving a grant before and after introduction of the rules-based regime for low and high SEIs (left panel) and Jewish and Arab municipalities (right panel). Following the reform, the predicted probability of municipalities with low socioeconomic indices overlaps

[10]Models 2–3 include controls in the form of binary variables only (variables that equal one for the upper half of the distribution), and models 4–5 contain continuous control variables.

that of municipalities with high socioeconomic indices, implying the disappearance of political favoritism along the socioeconomic dimension. In contrast, the chances of Jewish versus Arab municipalities receiving grants remain higher because the confidence intervals do not overlap during the rules-based period. Our results suggest that political favoritism has been reduced by the reform, but has not disappeared along the ethnic dimension.

The likelihood of receiving a grant is not affected by geographic remoteness, as implied by the estimated coefficients (Table 6). Peripheral localities are not favored or disfavored after the policy change relative to the earlier period in terms of the probability of receiving grants. No significant effect is found when grants per capita are examined rather than the likelihood of receiving any grant. The policy change also seems to have had no effect on the extent of political favoritism toward large municipalities (relative to small ones) in terms of both the probability of grant receipt or grants per capita conditional on receiving a grant (Table 7).

We run a series of robustness tests which includes replacing LPM with the probit model (Table A8 in the online appendix), various time windows between the decision to allocate a grant until the actual project took place (Table A9 in the online appendix), a continuous socioeconomic variable instead of a binary indicator to allow for different impact conditional on socioeconomic levels (Table A10 and Fig. A7 in the online appendix), and several different lengths of the investigated period (Tables A11–A13 in the online appendix). These robustness tests provide results that are consistent with our main findings.

# 6 Summary & conclusions

This paper examines political favoritism based on a policy shift from discretion-based allocation to rules-based allocation of state lottery funds in Israel. We find that Arab localities have relatively benefited from this policy reform, which means that the old regime was associated with a higher degree of political favoritism toward Jewish localities. Our results suggest that political favoritism along the ethnic dimension has been reduced since the reform but has not disappeared, as Jewish municipalities are still more likely to receive a grant compared to Arab municipalities. We also uncover that discretion-based allocation disfavors economically weak and favors less populated localities relative to rules-based allocation. Being located in the geographical periphery seems to be unrelated to higher chances of being favored or disfavored in terms of the probability of receiving a grant. Our results are valid as long as the utilization rates of Arab localities (or small, peripheral, etc.) have not increased relative to other localities between the two regimes.

One may assert that the differences between allocations before and after the policy reform may reflect differences in preferences between the Pais board and the public committee that recommended the formula, but we believe that it is nevertheless in line with our interpretation that these differences represent political favoritism. As mentioned above, the initiation of a respected public committee headed by the former finance minister reflected public discontent with the existing allocation of state lottery funds by the Pais board. It would be plausible to suggest that public dissatisfaction stems from perceived unfair allocation, and this is why designing the formula was delegated to an external and independent committee. The public committee could have constructed a formula that produced a similar allocation as in the old regime but it did not.

In this paper, we deviate from the standard identification strategy to uncover political favoritism. Our complementary approach to identify the extent of political favoritism estimates the gap in public funds granted to two groups under two different regimes rather than two different politicians. While the standard identification strategy is adequate for detecting a particular mechanism through which political favoritism is manifested, our approach may reveal the overall extent of political favoritism, which is influenced by multiple political interests and social affiliations of decision-makers in public bodies. In addition, our identification approach is suitable for assessing political favoritism even in the case of small or no exogenous variation in personal characteristics of politicians, as we compare two different grant-allocation regimes rather than two types of politicians. This allows us to estimate the overall or revealed political favoritism, but at the cost of keeping vague the underlying mechanism behind favoritism. Thus, the outcome rather than the process is at the center of this empirical strategy.

We cannot distinguish whether the effect of the reform is along the ethnic or socioeconomic dimension because of the correlation between ethnicity and socioeconomic status. Unfortunately, almost all Arab localities are at the bottom five SEI clusters (only one Arab municipality in SEI 6 and none in SEI 7–10), and therefore dividing the Arab municipalities into rich and poor is not a viable option to isolate the effect of ethnicity. However, we do find that Arabs gained from the reform regardless of whether it was due to their ethnicity or socioeconomic background.

The findings of our research suggest that the adoption of rules-based allocation is an effective policy for enhancing equity. This is even more important in view of the relative position of Arab citizens and the heated tension between Jews and Arabs in Israel. We speculate that delegating the task of shaping the formula to an external body such as an independent public committee, as has been done in our case, might be a way to increase the effectiveness of rules-based allocation as a tool to fix favoritism. Future research may explore the political and institutional conditions under which the adoption of rules is more effective in restraining political favoritism.

Table 1: Descriptive statistics (1990–2015)

| Variable | N | Mean | SD | min | max |
|---|---|---|---|---|---|
| Received any grant | 6455 | 0.3 | 0.46 | 0 | 1 |
| Grant ('000 shekels) | 6455 | 1,700 | 5,000 | 0 | 74,000 |
| Grant per capita | 6455 | 90.83 | 312.3 | 0 | 8,016 |
| Positive grant per capita | 1969 | 297.8 | 508.2 | 1 | 8,016 |
| Number of buildings (projects) | 6455 | 0.5 | 1.1 | 0 | 25 |
| Mayor ever on board* | 6455 | 0.087 | 0.28 | 0 | 1 |
| An Arab municipality | 6455 | 0.32 | 0.47 | 0 | 1 |
| Socioeconomic index | 6413 | -0.02 | 0.96 | -3.08 | 3.5 |
| Socioeconomic index 2008 | 6455 | 0.01 | 0.99 | -3.08 | 3.5 |
| Peripheral Index | 6455 | -0.0067 | 0.92 | -3 | 2.73 |
| Population ('000) | 6453 | 26.5 | 60.5 | 0.2 | 849.8 |

* Excluding mayors of the three big cities that are constantly members of the board. **The conventional socioeconomic index is updated every 3 years. The "socioeconomic index-2008" uses constant single year index for the full sample.

Figure 1: Annual state lottery grants, 1990–2015

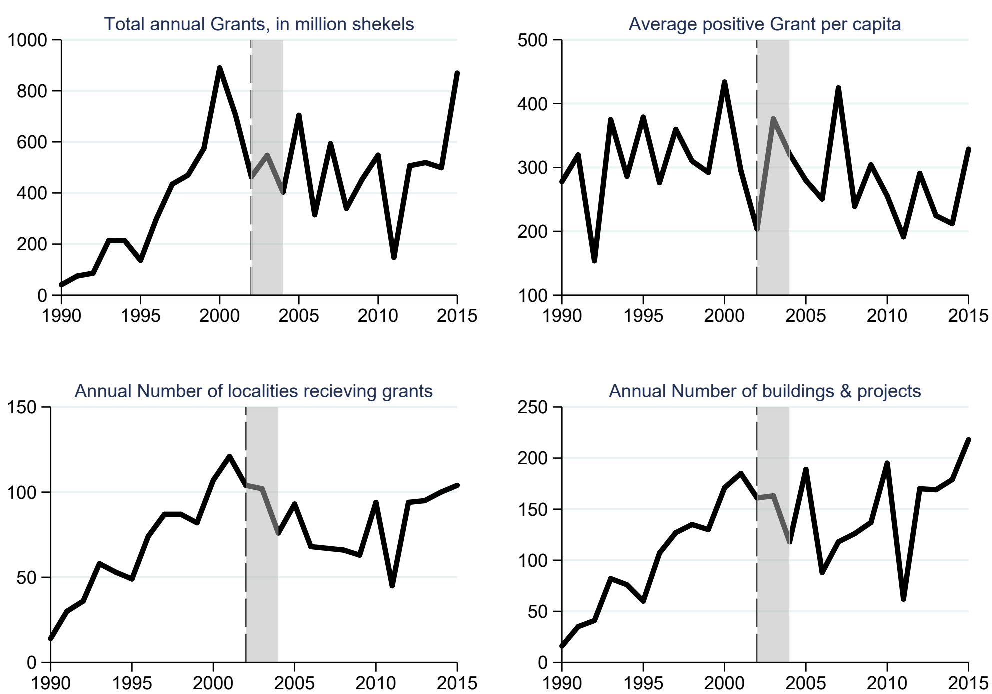


Figure 2: Unconditional ratio of the bottom three deciles in the socioeconomic index to deciles 4–10; and Arab to Jewish localities, 1996– 2001 vs. 2005–2015

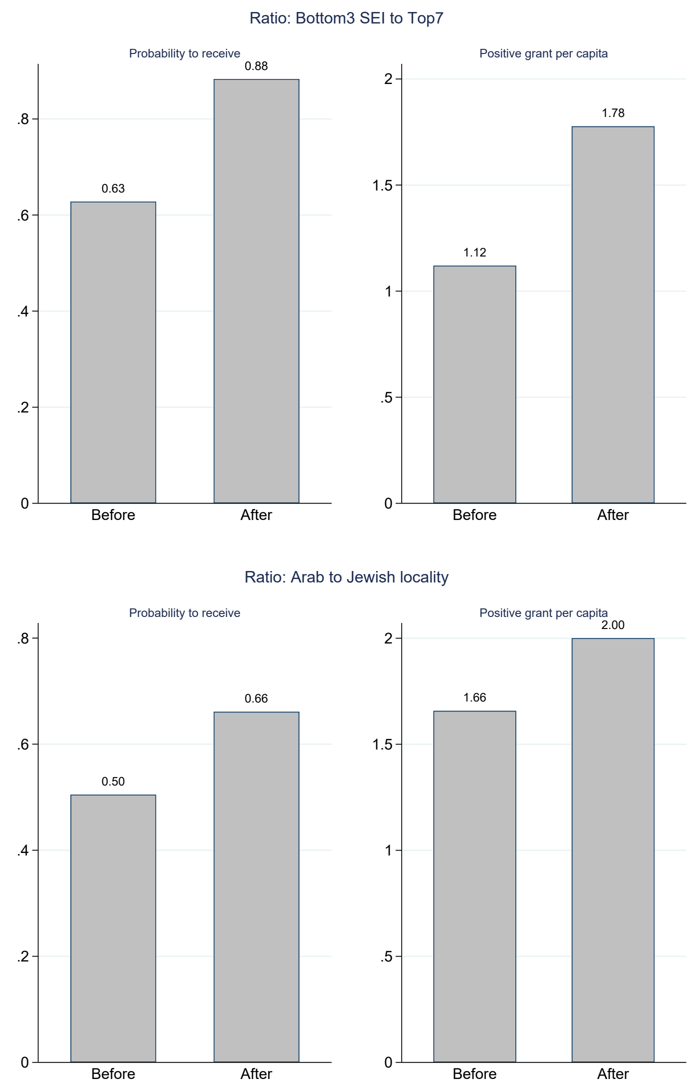



Figure 3: Left empty – to preserve consistent numbering with journal

Table 2: The change in outcomes between periods (1996–2001 vs. 2005–2015), by socioeconomic rank, geographical location and population size

*A. Change in the share of localities that received any grant*

| Decile | SEI | | | Peripheral | | | Population Size | | |
|---|---|---|---|---|---|---|---|---|---|
| | Before | After | *Δ* | Before | After | *Δ* | Before | After | *Δ* |
| 1 | 0.24 | 0.36 | 0.13 | 0.24 | 0.2 | -0.03 | 0.12 | 0.1 | -0.02 |
| 2 | 0.2 | 0.21 | 0.01 | 0.32 | 0.21 | -0.11 | 0.21 | 0.17 | -0.04 |
| 3 | 0.33 | 0.31 | -0.02 | 0.29 | 0.32 | 0.03 | 0.19 | 0.19 | 0.00 |
| 4 | 0.43 | 0.36 | -0.07 | 0.3 | 0.28 | -0.02 | 0.21 | 0.23 | 0.03 |
| 5 | 0.44 | 0.39 | -0.04 | 0.35 | 0.24 | -0.11 | 0.26 | 0.23 | -0.03 |
| 6 | 0.47 | 0.38 | -0.09 | 0.41 | 0.37 | -0.04 | 0.38 | 0.31 | -0.07 |
| 7 | 0.51 | 0.32 | -0.19 | 0.47 | 0.41 | -0.06 | 0.41 | 0.36 | -0.05 |
| 8 | 0.44 | 0.33 | -0.11 | 0.33 | 0.38 | 0.05 | 0.55 | 0.44 | -0.11 |
| 9 | 0.44 | 0.31 | -0.13 | 0.48 | 0.39 | -0.09 | 0.55 | 0.54 | -0.02 |
| 10 | 0.25 | 0.23 | -0.03 | 0.59 | 0.43 | -0.16 | 0.82 | 0.62 | -0.20 |

*B. Change in grant per capita*

| | SEI | | | Peripheral | | | Population Size | | |
|---|---|---|---|---|---|---|---|---|---|
| | Before | After | *Δ* | Before | After | *Δ* | Before | After | *Δ* |
| 1 | 482 | 426 | -0.12 | 483 | 263 | -0.45 | 1,000 | 866 | -0.13 |
| 2 | 432 | 411 | -0.05 | 515 | 269 | -0.48 | 569 | 620 | 0.09 |
| 3 | 309 | 294 | -0.05 | 506 | 404 | -0.20 | 612 | 586 | -0.04 |
| 4 | 315 | 213 | -0.33 | 428 | 364 | -0.15 | 688 | 346 | -0.50 |
| 5 | 287 | 218 | -0.24 | 316 | 294 | -0.07 | 367 | 321 | -0.13 |
| 6 | 254 | 172 | -0.32 | 230 | 280 | 0.22 | 321 | 285 | -0.11 |
| 7 | 297 | 277 | -0.07 | 396 | 301 | -0.24 | 433 | 246 | -0.43 |
| 8 | 293 | 173 | -0.41 | 323 | 199 | -0.38 | 246 | 215 | -0.13 |
| 9 | 329 | 207 | -0.37 | 275 | 202 | -0.27 | 235 | 169 | -0.28 |
| 10 | 487 | 218 | -0.55 | 123 | 80 | -0.35 | 91 | 81 | -0.11 |

* The SEI value used here is for the year 2008; and population size is the average size over each of the periods.

Table 3: Mayors' characteristics and political favoritism, the standard identification strategy

Panel A: Dependent variable: probability of receiving a grant

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Post | -0.043*** | -0.084*** | -0.048*** | -0.091*** | -0.122*** |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) |
| Mayor ever in board | 0.217*** | 0.120** | | 0.068 | |
| | (0.08) | (0.06) | | (0.05) | |
| Post x | -0.100 | -0.069 | -0.096 | -0.093 | -0.092* |
| Mayor ever in board | (0.06) | (0.06) | (0.06) | (0.06) | (0.05) |
| Constant | 0.352*** | 0.213*** | 0.375*** | -1.159*** | -2.193*** |
| | (0.02) | (0.02) | (0.02) | (0.08) | (0.46) |
| N | 4296 | 4296 | 4296 | 4277 | 4277 |
| R-sq | 0.012 | 0.105 | 0.006 | 0.154 | 0.017 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

Panel B: Dependent variable: ln(Grant per capita)

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Post | -0.235*** | -0.116 | -0.302*** | -0.092 | -0.248*** |
| | (0.07) | (0.07) | (0.07) | (0.07) | (0.09) |
| Mayor ever in board | -0.500** | -0.103 | | 0.067 | |
| | (0.23) | (0.20) | | (0.12) | |
| Post x | 0.196 | 0.031 | 0.024 | 0.066 | 0.023 |
| Mayor ever in board | (0.24) | (0.23) | (0.21) | (0.22) | (0.22) |
| Constant | 5.073*** | 5.951*** | 4.939*** | 10.602*** | 6.983*** |
| | (0.08) | (0.09) | (0.14) | (0.35) | (2.25) |
| N | 1447 | 1447 | 1447 | 1445 | 1445 |
| R-sq | 0.015 | 0.145 | 0.018 | 0.294 | 0.019 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

*Note*: The variable MayorEverInBoard identifies localities whose mayor has been a member of the Pais board along the period of the study. Control variables include the locality's ethnicity, socioeconomic index, geographic location and population (excluding the one interacted with the Post variable). In columns 2–3 the control variables are binary (above and below the median) and in columns 4–5 they are continuous. * Indicates significance level of 10%, ** indicates significance level of 5%. *** Indicates significance level of 1%. Clustered standard errors are in the parentheses.

Figure 4: The probability of receiving any grant before and after the policy change—by SEI deciles

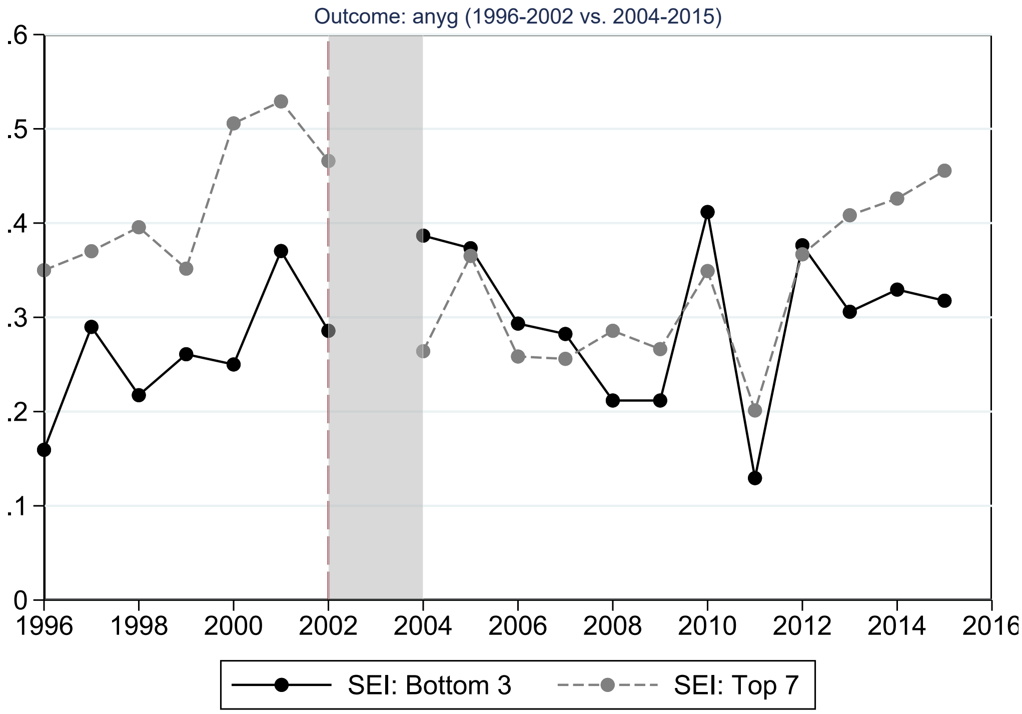



Figure 5: The probability of receiving any grant before and after the policy change—by city's ethnic majority

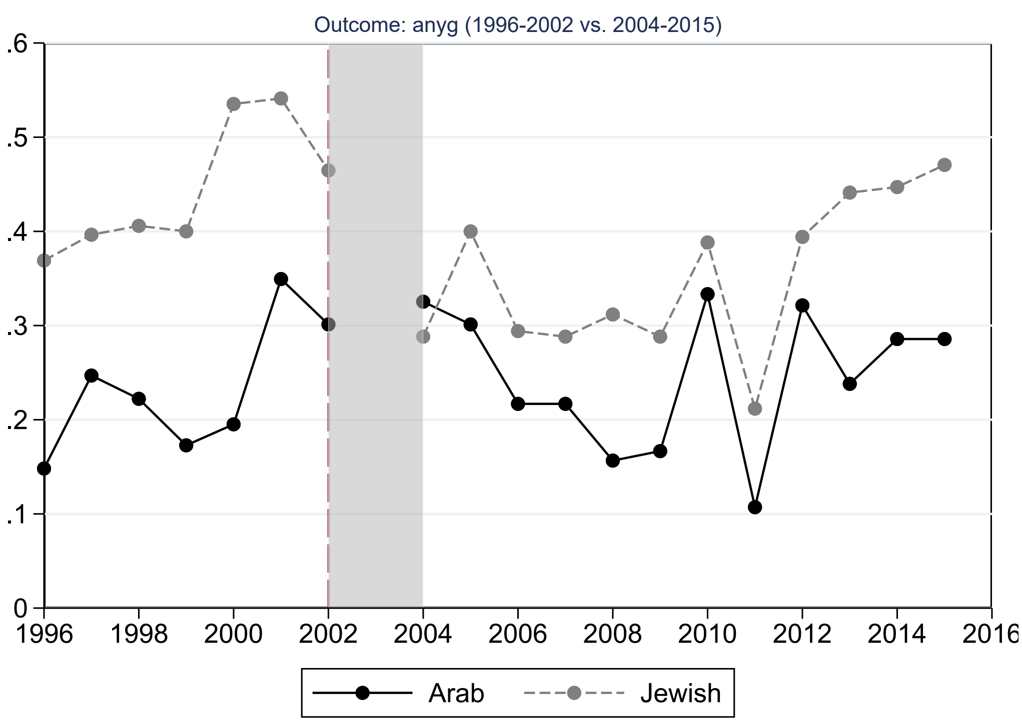


Table 4: The effect of the policy change on grants, by socioeconomic rank

Panel A: Dependent variable: probability of receiving a grant

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Post | -0.085*** | -0.128*** | -0.102*** | -0.138*** | -0.166*** |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) |
| Low SEI | -0.154*** | -0.020 | -0.122*** | -0.031 | -0.119*** |
| | (0.03) | (0.03) | (0.04) | (0.03) | (0.04) |
| Post x Low SEI | 0.118*** | 0.118*** | 0.154*** | 0.125*** | 0.133*** |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) |
| Constant | 0.415*** | 0.285*** | 0.392*** | -1.073*** | -1.931*** |
| | (0.02) | (0.02) | (0.02) | (0.07) | (0.40) |
| N | 4296 | 4296 | 4296 | 4295 | 4295 |
| R-sq | 0.011 | 0.113 | 0.010 | 0.164 | 0.020 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

Panel B: Dependent variable: ln(Grant per capita)

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Post | -0.407*** | -0.306*** | -0.368*** | -0.259*** | -0.310*** |
| | (0.08) | (0.07) | (0.06) | (0.07) | (0.08) |
| Low SEI | 0.301* | -0.167 | -0.404* | -0.144 | -0.410* |
| | (0.18) | (0.19) | (0.21) | (0.13) | (0.22) |
| Post x Low SEI | 0.575*** | 0.657*** | 0.423** | 0.618*** | 0.453*** |
| | (0.16) | (0.16) | (0.17) | (0.15) | (0.17) |
| Constant | 4.944*** | 5.702*** | 5.194*** | 10.317*** | 7.694*** |
| | (0.09) | (0.10) | (0.13) | (0.37) | (2.11) |
| N | 1447 | 1447 | 1447 | 1447 | 1447 |
| R-sq | 0.064 | 0.174 | 0.023 | 0.303 | 0.024 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

*Note*: Localities in the bottom 1–3 to upper 4–10 deciles, ranked by socioeconomic index, 1996–2001 versus 2005–2015. Post = 1 after the policy change. Control variables include the locality's ethnicity, socioeconomic index, geographic location, and population (excluding the one interacted with the Post variable). In columns 2–3, the control variables are binary (above and below the median) and in columns 4–5 they are continuous. * Indicates significance level of 10%, ** indicates significance level of 5%. *** Indicates significance level of 1%. Clustered standard errors are in the parentheses.

Table 5: The effect of the policy change on grants, by ethnicity

Panel A: Dependent variable: probability of receiving a grant

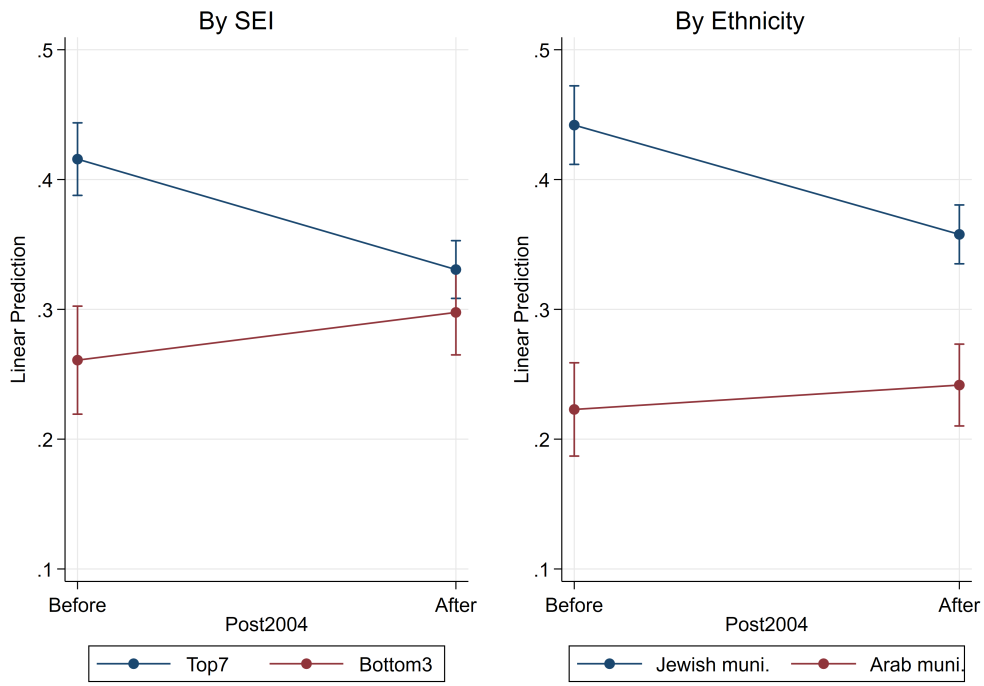
| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Post | -0.084*** | -0.107*** | -0.086*** | -0.120*** | -0.158*** |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) |
| Arab | -0.219*** | -0.200*** | | -0.211*** | |
| | (0.03) | (0.03) | | (0.03) | |
| Post x Arab | 0.100*** | 0.071** | 0.092*** | 0.080*** | 0.084*** |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) |
| Constant | 0.441*** | 0.344*** | 0.368*** | -0.969*** | -2.167*** |
| | (0.02) | (0.03) | (0.02) | (0.08) | (0.44) |
| N | 4296 | 4296 | 4296 | 4277 | 4277 |
| R-sq | 0.028 | 0.116 | 0.007 | 0.168 | 0.018 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

Panel B: Dependent variable: ln(Grant per capita)

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Post | -0.300*** | -0.210*** | -0.324*** | -0.139* | -0.278*** |
| | (0.08) | (0.08) | (0.07) | (0.08) | (0.09) |
| Arab | 0.687*** | 0.295** | | 0.049 | |
| | (0.14) | (0.14) | | (0.14) | |
| Post x Arab | 0.246* | 0.345** | 0.149 | 0.209 | 0.230 |
| | (0.15) | (0.16) | (0.16) | (0.14) | (0.16) |
| Constant | 4.871*** | 5.684*** | 4.946*** | 10.402*** | 7.413*** |
| | (0.09) | (0.13) | (0.13) | (0.38) | (2.37) |
| N | 1447 | 1447 | 1447 | 1445 | 1445 |
| R-sq | 0.076 | 0.163 | 0.019 | 0.296 | 0.020 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

*Note*: Arab relative to Jewish localities, 1996–2001 versus 2005–2015 See notes to Table 4. The Arab group dummy variable in columns (3) and (5) drops because it is time invariant and therefore captured by the fixed effects.

Figure 6: Predicted probability of receiving a grant, by ethnicity and socioeconomic index




*Note*: the predicted probabilities are computed based on Table 4 column 4 for the left panel and Table 5 column 4 for the right panel.

Table 6: The effect of the policy change on grants, by geographic location

Panel A: Dependent variable: probability of receiving a grant

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Post | -0.059*** | -0.091*** | -0.066*** | -0.102*** | -0.146*** |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) |
| Periphery | -0.129*** | -0.031 | | -0.012 | |
| | (0.03) | (0.03) | | (0.03) | |
| Post × Periphery | 0.024 | 0.018 | 0.023 | 0.029 | 0.042 |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) |
| Constant | 0.409*** | 0.364*** | 0.348*** | -0.962*** | -2.184*** |
| | (0.02) | (0.03) | (0.02) | (0.07) | (0.43) |
| N | 4296 | 4296 | 4296 | 4277 | 4295 |
| R-sq | 0.015 | 0.113 | 0.004 | 0.166 | 0.016 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

Panel B: Dependent variable: ln(Grant per capita)

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Post | -0.130 | -0.094 | -0.251*** | -0.025 | -0.205** |
| | (0.08) | (0.08) | (0.07) | (0.07) | (0.10) |
| Periphery | 0.692*** | 0.246** | | 0.055 | |
| | (0.15) | (0.12) | | (0.11) | |
| Post × Periphery | -0.328** | -0.203 | -0.184 | -0.278* | -0.187 |
| | (0.15) | (0.15) | (0.16) | (0.15) | (0.16) |
| Constant | 4.849*** | 5.452*** | 5.040*** | 10.637*** | 6.454*** |
| | (0.09) | (0.13) | (0.12) | (0.34) | (2.07) |
| N | 1447 | 1447 | 1447 | 1445 | 1447 |
| R-sq | 0.031 | 0.157 | 0.018 | 0.297 | 0.018 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

*Note*: Localities in the bottom 1–3 to upper 4–10 deciles, ranked by periphery index, 1996–2001 versus 2005– 2015 See notes to Table 4. The Periphery group dummy variable in columns (3) and (5) drops because it is timeinvariant and therefore captured by the fixed effects.

Table 7: The effect of the policy change on grants, by population size

Panel A: Dependent variable: probability of receiving a grant

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Post | -0.065*** | -0.061*** | -0.063*** | -0.061*** | -0.062*** |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) |
| Small pop | -0.288*** | -0.255*** | | -0.255*** | |
| | (0.03) | (0.03) | | (0.03) | |
| Post × Small pop | 0.049 | 0.052* | 0.046 | 0.044 | 0.043 |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) |
| Constant | 0.456*** | 0.507*** | 0.394*** | 0.521*** | 0.369*** |
| | (0.02) | (0.03) | (0.02) | (0.02) | (0.01) |
| N | 4296 | 4296 | 4296 | 4278 | 4278 |
| R-sq | 0.065 | 0.092 | 0.005 | 0.093 | 0.005 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

Panel B: Dependent variable: ln(Grant per capita)

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Post | -0.192*** | -0.212*** | -0.272*** | -0.218*** | -0.279*** |
| | (0.07) | (0.07) | (0.06) | (0.07) | (0.07) |
| Small pop | 1.286*** | 1.181*** | | 1.261*** | |
| | (0.14) | (0.13) | | (0.14) | |
| Post × Small pop | -0.156 | -0.230 | -0.271 | -0.252 | -0.213 |
| | (0.16) | (0.16) | (0.17) | (0.16) | (0.17) |
| Constant | 4.830*** | 4.930*** | 4.945*** | 4.737*** | 5.076*** |
| | (0.08) | (0.11) | (0.08) | (0.08) | (0.04) |
| N | 1447 | 1447 | 1447 | 1445 | 1445 |
| R-sq | 0.101 | 0.160 | 0.019 | 0.159 | 0.019 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

Note: Localities in the bottom 1–3 to upper 4–10 deciles, ranked by population size, 1996–2001 versus 2005– 2015 See notes to Table 4. The population group dummy variable in columns (3) and (5) drops because it is timeinvariant and therefore captured by the fixed effects.

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# For online Appendix

Table A8: Probit regression - Marginal effects

| Model | Variable | Coefficient | s.e. | Controls | N | Pseudo R2 |
|---|---|---|---|---|---|---|
| Col. 1 Table 4 | Post2004 | -0.081 | [0.018] | | | |
| | Post2004 x Bottom3 | 0.118 | [0.034] | No | 4,296 | 0.0088 |
| Col. 2 Table 4 | Post2004 | -0.134 | [0.018] | | | |
| | Post2004 x Bottom3 | 0.125 | [0.035] | Binary | 4,296 | 0.0926 |
| Col. 4 Table 4 | Post2004 | -0.153 | [0.019] | | | |
| | Post2004 x Bottom3 | 0.14 | [0.035] | Cont. | 4,295 | 0.134 |
| Col. 5 Table 4 | Post2004 | -0.209 | [0.027] | Cont. + | | |
| | Post2004 x Bottom3 | 0.156 | [0.04] | muni F.E | 4,210 | 0.192 |
| Col. 1 Table 5 | Post2004 | -0.079 | [0.018] | | | |
| | Post2004 x Arab | 0.098 | [0.033] | No | 4,296 | 0.0225 |
| Col. 2 Table 5 | Post2004 | -0.111 | [0.019] | | | |
| | Post2004 x Arab | 0.072 | [0.034] | Binary | 4,296 | 0.095 |
| Col. 4 Table 5 | Post2004 | -0.13 | [0.019] | | | |
| | Post2004 x Arab | 0.087 | [0.034] | Cont. | 4,277 | 0.139 |
| Col. 5 Table 5 | Post2004 | -0.2 | [0.028] | Cont. + | | |
| | Post2004 x Arab | 0.088 | [0.04] | muni F.E | 4,184 | 0.19 |

*Note*: Table 8 presents the marginal effects from a probit model. As can be seen, the estimated marginal effects are quite similar to those presented in Tables 4-5 using LPMs. The choice between LPM and index models for a binary dependent variable such as probit or logit involves both theoretical and practical considerations. The main concern with linear probability models is the potential bias and inconsistent estimates because the computed values do not necessarily lie between zero and one. But as Horrace and Oacaxa (2006) show, the risk of potential bias of LPM is relatively low if the relative share of predicted probabilities outside the unit interval is small or zero. In our case, very few cases (1%-3%) lie outside of the unit interval. Moreover, the correlation between LPM and Probit predicted probabilities is very high (0.91-0.99). In addition, a standard econometric textbook suggests that as long as the purpose is to estimate the partial impact "...the fact that some predicted values are outside the unit interval may not be very important." (Wooldridge, 2002:455). Theoretically, we do not know whether the true relationship is linear or non-linear. Selecting LPM as our estimation technique reflects the virtue of accessible presentation of our findings while paying a minor cost if any (probit or logit might be also biased as the true relationship is unknown).

Table A9: The effect of the policy change on the probability to get grants, by socioeconomic rank (panel A) and ethnicity (panel B), 1996-2001 versus 2002/2003/2004-2015

| | | | | | |
|---|---|---|---|---|---|
| | 1996-2001 versus **2002-2015**, by socioeconomic rank | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Post X SEI Bottom3 | 0.110*** | 0.105*** | 0.136*** | 0.114*** | 0.124*** |
| | [0.03] | [0.03] | [0.03] | [0.03] | [0.03] |
| Constant | 0.415*** | 0.282*** | 0.405*** | -1.020*** | -1.198*** |
| | [0.02] | [0.02] | [0.02] | [0.07] | [0.32] |
| N | 5055 | 5055 | 5055 | 5054 | 5054 |
| R-sq | 0.009 | 0.103 | 0.007 | 0.149 | 0.012 |
| | 1996-2001 versus **2002-2015**, by ethnicity | | | | |
| Post X Arab | 0.107*** | 0.082*** | 0.104*** | 0.091*** | 0.104*** |
| | [0.03] | [0.03] | [0.03] | [0.03] | [0.03] |
| Constant | 0.441*** | 0.342*** | 0.362*** | -0.916*** | -1.350*** |
| | [0.02] | [0.02] | [0.02] | [0.07] | [0.34] |
| N | 5055 | 5055 | 5055 | 5036 | 5036 |
| R-sq | 0.024 | 0.107 | 0.005 | 0.154 | 0.01 |
| | 1996-2001 versus **2003-2015**, by socioeconomic rank | | | | |
| Post X SEI Bottom3 | 0.121*** | 0.117*** | 0.149*** | 0.125*** | 0.135*** |
| | [0.03] | [0.03] | [0.03] | [0.03] | [0.03] |
| Constant | 0.415*** | 0.282*** | 0.402*** | -1.030*** | -1.450*** |
| | [0.02] | [0.02] | [0.02] | [0.07] | [0.33] |
| N | 4802 | 4802 | 4802 | 4801 | 4801 |
| R-sq | 0.01 | 0.106 | 0.009 | 0.153 | 0.015 |
| | 1996-2001 versus **2003-2015**, by ethnicity | | | | |
| Post X Arab | 0.111*** | 0.085*** | 0.105*** | 0.093*** | 0.103*** |
| | [0.03] | [0.03] | [0.03] | [0.03] | [0.03] |
| Constant | 0.441*** | 0.344*** | 0.361*** | -0.920*** | -1.639*** |
| | [0.02] | [0.02] | [0.02] | [0.07] | [0.36] |
| N | 4802 | 4802 | 4802 | 4783 | 4783 |
| R-sq | 0.025 | 0.11 | 0.006 | 0.158 | 0.013 |
| | 1996-2001 versus **2004-2015**, by socioeconomic rank | | | | |
| Post X SEI Bottom3 | 0.131*** | 0.128*** | 0.163*** | 0.136*** | 0.146*** |
| | [0.03] | [0.03] | [0.03] | [0.03] | [0.03] |
| Constant | 0.415*** | 0.285*** | 0.395*** | -1.058*** | -1.778*** |
| | [0.02] | [0.02] | [0.02] | [0.07] | -0.36 |
| N | 4549 | 4549 | 4549 | 4548 | 4548 |
| R-sq | 0.011 | 0.109 | 0.011 | 0.159 | 0.02 |
| | 1996-2001 versus **2004-2015**, by ethnicity | | | | |
| Post X Arab | 0.113*** | 0.085*** | 0.105*** | 0.094*** | 0.099*** |
| | [0.03] | [0.03] | [0.03] | [0.03] | [0.03] |
| Constant | 0.441*** | 0.349*** | 0.365*** | -0.948*** | -2.010*** |
| | [0.02] | [0.03] | [0.02] | [0.08] | [0.04] |
| N | 4549 | 4549 | 4549 | 4530 | 4530 |
| R-sq | 0.026 | 0.112 | 0.008 | 0.164 | 0.018 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

*Note*: Table A9 shows that our findings regarding the decline in political favoritism toward Jewish and more affluent municipalities are robust to shorter time windows, such as one year and two years. Given that the available data represent the time the grant was utilized, we previously assumed that the time window between the decision to allocate a grant until the actual project took place was three years, so that the discretion period ends in 2001 but the rules-based period starts only in 2005. Note that this finding rests on the idea that the capacity of a specific group of localities (e.g., Arab localities) to utilize grants received has not improved between the two periods relative to other localities, which is a plausible assumption. See notes to Table 4. For conciseness, only the effects of the interactions are presented.
A study by the Research & Information Center of the Israeli parliament (the Knesset) to examine a potential bias against Arab localities in allocating Pais grants found that grants in the years 2011-2015 (the post-reform period in our study) were proportional to their population shares but that Arab localities were characterized by lower rates of grant utilization.

Table A10: The effect of the policy change on grants, by SEI (continuous)

Panel A. dependent variable: any grant (=1)

| | (1) | (2) | (3) |
|---|---|---|---|
| Post | -0.052*** | -0.095*** | -0.130*** |
| | [0.02] | [0.01] | [0.02] |
| Socioeconomic index | 0.029 | -0.031** | -0.032 |
| | [0.02] | [0.02] | [0.04] |
| Post X SE index | -0.045*** | -0.044*** | -0.046*** |
| | [0.02] | [0.01] | [0.02] |
| Constant | 0.372*** | -0.975*** | -2.128*** |
| | [0.02] | [0.08] | [0.45] |
| N | 4278 | 4277 | 4277 |
| R-sq | 0.005 | 0.168 | 0.019 |
| Controls | No | Continuous | Continuous |
| Muni. FE | No | No | Yes |

Panel B. dependent variable: ln(grant per capita)

| | | | |
|---|---|---|---|
| Post | -0.233*** | -0.094 | -0.217** |
| | [0.07] | [0.06] | [0.09] |
| Socioeconomic index | -0.217** | 0.043 | 0.448*** |
| | [0.09] | [0.07] | [0.17] |
| Post X SE index | -0.155* | -0.242*** | -0.204*** |
| | [0.08] | [0.07] | [0.07] |
| Constant | 5.011*** | 10.397*** | 7.826*** |
| | [0.07] | [0.38] | [2.21] |
| N | 1445 | 1445 | 1445 |
| R-sq | 0.054 | 0.3 | 0.024 |
| Controls | No | Continuous | Continuous |
| Muni. FE | No | No | Yes |

Note: Table A10 shows that our results are robust with a continuous socioeconomic variable instead of a binary indicator to allow for different impact conditional on socioeconomic levels. The coefficient on the interaction term is negative and significant implying a lower (conditional) probability of more affluent municipalities to receive grants after the reform, in line with our previous findings. However, the interpretation of our main coefficient becomes more complicated as the marginal effect depends on the level of socioeconomic index.
Controls include log population size (in models 2 & 3) and peripheral index (in model 3 only). SEI values run from -3.1 to 3.5, with standard deviation equal (almost) 1.

Table A11: [11a] The effect of the policy change on grants, by socioeconomic rank (full period), localities in the bottom 1-3 to upper 4-10 deciles, ranked by socioeconomic index, 1990-2001 versus 2005-2015, table 11a in the journal version

Panel A: Dependent variable: probability to receive a grant

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Post | 0.026 | -0.016 | 0.007 | -0.034** | -0.094*** |
| | [0.02] | [0.02] | [0.02] | [0.02] | [0.02] |
| Low SEI | -0.110*** | 0.013 | -0.051 | -0.002 | -0.066** |
| | [0.02] | [0.02] | [0.03] | [0.02] | [0.03] |
| Post X Low SEI | 0.073*** | 0.072*** | 0.088*** | 0.086*** | 0.081*** |
| | [0.03] | [0.03] | [0.03] | [0.03] | [0.03] |
| Constant | 0.305*** | 0.189*** | 0.259*** | -0.973*** | -2.376*** |
| | [0.02] | [0.01] | [0.02] | [0.05] | [0.27] |
| N | 5696 | 5696 | 5696 | 5694 | 5694 |
| R-sq | 0.009 | 0.096 | 0.007 | 0.139 | 0.03 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

Panel B: Dependent variable: ln(Grant per capita)

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Post | -0.268*** | -0.142* | -0.235*** | -0.087 | -0.216*** |
| | [0.08] | [0.07] | [0.06] | [0.07] | [0.08] |
| Low SEI | 0.500*** | 0.005 | -0.198 | -0.012 | -0.192 |
| | [0.15] | [0.15] | [0.15] | [0.11] | [0.15] |
| Post X Low SEI | 0.377*** | 0.459*** | 0.216 | 0.442*** | 0.212 |
| | [0.14] | [0.14] | [0.15] | [0.14] | [0.15] |
| Constant | 4.806*** | 5.614*** | 5.062*** | 10.475*** | 5.909*** |
| | [0.09] | [0.09] | [0.11] | [0.33] | [1.42] |
| N | 1687 | 1687 | 1687 | 1687 | 1687 |
| R-sq | 0.058 | 0.19 | 0.011 | 0.326 | 0.011 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

Note: Using the full period, Table A11 shows that the bottom three socioeconomic deciles (relative to the upper 7 deciles) have benefited from the adoption of rules-based policy. The results are robust for the probability to receive a grant in all models and for the level of grants per capita in three models. See notes to Table 4.

Table A12: The effect of the policy change on grants, by ethnicity (full period), (Arab relative to Jewish localities, 1990-2001 versus 2005-2015), table 11b in *journal version*

Panel A: Dependent variable: probability to receive a grant

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Post | 0.030* | 0.003 | 0.018 | -0.015 | -0.089*** |
| | [0.02] | [0.02] | [0.02] | [0.02] | [0.02] |
| Arab | -0.169*** | -0.130*** | | -0.154*** | |
| | [0.02] | [0.02] | | [0.02] | |
| Arab X Post | 0.051** | 0.029 | 0.04 | 0.038 | 0.054** |
| | [0.02] | [0.03] | [0.03] | [0.02] | [0.03] |
| Constant | 0.328*** | 0.218*** | 0.236*** | -0.903*** | -2.432*** |
| | [0.02] | [0.02] | [0.02] | [0.06] | [0.28] |
| N | 5696 | 5696 | 5696 | 5652 | 5652 |
| R-sq | 0.025 | 0.096 | 0.005 | 0.14 | 0.029 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

Panel B: Dependent variable: ln(Grant per capita)

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Post | -0.186** | -0.059 | -0.215*** | 0.021 | -0.202** |
| | [0.08] | [0.08] | [0.07] | [0.08] | [0.09] |
| Arab | 0.809*** | 0.415*** | | 0.208* | |
| | [0.13] | [0.13] | | [0.12] | |
| Arab X Post | 0.124 | 0.204 | 0.018 | 0.071 | 0.065 |
| | [0.14] | [0.14] | [0.14] | [0.13] | [0.15] |
| Constant | 4.758*** | 5.629*** | 4.850*** | 10.550*** | 5.986*** |
| | [0.09] | [0.12] | [0.12] | [0.34] | [1.48] |
| N | 1687 | 1687 | 1687 | 1685 | 1685 |
| R-sq | 0.072 | 0.183 | 0.013 | 0.321 | 0.013 |
| Controls | No | Binary | Binary | Continuous | Continuous |
| Muni. F.E | No | No | Yes | No | Yes |

Note: Table A12 shows that the coefficient on the interaction between Arab and Post continues to be positive but borderline significant in two specifications when the investigated period covers the first six years and the dependent variable is the probability of receiving a grant. See notes to Table 4.

Table A13: The effect of the policy change on the probability to get grants, by socioeconomic rank (panel A) and ethnicity (panel B) and by investigated period (y to 2001 versus 2005 to 2015)

Panel A: By socioeconomic rank

| starting year y | 1990 | 1991 | 1992 | 1993 | 1994 | 1995 |
|---|---|---|---|---|---|---|
| Post | -0.094*** | -0.106*** | -0.114*** | -0.121*** | -0.128*** | -0.139*** |
| | [0.02] | [0.02] | [0.02] | [0.02] | [0.02] | [0.02] |
| Low SEI | -0.066** | -0.071** | -0.076** | -0.076** | -0.089*** | -0.102*** |
| | [0.03] | [0.03] | [0.03] | [0.03] | [0.03] | [0.04] |
| Post X Low SEI | 0.081*** | 0.090*** | 0.099*** | 0.106*** | 0.111*** | 0.114*** |
| | [0.03] | [0.03] | [0.03] | [0.03] | [0.03] | [0.03] |
| Constant | -2.376*** | -2.281*** | -2.130*** | -1.919*** | -1.913*** | -1.878*** |
| | [0.27] | [0.29] | [0.3] | [0.31] | [0.33] | [0.39] |
| N | 5694 | 5466 | 5236 | 5002 | 4767 | 4531 |
| R-sq | 0.03 | 0.024 | 0.02 | 0.017 | 0.017 | 0.017 |
| Controls | Continuous | Continuous | Continuous | Continuous | Continuous | Continuous |
| Muni. F.E | Yes | Yes | Yes | Yes | Yes | Yes |

Panel B: By ethnicity

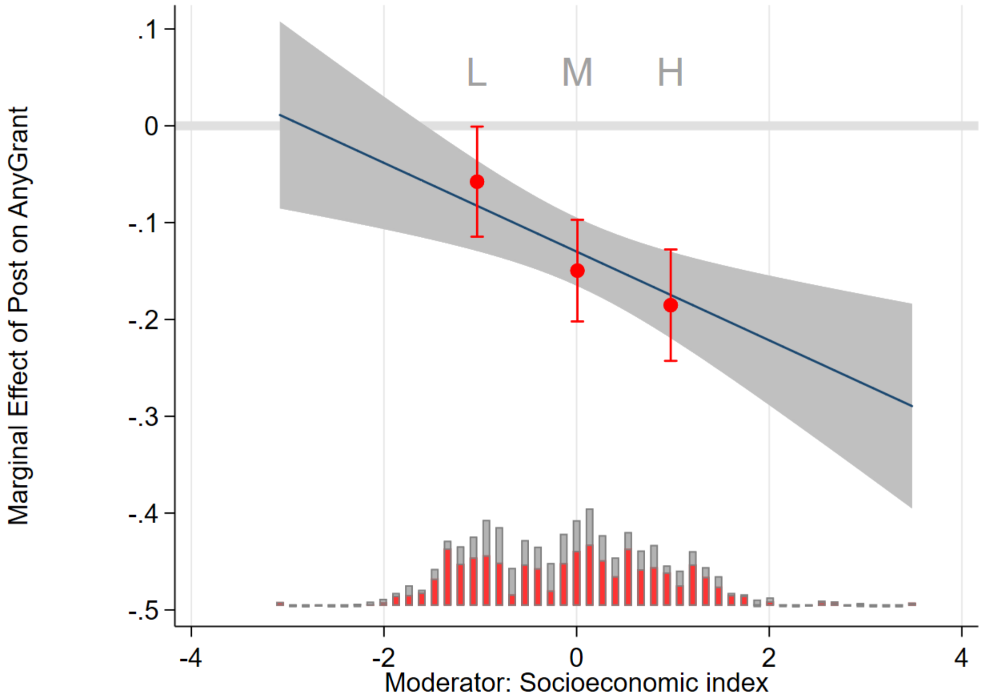
| starting year y | 1990 | 1991 | 1992 | 1993 | 1994 | 1995 |
|---|---|---|---|---|---|---|
| Post | -0.089*** | -0.101*** | -0.108*** | -0.116*** | -0.124*** | -0.134*** |
| | [0.02] | [0.02] | [0.02] | [0.02] | [0.02] | [0.02] |
| Arab X Post | 0.054** | 0.061** | 0.065** | 0.073*** | 0.079*** | 0.078*** |
| | [0.03] | [0.03] | [0.03] | [0.03] | [0.03] | [0.03] |
| Constant | -2.432*** | -2.348*** | -2.211*** | -2.017*** | -2.024*** | -2.027*** |
| | [0.28] | [0.29] | [0.32] | [0.32] | [0.35] | [0.41] |
| N | 5652 | 5428 | 5202 | 4972 | 4741 | 4509 |
| R-sq | 0.029 | 0.023 | 0.019 | 0.015 | 0.015 | 0.015 |
| Controls | Continuous | Continuous | Continuous | Continuous | Continuous | Continuous |
| Muni. F.E | Yes | Yes | Yes | Yes | Yes | Yes |

Note: Table A13 (12 in publication) presents estimates of our baseline regressions excluding the first year, first two years, and so on. We find that the coefficient of the interaction between Post and Arab is significant in all specifications. See notes to Table 4 (model 6).

Table A14: Parallel pre-trends test

| | Positive grant per capita | Any grant | | Positive grant per capita | Any grant |
|---|---|---|---|---|---|
| 1997xBottom3 | -180.6 | 0.109 | 1997xArab | -78.6 | 0.07 |
| | [201.9] | [0.08] | | [217.7] | [0.08] |
| 1998xBottom3 | 3.8 | 0.013 | 1998xArab | 99.99 | 0.037 |
| | [162.6 | [0.07] | | [182.18] | [0.08] |
| 1999xBottom3 | 3.6 | 0.1 | 1999xArab | 183.63 | -0.006 |
| | [160.4] | [0.07] | | [196.6] | [0.07] |
| 2000xBottom3 | -120.1 | -0.044 | 2000xArab | -47.6 | -0.118 |
| | [138.5] | [0.07] | | [155.4] | [0.07] |
| 2001xBottom3 | -267.1 | 0.054 | 2001xArab | -214.68 | 0.031 |
| | [141.2] | [0.08] | | [147.6] | [0.08] |
| N | 1447 | 4296 | N | 1447 | 4296 |
| R2 | 0.016 | 0.035 | R2 | 0.004 | 0.011 |

Figure A7: Conditional marginal effects




Note: Figure A7 depicts the results of binning the variable socioeconomic index into three values (low, medium, and high) and estimating the conditional marginal effect of Post on the probability of receiving grant (using STATA package interflex, Hainmueller 2012). The marginal effects suggest that under the rule-based period municipalities with a low socioeconomic index significantly improve their chances to receive grants compared to municipalities with a high socioeconomic index. The likelihood of municipalities with medium socioeconomic index of receiving grants are not significantly different from either disadvantaged or affluent municipalities.

*Chapter end.*

# Chapter 2.

# The impact of Active Labor Market Programs on disadvantaged income support recipients: Evidence from a Randomized Control Trial

**Abstract**

This paper studies the effect of an RCT-based intensive Active Labor Market Program on a series of labor market outcomes and benefits among a population of income support recipients. The program includes a change in the relative costs of benefits by extending the requirements for maintaining income support and easing the access to disability benefits, alongside soft skills enhancement. We find that after 2 years from allocation, the program had a significant and positive effect on employment (+16%), earnings (+32%) and disability benefits (+19%), alongside a significant decrease in income support benefits (−12%). Although the participants' total income shows a significant and positive increase, this effect is diluted at the household level. This result points to the replacement of benefits by earnings but also to the quick adjustment of labor supply among spouses, which also occurs in households that belong to the lower income brackets. The effect on labor market outcomes are stronger among men, people with low educational attainment and among Jews, relative to Arabs. A positive treatment effect on disability benefits can only be observed among men and Jews. While change in the relative costs of benefits seems to be a leading mechanism the program triggers, we cannot rule out that improved skills also play a role, but we do not find direct evidence for this possibility.

* This paper is based on a joint project with Analia Schlosser & Itay Saporta-Eksten. We thank the staff from the research department at the Israeli Employment Service for providing the administrative data on experiment participants. This research was conducted at the research room of the National Insurance Institute of Israel (NII). We thank the NII research unit for providing the administrative data.

> “Happy families are all alike; every unhappy family is unhappy in its own way”
>
> (Tolstoy, *Anna Karenina*)

# 1 Introduction

Active Labor Market (ALM) programs include various services provided to various subpopulations. Figuring out what works and for whom is an ongoing multifaceted challenge that occupies many policymakers and scholars. Methodologically, the main challenge is to evaluate effectiveness based on relevant comparison groups. Empirically, a main challenge is to locate the mechanisms responsible for the program’s (in)effectiveness, and thus, to examine specific theoretical predictions. Addressing these challenges is a precondition for answering a few practical questions, the central of which is: What programs should be replicated or extended?

Assuming the programs that ought to be replicated are the most effective ones, it is first crucial to note that the definition of effective has in recent years become increasingly diverse. Specifically, it has become more common to stretch the dimensions of effectiveness across time, locations, markets, family members, generations and components of utility. A specific program can be ineffective in the short term but effective in the longer term; effective in triggering additional income among participants, though by crowding-out non-participants from the labor market; or to be found unattractive in fiscal cost-benefit terms. It can be effective among participants, but it can have unintended consequences among their family members or the next generation. Finally, effectiveness can also be achieved in terms that go beyond consumption and leisure to include health and life satisfaction. Examining these various dimensions has become more common and feasible in recent years due to the availability of administrative data, the introduction of more ambitious designs and by stretching the horizon of analysis across longer durations and sample sizes.

Of high concern is what works for the least advantaged in society that stand on the margins of the labor market and who encounter multiple challenges of socioeconomic and psycho-social form. Considering "every unhappy family is unhappy in its own way," studying program effectiveness in the context of this population can be uniquely challenging given the diverse personal circumstances, and correspondingly, the various dimensions of effectiveness that can be considered.

We study the impact of an RCT-based intensive ALM program that was designed to serve highly disadvantaged income support recipients. As part of the program, participants went to occupational rehab centers. Attending these centers was planned to implement working habits, provide employment counseling and to perform better screening of participants to allow them to move between benefit types or to channel them to protected or supported employment. The program was mandatory, so that non-compliance could have been sanctioned by losing eligibility to guaranteed income support (GIS).

The program's effect is examined over 18 and 24 months after allocation. We find that the program had a significant and positive effect on employment (+16%) and earnings (+32%). In addition, it triggered a significant increase in disability insurance (DI) payments (+19%), alongside a drastic drop in the magnitude of income support payments (−12%). It also raised by 36% the share of individuals with no formal income from either wages or benefits.

At the household level, there seems to be no significant difference between the treatment and the control groups regarding the total household's income. We show that this result means the program has triggered a compositional change in households' income, between spouses and between sources of income, that is, wages and benefits.

The heterogeneous analysis shows that the effect on labor market outcomes is insignificant among Arab participants, while significant and strong among Jewish participants. In addition, the treatment effect on DI income is triggered by Jews and men, and is absent among women and Arabs. Finally, increased tendency to be left without formal income is much stronger among Arabs, women and married participants.

The study relates to three branches of literature. The first branch is the evaluation studies of ALM programs that examine what works for different population types via different types of programs and explanatory channels (Card, Kluve and Weber, 2018). We contribute by providing experimental evidence regarding the response to treatment of a highly disadvantaged low-skilled population that encounters multiple challenges. In addition, the program studied is unique in the sense that it focuses not only on labor market re-integration but also on shifts between benefit types.

The second branch is DI benefits. A number og studies have shown that DI receipt has a direct negative effect on labor supply (Koning, Muller and Prudon, 2022; Kostol and Mogstad, 2014; Maestas, Mullen and Strand, 2013; Gruber, 2000) and that the take-up of DI benefits is sensitive to application costs (Kearney, Price and Wilson, 2021; Deshpande and Li, 2019; Autor et al., 2015; Low and Pistaferri, 2015; Kellogg, 2022). In Israel specifically, opening new social security offices has shown to be positively associated with increased take-up of DI and means-tested income support benefits (Dahan, 2022). The program studied includes lowering DI application costs and directly relates to the labor supply response of participants and their spouses. In this context, we contribute by showing that the increased collection of DI among the disadvantaged population can be achieved via two distinct channels: improved screening and direct support in the application process as well as by increasing the costs (effort) of maintaining alternative benefits (e.g., income support) that in turn increases the motivation to collect DI.

The last point relates to a third branch of literature that focuses on the intra-household allocation of time and effort between labor (consumption), leisure and household duties (Blundell, Pistaferri and Saporta-Eksten, 2016, 2018). While one way to examine the theoretical predictions put forward by these papers is to analyze the household's response to income shocks (preferably idiosyncratic), governmental support may soften the effect of such shocks or create additional, parallel or consecutive shocks to income. In the case of unemployment insurance benefits (UIB), for example, studies have shown that when one spouse receives UIBs, this triggers a direct and drastic drop in the other spouse's wages (Cullen and Gruber, 2000). For DI, Autor et al. (2019) showed that an exogenous change in the probability of one spouse to receive DI also has a direct, negative and persisting effect on the other's wages and over a longer term, it increases the spouse's reliance on benefits.

We add to this literature by examining the employment dynamics of participants and spouses over a decade. In this period, household constrains were changed numerous times: before the program, with the allocation to the program and after its termination. A negative idiosyncratic income shock triggers the first change: a health or employment event that eventually lead participants to be allocated to the program. The monetary effect of this shock on the household's situation was mitigated by receiving GIS in the years leading to the program. The share of program participants that received GIS had reached 80% before allocation to the program. The second change was triggered by the very allocation to the program, which introduced the potential to increase income by improving labor-related capabilities or by enabling a shift from GIS to DI, and it has imposed additional conditions (i.e., costs) on maintaining eligibility to GIS. The COVID-19 crisis triggered the third change, which was an exogenous common shock that has put many of the program participants out of work. Because members of both groups had a similar chance to be exposed to the COVID-19 shock, this creates an additional opportunity to examine whether the program has equipped the treated with increased immunity, and we concisely examine this question.

In the context of household-level response, we contribute by showing that – rather quick – adjustments of labor supply within the household also occur at the lower end of the income distribution. This result is non-trivial because one could have assumed that constrained households would respond to shocks and to government-provided opportunities in a way that would increase their long-term (total) income trajectory. That they do not seem to act in this manner further strengthens the theoretical conclusions of Blundell, Pistaferri and Saporta-Eksten (2016) regarding the key factors that shape the common allocation of time among spouses. It seems that these factors are rather universal and generic because they can be observed across different income levels.

The rest of the paper is structured as follows. Section 2 provides a short review of the program, the background for its launch and its properties. Section 3 presents the data used and sample selection, and Section 4 presents the design. Section 5 describes the main results, and Sections 6 adds an integrative discussion of the results. Section 7 concludes.

# 2 Institutional background

## 2.1 Employment programs in Israel

Generally speaking, Israel maintains a social insurance and welfare support system that resembles Central European systems regarding the conventional functions covered, but it is somewhat less generous in fiscal terms. The system includes a few layers, such as unemployment insurance benefits (UIB) and DI benefits, in addition to welfare transfers such as GIS for the long-term unemployed, as well as universal child allowances and semi-universal old-age benefits. However, social services in the field of employment are less extensive and historically less developed, relative to Central and West European standards.

During the 2010s, Israel saw increased investment in ALM programs, following an earlier wave of cuts in transfer payments. The Israeli government conducted two rounds of welfare-to-workfare programs aimed at assisting GIS recipients from 2004–2007 and from 2007–2010. These programs were mostly unsuccessful and were criticized for low effectiveness and inappropriate delivery. A third round was launched in 2014, under the name "Employment Circles." This program was handled via an RCT, and was found to be highly effective, on average, and especially among Arabs, the long-term unemployed and people with self-reported health limitation (Schlosser and Shanan, 2022). A new sister program designed for a more disadvantaged sub-population was launched in June 2017: the "Tnufa" program (the Hebrew word for *momentum*).

Around the same time of increased financing of ALM programs, (low) take-up of benefits has started to become a major concern for the Israeli government – vis a vis a similar pattern observed in other developed countries (Currie, 2004; Kleven and Kopczuk, 2011).[1] The concrete sense of under-utilization of benefits (sometimes: rights) means that people do not collect the benefits to which they are eligible. This may be due to missing information; to the high costs of collecting the benefits, originating in bureaucracy; the costs of proving eligibility (e.g., tests); and additional costs that stem from stigma. Either way, non-take-up makes a critical policy problem.

[1] Gal (2007) pointed to the importance of benefits take-up in a general- and in the local (Israeli) context. As for governmental activity, in 2012, the National Insurance Institute of Israel established its own rights-utilization centers (the "directing hand" centers). The ministry of welfare was also active in this field throughout the 2010s. In 2014, The Committee for the War against Poverty in Israel (The *Alaluf committee*) marked non-take-up of benefits as a main systemic problem, that must be addressed in order to assist people in poverty.

## 2.2 The program

The *Tnufa* program was designed to serve a highly disadvantaged population – income support recipients encountering multiple challenges, who had to attend regularly the employment service's office to be eligible for income support benefits. Among other aspects, the challenges this group encounters stem from health limitations, limited proficiency in the Hebrew language, historical addictions, criminal records and long durations of non-employment. The program was active in Israel between June 2017 and December 2018, a period in which it served more than 4,000 treated participants (see below in section 3: Data). It was terminated upon the completion of a contract between the Israeli Employment Service (IES) and a private provider, after which it was re-launched under a new contract (the second period is not addressed in this study). The total cost of the program was around 24.3 million shekels (6.9 million USD, that is, around $1,570 per treated participant; or $2,100 per effective participant, when counting attendees only).

The program was introduced as part of an effort the Israeli Employment Services made during the 2010s to act and adjust services based on better segmentation of its clients and their needs. The program's objectives included both reintegrating jobseekers into the labor market while adjusting for some of them the right benefits composition and exempting them from mandatory attendance at the employment service offices (thereby also lowering the burden placed on its caseworkers).

The program was designed to increase soft capabilities and to strengthen working habits. In this sense, it followed the general framework of the *Employment Circles* program, but it differed in being even more social work oriented. Specifically, it differed in terms of higher intensity and personnel. Tnufa included personal and group meetings with psychologists and occupational social workers. The latter were also responsible for evaluating employability prospects, thereby identifying those individuals for which labor market re-integration was irrelevant and to help them in applying for DI or in joining protected employment. The formal duration in the program was planned to be up to 5 months; however, many participants have stayed for shorter and some have stayed for longer durations – overall between 3 and 8 months among the compliers.

The program included a sanction in the form of removing eligibility for GIS among non-compliers, that is, those among the treated who refused to attend the program's rehab centers (at baseline, eligibility to GIS is also conditioned by attending IES offices once a week or once a month for people aged 50 or over).

Adjusting the relevant benefit types to participants was a central feature of the program. The two relevant benefits in this regard are DI benefits and GIS benefits (UI benefits were not part of the program's scope because it has not addressed temporary and short-term unemployed). An individual can receive DI or GIS but cannot be eligible to the two benefit types simultaneously. Because the two benefits are mutually exclusive, and because the program was designed in part to shift some individuals from GIS to DI, we mention four main differences between the two. First, while GIS eligibility is determined at the household level, DI eligibility is determined at the individual level, although it includes added sums for dependent persons in the household, spouse and children. Second, the sums received in the two cases are rather different. In the relevant years, DI was higher by around 33% to 100%, depending on family composition and the severity of the impairment (see figure A23). Third, receipt of GIS, although not in all scenarios, depends on attending IES offices, which is not the case for DI. Finally, when considering other countries' experience in this regard, DI has a different effect from GIS in terms of weakening or cutting the people's attachment to the labor market. That is, the very entrance into the DI system lowers the likelihood that individuals will return to the labor market even after their health situation improves (Koning, Muller and Prudon, 2022). This means that DI dominates GIS, at least for people with very low labor market attachment.[2]

Finally, note that control group members are not ignored in terms of receiving some services. They are still eligible to attend IES offices, where caseworkers may offer them jobs or other employment services (and in fact, they indeed tend to attend these offices).

## 2.3 Importance beyond the local set-up

At the institutional level, IES officials emphasized that a clear motivation for setting up the program had to do with expertise. Although the IES caseworkers are trained in assisting various populations of job-seekers, this specific group that was eventually allocated to the program encountered additional challenges that went beyond job searching and skills formation, that is, challenges that are often addressed by local welfare departments. In addition, a significant share among them encountered health problems that fell within the range of health authorities and the National Insurance Institute. Finally, the case of a distinct population that encounters multiple challenges, each of which addressed by a different state authority, is not unique to Israel. In fact, it is a generic problem that other countries encounter.

For example, the move from income support (TANF) to DI, has also been addressed by the US Social Security Administration, that initiated the TANF/SSI Disability Transition Project (TSDTP) (reviewed by Farrell, 2013). A central motivation behind this project was that TANF recipients experience enhanced hardships in achieving DI eligibility. As part of launching the TSDTP, it has been shown that DI approval rates for TANF recipients – conditional on application – were lower by 5-11 percentage points, relative to a 49% baseline among other SSI disability applicants. In addition, a significant gap has been documented

[2] Nevertheless, *stigma* may play a role in this regard, in more than one way. On the one hand, GIS may be associated with poverty while DI seemingly stems from an objective situation that the individuals. On the other hand, some people may want to avoid the stigma associated with being disable.

in the duration from application to final decision: 13.7 months for TANF recipients and 11.3 months among non-TANF recipients (a 21% difference in duration).

# 3 Data

## 3.1 Data sources & variables

The analysis is based on two administrative data sets. The first – from the IES– includes the (a) experimental variables and an additional rich set of (b) background variables (sociodemographic and human capital characteristics). The IES data also includes records on participants' attendance in IES offices and in rehab centers. Based on this last type of data, we also construct some (c) outcome variables, namely, compliance to treatment and attendance duration.

The second data set is based on the National Insurance Institute (NII) records. This data set – which incorporates records from the Israeli Tax Authorities – includes details on the employment status and on personal wage income and benefits for the years 2010–2020. We use this data to construct (d) additional background variables, for example, employment history, and (e) a set of outcome variables.

The outcome variables include personal wages (monthly salaried income) and monthly employment (binary variable that equals 1 in those months in which the individual received any positive wage), as based on tax records. Based on the NII records, we also observe monthly receipt of benefits. We focus on DI and GIS, and for each, we compute both any monthly receipt (binary variables) and the corresponding sum. For all variables mentioned, we also compute the cumulative value, that is, the wages or benefits accumulated from t0 (the month of application) and up to each t. We also compute total income and cumulative total income that incorporate wages and benefits. Two final outcome variables are attendance at the IES offices and rehab centers – based on direct documentation of the IES – and disappearance, which is a binary variable that equals 1 for months in which individuals have no records of formal income (wags or benefits) or of attendance in IES offices or rehab centers.

## 3.2 Samples used

The full raw sample includes 5,985 individuals. Out of the full sample, 325 participants went to rehab centers outside the experiment without being allocated to one of the experimental groups (prior to randomization, caseworkers defined these individuals as clients that must receive the more intensive services the rehab centers provided). The final sample consists of 5,660 individuals experiment participants. Of the sample, 70% (N=4,042) were allocated to the treatment group and the other 30% (N=1,618) were allocated to the control group.

Thirty-eight individuals went to rehab centers, although they were allocated to the control group. These referrals were based on caseworkers' discretion given those individuals' personal (critical) situations. We include these individuals in our analysis – leaving them in the control group – to avoid an upward bias of our estimates, assuming that these individuals are characterized by an unusual number of challenges, so that removing them from the experiment would improve the average "quality" of the control group. The consequences of this is a potential downward bias of the results given that a few controlled participants were treated. However, this should be negligible given their low relative share (2.3% of the control group).

Allocations to the experiment were made between June 2017 and December 2018. Attendance in IES offices was observed up to February 2020, with benefits and labor market outcomes up to December 2020. However, for the main parts of the analysis, the COVID-19-period results (starting from March 2020) are excluded, so that outcomes are observed up to February 2020. With this background, we focus on two main samples, 18-months sample and 24-months sample, that include individuals observed over 18 and 24 months from allocation to the program, respectively. All those who are part of the 24-months sample are also included in the 18-months sample because all those observed over 24 months are also observed over 18 months, but the opposite is not true. Specifically, the 24-months sample includes people allocated to the program between June 2017 and February 2018 (for which outcomes are observed up to February 2020), and the 18-months sample includes people allocated to the program between June 2017 and August 2018 (for which outcomes are observed up to February 2020). This is why the sample with the longer horizon is smaller in size. Finally, note that outcomes are measured from the month of allocation and not from the end of participants' period there; hence, they are also observed while participants are in rehab centers.

## 3.3 Units of analysis

At the center of our analysis stands the comparison of participants' outcomes. We complement it with spouse-level and a household-level analyses. Focusing on the outcomes of different units of analysis relates to demonstrating different theoretical hypotheses, and necessitates some technical clarifications.

As stated, eligibility for GIS is determined at the household level,[3] whereas eligibility for DI is determined at the individual level but includes a supplement covering dependent household members (spouse and children). For technical reasons, in the case of married couples' households, the NII system splits the GIS sum between the two spouses' records

[3]Thus, a GIS eligibility can only be removed, or changed, for the household as a whole. Hence, if the sanction included in the program is activated the drop is almost identical among both spouses (see figure 16 in the appendix – verifying this point).

whereas the full DI sum, including the supplement, is attached to the eligible spouse's record; therefore, we have another reason to examine outcomes at the individual and household levels. That is, the GIS split is artificial; therefore, a household-level analysis is preferable. On the other hand, the GIS is in essence a household-level benefit, so attributing the entire sum to one spouse when conducting participant-level analysis is also irrelevant.

# 4 Design

## 4.1 Experimentation

Randomization took place at the IES headquarters, and the ratio of treatment group members to control group members was approximately 2:1. The initial candidate pool was created by local branches and approved by the five IES (geographical) districts. Allocation to the program then occurred in cells. A cell for our purposes is a combination of the local IES office, date of allocation and history in the Employment Circles (EC) program. Specifically, because the pool of potential candidates includes some who participated in the past in the EC program (see above), we had to account for this fact. Therefore, we split it into three statuses: (a) offices where EC was not active, (b) offices where EC was active and the participant has participated in the program and (c) offices where EC was active and the participant has not participated in the program.

We evaluated the program based on a randomized control trial, by which 70% of the experiment population are directed to rehab centers. Attending these centers is mandatory, so not attending them results in losing eligibility for GIS. In addition, attending rehab centers is much more demanding than the alternative, which includes shorter and less frequent visits to the employment services office. We estimate the treatment effect of being allocated to the treatment group (*intention to treat*) – rather than the effect of actually attending rehab – on numerous outcomes.

We focus on the intention-to-treat (ITT) estimates; that is, we compare outcomes between those assigned to the program and members of a control group, regardless of whether assignees have complied and attended rehab centers. However, we use the terms *program effect*, *treatment effect* and *assignment effect* interchangeably, though for precision, assignment effect is the most accurate one.

The equation describing the model is:

$$Y_{isct} = \tau_1(Treated)_i + X_i'\theta + \delta_{sct} + \varepsilon_{isct} \tag{1}$$

where $Y_{isct}$ is the outcome for individual $i$ directed via office $s$, belonging to EC group $c$, at time $t$. $X_i'\theta$ is a vector of control variables (personal characteristics), and $\delta_{sct}$ is an allocation (randomization) cell fixed effect. Standard errors are clustered at the level of randomization cells.

The covariates used include a rich set of variables. Sociodemographic variables (their value is true as of the month of allocation) include number of children, age and additional binary variables: married, Arab, female, ultra-orthodox (Haredi) Jew, migrant (in the country since 1989 or later), Ethiopia-born migrant, and single parent (for women only). Two human capital variables are self-reported health limitations (SRHLs) and educational level (primary education or less). The employment history variables are three binary variables noting whether the individual has been employed at least once during the first, second and third years before allocation. Six additional variables reflect the total annual income from wages and from income support (separately) in each of the three years leading to allocation.

## 4.2 Balancing

Table 2 shows a balancing test for a series of variables (we described the variables in section 4.1). We conducted the test with the main sample used, the 24-month sample, i.e., a sample that only includes participants whose earnings records are observed over 24 months after allocation. Column 1 shows the mean value for each variable in the control group, column 2 shows the mean value for the treated, and column 3 shows the estimated difference between the two groups. The random allocation of individuals into the two groups (hereafter the randomization process) means that people have equal chances (equal to one another) of being assigned to either of the groups. Therefore, if the randomization was properly conducted, the characteristics of participants in the two groups can be expected to have similar distributions. Indeed, the balancing test shows that all covariates are not statistically different across the two groups. The exceptions are marital status and health limitation, which show some significance, which we assume is coincidental. We also report the p-value from a joint-significance test. This test is based on regressing the treatment variable on all covariates, controlling for randomization cells, to examine whether all covariates can together predict allocation to the treatment group. The insignificance (p=.39) also proves that we achieved randomness.

Three important attributes of the program's population should be noted. First, regarding sociodemographic attributes, Arabs are over-represented in the program: they comprise 42% of the program participants, around twice their share of the population. This statistic is consistent with the fact that the poverty rate among the Arab population in Israel is higher than average and with the fact that, overall, this group is a more marginalized one in various dimensions (Endeweld and Dahan, 2019). Single parenthood is also directly associated with various hardships and a higher incidence of income support receipt.

Second, in human capital terms, educational attainment is exceptionally low among program participants, and almost 90% of participants had a self-reported health limitation when they were allocated to the program. This latter attribute is one of the main reasons people become potential participants in the program in the first place.

Finally, regarding employment history, program participants belong to the very low part of the income distribution and show low rates of historical employment even three years pre-treatment.

# 5 Results

## 5.1 From Theory & Design to Results

Before presenting the results, we clarify again how exactly the experimental design serves our theoretical interests.

The first theoretical context is the costs of achieving benefits. Because receiving and maintaining benefits is not costless, people may adjust their behavior in light of various costs (Dahan, 2022; Kearney, Price and Wilson, 2021; Deshpande and Li, 2019; Autor et al., 2015). We are therefore interested in the impact of changing the relative costs of benefit receipt. Specifically, being allocated to the treatment group means that GIS becomes costlier to maintain (additional attendance, i.e., additional efforts) and on the other hand that DI becomes cheaper to apply for (consulting and better screening). Likewise, when we compare earnings and benefits rather than one benefit to another, it can be assumed that increasing the costs (effort) of maintaining GIS will incentivize some people to go (back) to the labor market, especially if they do not meet the requirements for the alternative benefit (DI). By the same rationale, those who held an informal job alongside GIS receipt, at the time of allocation, are supposed to be incentivized by the new requirements to give up one of the two, i.e., to stick with their informal job and stay without a benefit or to give up or scale down that job to meet the new requirements (allocate more time to mandatory attendance). Alternatively, because the program intends to enhance participants' capabilities (increased general human capital, or soft skills), it should become easier for compliers to rejoin the labor market due to this development (Schlosser and Shanan, 2022).

The second theoretical context is intra-household allocation of time and effort. In this regard, we refer to the possibility that spouses provide each other income insurance (Cullen and Gruber, 2000) and to the hypothesis that each spouse's utility from work also depends on the other spouse's labor supply (Blundell, Pistaferri and Saporta-Eksten, 2016). This context may have special importance when one spouse receives DI because in this case, the other's commitment to compensate for the loss of income is somewhat reduced (Autor et al., 2019), but on the other hand, a spouse's leisure may be further consumed given the need to help his or her partner. Therefore, we are also interested in estimating the treatment's effect on spouses' and households' outcomes.

## 5.2 Compliance

Although we based our main analysis for labor market outcomes and transfers on ITT estimates, it is important first to review compliance patterns in the treatment group because they have implications for the interpretation of the results. We define compliance as any attendance of the treated in rehab centers, regardless of the number of visits. The number of months in rehab is also estimated separately.

Table 3 shows estimates for compliance (attendance). The unconditional compliance rates is 75%; that is, 3 of every 4 treated individuals attended rehab centers. The estimates shows that attendance is higher for (a) Arabs and (b) people with SRHLs and is lower for (c) people with at least one employment spell in the 36 months leading to treatment. The latter outcome is consistent with the possibility that for people more capable of reintegrating into the labor market, the program has triggered an effect by raising the costs of maintaining benefit eligibility (Black et al., 2003), as Schlosser and Shanan (2022) also documented for the ES program. Greater compliance among people reporting health limitations is consistent with the possibility that these individuals have a stronger motivation or need to utilize program-based services. In other words, they seemingly have more to gain from attending rehab and are less capable of entering the labor market on their own; therefore, they depend more on attending the program to avoid losing eligibility for income support or to shift from GIS to DI.

Although it is not entirely clear why Arab participants show a greater tendency to comply, it should be noted that Arab and Jewish participants go to different rehab centers, explained by geographical distribution; therefore, a large part of this difference may occur due to differences between rehab centers – technology, experience, burden of clients – rather than to differences between populations.

## 5.3 Main effect

Table 4 reports the assignment's effect on a series of outcomes, including employment, monthly earnings, and benefits. For every outcome estimated, we report the treatment effect (top row of each cell), standard errors (second row) and the control group's mean (bottom). Table 4 shows estimates from three models: without control variables, with demographic controls and with demographic and pre-treatment employment history controls. We continue with this last specification for the rest of the analysis, except for when estimating the effect on pre-treatment outcomes, in which case we excluded employment history controls.

Table 4 shows the results of the main analysis, including estimates for cumulative outcomes 18 and 24 months after treatment. A few important findings emerge from this analysis: first, assignment to the program triggers a significant and large effect on labor market outcomes: 24 months after allocation, the treatment effect on the cumulative number of months worked is .73 (s.e. .24) additional months (control mean: 4.4 months) and the effect on cumulative earnings is 4,500 NIS (s.e. ~1,000). Relative to the control group, the increase in earnings reflects a 32% difference. Second, whereas GIS benefits decreased by 3,340 NIS (-12% relative to the control group), income originating in DI benefits increased by 1,830 NIS (+19%). The sum of benefits includes zero and thus combines the share of recipients with the average benefit sum, which in the case of DI also reflects severity of impairments. In terms of the share of benefit receipt, the top rows of table 4 show a decrease of 5 percentage points in the share of individuals receiving GIS and a smaller increase of almost 2 percentage points in DI receipt. This last estimate is borderline significant, but a significant effect is clear in figure 1, which shows estimates for the entire period, and in appendix table A12, which also shows estimates after 18 months (in the 24-month sample). These results indicate that the treated indeed gain an advantage over the control in terms of the probability of receiving DI, but the latter catch up after some time, so the remaining difference after two years is evident in terms of cumulative DI income. Note also that a statistically significant and large effect on DI sum is observable only after 24 months, not after 18 months, pointing to the gradual increase in DI receipt and DI accumulation, which is consistent with the fact that achieving eligibility for DI takes a while.

The two results mentioned above are directly connected. Whereas the treatment effect on GIS is significant and strong and occurs across a large share of treated participants, this decrease is compensated for either via increased earnings or increased DI. In other words, these are the two channels through which the program has triggered results: labor market reintegration (due to the increased costs of preserving GIS eligibility or to improved capabilities) and benefit reallocation, that is, a shift of individuals across benefit types, in this case from GIS to DI (based on enhance screening and counseling).

Overall, given the increase in earnings alongside a smaller decrease in benefits, the treatment effect on the total income per participant, accumulated from all sources over 24 months from allocation, is around 3,100 NIS (s.e. 960), as table 4 shows (column 6). Relative to the 50,700 accumulated by control group members, this estimate corresponds to a 6% difference in participants' total income. Because the GIS is a household-level benefit, a comparison between households rather than participants shows a larger decrease in GIS and hence a smaller relative increase in total income (discussed below). Nevertheless, because spousal response is endogenous and because almost 60% of the participants do not belong to married households, a participant-level comparison is also important for understanding the program's effect, in which case splitting the GIS benefit between spouses can be justified.

## 5.4 Dynamic analysis

To complement the analysis of the main effect, we present a dynamic analysis, showing estimates for the main outcomes before and after allocation to the program. This analysis is meant to capture the timing in which a significant treatment effect can be observed, which can shed light on relevant mechanisms. This in turn enables the construction of a broader picture by going beyond an estimate that is (in)significant at a single point.

Figures 1-5 show the dynamic results.[4] Figure 1 shows the effect on the monthly GIS (panel A) and DI (panel B) benefits that participants receive from the NII. The estimates are based on separate regressions for each month over 60 months: 36 months pre-treatment and 24 post-treatment. For both benefits, the sum shown includes zero. The figures show an immediate decrease in income support benefits and a slower increase in DI benefits. Figure 2 shows a significant increase in employment (panel A) and even more so in earnings (panel B).

Taken together, the increase in DI alongside the decrease in GIS complement the results discussed above by emphasizing the timing of the various responses to treatment. Whereas GIS benefits decrease drastically and immediately after allocation, the effect on DI is observable 6 or 9 months after allocation. This pattern is consistent with the possibility that the decrease in GIS is a more common phenomenon that reintegrated participants, those who moved between benefits and those who did not comply and were sanctioned share. Specifically, the increase in earnings immediately after allocation (figure 2b) suggests that the tendency to avoid the costs associated with complying (rehab attendance) is a leading mechanism triggered by the program. On the other hand, it takes a while for DI benefits to increase, consistent with the fact that DI receipt depends on a longer process, which includes paperwork and attending medical committees of the NII, acts that were planned to be facilitated by the program's caseworkers.

Therefore, the increase in total income, which incorporates earnings and benefits, is only observable 6 months after allocation (figure 3), when earnings and DI income start to counteract the drastic decrease in GIS.

To gain a better understanding of the overall dynamics, we analyze two additional outcomes beyond benefits and labor market outcomes. The first is a binary outcome that captures participants' attendance at IES offices. The IES provides job-seeking and other employment services, and attendance there is mandatory for receiving GIS (in regular times). Figure 4 shows estimates for this outcome. By construction, 100% of participants attend IES

[4] As expected, a few results resonate the ones documented by Schlosser and Shanan (2022) that study the impact of a program that share a few characteristics with the program studied in this paper.

offices at t0, the month of allocation to the program. This proportion gradually decreases among the control group to around 55% after 18 months. In contrast, among the treated, it decreased to less than 30% after 5 months (which is the typical duration in rehab) and then gradually climbs back to around 45%, 10 percentage points below the control group. If attending IES represents a demand for (some sort of) governmental support, then the program triggers a decrease of 10 percentage points in this demand. However, those who are not attending the IES over this period belong to a few distinct groups: rehab attenders, those starting to work, those shifting to DI benefits and those who simply disappear from the formal records.

The second outcome is a binary outcome variable ("disappearance"), which equals 1 if a person has no formal income, neither from benefits nor from earnings, and does not attend IES offices or rehab centers.[5] Figure 5 shows that 18 months after allocation, the treatment effect on disappearance reaches 3.9 percentage points (30% higher than in the control group). Disappearance in fact means that a person either works an informal job or "stays home" without any income. Therefore, it seems that the increased costs of maintaining GIS have caused some participants to give up the benefit and instead rely solely on their job in the informal labor market or on other unobserved or undocumented incomes (e.g., originating in the family or the community).[6] Importantly, the effect on disappearance becomes significant rather quickly relative to allocation, lending some support to the informal job option (because adjusting family- or community-based support should, on average, be slower than the informal job scenario, which is immediate, because it simply means keeping an existing job).

## 5.5 Spousal response and Household level outcomes

Table 5 shows outcomes for single and married households. Columns 1-2 show outcomes for the entire program population, column 3 for singles and columns 4-6 for married households, excluding those in which the two spouses were allocated to the program. When relevant, we show outcomes at the level of participants, spouses and households. Note that for singles, households' and participants' outcomes refer, by definition, to the same value. We point to three important results.

First, 24 months after allocation, the effect on households' total income is either marginally significant or insignificant (statistically) for all types of households (table 5, columns 2 & 6). In light of the results presented above for earnings and benefits, this means that the program has triggered a change in composition, i.e., between earnings and social benefits, between

[5] Referring to this attendance is important assuming that contacting governmental agencies is as an indication that an individual may be on her way to get benefits eligibility; or on her way back to the market.

[6] This can also be seen in appendix figure A20, that shows flows between sources of income from 6 months before to 24 months after allocation. In total, after two years from treatment the share of people whose only source of income is benefits, is reduced by 10 percentage points among the treated compared with only 4 percentage points in the control group.

participants' and spouses' income or some of both (see figures 6-8).[7]

Second, whereas the effect on spouse's earnings is statistically insignificant,[8] assignment to the program triggers a marginally significant and positive effect on spouse's DI income (table 5, column 5, est. 1706; s.e. 1113). This positive effect is also shown in figure 9 and can be explained by two potential mechanisms: information and the pressure created by the decrease in GIS.[9]

Third, we find evidence for intra-household insurance. Table 6 and figures 10-11 show the program's effect on participants' main outcomes, divided into two groups by spouse's pre-treatment earnings: below and above the median of spouses' earning distributions. Participants married to spouses whose pre-treatment earnings are above the median generally have a lower level of labor market outcomes and higher transfer-based income than participants married to spouses with below-median pre-treatment earnings. This trend is more evident among the control group. A possible interpretation for this picture is that spouses of participants with lower (pre-treatment) income had to provide greater compensation via the labor market (placing their income above the median). This interpretation also helps explain the stronger treatment effect among the latter. Participants married to spouses whose pre-treatment earnings were high (above the median) responded more strongly to treatment because at this point, the compensation their spouses provided was to a large extent exhausted. In other words, treated individuals in both strata – married to spouses with below- and above-median pre-treatment earnings – experience the increased costs of maintaining GIS, but those who faced a narrower space of household income to use were more strongly pressured to respond via the labor market. Specifically, this difference can be seen in cumulative months employed and cumulative earnings, which are much greater among participants married to spouses with above-median pre-treatment income (table 6, second and third outcomes shown).

We provide additional demonstration of this trend in appendix figure A18, and we discuss it more broadly in the appendix. The figure shows that in the pre-treatment period, spouses' earnings and employment increase vis a vis the decrease in the participants' employment and earnings, i.e., spouses compensate for the decrease in participants' income. After treatment,

[7] Figure 7 shows levels and the treatment effect on cumulative income from benefits among participants and households. For both individuals and households, the effect is negative. The stronger decrease among households is for the most part due to the fact that GIS benefits are household-level benefits.

[8] Schlosser and Shanan (2022) document a significant and positive effect on spouse's earnings.

[9] Information means that spouses of treated individuals are more prone to hear about how to apply for DI, since treated individuals, even if not DI applicants themselves, were probably exposed more intensively to relevant know-how while attending rehab (see Duflo and Saez (2003); and Dahan and Nisan (2011) – two studies that document the dependence of take-up on information). On the other hand, the drop in GIS, which is a household-level benefit may increase the pressure (motivation) to prove or utilize potential eligibility for DI. This is in line with the studies documenting the impact of a negative income shock (e.g., unemployment) on the demand to DI (Maestas, Mullen and Strand, 2021; Kearney, Price and Wilson, 2021). It should however be noted that the number of cases triggering this effect (DI receipt) is rather small, which means that this analysis should be taken with a grain of salt.

outcomes among spouses of the treated are more strongly restrained, in accordance with the steeper restoration of the employment and earnings of the treated, i.e., the spouse's compensation continues as long as needed and is adjusted in response to the (magnitude of) increase in participants' employment and earnings.

### 5.6 Heterogeneous effect

Table 7 shows the heterogeneous effect for the main strata. The most important results relate to gender and ethnicity. First, it seems that Arab participants are much less responsive to treatment than Jewish participants. Other than a decrease in GIS payments, which almost all sub-groups experience, there seems to be no significant effect among Arabs, neither in employment and earnings nor in DI payments. This outcome deserves special attention in light of the fact that Arab participants' compliance (attendance in rehab centers) was higher than Jews'. Figure A13 shows that in fact, Arab women are indifferent to the program whereas Arab men are more similar to Jews in their responses.

Second, among women, the treatment effect on employment and earnings is weaker than among men, and there is no significant effect on DI despite similar levels of DI income in the control group and although group size among men and women is almost the same. This is triggered, at least, via the extensive margin: the effect on DI receipt among men is .055 (s.e., .019), relative to .173 in the control group, and among women, the effect is -.021 (s.e., .02), relative to .19 in the control group.

Third, in terms of total income, which includes earnings and benefits, the effect is large and significant among Jews, men and individuals with low educational attainment, and it is weaker or nonsignificant among the complementing groups.

Finally, figure 12 shows levels and the treatment effect on disappearance, by sub-group. Levels of disappearance are higher among Arabs, men, young and married participants and participants without SRHLs. The treatment effect on disappearance is especially high among people with low educational attainment, Arabs, women, married participants and participants with no formal employment spell in the 3 years leading to treatment.

## 6 Discussion

Of the extensive series of findings, two stand out, and we discuss them in depth in this section. The first is the differences between sub-groups in their response to the program, and the second is the role spouses of program participants play. We discuss alternative mechanisms and possible policy implications.

The program has increased the costs of keeping GIS by extending the number of weekly attendances necessary for benefit eligibility (in addition, the treated had to attend rehab centers rather than IES offices). The program has thus made the alternative, e.g., getting a job, applying for DI or staying without no income,[10] relatively cheaper. Similarly, it has become more expensive to hold an unreported job while claiming GIS, thereby pushing participants to give up one of the two. At the same time, the program has also reduced the application costs of DI by providing participants information and support in the application process. As a consequence, the results indeed include a decrease in GIS and an increase in employment, earnings and DI receipt.

The first part of the results – increased costs lowers GIS receipt – speaks to Nichols and Zeckhauser's (1982) theoretical prediction. They argue that ordeals can serve as a sorting mechanism, so that an increase in costs can improve targeting. This prediction has been demonstrated in field experiments conducted in Indonesia(Alatas et al., 2016) and Kenya (Dupas et al., 2016), where relatively small ordeals proved highly effective in sorting applicants. In contrast, in the US (Deshpande and Li, 2019) and Israel (Dahan, 2022), an increase (decrease) in application costs, triggered by the closing (opening) of application sites, lowered (raised) take-up rates of DI, but at the same time, targeting efficiency was hampered (improved).[11] In an ALMP context, increased costs introduced by allocation to the program have in some cases caused individuals to return to the labor market without ever attending the programs to which they were referred (Black et al., 2003; Rosholm and Svarer, 2008; Schlosser and Shanan, 2022). Our findings then seem to belong to this last group because we document that the least employable and most disadvantaged indeed tend to maintain eligibility for benefits (GIS) or to move toward DI whereas those more capable tend to give up benefits and return to (or stay in) the formal (informal) labor market (see also the results in table A11, discussed in the appendix).

The second part of the results – reduced application costs increase DI take-up – aligns with a growing body of evidence showing the sensitivity of DI take-up to application costs. These costs may arise due to processing times (Kearney, Price and Wilson, 2021), the geographical availability of governmental offices (Deshpande and Li, 2019; Dahan, 2022) and issues that relate to verifying the impairment type and severity (Autor et al., 2019). In addition, application complexity may pose a significant barrier in benefit take-up (Kleven and Kopczuk, 2011), as has been shown, for example, in the case of EITC take-up (Bhargava and Manoli, 2015). Therefore, attending rehab centers could have benefited participants by reducing the types of application costs (information, complexity).

Therefore, we ask why there seems to be no effect on collecting DI benefits among women

[10] It should be stressed that for people facing various physical or mental challenges, the costs of maintaining eligibility for GIS (even pre-treatment) may be non-negligible. Hence, when these costs further increase, due to being allocated to the program, also the stay-home alternative becomes cheaper.

[11] Nevertheless, one should be cautious about listing the two studies together as opening and closing of offices – and more generally, raising and lowering application costs – may not necessarily trigger similar and symmetric impact, as notes also by Dahan (2022).

and Arabs compared to men and Jews, respectively. Similarly, why do Arabs show an increased tendency to disappear – that is, to receive no formal wages or benefits – when allocated to treatment?

Regarding women, first, they do not differ from men in terms of compliance rates or their time in rehab (table 3), implying that they had similar chances of receiving relevant counseling. Therefore, fewer program inputs do not seem to be a factor. Second, in the 12 months leading to treatment, employment dropped more strongly among men than among women (result not shown), suggesting that men in the program are characterized by impairments that are somewhat more severe; therefore, they are more likely, on average, to be found eligible for DI. Although this should be equally true for the control group, it could be that if rehab centers, as an additional filter leading to DI, have a limited capability to screen participants, then they are more likely to locate men than women if the former indeed had more salient impairments. Finally, Low and Pistaferri (2019) show that women are less likely to receive DI "because [they]... are more likely to be assessed as being able to find other work than observationally equivalent men." If these findings are valid beyond the US case, they can explain gaps in both levels and in treatment effect.[12] In summary, (objective) differences in the severity of impairments and (subjective) differences in committees' decision making may be behind the gaps in DI collection across genders and behind gaps in the treatment effect in this regard. The difference in the treatment effect on (any) DI receipt lends support to the subjective channel (almost 5 percentage points among men and a null effect among women).

As for why Arabs do not collect more DI benefits when treated and show an increased tendency to disappear in response to treatment, numerous directions should be considered.

First, to explain gaps in levels, Kuka and Stuart (2021) show that Blacks in the US are less likely to receive UIB and that this gap does not originate in eligibility but rather in take-up (for similar levels of eligibility). In turn, the authors attribute lower take-up among blacks to the fact that they are over-represented in the southern parts of the country, where take-up is generally lower. This finding corresponds to the situation of Arabs in Israel, a group that is relatively less connected, less informed and more geographically remote, as Dahan and Nisan (2011) show regarding water subsidies and as Brender and Strawczynski (2020) show regarding EITC. Both papers document lower take-up among Arabs.

Second, to explain the gaps in treatment effects across ethnic groups, it is important to refer to differences between compliers and defiers in this regard. Arab participants attending rehab centers (compliers) indeed show a higher probability of receiving DI (17.3%) than the control group (15.5%), but this proportion is much smaller among those who have not attended rehab (9.9%, see table 8). This gap can either be explained by gaps in sorting

[12]This observation is in fact in line with a well-documented phenomenon on under-identification of health problems among women, relative to men in various medical situations.

among defiers (among Arabs, healthy people are less likely to comply) or by the possibility that those among Arabs who lose eligibility for GIS experience enhanced economic pressure (relative to Jewish defiers and Arab compliers), which makes them less able to deal with DI eligibility.

Third, a clear and rather strong gap can be observed between Arab men and women (figures A13-A14). Among the four sub-groups (of gender and ethnicity) – in terms of levels and treatment effect – it seems that Arab women constitute a unique case whereas Arab men are more similar to Jewish participants. This finding is consistent with the fact that among the four groups, Arab women are the one with the weakest labor market attachment (in and outside the program) and the strongest pre-treatment dependency on GIS. It therefore seems reasonable that Arab women only respond via the increased costs channel, do not increase employment and earnings and disappear in very large numbers when treated. Because most Arab participants are women, they have a significant impact on outcomes for the entire Arab group.

Finally, Arab participants are much more likely to be married (58% vs. 23% among Jews), which means that many more of them have a spouse's income to rely on.[13] This means that Jews – more of whom are single – should have stronger motivation to use the assistance rehab centers provide to earn DI; also, they are more sensitive to the loss of GIS. This finding is consistent with the stronger effect on DI among Jews and the stronger effect on disappearance among Arabs.

This last point relates to our second important finding, that is, the different responses of the two types of households – single vs. married – to treatment. We have shown that whereas singles experience a significant increase in earnings, this effect is much smaller in the case of married households. Specifically, singles' earnings 24 months after treatment are more than 1.5 times their earnings 36 months before allocation. In this regard, they also perform better than married participants (figures A15-A16). This difference also lends support to the increased-costs channel: those who do not have intra-household insurance in the form of spousal income are less tolerant to losing GIS eligibility or to spending extra time on mandatory attendance in rehab centers. Therefore, they are more likely to go back to the labor market (extensive margin) or increase labor supply (intensive margin). This result echoes findings by Autor et al. (2019) that document a significant decrease – of more than 40% – in the labor earnings of spouses of DI appellants in response to the appeals' approval. Autor et al. conclude that this decrease in spouse income, in response to the approved appeal,

[13] To the extent that Arab participants indeed make a disadvantaged group within the program population, the fact that they tend to disappear in higher rates stands in contrast to the prediction made by Nichols and Zeckhauser (1982), that higher costs should improve targeting. On the other hand, the costs of applying for DI are reduced and the treatment effect on DI collection among Arabs is lower which is consistent with the authors' prediction (lowered costs weaken targeting). This brings up the question of symmetry, and whether Nichols and Zeckhauser' argument is valid in both directions, and for different starting points. We leave this question open, as it cannot be directly examined in the current set-up.

reflects an added worker effect because it occurs after a previous increase in spouse income, which in turn had occurred earlier in response to the first application denial. In figure A18, we document a trend that corresponds with Autor et al.. Specifically, the employment rate among the control group's spouses stabilizes at a higher level than of spouses of the treated, and control spouses' earnings keep increasing whereas it stops increasing among spouses of the treated. This difference is consistent with intra-household insurance, which seems to take place also among the low earning brackets.

Interestingly, Autor et al. (2019) also document a significant increase in spouse's social benefits 4 years after approval of the appeals. In our case, we see that spouses of treated individuals collect more DI benefits than spouses in the control group (estimates are positive but below the significance threshold). Whereas in their case, this may be a long-term side effect of (spouses') detachment from the labor market, in our case, it occurs in the short to medium term, which suggests that it occurs due to the information channel, similar to findings by Duflo and Saez (2003) and Dahan and Nisan (2011). Nevertheless, the number of cases triggering this effect is rather small.

# 7 Conclusion

We study the effect of an Active Labor Market program designed to assist highly disadvantaged income support recipients via employment counseling, working habits implementation and in-depth screening. The program – that was based on a randomized control trial – was effective in significantly increasing employment and earnings among the treated, while also increasing the income originating in DI benefits. In total, after 2 years of treatment, the share of people among the treated who rely solely on benefits reduced drastically, compared with the control group. However, we also observe a small but significant increase in the share of people who have no source of formal income from either wages or benefits.

The effect on employment and earnings is stronger among men relative to women, Jews relative to Arabs, and among participants with very low education attainment relative to people with education level of high school and above. A positive and significant effect on DI benefits is unique to Jews and men.

The program's effect seems to be triggered by the increased costs of maintaining income support, which the requirement to attend rehab centers (employment, earnings and disability benefits) introduced, and by the increased inputs of counseling and improved screening (disability benefits). The centrality of the increased-costs mechanism is captured by that most of the increase in earnings and employment occurs in the short term, after which the gap between the two groups remains rather stable.

Importantly, when splitting the population by marital status, we document a significant and positive increase in earnings among singles, while the effect is lower and insignificant among married households. This result provides a clear demonstration for intra-household insurance that manifests rather quickly in response to income shocks such as job loss, health events and changes in benefits eligibility.

We conclude that changing the relative costs of maintaining or applying for benefits is a key for achieving effectiveness in ALM programs serving disadvantaged populations. Specifically, it seems that a simultaneous increase in the cost of one benefit (e.g., ordeals) alongside a decrease in the costs of another (e.g., screening and consulting) can enhance the shift between benefit types and trigger increased labor market re-integration.

Additionally, the study shows that observing spouses' outcomes in addition to those of participants is a critical input for evaluating the effectiveness of different programs and policies. Additional work is needed in theorizing and examining spouse and households' behavior in the context of ALM programs – based on larger samples and even longer durations.

Finally, we point to two additional avenues for future research to develop. First, addressing the questions of symmetry in applications costs and the role of reference points can be valuable. That is, whether for same benefit types, raising and lowering application costs triggers a behavioral response of similar magnitude in opposite directions. Furthermore, we question what role the initial monetary value of the benefits, and the initial costs of achieving benefits, play (reference-points). Second, a more systematic analysis of the moves between the welfare system and the informal labor market seems promising, which is a phenomenon that, to the best of our knowledge, has only been addressed in general terms.

# Tables & Figures

Table 1: Samples

| Sample | N | Last month of allocation included |
|---|---|---|
| h12 | 5,660 | December 2018 (the entire participants population) |
| 18-months sample | 4,355 | August 2018 |
| 24-months sample | 3,130 | February 2018 |

*Note*: This table shows the main samples used in the paper. The samples are defined by the horizon, that is, the duration during which participants are observed from the month of allocation to the program – until to the final month for which there is available data on their wages.

Table 2: Balancing test

| Variable | Control<br>(1) | Treatment<br>(2) | Difference<br>(3) |
|---|---|---|---|
| Age | 44.232 | 43.954 | -0.377 (.256) |
| Arab | 0.415 | 0.410 | -0.013 (.015) |
| Female | 0.503 | 0.494 | -0.008 (.02) |
| Haredi | 0.074 | 0.062 | -0.009 (.01) |
| Ethiopian | 0.024 | 0.027 | 0.002 (.005) |
| Married | 0.410 | 0.370 | -0.038** (.016) |
| Children | 2.651 | 2.634 | 0.001 (.083) |
| Single-Parent | 0.117 | 0.131 | 0.017 (.012) |
| Immigrant | 0.137 | 0.139 | 0.003 (.012) |
| Health-limitation | 0.888 | 0.865 | -0.022 (.011) |
| Low education level | 0.278 | 0.278 | -0.006 (.017) |
| | | | |
| Employed [-1,-12 ] | 0.248 | 0.273 | 0.023 (.018) |
| Employed [-13,-24] | 0.279 | 0.305 | 0.022** (.016) |
| Employed [-25,-36] | 0.291 | 0.309 | 0.014 (.016) |
| | | | |
| Earnings [-1,-12] | 2812 | 3171 | 243 (321) |
| Earnings [-13,-24] | 5129 | 5513 | 124 (504) |
| Earnings [-25,-36] | 6692 | 6874 | -34 (638) |
| | | | |
| GIS income [-1,-12] | 15632 | 15882 | 340 (414) |
| GIS income [-13,-24] | 14078 | 14483 | 494 (413) |
| GIS income [-25,-36] | 13207 | 13264 | 134 (422) |
| | | | |
| Joint test p. value | | | 0.39 |
| N | | | 3130 |

*Note*: This table shows descriptive statistics and a balancing test for the main covariates. Column 1 shows variables' mean in the control group, and column 2 shows variables' mean in the treatment group (in both cases this is the simple mean within each group, not controlling for randomization cells or weighting accordingly) . Column 3 shows the estimated difference, where the estimation includes randomization cell fixed effect. The sample used is the main sample (24 months-sample), that is, the sample in which individuals' employment and earnings are observed over a period of 24 months from allocation to the program, that ends before the outbreak of the Covid crisis in March 2020. Since the difference comes from estimation that includes randomization cells and columns 1-2 show simple means, the estimated difference is not identical to the difference between the simple means. Balancing test only for married participants is shown in appendix Table A13. The variable *Earnings* [-i,-j] refers to the total income originating in labor income that was accumulated during the period described in brackets. In the same manner, the variable *GIS income* [-i,-j] refers to the total income originating in GIS in that period. The variable *Employed* [-i,-j] is a binary variable that equals 1 if the individual. The variable "low education level" is a binary variable that equals 1 for people whose education attainment is primary school or less. The p-value reported comes from a joint-significance test, that is based on regressing the treatment variable on all covariates, within randomization cells, in order to examine whether all covariates can together predict being allocated to the treatment group.

Table 3: Compliance: attendance among the treated

| Sample | Ever Attended Rehab Center (=1) 24-months sample | 18-months sample | Number of months in rehab (including 0) 24-months sample | 18-months sample |
|---|---|---|---|---|
| Arab | 0.086*** | 0.048** | 0.540* | 0.241 |
| | (.029) | (.023) | (.309) | (.229) |
| Female | 0.015 | 0.016 | 0.18 | 0.253* |
| | (.021) | (.018) | (.183) | (.142) |
| Haredi | 0.035 | 0.003 | 0.05 | 0.113 |
| | (.048) | (.04) | (.359) | (.321) |
| Ethiopian | -0.048 | -0.031 | 0.383 | 0.473 |
| | (.049) | (.043) | (.46) | (.369) |
| Married | -0.002 | -0.017 | -0.017 | -0.179 |
| | (.028) | (.023) | (.223) | (.171) |
| Children (1) | -0.01 | 0.014 | 0.09 | 0.195 |
| | (.032) | (.027) | (.27) | (.236) |
| Children (2) | -0.070** | -0.051* | -0.385 | -0.303 |
| | (.034) | (.028) | (.257) | (.202) |
| Children (3) | -0.038 | -0.021 | -0.533** | -0.469** |
| | (.035) | (.027) | (.254) | (.208) |
| Children (4) | -0.082** | -0.053* | -0.741** | -0.495** |
| | (.038) | (.032) | (.293) | (.241) |
| Children (5+) | -0.021 | 0 | -0.173 | -0.109 |
| | (.035) | (.028) | (.265) | (.216) |
| Single parent | 0.049 | 0.002 | 0.31 | -0.039 |
| | (.032) | (.029) | (.309) | (.248) |
| Immigrant | -0.001 | -0.022 | -0.025 | -0.206 |
| | (.032) | (.025) | (.308) | (.229) |
| Health limitation | 0.102*** | 0.082*** | 0.692*** | 0.296 |
| | (.03) | (.026) | (.205) | (.209) |
| Age | 0.005*** | 0.005*** | 0.044*** | 0.037*** |
| | (.001) | (.001) | (.01) | (.009) |
| Low education level | 0.007 | 0.02 | 0.087 | 0.222 |
| | (.023) | (.018) | (.211) | (.167) |
| Ever Employed (-1-36) | -0.064*** | -0.068*** | -0.360** | -0.373*** |
| | (.022) | (.018) | (.17) | (.131) |
| Constant | 0.447*** | 0.510*** | 1.811*** | 2.604*** |
| | (.064) | (.059) | (.509) | (.515) |
| Unconditional compliance rate | .752 | | | |
| N | 2130 | 3050 | 2130 | 3050 |

*Note*: This table estimates for compliance. Compliance is defined as attending rehab centers at least once (one month). The estimation is made within the treated group only, and is shown for the two main samples: 18-months sample and 24-months sample . The second outcome examined is the number of months during which the individuals attended rehab (including zero). Recall that the conventional duration of the program is around 5 months.

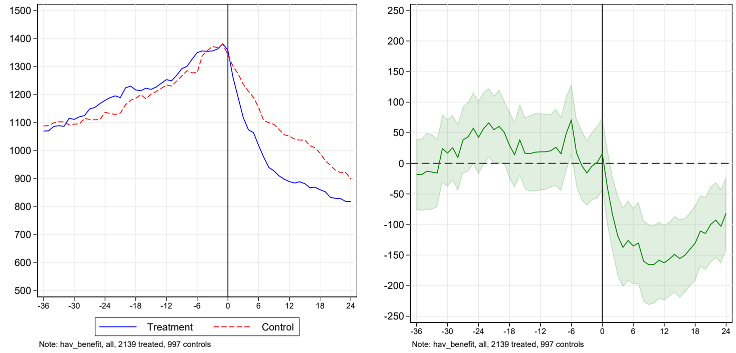
Table 4: Outcomes, at the participant level after 18 & 24 months after allocation

| | Outcomes at 18 for 18-months sample | | | Outcomes at 24 for 24-months sample | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ***A. Outcomes at t=24 or t=18*** | | | | | | |
| Employed | 0.023* | 0.020 | 0.015 | 0.037** | 0.032** | 0.030* |
| | [0.013] | [0.013] | [0.013] | [0.016] | [0.016] | [0.015] |
| | 0.227 | 0.227 | 0.227 | 0.194 | 0.194 | 0.194 |
| GIS (any) | -0.051*** | -0.051*** | -0.054*** | -0.055*** | -0.050*** | -0.055*** |
| | [0.017] | [0.017] | [0.016] | [0.019] | [0.019] | [0.019] |
| | 0.565 | 0.565 | 0.565 | 0.535 | 0.535 | 0.535 |
| DI (any) | 0.017 | 0.016 | 0.018* | 0.015 | 0.015 | 0.017 |
| | [0.011] | [0.010] | [0.010] | [0.013] | [0.012] | [0.012] |
| | 0.157 | 0.157 | 0.157 | 0.182 | 0.182 | 0.182 |
| ***B. Cumulative outcomes* (accumulated from 0 to 24 or 18)** | | | | | | |
| Cumulative Months employed | 0.73*** | 0.67*** | 0.56*** | 0.95*** | 0.82*** | 0.73*** |
| | (0.17) | (0.17) | (0.16) | (0.27) | (0.27) | (0.24) |
| | 3.42 | 3.42 | 3.42 | 4.43 | 4.43 | 4.43 |
| Cumulative Earnings | 2,896*** | 2,546*** | 2,246*** | 5,255*** | 4,652*** | 4,489*** |
| | (812) | (785) | (719) | (1191) | (1171) | (1072) |
| | 11333 | 11333 | 11333 | 14090 | 14090 | 14090 |
| Cumulative Income Support | -1,804*** | -2,066*** | -2,274*** | -2,859*** | -3,054*** | -3,340*** |
| | (569) | (543) | (412) | (881) | (828) | (639) |
| | 20980 | 20980 | 20980 | 26905 | 26905 | 26905 |
| Cumulative Disability benefits | 454 | 403 | 506 | 1,677** | 1,724** | 1,832** |
| | (544) | (533) | (517) | (805) | (779) | (764) |
| | 6688 | 6688 | 6688 | 9535 | 9535 | 9535 |
| Cumulative Total Benefits | -1,206* | -1,523*** | -1,640*** | -991 | -1,139 | -1,325 |
| | (628) | (585) | (543) | (965) | (891) | (826) |
| | 27721 | 27721 | 27721 | 36594 | 36594 | 36594 |
| Cumulative Total income | 1,690** | 1,023 | 606 | 4,263*** | 3,513*** | 3,164*** |
| | (830) | (791) | (702) | (1153) | (1106) | (962) |
| | 39055 | 39055 | 39055 | 50684 | 50684 | 50684 |
| *N* | *4355* | *4355* | *4355* | *3130* | *3130* | *3130* |
| Soc-demog controls | | X | X | | X | X |
| EmpHist controls | | | X | | | X |

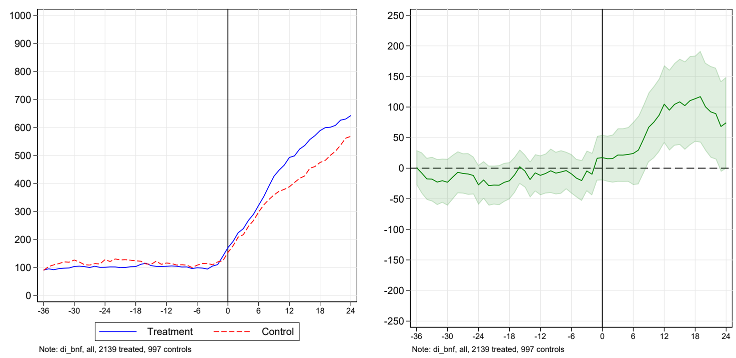
*Note*: all specifications shown include fixed effects for allocation cells. For every outcome the table shows estimates (top row), clustered standard errors in parenthesis (second row), and the control mean – in bottom row. All sums reported include zeros.

Figure 1: The treatment effect on main Benefits [-36,24]

A. Guaranteed Income Support benefits



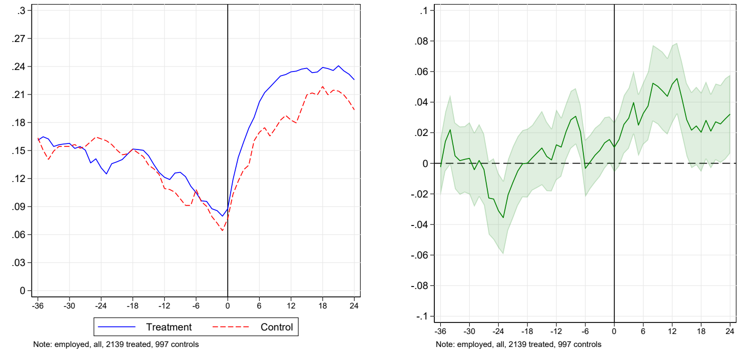

B. Disability Insurance benefits



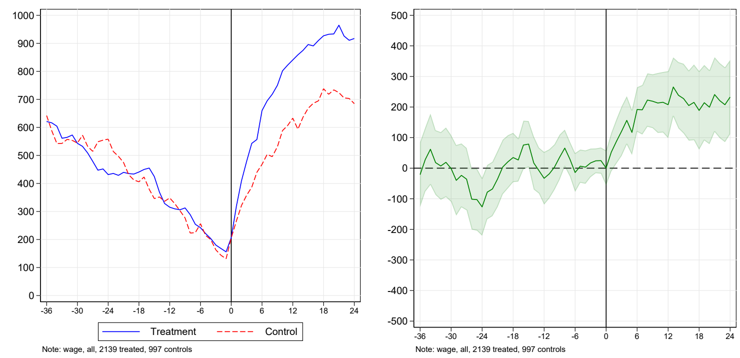

*Note*: This figure shows levels (left) and treatment effect (right) for the income originating in income support (top panel) and disability insurance (bottom) benefits. The period includes 36 months before- and 24 months after- allocation to the program. The results are based on regressions ran separately for each month.

Figure 2: The treatment effect on participants' employment & earnings [-36,24]

A. Employment

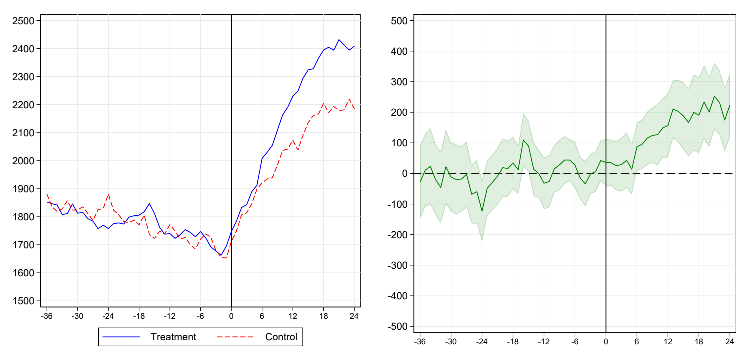



B. Earnings



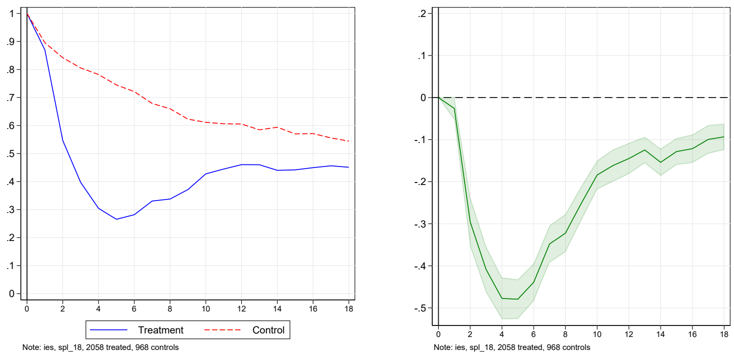

*Note*: This figure shows levels (left) and treatment effect (right) for the probability to be employed (top panel) and earnings (bottom). The period includes 36 months before- and 24 months after- allocation to the program. The results are based on regressions ran separately for each month.

Figure 3: The treatment effect on participants' Total Income [-36,24]



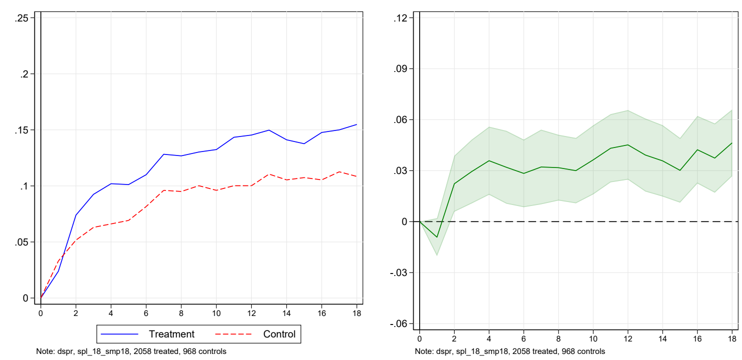

*Note*: This figure shows levels (left) and treatment effect (right) for the total income originating from all sources: earnings and benefits. The period includes 36 months before- and 24 months after- allocation to the program. The results are based on regressions ran separately for each month.

Figure 4: The treatment effect on the probability to attend IES offices




*Note*: This figure shows levels (left) and treatment effect (right) for the probability to attend the IES offices. The period includes the 18 months after allocation to the program. The results are based on regressions ran separately for each month. A shorter horizon is used due to the need to meet additional conditions (that is, to have sufficient horizon observed in terms of both the NII and the IES data).

Figure 5: The treatment effect on the probability to disappear




*Note*: This figure shows levels (left) and treatment effect (right) for the probability to *disappear*, where *disappearance* means being without any source of formal income (earnings or benefits), and also not attending the IES offices or Rehab centers. The period includes the 18 months after allocation to the program. The results are based on regressions ran separately for each month. A shorter horizon is used due to the need to meet additional conditions (that is, to have sufficient horizon observed in terms of both the NII and the IES data).

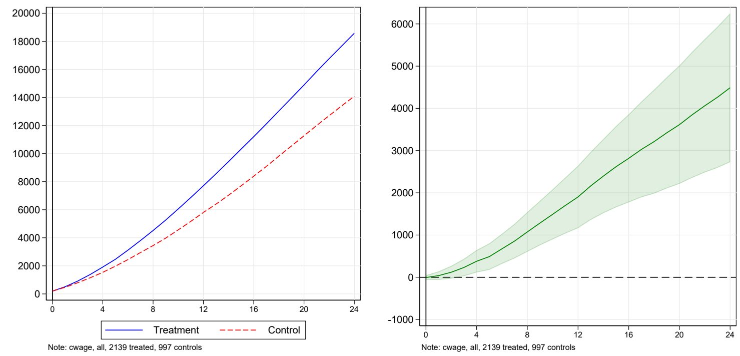
Table 5: Cumulative outcomes [0,24], by marital status

| | All | | Singles | Married, excld. both in prog. | | |
|---|---|---|---|---|---|---|
| | *participant* | *household* | *partic.* | *partic.* | *spouse* | *house.* |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Number of | 0.73*** | 0.55 | 0.41 | 1.09** | 0.07 | 1.16 |
| Months | [0.24] | [0.34] | [0.34] | [0.47] | [0.67] | [0.80] |
| employed | 4.43 | 8.23 | 4.76 | 3.95 | 9.5 | 13.45 |
| | | | | | | |
| Cumulative | 4,489*** | 5,213*** | 4,003*** | **4,204 | 3,107 | *7,310 |
| Earnings | [1072] | [1713] | [1375] | [2107] | [3894] | [4430] |
| | 14090 | 27886 | 14270 | 14202 | 35248 | 49450 |
| | | | | | | |
| DI (any) | 0.017 | | .015 | 0.028 | 0.021 | |
| | [0.012] | | [.017] | [0.023] | [0.014] | |
| | 0.182 | | .206 | 0.15 | 0.047 | |
| | | | | | | |
| Cumulative | 1,832** | 2,907*** | 1,497 | 2,230 | 1,706 | **3,936 |
| Disability | [764] | [851] | [951] | [1582] | [1113] | [1898] |
| benefits | 9535 | 11055 | 9842 | 9819 | 3673 | 13492 |
| | | | | | | |
| Cumulative | -3,340*** | -4,943*** | -3,278*** | -3,281*** | -3922*** | -7,204*** |
| Income | [639] | [784] | [868] | [1045] | [927] | [1607] |
| Support | 26905 | 35187 | 30,022 | 22221 | 19113 | 41334 |
| | | | | | | |
| Cumulative | -1325 | -3137*** | -1653 | -776 | -4544*** | -5320*** |
| Total | [826] | [956] | [1008] | [1514] | [905] | [1946] |
| Benefits | 36594 | 45329 | 40,042 | 32175 | 20365 | 52540 |
| | | | | | | |
| Cumulative | 3,164*** | 2,076 | 2,350* | 3,427 | -1437 | 1,991 |
| Total | [962] | [1479] | [1256] | [2184] | [3508] | [3756] |
| income | 50684 | 73215 | 54312 | 46377 | 55613 | 101990 |
| | | | | | | |
| N | 3130 | 3130 | 1893 | 1,065 | 1,065 | 1,065 |

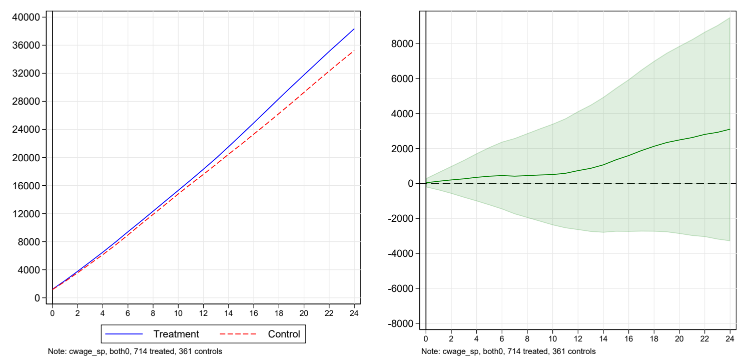
Note: This table shows estimates for different levels: participants, spouses and households. "married" here includes cohabitation. For every outcome the table shows estimates (top row), clustered standard errors in parenthesis (second row), and the control mean – in bottom row. All sums reported include zeros.

Figure 6: The treatment effect on cumulative earnings, across different levels of analysis

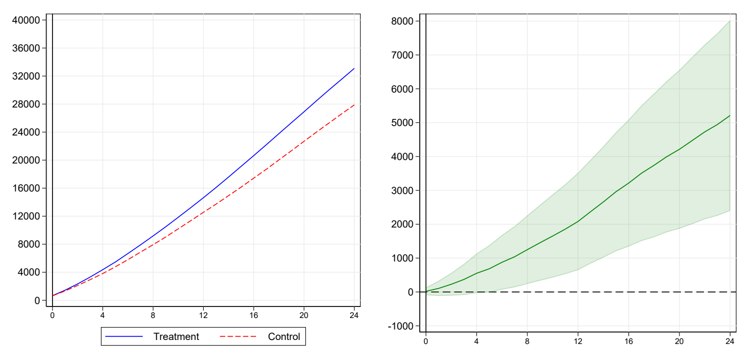
A. Among program participants
(both married and unmarried)




B. Among non-participating spouses



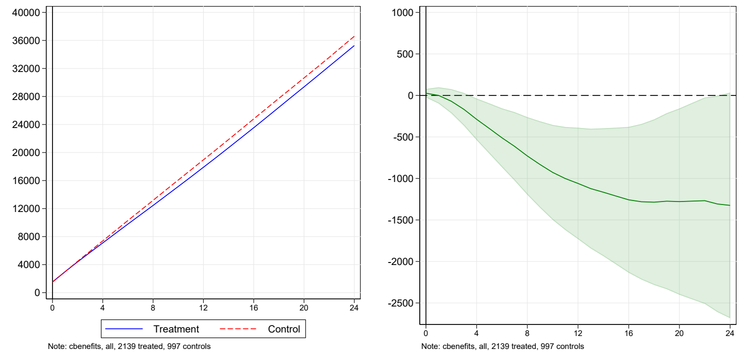

C. Among Households of all types



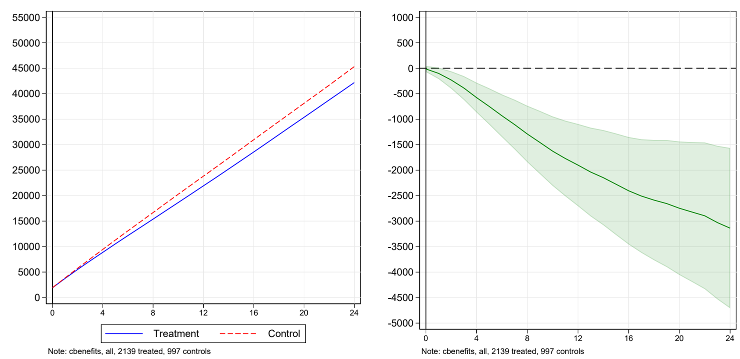

*Note*: This figure shows levels (left) and treatment effect (right) for earnings accumulated during the 24 months after allocation to the program among different levels of analysis: participants, spouses and households.

Figure 7: The treatment effect on cumulative benefits [0,24]

A. Among participants



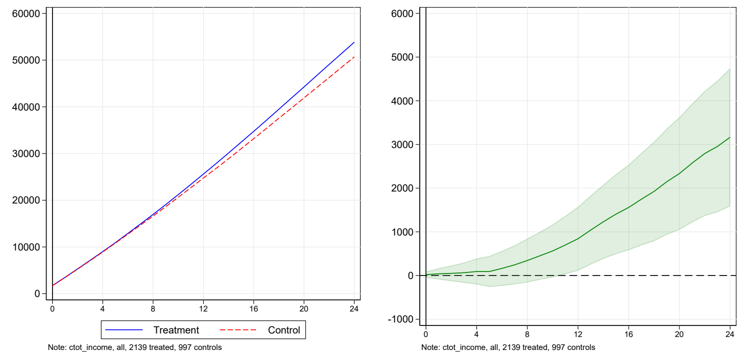

B. Among households



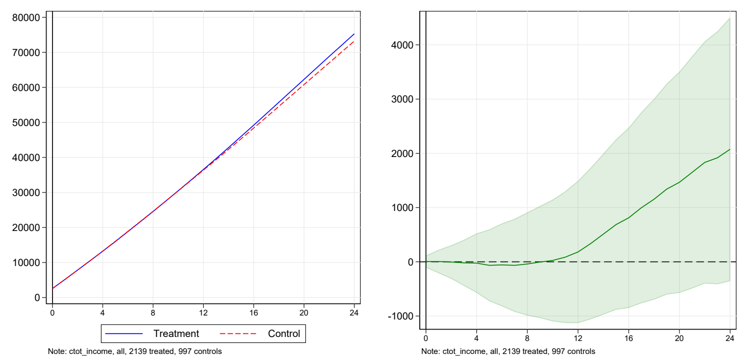

Note: This figure shows levels (left) and treatment effect (right) for benefits accumulated during the 24 months after allocation to the program among participants and households, where *households* include both single-headed and two-adults' households.

Figure 8: Effect on Cumulative Total income [0,24]

A. Among participants



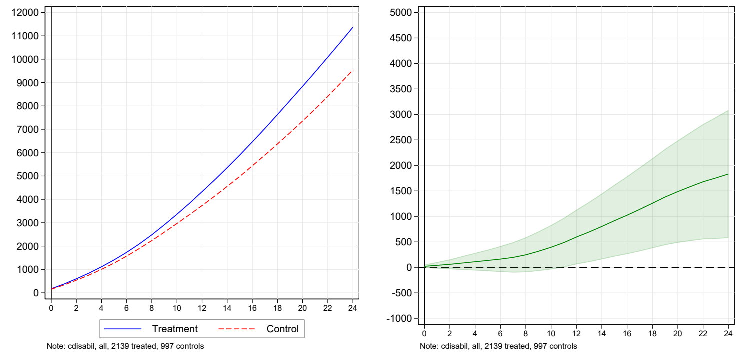

B. Among households



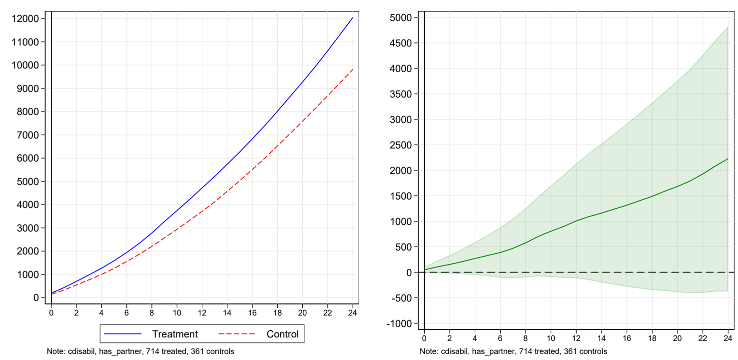

Note: This figure shows levels (left) and treatment effect (right) for the total income (earnings plus benefits) accumulated during the 24 months after allocation to the program among participants and households, where *households* include both single-headed and two-adults' households.

Figure 9: Cumulative DI income

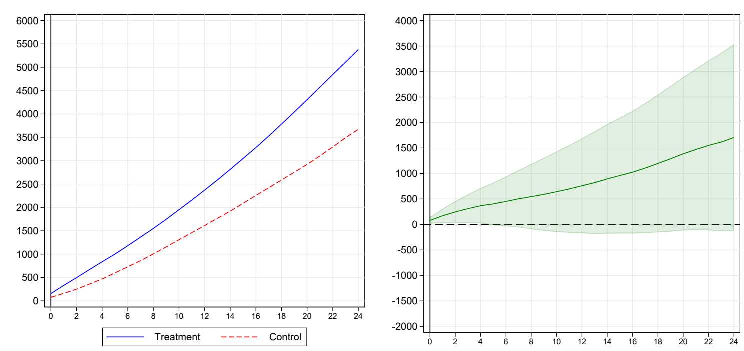
A. Among program participants
(both married and unmarried)




B. Among married participants
(excluding participating spouses)




C. Among non-participating spouses




*Note*: This figure shows levels (left) and treatment effect (right) for DI income accumulated during the 24 months after allocation to the program among different levels of analysis: participants, spouses and households.

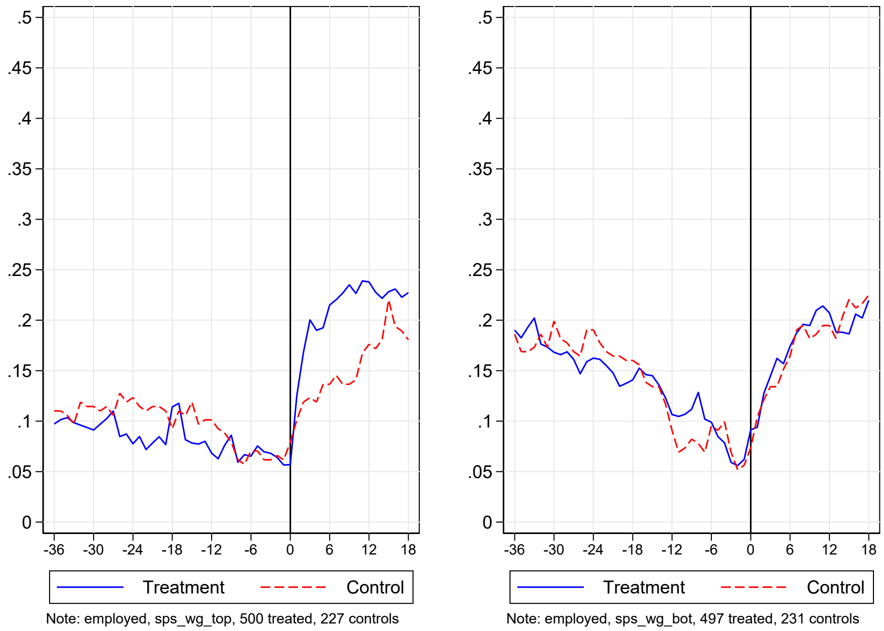
Table 6: Participant's outcomes, by spouse pre-treatment earnings

| Participant outcomes | Spouse pre-treatment earnings after 18 months (18-months sample) | | Spouse pre-treatment earnings after 24 months (24-months sample) | |
|---|---|---|---|---|
| | Below median (1) | Above median (2) | Below median (1) | Above median (2) |
| Employed | -0.005 | 0.047 | 0.022 | 0.079 |
| | [0.040] | [0.036] | [0.043] | [0.049] |
| | 0.225 | 0.181 | 0.186 | 0.146 |
| | *0.22* | *0.228* | *0.208* | *0.225* |
| Cumulative months employed | 0.041 | 1.244*** | 0.425 | 1.777*** |
| | [0.506] | [0.481] | [0.766] | [0.686] |
| | 3.286 | 2.855 | 4.366 | 3.528 |
| | *3.327* | *4.099* | *4.79* | *5.31* |
| Cumulative Earnings | 1,633 | 3,582** | 3,340 | 5,147* |
| | [2250] | [1763] | [3815] | [3088] |
| | 11278 | 9305 | 15621 | 12742 |
| | *12910* | *12887* | *18,961* | *17,889* |
| Cumulative GIS | -2664*** | -1476 | -3080*** | -3123* |
| | [790] | [989] | [1130] | [1616] |
| | 15005 | 19642 | 19531 | 24986 |
| | *12,341* | *18,166* | *16,451* | *21,863* |
| Cumulative DI | 379 | 1,099 | 955 | 1,626 |
| | [1649] | [1692] | [2659] | [2732] |
| | 7634 | 5945 | 10242 | 9384 |
| | *8,013* | *7,043* | *11,197* | *11,010* |
| $N_c$ | 231 | 227 | 183 | 178 |
| N | 694 | 689 | 516 | 509 |

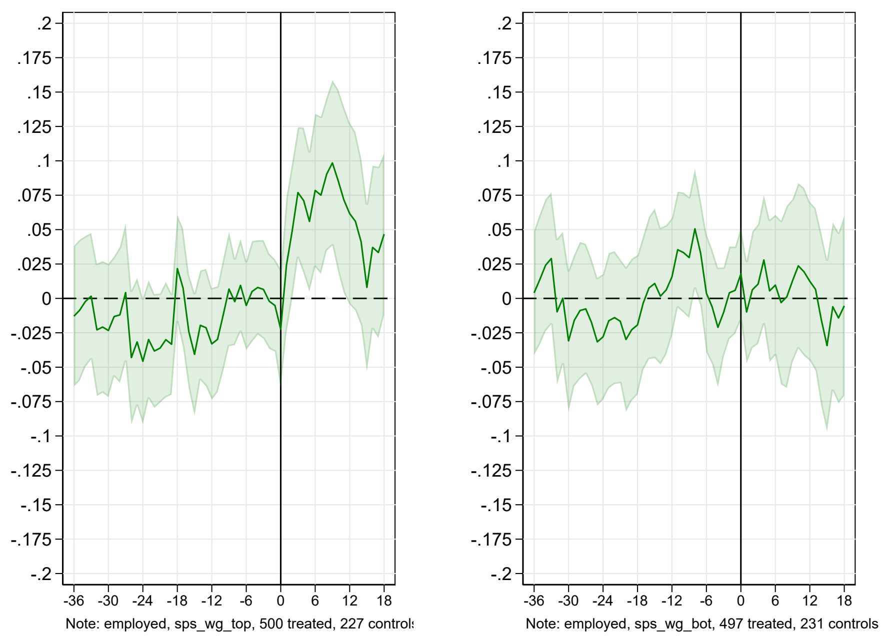
*Note*: This table shows estimates for main outcomes among married participants stratified by spouse pre-treatment earnings (below and above median). Estimates are shown in the two main samples – 18-months sample and 24-months sample. For every outcome the table shows estimates (top row), clustered standard errors in parenthesis (second row), the control mean in the third row, and the treatment mean, in italics, at the bottom row. All sums reported include zeros.

Figure 10: Participants' employment, by spouse's pre-treatment wage position: spouse's wage *above* (left) and *below* (right) median

A. Levels



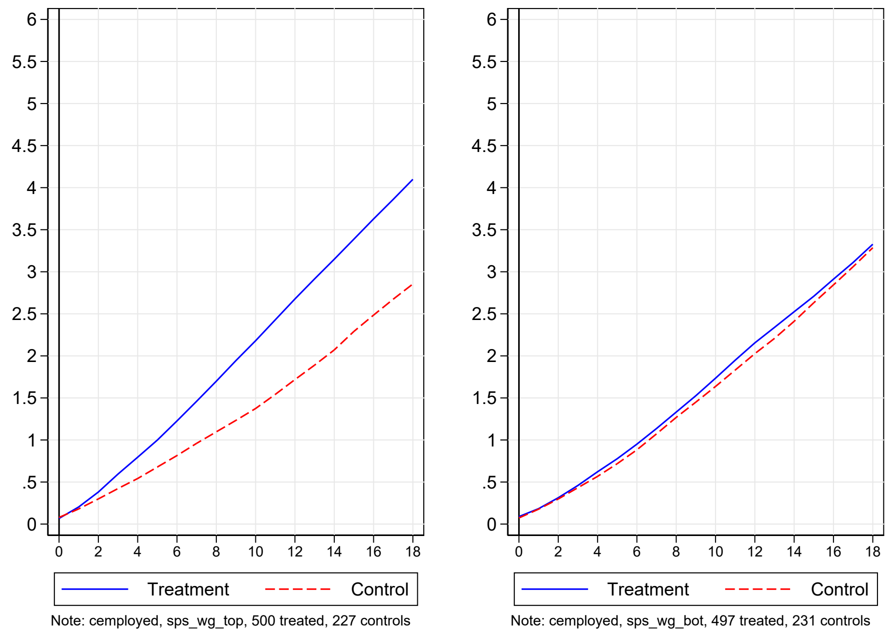

B. Treatment effect



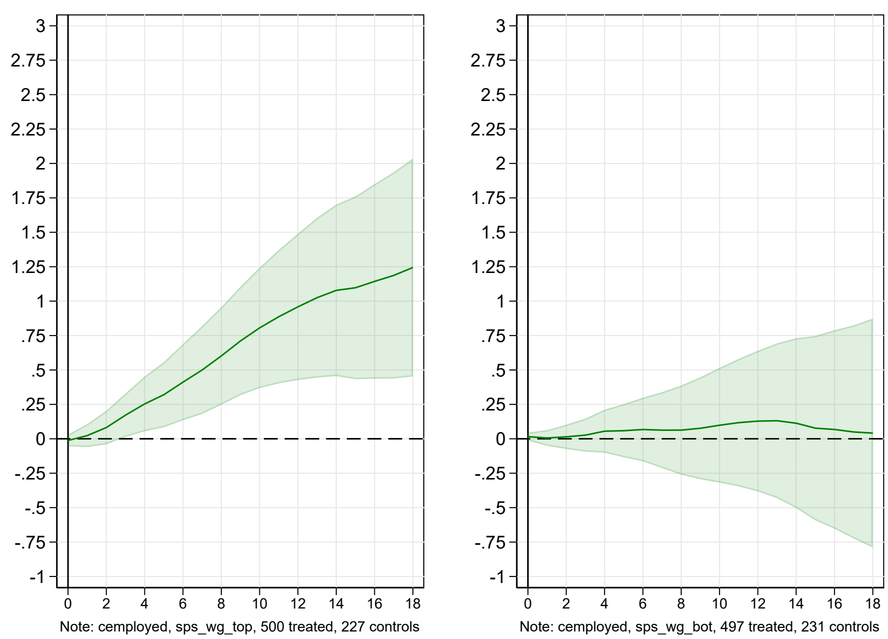

*Note*: This figure shows levels (top panel) and treatment effect (bottom) for the probability to be employed during the 18 months after allocation to the program. The two groups shown are participants whose spouse earned, pre-treatment, above (left) or below (right) median income. We use shorter horizon in order to have a large enough samples.

Figure 11: Participants' cumulative employment, by spouse's pre-treatment wage position: spouse's wage *above* (left) and *below* (right) median

A. Levels




B. Treatment effect



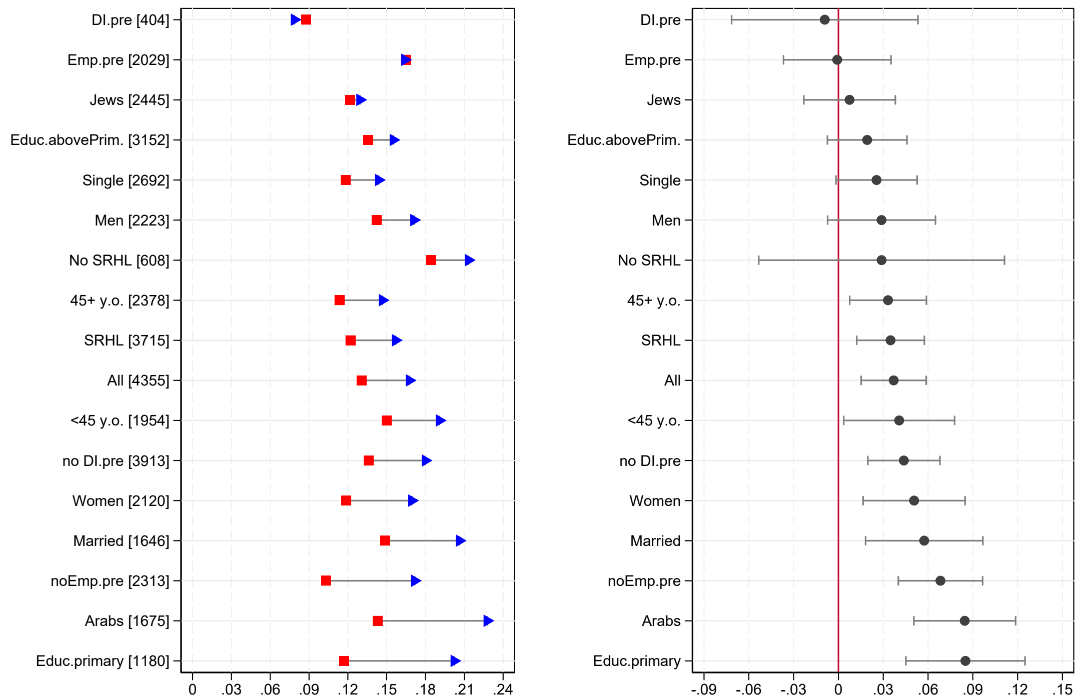

*Note*: This figure shows levels (top panel) and treatment effect (bottom) for the cumulative number of months employed during the 18 months after allocation to the program. The two groups shown are participants whose spouse earned, pre-treatment, above (left) or below (right) median income. We use shorter horizon in order to have a large enough samples.

Table 7: Cumulative outcomes, at the participant level [0,24], by sub-group

| | All | Nationality | | Gender | | Primary Education | |
|---|---|---|---|---|---|---|---|
| | | Arab | Jew | Men | Women | up to | Above |
| Employed | 0.030* | 0.000 | 0.047*** | 0.044* | 0.012 | 0.048* | 0.013 |
| | [0.015] | [0.026] | [0.018] | [0.023] | [0.022] | [0.027] | [0.019] |
| | 0.194 | 0.203 | 0.178 | 0.159 | 0.228 | 0.134 | 0.217 |
| Cumulative | 0.73*** | 0.33 | 0.87*** | 0.90** | 0.62 | 1.33*** | 0.436 |
| Months employed | (0.24) | (0.36) | (0.32) | (0.35) | (0.38) | (0.43) | [0.319] |
| | 4.43 | 4.48 | 4.41 | 3.81 | 5.04 | 2.83 | 5.037 |
| Cumulative | 4,489*** | 1,883 | 5,241*** | 5,268*** | 3,837*** | 6,249*** | 3,764*** |
| Earnings | (1072) | (1611) | (1420) | (1777) | (1403) | [1494] | [1410] |
| | 14090 | 14359 | 14232 | 15219 | 12973 | 8733 | 16151 |
| Receives GIS | -0.055*** | -0.049 | -0.051** | -0.083*** | -0.025 | -0.048 | -0.061*** |
| | [0.019] | [0.032] | [0.024] | [0.024] | [0.029] | [0.032] | [0.021] |
| | 0.535 | 0.517 | 0.548 | 0.530 | 0.539 | 0.567 | 0.522 |
| Cumulative | -3,340*** | -2,463** | -3,819*** | -3,803*** | -2,800*** | -3,084*** | -3317*** |
| Income Support | (639) | (961) | (805) | (710) | (1032) | (1107) | [680] |
| | 26905 | 23295 | 29353 | 23619 | 30159 | 27197 | 26793 |
| Receives DI | 0.017 | 0.013 | 0.027* | 0.055*** | -0.021 | 0.018 | 0.017 |
| | [0.012] | [0.021] | [0.016] | [0.019] | [0.020] | [0.023] | [0.015] |
| | 0.182 | 0.155 | 0.197 | 0.173 | 0.190 | 0.159 | 0.190 |
| Cumulative | 1,832** | 757 | 3,081*** | 4,491*** | -575 | 930 | 1,876* |
| Disability benefits | (764) | (1301) | (1019) | (1273) | (962) | (1542) | [1032] |
| | 9535 | 8530 | 9779 | 9934 | 9141 | 8702 | 9856 |
| Cumulative | -1,325 | -1,538 | -543 | 951 | -3,303*** | -1,894 | -1,271 |
| Total Benefits | (826) | (1365) | (1092) | (1325) | (970) | (1690) | [998] |
| | 36594 | 31929 | 39333 | 33684 | 39474 | 35933 | 36848 |
| Cumulative | 3,164*** | 345 | 4,698*** | 6,219*** | 534 | 4,355** | 2,493* |
| Total income | (962) | (1420) | (1289) | (1710) | (1420) | [2052] | [1282] |
| | 50684 | 46289 | 53565 | 48903 | 52447 | 44666 | 52999 |
| N | 3130 | 1269 | 1721 | 1575 | 1548 | 858 | 2127 |

*Note*: this table estimates using the main specification, stratified by main sociodempgraphic attributes. For every outcome the table shows estimates (top row), clustered standard errors in parenthesis (second row), and the control mean – at the bottom row. All sums reported include zeros.

Figure 12: Effect on Disappearance, by sub-group (after 18 months form allocation)




*Note*: this figure shows control mean, and the corresponding value among the treated (control mean + estimate] and the treatment effect on the outcome "disappear", that captures the situation in which a person has no formal income from either earnings or benefits, and he also does not attend IES offices or rehab centers. The left panel shows values for the control (squares) and the treatment (triangle) groups; and the right panel shows estimates with 95% confidence interval. The groups presented are (from top): "DI.pre" = Any Disability benefits within the 3 years pre-treatment; "Emp.pre" = any Employment spell within the 3 years pre-treatment; "SRHL" = Self-Reported Health-Limitation; "y.o." = years old; "Educ.abovePrim" = education level above primary school.

Table 8: Share receiving Disability benefits after 24 months from treatment

| Group | Men | | Women | | Jews | | Arabs | |
|---|---|---|---|---|---|---|---|---|
| | share | % DI | share | % DI | share | % DI | share | % DI |
| Control | .314 | *.173* | .322 | *.190* | .316 | *.201* | .321 | *.155* |
| Treatment, not attended | .165 | *.189* | .173 | *.138* | .166 | *.209* | .172 | *.099* |
| Treatment, attended | .521 | *.235* | .505 | *.179* | .518 | *.231* | .507 | *.173* |
| Total / average | 1.0 | *.208* | 1.0 | *.175* | 1.0 | *.218* | 1.0 | *.154* |
| N | 1,579 | | 1,557 | | 1,845 | | 1,291 | |

*Note*: This table shows the share of participants receiving disability insurance after 24 months from allocation to the program, in the control group, and among treated participants who have attended rehab and among treated who have not – across segments of ethnicity and gender. For each segment the left column shows the relative share of the group (the control group is always around 31-32%); and the right column shows the share of people receiving DI.

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# A Online appendix

## A.1 Results, by gender x ethnicity

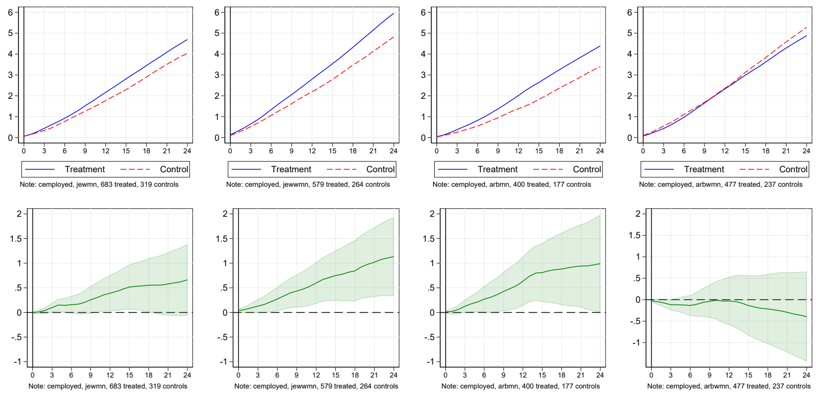
Figure A13 shows trajectories and treatment effect on cumulative employment, cumulative earnings and cumulative disability – among four distinct groups in the intersection of gender and ethnicity. Interestingly, the treatment effect on employment and earnings among Arab women is zero. Among Jewish women the effect on both employment and earnings is the strongest across the four groups.

As for disability benefits (panel C), there is a strong difference between men and women: there is no effect among both Jewish and Arab women; and the strongest effect can be observed among Jewish men, while among Arab men the effect is positive and marginally significant.

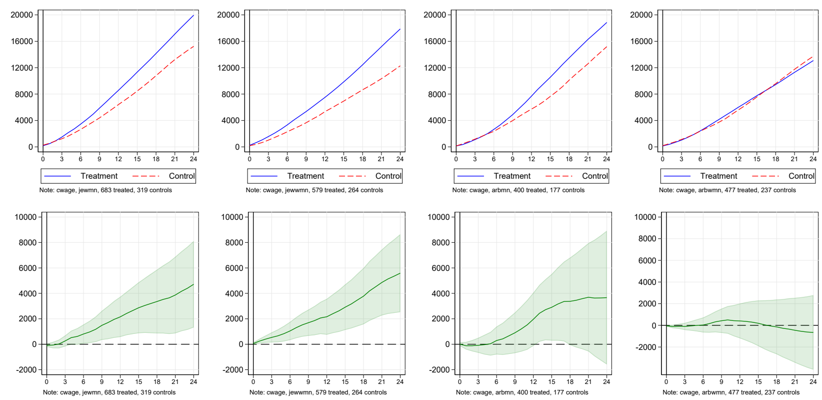
Finally, figure A14 shows trajectories and the treatment effect among the same four groups. The trajectory and the corresponding treatment effect on the probability to attend IES offices is rather similar across the four groups, and the strongest gap between the treatment and the control groups after 18 months is evident among Arab women (panel A). Most importantly, figure A14b shows that the effect on disappearance is triggered exclusively by Arab women.

Figure A13: Effect on employment & earnings, by ethnicity & gender [0,24]

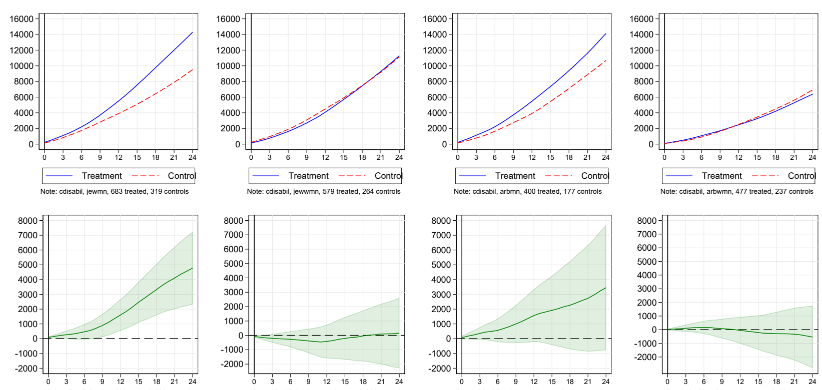
*A. Cumulative employment*




*B. Cumulative earnings*



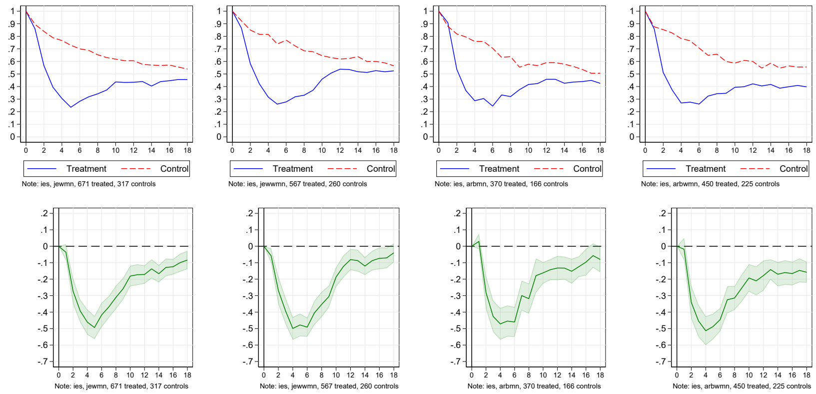

*C. Cumulative Disability insurance income*



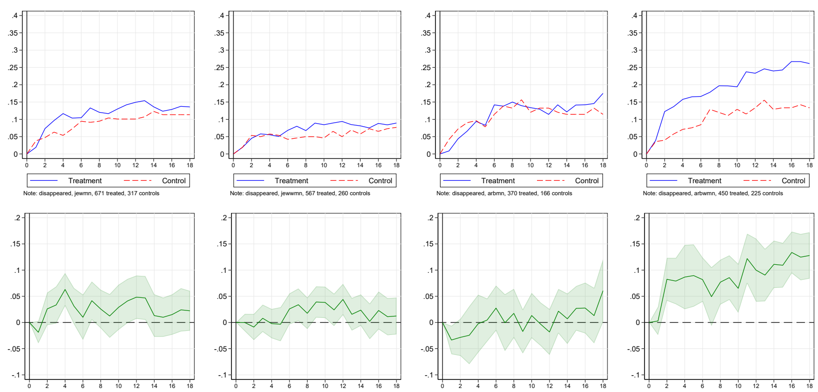

*Note*: This figure shows levels (top in each panel) and treatment effect (bottom) for cumulative employment (panel A), cumulative earnings (panel B) and cumulative DI income (panel C) – by ethnicity & gender, by the following order, from left to right: Jewish men, Jewish women, Arab men, and Arab women. The period includes 24 months after allocation to the program. The results are based on regressions ran separately for each month.

Figure A14: Effect on Attendance at the IES and on Probability to disappear, by ethnicity & gender [0,24]

*A. Attendance at the IES*




*B. Probability to disappear*




*Note*: This figure shows levels (top in each panel) and treatment effect (bottom) for attendance in IES offices (panel A), and the probability to disappear (panel B) – by ethnicity & gender, by the following order, from left to right: Jewish men, Jewish women, Arab men, and Arab women. The period includes 24 months after allocation to the program. The results are based on regressions ran separately for each month.

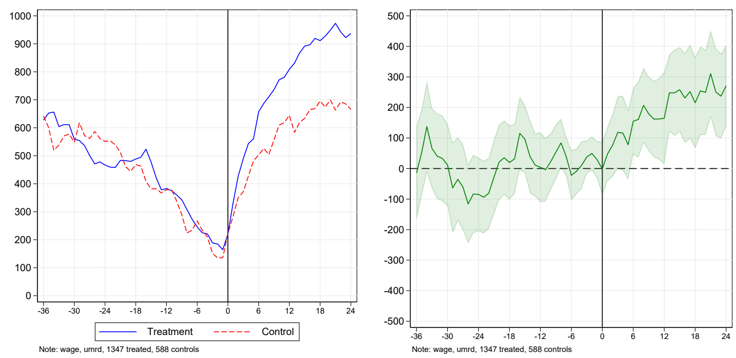
## A.2 Results, by marital status

Figure A15 shows earnings trajectory and treatment effect for participants by marital status. The figure shows that the treatment effect is stronger and persistent among single participants, relative to married participants (the treatment effect may be reflecting either a change through the intensive- or the extensive margin, or some of both). This is consistent with the stronger constrains experienced by single participants, that do not enjoy income insurance provided by spouses.

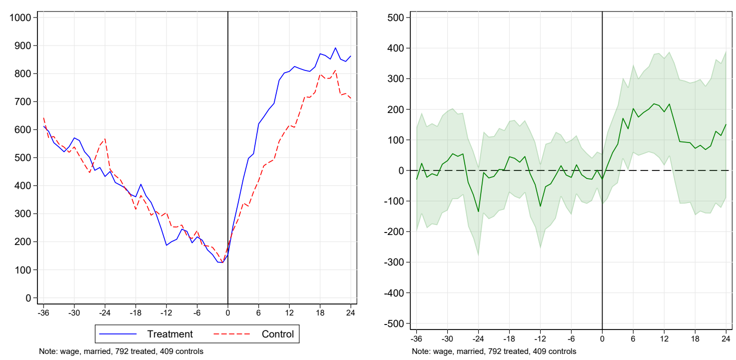
Figure A16 shows trajectories and estimated effect on cumulative employment. In this case, the effect is stronger among those *married*. This result lends support to the possibility that the treatment effect on earnings (as shown in figure A15) was triggered through the intensive margin. In addition, and while not intuitive at first sight, this result is also consistent with the explanation that focuses on constrains. That is because the stronger effect among those married does not reflect a lower level (of cumulative employment) among treated singles: they actually do better than their married (treated) counterparts. Instead, the stronger treatment effect among those married reflects the higher level of cumulative employment among *singles in the control group*. This is again consistent with the fact that singles are constrained to a greater extent, relative to married participants.

Figure A15: Effect on participants' earnings, by marital status [-36,24]

A. Singles



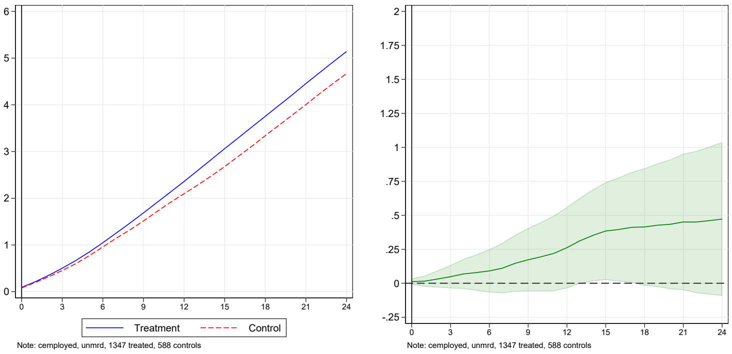

B. Married



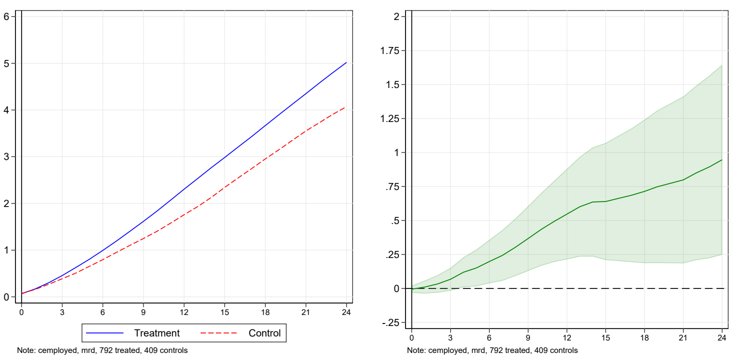

*Note*: This figure shows levels (left) and treatment effect (right) for earnings (bottom) – by marital status. The period includes 36 months before- and 24 months after- allocation to the program. The results are based on regressions ran separately for each month.

Figure A16: Effect on participants' cumulative employment, by marital status [-36,24]

A. Singles




B. Married




*Note*: This figure shows levels (left) and treatment effect (right) for cumulative employment (bottom) – by marital status. The period includes 36 months before- and 24 months after- allocation to the program. The results are based on regressions ran separately for each month.

## A.3 Covid period outcomes

While our main analysis uses outcomes that exclude the Covid-period, studying outcomes for this period is important for two reasons. First, it enables to further stretch the horizon of analysis up to 36 months from allocation. Second, the crisis created an exogenous economic shock and thus enables examination of the immunity obtained by individuals assigned to the program.

Table A9 shows results for outcomes before and after the Covid shock (March 2020) – for the 24-months sample. Different from the main analysis, outcomes in this case are not aligned to the month of allocation-to-program (that are unique to each program cohort), but are rather aligned on a calendar axis, relative to the month of crisis. Table A10 shows similar results for the h12 sample that includes people allocated up to December 2018, i.e., the entire population of the program.[14]

[14] This means that the first outcome measured, at January 2019, is only one month away for the youngest program cohort (allocated in December 2018) and 18 months after allocation for the oldest cohort (allocated in June 2017).

The results show that the impact of the program is preserved over the first year of the Covid crisis for GIS receipt: the probability to receive GIS benefits among the treated it is lower by 4-5 percentage points relative to the control (rather similar across both samples). The program's impact on employment decreases during the crisis (stays insignificant in the 24-months sample, and slightly decreases in the h12 sample). Also, UIB receipt, that is neglectable pre-crisis (lower than 1% of participants), increases to around 5-6% of the control group throughout the crisis, and is significantly higher, by 2-3 percentage points, among the treatment group. This is in line with a positive effect on employment in the pre-crisis period, as previous employment is a precondition for UIB receipt.

Figure A17 shows dynamic estimates (based on the 24-months sample) for five different outcomes from January 2014 to December 2020, that is, 3.5 years before the first allocation to the program, and 10 months into the crisis. The period during which people are allocated to the program is shaded. The figure shows that, overall, the treatment effect is stable when moving from pre- to post-crisis period for GIS and DI; but it shrinks for employment and earnings; and increases for UIB.

Table A9: Covid period outcomes – ***24-months sample***

| | Pre-crisis | | Post-crisis | | |
|---|---|---|---|---|---|
| Month | 2019m1 | 2020m1 | 2020m4 | 2020m8 | 2020m12 |
| Employed | 0.027* | 0.026* | 0.014 | 0.018 | 0.015 |
| | [0.015] | [0.015] | [0.014] | [0.014] | [0.015] |
| | 0.214 | 0.223 | 0.135 | 0.147 | 0.143 |
| Earnings | 205*** | 201*** | 105 | 136* | 76 |
| | [69.958] | [75] | [69] | [78] | [76] |
| | 685 | 724 | 523 | 576 | 593 |
| GIS | -0.087*** | -0.053** | -0.048** | -0.045** | -0.052*** |
| | [0.020] | [0.021] | [0.020] | [0.019] | [0.018] |
| | 0.611 | 0.522 | 0.541 | 0.53 | 0.511 |
| UIB | 0.003** | 0.00 | 0.030*** | 0.027*** | 0.023** |
| | [0.001] | [0.004] | [0.009] | [0.007] | [0.009] |
| | 0 | 0.013 | 0.05 | 0.033 | 0.05 |
| DI | 0.032** | 0.021* | 0.02 | 0.02 | 0.014 |
| | [0.012] | [0.012] | [0.012] | [0.013] | [0.014] |
| | 0.137 | 0.187 | 0.199 | 0.207 | 0.214 |
| N | 3130 | 3130 | 3130 | 3130 | 3130 |

Note: this table shows estimates for calendar-aligned outcomes. For every outcome the table shows estimates (top row), clustered standard errors in parenthesis (second row), and the control mean – at the bottom row.

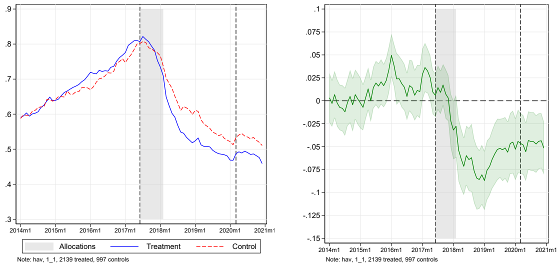
Table A10: Covid period outcomes – ***sample h12***

| | Pre-crisis | | Post-crisis | | |
|---|---|---|---|---|---|
| Month | 2019m1 | 2020m1 | 2020m4 | 2020m8 | 2020m12 |
| Employed | 0.043*** | 0.032*** | 0.025** | 0.026** | 0.025** |
| | [0.011] | [0.011] | [0.011] | [0.011] | [0.011] |
| | 0.202 | 0.232 | 0.136 | 0.159 | 0.151 |
| Earnings | 177.796*** | 172.270*** | 112.686** | 128.399** | 94.233 |
| | [50.906] | [55.925] | [52.898] | [56.104] | [58.214] |
| | 678.055 | 795.734 | 538.591 | 624.607 | 629.837 |
| GIS | -0.083*** | -0.059*** | -0.053*** | -0.049*** | -0.051*** |
| | [0.015] | [0.015] | [0.015] | [0.014] | [0.014] |
| | 0.643 | 0.536 | 0.56 | 0.548 | 0.523 |
| UIB | 0.003** | 0.002 | 0.018*** | 0.020*** | 0.019*** |
| | [0.001] | [0.003] | [0.007] | [0.006] | [0.007] |
| | 0.001 | 0.009 | 0.064 | 0.036 | 0.054 |
| DI | 0.013 | 0.017* | 0.018* | 0.016 | 0.014 |
| | [0.009] | [0.009] | [0.010] | [0.010] | [0.010] |
| | 0.12 | 0.167 | 0.179 | 0.186 | 0.192 |
| N | 5660 | 5660 | 5660 | 5660 | 5660 |

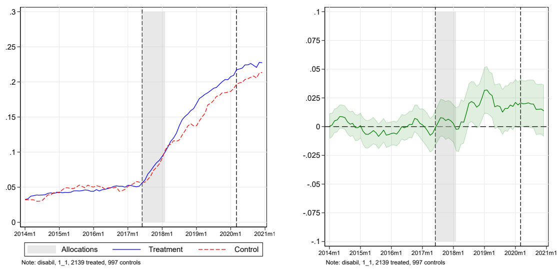
Note: this table shows estimates for calendar-aligned outcomes. For every outcome the table shows estimates (top row), clustered standard errors in parenthesis (second row), and the control mean – at the bottom row.

Figure A17: Trajectories and treatment effect on main outcomes in the Covid-19 period

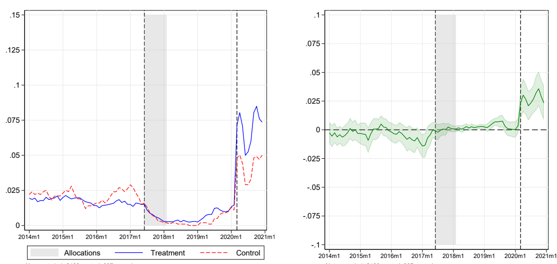
A. GIS



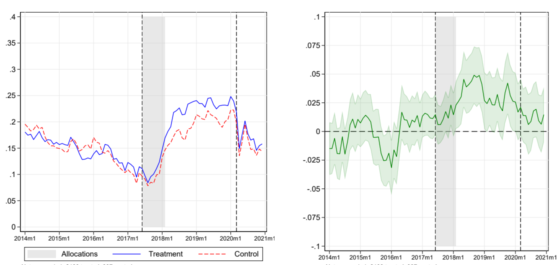

B. DI



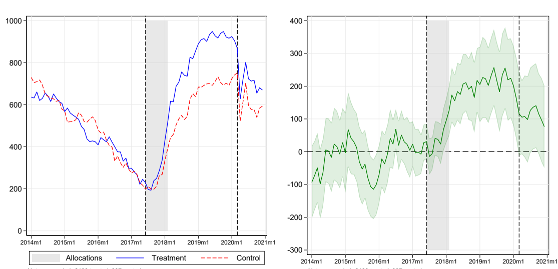

C. UIB




D. Employment




E. Earnings




Note: this figure show estimates (from separate regressions for each month) for outcomes aligned to calendar scale. In grey – the period during which the 24-months sample participants were allocated to the program.

## A.4 Household's employment dynamics over a decade

Figure A18 shows the employment and earnings trajectories for participants and spouses over 121 months, where outcomes are aligned to the calendar axis. The outcomes are shown as indices – to strengthen comparability[15] – where the average of year 2010 is used as reference level, defined as 1 (separately for each series). The decrease in the employment rates among participants – overall by 60%, relative to the 2010 baseline – is gradual and continues over the 8 years leading to treatment. As shown earlier, after treatment, the employment rate is gradually restored and reaches 90% relative to baseline, in the control group, and around 110% relative to baseline, among the treated.

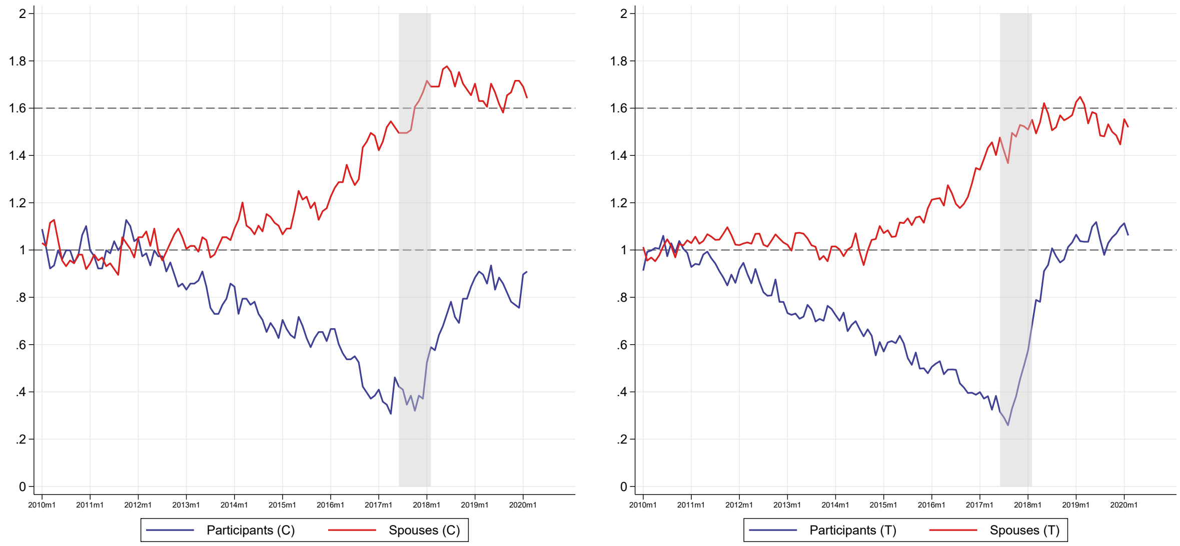
More importantly for this context, the employment rate among spouses starts climbing vis a vis the decrease in participants' employment, around 2 years pre-treatment – probably in order to compensate for the loss of earnings among (soon-to-be) participants. This increase starts slowing down and becomes flat around the time in which participants, in both groups, start going back to the labor market. Interestingly, the employment rate of spouses of the treated stabilizes around a lower level relative to spouses of control group members, so that the sum of employment rates at the household level is almost the same in both groups.

This result is very much in line with the findings of Autor et al. (2019), with regard to the different types of spousal response (Specifically, see detailed discussion in page 2639): in the years leading to DI application, applicants' earnings drop substantially, and the spouse of those whose application is denied increase their labor supply to compensate for the drop. In our case, participants are not only DI applicants, but rather include a more varied group of individuals that experienced a drop in employment and earnings, most of which gradually started to receive GIS up until being allocated to the program. At the same time their spouses increase their employment and earnings in order to compensate for this drop, and this compensation is re-adjusted rather quickly in response to the re-integration of participants after allocation to the program (recall that control group members also receive support by IES offices, hence they also gradually restore employment rates).

[15] When moving away (backward in time) from the month of allocation to program, the data on the different samples that are rather small in this case, becomes rather noisy

Figure A18: Calendar-aligned results over 121 months, among participants and spouses

***A. Employment index***




Note: Employment index (mean 2010=1.0)

***B. Earnings index***


Note: Wage index (mean 2010=1.0)

*Note*: this figure shows indices for employment and earnings among participants and spouses in the treatment and the control group (excluding participating spouses), between January 2010 and February 2020. The sample used is 24-months sample , which is the main sample used in the paper that includes people allocated to the program between June 2017 and February 2018. Outcomes are shown as indices where 1.0 is the mean value of 2010. In each panel, the series shown are the index for the control mean and the corresponding index for the computed treatment mean (control mean + treatment effect). Estimation is based on the main specification but outcomes are on a calendric scale rather than aligned to allocation.

## A.5 Heterogeneous effect: by allocation cohorts

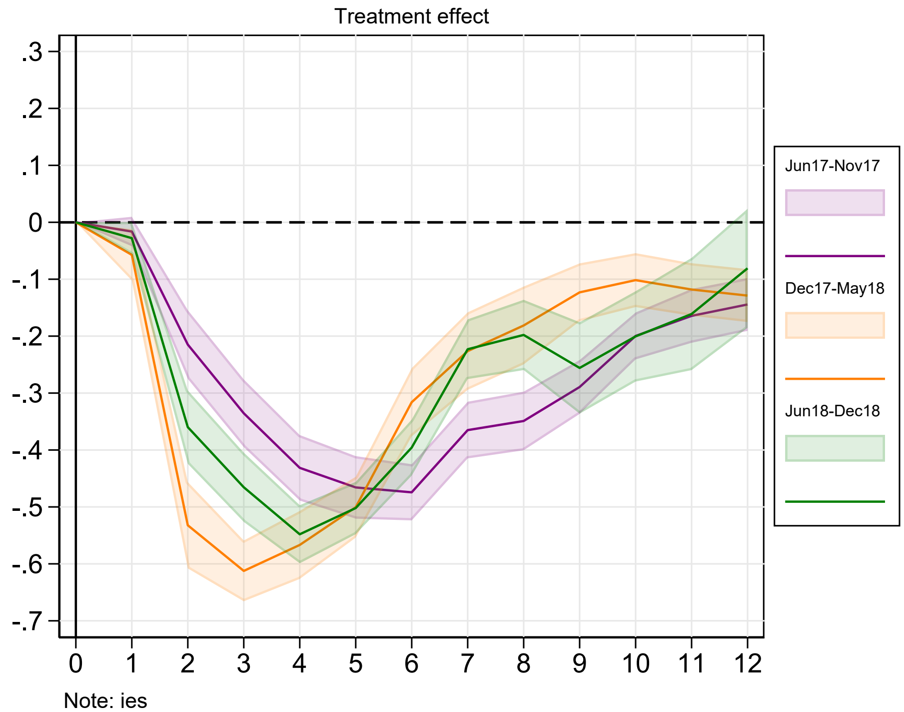
An additional important stratum is program cohorts. In this case, we split the participants of the program into three groups according to the period in which they were allocated to the program. Figure A19a shows the treatment effect, for this stratum, on the probability to attend the IES offices (Those that are not attending IES offices are in one of the following statuses: in rehab centers, employed, exempted from attending the IES while receiving benefits; or none of the above but without attending the IES). On the part of program participants this outcome reflects participants' dependence on governmental help. The results show a shallower trajectory of the treatment effect among early cohorts, allocated to the program between June and November 2017. This means that it took a longer while for these cohorts to disconnect from the IES, and to reach rehab centers, or to (re-)enter the labor market. Note however that this finding is not monotonic across the three cohort groups: the steepest curve is that of the middle cohort.

Figure A19b shows the treatment effect on cumulative employment over 14 months from allocation. In this case also the estimate among earliest cohort is significantly lower relative to older ones – and in this case the relationship *is* monotonic.

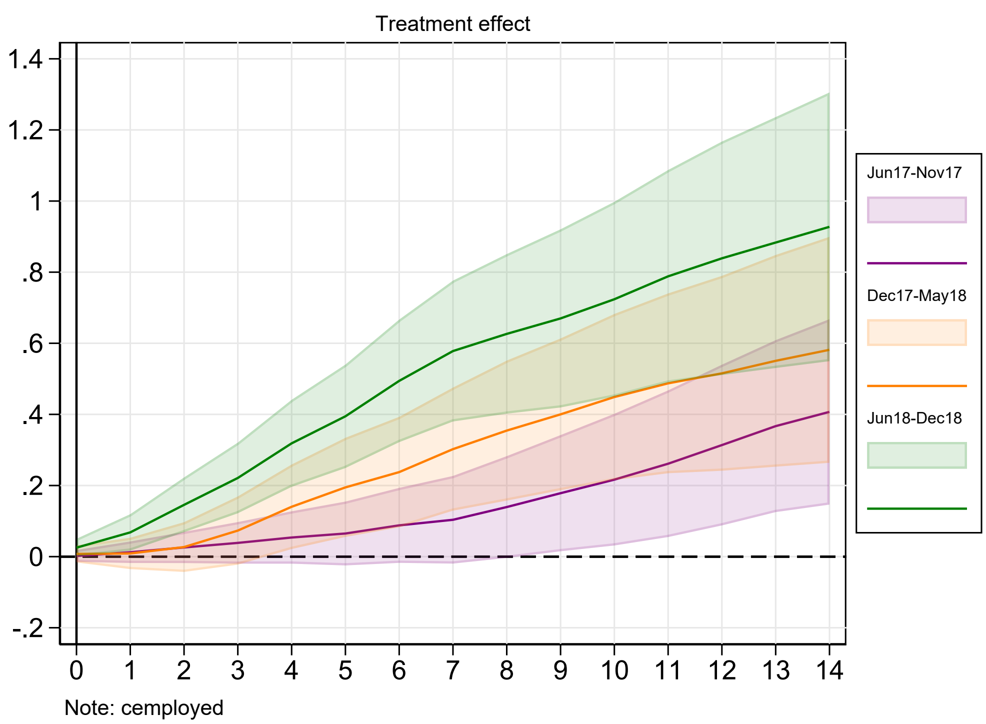
These two outcomes are consistent with the findings of Behaghel, Crépon and Gurgand (2014). Comparing the quality of employment services provided by public agencies and private contractors they document a gradual improvement of the latter in mastering the counseling technology. Nevertheless, it could also be the outcome of a gradual implementation of the program in different regions and offices that differ in the baseline characteristics.

Figure A19: Estimates for the treatment effect among different program cohorts

A. Attendance in IES offices




B. Cumulative months employed




*Note*: This figure shows estimates for the treatment effect on the probability to attend IES offices (panel A); and on cumulative number of months employed (panel B) – across three mutually exclusive program cohorts, that is, groups of participants that were allocated to the program

## A.6 Heterogeneous effect: By Employment & benefit history

Given the type of the program and the characterization of its population, an additional important stratification is based on pre-treatment status. Table A11 shows the main estimates by two additional strata: first, pre-treatment employment: one or more employment spells within the 36 months preceding treatment, vs. zero spells in that period. The second stratum is constructed in the same way with regard to DI.[16]

The results show that the program has been most effective – at least in monetary terms – for participants with some labor market attachment. For this group the effect on personal total income is significant, positive and strong: an increase of almost 7,000 NIS (s.e.~2000), a 12% difference relative to the control group. For those without any pre-treatment employment spell, and those with any pre-treatment receipt of DI there seems to be no effect neither on personal- nor on household total income. However, the program could have increased utility in non-monetary terms. For example, the shift from GIS to DI, even when holding the sum paid constant, can trigger an increase in utility in the form of exemption from attending IES offices (time saved), and a longer horizon of guaranteed stream of income (increased certainty).

[16] While the employment condition splits the participants' population into two groups whose size is of similar magnitude (1382 vs. 1738), in the DI case, group-size is less balanced (310 vs. 2797).

Table A11: Treatment effect on main outcomes, by pre-treatment records

| | All | Any employment (-36,-1) | | Any DI (-36,-1) | |
|---|---|---|---|---|---|
| | | Yes | No | Yes | No |
| GIS (any) | -0.05*** | -0.03 | -0.07*** | 0.02 | -0.06*** |
| | [0.02] | [0.03] | [0.02] | [0.05] | [0.02] |
| | 0.53 | 0.46 | 0.59 | 0.3 | 0.56 |
| Cumul. Income Support | -3,340*** | -2,834*** | -3,383*** | 624 | -3,697*** |
| | [639] | [1021] | [721] | [1973] | [644] |
| | 26905 | 24466 | 28718 | 13423 | 28628 |
| DI (any) | 0.02 | 0.03 | 0.01 | -0.03 | 0.02* |
| | [0.01] | [0.02] | [0.02] | [0.07] | [0.01] |
| | | 0.15 | 0.2 | 0.65 | 0.12 |
| Cumul. Disability benefits | 1,832** | 2,118* | 1,403 | -6.8 | 1,792** |
| | [764] | [1157] | [1130] | [3781] | [763] |
| | 9535 | 8430 | 10356 | 40428 | 5586 |
| Employed | 0.03* | *0.04 | 0.02 | 0.02 | 0.03* |
| | [0.02] | [0.02] | [0.02] | [0.05] | [0.02] |
| | 0.19 | 0.29 | 0.12 | 0.16 | 0.2 |
| Months employed | 0.73*** | 1.13** | 0.42 | 1.11 | 0.74*** |
| | [0.24] | [0.45] | [0.32] | [0.74] | [0.27] |
| | 4.43 | 6.96 | 2.54 | 2.68 | 4.65 |
| Cumulative Earnings | 4,489*** | 7,358*** | 2,568** | 5,209* | 4,553*** |
| | [1072] | [2146] | [1116] | [3146] | [1194] |
| | 14090 | 23292 | 7253 | 8007 | 14868 |
| Cumulative Total Benefits | -1325 | -426 | -1968 | 668 | -1722 |
| | 826 | 1332 | 1145 | 3409 | 797 |
| | 36594 | 33129 | 39168 | 53871 | 34385 |
| Cumulative Total income | 3,164*** | 6,932*** | 600 | 5,877 | 2,831*** |
| | [962] | [1988] | [1381] | [4368] | [1094] |
| | 50684 | 56421 | 46421 | 61877 | 49253 |
| Cumulative Total income | 2,076 | 5,012* | -64 | 4,619 | 2,429 |
| [household level] | [1479] | [2784] | [1764] | [5695] | [1597] |
| | 73215 | 76661 | 70654 | 84628 | 71756 |
| N | 3130 | 1382 | 1738 | 310 | 2797 |

*Note*: this table shows estimates using the main specification, stratified by pre-treatment employment history. The four categories are split by at least one month of employment (or DI receipt] within the three years pre-treatment or less, i.e., zero months. For every outcome the table shows estimates (top row], clustered standard errors in parenthesis (second row], and the control mean – in bottom row.

## A.7 Consistency tests

Table A12 compares treatment effect after 18 months in two different samples, the 18-months sample and 24-months sample , to verify stability of the results across samples.

The motivation for this comparison is to show that being assigned to the program has a similar and consistent impact when examining samples of different size, or when referring to groups allocated in different dates, with different horizons observed.

Estimated results 18 months after treatment in the 18-months sample and 24-months sample are rather similar for most but not for all outcomes. Dissimilarities can be identified in the case of DI cumulative income, and, to a lower extent, in cumulative earnings. This outcome is consistent with the differences documented across program cohorts, and with the possibility that a gradual improvement of program administrators and caseworkers took place (if not due to the gradual joining of different IES offices to the program, that differ in the composition of their clients).

Table A12: Treatment effect on main outcomes, by sample and distance from treatment

| Sample | 18-months sample | 24-months sample | |
|---|---|---|---|
| Effect after t months | 18 | 18 | 24 |
| Employed | 0.02 | 0.02 | 0.03* |
| | [0.01] | [0.02] | [0.02] |
| | 0.23 | 0.22 | 0.19 |
| Cumulative months employed | 0.56*** | 0.59*** | 0.73*** |
| | [0.16] | [0.18] | [0.24] |
| | 3.42 | 3.18 | 4.43 |
| Cumulative earnings | 2,246*** | 3,212*** | 4,489*** |
| | [719] | [748] | [1072] |
| | 11333 | 9821 | 14090 |
| Receives GIS [=1] | -0.05*** | -0.07*** | -0.05*** |
| | [0.02] | [0.02] | [0.02] |
| | 0.57 | 0.58 | 0.53 |
| Cumulative income from GIS | -2,274*** | -2,666*** | -3,340*** |
| | [412] | [478] | [639] |
| | 20980 | 21322 | 26905 |
| Receives DI [=1] | 0.02* | 0.03*** | 0.02 |
| | [0.01] | [0.01] | [0.01] |
| | 0.16 | 0.15 | 0.18 |
| Cumulative income from DI | 506 | 1,258** | 1,832** |
| | [517] | [536] | [764] |
| | 6688 | 6372 | 9535 |
| Does not receive DI or GIS [=1] | 0.03** | 0.04** | 0.04* |
| | [0.02] | [0.02] | [0.02] |
| | 0.3 | 0.28 | 0.3 |
| Cumulative income from benefits | -1640 | -1286 | -1325 |
| | 543 | 606 | 826 |
| | 27721 | 27733 | 36594 |
| Cumulative total income | 606 | 1,926*** | 3,164*** |
| | [702] | [692] | [962] |
| | 39055 | 37554 | 50684 |
| N | 4355 | 3130 | 3130 |

*Note*: This table shows estimates based on the main specification, where outcomes after 18 months are shown using two samples, the 18-months sample and 24-months sample . For every outcome the table shows estimates (top row), clustered standard errors in parenthesis (second row), and the control mean – at the bottom row.

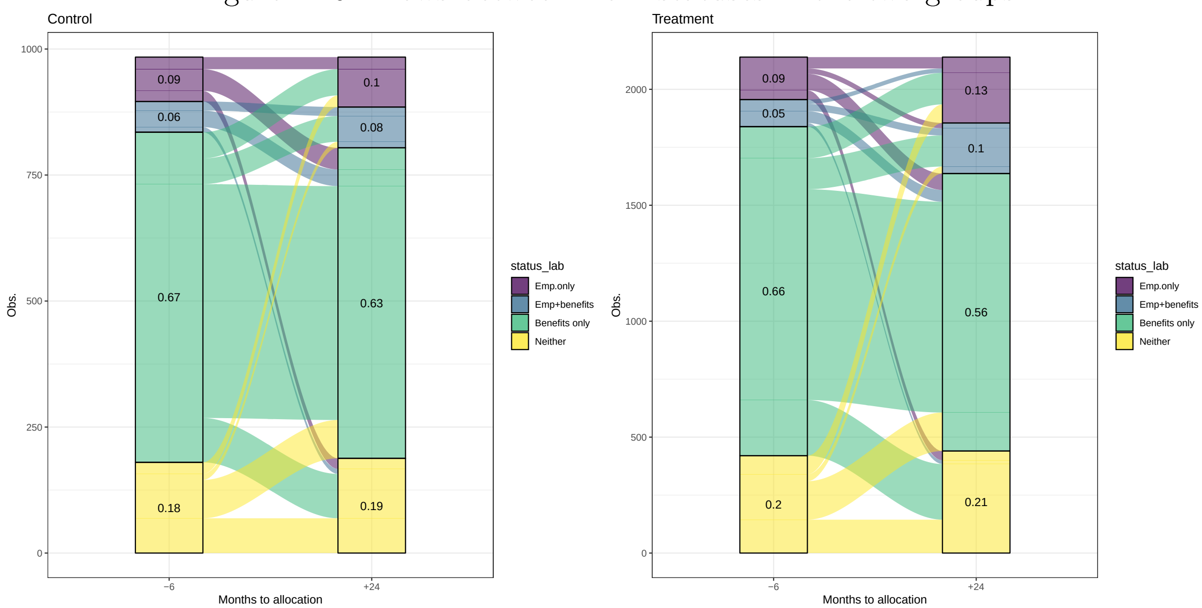
Table A13: Balancing test, among married participants only (24-months sample)

| Variable | C | T | diff. |
|---|---|---|---|
| Age | 44.154 | 44.843 | 0.396 |
| | | | (0.423) |
| Arab | 0.609 | 0.646 | -0.002 |
| | | | (0.019) |
| Female | 0.385 | 0.406 | 0.028 |
| | | | (0.029) |
| Ultra-orthodox Jew | 0.127 | 0.092 | -0.022 |
| | | | (0.017) |
| Ethiopian | 0.028 | 0.027 | -0.002 |
| | | | (0.008) |
| Officially Married | 0.95 | 0.927 | -0.019 |
| | | | (0.015) |
| Children | 4.332 | 4.427 | 0.137 |
| | | | (0.147) |
| Single-Parent | 0.022 | 0.028 | 0.006 |
| | | | (0.009) |
| Immigrant | 0.083 | 0.085 | 0.009 |
| | | | (0.016) |
| Health-limitation | 0.906 | 0.903 | -0.004 |
| | | | (0.015) |
| Low education level | 0.36 | 0.385 | 0.009 |
| | | | (0.027) |
| Employed [-1,-12] | 0.227 | 0.2 | -0.032 |
| | | | (0.026) |
| Employed [-13,-24] | 0.249 | 0.239 | -0.014 |
| | | | (0.028) |
| Employed [-25,-36] | 0.244 | 0.234 | -0.007 |
| | | | (0.029) |
| earning [-1,-12] | 2753.05 | 2425.849 | -571.065 |
| | | | (663) |
| earning [-13,-24] | 5083.749 | 4358.088 | -751.62 |
| | | | (825) |
| earning [-25,-36] | 7064.71 | 5729.501 | -807.114 |
| | | | (1114) |
| GIS income [-1,-12] | 13232.36 | 13568.604 | 299.009 |
| | | | (479) |
| GIS income [-13,-24] | 12622.676 | 13148.377 | 422.455 |
| | | | (581) |
| GIS income [-25,-36] | 12061.391 | 12383.089 | 238.145 |
| | | | (588) |
| N | *714* | *361* | |

Note: This table shows a balancing-test for married participants only.

Figure A20 shows the flows across the main statuses between 6 months pre-treatment and 24 months post-treatment – in the control (left) and the treatment (right) groups. Four statuses are examined: Employment only, Employment & benefits, Benefits only, and Neither of these categories. Within each panel the relative share in each status are shown, and the corresponding number of observations is shown on the vertical axis. Important to note the the share of people relying on benefits only is narrowed more strongly in the treatment group, and this seems to be due to thinner flows from any employment to benefits only; and thicker flows benefits only to any employment.

Figure A20: Flows between main statuses in the two groups




## A.8 Defiers & Compliers

Figure A21 splits trajectories of the main outcomes into three groups: the control group, compliers (treated that attended rehab) and defiers (treated that havn't attended rehab). Importantly, since this split is based on *post-treatment response* it has a different meaning than a split that is based on pre-treatment attributes (e.g., gender). While in the case of gender, the treatment- and the control-groups are compared *within* the female group, based on the assumption that also among women, the two groups are balanced, reflecting good randomization, in this case people are first allocated to the program, and their status as defires and compliers is defined ex-post (Hence, we refrain from showing a "treatment effect"). Nevertheless, a few important lessons can be inferred from these results, that are informative for understanding the mechanisms triggered by the program.

First, those who comply and attend rehab centers (*compliers*) are almost identical to the control group in terms of employment and earnings, pre-treatment and also up to 24 months *post-treatment*. This further strengthens the notion that the program has not contributed to participants via the human capital channel, and the entire gain in employment and earnings in the formal labor market is achieved by those who have not attended rehab centers even once (*defiers*). Pre-treatment levels show that these individuals are more competent ex-ante, and this fact in itself may explain their choice not to comply, and to give up their GIS benefit, i.e., they were more capable at the outset to go back to the labor market, and specifically, to do so on their own without the extra-support provided through the program.

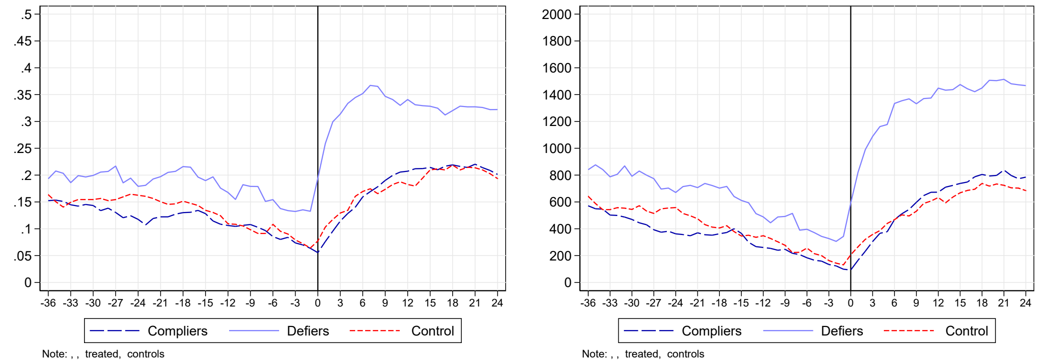
Second, as expected, the GIS only drops among defiers, while among compliers it follows a trajectory identical to that of the control group. This result approves enforcement of the program's protocol (inattendance results in losing benefit eligibility, while compliers keep their benefits similar to members of the control group), and also reinforces the former results regarding the treatment effect on employment, that is exclusively triggered by defiers.

Third, regarding DI, surprisingly, both groups increase eligibility to DI and the sum received via DI. Among compliers this increase is delayed and is slightly stronger. This gap is consistent with the possibility that the motivation to apply for DI, was in part driven by the drop in GIS (defiers); and also via a parallel process whereby compliers are supported by rehab centers and gradually apply and achieve DI eligibility (graduality captured by the delayed timing).

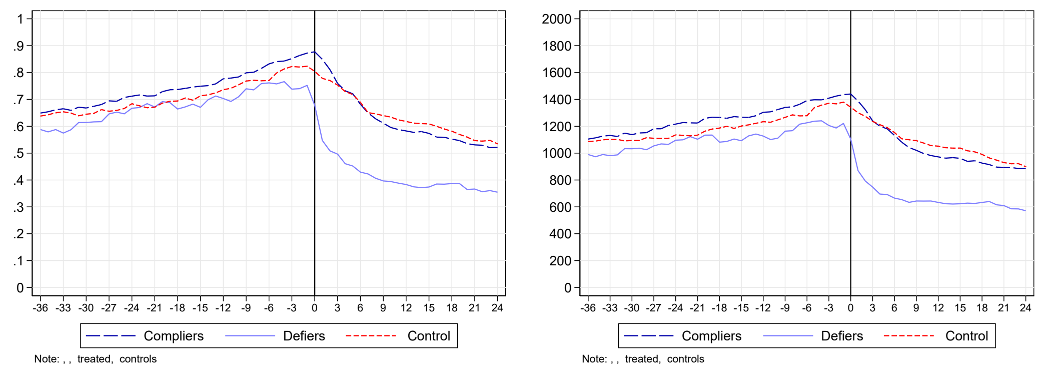
Finally, the bottom panel in figure A21 clearly shows that increased disappearance is exclusively driven by defiers; and likewise, that complying is typical to a sub-group among the treated that eventually tend to go back to the IES offices, i.e., a group of participants with stronger needs, and a weaker ability to go back to the labor market on their own (or: a group that has lower potential gains from doing so). Figure A22 shows trajectories, by similar format, for cumulative employment and earnings (top panel) and for income from DI and GIS (bottom panel).

Figure A21: Trajectories for main outcomes, among *Defires* & *Compliers*

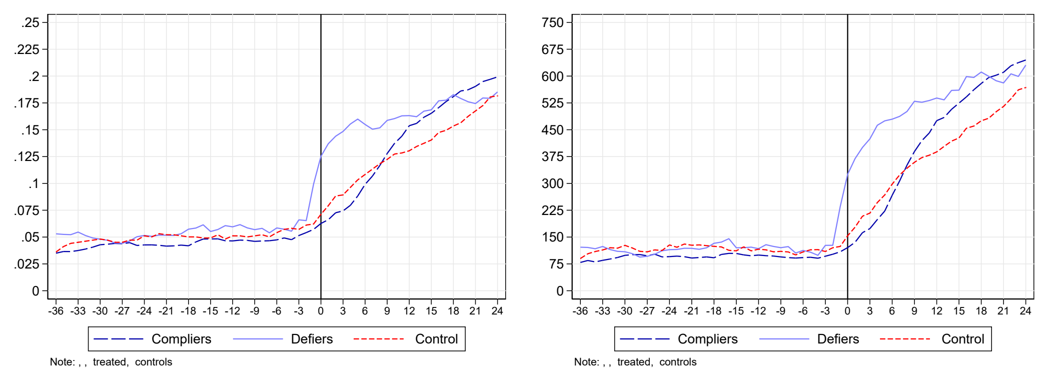
Employment & Earnings [-36,24]



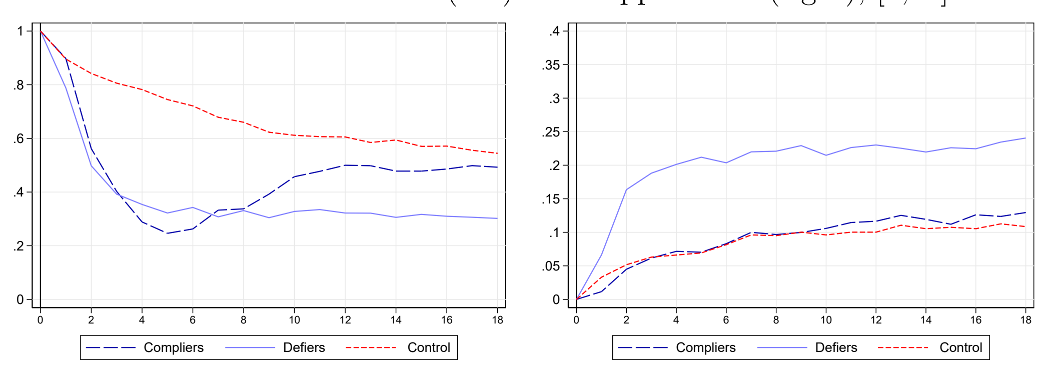

GIS: any & sum [-36,24]




DI: any & sum [-36,24]



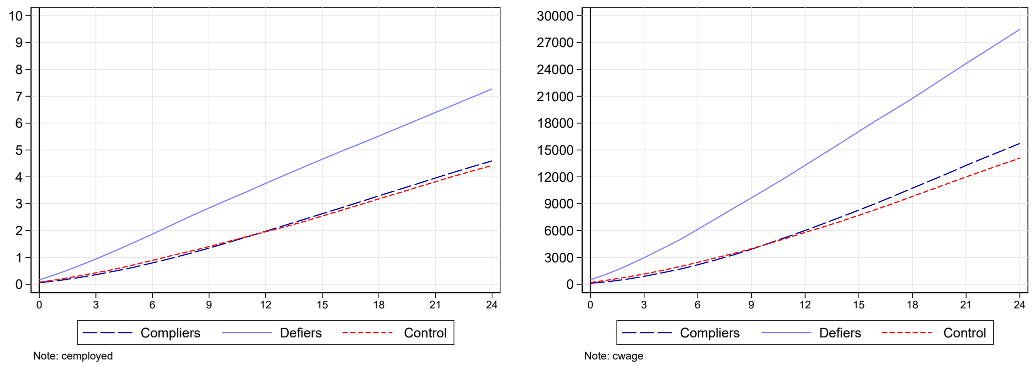

Attendance at IES offices (left) & Disappearance (right), [0,18]



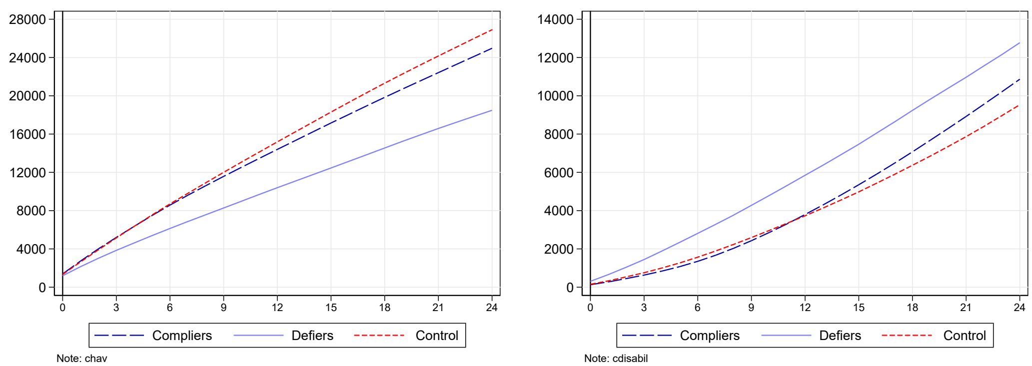

*Note*: This figure shows trajectories for the main outcomes among the control group and among compliers and defiers, i.e., those who have and those who have not attended rehab centers following their allocation to the treatment group. The number of compliers is 1610, and the number of defiers is 529.

Figure A22: Trajectories for main cumulative outcomes, among *Defires* & *Compliers*

Cumulative Employment & Earnings [0,24]

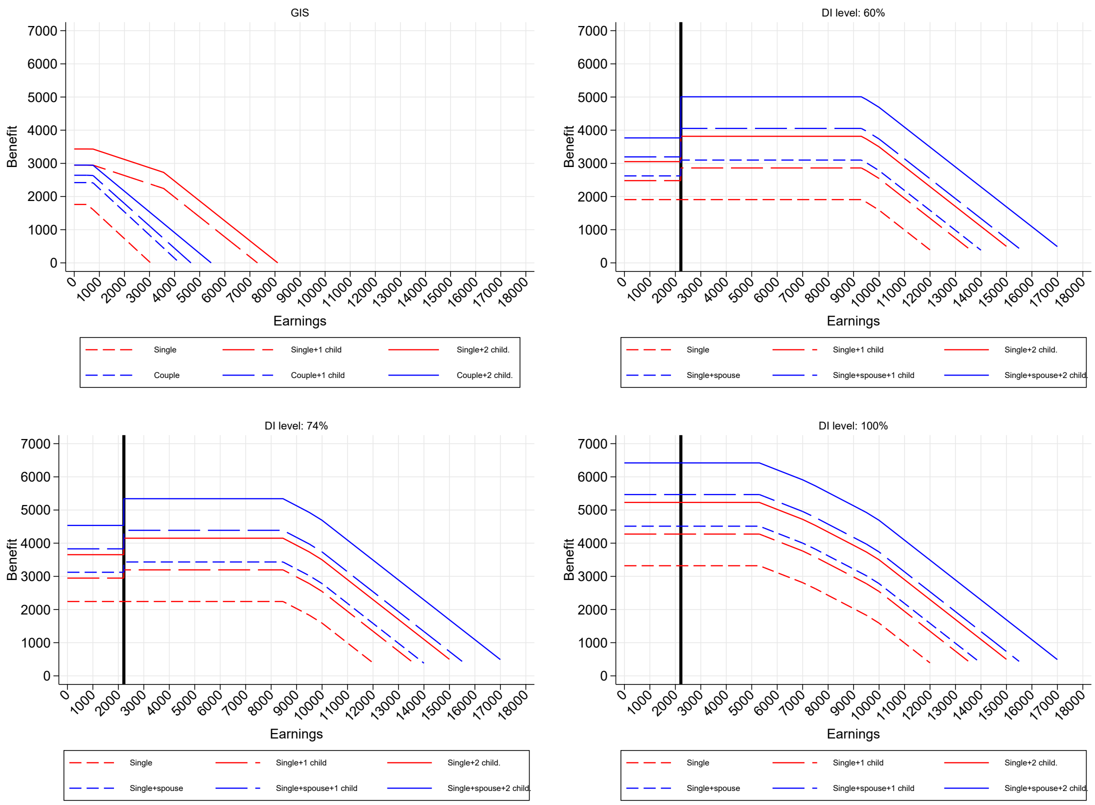



Cumulative GIS & DI [0,24]




*Note*: This figure shows trajectories for the main outcomes among the control group and among compliers and defiers, i.e., those who have and those who have not attended rehab centers following their allocation to the treatment group. The number of compliers is 1610; and the number of defiers is 529. The outcomes shown are cumulative sum (including zero).

Figure A23: DI & GIS disregard schemes, by family composition




*Note*: This figure shows disregard schemes, by family composition, across different benefits. The top left panel shows scheme for GIS, and the other three panels show schemes for DI in different degrees of severity, 60%, 74% & 100%. In each panel, the x axis show wage levels, and the y-axis shows the corresponding benefit sum for a person is eligible for this wage level.

*Chapter end.*

# Chapter 3.
# Long-term effects of training vouchers for the unemployed

**Abstract**

Although there are various justifications for governments to subsidize vocational training for the unemployed, it is not entirely clear whether and when such programs are effective, and to what extent such programs can benefit participants in improving their performance beyond their historical earnings. This paper studies the long-term effects of training vouchers on the labor market outcomes of unemployed in Israel using a re-weighting-based matching. After 4 years from treatment, significant and positive effects are found for employment (+6%) and for earnings (+7%). The program's effect is stronger for trainees who found a course-related job following training, pointing to the importance of achieving good matches between applicants and training. The effect is also stronger among women, Arabs, low-skilled workers and the long-term unemployed, suggesting that training vouchers are more effective for the least advantaged groups that are characterized by relative low attachment to the labor market. Assignment to subsidized training does not seem to increase survival rates in the labor market or to trigger an increase in wages beyond historical personal records. Thus, this paper suggests that short training for the unemployed is at most an effective tool for moderating the damage caused by unemployment and for restoring the previous situation but not as an opportunity to improve individuals' performance to new levels.

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# 1 Introduction

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

Vocational training (VT) is a central measure in an ALMP (McCall, Smith and Wunsch, 2016; Card, Kluve and Weber, 2018; Vooren et al., 2019). It can be delivered in various manners: privately financed, fully funded and subsidized. One way to subsidize VT is through vouchers – aimed at enabling a wider choice relative to programs directly delivered by the government. In turn, a wider choice is assumed to be key in achieving greater efficiency (Tomini, Wim and van den Brink, 2016). Ensuring the effectiveness of subsidized training is important both from the point of view of trainees' opportunity cost of time and from the point of view of appropriate use of taxpayers' money. However, training may be effective for some types of workers or for some social groups, but not for others, which means that studying the heterogenous effect of training should be a primary goal.

Training vouchers – or subsidized training at large – has three main target populations: youth (Attanasio, Kugler and Meghir, 2011; Attanasio et al., 2017; Card et al., 2011), employed workers (Schwerdt et al., 2012; Hidalgo, Oosterbeek and Webbink, 2014; Novella et al., 2018; Görlitz and Tamm, 2016, 2017; Dauth, 2020; Singer and Toomet, 2013) and the unemployed. Although randomized control trials (RCTs) have been used in numerous cases for studying the effect of training on the outcomes of youth and employed workers, using RCTs for studying the effect on the unemployed is less common (see Table 1). Studying the effectiveness of training for the unemployed in a non-experimental setup is challenging for a few reasons. First, it is a non-trivial question who among the unemployed is offered a voucher. Possible candidates are those who are assumed to benefit most from training or those with the highest potential to utilize the vouchers or complete training (cream-skimming). The next closely related questions are who should be included in the comparison group and how the timing of treatment (the start) should be identified for that group.

Matching is a common design for studying the effect of training vouchers on the outcomes of the unemployed. Based on matching, Andersson et al. (2013) find training to have positive but limited effects on employment and wages for adult American workers, but not for dislocated workers. For the unemployed in Germany, Huber, Lechner and Strittmatter (2018) find short-term negative and long-term positive effects of training on employment, that is, the unemployed who go through training experience delayed re-integration into the labor market relative to other unemployed (*lock-in effect*). Furthermore, Doerr et al. (2017)

find similar results for employment, with no effect on earnings, and a stronger effect among the low-skilled. Focusing on women only, Doerr (2022) finds large long-term effects on both employment and earnings, stronger among the low-skilled. These papers use the allocation of vouchers to study the effect of training on labor market outcomes by comparing the outcomes of voucher awardees to a relevant control and matched-control groups.[1]

Long-run studies of training programs are uncommon, and this is especially true when considering programs designed for the unemployed population (Card, Kluve and Weber, 2017). A selected list of papers that examine the medium to long term for this population and for others is shown in Table 1. Because governmental expenditure per participant may be considerable, and because exiting the labor market in specific ages and personal circumstances may be detrimental, there is a high importance for locating what works within and across program types and countries and across various types of workers.

Therefore, this paper examines whether training vouchers for the unemployed are effective in the long run, in what way they benefit participants and finally, whether some groups of participants benefit more than others, and if so, why.

It is important to question what mechanism is activated in the case of assignment to training programs. While one option is enhanced human capital, additional options include increased motivation, originating in the very act of being assigned to the program, and exposure to new job opportunities created by entering new networks of course participants and teachers.

Uncovering mechanisms is important also to draw more general conclusions regarding the potential of ALMPs. The question is whether ALMPs for the unemployed are only good enough for achieving restoration, or if can they also generate a significant leap forward in terms of skills and employability. In the former case, employment rates and earnings of the group studied are restored to pre-treatment levels; in the latter scenario, trainees reach new levels that are above their historical records. This difference is crucial for a country such as Israel in which training for current workers is very limited. Under such circumstances, events of unemployment create an opportunity to improve human capital among the labor force.[2] For example, Doerr (2022) finds a very strong treatment effect of training vouchers on the employment of German women returning to the labor market, of more than 15 percentage points, relative to the control group. However, 6 years after treatment, this group has not restored its historical wage levels, and it has only restored historical employment rates at the very end of the period (but has not surpassed them, up to that point).

[1] Another group of studies focuses specifically on comparing vouchers-systems to a more traditional direct assignment to training. They find short-term negative and long-term positive effects of vouchers on employment (Rinne, Uhlendorff and Zhao, 2013; Doerr and Strittmatter, 2021).

[2] While in some cases the very exist to unemployment is directly related to a decrease in the value of one's human capital (so that training can, for the least, help in restoration), in other cases the reasons behind unemployment are more arbitrary (so that training is offered almost as a coincidental opportunity for increasing human capital).

The relevant theoretical context for studying training for the unemployed has been human capital formation (HCF). However, another context that can be considered is social insurance in its broader sense, that is, governmental support provided for smoothing income over time by mediating the effect of idiosyncratic or exogenous income shocks. How does locating the right theoretical context matter? First, in modeling the decisions of individuals (jobseekers, caseworkers, or both); second, in guiding the concrete definition of effective ("effective" determined as beyond the level of the control group or as beyond one own's historical records). For example, if training is only good enough for achieving better results for the treated relative to a comparison group, then it can be compared to other measures of social insurance and ALMPs that also aim to achieve restoration. However, if it improves people's competence and outcomes beyond their historical records, it can be justified even when it is costlier relative to other policy measures. In either case, finding or constructing a relevant control group is a necessary pre-condition to examine effectiveness in the narrower sense.

In this paper, I utilize data from Israel that allows me to compare the outcomes of two groups in order to assess the effect of training vouchers – using re-weighting-based matching. The treated are unemployed who received employment counseling and have received vouchers for participating in training courses. By contrast, the control group includes a random sample of unemployed individuals who only received counseling, but have not been awarded a training voucher. Tracking pre-training outcomes shows that the pre-treatment employment trajectories of the two groups are almost identical. Next, I use re-weighting-based matching to strengthen comparability. Based on re-weighting, I study the effect of VT on various labor market outcomes in the medium and long term, up to 72 months after treatment.

I find that being assigned to the program had a significant and positive effect on employment (+6%), lasting in the long run. In other words, the advantage created by participating in training remains stable over time and non-participants do not catch up. I also find an effect on earnings, although this effect is less stable across specifications and horizons. The effect on employment is stronger for women relative to men, for people aged 40 and older relative to younger workers, for individuals with no college relative to those with any college and for those with less intensive employment history such as the long-term unemployed. Among Arabs, the treatment effect is stronger relative to the main sample. These latter two results suggest that the program is more effective for the least advantaged groups who encounter more difficulties in labor market integration, while stronger workers are more likely to have success on their own (so that in these cases, the control group catches up). In addition, training-to-job match seems to be crucial because the program's effect is stronger among assignees who, post-treatment, have found a course-related job (this result is only suggestive because job finding occurs post-treatment). Finally, training vouchers do not seem to shield participants from strong exogenous macroeconomic shocks, such as the one the COVID-19 pandemic created; they do not increase survival rates of participants in the labor market;

and they do not increase participants' level of earning, conditional on being employed.

Thus, the paper's contribution lies in numerous aspects. First, it is the first study that systematically examines the long-term effectiveness of VT in Israel. It relates to a relatively small number of studies that examine the effect of training on the unemployed over the long term, showing results up to 72 months from treatment, and observing pre-treatment results for a similar duration. This attribute is especially important because, as numerous studies show (Huber, Lechner and Strittmatter, 2018; Doerr and Strittmatter, 2021), the effect of training in the long term may differ from short-term effects, given the lock-in period in which individuals are trained and invest less in job searching. Hence, long-term comparisons are especially important from a cost-benefit point of view, considering some programs may become cost effective only in the long term.

Second, I utilize socio-demographic data to study effect heterogeneity across various strata and social groups. Although focusing on these social groups is in part a unique attribute of the local Israeli setup, this analysis has relevance for studying the effect of such programs on the outcomes of migrants and minority groups in advanced economies. Often, the heterogeneous effect in ALMPs has been examined across genders, age groups and education levels. However, it has hardly been applied regarding social groups and different minority groups. As one example, Sarvimäki and Hämäläinen (2016) study the effect of ALMPs among migrants regarding migrants-based programs and not in comparison to natives or the majority group.[3] Migrants and minority groups are in many cases characterized by lower labor market participation due to cultural reasons (Arabs and Ultra-orthodox in the Israeli case), or because they encounter challenges stemming from assimilating in a new country (Reimers, 1985). For example, Arab women in Israel have a very low participation rate in the labor market due to family and community norms (Yashiv and Kasir, 2013). Thus, this paper's results regarding minorities may be crucial for policymakers when adapting training to these populations.

Finally, it seems that VT for the unemployed – as applied in Israel during the 2010s – was mostly effective as a means for returning participants to employment but without further raising labor market performance to new levels beyond the historical ones.

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

[3] In a number of studies, citizenship is used as a control variable; and in German studies it is common to present outcomes by residency in East/West-Germany.

Table 1: Selected papers in the field of subsidized vocational training

| Paper | Population | Method | Treatment | Results | horizon |
|---|---|---|---|---|---|
| *Unemployed* | | | | | |
| Doerr (2022) | Female job-returnees in Germany | Matching | voucher awarded | employment (+), earnings (+), job quality & stability (+); stronger for the low-skilled | 72 |
| Doerr and Strittmatter (2021) | Unemployed in Germany | Matching | Reform in allocation to VT | Employment & earnings: short (–), long-term (+) | 88 |
| Doerr et al. (2017) | Unemployed in Germany | Matching | voucher awarded | short-term (–) & long-term ((+)) employment earnings (0); stronger effect for the low-skilled | 88 |
| Huber, Lechner and Strittmatter (2018) | Unemployed in Germany | Matching | voucher awarded | Employment, short term (-); long term (+) | 48 |
| Hirshleifer et al. (2015) | Unemployed in Turkey | RCT | Random allocation using over-subscription | Short-term employment ((+)); earnings (0); quality of employment (+); well-being ((+)) | 36 |
| Andersson et al. (2013) | Adult & dislocated workers in the US | Matching | Training | Employment (+); earnings (+), for adults but not for dislocated workers | 48 |
| Rinne, Uhlendorff and Zhao (2013) | unemployed in Germany | Matching | Reform in allocation to VT | employment ((+)); earnings (+). | 18 |
| *Employed workers* | | | | | |
| Dauth (2020) | low-skilled workers in Germany | IV | variance in generosity of regional offices | employment (+), earnings (+); stronger effect for women | 36; 60 |
| (Doerr and Novella, 2020) | vulnerable workers in Chile | RCT | Allocation to sub. training | Employment (~0); earnings ((+)) | 51 |
| Novella et al. (2018) | Current workers in Chile | DiD; IV | assigned to training / counseling | Employment (~0); earnings (-12%) | 60 |
| Görlitz and Tamm (2017) | Current workers in Germany | RCT | Information provision | Knowledge of vouchers (+); training (0) | 12 |
| Görlitz and Tamm (2016) | Low-income workers | Random event | Unintended voucher cancellation | Employment (0); Earnings (0); job quality (+) | 18 |
| Hidalgo, Oosterbeek and Webbink (2014) | low-skilled workers in Netherlands | RCT | voucher award | training (+); earnings (0); job mobility (0) | 12 |
| Singer and Toomet (2013) | Employed workers aged 45+ | Matching | | Survival in paid employment (+) | 24 |
| Schwerdt et al. (2012) | (mostly) workers in Switzerland | RCT (+IV) | voucher award | Actual training (+); earnings (0) and employment (0) | 12; 24 |
| *Youth* | | | | | |
| Attanasio et al. (2017) | Disadvantaged Youth in Colombia | RCT | Assignment to training program | contributions to social security (+); working in larger firms (+) | 120 |
| Cho et al. (2013) | Youth in Malawi | RCT | Allocation to training | skills development (+), short-term LM outc. (0) | 12 |
| Card et al. (2011) | Youth in Dominican Republic | RCT | | Employment ((+)); earnings ((+)) | 14 |
| Attanasio, Kugler and Meghir (2011) | Disadvantaged Youth in Colombia | RCT | Assignment to training program | Employment (+); Earnings (+) | 24 |

*Note*: Double brackets imply very small effect.

## 2 The program

VT in Israel includes fully funded and privately financed tracks as well as subsidized programs. Among the latter, one central channel is the Vocational Training Vouchers Program (VTVP) that was first introduced in 2006. In 2013, the program was handed over to the Employment Branch (a sub-unit of the Ministry of Labor) and its scope was extended. It serves both the unemployed (directed via the IES) and the employed workers (directed by governmental and semi-governmental directing centers) in roughly equal proportions. The direct costs of the subsidy over the years 2013–2019 for all the participants (more than 30,000 participants of both types) can be estimated around 165–210 million shekels (the equivalent of roughly 47–60 million USD), net of administrative costs.

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

As part of the program, each eligible applicant chooses a course from a closed list, which the Employment Branch authorizes, with a tuition the school sets where they intend to study (mean~9100; SD~4300). After paying the tuition upfront, participants are then eligible to three equal-sized refund payments, conditional on progress through the program. The voucher's value is computed based on a multiplication of the subsidy rate by the sum of the tuition, or by the subsidy cap (see eq. 1). The subsidy rate differs by course type and population group (see Table 2). The effective subsidy – endogenous to participants' choice-making – was on average, 6150 (SD~9,400). The sum of the subsidy in the relevant period was computed based on the following formula:

$$Subsidy_{g,c} = r_{g,p(c)} \times Min\left[Tuition, Cap\right] \tag{1}$$

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

# 3 Data

The data used in this paper are a unified data set originating from three administrative sources. The first source is the program data the Employment Branch provided, which contains details about the program's participants and their choices in training. The second source the Israeli Employment Services (IES) provided and includes a list of anonymized IDs of unemployed persons who received employment counseling, some of which have also been directed to the vouchers program (see below). This data also include the monthly dates of counseling and voucher assignment. The third source includes data on monthly earnings and benefits, which the National Insurance Institute (NII) provided. This data originates in the administrative records of the Israeli Tax Authority.

The population studied includes unemployed individuals who received employment counseling by the IES between the years 2013 and 2019. Employment counseling at the IES offices includes characterization of the jobseeker's skills and preferences, and presenting them with the relevant opportunities they have in the labor market. The control group includes unemployed individuals who received employment counseling from the IES without receiving a voucher. The treatment group includes unemployed individuals who, in addition to counseling, the IES also directed to the vouchers program (the counseling was provided 1 month before or, in most cases, in the very same month of being directed to the program). In the raw sample, there are almost 20,000 individuals who have received a voucher from the IES following their counseling (treatment), and almost 10,000 individuals who only received counseling (control). This sample is further restricted to meet numerous conditions (see below).

To clarify, the treatment group includes all the people who the IES referred to a voucher in the years 2013–2019, while the control group includes a random sample of individuals who were counseled but were not referred to a voucher. Specifically, the control group was constructed by the following steps and conditions: (a) individuals who have received employment counseling, and (b) those who the IES has not directed to the voucher program. Then, (c) after applying these conditions, the remaining sample was still very large in size, and not necessarily balanced with voucher recipients. Hence, this group was appended to the treatment group and individuals were randomly drawn, from the raw control group by allocation cells in 1:2 proportion (control:treatment). A cell for this matter is a combination of the local IES office and the month of counseling. From this group, individuals were (d) dropped if they were found in the program data (i.e., if they had been directed to the vouchers program by other organizations and not by the IES). In sum, the treatment group includes all those who the IES referred to a voucher program in the relevant period, and the control group includes only a partial group – randomly selected – from the recipients of employment counseling (whose total number is very large).

Following Huber, Lechner and Strittmatter (2018), I restrict the sample by age (18–61) and, as done also in and Doerr et al. (2017), by Unemployment benefits (UIB) status, that is, I only observe individuals who received UI benefits at least once during the 3 months before receiving employment counseling. Further restricting the sample to individuals whose labor market outcomes can be observed for at least 24 months from counseling brings the final sample to 7,566 observations (unique IDs). The main analysis focuses on a 48-month horizon, and in some cases, I also refer to the 72-month horizon, which leads to using smaller samples (Table 4). The month of receiving employment counseling is defined as $t_0$. This is also the month when most participants were referred to a voucher (in some cases it occurs 1 month later). The horizons are then computed as the number of months observed in the data, from the month of counseling, for example, *horizon 36* refers to all the people for which income data is observed over 36 months from treatment (i.e., from the month of counseling).

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

# 4 Design

To focus the analysis on a relevant sub-population, the control group was not drawn from the general population of unemployed, but rather from the group of individuals who received employment counseling. This strengthens comparability because all voucher recipients went through such counseling before or when being directed to the program. In addition, by limiting the sample to people who went through counseling, other population types – for example, short-term unemployed who approached the IES to bridge income falls when moving between jobs, but who do not encounter significant barriers in re-joining the labor market – are not included in the analysis.[4] Figure 1 shows that for every 1,000 job-seekers interacting with the IES each month, the number of vouchers the IES allocates is below 2 (i.e., 0.2%), and that for every 1,000 UIB recipients, this number was around 4 (0.4%) for most of the period examined.

[4]It should be noted that the number of vouchers allocated by the IES was very small relative to the number of potential participants, i.e., UIB recipients (or jobseekers) attending the IES offices and receiving employment counseling in the relevant period (see figure 1). This means that in order for cream-skimming to pose a potential threat to identification in this set-up, one must assume that caseworkers have invested significant efforts in locating a very specific sub-set of potential participants deemed more competent. If that was not the case then, it seems plausible that allocating vouchers was close to random. This approach can be regarded as implied over-subscription design. Nevertheless, a possible self-selection that is not reflected in observables cannot entirely be ruled out. Hirshleifer et al. (2015) utilize a reality of over-subscription to courses – in order to study the effect of training on labor market outcomes, however, by randomly allocating unemployed individuals (subscribers) to courses.

Albeit using a more homogenous sample, selection can still pose a potential threat to identification if, for example, caseworkers tend to offer a voucher to individuals with higher assessed ability (cream-skimming). To address the possibility of some sort of selection that may have taken place, and to address a possible imbalance between treatment and control more generally, the main analysis in the study is based on re-weighting – considering various background variables. The idea behind this matching technique is to attribute a specific weight to every observation (every individual) in the control group so that the re-weighted control group will share higher similarity with the treatment group, relative to the raw control group. Numerous algorithms have been developed to re-weight observations, and the one used here is the one Hainmueller (2012) suggested. Heinmuller's methodology (and algorithm) is based on recursively re-weighting the observations in the control group until the re-weighted sample meets the pre-defined criteria. These criteria (constrains) are user defined and describe the distribution of the treatment group. The algorithm re-weights the control group until the two groups resemble the main moments (mean, variance and skewness) for the list of variables defined.

The re-weighted sample is then used to create descriptive and OLS-based results. The estimates shown below capture the effect in terms of the intention to treat because they compare outcomes between those who only received employment counseling (control) and those who also received a voucher (treatment), regardless of whether they were trained.

The main estimation is based on the following basic model:

$$Y_{ist} = \tau_1 Voucher_i + X_i'\theta + \delta_{st} + \varepsilon_{ist} \tag{2}$$

where $Y_{ist}$ is the outcome for individual $i$ counseled at IES office $s$, in quarter $t$. $X_i'\theta$ is a vector of control variables (personal characteristics), and $\delta_{st}$ is an allocation cell fixed effect. These allocation cells are constructed from IES office and quarter of counseling, and thus, are meant to reflect that people going out of the labor market in different quarters over the year may differ systematically. Standard errors are clustered at the level of randomization cells.

I also estimate pre-treatment (pre-counseling) outcomes to verify comparability. Specifically, comparing outcome variables in the pre-treatment period helps examine (a) the similarity of the two groups pre-counseling, (b) the similarity of the groups without weighting, (c) and the improvement achieved by re-weighting in terms of groups' similarity.

To verify the construction of a relevant sample, Figure 2 shows the fraction of individuals receiving UI benefits in the period around the month of counseling in the treatment and the control group. That the two groups follow the same trajectory provides a first indication that the construction of the sample was done appropriately.

Next, I examine balancing the background variables, shown in Table 5. Columns 1–6 show central moments before re-weighting. After re-weighting the control and treatment groups are almost identical for all three moments, which is also reflected in columns 7–8 that show the standardized difference before and after re-weighting. I also examine heterogeneous effects at the sub-group level, in which case samples are re-weighted anew.

In summary, the validity of the analysis depends on the assumption that individuals from both groups had similar chances to be directed to a voucher, so that results are not biased by selection. However, since the study is not based on a randomized allocation of vouchers by the IES, selection bias cannot be entirely ruled out. Indeed, this is a typical problem in public policy analyses, and hence, it is important to consider the incentives caseworkers who were responsible for awarding the vouchers encounter. Caseworkers possibly are prone to award the vouchers to the stronger candidates, assuming they are the most likely to show good results post-treatment, which is desirable from the point of view of the organization's reputation. In contrast, IES caseworkers probably understand that providing vouchers to candidates with the strongest potential is problematic from the point of view of utilizing public resources (they can do without it). Overall, it is hard to conclude ex-ante which of the considerations is more dominant, hence, it is also hard to assess whether the estimation is prone to suffer from a systematic selection bias based on caseworkers' discretion.

Another possible source of selection is the participants rather than the caseworkers. In this case, it is important to differentiate between two stages in which self-selection can take place: voucher award and voucher take-up. Because I estimate the effect of being directed to the program (intention to treat) rather than the effect of using the voucher or the effect of training, selection in the take-up stage does not pose a threat to identification. However, if self-selection has taken place at the award stage – that is, if certain groups could have effected the very chances of being directed to a voucher, for example, to resist the caseworker's issuance of a voucher – this could also bias the results, based on participants' self-selection.

Finally, the similarity in pre-treatment employment trends strengthens the assumption regarding non-selection in the voucher award phase. In addition, re-weighting further improves comparability, so that even if there is a selection in the first phase, this matching technique further weakens the potential threat to identification.

# 5 Results

In this section, I present the main results. Results are shown for the entire population and for sub-groups. The main horizon examined is 48 months after treatment, and outcomes are shown for the 72-month horizon sample as well. For some sub-groups, shorter horizons are used to examine large-enough samples. In addition to estimates after 18, 24 or 48 months, and to outcomes accumulated over such periods, I also analyze the dynamic development of differences between the groups over time. The dynamic analysis enables tracing the timing of the effect, and thus, is informative regarding the manner through which the assignment to the program has triggered results.

The two main outcomes examined are employment (the probability to be employed in a certain month and the number of cumulative months employed up to that point) and wages (in a specific month or as accumulated up to that point). I also estimate the assignment effect on numerous additional outcomes, all of which are variations built around employment and wages.

The variable "ever back" is a binary outcome that equals 1 when the individual is employed for the first time post-treatment, and it stays equal to 1 from this point on. Thus, it goes beyond the group-level performance because the outcome "employed" is not sensitive to the case in which one person from the group exits the labor market and another one enters. The second outcome is "never-out." This outcome is measured relative to the first month of post-treatment employment, when it equals 1, it turns to 0 in the first month of a subsequent non-employment period, and it stays 0 from this point on. It thus measures the strength of attachment to the labor market post-treatment, assuming that more successful cases are associated with longer consecutive employment periods.

The two other outcome variables focus on wages. The third outcome is a binary outcome that measures whether individuals' post-treatment wages surpass their highest pre-treatment wage (in the 36 months preceding treatment). This outcome captures whether assignment to training has improved participants' human capital or at least extended their work intensity in terms of hours worked. The fourth and final outcome is positive earnings, that is, wages excluding zero or wages conditional on employment.[5]

## 5.1 Main effect

Table 6 shows the results for the probability of being employed and for wages 24 months before treatment and 24 and 48 months after treatment, using the main sample and different specifications. Results are based on OLS regressions using the re-weighted sample. Every cell in the table shows the estimate in the top row, standard errors in the middle row and the control group mean in the bottom row. The results show that pre-treatment estimates for both employment and wages indeed are insignificant. Post-treatment estimates for employment are significant and in the range of 4–5 percentage points 2 years after treatment and 3–4 percentage points 4 years after treatment. Relative to the control mean (employment rate of 71%–72%), these estimates reflect a 4%–7% difference. Estimates for wages (including zero) are insignificant.

Table 7 shows estimates for the effect of voucher receipt on employment and wages ac-

[5]The downside of using this variable is that the sample is not fixed, as (different) people with zero wages are thrown away from and enter estimation in different months.

cumulated over 48 months from treatment (panel A) and estimates for the same outcomes accumulated after 72 months (panel B). The results show additional 1.5–2 months of employment relative to 33 months in the control group after 4 years (a 4.5%–6% difference). Estimates for wages are significant in most of the models and reflect a 7%–9% difference. The gap between the two results – wages in a specific month (Table 6) and cumulative wages (Table 7) – can be explained by the possibility that a gradual and incremental insignificant effect accumulates to a significant difference.

That the effect on wages is not as stable and strong, such as the effect on employment, is consistent with the possibility that the program had an effect via the extensive rather than intensive margin. That is, it has increased the share of people employed in the treatment group beyond the corresponding increase in employment in the control group, but has not necessarily caused people to extend their working hours.[6] Alternatively, it had a limited or no effect on job quality as captured by hourly wages.

Panel 7b shows results 6 years after treatment, which is when the treatment effect on employment is around 5–6 additional months, more than a 10% difference relative to the control group mean (47.9 months). A strong and significant effect can also be observed for cumulative earnings (18%–20%).

Figure 3 complements the main analysis and shows employment trajectories for the two groups and the estimated effect over the 8-year period: 48 months before and 48 months after counseling. The results are based on regressions that were ran separately every 3 months.[7] The two groups follow the same trend pre-counseling, and around 6 to 12 months after being directed to the program, the employment rate among the treatment group bypasses that of the control group. The timing of the separation is consistent with training duration of most courses, which indeed falls in the range of 6 to 12 months. The employment gap between the groups reaches a maximum of 8 points difference after 18 months and remains positive, by around 4–5 percentage points, up to 48 months after counseling, which means the program had a profound and lasting effect on participants. In addition, the bottom panel shows there is no significant effect of receiving a voucher on earnings, at least when measuring earnings at the monthly level. However, over time, the small differences in wages accumulate into a more significant difference — as shown in figure 4, that shows estimates for cumulative employment and cumulative wages over the 48 months after treatment.

Importantly, Figure 4 shows that the effect on employment becomes significant only after 18 months from counseling. This means that short- to medium-term estimates (12–24 months) for this outcome could not have been enough in this case for determining the program is effective. Figures A16-A17 in the Appendix replicate Figures 3-4 for a longer period

[6] Working hours are not directly observed.

[7] Some of the figures shown later reflect higher frequency of estimates made every two months or every month.

of 72 months from treatment – for an "older" and smaller sample. The results are consistent with the results driven from the main sample; however, they are somewhat stronger in the common period.

## 5.2 Heterogeneous effect

In this section, I present a heterogeneous analysis for various strata that fall into two types: demographic attributes (gender, ethnicity and age) and human capital attributes (education level, employment history and status in the training program).

### 5.2.1 by human capital, and training status strata

Table 8 shows estimates by educational level and training status. Education is defined by a binary variable that equals 1 for individuals who appear in the post-secondary (academic) records and 0 otherwise. Many of these individuals have not attained academic qualifications; hence, this variable captures the any college category. The results show a stronger effect on employment among the no college group, relative to the any college group: additional 3.2 (s.e. 0.9) months employed among the former versus 0.6 (1.0) among the latter. Figures 5-6 show estimates for the treatment effect on employment and cumulative employment, respectively.

Next, I focus on the outcomes of participants who have completed training and of those who found course-related jobs (the two rightmost columns in Table 8). Estimates for participates who completed training are similar to estimates among the entire sample, in terms of both employment and wages. The effect on cumulative employment for trainees is around 2 additional months (or ~6%), 48 months after treatment, and estimates for wages are insignificant. In contrast, estimates for participants who found course-related jobs are significantly higher: in the case of cumulative employment, around 5 additional months (+15%) and for cumulative wages (+12%).

Note that in the case of these last strata, the comparison is made between all non-assignees and assignees who completed training or who also found a course-related job. Stratifying outcomes by post-treatment statuses – different from pre-treatment attributes such as gender — may be biased by self-selection, because stronger participants are more likely to find a course-related job (in other words, finding a course-related job is probably non-random). Hence, I also examine pre-treatment outcomes for these groups. Figure 7a shows the pre-treatment employment trend of trainees is almost identical to that of non-assignees, and Figure 7b shows the pre-treatment employment trend of course-related job finders is similar, although not identical, to that of non-assignees (the control group). This suggests that trainees and course-related job finders are not systematically self-selected, which makes it reasonable to attribute causally the stronger effect to the act of completing training or finding relevant jobs. Nevertheless, it can still be the case that some self-selection takes place but cannot be located based on observables. Therefore, results for these groups – especially the course-related job finders – may suffer from an upward bias and should be treated with some caution.

### 5.2.2 by employment history strata

Table 9 shows estimates by employment history, which is closely related to labor market attachment. Employment history is referred to in this case by the number of months employed in the pre-treatment period out of 36 possible months. It divides the population into two groups, according to the median value of pre-treatment employment: people with up to 27 of months of employment ("long-term non-employment"), and people with 28 months or more ("short-term non-employment"). The results show a stronger effect of voucher award among those with weaker employment history relative to those with a stronger pre-treatment employment record: 3.1 (s.e., 1.3) additional months among the long-term unemployed versus only 1.6 (0.7) among the short-term unemployed, 48 months after treatment (the table also shows estimates after 24 and 36 months). Figures 9-10 show the dynamic estimates for employment and cumulative employment among the long-term non-employed in comparison to the performance of income support recipients (see below).

The next group examined in this section includes people who have received guaranteed income support (GIS) at least once during the 36 months before treatment. Income support recipients differ significantly from the main sample. Specifically, when comparing historical earnings from labor income, accumulated between 36 and 24 months prior to treatment, it can be seen that while the former accumulated around 2,000 NIS during that year, the latter earned almost 5 times that amount (reflecting differences in both employment rates and in earning levels). In addition, among income support recipients, the share of Arabs and women is much higher relative to the corresponding figure among the unemployed. Meaning, income support recipients indeed make a distinct group whose labor market attachment and accumulated experience is especially low relative to other UIB recipient groups.

Table 10 shows estimates for the income support recipients – for six outcomes across three horizons (18, 24 and 48 months after treatment). The effect on the cumulative number of months employed is mostly insignificant. In contrast, among this group, the effect on the outcomes employed, ever employed post-treatment and cumulative wages is significant and rather strong. Figures 9-10 complement this picture, showing employment trajectories and the estimated effect on employment and cumulative employment among this group as well as among the long-term unemployed. The figures show that among income support recipients, the effect on employment is modest or insignificant for most of the period, but becomes significant and strong after 3 to 4 years from the voucher award. Finally, note that by the end of the period examined, both the treated and the control group among income support recipients reach employment rates that are beyond pre-treatment levels (groups' averages), probably due to employment counseling that both receive, and the treated go even further.

Regarding other quality measures, there seems to be no lasting effect of voucher award on the group of income support recipients. Specifically, the bottom rows of Table 10 show no significant effect on survival in the labor market, conditional on being employed, or on the probability to earn more than historical levels.

#### 5.2.3 by demographic strata

Table 11 shows estimates for the assignment effect on cumulative employment and wages by gender, ethnicity and age. The effect on employment is stronger among women relative to men, and among people aged 40 and above relative to people below 40 years of age.

At the level of ethnicity, the long-run effect among Jews is significant: 2 additional months. Among Arabs, the effect is even larger, although not precisely estimated, probably due to the small samples. When using shorter horizons to have large-enough samples, estimates for the treatment effect on short- to medium-term cumulative employment are statistically significant and higher among Arabs (Table 12 columns 1–2), and especially among Arab men (column 3), relative to Jews.

Another social group whose outcomes are of interest is the ultra-orthodox (Haredi) Jews whose employment rates, especially among men, are uniquely low (outside the program). The results in Table 12 (column 5) show that the program had no effect on employment among Haredi Jews, also when using larger samples for the 18- and 24-month horizon. This effect seemingly masks a difference between Haredi men (positive effects) and women (null or negative effect), although the sample of Haredi men is very small, hence jumpy, which demands carefulness.

Regarding additional quality measures – survival in the labor market post-treatment and wages above historical levels – the bottom rows of Table 12 show that the treated are not significantly different from the control when making the comparison between Arabs and Haredi Jews or between their sub-samples. The exception is the case of Haredi Jews whose survival rates show some limited difference post-treatment (est. $-.12$, s.e. .057).

This employment picture can be strengthened based on the Appendix figures A11-A12, showing the dynamic employment trajectory and estimated effect for the different groups. The effect on employment among Arabs is higher relative to Jews (8 relative to 4 percentage points) and even higher among Arab men (10–20 percentage points). In all cases, the difference between the control and the treated emerges between 6 to 12 months after treatment, which is consistent with the timing of training completion. Regarding ultra-orthodox, Jews there seems to be no effect of being awarded a voucher on employment, albeit a rather strong effect among Haredi men (recall the small sample size). Possible explanations for these results are discussed in Section 6.

## 5.3 Labor market quality measures

In this section, I refer to the program's effect on numerous quality measures outcomes, some of which were mentioned above in the context of the heterogenous effect. These outcomes help in assessing whether the program was successful in pushing participants beyond baseline pre-treatment levels, that is, beyond restoration.

Figure A13 shows dynamic estimates for two outcomes. Charts in the two left columns show that the share of those ever-employed post-counseling in the treatment group is higher by 3–4 percentage points relative to the control group. It is slightly lower (than average) among Jews, somewhat higher though not precisely estimated among Arabs and moves around 0 and insignificant among Haredi Jews. Charts in the two right columns show that survival rates in the labor market – strictly defined so that a person survives in the labor market only as long as they have no non-employment periods – is not significantly affected by voucher receipt.

Figure A14 shows that there is no significant difference between the control and the treatment groups regarding the probability to earn wages above historical levels. The share of individuals among both groups that earn wages beyond their historical record is relatively the same – around 10%.

Finally, Figure A15 shows trajectories and the estimated effect for positive wages (or wages conditional on employment). The results capture two phenomena. First, mean wage increases toward $t_0$ and then return to (almost) similar levels. This trend is consistent with the possibility that those who lose their job at the latest point relative to counseling are the economically stronger workers who earn more. Hence, as their relative share gradually increases when approaching $t_0$, the overall mean decreases toward their wage levels. Likewise, as people start going back to the labor market the adverse process occurs (low-wage earners are the last to rejoin the market), and post-treatment wages stabilize around a level slightly higher than pre-treatment, which is consistent with the positive share of people (in both the treatment and the control) that go beyond their historical wages. Second, this process looks very similar across the two groups, showing that also when conditioning on employment, receiving a voucher does not trigger any effect on wage levels.

## 5.4 Cost-benefit

The treatment effect on wages after four years, shown in Table 7a, is in the range of 2,465–3,252 NIS. The value in mid-range is 2,859. In contrast, the average subsidy was 6,150. This means that after 4 years, 46% of the costs per participant are paid back in the form of increased earnings (recall that estimates used are based on intention to treat, i.e., those who received a voucher and were not trained eventually are not excluded). Based on a naïve imputation that assumes a linear trend of the treatment effect, the gains associated with the program should surpass the direct costs after 103 months (8.6 years). That is, the program is cost-effective only in the very long term, if effectiveness is only referred to in monetary (wage-based) terms. Nevertheless, increased employment means additional savings of UI benefits and income support that may accumulate over time, which further increases gains relative to costs.

Regarding those who found a course-related job, they are eligible for an additional grant in the range of 1,500–2,000 NIS, but also show a stronger increase in wages. Thus, a 4,560 increase in earnings should be examined against a cost of 7,900 per participant (6,150 + 1,750). The ratio in this case is 58%, which corresponds to a shorter period of 83 months (6.9 years) needed for gains to surpass costs.

Finally, caseworkers could have directed the least advantageous candidates to training – assuming they are the most likely to benefit from training because stronger candidates can be successful on their own. If so, this would imply that the program is more cost-effective than suggested above.

# 6 Discussion

In this section, I position the findings in light of the existing literature, and thus try to explain the differences between groups regarding the program's impact. I discuss the importance and implications of the results, and point to possible extensions that can be part of future research.

The discussion is constructed around an overarching question. What are ALPMs good for? Furthermore, does the added value of these programs lie – solely or mainly – in minimizing the damages caused to individuals by lay-offs and other life-cycle and business-cycle events? Can they also improve and increase productivity, employability and employment stability beyond pre-treatment levels, thereby turning "a crisis into an opportunity" (at least at the personal level)? In this setup, I can only examine these questions regarding training for the unemployed and for income support recipients, which are only two of a few relevant target populations, although rather central ones.

Estimates for the main outcomes show that training vouchers are effective for the unemployed, and they trigger an increase in employment that lasts over a long period. Specifically, the effect of 5 percentage points after 2 years and 4 percentage points after 4 years is close to and slightly below the range documented in the meta-analysis by Card, Kluve and Weber (2018),[8] that in the relevant categories – training for the unemployed in the medium

[8] My results fall close to the relevant ranges sketched in the meta-analysis by Card, Kluve and Weber (2018): training programs (6.6 ppts. in the medium term; 6.7 in the long term); and among unemployment recipients (4.3 and 8.5).

and long term – document a 6–7 percentage point increase. Relative to more similar setups of long-term voucher studies, it falls within the range of the findings by Doerr et al. (2017) (2 percentage points increase after 48 months and no effect on earnings) and Doerr and Strittmatter (2021) (7 percentage points increase after 48 months, and modest effect on earnings).[9] Different from these studies, I do not find a significant lock-in effect, which can be explained by the fact that the types of training studied in this paper are mostly short training, some of which can be taken alongside part-time jobs.

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

The heterogeneous effect lends support to this last possibility that the program was indeed more beneficial for those with lower attachment to the labor market. Specifically, a stronger effect among women relative to men, those without college relative to those with some college, and among the long-term relative to the short-term unemployed is in line with findings in the literature that document higher returns for ALMPs and training in specific, among women and the low-skilled (Card, Kluve and Weber, 2017; Doerr, 2022; Dauth, 2020). One explanation for this pattern is that because the latter are characterized by lower labor market attachment, they are more likely to benefit from re-integration programs such as training. In contrast, men and the highly skilled are more likely to be successful on their own without external help.

The lower attachment explanation is also relevant for explaining the strong(er) effect among Arabs (a disadvantaged minority group in the country) relative to Jews. However, the relation between labor market attachment and program effectiveness is not monotonic: the weakest segment of participants with the weakest labor market attachment – Arab women and to some extent, recipients of income support – show a further limited effect relative to Arab men and to the long-term unemployed, respectively. This gap suggests that some minimal level of attachment to the labor market is necessary, below which training is less effective, if anything. This is consistent with the notion of training as a means of restoration because there must be some basic level of human capital that can be restored. Income support recipients who experienced very long periods outside the labor market may be characterized by hysteresis, and hence, need support in strengthening more basic (soft) skills (Schlosser and Shanan, 2022).

The labor market attachment explanation can also be applied to the case of the Haredi

[9]Recall that Andersson et al. (2013) find no effect of training on the employment of unemployed workers.

Jews, for which there is also a null effect on employment. Haredi Jews are also a minority group with low participation rates. An alternative explanation is that members of the Haredi group are successful without the government's help (i.e., the control group catches up), consistent with previous studies in Israel that documented the high efficacy of this group regarding mutual support and specifically in utilizing benefits and services (Brender and Strawczynski, 2019).[10]

Regarding quality measures,[11] similar to Doerr (2022) that analyze the probability to earn "at least 100% of previous earnings," I examine the probability that individuals will earn above their pre-treatment wages. Although Doerr finds a significant effect for this outcome, I find no such indications. The difference may be attributed to the different populations examined (unemployed, here vs. female "job-returnees" in Doerr's paper[12]), and to the intensity of the training (short training in this paper vs. various durations, including long ones, in Doerr's paper).

Therefore, it seems that even when considering the populations who benefit the most from it, short subsidized training for the unemployed is first and foremost a means of restoration and only to a lesser extent can it be regarded as a human capital enhancer that can increase labor market performance beyond pre-treatment levels. In addition, short training seems to have limited effects for those with the very low (lowest) labor market attachment. These empirical observations can guide optimal policymaking in locating what sub-groups among the unemployed should be directed to training, given the limited budget for training subsidies.

# 7 Conclusion

This paper examines the long-term effects of subsidized vocational training on the labor market outcomes of the unemployed. I use re-weighting-based matching to assess the medium- and long-term effects of being assigned to the Vocational Training Vouchers Program on the labor market outcomes of assignees relative to a random sample of unemployed.

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

[10]It should also be noted that as the population standing at the center of the analysis includes people that were eligible to UIB, their pre-treatment employment rates are very high, around 90%, much higher than their counterparts in the general population (especially Arab women and Haredi men, see Strawczynski, 2020).

[11]Recent ALMP studies have started to go beyond the conventional effectiveness evaluation that examines impact in terms of labor market performance. They also examine impact on cognitive and non-cognitive skills (Schlosser and Shanan, 2022); Long-run or intergenerational outcomes, like children's criminal record (Arendt et al., 2021); and some also address quality measures, like the probability to work in large firms (Attansio et al, 2017).

[12]Doer's population is Women that stopped working due to family circumstances, many of which are not eligible to UIB.

The effects are further pronounced for women, low-skilled workers and for people with low labor market attachment, which is consistent with parallel findings in the literature. Additionally, the effect is very strong among participants who found a course-related job, pointing to the importance training participants' relevant course choice.

From a policy point of view, this last finding suggests that governments should invest special resources in maintaining and improving the quality of matching applicants and relevant training, for example, occupational diagnosis, and by exposing potential candidates to opportunities in the relevant timing.

Furthermore, note that while labor market attachment seems to be negatively correlated with program effectiveness, this is not true for the entire range of attachment levels. Those with very low attachment seem to benefit less relative to the rest, which may be due to lacking basic and generic soft skills.

Future work can focus on vouchering, disentangling the effect of training from its financing scheme. To the best of my knowledge, such an analysis has thus far been performed only regarding the German program (Doerr and Strittmatter, 2021; Rinne, Uhlendorff and Zhao, 2013). Hence, performing a similar analysis in other countries may promote scholars and governments' understanding regarding the best manner to administer and finance training programs overall and specifically, for unique sub-groups in the population. Another promising avenue for future research lies in focusing on how training and ALMPs effect transitions between firms and industries among current workers and the unemployed.

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# Tables & Figures

Table 2: Program main characteristics

| *Priority group** | *Subsidy rates, by courses type* | | *Subsidy cap (NIS)* | *Effective maximal subsidy* | *Placement grant (NIS)* | *Refund to participants* |
|---|---|---|---|---|---|---|
| | *preferred* | *all others* | | | | |
| A | 85% | 80% | 9000 | 7.2-7.65K | 2000 | 3 unequal |
| B | 80% | 75% | | 6.75-7.2K | 1500 | shares |

*Note*: Group A includes Arab women, Ultra-orthodox men, migrants from Ethiopia, and people with disabilities. Group B includes all the rest.

Table 3: Main fields of courses (if available)

| Main field | *N* | *Share* | *Gross monthly duration* | |
|---|---|---|---|---|
| | | | mean | s.d. |
| Administration | 4236 | 0.291 | 9.7 | 2.6 |
| Cosmetics | 2869 | 0.197 | 6.4 | 3.6 |
| Transportation | 2006 | 0.138 | 3.6 | 2.8 |
| Computers & technology | 1316 | 0.091 | 7.7 | 2.9 |
| Primary education | 1202 | 0.083 | 8.6 | 2.9 |
| Print, photography & production | 592 | 0.041 | 8.4 | 2.6 |
| Fashion & Textile | 447 | 0.031 | 8.7 | 3 |
| Construction & environment | 412 | 0.028 | 7.8 | 4.4 |
| Electronics, electricity | 378 | 0.026 | 9.4 | 4.1 |
| Metal & machinery | 362 | 0.025 | 5.1 | 3.6 |
| Hospitality | 210 | 0.014 | 5.6 | 3.2 |
| Other | 210 | 0.014 | 6.6 | 2.9 |
| Para-medical | 118 | 0.008 | 8.8 | 3.4 |
| Car mechanics | 74 | 0.005 | 9.6 | 5.1 |
| Sales & marketing | 68 | 0.005 | 6.4 | 2.3 |
| Lifeguards | 28 | 0.002 | 3.9 | 2.6 |
| Jewelry | 12 | 0.001 | 2.7 | 0.6 |
| Total | 14540 | 1 | 7.52 | 3.06 |

Table 4: Sample selection & sample size

**A. *sample-selection***

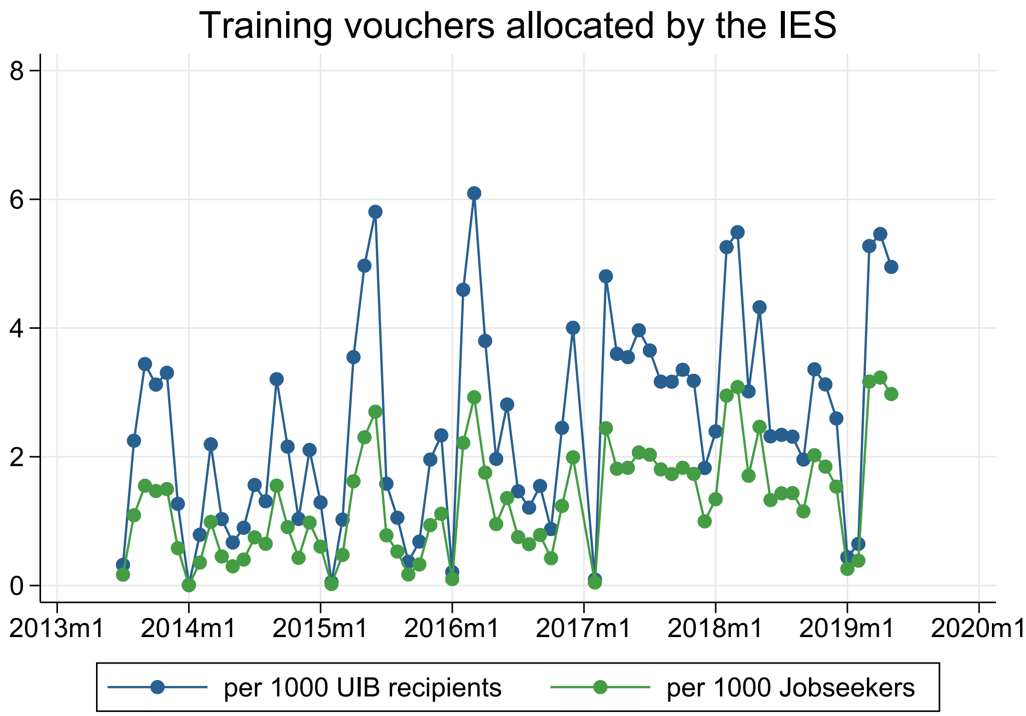
| Group | *N* |
|---|---|
| Full data | 29,565 |
| of which: Aged 18-62 not directed to the program via other channels | 27,206 |
| of which: Received UIB at least once in the 3 months before being counseled | 11,640 |
| of which: Employment observed for at least 24 months from counseling | 7,566 |

**B. *sample size***

| Sample | Nc | Nt | N | Last counseling month |
|---|---|---|---|---|
| h18 | 2653 | 6128 | 8781 | June 2018 |
| h24 | 2290 | 5276 | 7566 | December 2017 |
| h36 | 1613 | 3900 | 5513 | December 2016 |
| h48 ("main sample") | 1140 | 2782 | 3922 | December 2015 |
| h60 | 738 | 1748 | 2486 | December 2014 |
| h70 | 456 | 1067 | 1523 | February 2014 |
| h72 | 395 | 954 | 1349 | December 2013 |

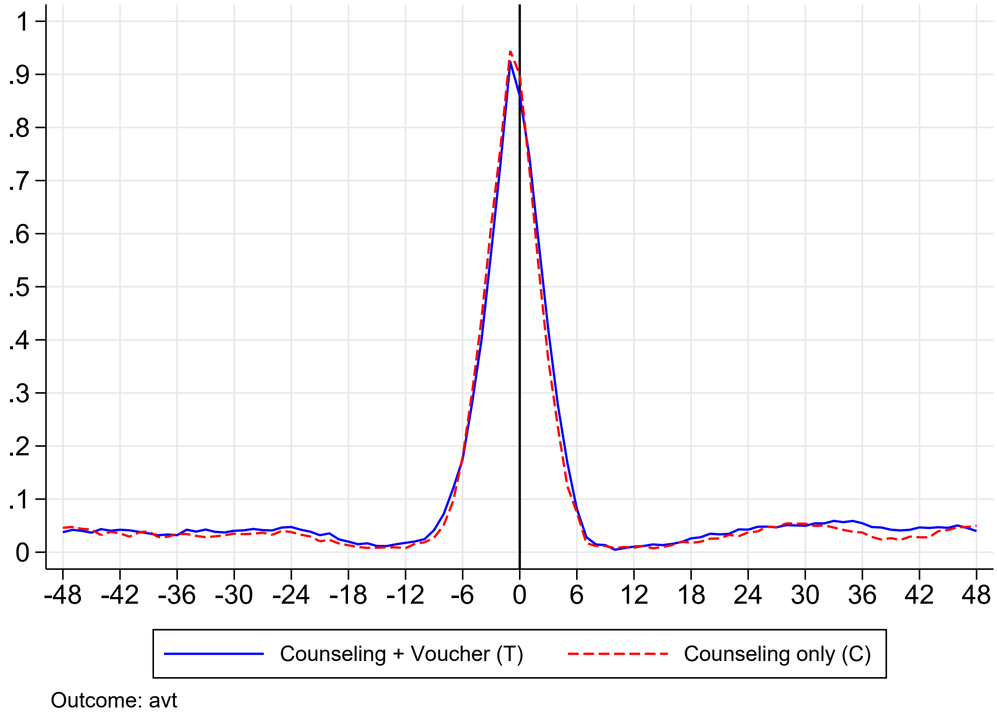
*Note*: this table presents sample-selection, moving from the full relevant population available for the study to the final sample used. Panel B shows the size of the samples used in the paper. The samples differ by the horizon, i.e., number of months observed from treatment.

Figure 1: Training vouchers allocated by the IES, 2013-2019




*Note*: This figure shows the number of vouchers allocated by the Employment Services each month, relative to the number of potential candidates – using the number of job-seekers and UIB recipients that were treated by the IES in that month. As training was probably not relevant for all of the job-seekers, the actual ratio with regard to the actual number of relevant potential applicants, may be somewhat higher.
*Source*: Program Data and IES public data

Figure 2: Share receiving UI benefits, months relative to counseling (h48 sample)




*Note*: This figure shows the share of individuals receiving UIB in the control and treatment groups – in the four years leading to treatment and in the four years after treatment.

Table 5: Balance for the h48 sample, moments for the Treatment and (pre-match) control group

| Variable | Mean | | Variance | | Skewness | | Stand. diff. | |
|---|---|---|---|---|---|---|---|---|
| | T | C | T | C | T | C | *Pre match* | *Post match* |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| ***Demography*** | | | | | | | | |
| Age at counseling | 38.92 | 38.33 | 71.41 | 70.29 | 0.23 | 0.36 | 0.071 | 0.000 |
| Female | 0.63 | 0.57 | 0.23 | 0.25 | -0.56 | -0.28 | 0.136 | 0.000 |
| Migrant (1989+) | 0.21 | 0.18 | 0.16 | 0.15 | 1.44 | 1.68 | 0.072 | 0.000 |
| Married at counseling | 0.66 | 0.63 | 0.23 | 0.23 | -0.66 | -0.55 | 0.053 | 0.000 |
| Ever married at counseling | 0.83 | 0.75 | 0.14 | 0.19 | -1.74 | -1.18 | 0.197 | 0.000 |
| Number of children up to counseling | 2.08 | 1.86 | 2.59 | 2.71 | 1.03 | 0.94 | 0.134 | 0.000 |
| ***Employment history*** | | | | | | | | |
| Wages (-13,-24) | 9947 | 10737 | 5.44E+07 | 1.04E+08 | 2.86 | 5.28 | -0.107 | 0.000 |
| Wages (-25,-36) | 9228 | 9969 | 5.77E+07 | 1.16E+08 | 2.83 | 9.47 | -0.097 | 0.000 |
| ***Economic sector*** | | | | | | | | |
| Manufacturing, Mining and quarrying | 0.14 | 0.12 | 0.12 | 0.11 | 2.13 | 2.31 | 0.040 | 0.000 |
| Electricity, gas & water supply | 0.00 | 0.01 | 0.00 | 0.01 | 13.51 | 12.64 | -0.010 | 0.000 |
| Construction | 0.07 | 0.05 | 0.06 | 0.05 | 3.47 | 3.97 | 0.053 | 0.000 |
| Wholesale & retail trade; repair of motor vehicles... | 0.20 | 0.16 | 0.16 | 0.14 | 1.53 | 1.82 | 0.083 | 0.000 |
| Transportation & storage, postal & courier activities | 0.03 | 0.03 | 0.03 | 0.03 | 5.39 | 5.13 | -0.017 | 0.000 |
| Accommodation and food service activities | 0.04 | 0.05 | 0.04 | 0.05 | 4.78 | 4.09 | -0.062 | 0.000 |
| Information and communication | 0.06 | 0.09 | 0.06 | 0.09 | 3.58 | 2.77 | -0.127 | 0.000 |
| Financial and insurance activities | 0.04 | 0.04 | 0.04 | 0.04 | 4.52 | 4.51 | -0.001 | 0.000 |
| Real estate activities | 0.01 | 0.01 | 0.01 | 0.01 | 10.00 | 8.86 | -0.026 | 0.000 |
| Professional, scientific and technical activities | 0.10 | 0.12 | 0.09 | 0.11 | 2.68 | 2.29 | -0.082 | 0.000 |
| Administrative and support service activities | 0.07 | 0.08 | 0.07 | 0.07 | 3.25 | 3.21 | -0.005 | 0.000 |
| Local administration, public admin. & defense | 0.03 | 0.03 | 0.03 | 0.03 | 5.64 | 5.28 | -0.022 | 0.000 |
| Education | 0.04 | 0.03 | 0.04 | 0.03 | 4.48 | 5.28 | 0.054 | 0.000 |
| Human health and social work activities | 0.06 | 0.05 | 0.06 | 0.05 | 3.64 | 3.97 | 0.035 | 0.000 |
| Arts, entertainment and recreation | 0.02 | 0.02 | 0.02 | 0.02 | 7.42 | 7.77 | 0.011 | 0.000 |
| Other service activities | 0.04 | 0.03 | 0.04 | 0.03 | 4.99 | 5.28 | 0.019 | 0.000 |
| ***Geographic district*** | | | | | | | | |
| Jerusalem | .082 | .059 | .075 | .055 | 3.048 | 3.752 | .085 | 0 |
| North | .188 | .146 | .152 | .125 | 1.6 | 1.999 | .105 | 0 |
| Haifa | .185 | .143 | .151 | .123 | 1.618 | 2.04 | .109 | 0 |
| Center | .191 | .26 | .154 | .192 | 1.576 | 1.096 | -.176 | 0 |
| Tel-Aviv | .099 | .213 | .089 | .168 | 2.681 | 1.401 | -.381 | 0 |
| South | .203 | .153 | .162 | .129 | 1.473 | 1.932 | .126 | 0 |

Note: This table shows Mean, Variance and Skewness and standardized differences for various background variables. The comparison is made between the treatment and the control group before re-weighting, since after re-weighting all moments are identical or almost identical.

Table 6: The effect of training vouchers on employment and wages – two years before, two and four years after treatment

| *months to treatment* | Employment | | | | | Wages | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| -24 | 0.006 | 0.005 | 0.005 | 0.001 | 0.001 | -20 | -18 | -36 | -33 | -40 |
| | [0.011] | [0.011] | [0.015] | [0.015] | [0.015] | [28] | [27] | [38] | [38] | [39] |
| | .863 | .863 | .863 | .863 | .863 | 790 | 790 | 790 | 790 | 790 |
| +24 | 0.047*** | 0.047*** | 0.053*** | 0.053*** | 0.051*** | 1 | 3 | 0 | -7 | -17 |
| | [0.014] | [0.014] | [0.015] | [0.017] | [0.017] | [33] | [33] | [43] | [44] | [48] |
| | .706 | .706 | .706 | .706 | .706 | 709 | 709 | 709 | 709 | 709 |
| +48 | 0.030** | 0.029** | 0.037** | 0.042** | 0.042** | 10 | 9 | 10 | 29 | 24 |
| | [0.014] | [0.014] | [0.016] | [0.021] | [0.021] | [36] | [36] | [38] | [40] | [44] |
| | .72 | .72 | .72 | .72 | .72 | 772 | 772 | 772 | 772 | 772 |
| N | 3922 | 3922 | 3919 | 3902 | 3902 | 3922 | 3922 | 3919 | 3902 | 3902 |
| Controls | | X | X | X | | | X | X | X | |
| Office FE | | | X | X | X | | | X | X | X |
| C.Quart. FE | | | | X | X | | | | X | X |

*Note*: This table shows the effect of receiving a voucher on the probability to be employed and on earnings, in three specific points in time. The sample used is h48, i.e., includes individuals for which wages are observed over a period of 48 months (or more) from counseling.

Table 7: Voucher effect on *cumulative* employment and earnings over 48 & 72 months from treatment

**A. outcomes [0,48], sample 48**

| | Employment | | | | | Wages | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| est. | 1.54*** | 1.81*** | 1.98*** | 2.12*** | 2.02*** | 2,499*** | 3,252*** | 3,061** | 3,111*** | 2,465* |
| s.e. | [0.46] | [0.46] | [0.53] | [0.62] | [0.62] | [885] | [847] | [1247] | [1123] | [1270] |
| c. mean | 32.86 | 32.86 | 32.86 | 32.86 | 32.86 | 34014 | 34014 | 34014 | 34014 | 34014 |
| N | 3922 | 3922 | 3919 | 3902 | 3902 | 3922 | 3922 | 3919 | 3902 | 3902 |

**B. outcomes [0,72], sample 72**

| | Employment | | | | | Wages | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| est. | 5.50*** | 5.6*** | 5.52*** | 5.95*** | 5.94*** | 8,476*** | 9,191*** | 8,256*** | 10,104*** | 8,689*** |
| s.e. | [1.16] | [1.16] | [1.75] | [2.27] | [2.28] | [2246] | [2119] | [3077] | [2660] | [3238] |
| c. mean | 47.9 | 47.9 | 47.9 | 47.9 | 47.9 | 49254 | 49254 | 49254 | 49254 | 49254 |
| N | 1349 | 1349 | 1343 | 1303 | 1303 | 1349 | 1349 | 1343 | 1303 | 1303 |
| Controls | | X | X | X | | | X | X | X | |
| Office FE | | | X | X | X | | | X | X | X |
| C.Quart. FE | | | | X | X | | | | X | X |

Note: This table shows the effect of receiving a voucher on the number of months employed, and on earnings – accumulated over a period of 48, or 72, months after treatment.

Figure 3: Trajectories of employment and earnings and the estimated effect for the period (-48,48)

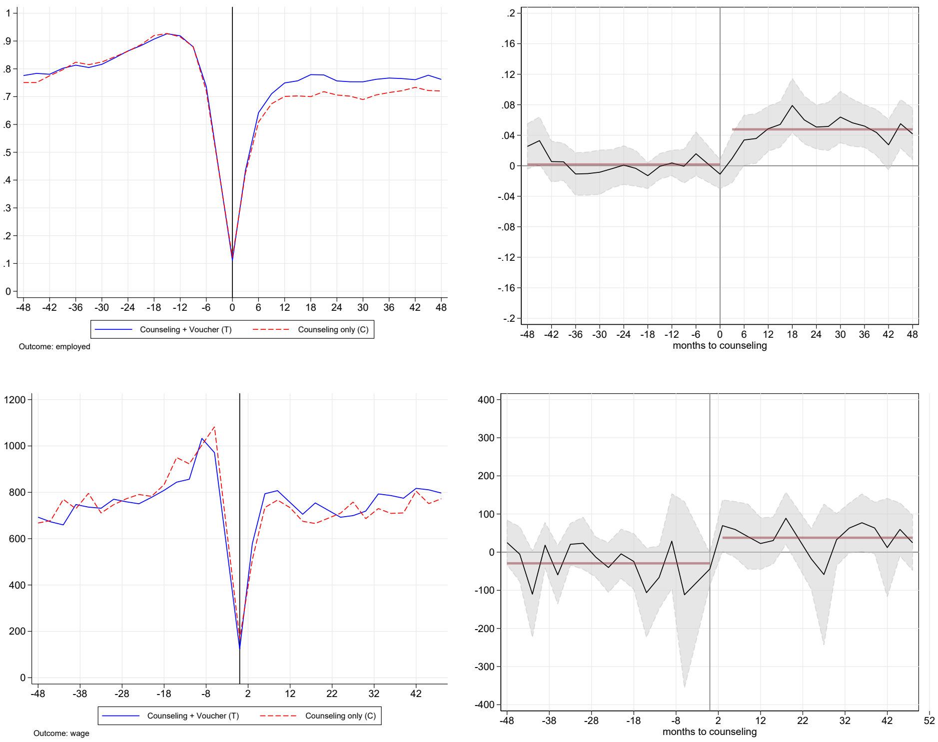



Figure 4: Trajectories of the cumulative number of months employed and cumulative earnings in the period (0,48)

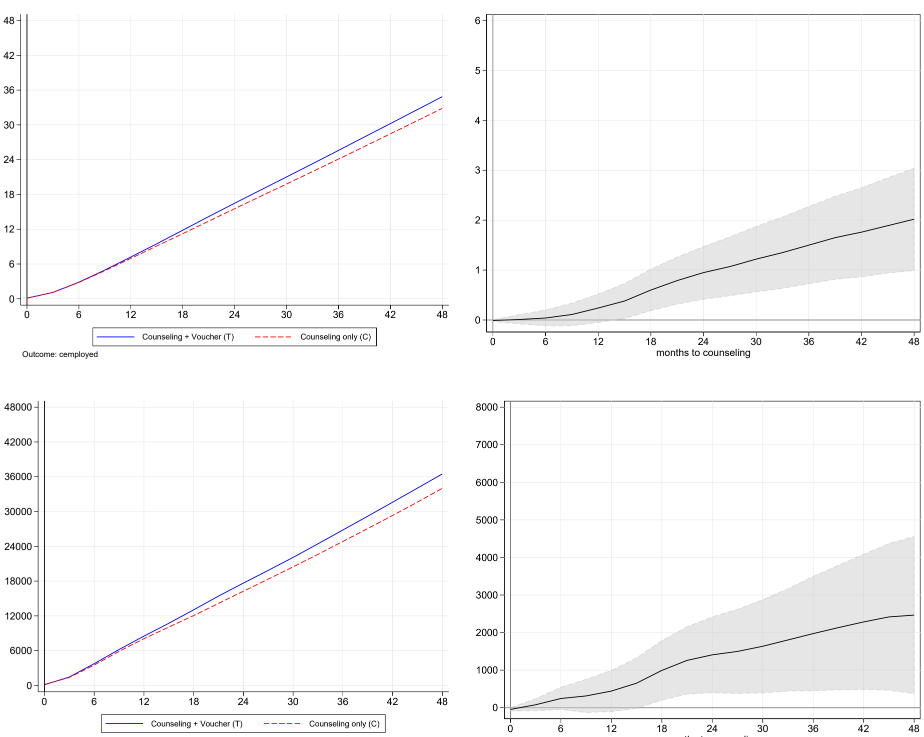


Table 8: Voucher effect on *cumulative* employment and earnings over 48 months, by human capital and training characteristics

| | *All* | *Education* | | *Training status* | |
|---|---|---|---|---|---|
| | | *some college* | *no college* | *Completed Training* | *Found a course-related job* |
| Employment | 2.02*** | 0.610 | 3.200*** | 2.110** | 4.950*** |
| | [0.62] | [1.040] | [0.890] | [0.850] | [1.030] |
| | 32.86 | 34.47 | 31.57 | 32.72 | 32.9 |
| Wages | 2,465* | 4,440* | 3,282** | 601 | 4,560** |
| | [1270] | [2460] | [1380] | [1472] | [1862] |
| | 34014 | 40176 | 30098 | 32322 | 35015 |
| N | 3902 | 1323 | 2525 | 2093 | 1445 |
| Controls | | | | | |
| Office FE | X | X | X | X | X |
| C.Quart. FE | X | X | X | X | X |

Note: This table shows the effect of receiving a voucher on the number of months employed, and the earnings accumulated over a period of 48 months after treatment.

Table 9: Voucher effect on *cumulative* employment and earnings, by employment history

| | *short term non-employment worked 28+ months in (-36,-1)* | | | | *long term non-employment worked up to 27 months in (-36,-1)* | | | |
|---|---|---|---|---|---|---|---|---|
| *Horizon* | 24 | 36 | 48 | 48 | 24 | 36 | 48 | 48 |
| *Time* | 24 | 36 | 36 | 48 | 24 | 36 | 36 | 48 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Employment | 0.78*** | 1.135** | 1.15** | 1.58** | 0.85* | 2.05** | 2.33** | 3.10** |
| | [0.286] | [0.452] | [0.557] | [0.738] | [0.481] | [0.825] | [1.029] | [1.292] |
| | 16.1 | 25.3 | 25.38 | 34.5 | 14 | 21.4 | 21.5 | 29.5 |
| Earnings | 665 | 571 | 604 | 684 | 1,544** | 2,504* | 2,393* | 2,947 |
| | [505] | [934] | [1218] | [1638] | [710] | [1371] | [1397] | [1823] |
| | 18432 | 28192 | 28158 | 38516 | 13591 | 20367 | 19802 | 27113 |
| *N* | 5194 | 3795 | 2699 | 2699 | 2342 | 1671 | 1152 | 1152 |

*Note*: This table shows the effect of receiving a voucher on the number of months employed, and the earnings accumulated over a period of 48 months after treatment – stratified by the number of months worked in the period (-36,-1).

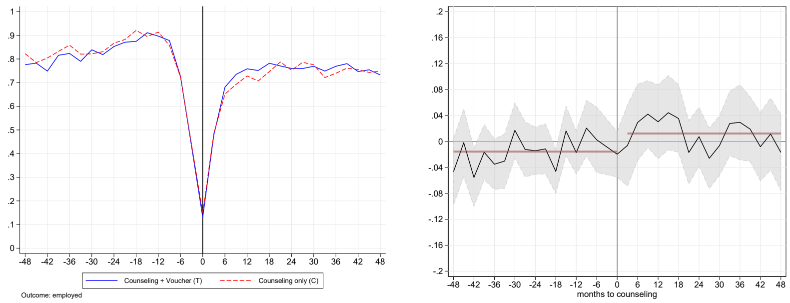
Table 10: Treatment effect on employment and wages by group

| Sample | *Income support recipients* | | |
|---|---|---|---|
| time & horizon | 18-18 | 24-24 | 48-48 |
| Employed | 0.081*** | 0.071** | 0.081** |
| | [0.028] | [0.028] | [0.033] |
| | 0.434 | 0.449 | 0.483 |
| Ever employed | 0.079*** | 0.087*** | 0.084*** |
| post treatment | [0.027] | [0.025] | [0.021] |
| | 0.629 | 0.667 | 0.775 |
| Months employed | 0.79* | 1.025* | 1.503 |
| | [0.43] | [0.59] | [1.429] |
| | 6.91 | 9.54 | 20.25 |
| Cumulative wages | 906*** | 1,257*** | 2,231** |
| | [321] | [480] | [1012] |
| | 4004 | 5638 | 12390 |
| Wage above | 0.013 | 0.017 | 0.018 |
| historical | [0.018] | [0.016] | [0.022] |
| record | 0.154 | 0.172 | 0.222 |
| *N* | *3708* | *3405* | *1965* |
| Never out if employed | 0.007 | 0.01 | -0.015 |
| post-treatment | [0.031] | [0.028] | [0.039] |
| | 0.313 | 0.265 | 0.175 |
| *N* | *2891* | *2656* | *1247* |

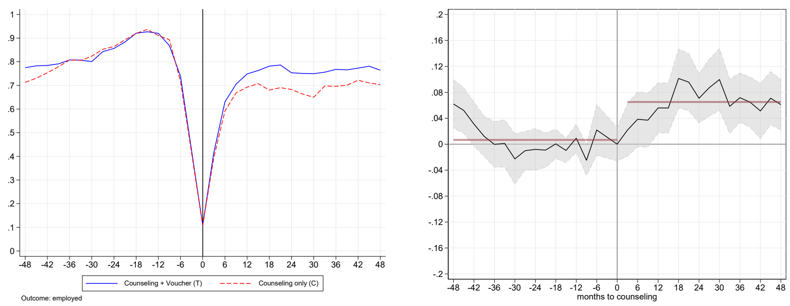
*Note*: This table shows the effect on the main outcomes among participants that received income support at least once in the 36 months leading to treatment.

Figure 5: Employment and the estimated effect over the period (-48,48), by education

A. *Any college*



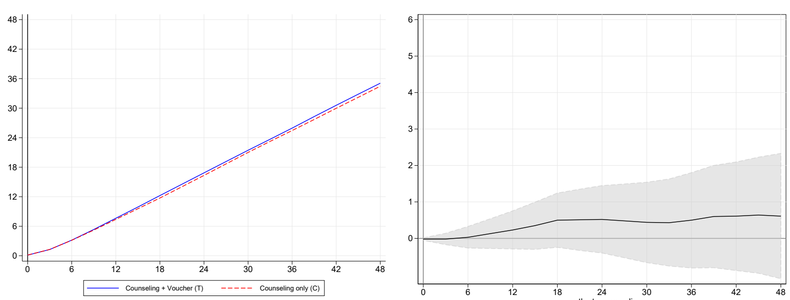

B. *no college*



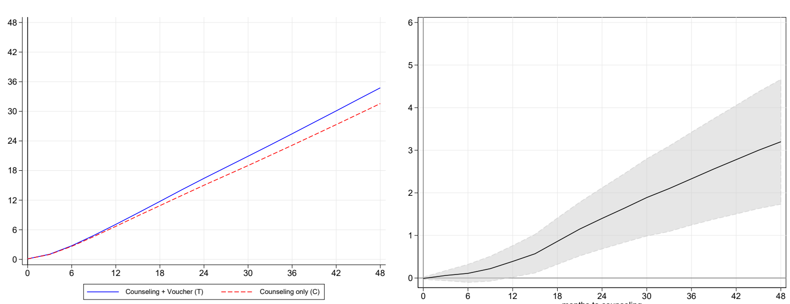

*Note*: This figure shows the employment trajectory for the control and treatment groups, and the estimated effect, by educational attainment.

Figure 6: Cumulative employment and the estimated effect over the period (0,48), by education

A. *Any college*



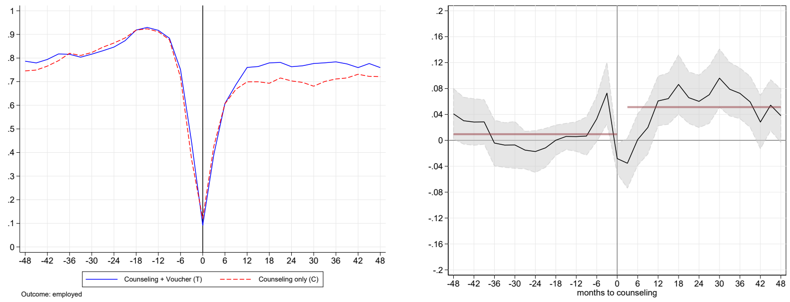

B. *no college*




*Note*: This figure shows the cumulative employment trajectory for the control and treatment groups, and the estimated effect, by educational attainment.

Figure 7: Employment trajectories & estimated effect, by training status & FCRJ status

A. *Completed training* (T) vs. *Counseled only* (C)




B. Trained & found a course related job (T) vs. *Counseled only* (C)

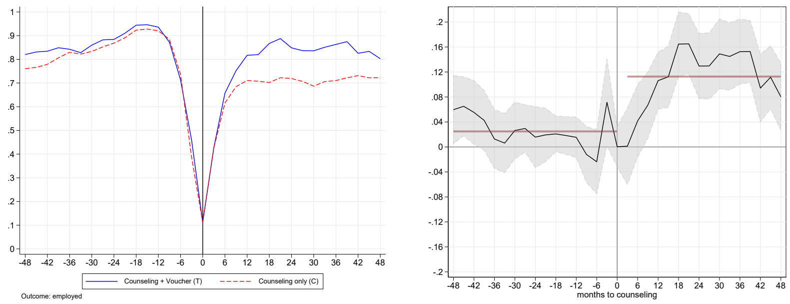
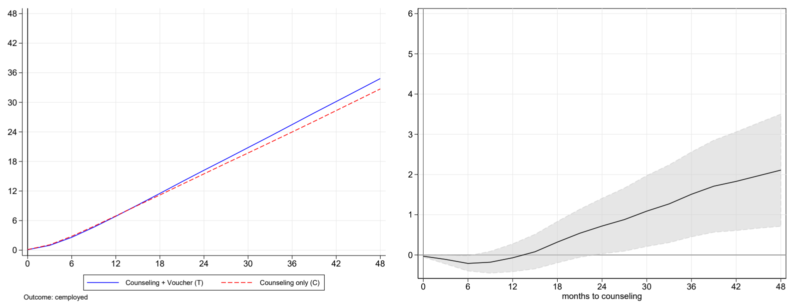



Figure 8: Cumulative employment & the estimated effect, by training status & FCRJ status

A. *Completed training* (T) vs. *Counseled only* (C)




B. Found a course related job (T) vs. *Counseled only* (C)

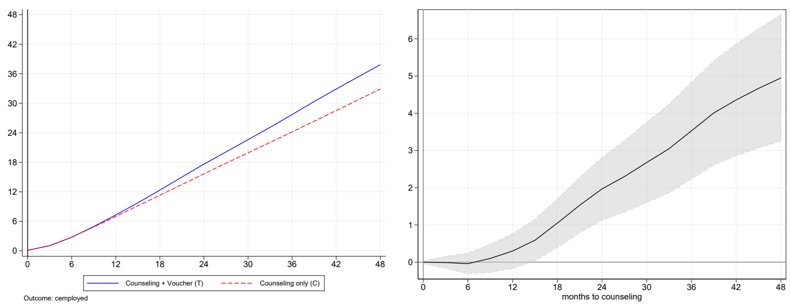
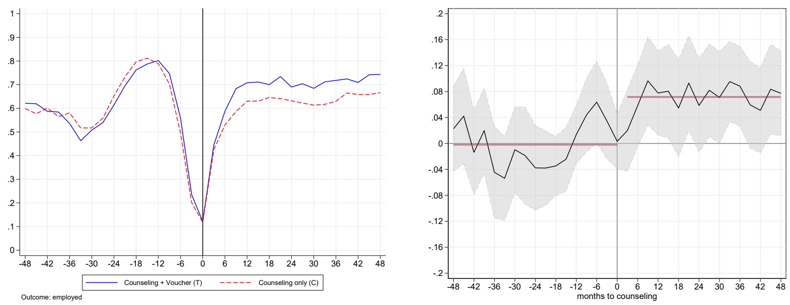
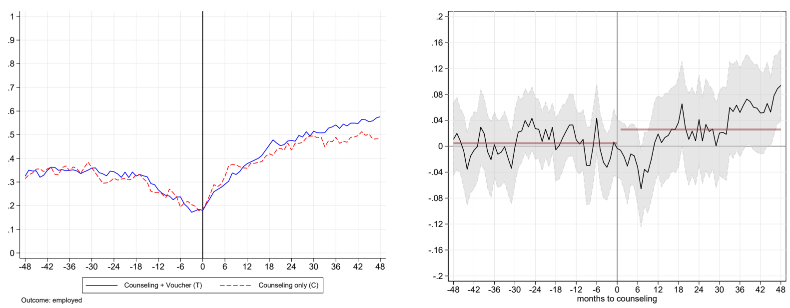


Figure 9: Effect on employment, among workers with weak LM attachment (-48,48)

A. *long-term unemployed*



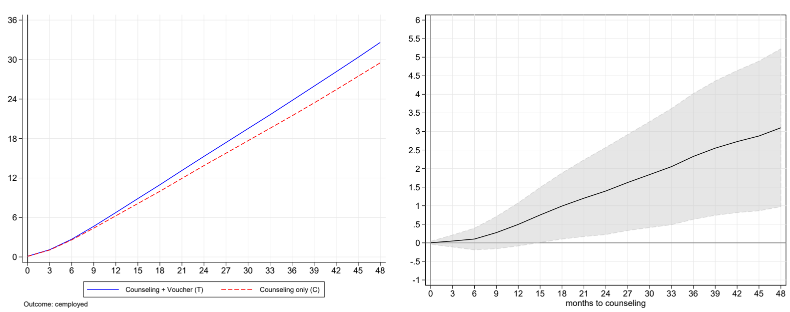

B. *Income support recipients*



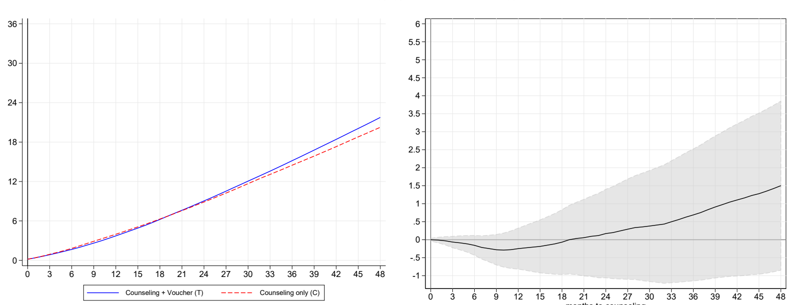

*Note*: This figure shows the employment trajectory for the control and treatment groups, and the estimated effect, among income support recipients and among long-term unemployed (employed up to 27 months within the 36 months pre-treatment).

Figure 10: Effect on *cumulative* employment, among workers with weak LM attachment(0,48)

A. *long-term unemployed*




B. *Income support recipients*




*Note*: This figure shows the employment trajectory for the control and treatment groups, and the estimated effect, among income support recipients and among long-term unemployed (employed up to 27 months within the 36 months pre-treatment).

Table 11: Voucher effect on *cumulative* employment and wages over 48 months, by sub-groups

**by demographic characteristics**

| | All | Gender | | Ethnicity | | Age | |
|---|---|---|---|---|---|---|---|
| | | Men | Women | Arabs | Jews | <40 | 40+ |
| Employment | 2.02*** | 1.79** | 2.65*** | 3.62 | 2.12*** | 1.64** | 3.55*** |
| | [0.62] | [0.89] | [0.87] | [2.46] | [0.68] | [0.81] | [1.2] |
| | 32.86 | 33.98 | 32.03 | 28.24 | 33.02 | 33.13 | 32.34 |
| Wages | 2,465* | 4,722** | 2,023 | 1,809 | 2,918** | 1,078 | 4,639* |
| | [1270] | [1976] | [1444] | [4611] | [1370] | [1484] | [2370] |
| | 34014 | 44088 | 28079 | 28951 | 34555 | 33518 | 34417 |
| N | 3902 | 1461 | 2384 | 557 | 3313 | 2240 | 1603 |
| Controls | | | | | | | |
| Office FE | X | X | X | X | X | X | X |
| C.Quart. FE | X | X | X | X | X | X | X |

*Note*: This table shows the effect of receiving a voucher on the number of months employed, and on earnings accumulated over a period of 48 months after treatment – among the main demographic groups.

Table 12: Treatment effect on employment and wages by group & sub-group

| | Arabs | | | | Haredi | | | |
|---|---|---|---|---|---|---|---|---|
| | 24-24 | 18-18 | | | 24-24 | 18-18 | | |
| Group | *All* | *All* | *Men* | *Women* | *All* | *All* | *Men* | *Women* |
| Employed | .113** | 0.114** | 0.206*** | 0.058 | -.036 | 0.016 | 0.209** | -0.110*** |
| | [.046] | [0.045] | [0.053] | [0.093] | [.06] | [0.035] | [0.089] | [0.041] |
| | .605 | 0.643 | 0.588 | 0.687 | .724 | 0.678 | 0.569 | 0.723 |
| Ever employed | 0.068** | 0.076** | 0.070* | 0.055 | -0.001 | 0.045 | 0.182** | -0.026 |
| post treatment | [0.034] | [0.034] | [0.041] | [0.066] | [0.029] | [0.030] | [0.076] | [0.045] |
| | 0.867 | 0.823 | 0.841 | 0.824 | 0.915 | 0.855 | 0.791 | 0.878 |
| Months employed | 1.730** | 1.201* | 1.607* | 0.649 | 0.571 | 0.614 | 3.459** | -0.747 |
| | [0.845] | [0.659] | [0.868] | [1.110] | [0.864] | [0.594] | [1.372] | [0.874] |
| | 13.83 | 9.911 | 10.551 | 9.964 | 14.23 | 10.508 | 10.007 | 10.751 |
| Cumulative wages | 4,182*** | 2,988*** | 3,326*** | 1,244 | 1,824* | 1,615* | 4,025** | 271 |
| | [1069] | [784] | [1176] | [941] | [985] | [870] | [2010] | [1134] |
| | 12198 | 9024 | 12340 | 6443 | 13418 | 10039 | 13255 | 8894 |
| Wage above | 0.002 | 0.003 | 0.046 | 0 | -0.034 | -0.015 | -0.099* | -0.003 |
| historical | [0.026] | [0.031] | [0.029] | [0.035] | [0.035] | [0.023] | [0.052] | [0.035] |
| record | 0.066 | 0.075 | 0.038 | 0.106 | 0.113 | 0.078 | 0.13 | 0.051 |
| *N* | *1172* | *1330* | *759* | *550* | *615* | *736* | *224* | *450* |
| Never out if emp. | 0.066 | 0.034 | 0.067 | -0.024 | -0.118** | -0.068 | 0.093 | -0.118 |
| post-treatment | [0.054] | [0.049] | [0.062] | [0.091] | [0.057] | [0.053] | [0.087] | [0.077] |
| | 0.398 | 0.48 | 0.447 | 0.581 | 0.425 | 0.461 | 0.431 | 0.469 |
| *N* | *1093* | *1236* | *716* | *496* | *574* | *673* | *201* | *410* |

*Note*: This table shows the effect of receiving a voucher on the number of months employed, and on earnings accumulated over a period of 48 months after treatment – among the main demographic groups and gender, across different horizons.

## Robustness

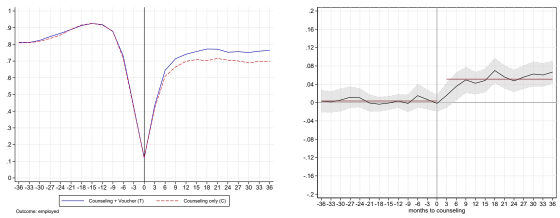
Table 13: Estimates for employment – across different samples and distances from treatment

| *Months* | Horizon | | | | |
|---|---|---|---|---|---|
| *from* | 24 | 36 | 48 | 60 | 72 |
| *treatment* | (1) | (2) | (3) | (4) | (5) |
| 6 | 0.020 | 0.080 | 0.040 | 0.110 | 0.450** |
| | [0.080] | [0.080] | [0.100] | [0.140] | [0.200] |
| | 2.81 | 2.8 | 2.82 | 2.88 | 2.66 |
| 12 | 0.220* | 0.270* | 0.240 | 0.290 | 0.940*** |
| | [0.130] | [0.150] | [0.170] | [0.250] | [0.340] |
| | 6.91 | 6.95 | 6.97 | 7.11 | 6.58 |
| 18 | 0.530*** | 0.600*** | 0.600** | 0.720* | 1.530*** |
| | [0.180] | [0.210] | [0.250] | [0.370] | [0.520] |
| | 11.16 | 11.22 | 11.23 | 11.38 | 10.54 |
| 24 | 0.820*** | 0.910*** | 0.950*** | 1.120** | 2.100*** |
| | [0.230] | [0.260] | [0.320] | [0.470] | [0.710] |
| | 15.43 | 15.54 | 15.54 | 15.68 | 14.6 |
| 30 | | 1.180*** | 1.220*** | 1.480** | 2.790*** |
| | | [0.320] | [0.400] | [0.580] | [0.910] |
| | | 19.8 | 19.8 | 19.92 | 18.62 |
| 36 | | 1.500*** | 1.500*** | 1.810*** | 3.420*** |
| | | [0.370] | [0.470] | [0.680] | [1.110] |
| | | 24.1 | 24.1 | 24.25 | 22.71 |
| 42 | | | 1.760*** | 2.060*** | 4.090*** |
| | | | [0.540] | [0.780] | [1.290] |
| | | | 28.46 | 28.65 | 26.8 |
| 48 | | | 2.020*** | 2.280** | 4.470*** |
| | | | [0.620] | [0.910] | [1.470] |
| | | | 32.86 | 33.1 | 31.0 |
| 54 | | | | 2.450** | 4.870*** |
| | | | | [1.040] | [1.680] |
| | | | | 37.5 | 35.3 |
| 60 | | | | 2.660** | 5.330*** |
| | | | | [1.170] | [1.880] |
| | | | | 41.8 | 39.5 |
| N | 7556 | 5503 | 3902 | 2450 | 1303 |

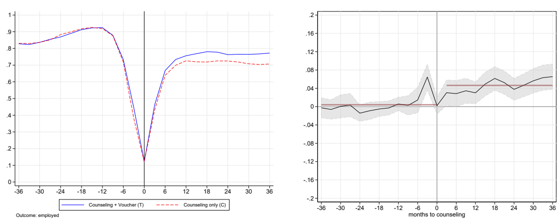
*Note*: this table shows the estimates for the treatment effect on employment across different samples and different distances, where each distance is examined via more than one sample.

Figure A11: Employment trajectories & the treatment effect, across the main demographic groups

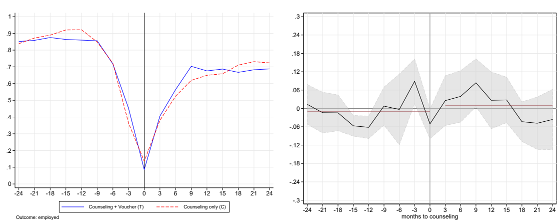
*All* [-36,36]



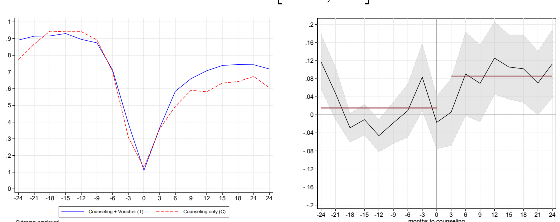

*Jews* [-36,36]




*Ultra-orthodox Jews* [-24,24]




*Arabs* [-24,24]




*Arab men* [-18,18]

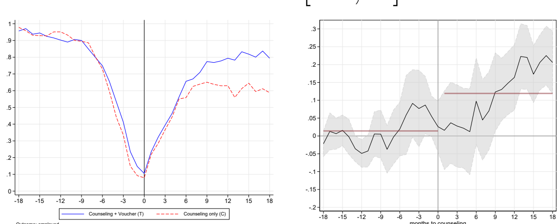
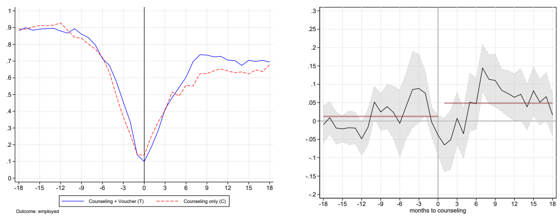
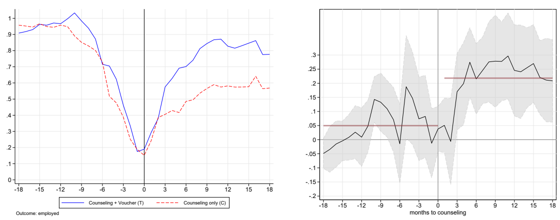


Figure A12: Employment trajectories & treatment effect, across demographic sub-groups (-18,18)

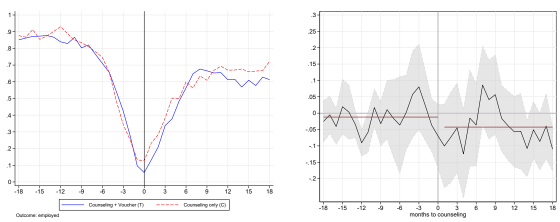
*Ultra-orthodox Jews*



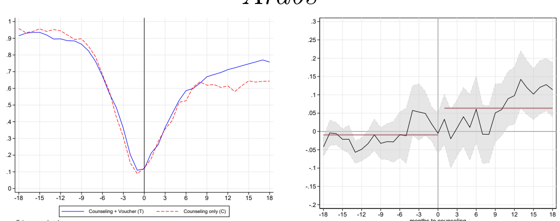

*Ultra-orthodox men*



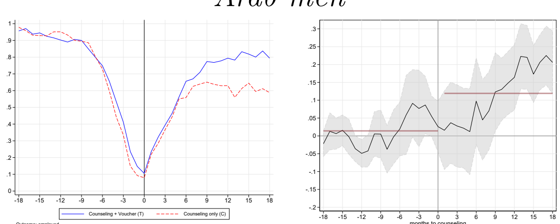

*Ultra-orthodox women*




*Arabs*




*Arab men*




*Arab women*

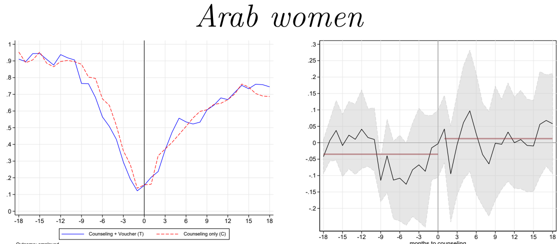
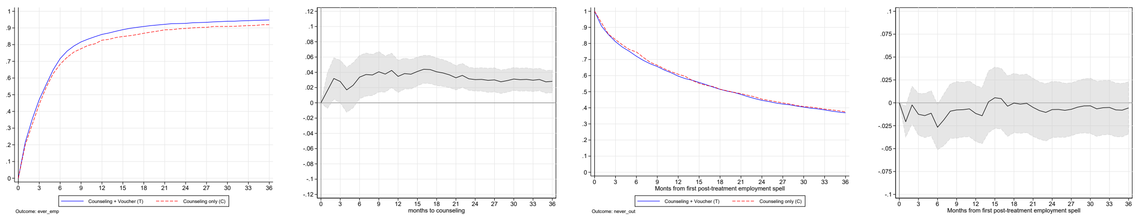
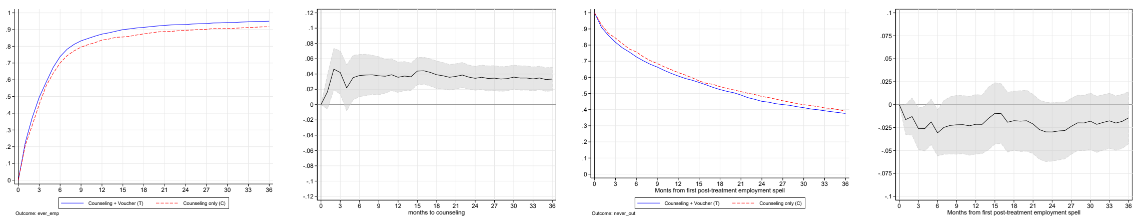


Figure A13: Ever-back to the labor market (columns 1-2) and Never-out again (c. 3-4), by group

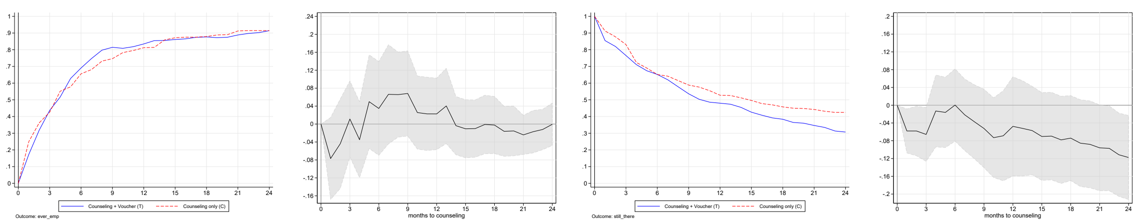
*All* [-36,36]



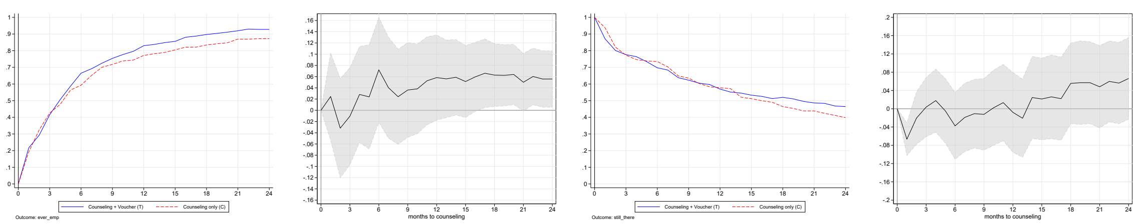

*Jews* [-36,36]




*Ultra-orthodox Jews* [-24,24]




*Arabs* [-24,24]




*Arab men* [-18,18]

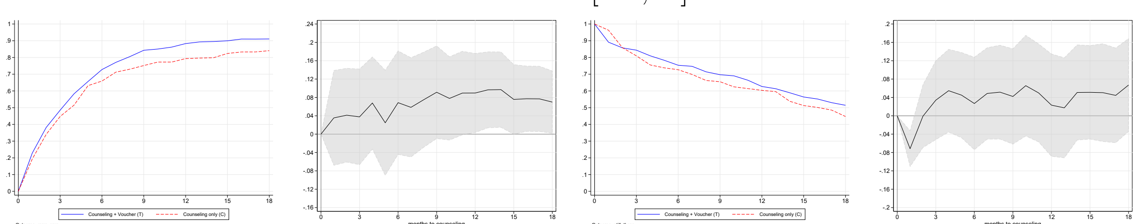
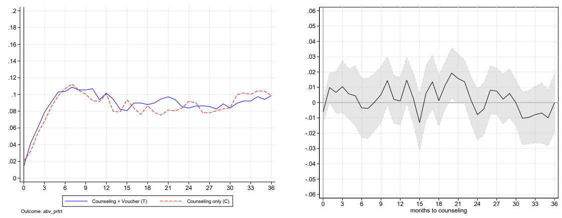
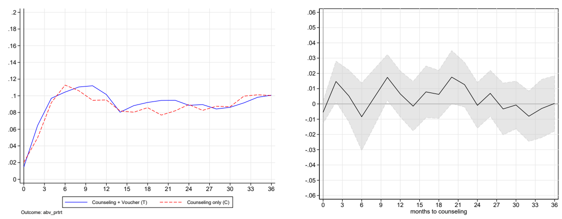


Figure A14: Wage above historical record, across the main demographic groups

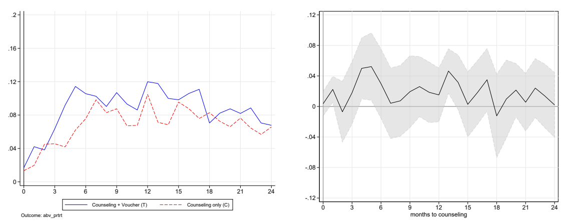
*All* [-36,36]




*Jews* [-36,36]




*Arabs* [-24,24]




*Ultra-orthodox Jews* [-24,24]

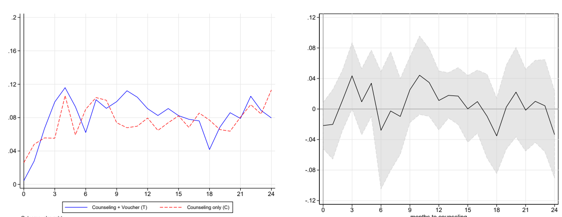
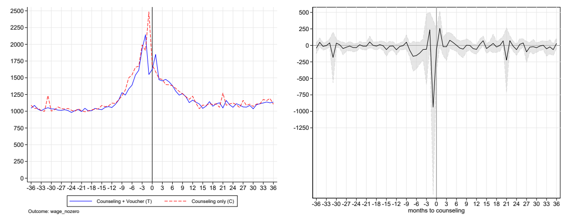
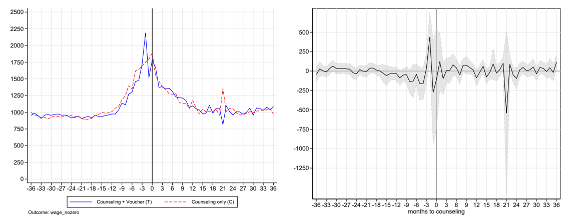


Figure A15: Trajectories and the treatment effect on positive wage

*All*




*No college*




*Any college*

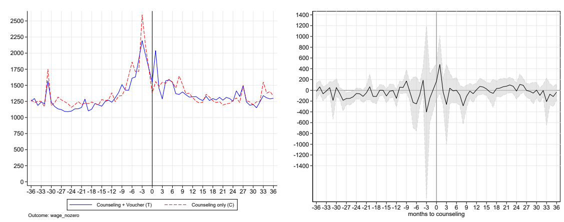


Figure A16: Employment and earning trajectories and the estimated effect for the period (-70,70)

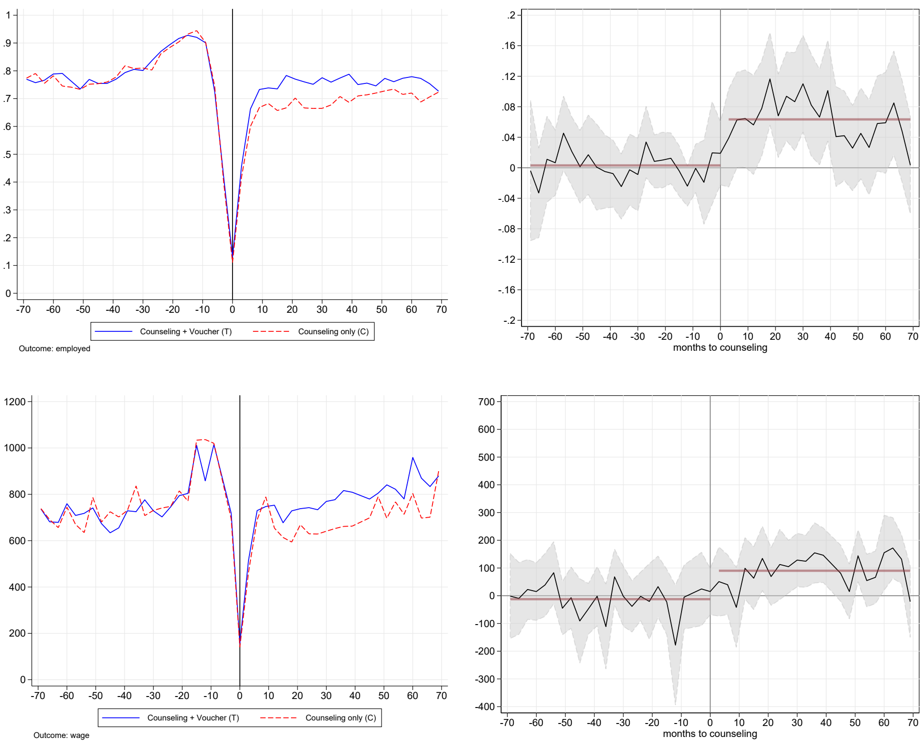
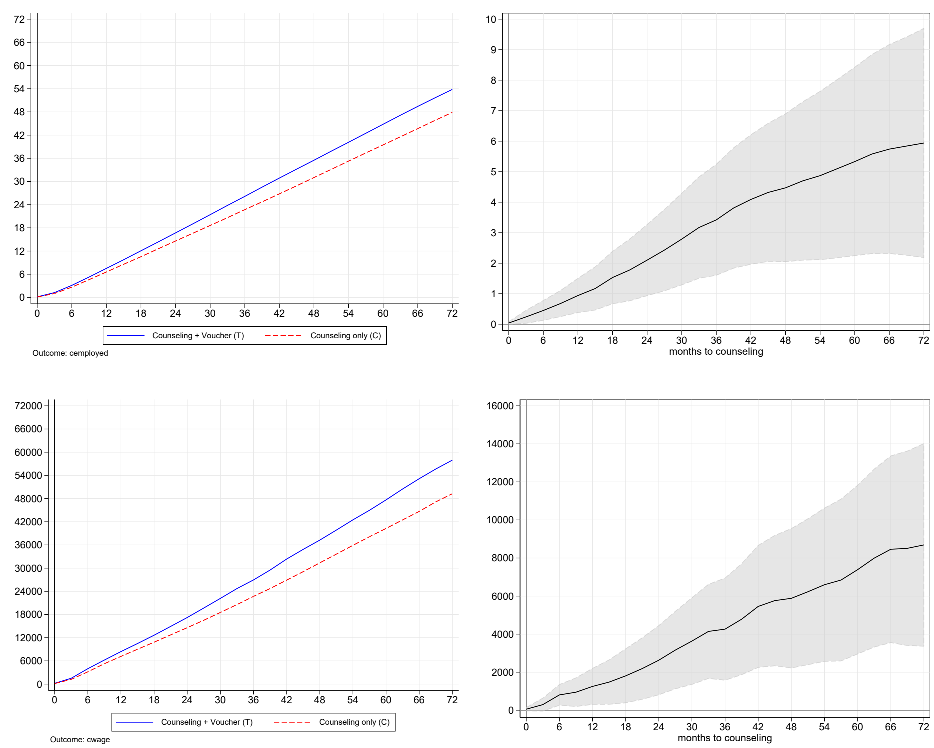



Figure A17: Cumulative number of months employed and earnings for the period (0,72)



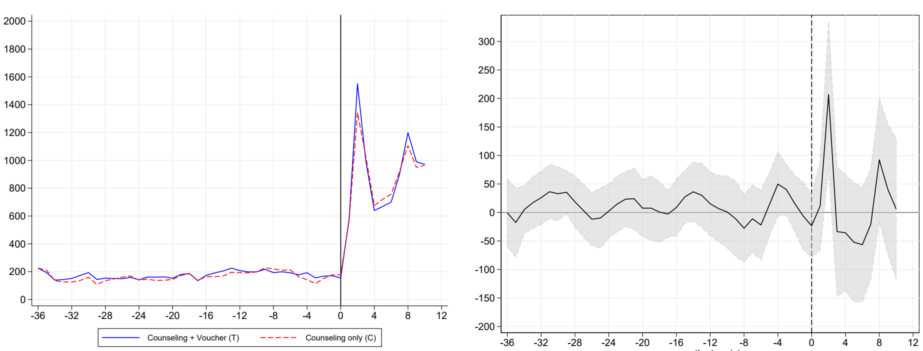
## Covid

Table A14: Voucher effect on the main outcomes before and through the Covid crisis

| | *Month to crisis outbreak* | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Outcome | -24 | -12 | -6 | 0 | 2 | 4 | 6 | 8 | 10 |
| UIB (any) | -0.003 | 0.005 | 0.002 | 0.003 | 0.038** | -0.002 | -0.001 | 0.032* | 0.021 |
| | [0.009] | [0.009] | [0.009] | [0.008] | [0.018] | [0.014] | [0.014] | [0.017] | [0.017] |
| | .045 | .045 | .048 | .04 | .28 | .15 | .15 | .23 | .19 |
| UIB sum | -5 | 16 | -2 | 9 | 216** | -27 | -26 | 120 | 89 |
| | [35] | [44] | [42] | [41] | [95] | [76] | [69] | [86] | [90] |
| | 169 | 182 | 200.3 | 164 | 1307 | 704 | 756 | 1098 | 913 |
| Benefit (any) | -0.002 | 0.009 | 0.001 | 0.009 | 0.040** | -0.003 | -0.000 | 0.033* | 0.019 |
| | [0.011] | [0.010] | [0.010] | [0.009] | [0.018] | [0.015] | [0.015] | [0.018] | [0.018] |
| | .066 | .065 | .065 | .055 | .298 | .174 | .174 | .25 | .21 |
| N | 3902 | 3902 | 3902 | 3902 | 3902 | 3902 | 3902 | 3902 | 3902 |

Note: This table shows the effect of receiving a voucher on benefits before and throughout the crisis period. Note that outcomes are aligned to the crisis (February is defined to be zero) and not to counseling month (which is participant sepecific). The sample used is the h48, the main sample used in the paper, i.e., people that received employment counseling up to December 2015.

Figure A18: The sum of UI benefits received 36 months pre-crisis and in the first 10 months of the crisis



*Chapter end.*

# Conclusion

This section offers a short integrative discussion of the results and points to the main lessons that can be learned based on the studies' findings included in this dissertation. I point to additional questions that can be discussed in future studies.

The first study shows that adopting rules for the purpose of allocating public resources can be an effective tool for restraining political favoritism. It seems that the effective adoption of rules also relates to placing the responsibility for shaping the rules at the hands of an independent organization. Economically weak and minority groups seem to be the most likely to benefit from the move to rules-based allocation because they are characterized by lower accessibility to decision makers, whose importance is higher when allocation is based on discretion.

The second study shows that changing the relative costs of benefits is an effective tool for changing the behavior of disadvantaged groups (income support recipients). Specifically, this adjustment included raising the costs of preserving the current benefit, alongside lowering the costs of applying to another. The change in behavior was reflected in increased employment and earnings and a shift from income support to DI benefits. Members of the minority group – Arabs and especially Arab women – were less responsive to the program. Lower responsiveness could also be observed among people with weak(er) employment backgrounds and among participants who have a higher level of education relative to less-skilled participants (primary education or less).

The third chapter shows that short training for the unemployed has a significant and long-term effect on employment, while it does not trigger a significant improvement in participants' productivity (wages) relative to pre-treatment levels. Unemployed individuals with low labor market attachment (pre-training), including minority men, are more strongly affected by such training, but this is not true for those with very weak labor market attachment – including income support recipients and minority women.

How can the different findings relate? A relevant comparison can be made based on the heterogeneous analysis. Among the unemployed, those who have a weaker attachment, including minorities, benefited more than others did from training (Chapter 3). In contrast, among income support recipients, already a very constrained group, the weaker segment of participants benefited less (Chapter 2). A possible explanation that brings together the two findings is that the stronger among the weaker group and the weaker among the stronger group come from a similar population (similar distribution), that is, a population for which ALM programs are the most effective. In addition, the stronger among the stronger group seem to be successful without external help; hence, the weaker treatment effect of training among this sub-group. The weaker among the weaker group is too constrained to respond either via the formal labor market or via move between benefits; hence, the small or null treatment effect of the program among that sub-group and the increased tendency to disappear from formal records.

A similar argument can be made in the case of municipalities (Chapter 1): both the economically weaker localities and the minority localities gained from the reform, but the former gained more than the latter did. This may be due to that, even after the reform, Arab localities were less competent to utilize the increased stream of grants (recall that we observe the actual use of the grants, rather than eligibility). Although these are institutions (municipalities) rather than individuals, the argument is rather similar. Providing specific services and programs or introducing reforms cannot "work like magic" because it interacts with concrete constraints and incentives that are present at baseline (pre-treatment).

Thus, a few additional and more general lessons can be learned from the main findings. Some of these lessons directly relate to minorities and disadvantaged groups, while others are more general and not necessarily related to specific social groups. Numerous lessons and conclusions are focused on theoretical matters, while others are more methodological.

First, when referring to a move from baseline, universalization and formulization should in many cases be seen as key for improving the situation of the worst-off and minority groups – provided that at baseline they are not discriminated for the better (e.g., do not benefit from affirmative action).

Second, it seems that minorities, especially minority women, have different sensitivity to the costs of receiving benefits, relative to other disadvantaged individuals who belong to the majority group. This is probably due to differences in financial and family constraints and due to lower labor market attachment so that a change in incentives is not always enough.

Third, although stratifying results by gender is a conventional step in assessing the effectiveness of services, programs and reforms, such a split may have an even stronger importance in the case of minorities. This seems especially true in the context of labor markets and when working with minorities who belong to traditional societies because such groups usually differ from the majority in the relevant aspects, for example, women's labor participation. These characterizations also have direct methodological implications. It points to the importance of performing the analysis at the level of groups and sub-groups, and it justifies using methods that emphasize the order – thereby also the relative importance – of different background variables in explaining variance of the dependent variable, for example, regression trees and principal component analysis.

Fourth, (mapping) the situation of minorities at baseline – and especially pointing to the differences between the minority and the majority groups – is crucial for interpreting main estimates. For example, because low labor market attachment in many cases characterize minorities, they may benefit from different services to a greater or lesser extent, relative to the majority. More specifically, the situation at baseline relates to the process of sorting into the pool of potential candidates. As sorting into that pool can differ between the majority and the minority, the gaps between the two groups regarding a single service can be wider or narrower – in comparison to a parallel gap between the two groups in the case of a uniform sorting. (A uniform sorting for this matter can mean, for example, that participants from Group A belong to the bottom earnings decile, as do participants from Group B, or that participants from Group A belong to the bottom decile of Group A's earnings distribution, while participants from Group B belong to the bottom decile of Group B's earnings distribution. Non-uniform sorting would breach this pattern.)

Finally, note that most of the lessons mentioned regarding minorities were studied as typical to minorities or as common attributes among minorities, but not necessarily as exclusively unique to minorities as such. This points to a promising direction for future research that could focus on effectiveness evaluation in the context of minorities. This means utilizing variance in the proximity between the unemployed and caseworkers, or analyzing the quality, effectiveness and impartiality of services provided to minorities in and around times of social tensions. [1]

[1] While not numerous, a number of studies have started walking in this direction, e.g., Glover (2019); Behncke, Frölich and Lechner (2010). This direction has also been examined in papers that study the effectiveness of health services (e.g., Howard et al., 2001).

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**תקציר**

עבודת גמר זו בוחנת את היעילות של מספר צעדי מדיניות, על ידי בחינת ההתנהגות והמצב של קבוצות חברתיות שונות, במסגרת מספר מערכים מחקריים. בצורה זו מכוונת העבודה להסקת מסקנה כללית בנוגע לתנאים להיווצרותה של מדיניות אפקטיבית במקרה של קבוצות-מיעוט וקבוצות חלשות-כלכלית. עבודת הגמר כוללת שלושה פרקים, אשר כל אחד מהם בוחן את האפקטיביות בהקשר אחר: רפורמה בהקצאת מענקים ממשלתיים לרשויות מקומיות, תכנית אקטיביזציה למקבלי הבטחת הכנסה, ותכנית הכשרה מקצועית למובטלים. המשותף לשלושת המאמרים הוא ההתמקדות באפקטיביות, בחקר מקרים ישראליים והמרכזיות של קבוצות-מיעוט. המאמרים נבדלים זה מזה מבחינת יחידת הניתוח והמתודולוגיה. הפרק הראשון בוחן את ההשפעה של רפורמה (ניסוי טבעי) על המענקים המתקבלים על-ידי רשויות מקומיות, תוך שימוש בשיטת הפרש-הפרשים. הפרק השני עושה שימוש בניסוי מבוקר על-מנת לבחון את ההשפעה של תוכנית אקטיבציה על תוצאות בשוק העבודה של מקבלי הבטחת הכנסה מרובי-חסמים. הפרק השלישי הוא מחקר תצפיתי, המבוסס על יצירת התאמה בין קבוצות (*matching*), והוא חוקר את השפעתם של שוברי הכשרה מקצועית על התוצאות התעסוקתיות של מובטלים. ממצאי המחקרים מעלים כי אימוץ כללים לצורך הקצאת משאבים היא דרך אפקטיבית לצמצום אפליה על-בסיסי פוליטי; ששינוי בעלות ובמאמץ הדרוש לצורך קבלת קצבה יש בה כדי להגדיל את שיעור התעסוקה של הפרטים וגם להשפיע על היצע העבודה של בני זוג; וכי לשוברי הכשרה מקצועית הניתנים למובטלים יש השפעה ארוכת טווח על רמת התעסוקה שלהם. הממצאים שופכים אור על התנאים להשגת אפקטיביות בהקשרים שונים, וגם על הנסיבות שבהן קבוצות מיעוט עשויות להתכנס למצבה של קבוצת הרוב.

**אסופת מאמרים בנושא**

# האפקטיביות של שינויי מדיניות והשפעתם על קבוצות מיעוט

חיבור לשם קבלת תואר "דוקטור לפילוסופיה"

מאת

איתמר יקיר

הוגש לסנאט של האוניברסיטה העברית

אוגוסט 2022

עבודה זו נעשתה תחת הדרכתם של

פרופסור מומי דהן ופרופסור מישל סטרבצ'ינסקי